PILOTED ORBITAL REENTRY LIFTING BODY CANDIDATES TESTED AT EDWARDS AIR FORCE BASE

By Robert W. Kempel

Senior Flight Research Engineer, (Ret.)
Independent Consulting Engineer

The Union of Soviet Socialist Republics, USSR, launched the world's first artificial satellite on October 4, 1957, Sputnik-1 — a larger than beach ball sized polished sphere — this was a shock that chagrined scientists and engineers in the United States. The U.S. had attempted to be the first to launch a satellite using the Vanguard rocket in late 1956, but failed and this turned out to generate an international political and public relations debacle. We were, after all, involved in the space race. Scientists and engineers don't often get very excited, disturbed, or disrupted from their dedicated work by these kinds of situations, but they do when the politicians suddenly become willing to fund their favorite projects. This was exactly the situation that was brewing in the United States toward the late mid-decade of the 20th century.

We can get an idea of where the National Advisory Committee for Aeronautics (NACA — the forerunner of NASA) and the USAF were directing their efforts by reviewing the paper titles and list of attendees of their 1958 *NACA Conference on High-speed Aerodynamics: A Compilation of Papers Presented* (ref. 1). This was a large conference, at Ames Aeronautical Laboratory (later Research Center or ARC), that included general subjects concerning *manned and piloted* reentry satellites and vehicles into planetary atmospheres. They presented various topics related to:

1) The ballistic capsule type characterized by use of drag alone for entry deceleration;
2) The semi-ballistic vehicle that uses lift to reduce peak deceleration and a degree of aerodynamic flight-path and ground track control; and
3) The so called winged vehicles, those that are capable of more efficient aerodynamic flight.

Shown below are examples of the three types of entry vehicles hypothesized. The capsule, a segmented nose cone — lifting body, and a delta winged vehicle (Fig. 1).

Although very successful, the ballistic capsule parachute recovery approach has left much to be desired, especially if it involved open-sea recoveries. In addition, it had little to no terminal area maneuvering capability and fewer orbit selections for a specific landing site. If the semi-ballistic and winged configurations were to be controlled by a pilot, the question posed was: How would such a program be initiated, launched and expanded to provide adequate information to support a firm basis for an actual orbital program? Who would be the advocates?

Fig. 2 is presented to show the Earth's surface area, footprint, available for potential lifting body landing sites for a given orbit compared with that of ballistic capsules — in this case a Mercury or Gemini capsule off the east coast of Florida (ref. 2). It can be seen for the capsules, that only a small geographical area is available for splash-down recovery while a lifting body has the entire western United States available for a horizontal landing.

With the political and technical pressure of an unofficial space-race in full swing, it seemed that toward the early 1960s the time was right for even bolder steps to be taken and for more elegant ways to return from orbital flight, and the stage seemed to be perfectly set for NASA Flight Research Center (FRC) to institute such programs to investigate a variation of the lifting body and winged vehicles for piloted controlled terminal area research programs at Edwards AFB. It would be risky and somebody in higher authority would have to put his or her reputation or more on the line and become the advocate. To bolster the argument for a 'new-start' figure 2 was presented

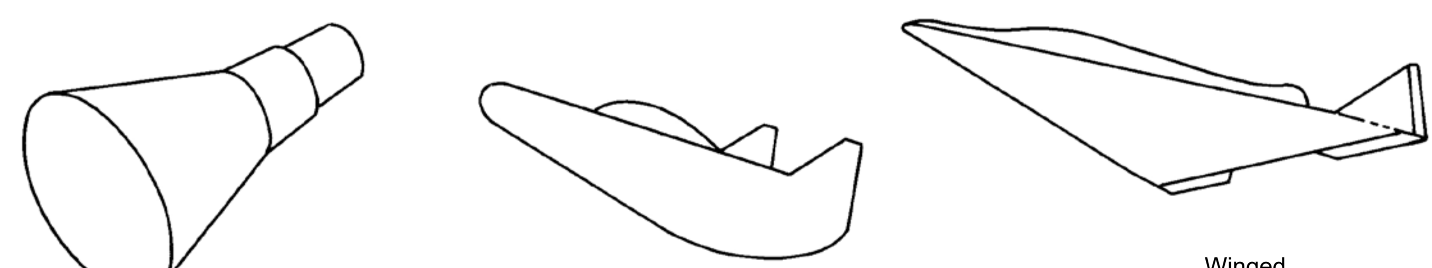


Figure 1. – Of the three possible planetary reentry configurations only the semi-ballistic, with aerodynamic surfaces and intermediate L/D, or winged type, with higher L/D could offer the possibility of a horizontal landing. See page 98 of ref. 1 and ref. 2.

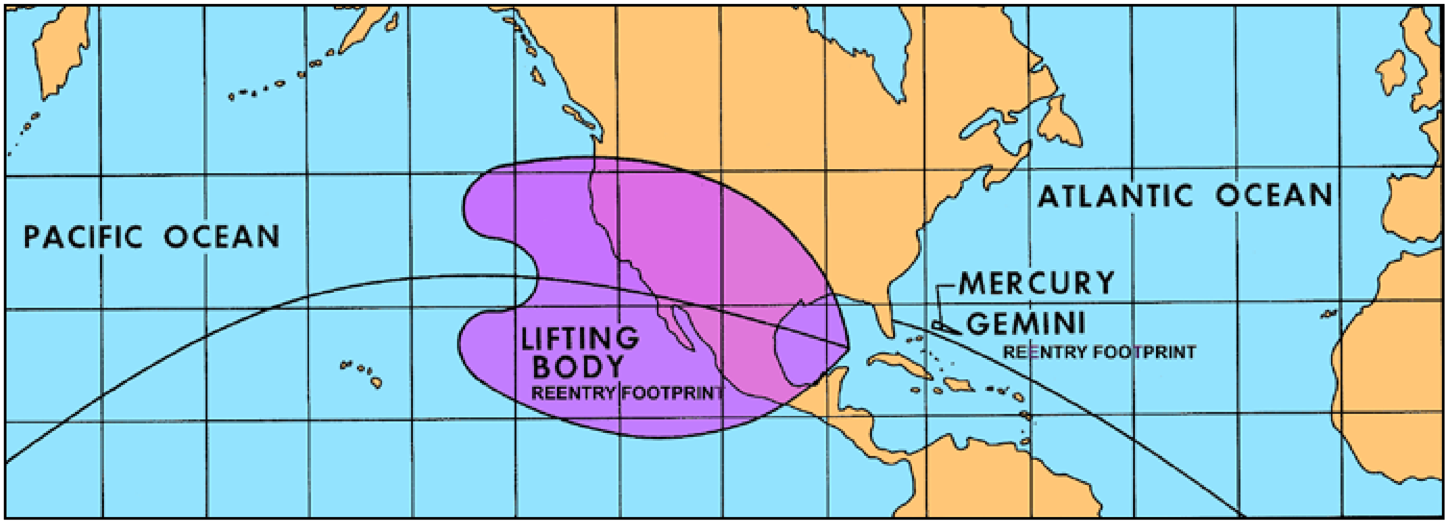


Figure 2. – Comparison of ballistic capsule reentry footprint compared with that for a lifting body. (Ref.2)

illustrating the vast capability of lifting bodies compared with capsule type vehicles.

With the surface area defined by the Lifting Body in figure 2, who could deny the potential of such a vehicle? There is, however, a caveat and that is that orbital speed is about 24,000-ft/sec. (16,364 mph ≈ 26,335-km/hr) and if this is the initial speed of an orbital reentry vehicle then: at these speeds any vehicle or mass entering the Earth's atmosphere has a high level of kinetic energy that will require some form of energy transfer or transformation. In FRC's advocacy we did not address this problem because in our proposed lifting body program we expected to be flying at less than 2,000-ft/sec. (610-m/sec.) and aerodynamic heating would not be a concern.

NASA FRC launches a piloted Lifting Body program

It was about 1962 when Dale Reed, a freethinking FRC engineer, began flying a balsa model of an ARC's M-2 lifting body around FRC (ref.3). I can recall seeing him doing this and he stopped to show me how stable the vehicle was and told me a little about the configuration. Like he always did, Dale captured my attention. One characteristic of this little model was when Dale launched his model it always came 'wings-level' and touched down on its wheels — it had natural spiral stability, and I was intrigued.

Dale soon took his advocacy for instituting a lifting body program at FRC to highly regarded X-15 test pilot Milt Thompson (ref 4) and with his help they soon convinced FRC Director Paul Bikle that a lightweight piloted program needed to be pursued.

The implementation of a small in-house lightweight piloted lifting body effort was launched, to the credit of Paul Bikle who was much admired and respected within FRC. For a more complete story of the genesis of lifting bodies see references 3 and 4. A ranking Research Division Director could not agree and deemed the concept too outlandish and outrageous even for a research organization and resigned his position. See reference 5, for additional information concerning lifting bodies and the history of FRC.

So, in early 1962, the M2-F1 lightweight flight vehicle was being fabricated somewhat under cover; it might even be said that this was a covert project. Made of steel-tubing with plywood skin, the vehicle weighed about 1,200 pounds (544kg) and began its flight-test in early 1963 staggering into the air by being towed across Rogers Dry Lake behind a high-powered convertible with a racing engine. As the M2-F1 progressed and matured it evolved to being towed into the air by FRC's R4D, navy version of the DC-3, as was the case in figure 3. Several pilots, including AFFTC's Chuck Yeager, were checked-out in the M2-F1 and its last flight was in August 1964.

Joint NASA AFFTC Lifting Body program

With the successes of the M2-F1 advertised within other NASA Centers, NASA Headquarters took notice and got involved in the lifting body program as well, and it took on the status of legitimacy. NASA Langley Research Center (LRC) even had a lifting body, the delta-planform horizontal-

Figure 3. The airborne lightweight M2-F1 being towed by an R4D (C-47) over Edwards main base, circa 1964. (Photo from NASA FRC, No. EC 64-404)

landing -10 or HL-10, configuration they wanted to evaluate in flight-test. In June 1964, FRC awarded a contract to Northrop Norair in Hawthorne, Calif., for the design and fabrication of two research lifting body vehicles — the M2-F2/HL-10; these were the heavyweights. The aerodynamic configurations were NASA's, but all the engineering design, inner workings, and fabrication was Northrop's. In August 1964 Paul Bikle proposed, and other NASA managers agreed, that these vehicles should be powered by existing XLR-11 liquid fueled rocket engines giving them supersonic capability. These engines had been used in the early X-15 flights prior to the installation of the much larger XLR-99 rocket. The history of the XLR-11 goes back to the X-1.

In early 1965 the Air Force contracted, with Martin, for their SV-5P ('P' for piloted) to be fabricated into the piloted X-24A that would join NASA's two heavyweight vehicles. It is certainly true and correct that *nothing* like the piloted lifting body program had been attempted before, especially with a trio of unique vehicles. About this time period, late 1964 — 1967 the Air Force began its SV-5D hypersonic atmospheric maneuvering unmanned sub-scale program to validate its cross-range maneuvering capability. The following is a short summary of the SV-5D, aka X-23A program.

The Air Force launched the PRIME program in late 1964

The Precision Recovery Including Maneuvering Entry, or PRIME, hypersonic program was instituted using Martin Marietta's SV-5 lifting body shape and, in this case, a sub-scale vehicle designated the X-23A. This vehicle was 6.75-feet (3-m) long and weighed 890 pounds with thermal shielding. The X-23A was designed for hypersonic near orbital speeds to a maximum Mach number ≤ 25 using titanium alloy and other heat-resistant alloys. Nitrogen reaction control thrusters provided roll, pitch and yaw control. The objective of this program was to execute a hypersonic cross-range maneuver, from an entry point of a simulated orbital track, using banked turns and to determine the maximum L/D ratio; thus validating the higher Mach range spectrum and that a lifting body could indeed successfully alter its flight-path during reentry as well as using its lift for deceleration.

Four of these vehicles (ref. 6) were built; these were to be launched individually from the Western Test Range, Vandenberg AFB, Calif., atop an Atlas booster. The first two flights were successful, but were lost in the recovery phase. The most successful of the vehicles was the third and last flight on April 19, 1967. On this flight the vehicle performed a cross-range maneuver of 710 miles (1,143km) or only about 77% of the footprint illustrated on figure 2. The maximum hypersonic L/D was a bit disappointing at only 1.1 or 73% of what was hoped for.

In flight the X-23A vehicles performed so well during their hypersonic maneuvering that the USAF found no reason to launch the fourth. The flight condition of interest was at a Mach number of 22 at 218,000 feet at a speed of 14,940 mph. The third X-23A vehicle is now on display at the National Museum of the Air Force in Dayton, Ohio.

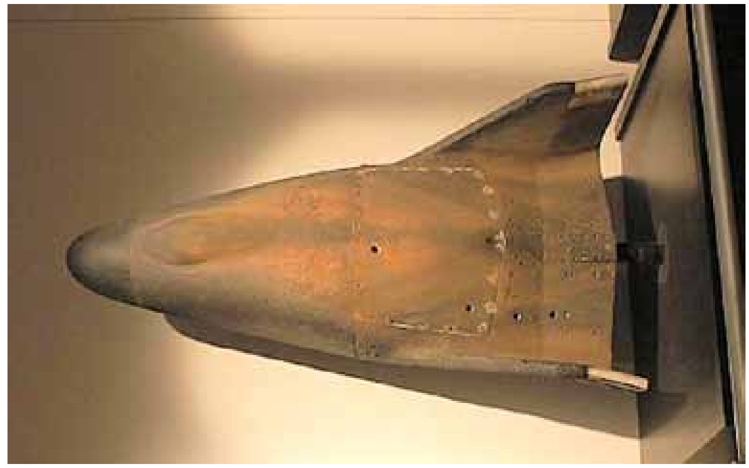


Figure 4. X-23A No. 3 flight vehicle currently on display at the National Museum of the USAF. (USAF photo)

Meanwhile back at Edwards

In 1965 we were formalizing the piloted lifting body objectives. On April 19, 1965, by mutual agreement, the Joint NASA/USAF, FRC/AFFTC Lifting Body Flight Test Committee was established with the objectives of: Building piloted rocket-boosted, aerodynamically controlled, lifting bodies capable of supersonic flight to demonstrate that such vehicles could execute precision unpowered approach and horizontal landings from 100,000 feet (30,480m) and Mach numbers ≤ 2, and verify the predicted supersonic, transonic and subsonic performance and handling qualities of the designs. The first flight of two of three candidate supersonic vehicles happened in 1966 with promising but mixed results.

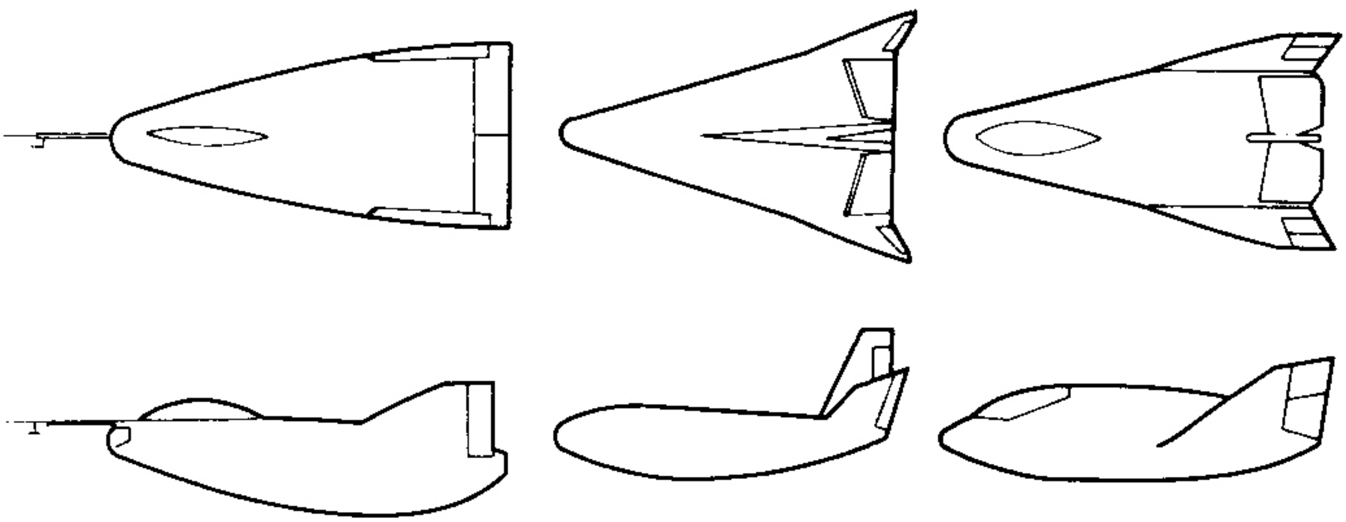
Highlights and summary of the piloted program

A summary of the three heavyweight lifting bodies is presented and some of their accomplishments, the method of launching these vehicles, the unusual method of unpowered approach and landing, the successes of the program and the lifting body legacy for future lifting spacecraft. I will include a short presentation of my personal responsibilities on the HL-10 flight-test program and later as a member of the M2-F2/F3 test team.

In the detailed discussion, I will only make references will be limited to the X-24A and later the winged X-24B due to my lack of personal involvement with these vehicles. The two program heavyweight vehicles that are the focus of this paper will be the M2-F2 and later modified to the M2-F3, and HL-10. Mention of the X-24A and 'B' will be made only in passing because I was not a member of either of these teams. The reader is directed to reference 5 for X-24A and 'B' discussions.

The M2-F2 and the HL-10 each suffered major controllability problems during their glide-flight phase that required identification, in-depth analysis and solution before either could resume flight-test. In chronological order — on its first flight the HL-10 suffered from intermittent lateral controllability ranging from lethargic to almost no

Figure 5. The trio of lifting bodies on Rogers Dry Lake, the X-24A (SV-5P), M2-F3, and HL-10. (Photo from NASA, No. EC69-2358)



	M2-F2	HL-10	X-24A
BODY LENGTH	22-ft (6.7-m)	22-ft (6.7-m)	24.5-ft (7.5-m)
SPAN	9.5-ft (2.9-m)	14.5-ft (4.4-m)	13.5-ft (4.1-m)
MAX SUBSONIC L/D	3.1 at $\alpha \approx 2.5°$	3.6 at $\alpha \approx 15°$	4.2 at $\alpha \approx 8°$

Figure 6, Drawing of the three lifting bodies in planform and side-view, reference 7.

roll control at all. This problem demanded a solution. The M2-F2 displayed a pilot induced oscillation (PIO) problem three times in early flights and simulation at low and negative angle-of-attack (AOA); described by test pilots, as *nibbling* but its source remained a mystery — and unfortunately was not *investigated* in-depth until the vehicle landed gear-up on Rogers Dry Lake following a severe pilot disorientating PIO. This problem should have been cured but now it, too, demanded a solution. How these problems were identified, analyzed and cured will be discussed. Each of these problems can be referred to as 'showstoppers' because each one brought their flight-test program to an immediate halt — each one will be reviewed in order of occurrence. Once these problems were overcome each of the vehicles went on to successfully achieve their individual program objectives and contributed to the extant lifting body database.

The significance of this program

At the end of June 1970, after the rebuilt M2-F3 had successfully flown, the HL-10 had all but completed its flight-test program and the X-24A had flown its initial powered flights; We at FRC and AFFTC presented a conference concerning our results to date. This conference was titled: *Flight Test Results Pertaining to the Space Shuttlecraft* (ref. 7). This conference was directed toward NASA Manned Spacecraft Center (later Johnson Space Center in 1973), NASA Headquarters, and selected Air Force personnel — people who would decide if a full-scale program would be undertaken or not.

RESULTS AND DISCUSSION

Three lifting bodies

The vehicles involved in our Joint program are presented in figure 5. From left to right, they are: the X-24A (aka SV-5P), the M2-F3 (modified with a fixed center fin from the rebuilt M2-F2) and the HL-10. All lifting body launches were from a B-52 mother ship from about 45,000 feet. Their landing weight was between 6,000 lb and 6,400 pounds. Each was powered by a refurbished 4-chambered liquid fueled XLR-11 rocket engine. Each chamber produced 2,120 lb of thrust and each chamber could function independent of the others — thus modulating thrust and burn time as required meeting the flight-plan objectives. Liquid oxygen was the oxidizer and water alcohol was the fuel. Propellant weight was about 3,500 pounds. Propellants were delivered to the engine by a decomposed H_2O_2 driven turbo-pump. Four-chamber burn time was around 100 seconds.

Following the results and discussion, first-hand flight data analysis, observations, hypotheses, the preflight and in-flight problems, or 'showstoppers', solutions and conclusions for the HL-10 and M2-F2/F3 will be presented. The discussion of both the X-24A and X-24B programs will be limited.

Configuration change in flight

Each vehicle was required to deflect a combination of aft flaps and tail surfaces into what we called the 'transonic-configuration.' The transonic-configuration was obtained in each of the vehicles by deflecting or biasing out or up a combination of aft surfaces to maintain *shuttlecock* stability through transonic and supersonic speeds. If a flight was planned to transonic and supersonic speed then launch was in the transonic-configuration. For glide flights the vehicles were launched in their subsonic-configuration with aft flaps and tail surfaces streamlined to achieve the minimum subsonic drag and maximum L/D. Typical glide flight time from launch to touchdown was about four minutes — at 'blistering' descent rates.

Our *modus operandi*

For all glide flight launches, using the M2-F2 as an example as shown in fig. 7, the B-52 launch aircraft was flown to an altitude of about 45,000 ft at about 170 knots IAS and 369 knots-true (Mach ≈ 0.64) on a northerly heading above the east side of Rogers Dry Lake, whose field elevation of was about 2,300 feet MSL.

Powered flights launch points would be moved as needed depending on the mission requirements. As an example, the HL-10 first supersonic flight in early 1970 was launched at 45,000 feet at 185 knots IAS on a southwesterly heading about 28 miles northeast of Edwards. After rocket burnout the flight profile was to come over Edwards, at about 40,000 ft. from the north and make an overhead 360° approach over main base. Pilot's space positioning adjustments were made based on calls from ground control — a very important facet on all flights. Lifting body operational ground rules were never violated.

Typically, the lifting bodies intersected a ground track profile at a point above the Lake at the runway's intersection (at about 35,000 feet) and changed from the transonic to subsonic configuration. Adjustments to the flight-path as required were made to the low-key point at about 20,000 ft to merge with the glide flight profile. All landings were on runway 18.

Final approach to touchdown

In the glide flight profile, the final approach was established at about 4,000 feet AGL. The final approach was flown at 270 to 300 knots IAS. The flare, figure 8, was initiated at 1,000 ft AGL

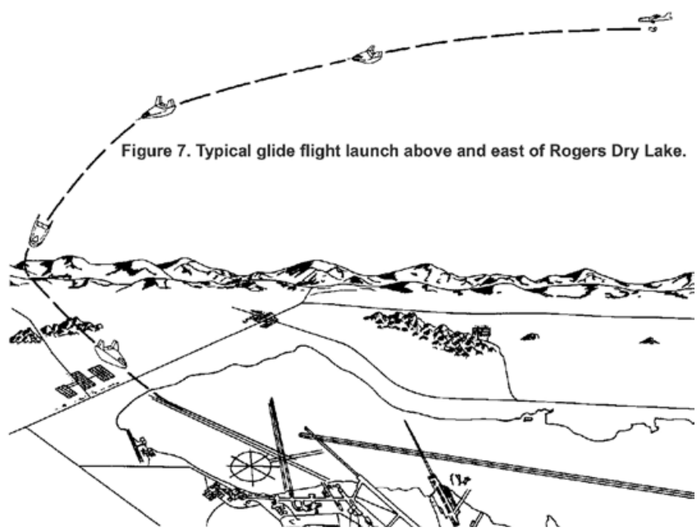


Figure 7. Typical glide flight launch above and east of Rogers Dry Lake.

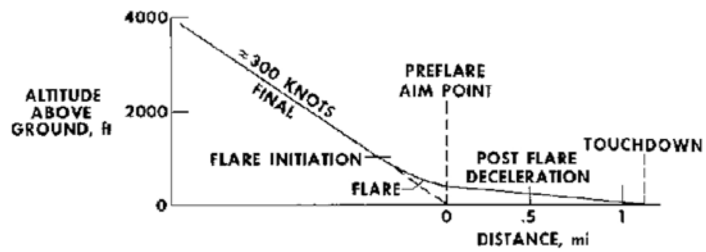


Figure 8. All unpowered flare and landings.

with a 1.5-g pull-up to level flight at 100 ft. above the runway to extend the gear. Touchdowns were between 223 and 155 knots.

Each of the lifting bodies flew a series of glide flights for pilot training and to verify the airframe dynamics, trim, flight control system operation and other systems, and this flight testing was complete, for the most part, by mid-1970.

The flight-test program was now mature enough that the entire lifting body team could stand-down and write some reports. We wrote on our progress to date for presentation to the high-ranking people who were deciding whether or not the United States should proceed with a full-sized space shuttle.

Flight-Test Progress With Adjunct Studies Through June 1970

The complete status of the lifting body flight-test program through late June 1970 was:

1) The HL-10 had completed its operational flight-test program;
2) The X-24A was about 50% finished with its flight-test program, had all but reached supersonic speed;
3) The refurbished M2-F2, designated the M2-F3 had completed a very successful first glide flight on June 2,and;
4) Supporting adjunct research consisting of two studies that included:
   a) Flight-test evaluation of the HL-10 using three H_2O_2 rockets to shallow descents followed by approach evaluations of shuttle-sized vehicles

Vehicle	First & Last Flight	Max Mach (Flt No.)	Maximum Altitude (Flt No.)	Comments
M2-F2 & M2-F3	12/7/66 6/2/70 12/21/72	1.61 (42)	71,500-feet 21,793-meters (43)	Crash on May 10,1967. All M2-F2 flights were subsonic. 37.5-month hiatus after crash; NASA HQ decides to rebuild the M2-F2 as the M2-F3 in late Jan. 69—26-months after crash. Total flights = 43.
HL-10	12/22/66 7/17/70	1.86 (34)	90,303-feet 27,524-meters (35)	15.5-month hiatus after first flight on Dec. 22, 1966, modified tip fins. Next flight 15 March 1968 following wind tunnel tests. Total flights = 37.
X-24A	8/5/69 6/4/71	1.60 (25)	70,500-feet 21,488-meters (25)	Flight-test program finished on June 4,1971 for airframe modification to the X-24B. Total flights = 28.
X-24B	8/1/73 11/26/75	1.76 (16)	74.100-feet 22,586-meters (23)	Modified airframe from X-24A; With nose wheel steering—only lifting body to land on main runway. Three guest pilots checked-out on last six glide flights. Total flights = 36.

Table 1. Summary of progress of each of the four vehicles in flight-test, including: maximum Mach number, altitude, and first and last flight date, etc.

using the B-52 mother ship and a Convair CV-990 and finally;

b) An instrument flight rules (IFR) unpowered low L/D approach study using an F-111, F-104, and B-52.

On June 30 the entire Lifting Body Team presented a symposium at Edwards titled: *Flight Test Results Pertaining To The Space Shuttlecraft*.[1] All the papers and results of this symposium, and a list of the attendees can be found in reference 7. This symposium consisted of two major parts:

I. Lifting Body Flight Test Results

II. Additional Space Shuttle Orientated Studies.

LIFTING BODY GRAND FINALE AND LEGACY

The last rocket-powered lifting body flight – the X-24B at Edwards – was accomplished on September 23, 1975, on its 30th flight. Six more glide flights were accomplished during October and November when three additional pilots were checked out, with two flights each. Thus ended the lifting body program at Edwards with the X-24B's last flight on November 26. A total of 144 lifting body flights (108 for the trio + 36 for the 24B) had been logged. We hadn't killed anybody, but we had suffered a test pilot injury with a M2-F2 gear up landing on the Dry Lake.

A program summary is presented in the table above.

What we actually accomplished.

As a result of what we at Edwards did, NASA Ames Research Center scientist-engineer, Clarence Syvertson, prognosticated that: ". . . *splashing down in an ocean, [was] a relatively crude and inefficient way of returning from a space mission. The lifting body offers an alternative that is already proved in principle.*"

And FRC test pilot-engineer, Milton O. 'Milt' Thompson, told the Shuttle Conference attendees and involved agencies that based on our demonstrated experience that: ". . . *if you can get the space shuttle off the ground, into orbit, and insure that it survives the entry, we at the [AFFTC and FRC] can guarantee that it can be flown to the destination and landed safely*."

The space shuttle became a reality on July 26, 1972, when North American Rockwell won the contract to design and build a space shuttle (ref. 8) orbiter. The first *space shuttle Enterprise*, was introduced to the public on September 17, 1976. Based directly on the Edwards lifting body flight-test results speed brakes were *included* and landing engines *excluded* from the design.

Noted pioneer rocket scientist, Dr. Walter R. Dornberger, stated in 1964 that: " . . . *in technical pursuits, the first idea put into practice can never be perfect — and can never mean the end of the work*." In 1964 the M2-F1, a *first idea*, was just beginning to successfully crawl its way across the sky, under somewhat shaky control, above Edwards AFB and land on Rogers Dry Lake. From our lifting body efforts we agree that we proved the concept.

The space shuttle at Edwards

On April 14, 1981, the first *space shuttle Columbia*, to be injected into orbit landed on Edwards AFB runway 23. It was late 1984 when the first runway landings took place at Kennedy

1 A PDF version of reference 7 can be obtained through the online NASA Scientific and Technical Information (STI) Program. Older NACA repots can be downloaded. The NASA Technical Reports Server (NTRS) asks: What do you want to find? Simply type the *report title* in the field provided in the 'Search NTRS box.'

Figure 9. Returning to Earth, the space shuttle Discovery lands on the main runway at Edwards AFB following an orbital mission. (Photo from NASA, ED09-0253-02)

Space Center, but Edwards was always a primary backup.

Post-script to this section—the FRC simulations go from all analog computer to completely digital. In the latter half of 1967 funds from the HL-10 were provided to FRC's simulation group to implement FRC's first all-digital real-time simulation capability using a SDS 9300 digital computer system. This request was justified due to excessive nonlinearities in new HL-10 aerodynamic data. The transition was seamless and transparent to HL-10 pilots and engineers.

TOWARD AN UNKNOWN TECHNICAL ASSAULT — IN RETROSPECT

The background

I came to NASA FRC in the spring of 1966 after being at AFFTC supporting the ASSET and X-20 Dyna-Soar programs. While there I had become proficient in the mechanization and use of the analog computer, the ASSET vehicle for example. In addition, I became proficient and very interested in the in-depth derivation, understanding and manipulation of airplane equations of motion. I could see and understand the analog computer's time domain solutions of these equations using an aerodynamic model, but trying to solve these equations mathematically[2] to determine the various airplane classical modes, such as short period or Dutch roll modes of motion in the frequency domain, was exceptionally difficult and time consuming using a slide-rule. One needed a digital computer and someone to generate and implement the program. We (I) could and did use approximations, N_b*, aka dynamic N-beta, for example (ref. 7), from time-to-time from such references as reference 9, but this seemed rather antiquated. Even in this era when slow digital computers were here and one was functional and available at AFFTC for data reduction, I felt the need for this computational tool.

A digital computer approach with its ability to solve higher order equations, to me, held the key and promise of unlocking the mysteries behind understanding flight dynamics. The thing that captured my imagination as a neophyte was the merging of the time domain flight or simulation responses and the solution of the equations of motion in the frequency domain.

There was a programmer at AFFTC who had a generalized program for the solution of matrices. I learned to use this program for solving the equations of motion for airplanes although I had to populate the matrix by computing the elements, a very time consuming and arduous task. After finishing this, I had to punch the data cards, input the deck to the operator and pick up the result the next day and hope I hadn't made a mistake or the task, to some degree, had to be repeated.

Dead-end approach to flight

The downside of the job at AFFTC, or so I thought at first, suddenly came when Dyna-Soar was cancelled on December 10, 1963. I was reassigned to AFFTC's new interest — vertical takeoff and landing (VTOL) airplanes. I had never derived the equations of motion for zero airspeed, but I went to work on that problem. I was assigned to learn about the Ling-Temco-Vought (LTV) XC-142 tilt wing, and I made a trip to LTV near Dallas to see what I could find out. The control system of the

2 An airplane, in flight, as a whole can be described accurately by linear differential equations with constant coefficients. The airplane can be expressed mathematically in what is called a linear time invariant or a LTI system. The solution of LTI system, airplane, equations for the motion of a particular airplane at some flight condition is frozen in time so to speak following a disturbance, and results in two fourth-degree equations. I challenge any reader, using a slide-rule, to solve both these fourth-degree equations in a timely fashion or even one. I doubt there will be any takers. From these the eigenvalues, eigenmodes, or modes of motion, etc. are determined.

XC-142, hover to conventional flight, was a heavy all-electro mechanical system, very cleverly done, but a nightmare of nonlinear possibilities. I went to New Jersey to Curtiss-Wright to find out what I could about the X-19. The X-19 had four tilt props on something reminiscent of a four-poster-bed and, instead of the bed, an Aero Commander fuselage with props on each of the four posts. On April 25, 1965, I witnessed the XV-5A fatal crash on Rogers Lake and an Edwards test-pilot friend was killed in the French Dassault Balzac VTOL on September 8, 1965. I decided I had enough of this mode of flight. These airplanes were novel and impressive when they worked, but they needed digital-fly-by wire and higher thrust-to-weight capability, and these technologies were several years away. I did learn that these vehicles had large varying dynamics, some very unstable, through transition to normal flight.

Fortuitous meeting

In early 1966 when I met an old supervisor friend from FRC at an AIAA conference; he told me that they were looking for somebody to assume the position of stability and controls engineer and coordinate the development of a piloted simulation of a vehicle they called the HL-10. He explained that the HL-10 was a candidate reentry vehicle that had been designed by NASA Langley Research Center and fabricated by Northrop in Hawthorne. The HL-10 airframe had already been delivered from Northrop to Edwards in mid-January 1966, had never flown and was even then being instrumented for flight-test in FRC's main hangar across the field north of the Air Force main base. The vehicle was projected to fly in six months. This sounded like a dream job for me — something I could work on from day one, before the vehicle ever flew, so to speak. In addition this was my way out of the VTOL business — vehicles that I came to detest so much.

It was spring 1966 when I accepted the position offered by NASA FRC and reported for work the second week of May.

The target of my technical assault

The vehicle

Sitting in NASA's main hangar the HL-10 to me was a beautiful vehicle. I felt like a kid getting the keys to a never-before driven new car and it had my name all over it. I was fully armed with a slide rule and a 2H pencil and FRC supplied all the graph paper I would need but no automatic plotting routines.

The vehicle had a 74° sweep leading edge with a delta-planform, a large center vertical fin with rudder/speed brake, fixed tip fins and a forward single place cockpit. The fuselage trailing edge elevons were of a trapezoid shape and these provided both pitch and roll control with no adverse yaw. The tip fins had small two position movable flaps for configuration change but no control function.

Northrop's features included:

1) Pressurized cockpit, a pilot's Mach meter, AOA indicator and a three-axis attitude indicator with sideslip angle indicator.
2) Dual hydraulic powered irreversible controls.
3) Three-axis angular-rate feedback, limited authority, stability augmentation system (SAS) based on classical design, with pilot select gain capability.
4) Landing gear extension time ≤ 1.5-sec,
5) Electrical power was from three silver-zinc 28-volt battery packs—good for 40-minutes.

Breaking the digital barrier with PCM

For flight-test, FRC's instrument engineers were in the process of installing a *pulse code modulation* (PCM) data acquisition system — one of their first. FRC's full set of flight-test instruments were all analog devices, their outputs were the inputs to the PCM system. In this system each of the analog signals were sampled at 200 times per second, converted to a digital data stream that was transmitted to the ground station for display real-time in the flight-test control room and recorded on magnetic tape for storage. This system replaced the large cumbersome rolling film canisters, with as many as 36 channels, used in the X-15 program and associated labor intensive data reduction this entailed.

This was a vast improvement in data acquisition over film recording; the fidelity of the data was excellent, data handling was greatly improved, replay of segments was quicker, data reduction was faster and resolution was improved. This also presented the possibility of real time computation of outputs that consisted of more than one down-linked variable — like Mach number for example. Determining indicated Mach number, in near real-time, required the ratio of two down linked variables plus a table lookup routine stored in a ground computer. I infringed on the data reduction group's domain and unknowingly stepped on some toes with this suggestion.

Figure 10. Circa 1966, HL-10 on NASA's FRC ramp. (Photo from NASA, ECN-1289)

Launching the technical assault

My primary computational tool was my slide rule or perhaps a borrowed Friden electro-mechanical calculator. My resources consisted of an aerodynamicist, engineering intern, and simulation engineer. We were the entire stability and control team.

Fortunately all the latest force and moment aerodynamic wind tunnel data were all logged in by our aerodynamicist and in logical presentable plotted understandable order as a function of Mach numbers (up to about 2), angles of attack, sideslip, control surface variations, subsonic or transonic configuration, etc. The simulation engineer was eager to get to work on his full nonlinear analog six-degree of freedom (6-DOF) piloted engineering simulation and all he wanted from me were the nonlinear equations of motion I wanted implemented. Fortunately, I had all these in hand-written format and gave them a final review before I passed them on. He had been working on the force and moment data with the aerodynamicist prior to my arrival that gave us a jump on that task. The intern was eager to be turned loose on some problem.

I was not a digital (Fortran) programmer but the intern was and this was a stroke of luck for me. One big item that FRC didn't have was a frequency domain or linear analysis program from which we could determine frequencies and damping of the longitudinal and lateral-directional modes of motion throughout the HL-10 flight envelope. Using the simulation equations, I linearized this set for use in the digital frequency domain (3-DOF) program the intern would produce. I wanted to keep this program generalized for almost any airplane and later we added feedback loops and aileron-to-rudder (ARI) cross-feed and other features such as computed pilot ratings later.

I set to work determining two sets of linearized aerodynamic derivatives for use in the program the intern would produce. This was no trivial task because I had to go through all the wind tunnel data and compute the longitudinal and lateral-directional stability and control effectiveness derivatives, using my slide-rule, as functions of angle of attack and Mach number. In addition I had to plot all the data using my 2H pencil. This was a labor of necessity, there were no automatic plotting routines. I only did the subsonic-configuration first because we would be doing glide flights initially. I could do the transonic-configuration later.

Our aerodynamicist had all the vehicle reference physical characteristics such as wing area, span, mean aerodynamic chord, etc. What she didn't have were the vehicle's mass properties including the vehicle's body axes roll, pitch and yaw moments of inertia or product of inertia. About this time our aerodynamicist left us when her husband was transferred to a new job out of the Edwards area and I had to take over.

During fabrication, Northrop carefully tracked weight and balance for both the M2-F2 and HL-10 to comply with the Statement of Work's specified center of gravity. They generated a digital program for tracking changes and computed new values as equipment was added or removed; in addition roll, pitch and yaw moments of inertia and product of inertia were computed for use in flight test.

The intern worked with much enthusiasm and had a working program in very short order. I could even give him some preliminary numbers to work with. We ran numbers in the frequency domain and checked the simulation responses to validate the simulation. One thing I noticed about my linearized data was that the aerodynamics seemed quite benign. There were no abrupt characteristics such as sharp changes like a stall that would surprise a pilot and there were no instabilities. Control effectiveness was good everywhere and ailerons in particular were very effective and there was no tendency for adverse yaw. There was no indication of stalled flow that we would encounter in flight.

Once this all came together, I could say we were now open for business. The simulation matured and we added a cockpit. When the time was right we invited our program pilot to test fly the simulator. He thought the HL-10 looked very good - *almost too good* - by comparison with the M2-F2 simulation that the Air Force was using at their facility. In addition he had the flown the M2-F2 in late September 1966. It's amazing how the 'word' got around about how good the HL-10 sim looked. Management asked themselves: Could the HL-10 really be a much better flying vehicle than the M2-F2? They really didn't think so, but nobody could prove us wrong.

The HL-10's first flight

We survived all the administrative hurdles before a first flight such as the Technical Review, etc. The simulation passed a quality test. It was late in the year before we were ready. Flight date was set for December 22, 1966, and we hadn't yet installed the XLR-11 rocket engine. The flight was to be unpowered for pilot familiarization and systems checkout.

The launch was typical of our glide flights shown in figure 7, but trouble was evident almost immediately. Two problems associated with the flight control system were encountered. The first was a limit-cycle in pitch, that is a sustained vibration at a fixed frequency and limited amplitude. Secondly, the pilot's pitch stick was excessively sensitive. The limit-cycle problem was traced to contaminated hydraulic fluid (ref 10). The excessive pitch stick gearing was selected based on simulator studies for the entire two configurations, subsonic and transonic, and we didn't need that range of pitch control authority. These problems were corrected for the second flight. Reference 10 discusses these various problems and the solutions in detail. In addition, I had recommended initial SAS gain settings that were excessively high. We modified the frequency domain program to compute results with SAS gains included to better select these for later flights. But a bigger problem remained and I was disappointed with the flight — I had assumed 'ownership' of the HL-10 and didn't see any indication of stalling in the tunnel data or even a possibility.

THE SHOWSTOPPERS

The HL-10 is grounded

Our third and most significant problem, one we couldn't fix, was an intermittent airflow separation (stall) over the aft end of the vehicle that affected the elevon roll and pitch control effectiveness, illustrated in fig 11, for the roll axis.

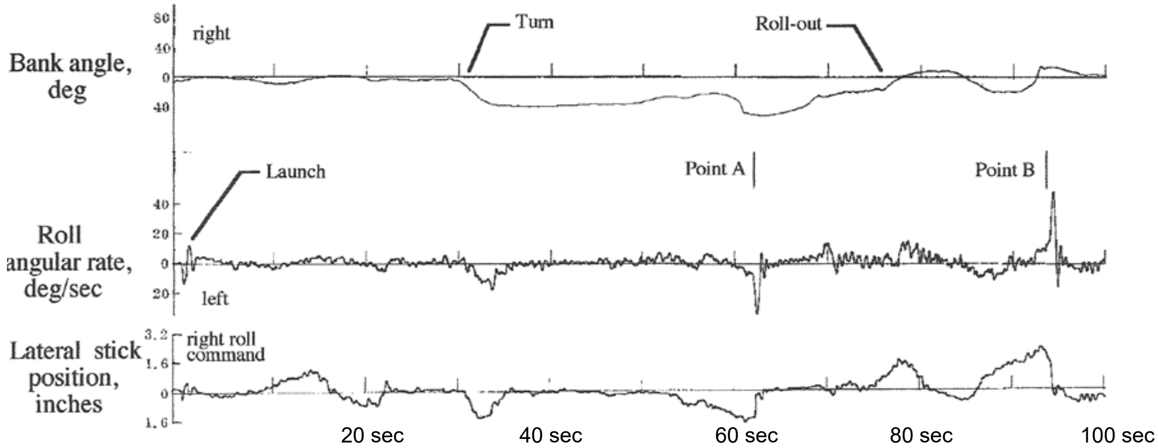


Figure 11. HL-10 first flight, abbreviated illustration of airflow stall intervals. The roll rate should approximately follow the roll command and doesn't. Note the large accelerations at A and B, ref 11.

The effects of the separation could be seen in the normal and side acceleration and in the inboard tip fin hinge moment data traces as a high frequency disturbance. Vehicle motion was also affected as illustrated in figure 11.

The flight was very short even for a lifting body. Total flight time was only 3.1 min - figure 11 represents over 50% of the entire flight. The left turn to 'base-leg' was started at 30-seconds. The left roll-rate response was rather lethargic, but the vehicle did roll into the turn. At about 50 seconds the pilot started to ramp in more left roll command, but the vehicle was wandering off to the right until Point A was reached and then there was a large left roll spike when AOA reached about 2°, left bank was reestablished and the pilot neutralized roll command. At about 75 seconds a right roll command appeared, the right roll was rather lethargic this time. Zero bank angle was reached at 78 seconds, passed through zero bank angle and rolled left again. At about 87 seconds roll command was ramped to the right until 93 seconds when another roll spike was experienced, this time to the right — Point B, and the vehicle rolled rapidly to the right — very curious behavior for any airplane and not crispy roll-rate responses except at Points A and B. Between 80 and 100 seconds it appears that the pilot was attempting to fly at zero bank. After about 110 seconds, beyond the limit of fig 11, the vehicle was flown at lower angles of attack. The turn to final approach was begun at 114 seconds and took 45 seconds leaving the last 28 seconds for flare and successful landing.

We at FRC, after scrutinizing all related flight data, theorized that: On this first flight the aft portion of the HL-10 in the vicinity of the elevons was, at various times in separated (stalled) flow; the separation was a function of AOA and sideslip and affected roll and pitch control. We immediately contacted the aerodynamicists at Langley and almost immediately they put their HL-10 back in their wind tunnel — only the owner of the tunnel could do this.

Langley proposes candidate fixes

We prepared a complete flight data set that illustrated our theory of flow separation for presentation at Langley. While in our presentation of data, one of the aerodynamicists jumped up slammed his mechanical pencil on the floor in a fit of self-deprecation saying to himself and us that: *"I knew that something like this would happen, etc."* We left the copies of our data set with the Langley personnel to ponder, they said they would be in touch and we headed for home.

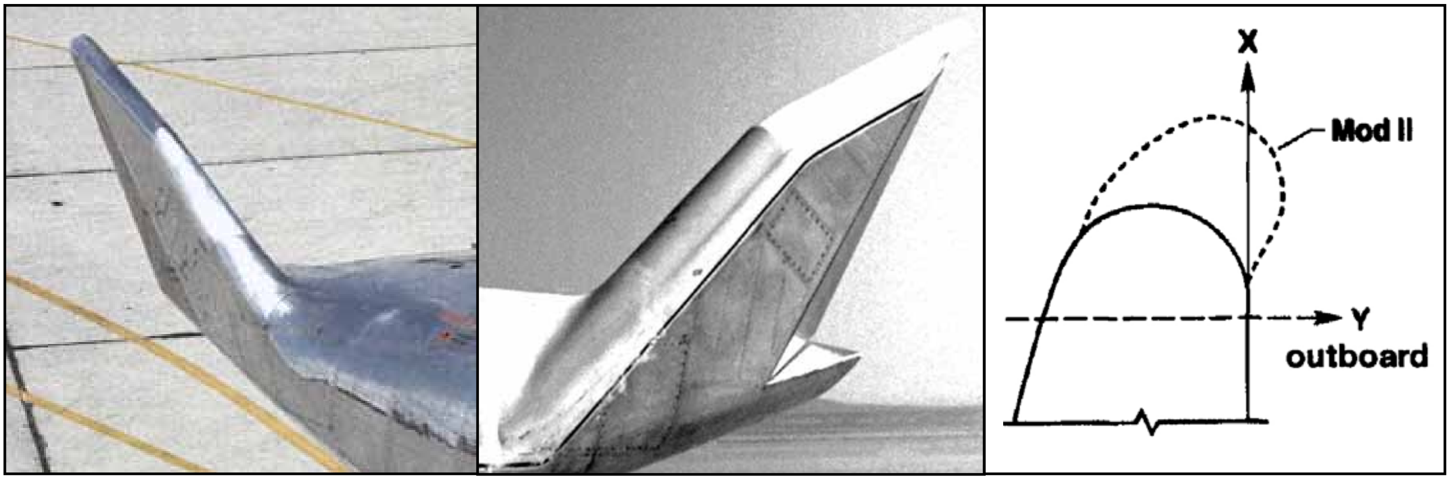
The LRC aerodynamicists soon came up with two possible modifications for the tip fins they thought would fix the problem. They recommended either fix, but would not commit to one above the other. They sent us both sets of preliminary wind tunnel data and I went to work plotting it all up, and then reviewing which I personally preferred as a fix. I selected the modification (Mod II) that extended and cambered the tip fin leading or forward edges outboard. The data for this modification was the *more linear* of the two and so I selected it and my supervisor agreed (ref 11). The HL-10 tip fin comparison unmodified (left) and modified (right) is shown in figure 12.

While we were working on a new set of wind tunnel data during the latter half of 1967, we transitioned from the first HL-10 analog simulation mechanization to an all-digital real-time simulation. The HL-10 project helped the simulation group pay for their new equipment (see reference 12 for simulation history and details).

We flew the remainder of the program, including supersonic flights, with no difficulties with flow separation and there were no significant differences between flight and our aerodynamic model. Our second flight was on March 15, 1968, and all expectations and objectives were met.

The HL-10 flies its second flight

The second flight was a resounding success. According to the pilot there were no surprises this time. Everything was just as the simulation predicted. The HL-10 was reported to fly as well as a F-104. The HL-10 was the only lifting body to fly the remainder of the year. We installed the rocket engine in September, but on the first attempted powered flight on the 24th, there was a malfunction and Major Gentry landed safely



Right Fin Unmodified **Left Fin Modified** **Mod II, section view, X is fwd.**

Figure 12. HL-10 unmodified and modified tip fin forward leading edges, extended and cambered outboard, right.

on Rosamond Dry Lake. He didn't have much to do while the support team headed there.

We had the first successful powered flight on November 13, 1968, to Mach 0.84. In April 1969, the X-24A flew its first flight and during this year we had to alternate flights with them. We were busy expanding the HL-10's transonic envelope and checking out pilots. On May 9, the 17th flight the HL-10 made lifting body history by flying supersonic for the first time. In the following flights we were busy giving pilots flight time and expanding the supersonic envelope to maximum speed.

In 1970, we were again alternating flight schedules with the X-24A and then on February 18 the HL-10 flew to a Mach number of 1.86, its highest and higher than any lifting body after - flight time was 6.3 minutes. Then on February 27 the HL-10 flew to a maximum altitude of 90,303 ft, higher than any lifting body - flight time was 6.9 minutes.

Typical pilot ratings for handling qualities was '2'. This indicated that its handling qualities were satisfactory without any improvement or good with only negligible deficiencies and that pilot compensation was not a factor for achieving desired performance, see ref. 13.

Figure 13. The HL-10 breaks the sound barrier May 9, 1969. (Photo from NASA, FRC E-21090)

Today the HL-10 stands proudly on a pedestal in front of FRC with a plaque that lists the program participants — my name is not among them.

The M2-F2 is grounded

I had been aware of an oscillatory coupled roll-spiral mode (*but not its implication*) in the M2-F2, but this was no great surprise without wings. Natural roll damping would be low affecting the conventional aperiodic roll mode and remembering Dale Reed's model demonstration of strong spiral stability. (I visualized these two modes on the real axis of the complex plane as two stable modes.) Having worked in the VTOL world I was familiar with unusual varying dynamics from zero airspeed through transition. Shortly after my arrival to the HL-10 program, I was told by my counterpart working the M2-F2 program that 'yes, this second-order, coupled roll-spiral, mode existed' but because the modal characteristics — period was so long and damping light, she concluded that this lateral-directional mode was akin to the typical, relatively innocuous, longitudinal long-period mode, the phugoid in relatively conventional airplanes and declared; 'it was no problem in the M2-F2.' The problem, as far as I was concerned, was that the M2-F2 was no conventional airplane so: Why would its dynamic characteristics be conventional and probably innocuous too? They, or it, was not as we would painfully learn in the future.

The M2-F2's Nemesis

The 'innocuous' coupled roll-spiral mode

When I arrived at FRC in early 1966, they had an up and running M2-F2 simulation available to fly if I wished, but it was in the process of being transferred over to AFFTC's simulation lab as they would be working that vehicle as part of the Joint Program. On several occasions I did fly FRC's M2-F2 simulation and I did have problems in the 300-knot final approach — I crashed more times than not when I flew this flight condition and this was very puzzling to me. I attributed my problems to being an unskilled test pilot or '*ham-fisted*' as the pilots called it, but the puzzle remained. Again I conferred with my M2-F2 counterpart as to why she never flew the simulation? The answer was that the environment was just 'too artificial.' I retorted with: "But where else can you see the result of solving six differential equations simultaneously?" I had my problems working the HL-10 and took no more notice of the M2-F2 after the simulator was dismantled. There was no more interchange of ideas or response until one day standing in front of my desk, my M2-F2 counterpart, told me she had been conferring with FRC's resident flight dynamic expert and *they* had concluded that: 'I was doing everything [on the HL-10] all wrong.' No management personnel, nor the 'resident expert,' ever approached me on this subject or told me I was doing things wrong. There was still a certain work-place protocol to be followed and there were expert pilots and other engineers involved in the M2-F2 program. I had been assigned to do the M2-F2 flight parameter identification (ref 14). I attended the M2-F2 preflight briefings and don't remember any dissent in their ranks. I listened without comment and went about my business without interruption.

The first flight of the M2-F2 occurred on July 12, 1966, with NASA's highly experienced Milt Thompson at the controls (see his book, reference 4 for details) of this flight. As I monitored this flight I noticed the flight characteristic was very 'rolly' and before landing experienced a good dose of a pilot-induced-oscillation, at negative angles of attack consisting of large amplitude bank angles, just below 10,000 ft (3,048m) Milt recovered after making several changes in the aileron-to-rudder interconnect (ARI) gain setting during this oscillation (for details on this, see reference 4). The ARI gain sense had been set opposite to that of the simulation and was an in-flight confusion factor. After that FRC simulation rules dictated that simulations would be configured like the airplane.

Milt made a very telling comment in his book about the lateral-directional stability right after a practice flare, he stated:

> "Almost immediately after pushing over, I noticed a slight lateral-directional oscillation, which seemed to confirm that I was *nibbling* on the edge of a PIO, *just as our simulator predicted*." (Milt apparently experienced a similar simulation problem — to a lesser degree as I did, but I didn't know it.)

A similar oscillation happened on flight 10 with another pilot also flying at negative AOA and then on May 10, 1967, on flight 16 just seconds above Rogers Dry Lake, again at negative AOA, in the landing approach the PIO struck again. Recovery was accomplished as AOA was increased and rudder activity ceased, but the flare was microseconds late and just as the gear were extending. The aircraft touched solid ground before the gear locked, forcing them back into their wells. The result was that the M2-F2 rolled and tumbled across the lake surface.

This event happened while I was focused on gathering new wind tunnel data for the HL-10 and reworking the new aero model, but I was in the control room and saw the crash on our video. Immediately after the crash, I was assigned to analyze the dynamics of the M2-F2 to see what I could find that could have precipitated the crash-landing. I was subsequently assigned to the M2-F2 program. It seems that my counterpart had resigned and relocated to fill a position more aligned with her specialty as a theoretical aerodynamicist.

Unlocking the mystery of the roll-spiral mode

My first action was to get a lateral-directional linearized aerodynamic model at the correct flight condition and do a frequency domain analysis. Having worked in the process of parameter identification (pid) during the fall and winter on the M2-F2 flight data while working the HL-10 as well, I had these results in my personal files. With all the M2-F2 aerodynamic data, physical characteristics, and flight condition of the PIO, I ran a single case of a single axis bank angle control problem assuming the pilot as a pure gain. This analysis formed the basis for a study that was later published in ref. 15.

The PIO time response of flight 16 is shown in figure 14. The bank angle instrument wasn't calibrated beyond about ± 50° so beyond this value the data are incorrect. The numbers shown are approximate and based on visual observation.

What we see, from the data at $t \approx 0$ sec, is the M2-F2 banked to the left, at a negative AOA, the pilot attempted to come to a wings-level final approach flight condition. For the first 9 seconds the pilot used only lateral stick to control the airplane. At about 2 seconds the vehicle was at about –2° AOA.

At this point the airspeed was 310 knots IAS and this is the snapshot in time (flight condition) that I chose to compute what the dynamics of the vehicle were at that instant.

We can see that the pilot did get on the rudder pedals that exacerbated the situation. Recovery was affected after AOA was increased to positive values and the pilot ceased rudder activity.

The revealing result

At this flight condition the vehicle with SAS on and aileron-

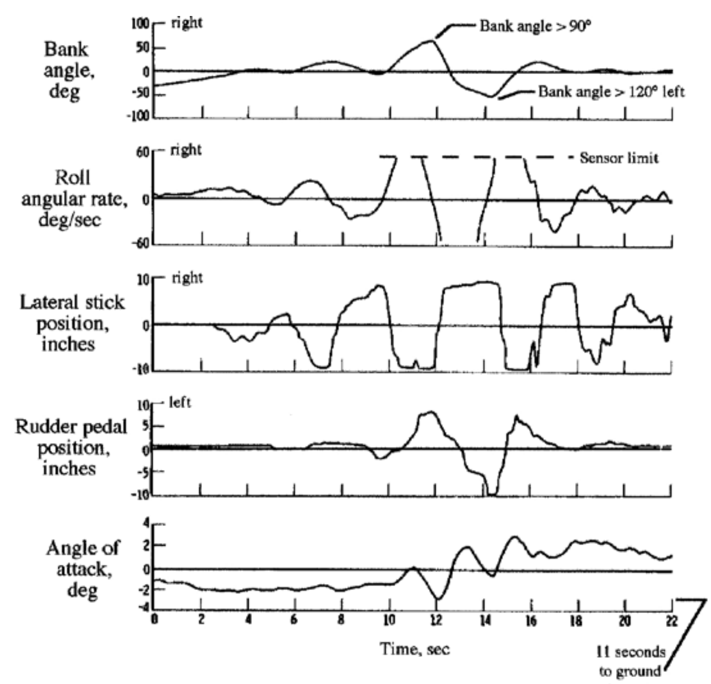


Figure 14. M2-F2 PIO of flight 16, 33 sec. to impact, 310 kts.

to-rudder functioning, the roll-spiral mode was practically neutrally stable. This result prompted me to go back and see how this mode behaved as a function of AOA. What I found was the roll-spiral mode didn't form until about 5° AOA and it was headed into the direction of instability as AOA decreased. The question was then: If the pilot was controlling bank angle what then? When I considered the pilot as a gain only flying a bank angle task the system tended to head toward instability. This appears to be exactly what happened in the M2-F2 at negative AOA — the instability occurred that we called a PIO. From this I concluded that the coupled roll-spiral mode, despite what open-loop modal responses may look like, at this flight condition was not innocuous after all. The piloted simulator result was probably true and correct and Milt saw the problem, nibbling on the edge of a PIO, but he was not ham-fisted like I was, he didn't raise a red flag and nobody took an in-depth analysis to understand what the 'nibbling' was, including myself.

Figure 15 illustrates hypothetically, for simplicity rather than fidelity so as to not make it too confusing to the reader, the frequency domain solution of the M2-F2 at –2° AOA and 310 knots. This solution represents a frozen instant in time. We have seen the time domain in figure. 14.

In my frequency domain program, figure 15, I have computed the vehicle's lateral-directional modes of motion — these are the poles as shown for the Dutch roll and coupled roll-spiral modes or for a more conventional situation simply as a roll mode and spiral mode that have migrated and joined together, along the dotted line, as AOA was decreased, forming what we call a coupled roll-spiral mode.

The bank angle zero illustrates the terminus of the closed-loop system's path progression, from the pole, as the gain is increased — the pilot in the M2-F2's case. The higher the gain the closer the locus gets to the zero — representing the harder the pilot is working to control bank angle; finally getting on the rudder pedals, that only made the problem worse.

Zeros and poles are the jargon used in classical, not 'modern,' control system design and analysis in the determination of system response to an input. The vertical axis is the imaginary (the negative conjugate or mirror-image axis is not shown) and the horizontal axis is the real axis. Any pole in the right-hand plane (positive real part) indicates an unstable mode. This analysis is valid for a single input and for the first seven seconds the M2-F2 pilot didn't use the rudder so this result should be relatively accurate. When the rudder was used, the situation got worse. In the actual calculation the roll-spiral wasn't quite as far to the right, unstable, as illustrated in figure 15, but the M2-F2 was driven by the pilot, controlling an airplane just as he had been trained early in his career, into the unstable region at negative AOA; the M2-F2 was a 'horse of a different color.'

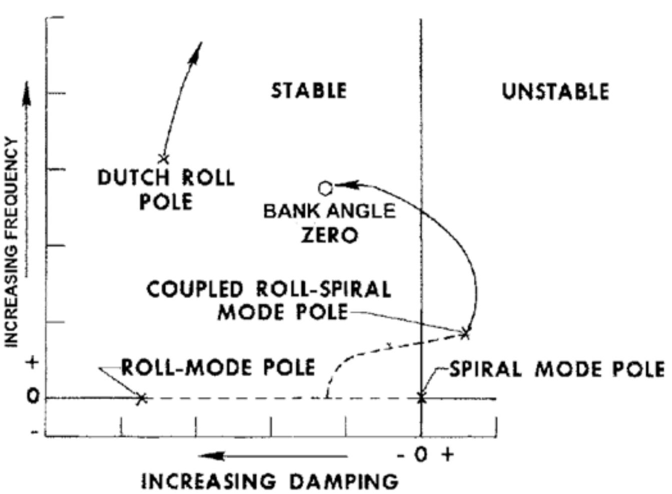


Figure 15. Frequency domain solution showing the pilot-vehicle interaction with the roll-sprial mode at the flight condition of -2° AOA and 310 knots IAS.

My conclusion

My message to our project manager, FRC management, and the Joint Test Team was that the roll-spiral mode was the root source of the M2-F2's dynamic instability at negative AOA. A coupling of the roll-spiral mode was unique and in the M2-F2 a complicating feature were the combined effects of large adverse aileron yaw that demanded an aileron-to-rudder interconnect to control bank angle along with the exceptionally large dihedral effect with low roll damping and negative AOA — these didn't help the lateral-directional dynamics.

In retrospect, the HL-10 or the X-24A was not required to fly near negative AOA during their high-speed approach flight condition. This flight condition was a bane suffered only by the M2-F2 due to its aerodynamic shape. Airplanes don't usually fly or like to fly at negative AOA and their equations of motion usually testify to this. The M2-F2 was an unusual vehicle and a better flight control system and or improved aerodynamics could possibly have improved its controllability.

The M2-F2 was unique to FRC's typical airplane experience over the years with its mass very close to the roll axis resulting in a relatively small roll moment of inertia compared with pitch and yaw and it had an enormous dihedral effect. As such, any disturbance of sideslip caused roll acceleration out of proportion to aileron command. Penetrating turbulence caused anxiety in the pilots. With no wings, the natural roll damping was very low.

In the X-15 a pilot-airplane lateral instability was experienced in flight and on the simulator. That instability was a pilot-vehicle interaction with the Dutch-roll mode — see ref.16. FRC managers were familiar with that instability and the analysis of ref. 16 revealed its source. In the M2-F2 the pilot-airplane lateral instability was a pilot-vehicle interaction with the coupled roll-spiral mode and had nothing to do with its Dutch-roll mode. Not all managers or team members accepted my conclusion. But be that as it was, until further analysis could prove me wrong, I was convinced I had found the source and cause of the M2-F2's problem and there were no takers to challenge me face-to-face. This instability was also present in the simulator, but obviously subtler — nobody flagged it. I was disappointed that we, at FRC, could send a pilot into such a treacherous condition.

It is mandatory for a lifting body to have effective ailerons with no adverse yaw throughout its flight envelope, that provide good roll-rate, bank angle control, and good roll damping

Figure 16. The M2-F2 on Rogers Dry Lake after its crash on May 10, 1967. (Photo from NASA, FRC E-16731)

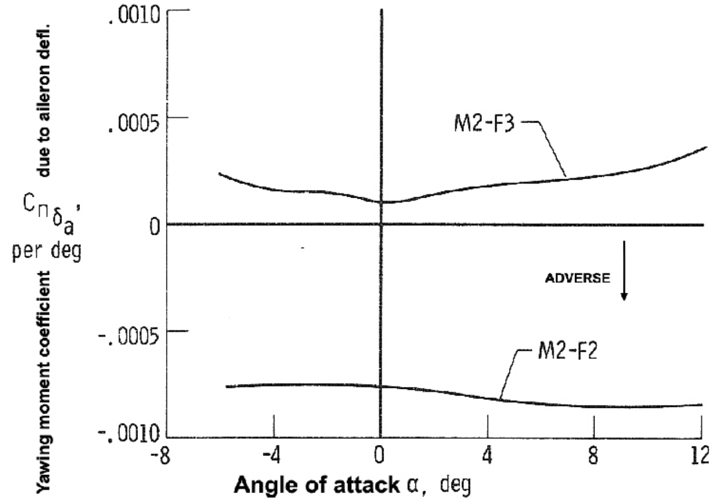


Figure 17. Adverse aileron yawing moment of the M2-F2 (bottom) compared with the improved M2-F3 at subsonic speed (top).

through roll-rate feedback — these were one of the most beneficial features in the HL-10. Langley went out of their way, based partly on a FRC recommendation, to provide good ailerons on the HL-10.

Rescuing the M2-F2

Following its rolling and tumbling across the Dry Lake on May 10, 1967, the vehicle after the crash is shown in figure 16. This was loaded on a truck and taken back to Northrop in Hawthorne for inspection. Management had the idea of possibly rebuilding the M2-F2 if Northrop's inspection deemed this reasonable.

The M2-F2 was a NASA Ames design so they were consulted as to how aerodynamic improvements might be made; reference 15 presents a relatively complete story of the process. NASA didn't specifically decide to rebuild the M2-F2 immediately following the crash, but Northrop, with the assistance of NASA personnel, continued their slow detailed inspection of the airframe. Ames determined that the addition of a fixed aft-center fin would cure the M2-F2's adverse aileron yaw. They determined that a fixed center fin would provide a better aerodynamic pressure field by eliminating the adverse yawing moment produced by elevon deflection for roll control by creation of a favorable pressure field between this fin and the outboard surfaces. The program recommended this simple addition to the M2-F2 and NASA Headquarters decided to approve the rebuilding of the vehicle in late January 1969, 26 months after the crash. The rebuilt modified vehicle became the M2-F3. I was opposed to rebuilding the vehicle and quite vocal about it due to what I termed its insidious dynamic characteristics, but if we were really going to fly we should have a new set of wind-tunnel data.

The HL-10 flew its first powered flight on October 23, 1968. This was the first lifting body to do so and the rocket shutdown prematurely, but we were prepared for this event. The day after this, October 24, the X-15 flew its last flight — number 199, and now FRC management could focus their complete attention on the lifting body program — up to now they had almost left us to our own devices. A series of HL-10 pilot checkout and powered flights were planned, expanding the envelope to supersonic flight through the rest of the year and well into 1969. I was fully involved in the flight activity working with pilots and the planning effort toward supersonic flight and not very interested in the M2-F3 as it was now called.

It was about February 1969 the program manager approached me and insisted I travel with him to Northrop in Hawthorne to see the progress being made on the M2-F3. I was busy supporting the early powered HL-10 flights so I told him I had more important things to do than take a day off for an inspection tour — but he insisted. I reluctantly went and shortly after we arrived at Northrop I quickly saw all I wanted to see of the progress on the M2-F3, and I was now a captive so could not depart. About lunchtime we went to a small side room and much to my surprise I saw a relatively large model of the M2-F3 ready for wind tunnel testing. Tunnel tests, unknown to me like the tunnel model, were scheduled for early 1969 and I was expected to be at Ames to support this effort and after this I gathered up all the data and headed home for another cycle of analysis.

Figure 18. The M2-F3 is shown on Rogers Dry Lake with the XLR-11 rocket engine installed. (Photo from NASA, FRC E-21533)

Taming the coupled roll-spiral mode

With the new M2-F3 wind tunnel data in work for the simulator and our extraction of a linear model, we had determined that the yaw due to aileron deflection was indeed positive, as shown in Figure 17. This result completely changed the complexion of the M2-F3's lateral-directional dynamics. First, the vehicle required no aileron-to-rudder interconnect to make turns with the aileron adverse yaw gone. These changes of course pleased the pilots with better roll response and control with the added benefit of improved SAS roll damping and by increasing gain setting. Looking at Figure 15 — visualize that the roll-spiral mode still exists, but the locus is moved to the left into the stable plane and not toward instability. With better roll damping, clear of the unstable boundary, the vertical axis of Figure 15, and the tendency toward PIO was completely gone, see reference 15 for details — this report represents a study I did in my spare time using the pilot who survived the crash of the M2-F2.

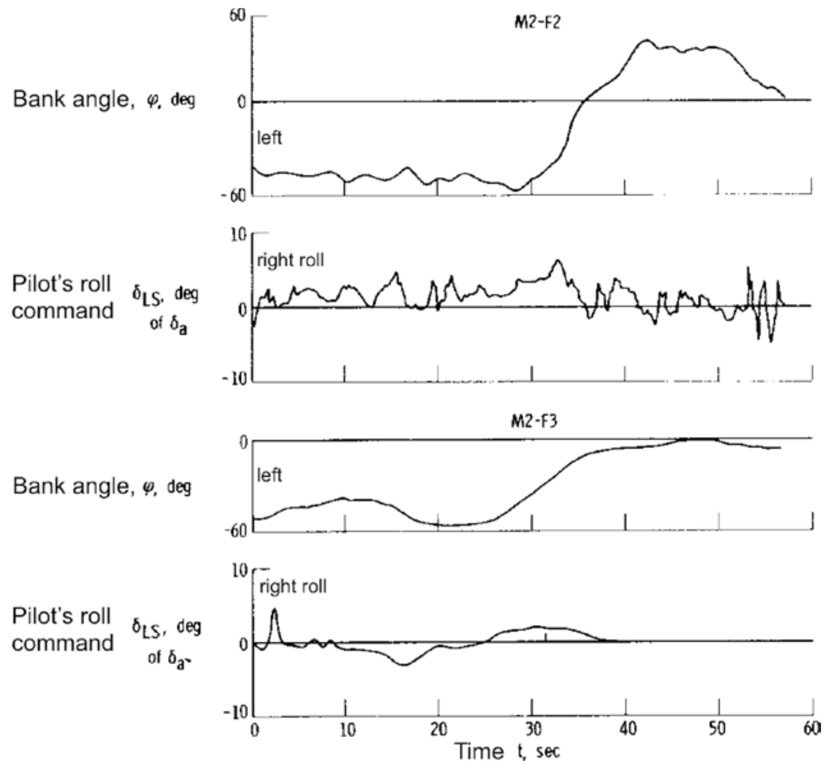


Figure 19. Landing approach comparison of the M2-F2 with the significantly improved first flight of the M2-F3. Reference 15.

The flight with the center fin

The M2-F3's first flight — flight 17 (retaining the numerical sequence of flights) was on June 2, 1970, and was a resounding success according to the pilot, there were no surprises this time, fig 19. The vehicle's responses were just as predicted by the simulator.

Looking at Figure 19 — the last 60 seconds of the flight records of two different approaches, one for the M2-F2 and the other for the M2-F3, showing bank angle and pilot's lateral stick. About the first 30 seconds of each is devoted to flying the base-leg and the last 30 seconds to flying final approach. In the M2-F2's case (top) in a steep 50° left bank with very ragged pilot commands followed by a right S-turn to about 30° right, again with unsteady bank command, and a roll-out the last 10 seconds. I would characterize this performance as Milt Thompson described it, back on page 15 as "*nibbling* on the edge of a PIO" because no test pilot would fly an approach as ragged as this intentionally. In the M2-F3's case, I would characterize this maneuver as almost leisurely, as one would expect from a test pilot in command of a controllable airplane. The raggedness of the M2-F2 is completely gone from the M2-F3. Pilot comments and fig 19 convinced this author that our modifications to the airplane and control system worked as we designed them and the insidious dynamic characteristics were gone. To make the task more difficult, remember that the airplane was descending in excess of 10,000-ft/min. Remember, as well, that no mode of motion should be dismissed out of hand.

We all took some time off to breathe easier and prepare for the big space shuttle symposium, Ref. 7, at the end of the month. We were able to present a flight landing approach comparison between the M2-F3 and M2-F2, as seen in Figure 19, to the august assembled symposium group.

Shortly after the M2-F3 flew for the first time, the HL-10 flew its 37th and last flight on July 17. The M2-F3 followed an active flight schedule, as did the X-24A through the year of 1970.

On February 9, 1971, the only pilot to fly both the HL-10 and M2-F2, guest pilot and friend Maj. Jerry Gentry made a glide flight in the M2-F3, its 21st. Major Gentry reported that: " . . . *The M2-F3 flew as well as the HL-10 . . ..*" Jerry had flown five M2-F2 flights and nine flights in the HL-10 so he could make a valid comparison, but he made no recorded comparison with the M2-F2.

On December 13, 1972, X-15 pilot Bill Dana, made the M2-F3's, flight number 42, to a maximum speed of Mach 1.61. John Manke flew the next and last M2-F3 flight on December 21 to its maximum altitude of 71,500 ft (.

Overall typical Pilot Ratings for the M2-F3 were 3. This meant that the handling qualities of the vehicle required minimal pilot compensation to achieve desired performance and over-all the vehicle was fair with some mildly unpleasant deficiencies and was satisfactory without improvement; see Ref. 17.

What we couldn't fix on the M2-F3 was the extraordinarily high dihedral effect and the low L/D. The pilots could accept these and handle them as long as they were sure they were flying a reliably stable airframe.

The X-24A had successfully completed its flight-test program on its 28th flight on June 4, 1971, having achieved a maximum Mach number of 1.60 in late March. Thus ended this chapter in the lifting body saga at Edwards.

CONCLUSIONS

The inauspicious launching of the Space Race in 1957 led directly to a conducive political and technical environment that induced scientific symposia concerning possible methods of manned and piloted orbiting satellites, at high-speed, entering then maneuvering within planetary atmospheres to horizontal airplane-like landings. Ideas and concepts that emerged from these symposia germinated ideas at NASA Centers, in particular NASA's Flight Research Center, at Edwards AFB, Calif., and in a single engineer with a vision of the future of space flight — R. Dale Reed. In 1962 Dale promoted the idea to build a low cost proof-of-concept lightweight piloted lifting body for experimental gliding flight-test. Thus, the low-speed unpowered M2-F1 was conceived and successfully flown giving rise to the development of three piloted, heavy-weight, rocket-powered low-supersonic vehicles — that included the NASA Ames/Northrop M2-F2/F3, the NASA Langley/Northrop HL-10, and the USAF/Martin X-24A (aka SV-5P). These vehicles, beginning in 1966, flew very successfully with the completion of a total of 108 flights and each achieved Mach numbers approaching two and altitudes approaching 100,000 feet; accomplishing the objectives of precision unpowered approach and horizontal landings. The successes of this program had a significant influence on the development of the Space Shuttle.

Concurrently, the USAF/Martin X-23A (aka SV-5D) hypersonic sub-scale vehicle validated its high-speed maneuvering cross-range capability in 1967 from a simulated orbital track and entry point at a velocity of about 15,408 mph, but with a less than desired L/D ratio. Thus, answering the final question for lifting bodies of hypersonic flight-path control.

The questions posed in 1964 and prior for piloted and unmanned lifting bodies have been answered by the demonstrated capability of the trio of lifting bodies for the piloted rocket-powered low-supersonic mission that provided the mechanics of executing precision unpowered approach and horizontal landings. In addition, the X-23A high-speed vehicle demonstrated that a simulated orbital entry at hypersonic speed could indeed employ lift to reduce peak deceleration of reentry and provide flight-path control for cross range maneuvering. Thus the full spectrum of atmospheric return to Earth from orbital flight and maneuvering to desired landing locations without the requirement for engine power has been demonstrated.

What then of the future? Take the NASA/USAF/Boeing X-37 as an example; this is a current pilotless winged hypersonic reusable orbital vehicle. This vehicle has been operating successfully during the early 21st century as a subsonic flight-test vehicle, the X-37A, and later its primarily role, the X37B, as a hypersonic orbital vehicle with atmospheric entry flight capability including autonomous high Mach number maneuvering and subsonic selected airport/runway automatic landings. Several flights of the X-37B have been completed and some for exceptional long duration and unannounced objectives. From what we know the X-37 has been a very successful program.

The physical characteristics of the X-37 include its: length of 29.25-feet (8.92-m) long, wing-span of 14.92-feet (4.55-m), has a 'V' tail probably for roll, pitch and yaw aerodynamic control and reaction control rockets for use above 300,000 feet, is powered by an internal Aerojet AR2-3 liquid rocket, is battery and solar panel powered, constructed of advanced materials, with a launch weight of about 11,000 pounds (4,990kg), and is launched atop an Atlas V rocket as a first stage.

Beyond the year 2015, we can only dream. ✈

ACRONYMNS AND SYMBOLS

β	angle-of-sideslip, deg; wind on the right cheek is positive (as pilots like to say).
C_L	coefficient of lift
C_D	coefficient of drag
$C_{l\beta}$	effective dihedral dihedral derivative, deg^{-1}
$C_{n\beta}$	directional–stability derivative, deg^{-1}
ϕ	bank angle, deg; right bank is positive.
p	roll angular rate, deg/sec; right roll rate is positive.
r	yaw angular rate, deg/sec; right yaw rate is positive.
δ_{LS}	pilot's stick lateral deflection, in degrees of aileron; right stick is positive.
$C_{n_{\delta\alpha}}$	yawing moment coefficient due to aileron deflection derivative, deg^{-1}
g	gravitational constant, ft/sec^2
K_{ARI}	gain setting of aileron-to-rudder interconnect, deg/deg
LTI	linear time invariant differential equation
q	dynamic pressure, lb/ft^2; other places perhaps pitch angular rate deg/sec.
t	time in seconds
V_c	calibrated airspeed, knots

REFERENCES

1. Anon., *NACA Conference on High-speed Aerodynamics—A Compilation of Papers Presented*, Ames Aeronautical Laboratory, Moffett Field, Calif., NACA TM X-67369, March 1958. (NASA Document ID 19710069971, Accession No. 71N75285.)*
2. John G. McTigue, FRC Program Manager, et al, *Lifting-Body Research Vehicles in a Low-Speed Flight-Test Program, International Congress of Subsonic Aeronautics*, New York, N.Y., April 1967.
3. R. Dale Reed, et al *Wingless Flight—The Lifting Body Story*, NASA History Office, Washington, DC, 1997, NASA SP-4220.
4. Milton O. Thompson, et al, *Flying Without Wings—NASA Lifting Bodies and the Birth of the Space Shuttle*, Smithsonian Institution Press, Washington and London1999.
5. Richard P. Hallion, *On The Frontier—Flight Research at [NASA] Dryden, 1946—1981*, The NASA History Series, NASA SP-4303, 1984, Washington, DC.

EPILOGUE: THE END OF AN AUDACIOUS SAGA

The winged class (reference 1) X-24B flew its 36th and last flight on November 26, 1975, and with this, the curtain closed on the last of the flight-test experimental rocket propelled research airplanes and vehicles at Edwards Air Force Flight Test Center. Now ‘Flight’ has been excluded from the name. The X-24B was the last and final chapter in the book to be written.

The X-24B over Rogers Dry Lake, circa 1975. (Photo from NASA, EC75-4642)

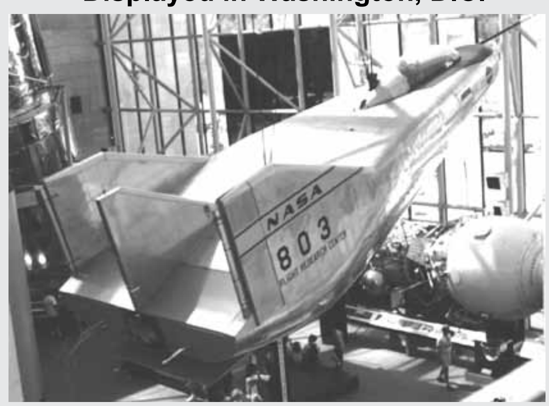
The X-24B about to extend the gear for landing on Rogers Dry Lake runway 18. The X-24B is on display at the National Museum of the USAF, Dayton, Ohio. (Photo from NASA, FRC ECN-4916)

REMEMBRANCES OF THE LIFTING BODIES

Participating Pilots

Pictured, with the HL-10 from left-to-right, is USAF Major Jerry Gentry, Major Pete Hoag, and NASA test pilots John Manke and Bill Dana. (Photo from NASA, FRC E-21539)

Displayed in Washington, D.C.



The M2-F3 on display in the Air & Space Museum. (Photo from the author’s collection)

On Rogers Dry Lake, October 5,1972, the 100th flight of a lifting body at AFFTC, the M2-F3’s 35th flight. Bill Dana, the pilot of the flight is left of the sign. Far left is R. Dale Reed, father of the lifting body program, second from left is the author, Robert W. Kempel with Milt Thompson, retired X-15 test pilot. (Photo from NASA E-25161)

6. Richard P. Hallion, *The Path to the Space Shuttle*: *The Evolution of Lifting Reentry Technology*, Journal of the British Interplanetary Society, [Volume] 36, No. 12 (December 1983), p. 523-541.
7. Anon., *Flight Test Results Pertaining To The Space Shuttlecraft*, compilation of papers from a symposium at NASA Flight Research Center, Edwards, Calif., June 30, 1970. NASA TM-X-2101, publication dated Oct. 1, 1970.
8. Dennis R. Jenkins, *National Space Transportation System*, Walsworth Publishing Co., Marceline, Mo., 1992.
9. Irving L. Ashkenas, et al, *Approximate Airframe Transfer Functions and Applications to Single Sensor Control Systems,* Wright Air Development Center, Technical Report 58-82 (ASTIA No. AD 151025) 1958.
10. Weneth D. Painter, et al, *HL-10 Lifting Body Flight Control System Characteristics And Operational Experience,* NASA Flight Research Center, Edwards, Calif., NASA TM X-2956, January 1974.
11. Robert W. Kempel, et al, *Developing and Flight Testing the HL-10 Lifting Body: A Precursor to the Space Shuttle*, Dryden Flight Research Center, Edwards, Calif., NASA Reference Publication, RP-1332, April 1994.
12. Gene L. Waltman, *Black Magic and Gremlins: Analog Flight Simulations at NASA's Flight Research Center*, NASA SP-2000-4520, Monographs in Aerospace History, No.20, Washington, DC, 2000.
13. Robert W. Kempel and John Manke, *Flight Evaluation of HL-10 Lifting Body Handling Qualities at Mach Numbers from 0.30 to 1.86*, NASA Flight Research Center, Edwards, Calif., NASA TN D-7537, January 1974.
14. Robert W. Kempel and Ronald C. Thompson, *Flight Determined Aerodynamic Stability And Control Derivatives Of The M2-F2 Lifting Body Vehicle At Subsonic Speeds*, NASA Flight Research Center, Edwards, Calif., NASA TM X-2413, December 1971.
15. Robert W. Kempel, *Analysis Of A Coupled Roll-Spiral-Mode, Pilot Induced Oscillation Experienced With The M2-F2 Lifting Body*, NASA Flight Research Center, Edwards, Calif., NASA TN-6496, 1971.
16. Lawrence W. Taylor Jr., *Analysis Of A Pilot-Airplane lateral Instability Experienced With The X-15 Airplane*, NASA Flight Research Center, Edwards, Calif., NASA TN D-1059, November 1961.
17. Robert W. Kempel, William H. Dana, et al, *Flight Evaluation of M2-F3 Lifting Body Handling Qualities at Mach Numbers from 0.30 to 1.61*, NASA Flight Research Center, Edwards, Calif., NASA TN D-8027, July 1975.

* All unclassified NASA or NACA reports are usually available at NASA Scientific and Technical Information (STI) using the NTRS, NASA Technical Reports Server Program-- http://www.sti.nasa.gov Begin with entering *just the report title* or NASA (or NACA) designation and number such as NASA TN-6496 or NAS RP-1332 or NASA SP-2000-4520, etc into the dialog box and work from there; also see the footnote on page 7.

INFORMATION FOR AUTHORS

Manuscripts can be on a **CD** or **Floppy Disk**. Macintosh or Windows if it has been purged of viruses. A hard copy of the Manuscript with separate Captions, References or Footnotes, Authors Notes, etc., must accompany the CD or Floppy Disk.

- **Manuscripts** typed on one side of 8 1/2 x 11-inch paper. Include author's name on first page.
- **Computer printouts** fine, but please NO fancy type fonts, and no second-generation copies! Your copy is scanned by computer to generate type for printing your article. Italics or fancy fonts are not recognized by the scanner program and errors abound. This increases time required to produce the Journal and increases the cost.
- Dot matrix and typewriter of Arial/Helvetica/Geneva fonts read well, but please, ragged right margins for word spacing.
- **Captions**, References, bibliography and author's biography should follow manuscript.
- **Prefer photos** to be glossy but not necessary. Color photos are fine. If you are providing scanned images (we prefer to scan ourselves) they must be highest quality JPEG and at least 300 dpi resolution. For example, an 8"x10" print would be 2400 x 3000 pixels in size. ***Please DO NOT provide prints made on ink jet printers - send the digital image*.**
- **Adequate captions** should be furnished including credit (original owner/photographer) followed by the lender. Match the caption number with the photo number.
- **Do NOT** attach captions to back of photos—for scanning they must be in manuscript form.

While every effort will be made to safeguard submissions, AAHS assumes no responsibility for the loss of materials. If original photos are of precious nature, it is suggested duplicate prints be furnished.

Journal articles are accepted from AAHS members only except in instances where the editorial staff solicits a journalistic piece of meritorious quality. All articles must have an American flavor. For example, American personnel or planes in foreign service or foreign planes or personnel in U.S. service are acceptable. Not accepted are articles unrelated to America.

A particular subject should not normally be longer than two parts. Exceptions may be made where the subject matter has outstanding merit. Ideal manuscript length for a single insertion is 30 pages, including references, etc. For style and form consult recent Journals.

Please include brief author's biography written in third person, together with author's photo.