CHAPTER 5

Space Simulation Studies

In the period before the start of the space program, the division to which I was assigned was called the Flight Research Division. As described in reference 1.1, most of the research work was conducted on full-scale airplanes. Some simulation studies were made, however, using special simulators designed to study specific problems. These simulators included a device called the yaw chair, that enabled a study of the ability of a human pilot to control lateral oscillations over a wide range of oscillation periods with both stable and unstable damping. In addition, a device called the NAP (Normal Oscillation and Pitch) chair was built that simulated the vertical and pitching motion of an airplane over a range of vertical motion of about 6 feet. These simulators allowed covering a range of conditions systematically, rather than obtaining different conditions by considering results on a number of different airplanes. At the time the space program started, a three-axis rotational simulator was under construction in which a cockpit was mounted to provide angular motion in pitch, roll, and yaw. This simulator was intended to study combined rolling and yawing oscillations of an airplane, but it found use for other purposes during the space program. In contrast with modern simulators that use electronic displays and general-purpose motion bases, these simulators used mechanical systems to simulate accurately the motion of the vehicle. The output of the mechanical system was amplified by a hydraulic servomechanism based on a variable displacement hydraulic pump. Servomechanisms of this type were available in Naval gun turrets. I have since learned that the development of these servomechanisms was largely attributed to Charles Manley, the same man who earlier perfected the excellent radial motor used in Samuel Langley's aerodromes.

Almost simultaneously with the start of the space program, all testing of high-speed airplanes was transferred to the High-Speed Flight Research Center at Edwards Air Force Base, now called the Dryden Flight Research Center. I was left with the problem of deciding on the best use for the engineers under my supervision, who had been trained in studying the stability and control of airplanes.

Airplanes, of course, had been flying for many years, and there was little need for studying the basic principles of stability and control. Most simulation work on airplanes was devoted to studying optimal stability and control characteristics or to finding the characteristics that *would provide the most desirable handling qualities for the pilot. At the start of the space program, before the first manned orbital flight, there was much less confidence in the ability of a human pilot to perform the tasks required for space operations. Many engineers expressed the view that it would be better to design spacecraft with completely automatic control. Test pilots, on the other hand, who had at least approached orbital flight conditions in tests of very high-altitude airplanes, usually felt confident that they could control the entire flight of a spacecraft just as they had controlled high-altitude airplanes.*

To resolve some of these questions, I felt that the conditions encountered in the various phases of a space vehicle flight should be simulated as accurately as possible to give the astronauts experience with the new problems of space flight. The following discussion describes some of the work done in this period.

After the successful completion of John Glenn's first orbital flight, most doubts concerning the effects of weightlessness were dispelled. Soon after this time, however, definite space programs, such as the Gemini and Apollo missions, were planned. The simulation work then focused on specific problems encountered in the launching, flight, entry, and landing of the spacecraft designed for these missions. These simulations are described in reference 5.1.

Lunar Landing Research Facility

Landing on the surface of the Moon was known to be one of the most critical phases of the Apollo program. Control by an astronaut, at least during the final phases of the descent, was considered mandatory because the nature of the lunar surface was not known in sufficient detail to plan the exact spot for touchdown. Several conditions present a control problem considerably different from that of landing an airplane on Earth. The lunar gravity is one-sixth that on the Earth. All control of lift and attitude is provided by rockets, which often provide a discontinuous, on-off control rather than a linear variation of control force familiar on airplane controls. The complete lack of an atmosphere on the Moon makes it impossible to use any type of aerodynamic control.

When President Kennedy made his announcement of a major program to send men to the Moon on May 25, 1961, I immediately started thinking about how this operation could be simulated. I wrote a memorandum on this subject in May 1961 and discussed the subject with the Associate Director, Lawrence K. Loftin, Jr. on June 26, 1961.

The trajectory of the lunar vehicle would be different from that on Earth because, as stated previously, the gravitational attraction of the Moon is only one-sixth that of the Earth. To simulate the reduced gravity, I visualized a suspension system for the simulated vehicle that would exert a constant force in the vertical direction equal to five-sixth the weight of the vehicle. The force on the cable on which the vehicle was suspended could be measured by a strain-gauge balance at the vehicle and used to control the output of a servomechanism that reels the cable in and out as required to apply the desired constant force to the top of the cable. To provide for horizontal motions of the vehicle in the fore-and-aft and lateral directions, sensors would measure the tilt of the cable from the vertical and would be used to control servomechanisms that moved the suspension point to keep it directly over the vehicle.

The motions of the vehicle in response to pilot commands would be provided by rockets. As in an actual lunar vehicle, a rocket sufficiently powerful to support the weight of the vehicle in the lunar environment, plus some extra power to maneuver, is required. Smaller rockets are used to provide pitching, rolling, and yawing moments. Previous studies had found that a system using a platinum catalyst to decompose hydrogen peroxide into steam and oxygen provided a convenient and relatively safe means to make a controllable rocket.

My first concern in designing the lunar landing facility was to analyze the servomechanism used to maintain a constant force in the suspension cable while the vehicle was going through the maneuvers of landing. While the technical details of this analysis are too involved to present in this discussion, a brief review of the problems involved in servomechanism design may be of interest. An example of a simple type of servomechanism is an autopilot to hold an airplane on a desired constant heading. If the heading deviates from the desired value, a compass or other heading detector measures the error in heading. The error may be converted to an electric voltage that is fed to an electronic amplifier. The output of the amplifier drives an electric motor that moves the rudder of the airplane in the direction to reduce the error. As the heading error is reduced to zero, the rudder is returned to its neutral position. The ratio between the rudder angle and the error in heading is called the gain of the servomechanism. Increasing the gain increases the speed with which the heading error is reduced, but it may cause the rudder to overshoot its neutral position and oscillate about zero. Beyond a certain value of the gain, the oscillations increase with time, a condition called dynamic instability. The gain must be kept to a value safely below that which produces instability.

In the case of the lunar landing research facility, a servomechanism maintains a constant force in the suspension equal to five-sixth of the vehicle weight while the pilot controls the rocket that provides an additional one-sixth of the weight plus whatever additional force is required to control the rate of descent or to slow the vehicle down for landing. These force variations supplied by the rocket act as disturbances to the force in the cable. The error in the cable force is corrected by the servomechanism by varying the speed at which the cable is reeled in or out.

If a steady disturbance is applied to a system controlled by a servomechanism, a steady error may result. This condition may be corrected by placing an integrator in the feedback loop. The integrator builds up a signal that increases with time to offset the steady error. An integrator also has a gain to determine its speed of operation. If the gain is too large, dynamic instability will again result. In general, the tendency to instability is greater with an integrator in the circuit.

In the case of the lunar landing facility, a typical mode of operation is to let the vehicle fall freely under a steady acceleration of one-sixth g until the descent rate reaches the desired value to start the landing run. In this case, the cable tension should remain at its desired value during this period of constant acceleration. To maintain this steady value during conditions of constant acceleration, servomechanism theory shows that a double integration of the error is required. Such an arrangement is called a type 2 servomechanism. I therefore realized that a double integration should be included in the computer of the lunar landing facility. With such an arrangement, careful design of the gains is required because an even greater tendency to instability exists.

Another effect that influences the stability of the response of the system is the time lag between the output of the servomechanism and the resulting force applied to the vehicle. This lag results from the time required for tension and compression waves to travel down the suspension cable. This speed is equivalent to the speed of sound in the cable. If a braided steel cable is used, the speed of sound in the cable is about 1000 feet per second. For a cable 200 feet long, the lag is two-tenths of a second.

Despite the effort to make an accurate simulation of the trajectory and control characteristics of the lunar module in the lunar environment, one factor that cannot truly simulate conditions on the Moon is the gravitational force of the Earth acting on the pilot's body. The effect of this force can be minimized by strapping the pilot in and otherwise supporting his body so that this force does not interfere with his control activities.

Other factors that complicate the analysis of the servomechanism are the springiness of the cable, which changes with the altitude of the vehicle, the damping of the drive mechanism, and the change in weight with fuel usage. I considered as many of these factors as possible in an analytical study of the system. All these factors made the analysis difficult either with available theory or with the computers available at that date. I therefore considered an experimental study of the system necessary.

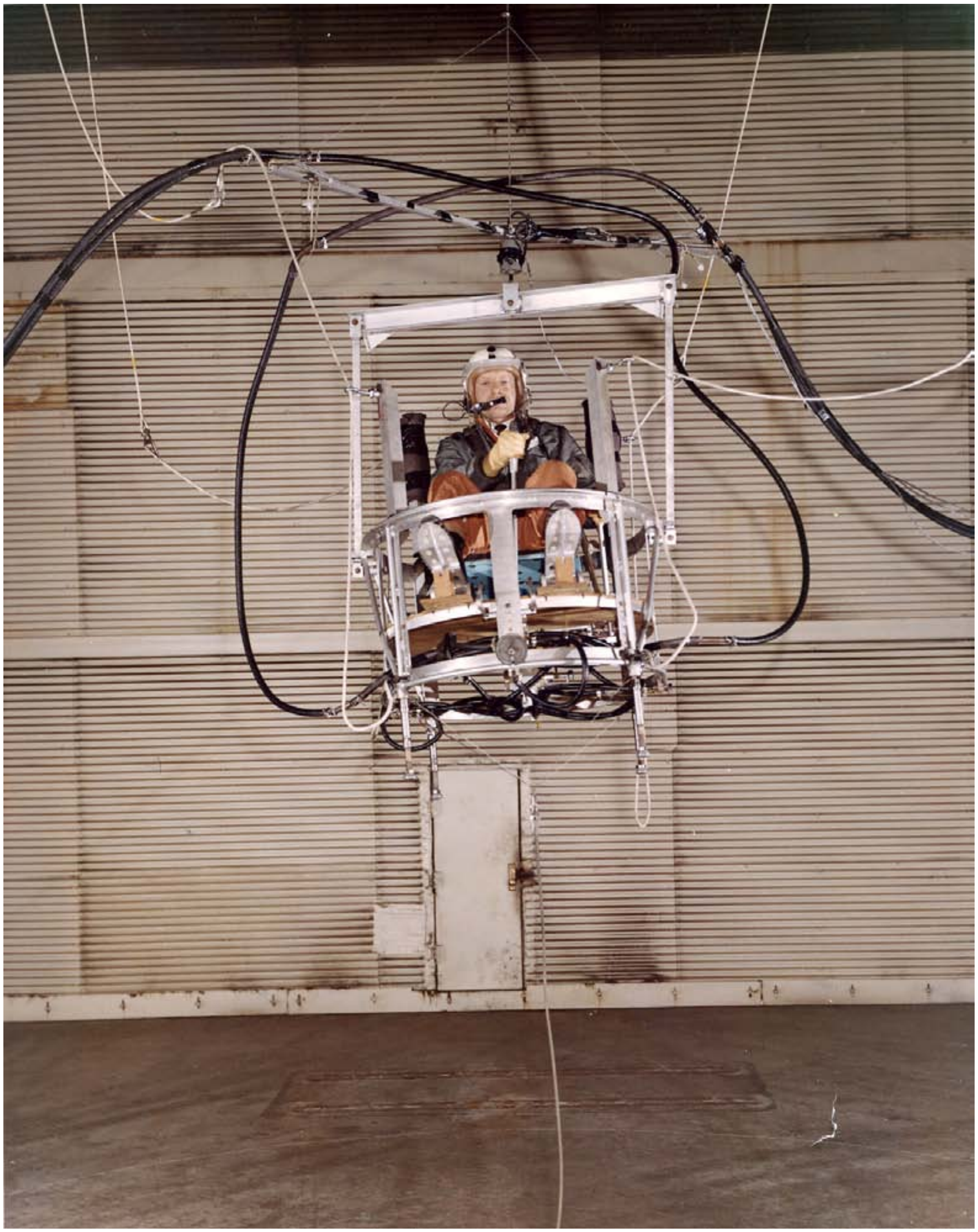
To test the feasibility of the contemplated system, a simplified system, called the pilot model, was built in which a pilot's chair was suspended by a vertical cable. A photograph of the pilot model in operation is shown in figure 5.1-1 with the engineering test pilot Jack Reeder at the controls. A servomechanism that reeled the cable in or out was mounted in the girders in the roof of the NACA Full Scale Tunnel, 60 feet above the vehicle. This facility was chosen because the wind tunnel had a compressed air supply that could be used to provide thrust for the rocket that was used to overcome the simulated lunar gravity of one-sixth of the vehicle weight. An available analog computer of fairly early design was used to calculate the signals driving the servomechanism. With this apparatus, the gains of the system were varied experimentally until a reasonably constant cable tension could be maintained as the pilot performed a simulated landing.

An important reason for using the overhead suspension is that if any loss of control occurs, either due to pilot error or some malfunction in the system, the vehicle can be locked in place or lowered slowly to the ground like an elevator.

The successful operation of the pilot model, despite some claims that it would not be feasible, gave me confidence to propose the design and construction of a full-scale system. I wrote a memorandum on the proposed system and made a trip with Lawrence K. Loftin to NASA Headquarters in Washington, DC. There we discussed the project with Ira H. Abbot, who at that time was an assistant director for space programs. He quickly agreed to support the project and to provide the necessary funds.

At the same time that my project was proposed, engineers at the Ames Research Center proposed a flight vehicle that worked on the same principle. This device, called the Lunar Landing Training Vehicle (LLTV), used a turbojet engine to support five-sixth of the vehicle weight. The engine was mounted on gimbals and controlled by servos connected to an inertial measurement unit so that it would always point vertically.

FIGURE 5.1-1. Pilot model for LLRF being flown by NASA test pilot Jack Reeder.



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The pilot's cockpit was mounted in a large frame that surrounded the jet engine and contained the necessary rocket engines to exert force to offset the simulated lunar gravity and to provide pitch, yaw, and roll control. I thought that this device would be very dangerous to fly because it had no means to recover in case of loss of control. Eventually, three of these vehicles were built. The cost of this project was undoubtedly many times that of the Lunar Landing Research Facility (LLRF), but the funding for the Apollo

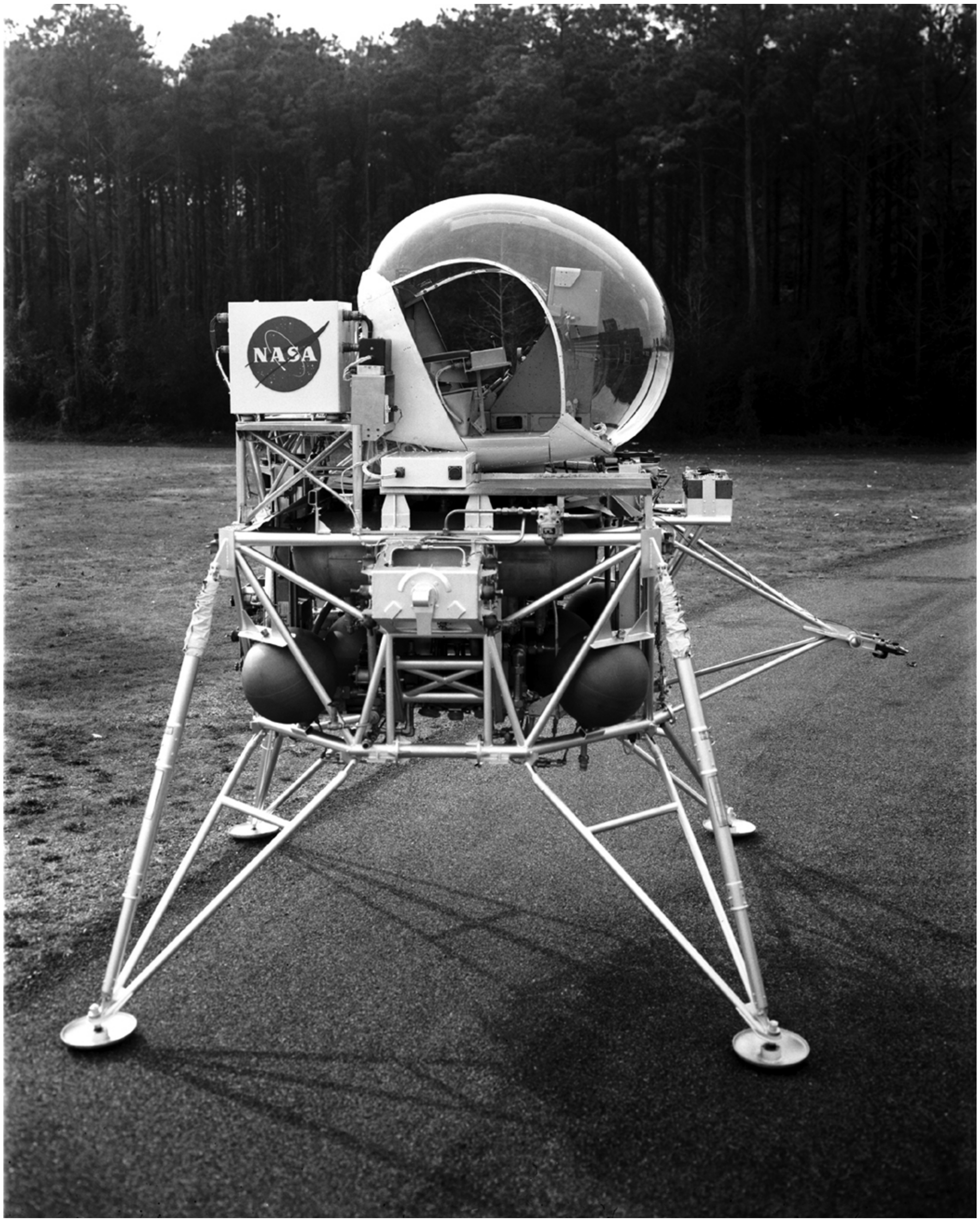

FIGURE 5.1-2. **Lunar Landing Research Vehicle showing original cab for pilot.**

program was so generous that both projects were supported.

The firm of Jackson and Moreland was selected to build the gantry and operating system for the LLRF. The estimated cost was $4,997,700, which was close to the actual cost. The contract for a piloted vehicle to be lifted on the LLRF was let to Jered Industries, a small company with shops located under an old football stadium in New England. This company built the piloted vehicle for the remarkably low cost of $250,000. This vehicle differed from the LM used in the

Apollo mission because the LM had not been designed at the time the LLRF was constructed. The test vehicle differed from the LM in being smaller and initially having a bulbous helicopter type cockpit (fig. 5.1-2). Later the LLRF test vehicle was equipped with a cockpit that placed the astronaut in a standing position like that in the LM, with wide windows to see the lunar landscape (fig. 5.1-3). Both vehicles had outstretched legs with pads on the ends to give a stable support on the lunar surface.

A photograph of the completed LLRF is shown in figure 5.1-4. The gantry, or large crane structure used to suspend the vehicle, is 300 feet long, 250 feet high, and 100 feet wide. The overall height of 250 feet was set by the limits on the height of buildings on Langley Air Force Base to avoid interference with air traffic. The tracks on which the suspension system rode allowed a lateral travel of ±15 feet and a movement down the track of 250 feet. With allowance for the space taken up by the structure and suspension system and the overall height of the suspended vehicle, the change in altitude during a test run was about 185 feet. Part of this amount was taken up by the distance required to fall at 1/6 g to the initial descent velocity. The system was finished in 1966 and completed initial testing in 1967.

After the initial planning of the LLRF and the construction had been completed, I took little part in actual operations. Donald E. Hewes was placed in charge of running the facility, with Thomas O'Bryan as assistant. Among the other NASA engineers assigned to the facility were Eric Stewart, Maxwell Goode, Randall Harris, Max Kurbjun, Amos Spady, Marna Mayo, and Frank Read.

As the name implies, research work was done on the facility to study the control laws relating the pilot's control inputs to the vehicle response, the ability of the pilot to make a sufficiently soft landing, and so forth. NASA test pilot Lee Person did much of this work. In later stages of the work, Don Hewes arranged to have piles of cinders placed on the ground under the facility that were shaped to simulate lunar craters and terrain. In addition, tests were made at night with lighting simulating the low position of the Sun that would be present in the actual lunar landing. The shadow of the vehicle on the terrain, shown in figure 5.1-5, gave the astronauts a good impression of the height of the lander as it approached the surface. All the astronauts who were scheduled to make lunar landings in the Apollo program took part in test runs made under these conditions. The astronauts later stated that the landings were very good simulations of the actual landings on the Moon, and that they were very helpful in training for these operations (ref. 5.1).

While the tests at Langley were in progress, the Ames flying vehicle, the LLTV, was placed in operation at the Manned Space Center at Houston and was flown by the astronauts. In one of the early tests, the vehicle went out of control. The astronaut, Neil Armstrong, ejected barely in time to save his life, and the vehicle was destroyed. I was called to Houston to serve on the review board that studied the causes of the crash. The cause was found to be that aerodynamic forces on the framework of the vehicle in forward flight were large enough to overpower the jets used for control. One of the remaining vehicles was then mounted in the Langley Full-Scale Tunnel to study means for reducing the aerodynamic forces on the framework.

Later, another of the LLTVs went out of control and crashed while being flown by Joe Algranti, a former Langley test pilot who was then in charge of flight operations at Houston. The crash was

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FIGURE 5.1-3. **Lunar Landing Research Vehicle hovering with stand-up cab for pilot.**

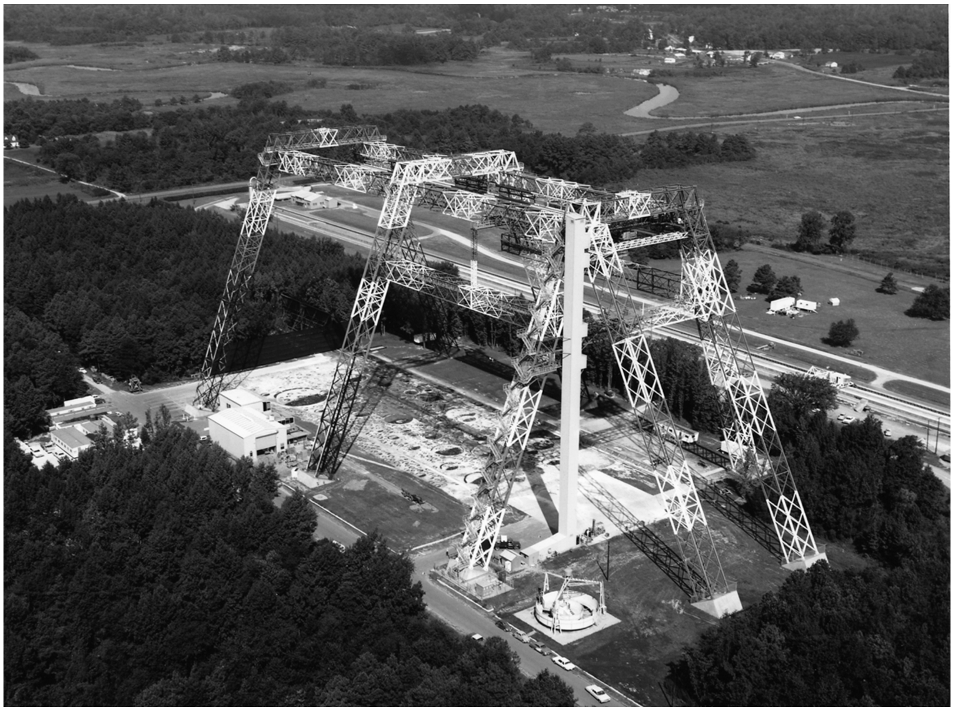

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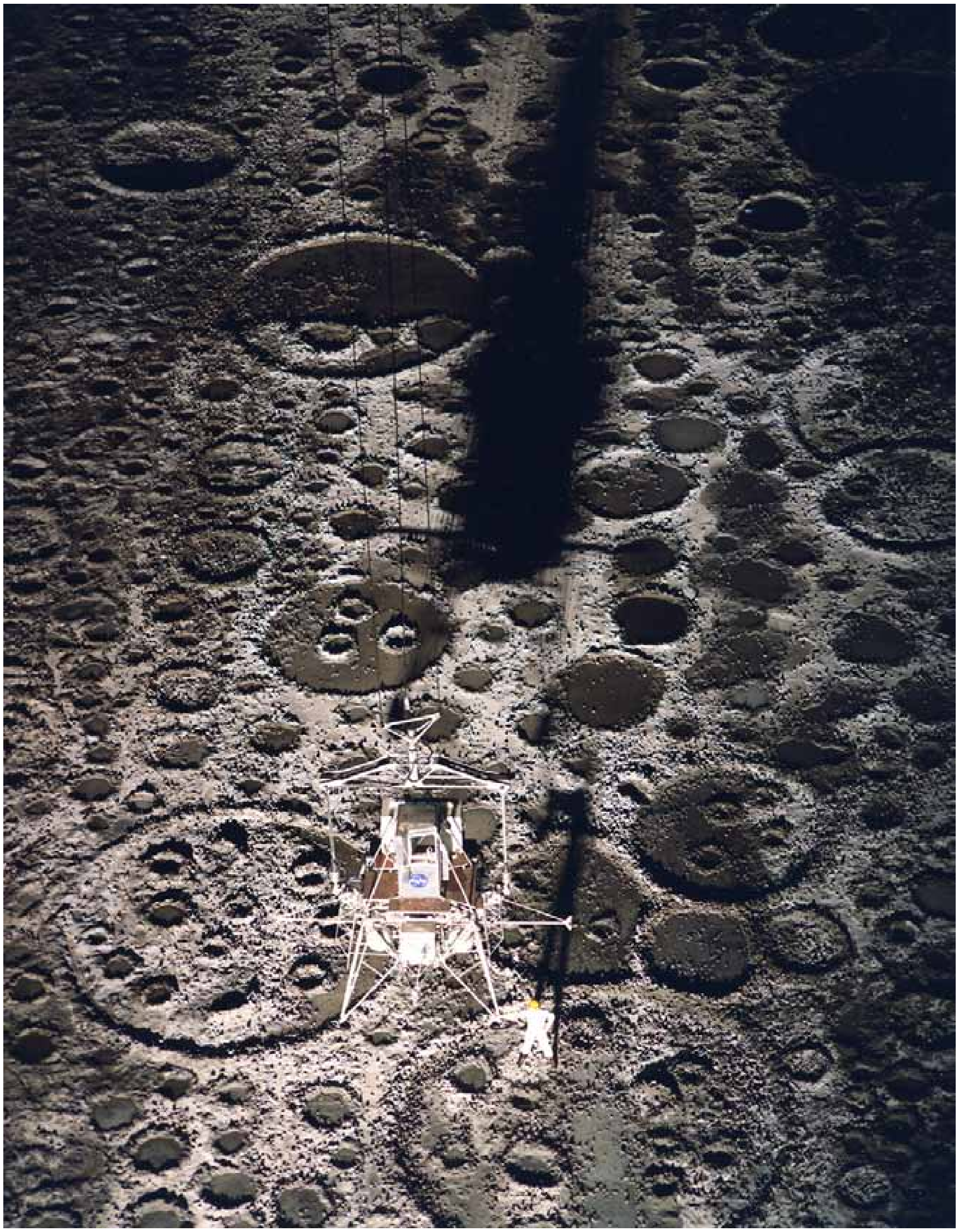
FIGURE 5.1-4. **Lunar Landing Research Facility at NASA Langley Research Center, Hampton, Virginia.**

FIGURE 5.1-5. **Simulated craters on terrain in landing area of LLRF. Shadow of vehicle with lighting simulates low Sun angle.**



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again found to be caused by a wind gust, this time from the side, that overpowered the controls. Both Armstrong and Algranti were saved by the Martin-Baker ejection seat, a so-called zero altitude ejection seat that shot the pilot up from ground level with a rocket sufficiently powerful to put him at an altitude where he could be saved by his parachute. This seat, an English invention, had become available just in time to be used in the program.

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FIGURE 5.1-6. Lunar Landing Research Facility in use at crash test facility.

(a) Test aircraft about to impact ground in crash test facility.

(b) Results of crash test.

The astronauts felt that both simulators were of value in training for the lunar landing. The LLTV had the advantage of providing for a greater altitude at the start of the maneuver, 300 or 400 feet instead of about 185 feet in the LLRF. On the other hand, the LLRF had the advantage of more realistic simulation of the terrain and lighting conditions. The astronauts, like typical test pilots, ignored the dangers inherent in the flight vehicle and continued making simulated landings with the remaining vehicle, but great care was taken to restrict testing to calm days.

The Apollo program was stopped abruptly after six successful lunar landings, even though two additional Saturn launch vehicles had been constructed to continue the program through two more flights. Astronaut training flights were therefore stopped. Don Hewes continued research with a small one-man vehicle to study the feasibility of a flight vehicle for lunar exploration in place of the lunar rover that had been used on the last three Apollo flights. The flight vehicle showed promise for much greater range than the rover because the low gravity on the Moon allows a rocket-supported vehicle to fly for long distances. This project was also dropped, however, when there was no prospect for future lunar landings.

The LLRF stood idle for a number of years and the servos and other equipment on top of the gantry were removed. A new use was found for the gantry as a crash test research facility. In this application, used airplanes or airplane structures were hauled up to the top of the gantry and allowed to fall in a circular path, like a pendulum, to impact the ground. For some tests, a rocket boost was used to increase the speed of the impact. These tests have been found to be very useful in designing the airplane and the cockpit structure to protect the pilot in case of a crash. Pictures of such a test are shown in figures 5.1-6 (a) and (b).

The most novel feature of the LLRF from a technical standpoint was the servomechanism that maintained a constant tension in the suspension cable during the landing maneuver. Other uses for this feature are rather scarce, but I did learn of one practical application. A group of engineers from Canada came to Langley to discuss the use of the constant-tension servo to haul a helicopter down to the deck of a ship in a rough sea. In this application, the cable would be lowered from the helicopter and hooked onto the deck of the ship by a crewman on the ship. Then the cable would aid the pilots of the helicopter in descending to the deck without experiencing large impact loads due to the motions of the deck.

The LLRF and the Research Vehicle have been declared State Historic Landmarks. The gantry is still the most prominent feature on the NASA Langley landscape, and the vehicle is on display in the Virginia Air and Space Center in Hampton.

Lunar Orbit and Landing Approach (LOLA) Simulator

The Lunar Orbit and Landing Approach (LOLA) simulator was, as the name implies, intended to study the ability of a pilot to control the LM following its separation from the Command and Service Modules during the lunar orbiting phase of the flight until it reached the desired landing area. This simulator was placed under the direction of my division, although I was not involved in its concept or design. Most of the planning for the Apollo mission had been based on the idea of computer-controlled navigation based on either astronaut measurements of angles to the Sun, Moon, and stars, or on Earth-based radar measurements. This idea was opposed by Charles H. Zimmerman, an early Langley employee who had made notable contributions to stability and control research by originating the free-flight tunnel and the spin tunnel and by publishing reports on airplane stability theory. He later left to join the Chance Vought company where he supervised the design of the Vought F5U-1 “Flying Turtle” VTOL fighter airplane, but later returned to Langley after this project was cancelled. As a result of his long experience, his opinions carried considerable weight among the center supervisors. For a time, he was assigned to the Director’s office but later was appointed as assistant head of my division, the Aerospace Mechanics Division. Zimmerman felt strongly that all the complex computers and instrumentation planned for navigation of the Apollo were unnecessary and that an astronaut could direct a space vehicle to its destination based solely on his visual cues, just as a pilot (at that time) often did in flying an airplane cross-country. This opinion was supported by Clinton E. Brown, an Assistant Director at Langley. As a result, they placed much emphasis on building a simulator that could investigate the Apollo pilot’s navigational ability and could be used to train astronauts to fly to the Moon in this manner.

The simulator involved several original features. A camera was moved in response to the pilot’s inputs over a large map covered with three-dimensional images of the lunar terrain. The lunar craters and other features were based on photographs of the Moon and machined into Styrofoam® blocks with a computer-controlled milling machine. The terrain modeled covered a path around the Moon in the vicinity of the orbit. The images photographed by the camera were projected on the interior of a sphere about 10 feet in diameter, where the pilot’s cockpit was located. As the pilot flew the simulated vehicle, the view of the Moon slowly moved under the pilot and appeared as it would in an actual lunar orbit. The altitude had to be limited to a minimum value equivalent to a few hundred feet on the actual Moon to prevent the camera from bumping into some of the higher features on the map.

The construction of the simulator was very expensive and time-consuming because a simulator based on these ideas had not been constructed before. The terrain map was constructed of large blocks of Styrofoam®, which were carved by digital milling machines or molded to the correct shapes. A special TV camera with multiple lenses provided a wide field of view. The pilot’s control inputs were fed into a computer that calculated the attitudes and orbital motions of the vehicle. The camera moved along a track, with its height and attitude controlled by servomechanisms (ref. 5.2).

Because the project was supported by high officials at Langley, funds and personnel were provided to keep the project moving. The engineers said: “what LOLA wants, LOLA gets,” the words of a popular song of that period. When the simulator was finally tested, it worked as planned, but the results proved rather disappointing. In typical test runs, the pilot sat in his cockpit for up to an hour with little to do but watch the landscape go by. When the pilot started his descent to land, the simulator cut off to protect the camera. The main benefit gained from this project was the later development of simulation equipment suitable for aircraft landing studies. The lunar terrain was replaced by a picture of the airport. The same camera and projection sphere were employed to study the ability of a pilot to land an airplane under various conditions. Some years later, electronic displays were developed and the large terrain map was no longer required.

I wrote a memorandum to Charles J. Donlan, then the Director of Research at Langley, pointing out the fallacy in the use of piloted control for navigation of a space vehicle. Because of the large amount of fuel required to put each pound of weight into orbit, missions must be planned to use an optimal trajectory to minimize the amount of fuel required. This system is possible on a spacecraft because the trajectory, influenced mainly by the accurately known gravitational attraction of the Earth, Moon, Sun and other heavenly bodies, can be calculated with extreme precision. Disturbance from other sources, such as atmospheric drag, solar wind, radiation pressure, and magnetic effects are ordinarily very small, and can be allowed for by small corrections to the ideal trajectory. By contrast, an airplane flying across the country is subject to large unpredictable disturbances due to winds and gusts, weather variations, and so on. Sufficient excess fuel is always carried to allow flight to an alternate airport, usually at least 500 miles from the scheduled landing field. This excess fuel allows the errors involved in navigation by a human pilot to be tolerated. In space, on the other hand, automatic guidance equipment and computer-controlled corrections for any errors in the trajectory must be used. As an example of the margin of error that can be tolerated, Astronaut Neil Armstrong, in making the first manned lunar landing, had to make a slight manual correction during his final landing approach to avoid a crater. The landing was made with just 3 seconds of fuel remaining, yet no one seemed concerned about this small tolerance for error. I have heard estimates of the fuel remaining ranging from 3 to 20 seconds.

Docking Simulator

Rendezvous and docking with a target vehicle was one of the main objectives of the Gemini program because it was required in the Apollo program when the upper part of the LM was propelled up from the Moon and docked with the command and service modules in lunar orbit for the return trip to the Earth. The final stage of the docking involved manual control by an astronaut because precise control was required for the close alignment and gentle impact required for a successful docking.

The docking simulator, suspended from the roof of the large flight research hangar in the west area of the Langley Research Center, was built to explore the problems of this operation. Arthur W. Vogeley proposed the concept for this device and supervised the personnel who made most of the tests with it. A target vehicle was suspended at one end of a track attached to girders in the hangar roof. A movable vehicle was driven along the track to approach the target during a docking maneuver. A gimbal system was suspended from the movable vehicle by an ingenious system of cables originated by Vogeley. The cables were arranged in V-shaped pairs that maintained the outer gimbal in a horizontal position while keeping it directly under the movable vehicle. The gimbal system could be moved up and down by reeling in each pair of cables simultaneously on a large pulley in the overhead vehicle. Inside the gimbal system was the approach vehicle, which for most tests was a replica of the Gemini capsule.

The gimbal system with the capsule mounted inside was an existing piece of equipment that had been built to study the pitch, yaw, and roll oscillations of airplanes in lateral maneuvers. The gimbal angles were controlled by

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FIGURE 5.3-1. Docking simulator.

electrohydraulic servomechanisms that were operated through an analog computer system with inputs from the pilot's control stick. The main application of the docking simulator was to study sighting and target devices that would allow the astronaut to sense his rate of approach and alignment with the target vehicle. The most successful devices incorporated a "two-layer" device in the target vehicle, such as a cross mounted ahead of a small circle that would allow the astronaut to judge when he was properly aligned. In recent years, the docking simulator was honored as a State Historical Landmark. A photograph of the docking simulator is shown in figure 5.3-1.

Aids for Extravehicular Maneuvering

Two devices were proposed for extravehicular maneuvering, one by Harold I. Johnson and one by John D. Bird. In discussing the reasoning behind these devices, the approaches proposed by the Johnson Space Center and their contractors should be kept in mind. In general, the devices considered by the space center consisted essentially of a strap-on vehicle that could hold the astronaut in his space suit. The vehicle itself had a stabilization system with gyroscopes and automatic control equipment, a set of rockets to allow propulsion and attitude control, and two hand controllers, one for attitude control and one for translation. Essentially, the proposed vehicles were complete spacecraft that could fit around an astronaut. No one doubted that such a device could be made to work, but the cost of manufacturing such a device would run into millions of dollars. The frugal NASA engineers at Langley thought that much simpler and less expensive devices could be made to accomplish the same purpose, while at the same time allowing the astronaut to work with a less encumbering piece of apparatus.

Harold I. Johnson called his device a “Space Gun.” It consisted simply of a tank of compressed gas and a hand-held tube with a nozzle at the end. The astronaut was intended to hold the tube and control the flow of gas with a trigger-type controller. Harold Johnson believed that the astronaut could readily learn to control his attitude and motion through space instinctively by directing the thrust of the device properly with respect to his center-of-gravity position.

John D. Bird’s device (called “jet shoes”) was inspired by swimmers using swim fins. He proposed putting a pair of nozzles on the astronaut’s shoes that would allow control of the thrust by valves operated by the astronaut’s toes and directed as required by motions of his legs and ankles. This method had the advantage of allowing complete freedom of the astronaut’s arms and hands.

These devices seemed to me so unusual that simulation of their operation would be required before they could be considered for use in space. To illustrate the varied opinions of the value of simulation studies, Hartley A. Soule, an early expert on aircraft control and handling qualities, said that he had no doubt whatever that such devices could be operated by an astronaut, and did not see the need for any simulation tests. For a time this opinion put an end to tests of these ideas at Langley.

Harold Johnson, perhaps because of his desire to try out his concept, transferred to the Johnson Space Center. In time, the “space gun” was assigned a position as an experiment on one of the space flights involving EVA, and a brief test in space was made. The astronaut involved thought that the device performed successfully. So far as I know, no further use was made of the device. The Johnson Space Center personnel contracted for the construction of the “space vehicle” type of device, and it was tested in a large simulation facility constructed by the Martin Company. This facility, somewhat on the same principle as the Langley Lunar Landing Facility, suspended the astronaut in his chair and simulated its motions in a zero g environment. The simulation facility allowed perfecting the characteristics of the device. This system has since been used for all tests involving EVA. The astronauts expressed preference for the vehicle-type device, probably because of a greater feeling of security obtained in a vehicle that was automatically stabilized.

Don Hewes undertook the tests of John Bird’s idea of control through foot motions. He made a very flat and smooth floor by pouring slow-drying epoxy cement on a flat floor area that had boundaries at its edges. Don thought Bird’s idea of thrust control by toe switches was rather far-fetched. Instead he used a hand controller, and fitted the astronaut’s shoes with nozzles that could fire fore or aft. The astronaut lay on his side on a framework supported by three air bearings. These bearings were flat disks that had small holes on the bottom through which compressed air was admitted. These bearings supported the weight of the astronaut and his equipment so that the system was floating on a cushion of air.

Similar air bearings were used by numerous people at Langley for space simulations. They reduced the friction to zero, so that the motion on the floor provided a two-dimensional representation of the motion in space. Hewes found that motion over the flat floor with the foot-operated device could be controlled very easily.

To avoid the limitation to two-dimensional motion, several organizations built large water tanks in which the astronauts in their space suits, loaded to a condition of neutral buoyancy, could practice operations required in space. This method eliminated any steady forces on the astronaut in a motionless condition, but of course it introduced rather large drag forces opposing any motion. Nevertheless, the method proved effective because most motions used in EVA were slow. Art Vogeley built a water tank at Langley 30 feet deep and 30 feet in diameter, in which the Langley test pilots could don space suits and experience the feeling of zero gravity. These tests were soon stopped because the space centers had larger facilities, more trained personnel, and because of safety considerations.

A fixed-base, 6 degree-of-freedom visual simulator utilizing a projection sphere, to be described later, was also used to test Hewe's foot-controlled maneuvering device. These tests were successful in demonstrating the ability of a pilot to control tumbling motion in space. Tests of the foot-controlled maneuvering unit were later made in the Skylab by astronauts Alan Bean and Gerald Carr during space missions. The hand-controlled unit proposed by the Johnson Space Center was also tested in these missions. As mentioned previously, the astronauts preferred the more complex vehicle made under contract by the Martin Company.

CHAPTER 6

My Work With Johnson Space Center

When the Space Task Group moved to the Johnson Space Center, they soon were in a period of rapid expansion and were involved in contract work to produce ground facilities and space vehicles for the space program. Close contact was maintained with Langley research engineers to provide assistance with research and development problems that arose. Axel Mattson, a Langley engineer with long experience in aerodynamics and in facility development, was appointed as the liaison man stationed at Johnson Space Center to maintain contact with Langley personnel.

Dr. Christopher C. Kraft, an engineer who worked under me in the Stability and Control Branch of the Flight Research Division, joined the Space Task Group and moved to the Johnson Space Center. He first held a position as Flight Controller and was instrumental in designing the Flight Control Centers at Cape Kennedy and later at Houston as well as in directing many early flights. Kraft was an excellent administrator and held successively higher positions at the Johnson Space Center until he became the director of the Center shortly after the start of the Shuttle program. Kraft has written a book describing his work as a flight controller (ref. 6.1). Kraft always very generously gave me credit for his early training in technical work. When problems arose at Johnson, he frequently called on me to assist with their solution. I did not always have the knowledge to provide the solutions to these problems, but I was usually able, through my contacts at Langley, to find the necessary information or at least to give a reasonable opinion as to the work required. I cannot present in detail all the problems on which I worked, but I will summarize some of those that seem most important.

Design of Shuttle Control System

My first important work connected with the Johnson Space Center concerned the Shuttle control system, although I had made previous visits to that center to discuss simulators for the Apollo. A brief review of the background of the Shuttle development is desirable to give a basis for the various problems that arose.

The Johnson Space Center was an impressive place during the Apollo program. It had a beautiful campus with large office buildings and laboratories. No expense had been spared to make

Apollo a success. As the Apollo program neared a close, however, the Johnson engineers started to give serious thought to a follow-on program. Perhaps the most popular proposal was to build a space station, which in turn required a shuttle vehicle to supply it. The Apollo program was a great success. It beat the Russians to the Moon, completed its program on schedule, and provided much important scientific data. After the sixth Apollo flight, however, the program had lost its political popularity, and Congress failed to appropriate funds for a seventh flight, even though a Saturn launch vehicle had already been built to carry the Apollo vehicle to the Moon on its seventh mission.

With the abrupt halt of the Apollo program, funding for a space station was not available. As a possible alternative, the management proposed a shuttle vehicle as a first step toward a space station. Such an interim project would keep the Space Center in operation. Little study had been made, however, on the design of such a vehicle.

A young engineer in the Space Task Group, Max Faget, had proposed the design of the Mercury capsule, and later extended the capsule concept to the Apollo vehicle. His proposal for a space shuttle also provided the basic concept for the Space Shuttle that later was actually built. Faget, a model airplane enthusiast, brought in a balsa and tissue paper model of a shuttle that incorporated an unswept, low aspect ratio wing, a rather fat fuselage, and a tail with a wide-chord elevator that provided sufficient control power to trim the vehicle to a very high angle of attack, probably 45°. The concept was to enter the atmosphere at a very high angle of attack to reduce the heating on the leading edges and lower surface of the vehicle, and later to pitch down for a conventional landing at an airport. Because of the capability of an unswept wing to provide a relatively high maximum lift coefficient at subsonic speeds, the vehicle would have had a landing speed low enough to land at practically any airport.

I am not sure of the sequence of events that followed, but wind-tunnel tests of Faget's concept showed that at hypersonic speeds, very strong shock waves formed at the intersection of the wing and fuselage that would have caused a heating problem on the side of the fuselage. Later, the proposal for a shuttle design was sent to three contractors. The winner, Rockwell, Inc., proposed a delta-wing configuration with a large fairing between the wing root and the fuselage and included a large elevator capable of pitching the vehicle to high angles of attack at hypersonic speeds. This configuration avoided the excessive heating on the sides of the fuselage, but it was capable of only relatively low values of lift coefficient for landing. As a result, long runways would be required for landing. Later, the high landing speeds resulted in problems with brakes overheating and tires exploding, but these problems were overcome with new designs for brakes and tires.

I have often thought that more study should have been made of the problems involved in Faget's original design. Possibly wing fillets and root fairings could have overcome some of the fuselage heating problems. The capability to land on shorter runways would certainly have been a safety feature in later operations.

My first involvement in the design of the Shuttle involved the control system. The Shuttle was one of the first airplanes to incorporate a complete digital fly-by-wire control system. The term fly-by-wire means a system in which all control surfaces or other control components are operated by electrical signals sent through wires. A digital system means that control signals are generated by a digital computer that contains all the necessary control laws and signal transmission devices. Previous airplanes had used fly-by-wire control systems using analog components and usually with a mechanical back-up system. An example is the highly successful control system on the Concorde. In the case of the Shuttle, however, a digital system was considered necessary because of the wide changes in the control laws throughout the flight required by the wide range of Mach numbers and flight conditions encountered. Mechanical systems consisting of control cables and pushrods would have had to handle excessive forces and would have encountered problems due to heating. The development of digital computers had reached a state that was considered to have adequate capability and reliability to perform the control task, though much development was required to overcome the new problems encountered.

To verify the design of the control system, a large working mock-up of the system was built at the Johnson Space Center. One of the lead engineers in this project was Robert G. Chilton, who had worked under me in the Flight Research Division at Langley. He gave me a tour of the facility and a briefing during one of my visits to the Center. The system used three digital computers designed and programmed by the Honeywell Corporation. Though each computer was highly reliable, the reliability requirements are such that they cannot be met by a single computer. The consequences of a failure are so severe that the system is required to perform safely for millions of flight hours, representing the lifetime of not just one vehicle but of a whole fleet of vehicles. This degree of reliability can be met by using the principle of redundancy: that is, three or more computers perform the control task simultaneously. If any one computer disagrees with the other two, it is immediately shut down and repaired after the vehicle lands. If a second computer fails, a comparison of the output of the two computers is made and a check based on expected output or other means picks the best remaining computer.

I was aware of an experimental triply redundant digital control system that had been installed in a helicopter at MIT. Despite the redundancy, this system had failed in flight. The problem was that a programming error had occurred in the software for the computers. The computer program was identical for the three computers. When this error was encountered, all three computers shut down simultaneously, leaving the helicopter without a control system. I discussed the problem with Dr. Raymond C. Montgomery, an engineer in my division who knew more about computers than anyone else in the division. We concluded that the only way to avoid such a problem on the Shuttle was to install a fourth computer programmed by an independent group. The problem was discussed with personnel at the Johnson Space Center. As a result, a fourth computer was programmed by personnel at the Draper Lab. It was ruled that the fourth computer, like the other three, was a safety of flight item. Thus, the Shuttle could not be launched unless all four computers were working properly. Once, while preparing for a Shuttle launch, the fourth computer malfunctioned. The launch was delayed until this computer had been fixed.

Studies of Shuttle Control System

The Shuttle system was designed to be fully automatic except for the final stage of the approach and landing, when the astronauts took over. The initial design of the system software, when tried on a simulator, resulted in the Shuttle diverging to large angles when subject to certain disturbances. Such a divergence would be catastrophic, and therefore caused considerable concern among the designers. At this point I was called down to Houston by Dr. Kraft to study the system and work with the designers to obtain a satisfactory system.

I worked with Ken Alder, a contractor from Lockheed who obviously had an excellent knowledge of stability and control. The work was mainly educational for me because considerable work had already been done to provide a satisfactory system. Later, I observed test runs on a simulator that traced the re-entry trajectory of the Shuttle and allowed the study of the effects of gusts, cross winds varying with altitude, and so on to provide a sufficient margin of safety for all conceivable disturbances.

As mentioned previously, the vertical tail of the Shuttle, mounted on top of the fuselage, became ineffective at high angles of attack. A method was worked out to provide directional control using only the elevons during the high angle of attack part of the entry, which lasted from the start or the descent at about Mach 23 to Mach 2. By this method, the elevons moved differentially in the opposite direction from what would be required for roll control at subsonic speeds. To yaw to the right, for example, the right elevon would move down (or to a smaller upward deflection), increasing the drag of the right wing. The left elevon would have the opposite movement. This right yaw (or left sideslip), because of the large dihedral effect of the delta wing, would cause the Shuttle to roll to the right despite the left rolling moment caused by the elevon deflection.

At low supersonic Mach numbers, when the aerodynamic heating was reduced, the Shuttle would be pitched to a lower angle of attack, so that the rudder was unshielded and became effective for yaw control. In this regime, the elevons were moved differentially in the normal direction for roll control, opposite from what had been done at higher Mach numbers. The rudder then served to offset the yawing moments from the differentially deflected ailerons as well as to provide yaw control. The controls were then in a normal configuration so that the astronauts could take control when they made the flare and landing.

The variation of control laws with Mach number is an example of a change that can be made readily with a digital control system but would require some complex device with a mechanical system. In fact, the digital system provided a smooth ramp-like reversal in the elevon control when the shift was made, and in addition provided control gain variations from various sensors such as rate gyros and angle of attack and yaw sensors throughout the descent. The studies being made to improve the safety and control effectiveness of the system were mainly concerned with the adjustments of these gain values for the various phases of the descent.

After the changes in the software had been worked out, I spent some time watching the runs on a simulator, along with one of the engineers. This simulator just plotted a trace of the trajectory on a screen. On one run, I observed that the trajectory near the end of the run diverged. The engineer, who had not been watching, was quite surprised. After analyzing the data, he concluded that this fault had been observed before and had been corrected, but the change had not been made in the latest version of the program. This experience illustrates the vigilance required in preparing software for computers in a flight vehicle.

Problem With Pilot-Induced Oscillations

To give the astronauts practice landing the Shuttle before going through the dangers of a complete orbital flight, a number of flights were made in which the Shuttle was mounted on a trapeze on top of a Boeing 747 airplane and was released from an altitude of about 10,000 feet to glide to a landing on the long runway at Edwards Air Force Base, now the Dryden Flight Research Center. In the fifth landing, the Shuttle made a normal flare, but just before touchdown it made two or three rather violent short-period oscillations. The Shuttle landed safely after the co-pilot had told the pilot to stop trying to control the oscillation and let the Shuttle land itself.

Oscillations of the type experienced on the Shuttle are called "Pilot Induced Oscillations," (PIO). for short. Such oscillations have been experienced on many airplanes, starting with the Wright Brothers' first flights, and became more frequent with the introduction of hydraulically operated control systems. The cause of these oscillations can often be analyzed, and their reoccurrence can be avoided after they have occurred, but predicting the tendency for oscillations beforehand can be much more difficult because they can result from many different causes. In the case of the Shuttle, a detailed analysis was made on the response characteristics of the digital control system, and the system was found to have a response lag of 0.2 seconds or more following a pilot's input.

Again I was called to the Johnson Space Center to help with the analysis. Dr. Robert Gilruth, who was the Director of the Space Center, had run flying qualities tests on 16 airplanes and, in the early forties, had written a celebrated report called *Requirements for Satisfactory Flying Qualities of Airplanes*. In this report he had stated that a lag in the control system response of greater than 0.1 second was unsatisfactory. Gilruth's conclusion was based on tests of spoiler ailerons on the wing of a light plane. These spoilers were located near the leading edge of the wing. When they were abruptly deflected, the flow over the spoiler would initially have an effect similar to an increased wing camber and would cause the lift to increase. Then, as the boundary layer air collected behind the spoiler, the wing lift would indeed be "spoiled" and the airplane would roll in the desired direction. The pilots considered the resulting lag in response very undesirable.

Another problem with the Shuttle was the time required for motion of the elevons. These elevons were unusually large and heavy and were operated by hydraulic motors. The time to make a large deflection was appreciable and to return the elevon to neutral before making a movement in the opposite direction also had to be considered.

A third characteristic of the Shuttle that was different from that of most airplanes was the large, downward force applied to the wing when the elevons were deflected up. This force caused the center of gravity of the vehicle, as well as the pilot's cockpit, to move downward following application of nose-up control, and considerable lag occurred before the pilot could feel that he was rising.

Apparently the designers of the Shuttle had not considered the importance of these effects. The 0.25-second lag in the response was caused by the repetition rate of calculations in the digital computer. An obvious correction that was easy to apply was to approximately double the repetition rate. This change was made before subsequent glide tests, together with warnings to the astronauts to apply the controls gently and to allow sufficient time for flight path changes required in landing. These measures were apparently successful in correcting the problem in subsequent glide tests, although one of the later landings showed some signs of an oscillation.

Flight Control Review Group

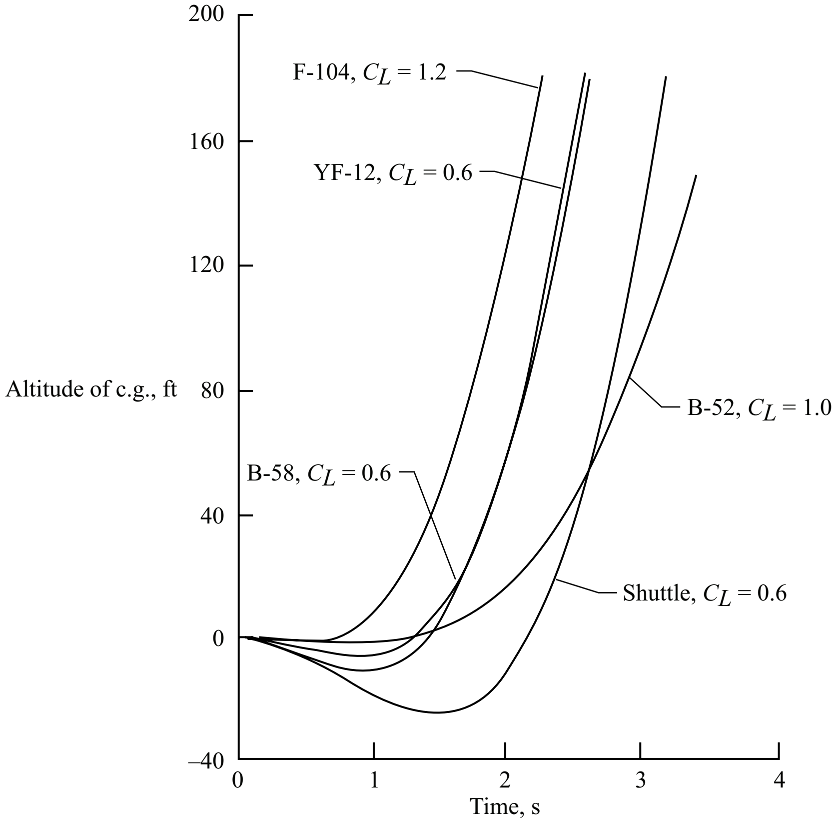
Because of the complication of the Shuttle Control System and the number of organizations involved in its design, the Flight Control Review Group was organized to coordinate the work of the various organizations. The Chairman of the group was Donald C. Cheatham, a former Navy pilot and a member of the Naval Reserve, who had previously worked for me at Langley in the Stability and Control Branch of the Flight Research Division and was now at the Johnson Space Center working on the Shuttle Control System. I was a regular member. Other members were S. Bray of the Ames Research Center, J. Weil of the Dryden Flight Research Center, R. G. Hoey of the Air Force Flight Test Center at Wright-Patterson Air Force Base, and about six members from various branches at the Johnson Space Center. Meetings were held approximately quarterly in the 1978–79 period.

The number of organizations working on the Shuttle Control System and the number of independent simulators set up to study its problems far exceeded the numbers devoted to any other aircraft. At least seven simulators designed to study the complete entry of the Shuttle from orbital flight to landing were in existence. These included one at Langley, one at the Dryden Flight Research Center, one at the Air Force Flight Test Center, and several at the facilities of Johnson and its contractors. In addition, the Johnson Space Center had the Shuttle Training Aircraft (STA), a Gulfstream with a modified control system to simulate as closely as possible the control characteristics of the Shuttle. The TIFS (Total In-Flight Simulator) airplane at Calspan was also used to study specialized piloting problems. Just getting the various simulators to agree on the same problem was a major task but was solved with the aid of the large number of engineers involved.

The large number of independent groups working on the flight control problem was an excellent method to catch and eliminate errors in the programming as well as to take advantage of the knowledge of the experts working for the various contractors. The method was also very expensive, but the support for the Shuttle at that time was sufficient to allow such expenditures. Later airplanes, both military and commercial, that made the first attempts at using digital fly-by-wire control systems without the aid of such intensive design efforts almost always encountered failures or crashes.

The problem of pilot-induced oscillations was a major concern of the Flight Control Group. The use of an increased repetition rate in the computers undoubtedly helped the situation. Still, the slow response of the Shuttle to longitudinal control inputs was a matter of concern. This lag in response was caused by the large inertia of the vehicle in pitch and the short moment arm between the elevons and the center of gravity. I later made an analysis that compared the longitudinal response of the Shuttle with that of several other large airplanes. The response time of the Shuttle was over twice as long as that of any other airplane (fig. 6.1). As shown by this figure, the download on the elevon at the start of a pull-up caused the center of gravity of the

FIGURE 6.1. Comparison of height response of several airplanes: F-104 and B-52—conventional aft tail, YF-12, B-58, and Shuttle—delta wing. Note that the Shuttle requires 2.15 s for the c.g. to return to its original altitude.



Shuttle to move down. Only after some time did the pitch angle increase the lift and cause the Shuttle to rise. A time lag of 2.15 seconds passed before the center of gravity had returned to its original altitude. This type of motion occurred on other airplanes with delta wings but was never as severe as on the Shuttle.

The sensation that the pilot experienced of accelerating downward when the control was applied for a nose-up response had been analyzed previously by some researchers and had been blamed as a cause of pilot-induced oscillations. I did not agree with this conclusion. I pointed out that several other airplanes, such as the Gee Bee racers, had been flown with the pilot sitting near the tail without encountering any difficulty. I felt that the pilot could become accustomed to his location in the airplane and could visualize the response of the nose of the airplane or of its attitude in pitch.

As usual, accurate predictions of the tendency to pilot-induced oscillations was difficult. R. G. Hoey of the Air Force Flight Test Center felt that despite instructions and practice to teach the astronauts to control the Shuttle with slow and deliberate movements, they would someday hit a disturbance that would require a fast control movement that would start a pilot-induced oscillation (PIO).

A device called the PIO Suppression Filter was devised by an engineer at the Johnson Space Center to make the Shuttle more resistant to PIO. This filter, inserted in the control system after the pilot's controller, sensed the frequency and amplitude of the motions of the control stick. If these motions became too large or frequent, the amount of motion of the elevons for a given controller deflection would be reduced, eventually reaching only one-third of its normal value. This device was tested on simulators and on the Shuttle Training Airplane. The astronauts commented that with the device in operation, it was very difficult to produce a pilot-induced oscillation. There was some objection that in a critical situation the pilot would be unable to use full control, but in such a situation the danger of a pilot-induced oscillation would be greatest. After extensive testing, the device was installed in the Shuttle control system.

The subsequent series of successful landings of the Shuttle can be attributed to the astronaut training program as well as to the suppression filter. Most of the success can probably be attributed to the training program. The suppression filter has, to my knowledge, never been forced to come into operation. Thus, the fears expressed by R. G. Hoey and other engineers were perhaps never realized in practice.

CHAPTER 7

Continuation of Research Following Decline of Space Program

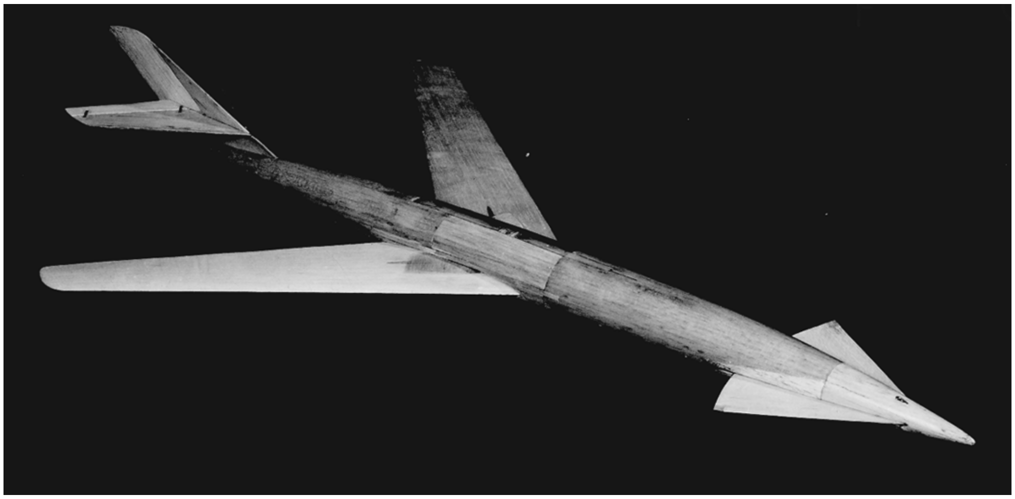
Following the completion of the design and construction of the Space Shuttle, administrators of the program had expected a continuation of active space activity involving manned space flight. Changes in the policies of the presidents who later came into office and the general lack of public interest in space developments resulted in a rapid decline in funding for this work. The Shuttle was a technical success, but the expense of its operation and the lack of a major program that required its use caused a reduction in the frequency of flights to less than four per year, instead of every two weeks, as first envisioned by some space enthusiasts. A temporary space station, the Skylab, using one of the Saturn tanks as its major component, stayed in operation for a while, but even this vehicle was allowed to fall back into the atmosphere and burn up rather than be sustained in orbit with a small expenditure of fuel.

I had continued to study some aeronautical problems during the space program and following the decline in space activity, I resumed work to clean up some problems that had not been completed before the space program, and made studies of a number of new aeronautical problems. The remainder of this volume gives brief accounts of a number of these problems.

Variable Sweep Wing Supersonic Transport

A variable sweep wing appears desirable for a supersonic airplane because for takeoff and landing an unswept wing of high aspect ratio provides lower takeoff and landing speed, whereas at supersonic speeds a highly swept wing has much less drag. The X-5 experimental airplane was perhaps the first that allowed us to study a variable sweep wing in flight.

A problem with the variable sweep wing is that as the wing is swept back, the aerodynamic center of the wing moves back with respect to the center of gravity of the airplane. To balance the airplane, a large download on the stabilizer is required in the swept condition, resulting in high drag. Alternatively, if the airplane is balanced in the swept condition, it becomes longitudinally unstable in the unswept condition. This problem is particularly critical on a supersonic airplane because the aerodynamic center of the wing also moves back by about 25 percent of the chord at



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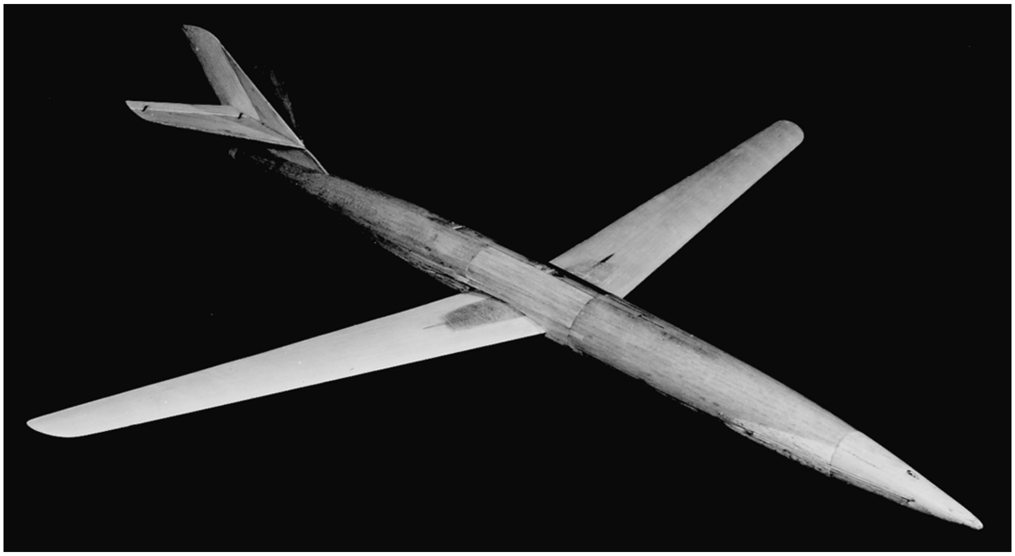

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FIGURE 7.1. **Figure 7.1. Glider model of supersonic transport with variable-sweep wing and retractable canards. Length, 30 in., span (swept) 15 in., span (unswept) 26 in.**

(a) Wing swept, canards extended.

(b) Wing unswept, canards retracted.

supersonic speed because of the effects of high Mach number.

To avoid this problem, the X-5 had the wing mounted on a movable cradle so it could be moved back or forward to suit the flight condition. Such a system, however, occupies an undesirably large volume in the fuselage.

In 1948, I made a model glider simulating a supersonic transport with a variable sweep wing, a picture of which is shown in figure 7.1. To keep the model in trim with the wing swept back, I added a retractable delta surface near the nose. Suitable hills to test model gliders are not common in the area near Hampton, Virginia, where I live, but I found a suitable small hill on the Yorktown Battlefield and spent an afternoon gliding the model. The model glided well either with the wing swept and the delta surface extended or with the wing unswept and the delta surface retracted. In this glider, the wing hinge was at the centerline of the fuselage.

Later, airplane companies discovered the idea that if the wing panels were hinged at a point some distance out on the wing, the sweeping action would move the inboard section of the wing farther forward. This principle was used on such airplanes as the Air Force F-111 and F-14 Navy fighters and on the B-1 Bomber. This system has been successful, but it poses complicated structural problems for the wing mounting and for fairings at the wing root.

Differential Maneuvering Simulator (DMS)

After graduating from MIT, I kept in touch with another MIT student named Herbert K. Weiss. He and I both came to Hampton, Virginia, I with the NACA and he with the Coast Artillery Board at Fort Monroe. He was a brilliant engineer and mathematician, and I often consulted him on my problems. Later he went to work at the Aberdeen Proving Ground in Maryland. His work was primarily in missile design, vulnerability of military aircraft, and air combat problems. On one occasion, probably in the forties after WWII, he asked me to come with some of my engineers to discuss mutual problems. During the meeting, he suggested that it would be possible to build an air combat simulator with two cockpits, two pilots, and displays showing the image and motion of the opponent's airplane to each pilot. I thought about this idea but concluded that the NACA at Langley did not then have the facilities or funds to build such a simulator and that the state of simulator design had probably not advanced far enough to undertake such a project.

In 1965, when the LOLA simulator was well underway, an engineer named Dr. John D. (Jay) Bird, who was a branch head in my division, made a similar suggestion for an air combat simulator. At this point, there was much interest in air combat as a result of the wars in Korea and Vietnam. Jay Bird was an ingenious research man. I concluded that with his enthusiasm it would be possible to undertake an air combat simulator. To gain experience, a simulator with just one projection sphere, called the Tactical Effectiveness Simulator (TES), was built. It had a projection sphere about 20 feet in diameter containing the pilot's cockpit and a projector to produce an image of the target airplane. The target airplane was controlled by a pilot in an external cockpit. All the motions were controlled by a large, digital simulation installation recently installed at Langley.

A contract for building the TES was won by the Rheem Corporation, a company better noted for its work on heaters and air conditioning systems, that had previously worked on simulators for the Air Force. I was surprised that they did an excellent job in designing the TES, and especially the detailed solution of the mathematics of the motions to be solved by the digital computer. The motions in three dimensions were solved by the use of quarternions, a system that avoided any gimbal lock in the projectors.

The TES was highly successful and was used for several research studies. The plans for the Differential Maneuvering Simulator (DMS) went ahead rapidly. A contract for its construction was let to the Northrop Corporation. An artist's drawing of the simulator is shown in figure 7.2-1. A later drawing that is perhaps more accurate in certain details is shown in figure 7.2-2.

The simulator had two large projection spheres, each 40 feet in diameter, on the inside of which was projected the target airplane image, view of the Earth and sky, and a Sun image. The pilot sat in a cockpit which did not rotate but was

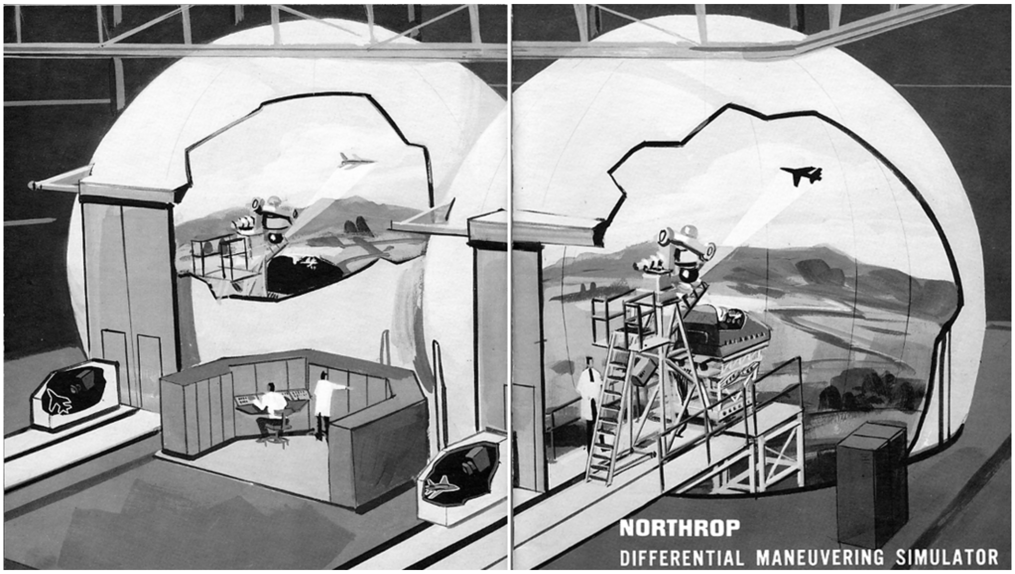


FIGURE 7.2-1. **Pictorial view of Differential Maneuvering Simulator (DMS). The large spheres are projection screens, and they contain the pilots' cockpits, projectors, and servomechanisms to move the projected displays. Each pilot sees a view of the opposing airplane.**

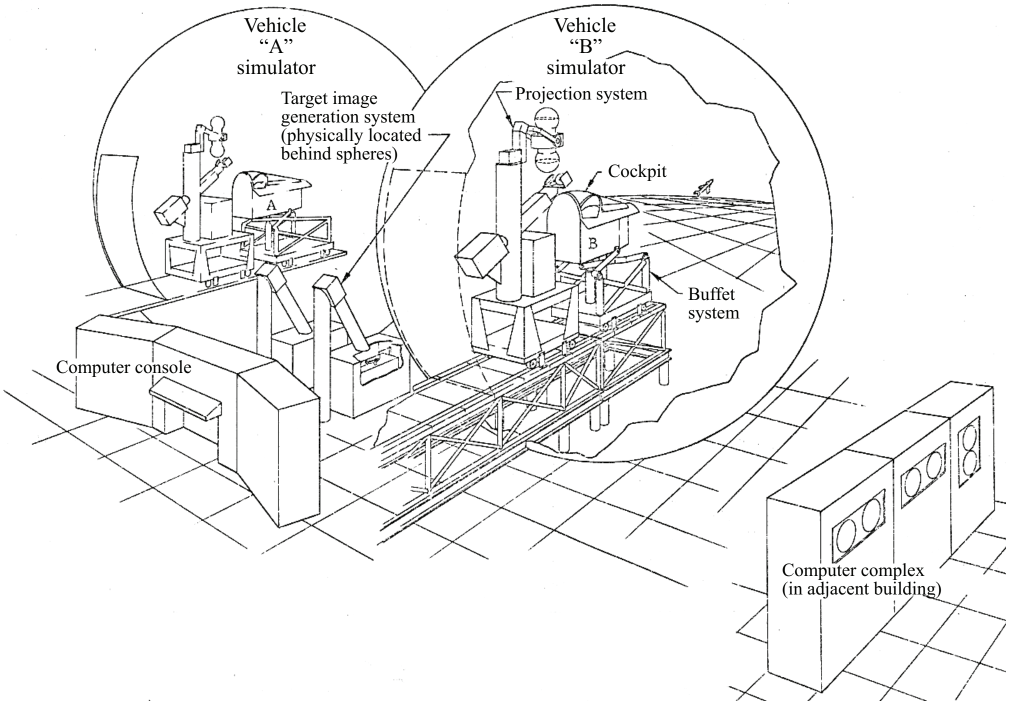


FIGURE 7.2-2. **More detailed sketch of Differential Maneuvering Simulator.**

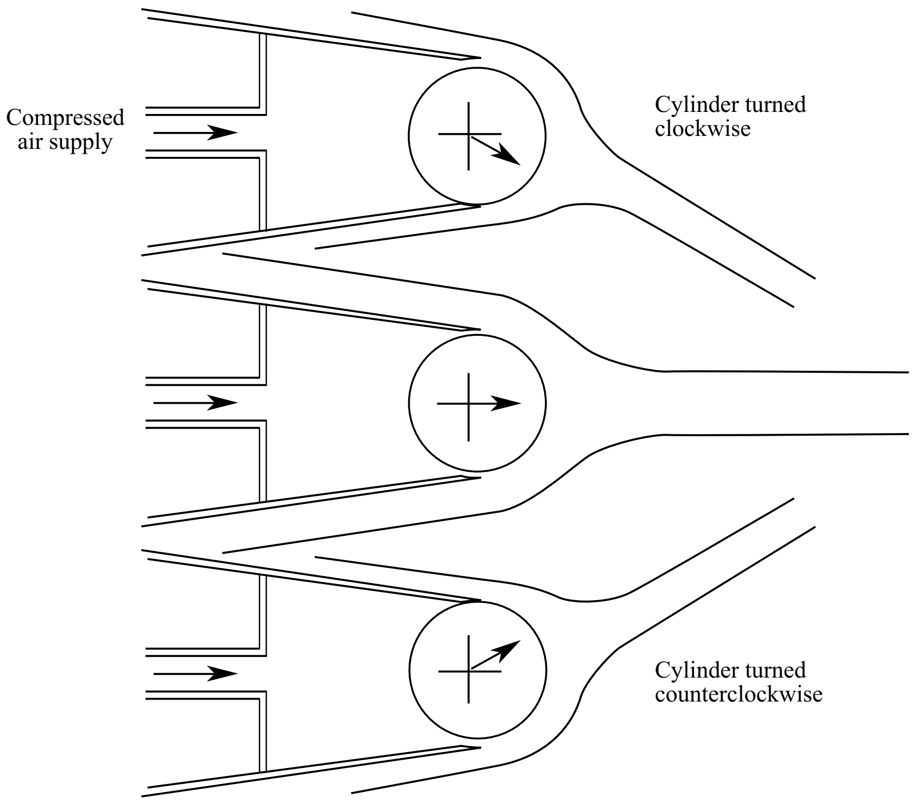
capable of oscillating to simulate buffeting. In addition, the pilot could wear a "g suit" that could be inflated to simulate g loads. The image of the target airplane was simulated by a detailed scale model about 15 inches long that was suspended in a box by a system of wires moved by servos to rotate the target airplane in roll, pitch, and yaw and was photographed by a camera to present a correct image of the target airplane in the projection sphere. Motion of the entire scene through an angle of 270° was produced by a large hydraulic servomechanism rotating the previously mentioned projectors to produce the image as seen from a maneuvering airplane. All these effects were duplicated in the other sphere so that either pilot could be the attacker as the combat progressed.

The simulator was completed and placed in operation in July 1969. By the time the simulator was finished, Jay Bird had lost interest in the project, and I was faced with selecting a group of engineers to run the programs. Continual help in operating the mechanics of the simulator and modifying the digital computer programs was provided by the Analysis and Computation Division (ACD). The simulator cost $5.5 million. The successful completion of the DMS was valuable to both the Air Force and Navy, who started construction of their own combat simulators. These simulators were more complicated, with such facilities as moving cockpits and the capability of displaying four airplanes. Both were finished after the DMS and cost many times as much.

I once thought that a simulator such as the DMS would be useful in developing a theory of air combat. Consultation with some noted mathematicians who had considered this problem, however, showed that it was an extremely difficult problem. Certainly it was beyond the capabilities of any of the engineers at Langley. The simulator was very useful, however, in developing empirical theories and in checking combat strategies that had been developed by military pilots. After some initial runs, pilots from the Air Force and Navy were invited to fly the simulator on a regular schedule. These pilots all considered the simulator runs extremely beneficial in improving their flying techniques. A series of rules for air combat developed by Al Meintel was useful in training these pilots.

Perhaps the most useful analytical study to come out of the simulation studies was a program developed by George H. Burgin, Lawrence J. Fogel, and J. Price Phelps of Decision Science, Inc., San Diego, California, working under contract to NASA (ref. 7.1). This program acted as an artificial pilot to serve as an opponent for a human pilot in the simulator. Later, Walter W. Hankins III, an engineer at the Langley Research Center, gave an AGARD talk on the program and wrote a Langley Working Paper summarizing the results of tests with the program (refs. 7.2 and 7.3). This program was so good that experienced military pilots were often beaten by it, and it was only after making a number of runs to uncover slight weaknesses or peculiarities in the program that they were able to beat it consistently. This type of program, of course, suggested its use as an automatic pilot to replace the human pilot in air combat. Such a program could save many pilots' lives in actual warfare. The pilots, however, always opposed the use of such a method. They had trained to be combat pilots and did not wish to leave the job to a machine, even if it might have saved their lives. Only in recent years when the use of unmanned vehicles for military missions has received increased attention has the use of automatic combat pilots been seriously studied.

The DMS is still in operation (2004) and has served many useful purposes in addition to studying air combat. For example, the use of a single sphere to study spinning characteristics of airplanes has allowed investigation of a much wider range of characteristics than could be done in flight tests. Since my retirement in 1979, I have been out of touch with simulation work, so I am not familiar with much of the work that has been done.



In practice, gaps would be only a few thousandths of an inch wide.

FIGURE 7.3-1. **Use of Coanda effect to deflect airstream at trailing edge of wing. Illustration shows effect of oscillating cylinder that closes one side of the opening or the other. A slightly smaller rotating cylinder could alternately close and open gaps at a high frequency, producing a rapidly oscillating airflow.**

Circulation Control

In my own experiments on gust alleviation, as well as in most other studies that have been made, flaps on the wings are moved up and down to offset upward and downward gusts, respectively. This method works well in an experimental study. On a practical airplane, flaps must also be used for generating high lift for landing. On transport airplanes, complex two-segment, double-slotted flaps are commonly used, which would complicate the problem of using flaps for gust alleviation. Possibly, the rear segment of the existing double segment flaps could be used for this purpose, but no aircraft company, to my knowledge, has attempted to study such an arrangement.

Another method to produce high lift, known mostly from wind-tunnel tests, is circulation control produced by a jet of air directed downward at the trailing edge. With this method, very high values of maximum lift, around six or seven, can be produced. Another technique, known as the Coanda effect, has been used for this purpose. In this method, a thin jet of air directed along a sharply curved surface at the trailing edge can deflect the jet downward and produce high values of maximum lift.

An engineer and assistant professor at MIT named Joseph Bicknell was at MIT when I was studying there. Later he published a paper describing a method for producing oscillating flow in a wind tunnel. In this method, illustrated in figure 7.3-1, he used a cylinder, with its axis slightly off center, rotating in an opening at the rear of a symmetrical airfoil that was mounted to span the wind tunnel ahead of the test section. Compressed air was blown through the rear of the airfoil. The air jet, influenced by the Coanda effect, followed first the upper and then the lower surface of the cylinder as a slot was opened at the upper and lower surface of the airfoil. The cylinder could be rotated with very little power and could produce an oscillating flow in the tunnel at very high frequencies.

With slight modifications, this method could be used as a control for gust alleviation. A source of compressed air, such as bleed air from a turbojet engine or a separate compressor, would be required to produce the air jet. The cylinder would not rotate continuously, but would oscillate about its axis just like the flap on a gust-alleviation system. This action would open a slot up or down to deflect the flow as required to produce a varying circulation about the airfoil.

A possible advantage of this system would be that the deflected flow could also be used as a landing flap, taking advantage of the very high values of lift coefficient that can be obtained with a jet at the trailing edge.

Plans were underway at the Langley Research Center to try this system on a Cessna 402B airplane, but the funding for the project was cancelled. A patent was taken out on the system in the names of Eric Stewart and myself. In view of the continual disinterest of the airplane companies in gust alleviation, I do not believe that this system is widely known.

Control Configured Vehicles (CCV)

The term CCV was introduced about 1969 by personnel of the Air Force Flight Dynamics Lab to describe the design of an airplane to obtain performance improvements by use of automatic control systems. The term was much publicized about that period, resulting in formation in a special committee at Langley and an intercenter symposium on the subject. In general, the term implied an airplane loaded with electronic equipment with rather poorly defined benefits in performance. At the request of the associate director of Langley, Lawrence K. Loftin, I wrote a memorandum describing my ideas on the benefits obtainable with this system.

The most apparent benefit was longitudinal stability augmentation, allowing the use of a smaller horizontal tail or a more rearward center-of-gravity location, thereby reducing drag. This benefit was so obvious that some airplanes were already being designed with this feature. Many other suggested benefits, however, required severe compromise of other essential features of the airplane and were rarely used.

After considerable thought, my overall conclusions were summarized in a pair of charts shown as figures 7.4-1 and 7.4-2. The main point was that the sizes of vertical and horizontal tails of airplanes and their associated control surfaces were based mainly on emergency flight conditions such as spin recovery, stall avoidance and recovery, and control with asymmetric power. The reductions in tail sizes offered by CCV were therefore not usable. As illustrated on the second chart, the control power required for increased stability was usually about one-third of that required for emergency flight conditions, and the control power for improved damping of

WHAT IS A CCV?

A CONTROL– CONFIGURED VEHICLE IS AN AIRPLANE FOR WHICH THE MOMENTS REQUIRED FOR TRIM THROUGHOUT THE NORMAL AND EMERGENCY FLIGHT ENVELOPE ARE MINIMIZED

FIGURE 7.4-1. **Control-configured vehicle (CCV) chart.**

CONTROL REQUIRED FOR DAMPING AND STABILTY

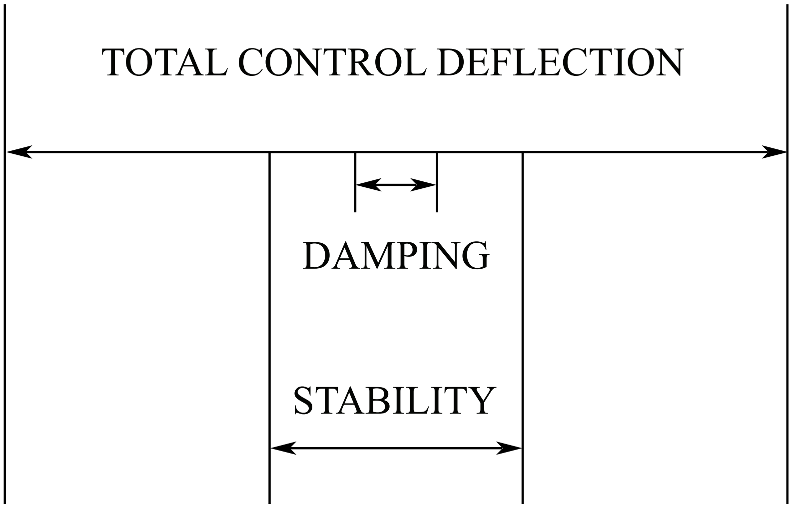


FIGURE 7.4-2. **Damping and stability control chart.**

oscillations was only one-ninth of that required for emergency conditions. The benefits of CCV could be increased only by designing the airplane to reduce the control power required for emergency conditions. Methods for doing this were developed, but in the ensuing years, such methods have not been generally adopted.

Propulsive Effects Due to Flight Through Turbulence

In a wind-tunnel study made in 1922, R. Katzmayr showed that an airfoil subject to periodic vertical motions of air in a wind tunnel would experience a propulsive force. A more practical problem is to determine the effect of flight through random turbulence. The French aerodynamicist Breguet made an early study of this problem. His analysis was correct in principle, but at that time the mathematical tools for analyzing turbulence, such as power spectral density and the spectra of atmospheric turbulence, were not available. As a result, quantitative results were not obtained. This problem, though it is considered to be of some practical importance, had apparently never been solved. I made an analysis of this problem and presented the results in a note in the Journal of Aircraft (ref. 7.4). In this analysis, the spectrum of atmospheric turbulence, which expresses the amplitude of the gust intensity as a function of frequency, is represented by the familiar Dryden spectrum, and the response time of the airplane in responding to a vertical gust is placed in frequency-response form. These two quantities are then combined to give a formula for the thrust coefficient.

The results show that a lightly loaded aircraft, such as a soaring glider, flying at a high lift coefficient, experiences an increase in lift and a forward tilt of the lift vector in upward gusts and a decrease in lift and a rearward tilt of the lift vector in downward gusts. A net increase in thrust is produced, but such a lightly loaded aircraft responds vertically to the gust very quickly, reducing the effects of change in angle of attack to a very short duration. A heavily loaded aircraft, such as a fighter or transport, is flying fast and in the same turbulence experiences much smaller changes in angle of attack, but the heavy airplane moves vertically much more slowly, so the duration of the change in lift is longer. The result is that both types of airplanes experience about the same thrust effect, which is very small even in severe turbulence. Calculated results for typical examples show that a change in thrust coefficient in severe turbulence would be about 0.003 to 0.005.

Decoupled Controls

Though the early Wright Brothers' airplanes used control systems that were not consistent with the pilot's normal reactions, the early pioneers Paulhan and Bleriot originated a system in which fore and aft motion of the controller controlled the pitch control surface, side-to-side motion of the controller controlled the roll control surfaces, and rudder pedal motion controlled the yaw control surface. A throttle was used to control engine power or thrust. This system has been used almost universally since then. With the development of automatic electronic control systems, however, designers realized that other types of airplane response could be obtained. One possibility was called decoupled controls, in which a given controller would control just one motion of the airplane. For example, one controller might control only pitch angle, another might control airspeed, and another lateral velocity. Such controls were sometimes thought to be easier for novice pilots to learn, and in other cases, advantages were claimed for gunnery or missile accuracy. It should be realized that more than one airplane control surface would be used for each of the decoupled motions.

Numerous analyses of controls with various types of decoupling are available in the literature, and flight tests of some arrangements have been tried, in particular to study gunnery or missile accuracy. No great advantage has been found for using a system different from the standard control system. In fact, some rather serious objections have been raised by pilots to these systems. For example, pilots use different control techniques for upward or downward control when flying near the ground. They object to any control which applies down elevator when flying near the ground, even if the decoupled system might not result in a dangerous flight path. Decoupled systems are usually unsuitable for emergency conditions such as spin recovery, where pilots have learned techniques appropriate to a given airplane that might apply controls opposite from those produced by a decoupled system. Most work on decoupled controls appears to have disappeared, although automatic systems may be used for specific flight regimes on individual airplanes, such as stall-limiting devices or Mach number control.

Soaring Gliders

The Wright Brothers experimented with man-carrying gliders for three years before they made their successful flight in a powered airplane. Later, Orville Wright, in 1911, returned to Kitty Hawk to try a glider with a tail-aft location which, by that time, had been adopted by most other aviation pioneers. Orville, slope soaring in the wind on the large dune near the site of their first flight, made a soaring flight of 9 minutes 45 seconds, which stood as a world record for soaring endurance until after WWI.

After the war, construction of airplanes in Germany was forbidden. Many of the German manufacturers went in for soaring glider activities, both for sport and experimentation. The design and performance of soaring gliders improved rapidly. In this country, the center of glider activity became concentrated in Elmira, New York, where a level area at the top of a steep hill provided a good site for launching. This area became known as Harris Hill, where a Lieutenant Harris had been killed when his car overturned while towing a glider.

The performance of soaring gliders depended to a great extent on aerodynamic efficiency. As a result, many aeronautical engineers did analytical studies in this field. There had been some glider activity at the Langley Research Center before I arrived in 1940, but very little was done after that. When the first radio-control systems for models were developed in the early 40s, I made some of the first radio-controlled soaring glider models, which served to keep up my interest in this field. Later, about 1955, radio-controlled gliders became a very popular branch of model aviation. About 1968, Oran Nicks, who was a soaring enthusiast, became Deputy Director of Langley. He organized an annual technical conference on soaring, held at MIT, and always requested that I try to prepare a paper to be given at the conference.

The first paper that I presented was entitled Analysis of Effect of Asymmetric Loading on Sailplane Performance in Circling Flight (ref. 7.5).

The second paper that I presented was entitled *Gyroscopic Moments on a Glider in Turning Flight* (ref. 7.6). Soaring gliders, because of their high aspect ratio, have a strong tendency to roll to a larger bank angle when in a turn. Opposite aileron and rudder controls must be applied to offset this tendency. This effect was discovered by the Wright Brothers and resulted in their development of the coupled rudder and wing warping lateral control system that was a major factor in their success in making controllable flights.

The gyroscopic moment acting on a glider results from the tendency of all rotating bodies to align themselves with the plane of rotation. This moment tends to reduce the tendency to roll to a larger angle of bank. This effect is not very

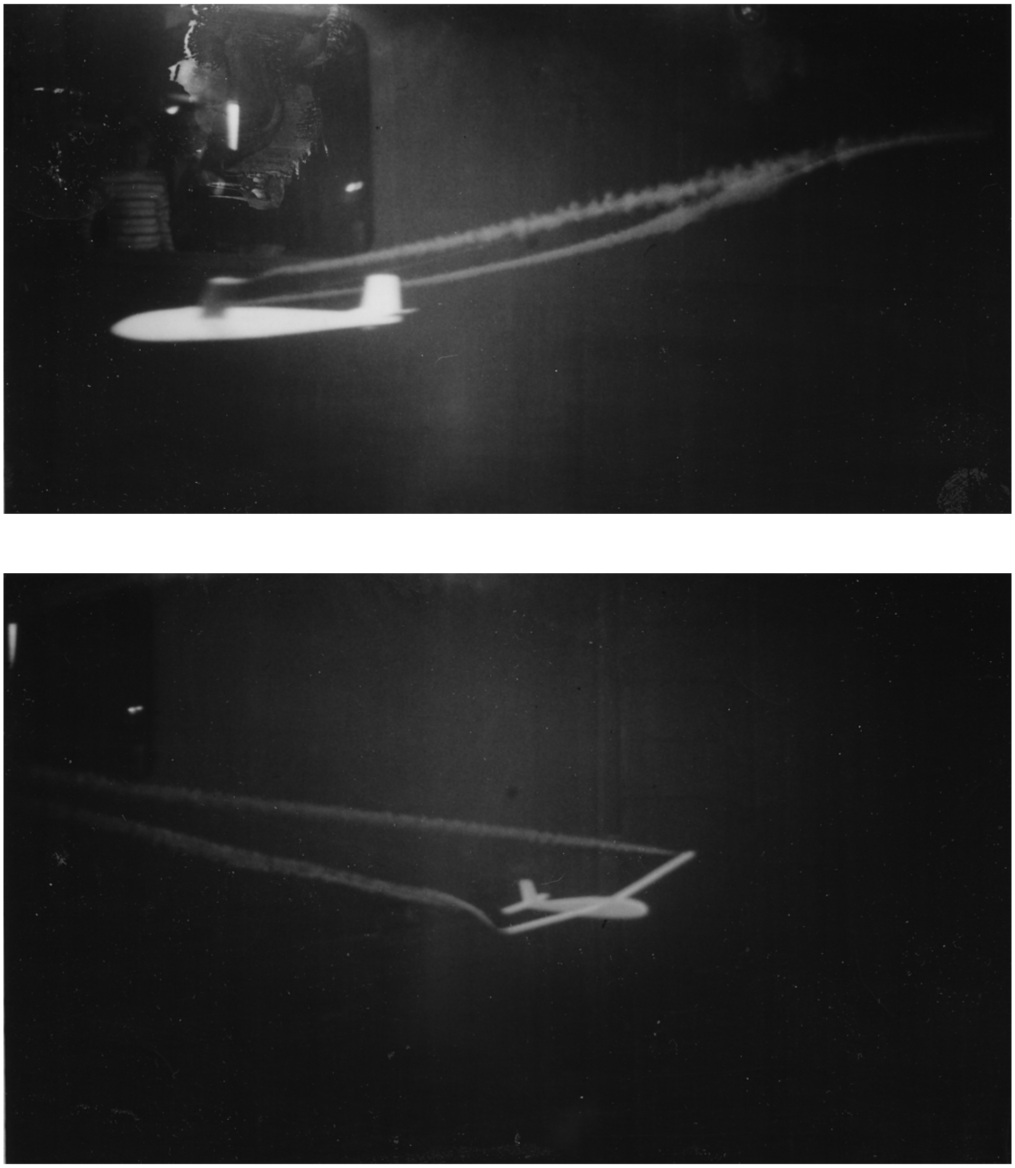


FIGURE 7.5. Heavily weighted glider (40-in. wingspan) gliding slowly (about 3 ft/s) underwater in large tank. Fluorescent dye is emitted from wing tips. When glider made complete circles, trails from previous circle were always well above glider.

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large, however. In a typical example, the gyroscopic rolling moment was only 13 percent of the aerodynamic rolling moment.

The third paper that I presented was intended to study the effect of the trailing vortices shed from the wing tips of a glider in circling flight on the forces acting on the glider in subsequent turns. To study this effect, I made a flow visualization study. At that time, a large water tank, 30 feet in diameter and 30 feet deep, was being used at Langley to study zero gravity effects on the performance of astronauts in space. I made a model glider of 40-inch span out of solid mahogany and weighted with about 4 lb of lead in the fuselage. The glider was launched underwater with a rubber catapult and would slowly glide in circles in the tank at a speed of about 3 to 4 feet per second (fig. 7.5). A fluorescent dye carried in a tank in the model was expelled through nozzles on the wing tips. This dye left a clear trace of the trailing vortices from the model as the model slowly descended in the tank. The conclusion was that the model was always well below the vortex shed in a previous circle. As a result, the vortices would be expected to have a negligible effect on the efficiency of the model. These results were demonstrated to the attendees at the symposium with movies.

As a matter of interest, I took the lead weights out of the model, equipped it with radio control, and tried flying it in air. The model weighed about a pound, a rather heavy wing loading for a model. Launched from a dune into a strong wind of about 35 miles per hour, it would glide with a speed exceeding 40 miles per hour. I claimed that I had made the first glider that flew both in air and underwater, like a duck. The ratio of densities of water and air is about 1000.

Another fact that I learned from these experiments was that the buoyancy of the parts of the model that are lighter than water must be taken into account in trimming the model for underwater flight. For example, the tail surfaces made of mahogany were buoyant underwater, allowing the model to be in trim with a center of gravity that would be too far rearward to be stable in air. As a result, I had to make a lighter set of tail surfaces for flight in air. Buoyancy effects also exist on the structure of a conventional airplane, but the effects are so small compared to the weight of the airplane that they are rarely mentioned.

Complementary Filters

When considering the subject of aircraft control systems, a filter is considered to be a device that modifies the pilot's input to improve the response of the airplane. In many cases, the filter is designed so that its presence is not felt at the pilot's controller but is simply recognized as part of the airplane dynamics. For example, a longitudinally unstable airplane may be made longitudinally stable from the pilot's standpoint.

The simplest types of filters may produce undesired side effects. For example, a filter that improves the longitudinal stability may introduce an undesired structural oscillation. To avoid such undesired effects, a device known as a Complementary Filter has been introduced. This system has relatively simple design, and does not require complicated mathematics for its analysis, yet has many applications in aeronautical problems.

During the Apollo and Shuttle programs, mathematicians introduced filters to obtain an optimal flight path or other objective based on some criterion such as minimum time of flight, with provision for continually correcting the path based on periodic observations of measured references such as line of sight to navigational stars and planets. The best-known filter for this purpose is called the Kalman-Bucy Filter. The design of such a filter required advanced mathematics that had never been covered in my college courses. Some of the bright younger engineers had studied this subject in college and were able to follow the development. Because of the importance of this subject, some prominent mathematicians were hired to give courses to the personnel of the Langley Dynamics and Control groups, including Kalman and Bucy themselves, as well as other engineers prominent in this field. Because of my lack of background, I was never able to get anything but a general understanding of such subjects. In later years, companies were formed that specialized in these analyses and apparently made a good living by assisting the aerospace companies in work that required this type of knowledge.

Most airplane control system applications do not require the complication of the Kalman-Bucy filter. In these cases, the Complementary Filter provides a readily understandable device that can solve many of the practical problems encountered. I was first introduced to this filter by John F. Garren, Jr., an engineer in the Helicopter Branch. He found that the conventional helicopter control system that incorporated angle-of-attack sensors to improve the longitudinal stability and rate sensors to improve the damping of oscillations caused an undesirable amplification of the one-per-revolution vibration of the rotors. The Complementary Filter provided an analog model of the helicopter response. Then the output of a simplified response model was passed through a high-pass filter, the output of which was added to that of a low-pass filter on the rate gyro. In this way the high-frequency vibrations sensed by the rate gyro were eliminated, whereas other high-frequency motions as well as the low-frequency response of the helicopter were allowed to operate the controls. The device proved quite effective in reducing the one-per-revolution vibration of the helicopter. This study is reported in reference 7.7.

I later made a brief study of the use of a Complementary Filter in reducing the structural vibrations of a rocket during launch, often called the pogo effect.

Turbulence Problems

In my previous book, *Journey in Aeronautical Research*, I devoted a large amount of space to my studies of response of airplanes to gusts and the design of systems to produce a smoother ride in turbulence. These systems have been called “gust alleviation systems,” although it was the response to gusts rather than the gusts themselves that was alleviated. In most of this work, the gust disturbances were assumed to be one-dimensional; that is, the gust velocities varied along the flight path but were assumed constant across the wing span at any instant. This type of analysis was useful because most of the response of airplanes comes from long wavelength gusts that do not usually have much variation across the span.

After finishing this work, which included flight tests of a gust-alleviation system on a Beech Model 18 (Navy C-45) transport, I went to other subjects, but I always kept in mind some related problems that had never been solved. One of these problems was the effect of two-dimensional turbulence, that is, gusts with variations across the span as well as along the flight path. To make such studies mathematically tractable, the assumption of isotropic turbulence is usually made. This type of turbulence shows the same statistical properties along any flight path that penetrates the turbulent region. Experimental studies have shown that turbulence of this type frequently occurs in the atmosphere.

A Langley engineer in the Structural Dynamics Branch, Franklin W. Diederich, made some valuable turbulence studies in which he calculated the statistical properties of the wing response of various planforms to flight through isotropic turbulence. (The turbulence might be called axisymmetric because the airplane was assumed to be in straight horizontal flight.) Although the mathematics involved was complex, I considered Diederich's approach easier to use than studies made previously by other investigators.

I knew from previous studies that when an airplane hit an abrupt gust, the response was not instantaneous. There was a lag in response called the unsteady lift effect, which had been investigated by several noted aerodynamicists. If a high-aspect ratio wing hits a step-shaped gust, it travels five or six chord lengths before the lift builds up to a steady value. As the aspect ratio is decreased, the lag is reduced until at an aspect ratio of 3, the response is practically instantaneous. In the gust-alleviation systems that have been tried, the gusts are usually measured by a vane located ahead of the nose of the airplane. If an airplane flies through a turbulent region with high-frequency disturbances, the vane will respond accurately to the disturbance, but because of the unsteady lift effect, the average response of the wing lift over a period of time will be reduced. A plot of the amplitude of the lift as a function of gust frequency will therefore show a decrease as a function of frequency.

Other investigators studying two-dimensional turbulence had concluded that the response to high- frequency disturbances may be reduced because of variations of gust velocity across the wing span. In this case, the vane measures the disturbances along the centerline of the airplane, but the gust velocity at other points along the wing will be different, and if the amplitude of response is averaged across the span, the value will be less than the amplitude at the centerline. This problem was called the "spanwise averaging effect." These phenomena had been studied by different groups of engineers and no effort had been made to compare the relative values of the decrease in the amplitude of response due to unsteady lift with the decrease due to spanwise averaging. In fact, in most studies, one or the other of these two effects was investigated and no account was taken of the other one. I therefore undertook a study of this problem.

Without my knowledge, an engineer in the Structural Dynamics Branch named Kermit G. (Cary) Pratt had been studying this same problem and published a report before mine was completed (ref. 7.8). It turned out that both analyses reached similar conclusions, but my analysis was more complete from the standpoint of logic and mathematics. I therefore completed my report, which was published as a NASA Technical Memorandum (ref. 7.9).

In this presentation, no attempt is made to give the mathematical details. Several interesting points were encountered in the analysis. The decrease in amplitude of response with increasing gust frequency resulting from the spanwise averaging effect obtained from Diederich's report was given by a complex mathematical expression involving Bessel functions. A complete plot of this curve showed that a slight decrease in amplitude of lift occurred as the frequency approached zero at very low frequencies. This slight change in lift was unexpected. I believe it may be caused by the rare occurrence of an occasional isolated gust of large amplitude, which when averaged over the long period of time between such gusts, shows up as a decrease in amplitude of very low frequency.

Another interesting discovery I made was that Diederich's complex expression could be approximated very accurately by a very simple expression containing just two terms and no Bessel functions, which applied at reduced frequencies greater than about 4.

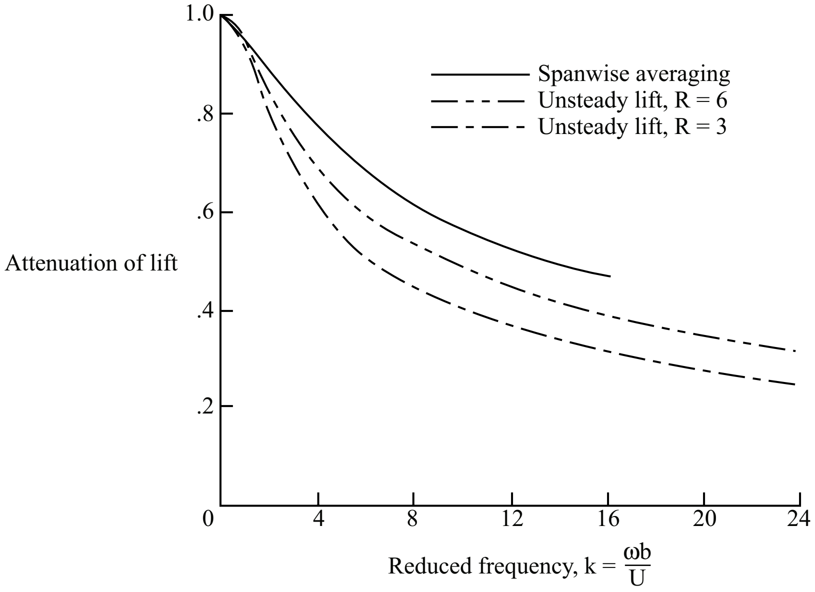

FIGURE 7.6. Comparison of attenuation of lift due to spanwise averaging on an elliptical wing with that due to unsteady lift effects for wings of aspect ratios 3 and 6.

The expressions for the reduction in amplitude of the response caused by the unsteady lift effect was calculated for aspect ratios of 3 and 6 by methods described in reference 7.10. A comparison of these results with those due to the spanwise averaging effect are given in figure 7.6. In this figure, ω = frequency, rad/s, b = wing span, ft, and U = airspeed, ft/s. These curves are remarkably similar considering that they came from completely different theories. In practice, the values of the two effects should be multiplied together to get the total attenuation.

CHAPTER 8

Some Nonaerodynamic Studies

Some Studies of Fireplaces

At one time, I was approached by William J. Michael, then the Chief Scientist of the Langley Research Center, to try to find some work for the Space Radiation Effects Laboratory (SREL). This laboratory was under the combined direction of Langley and the College of William and Mary and had been used for studying radiation effects on materials for use in space. The laboratory was also associated with the Virginia Associated Research Center (VARC), a headquarters building for SREL. The SREL building contained a cyclotron to produce the radiation. These laboratories were located in the northern end of Newport News in an area separate from the main NASA center and now called Oyster Point. Unfortunately, funding for the activities at SREL had been cut off with the decline of the space program. A proposal had been made to develop the SREL into a nuclear research center. This program was in the early development stages and required approval by a consortium of Southern universities (SURA), as well as approval by Congress and other groups. The problem was to keep VARC alive with some small contracts until the large nuclear research program could be put into operation.

I knew that VARC was a very useful facility with a fine technical library and several permanent employees, including mechanics. I found that the Department of Energy had a fund to provide small grants to individuals or research groups to improve the efficiency of fireplaces. The interest in fireplaces developed because a serious oil shortage existed in this period, and many people were trying to burn firewood to heat their houses. Although I knew very little about heat transfer or thermodynamics, I submitted a proposal to the Department of Energy to study the efficiency of fireplaces.

The funding was sufficient to give employment to three summer students, two men and one woman, who were very capable and benefited from the exposure to a technical problem. I went to work at VARC two afternoons a week to supervise the work.

I studied the works of Count Rumford and Benjamin Franklin, both of whom were noted for improving the fireplaces of their day. Count Rumford's main contribution was to change the shape of the fireplace to a more narrow, tapered shape to direct more heat radiation into the room. I concluded that Benjamin Franklin knew more about the problems of fireplaces than most people living today. He developed a fireplace in which the firebox was airtight. Air from the room was heated in a closed container in the firebox and returned to the room, while air from the outside, coming under the house, burned the logs and went up the chimney. All of the circulation was done by convection as there were no electrically driven blowers in those days. The fireplace greatly improved efficiency because cold air from the outside was not sucked into the room. Franklin, always a genius, ran the Post Office in Philadelphia and installed one of his fireplaces there, while selling piles of iron castings to the customers to build their own fireplaces. These fireplaces were short-lived, however, because sealing of the joints between the castings, done with a mixture of mud and straw, soon developed leaks and allowed smoke to escape into the room. Franklin's fireplaces using welded steel joints would be excellent for use today and would not make the distracting noise of the blowers used on present day fireplace inserts.

Franklin's knowledge of fireplaces soon became well-known, and many manufacturers called their fireplaces Franklin stoves, though they did not incorporate the advantages of Franklin's invention.

When I started work on fireplaces, some manufacturers of fireplace inserts had already produced inserts with glass windows that allowed a view of the fire. These glass windows quickly became dirty with soot and pitch, which was very difficult to clean off. I conceived the idea of using a fine-mesh stainless steel screen for the window, which would allow some view of the fire, but which would be kept clean by the small flow of air through the screen. I found that fine-mesh stainless screen was produced in large quantities for the paper industry, and as a result was not as expensive as I had imagined. One project that the group undertook was to build a small wind tunnel, with a test section about 4 by 8 inches, in which the pressure drop through screens could be measured. Although screens were used in some NASA wind tunnels to smooth the airflow, no tests of the pressure drop through screens as fine-meshed as desired had ever been made.

Another project was to write a computer program for predicting the efficiency of fireplaces. This program accounted for the heat transfer to the air in the chimney and to the room, but accounting for the radiation between all the heated parts was probably not done very accurately. The young lady who worked with the group proved very skillful at computer programming.

Finally, two commercially available fireplace inserts were obtained to measure their efficiency. Special instruments were made to measure the airflow up the chimney and the pressures and temperatures in various parts of the system. One of the inserts, which used a set of stainless steel tubes behind the fire to take in air at floor level and blow it into the room above the fire proved to be about 60 percent efficient. The other insert, which had a metal shell completely surrounding the fireplace to heat air from the room, was found to be about 80 percent efficient.

I soon found that numerous manufacturers wanted to improve fireplaces at the same time that I did, and many of them came out with efficient units. The air above New Hampshire became so polluted by wood smoke that fireplace inserts there were required to have a catalytic converter, similar to those used in automobile exhausts, to remove the carbon monoxide and nitric oxide from the combustion products.

I wrote a report on the studies that was submitted to the Department of Energy. Interest in the project soon declined when the fuel shortage ended, and the report was never published. The VARC and SREL stayed alive, however, and subsequently developed into the Jefferson Laboratory, one of the main centers in the country for nuclear research.

Control System for a Small Hydrofoil Boat

My first boss, Dr. Robert R. Gilruth, who was head of the Stability and Control Branch of the Flight Research Division when I came on duty, had shown an interest in hydrofoil boats when he first came to Langley. He had already built a small hydrofoil sailboat and later built a small outboard motor-propelled run-about and a larger sailboat, both lifted from the water on hydrofoils. These boats used surface-piercing foils, on which the tips of the foils project above the water when cruising. This feature provides lateral stability, much like the dihedral on an airplane wing. Longitudinal stability was provided by a submerged rear foil in conjunction with the correct center of gravity. Later Gilruth became a consultant to the Grumman Company on the design of some large hydrofoil boats for use by the Navy in WWII. All this work was done as a hobby, outside of working hours. Naturally, I also became interested in boats of unusual design, although I did not build any full-scale hydrofoil boats. I did build a small rubber-motor propelled hydrofoil boat with submerged foils, which was stabilized laterally by a gyroscope linked to the front foil. The gyro wheel was spun up by pulling a string wound around the shaft. This boat conclusively proved to be stable in its short runs.

In later years, hydrofoil passenger boats were made in several European countries. The Boeing Company made one in this country. These boats, which used submerged foils for greater efficiency, required complex gyroscopic systems and electronic autopilots to provide stability.

I had a conventional 15-foot outboard boat that I sailed in Hampton Roads. I realized that a hydrofoil boat of the same size and power would sail much faster and more smoothly. I did not, however, have the time or facilities to build a full-scale boat. I did consider how a simple mechanical autopilot might be built to stabilize a hydrofoil boat of this size. I made an analysis of such a system at work with John D. Shaughnessy. The analysis was done by using a high-speed NASA digital computer. I felt that this work was appropriate in my position because I was head of the Stability and Control Division and because the AIAA at that time published a Journal of Hydronautics along with its other technical journals. A report on this work was later published in the AIAA Journal of Hydronautics (ref. 8.1).

The primary sensor for the control system was visualized as a long, streamlined stick pivoted at the hull to allow it to swing fore and aft. The drag force of the water on the stick would increase with the depth of submersion. This stick was linked to a trailing-edge flap on the front foil to produce more downward flap deflection when the drag on the stick increased. Some damping of the slick motion results from the variation of drag on the stick with stick motion fore and aft, and from the variation of flap hinge moment with rate of change of flap deflection. It was not known whether these sources of drag would be sufficient to damp oscillations of the stick. To further increase the damping, a bob-weight was linked to the stick so that as the stick moved rearward, the bobweight would move up. The bobweight is a weight mounted on a pivoted arm and restrained by a spring. The bobweight senses vertical acceleration at the location of its pivot. Locating the bobweight near the front of the boat would presumably produce some lead in the acceleration of the weight, causing the flap to be deflected up by an upward pitching or by an upward vertical motion of the hull.

The equations of motion of the systems are too complex to present in this report but may be found in reference 8.1. The stability of the system was studied by root locus plots, by transient responses to disturbances, and by frequency-response plots of the various variables in the system. The root locus studies show that adequate stability of all modes of motion may be obtained by a system of the type analyzed. The predominant low-frequency mode of the boat, however, appears to have a frequency too low to interact with the bobweight system on the flap. The original premise that the bobweight would contribute to the damping of the low-frequency modes of the boat, therefore, was found to be incorrect.

The system studied with a relatively low value of the variation of restoring force with vertical displacement provides excellent attenuation of the vertical motions of the boat due to head waves through a large range of frequencies, whereas in stern waves the motion is attenuated to a value less than the wave amplitude at frequencies above 0.8 Hz but amplified at frequencies below this value. Stabilization of the boat in stern waves of low frequencies would probably require a more sophisticated control system involving an attitude gyro. In restricted bodies of water, long wavelengths are probably rare.

CHAPTER 9

Concluding Remarks

This volume has contained most of the work that I conducted at Langley during the space program. After this work declined, I worked on many different projects, mainly to provide interesting subjects for my employees and to solve some problems that arose in work before the space program. I have presented four chapters that contain twelve examples of projects or research conducted during this period. These examples represent only a small fraction of the number of different studies that I made. A bibliography of my reports that were published during this period is presented at the end of this volume.

In 1979, after 39 years of service, I found that much of my time as Chief of the Stability and Control Division was taken up by administrative matters. My background and much of my earlier work had been devoted to research. Most of the other division chiefs at Langley had for some time been administrators, leaving the conduct of their research to the personnel assigned to their divisions. I found that I did not have enough time to do personal research and to adequately perform the administrative duties. I discussed this problem with Oran Nicks, an assistant Director of the Langley Research Center. He advised me to resign and accept the position of Distinguished Research Associate (DRA). In this position, somewhat like that of a Professor Emeritus in a college, I would be free to use the facilities at Langley and to do research as I desired. My retirement annuity would be about two-thirds of my maximum salary as a federal employee. I considered this offer as a good opportunity, and started what turned out to be a long career as a DRA.

One objective of my career as a DRA was to conduct a wind-tunnel test. Many of the engineers at Langley were engaged in operating the numerous wind tunnels at this center. I had never had this opportunity because when I came on duty, I was assigned to the Flight Research Division, in which the duty of a flight engineer was to analyze recorded data obtained by the test pilots in flights of full-scale airplanes. The closest I came to a wind-tunnel test was shortly after I came on duty, when I was assigned to bundle up in my overcoat and climb into the cockpit of a Fairchild F-22 airplane mounted in the Full-Scale Tunnel. The airplane had been equipped with a bob-weight in the control system. My duty was to apply impulses to the control stick to measure the damping of the elevator motion as affected by the bob-weight.

After I started work as a DRA, the Shuttle had started to fly, and it was soon found that the very high landing speed resulted in damage to tires and overheating of brakes. I believed that the landing speed could be lowered by installing a canard surface near the nose of the Shuttle that would help to lift the nose so that the elevons at the rear of the delta wing could be trimmed down and further increase the lift prior to touchdown. I did not contemplate that the test would be difficult, but I soon found that all the major wind tunnels had schedules that were booked up for at least three years. I was able to get some test time in the old 12-Foot Tunnel, a wind tunnel that was built in the thirties and had originally been used to create an airstream in which freely flying small models would be tested. Later, the tunnel was equipped with balances and used in the conventional manner. The model that I acquired from Rockwell was an old balsa-wood Shuttle flutter model, about 5 feet long that had been partially crushed. I rebuilt the model and ran tests on a number of canard designs. I found that the flow in the tunnel was variable along its length so that the results were not quantitative but could be used for comparative purposes. The tests took over three years to make and analyze. As a result, I became more impressed by the work required in wind-tunnel testing. On completion of the study, the results were presented in a talk at the Johnson Flight Research Center. The canard surfaces were never used on the Shuttle because of the difficulty in changing the design of the Shuttle, and because some reduction in landing speed could be obtained by rearward positioning of the center of gravity combined with some downward deflection of the elevons. This arrangement reduced the longitudinal stability, but the stability could be restored by adjusting the electronic control system.

A second interest that I had after becoming a DRA was to learn more about airfoils. I had studied airfoil theory at MIT some 40 years earlier, but many advances had been made in this field with which I was not familiar. A notable theory had been developed by Theodore Theodorsen at Langley to calculate the pressure distribution on an airfoil given its contour. Later, Lighthill in England, Trockenbrot in Germany, and others had solved the inverse problem of determining the contour required to produce a given pressure distribution. Robert T. Jones and Eastman Jacobs at Langley also had developed an iterative technique to solve this problem, but as far as I know it was never published.

Other groups in this country and at Langley developed boundary-layer theory that allowed the development of the boundary layer and the resulting friction drag to be calculated. These investigators at Langley were in a different group from the airfoil theorists. I had suggested to Oran Nicks that the theories be combined to allow the boundary-layer distribution on an airfoil to be calculated given the airfoil shape. Before this analysis was done at Langley, however, Richard Eppler in Germany had developed a computer program for a theory that combined these parts of the problem. Dan M. Somers, an engineer at the Low-Turbulence Tunnel at Langley arranged for Eppler to come to Langley and explain his program to him and to the people in his group. (refs. 9.1 and 9.2). I consulted with Somers to learn the details of this program, and I made many runs to study airfoils of different types. Later, I modified the program to allow the calculation of both friction drag and pressure drag on an airfoil as a function of angle of attack. This work was presented in a Society of Automotive Engineers (SAE) meeting at Anaheim, California in October, 1988. 1 believe that experts at the large airplane companies had developed similar programs, but the results had not been published previously.

In most wind-tunnel tests to determine drag of airfoils, the total drag (pressure drag plus profile or skin-friction, drag) is measured by means of a rake survey. Until recent years, very few attempts were made to separate the two sources of drag or to determine the distribution of these components of drag over the surface of the airfoil. The only study of this type with which I was familiar was a remarkable analytical study by H. B. Squire and A. D. Young made in 1937 in England (ref. 9.3). Without modern computing facilities, calculations of this type were extremely tedious. My paper showed these characteristics for three airfoils, each at three values of Reynolds number. The effects of uniform suction through the surface of the airfoil was also studied, and the possibility of negative pressure drag, or thrust, on the airfoil was demonstrated.

At that time, there was interest in very high-altitude, unmanned aircraft to make possible long endurance for surveillance or for communication purposes. I made recommendations for airplane airfoils of this type. The personnel at the companies doing this work contacted me because they had heard of my experience with model airplanes. They learned of my theoretical studies later.

I was also interested in learning about the theory of propellers. At this time, most airplanes of interest were jet-propelled, and very little work on propellers was being done at Langley. Earlier, in the thirties and forties, there was much interest in this work, and Theodorsen had published a propeller theory that was based on accurate aerodynamic theory. Even as early as 1919, Betz and Prandtl in Germany had developed a theory based on some good approximations to the flow characteristics, and Fred Weick at Langley in the thirties built the Propeller Research Tunnel and made empirical studies of full-scale propellers. By this time, propellers good enough for all practical purposes could be designed. Incorporating the fine points of aerodynamic theory did not change the efficiency more than 2 or 3 percent.

A paper summarizing practically all that was known about propeller design was written by H. Glauert in England and published in Volume IV of the Durand Series in 1934. By the seventies, few people paid any attention to this work, but E. E. Larrabee at MIT studied the article, made a few important corrections, and programmed the theory on a pocket calculator. Later, I used this theory as an example of programming on the HP 9820 and later on the HP 9830 computers. Designers of commercial airplanes at that time could purchase propellers from companies that specialized in this work, but homebuilders often wished to design and build their own propellers. I received over 50 requests from all around the world for copies of my programs.

The preceding paragraphs are examples of my work as a DRA. Many other studies, probably of lesser importance, were also conducted, some of which are mentioned in the bibliography. In addition I started writing a history of my work at Langley. This book, entitled *Journey in Aeronautical Research* was published in November 1998, as the NASA publication *Monographs in Aerospace History, Number 12* (ref. 1.1). This document covered the work to the start of the space program in 1958. The present volume contains my work during the space program and later work to 2004. This work is necessarily abbreviated because of the large number of subjects that were encountered in the work after the space program.

The long duration of my work at the Langley Research Center has involved many changes. Perhaps the greatest change, from the engineering standpoint, is the development and widespread use of computers. These marvelous devices allow analyses to be made in seconds that previously required days or weeks. Most of the older techniques are now obsolete because they are incorporated in computer programs. Younger engineers who grew up with these methods produce results with computers with a facility that is beyond my abilities. The changing research emphasis, as well as the new body of personnel involved, makes this a good time to end this volume.

Appendix

Abbreviated Chronology of Space Flight Accomplishments

List of major accomplishments and total launch attempts in each year through 1962, showing number of failures. Major milestones in U.S. manned space program through first lunar landing and return.

Year	Date	Name	Notes
1957			
	10/04/57	Sputnik I	First artificial satellite (Russian)
	11/03/57	Sputnik II	Second artificial satellite (Russian), carried dog, Laika
1958			
	2/01/58	Explorer I, Jupiter C	First successful U.S. artificial satellite, discovered Van Allen belts
	3/26/58	Explorer III, Jupiter C	Radiation, micrometeoroid data
	22 total launch attempts, Russian and U.S.; 7 achieved orbit		
1959			
	U.S. made launch attempts of several artificial satellites, including Vanguard, Discoverer and Transit. Russia launched Luna II		
	25 total launch attempts, 13 achieved orbit		
1960			
	U.S. launched Atlas Able (Pioneer 1960)		
	40 total launch attempts, 20 achieved orbit		
1961			
	4/12/61	Vostok 1	Orbit, Yagi Gogarin (Russian, first man in space)
	5/05/61	Mercury, Redstone 3	Suborbital Flight, Alan Shepard (first U.S. man in space)

9/13/61	Mercury, Atlas D	One orbit, unmanned
10/29/61	Mercury, Atlas 5	Two orbits, Chimpanzee Enos

57 total launch attempts, 39 achieved orbit

1962

2/20/62	Mercury, Atlas 6	Three orbits, John Glenn
5/24/62	Mercury, Atlas 7	Three orbits, Scott Carpenter
10/03/62	Mercury, Atlas 8	Three orbits, Wally Schirra

81 total launch attempts, 74 achieved orbit

1963, 1964: at this point, total launch attempts are omitted, skip to major U.S. space programs in 1965

1965

7/28/65	Ranger 7	First successful close-up photos of Moon
12/04/65	Gemini 7	Borman and Lovell
12/15/65	Gemini 6	Schirra, Stafford; rendezvous with Gemini 7

1966

3/16/66	Gemini 8 target Atlas Agena D	
3/16/66	Gemini 8	Armstrong, Scott docked with Gemini 8 target; stuck thruster caused emergency.
7/05/66	Apollo 2, Saturn 1B	AS-203, unmanned test
7/18/66	Gemini 10, Titan II	Young, Collins; first EVA, docked with Gemini 10 target; raised its apogee to 755 km
8/10/66	Lunar Orbiter I Atlas Agena D	Returned lunar photos, crashed on Moon
11/08/66	Gemini 12, Titan II	Lovell, Aldrin docked with target, successful EVA tests
11/08/66	Atlas Agena D	Target

1967

2/05/67	Lunar Orbiter 3 Atlas Agena D	Returned lunar photos, crashed on Moon

1968

4/04/68	Apollo 6, Saturn V	Command module test
10/11/68	Apollo 7, Saturn 1B	Schirra, Cunningham, Eisele, Earth orbital test of Apollo
12/21/68	Apollo 8, Saturn V	Borman, Lovell, Anders; first circumlunar mission

1969

3/3/69	Apollo 8, LM Saturn V	McDivitt, Scott, Schweickert; Earth orbital test.
5/18/69	Apollo 10, LM, Saturn V	Stafford, Young, Cerman; LM undocking and docking in Lunar orbit
7/16/69	Apollo 11, LM, Saturn V	Armstrong, Aldrin, Collins; first lunar landing and return

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Phillips in 2002 at the age of 84. From 1979 to 2005, he was a
Distinguished Research Associate at the NASA Langley Research Center.

About the Author

William Hewitt Phillips was born in Port Sunlight, Merseyside, England, and came to the United States with his parents at the age of 2. He was educated in the Belmont, Massachusetts public schools and studied aeronautical engineering at MIT where he obtained his S.B. degree in 1939 and his S.M. degree in 1940. His entire professional career has been spent with the NACA, later NASA, at Langley Research Center in Hampton, Virginia. Langley Research Center is the original government center for aeronautical research in the United States. On entering duty in July 1940, he was assigned to the Flight Research Division. He specialized in the study of flying qualities and stability and control of airplanes. His duties included studies to improve the flying qualities of many World War II military airplanes. After the war, he was involved in research on the development of jet-powered fighter airplanes, supersonic airplanes, stability augmentation and its effect on human pilot control, automatic control, gust alleviation, and aeroelastic effects. His previous book, Journey in Aeronautical Research, ends with the advent of the nation's space program. After the start of the space program, he became Chief of the Space Mechanics Division and supervised 80 to 90 people in the areas of space rendezvous, navigation, and lunar landing. As a part of his responsibility to the space program, this division developed simulators for the Gemini and Apollo programs. He developed the Lunar Landing Facility that was used for training astronauts in landing on the Moon. His work also included consultation and analysis in the development of the Space Shuttle. Later work included supervising studies of effects of turbulence and of application of control theory and contributing to the development of the Differential Maneuvering Simulator, a facility used for air combat studies. He retired from government service in February 1979 but continued until 2004 in the position of Distinguished Research Associate, during which time he performed original research on solar-powered aircraft, propellers, airfoil design, and wind-tunnel studies of canard surfaces use for the Space Shuttle. He served as a consultant on studies of flight dynamics and control. He has received numerous awards throughout his career, including the IAS Lawrence Sperry Award for aeronautics in 1944 and the President's Award for Distinguished Federal Civilian Service in 1979. In 2005, at the age of 86, he continues to design and fly model airplanes and still has a keen interest in aeronautics.

Phillips married Viola Ohler in 1947 when she was head of the Editorial Office at Langley. They had three children, Frederick H., Robert O., and Alice B. Phillips. All are now married. Frederick, whose wife is Joanne, is a financial consultant. Robert and wife Cheryl have three children: Tyler, 25; Ross, 22; and Jocelyn, 20. Robert works at The Volpe Center in Cambridge, Massachusetts. Alice and husband Thomas Check have three children: Candace, 18; Nolan, 16; and Aubree, 14. Alice formerly worked for robotics firms and is now a homemaker.

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