Neil Armstrong, amongst others, has called the X-15 “the most successful research airplane in history.” That might be stretching a point, but it was certainly the most successful of the high-speed X-planes.

It had taken 44 years to go from Kitty Hawk to Air Force Captain Charles E. Yeager’s first supersonic flight in the Bell X-1 on 14 October 1947. Six more years were required before NASA test pilots Scott Crossfield got to Mach 3 in the Navy-Douglas D-558-2 Skyrocket. A relatively short three years had passed when Captain Milburn G. Apt crossed the X-2 above Mach 3, before tumbling out of control to his death. There, progress stalled, awaiting the arrival of the three small, black North American X-15 research airplanes that would more than double the speed and altitude milestones.

The X-15 flight program began slowly, mostly because the million-horsepower XLR99 engine was not ready. This undeniably worked in the program’s favor since it forced the engineers and pilots to gain experience with the airplane and its systems prior to pushing the envelope too fast. The first 20 months took the X-15 from Crossfield’s glide flight to essentially duplicating the performance of the X-2; Mach 3.5 and 136,300 feet. Then the XLR99 arrived and things got serious. Six days after the last flight with the interim XLR11s, Major Robert M. White took X-15-2 past Mach 4, the first time a piloted aircraft had flown that fast. Mach 5 fell, also to Bob White, four months later. Mach 6, again to White, took six more months. Once it began flying with the ultimate engine, it took only 13 flights to double the maximum Mach number achieved by the X-2.

Altitude was a similar story. Captain Iven C. Kincheloe, Jr. was the first person to fly above 100,000 feet, in the X-2 on 7 September 1956. Thirteen flights with the big engine allowed Bob White to break 200,000 feet for the first time. Three months later, he broke 300,000 feet. Once it began flying with the ultimate engine, the X-15 took only 19 months to double the maximum altitude achieved by the X-2. Ultimately, during its 199 flights, the X-15 recorded a maximum altitude of 354,200 feet and a maximum speed of 4,520 mph (Mach 6.7). They were stunning achievements.
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FOREWORD: WILLIAM H. DANA

The X-15 was an airplane of accelerations. When an X-15 pilot looks back on his X-15 flights, it is the accelerations he remembers. The first of these sensations was the acceleration due to B-52 lift, which held the X-15 at launch altitude and prevented it from falling to Earth. When the X-15 pilot hit the launch switch, the B-52 lift was no longer accessible to the X-15. The X-15 fell at the acceleration due to Earth’s gravity, which the pilot recognized as “free fall” or “zero g.” Only when the pilot started the engine and put some “g” on the X-15 was this sensation of falling relieved.

The next impression encountered on the X-15 flight came as the engine lit, just a few seconds after launch. A 33,000-pound airplane was accelerated by a 57,000-lbf engine, resulting in a chest-to-back acceleration of almost 2 g. Then, as the propellant burned away and the atmosphere thinned with increasing altitude, the chest-to-back acceleration increased and the drag caused by the atmosphere lessened. For a standard altitude mission (250,000 feet), the weight and thrust were closer to 15,000 pounds and 60,000-lbf at shutdown, resulting in almost 4-g chest-to-back acceleration. The human body is not stressed for 4 g chest to back, and by shutdown the boost was starting to get a little painful. Milt Thompson once observed that the X-15 was the only aircraft he had ever flown where he was glad when the engine quit.

On a mission to high altitude (above 250,000 feet), the pilot did not regain any sensible air with which to execute a pullout until about 180,000 feet, and could not pull 1 g of lift until 130,000 feet. Flying a constant angle of attack on reentry, the pilot allowed g to build up to 5, and then maintained 5 g until the aircraft was level at about 80,000 feet. There was a deceleration from Mach 5 at 80,000 feet to about Mach 1 over the landing runway, and the pilot determined the magnitude of the deceleration by the use of speed brakes. This ended the high-g portion of the flight, except for one pilot who elected to start his traffic pattern at 50,000 feet and Mach 2, and flew a 360-degree overhead pattern from that starting point.

Flight to high altitude represented about two-thirds of the 199 X-15 flights. Flights to high speed or high dynamic pressure accounted for the other third, and those flights remained well within the atmosphere for the entire mission. The pilot of a high-speed flight got a small taste of chest-to-back acceleration during the boost (thrust was still greater than drag, but not by such a large margin as on the high-altitude flights). The deceleration after burnout was a new sensation. This condition was high drag and zero thrust, and it had the pilot hanging in his shoulder straps, with perspiration dripping off the tip of his nose onto the inside of his face plate.

Milt Thompson collected anecdotes about the X-15 that remain astonishing to this day. Milt noted that at Mach 5, a simple 20-degree heading change required 5 g of normal acceleration for 10 seconds. Milt also pointed out that on a speed flight, the (unmodified) X-15-1 accelerated from Mach 5 to Mach 6 in six seconds. These were eye-opening numbers at the time of the X-15 program.
Neil Armstrong, among others, has called the X-15 “the most successful research airplane in history.” That might be stretching a point, but it was certainly the most successful of the high-speed X-planes. Given the major advances in materials and computer technology made in the 40 years since the flight program ended, it is unlikely that many of the actual hardware lessons are still applicable. Having said that, the lessons learned from hypersonic modeling and pilot-in-the-loop simulation, and the insight gained by being able to evaluate actual X-15 flight test results against wind-tunnel and theoretical predictions greatly expanded the confidence of researchers during the 1970s and 1980s.¹

It would not have surprised anybody involved that the actual X-15 technology did not find further application. Researchers such as John Becker and Norris Dow, and engineers like Harrison Storms and Charlie Feltz never intended the design to represent anything other than a convenient platform to acquire aero-thermo data. Becker once opined that proceeding with a general research configuration rather than a prototype of a vehicle designed to achieve a specific mission was critical to the ultimate success of the X-15. Had the prototype route been taken, Becker believed, “we would have picked the wrong mission, the wrong structure, the wrong aerodynamic shapes, and the wrong propulsion.” They are good words of advice.²

In fact, the decision to pursue a pure research shape was somewhat controversial at the beginning. Kelly Johnson, for one, believed the vehicle should be adaptable as a strategic reconnaissance aircraft. Indeed, several of the proposals for the X-15 sought to design a vehicle with some future application. Nevertheless, the original Langley concept of a vehicle optimized to collect the desired data as safely as possible ultimately won. As Harley Soulé told Harrison Storms, “You have a little airplane and a big engine with a large thrust margin. We want to study aerodynamic heating. We do not want to worry about aerodynamic stability and control, or the airplane breaking up. So, if you make any errors, make them on the strong side. You should have enough thrust to do the job.”³ North American succeeded brilliantly.³

It had taken 44 years to go from Kitty Hawk to Chuck Yeager’s first supersonic flight in the X-1. Six more years were required before Scott Crossfield got to Mach 2 in the D-558-2 Skyrocket. A remarkably short three years had passed when Mel Apt coaxed the X-2 above Mach 3, before tumbling out of control to his death. There progress stalled, awaiting the arrival of the three small black airplanes that would more than double the speed and altitude milestones.

The X-15 flight program began slowly, mostly because the XLR99 was not ready. This undoubtably worked in the program’s favor since it forced the engineers and pilots to gain experience with the airplane and its systems prior to pushing the envelope too far. The first 20 months took the X-15 from Crossfield’s glide flight to essentially duplicating the performance of the X-2: Mach 3.5 and 136,500 feet. Then the XLR99s arrived and things got serious. Six days after the last flight with the interim XLR11s, Bob White took X-15-2 past Mach 4, the first time a piloted aircraft had flown that fast. Mach 5 fell, also to Bob White, four months later. Mach 6, again to White, took six more months. Once the X-15 began flying with the ultimate engine, it took only 15 flights to double the maximum Mach number achieved by the X-2.

Altitude was a similar story. Iven Kincheloe was the first person to fly above 100,000 feet, in the X-2 on 7 September 1956. Thirteen flights with the big engine allowed Bob White to fly above 200,000 feet for the first time. Three months later, he broke 300,000 feet. Once it began flying with the ultimate engine, the X-15 took only 19 months to double the maximum altitude achieved by the X-2. These were stunning achievements.

It is interesting to note that although the X-15 is generally considered a Mach 6 aircraft, only two of the three airplanes ever flew that fast, and then only four times. On the other hand, 108 other flights exceeded Mach 5, accumulating 1 hour, 25 minutes, and 33 seconds of hypersonic flight. At the other end of the spectrum, just two flights were not supersonic (one of these was the glide flight), and only 14 others did not exceed Mach 2. It was a fast airplane. Similarly, there were only four flights above 300,000 feet (all by X-15-3), but only the initial glide flight was below 40,000 feet.⁴

Despite appearances, however, the program was not about setting records.⁵ The actual speed and altitude achieved by the program was not the ultimate test, and the fact that the basic airplane never achieved its advertised 6,600 feet per second velocity was of little consequence. What interested the researchers was the environment in which the airplane flew. They wanted to study dynamic pressures, heating rates, and total temperatures. More specifically, the goals were to:

1. Verify existing (1954) theory and wind-tunnel techniques
2. Study aircraft structures and stability and control under high (2,000 psi) dynamic pressures
3. Study aircraft structures under high (1,200°F) heating
4. Investigate stability and control problems associated with high-altitude boost and reentry
5. Investigate the biomedical effects of both weightless and high-g flight

The X-15 achieved all of these design goals, although Project Mercury and other manned space efforts quickly eclipsed the airplane’s contribution to

¹ The Armstrong quote is in the foreword to Milton O. Thompson, At the Edge of Space: the X-15 Flight Program (Washington, DC: Smithsonian Institution Press, 1992), p. xi.
⁴ In the 3rd Eugen Sänger Memorial Lecture in 1968, John Becker stated that 109 flights exceeded Mach 5. A reevaluation of the flight data shows that only 81 actually did. See Becker, “The X-15 Program in Retrospect,” p. 3 for Becker’s original numbers.
⁵ Despite all that is written, the program held only few “official” records, mainly because it seldom invited the FAA out to witness the flights. In fact, it appears that the 314,750-foot-altitude record set by Bob White is the only official record ever set by the program.
American made some adjustments and launched Crossfield again three months later. It was a short-lived reprieve. Less than 60 days later, Crossfield broke the back of X-15-2 during a hard landing that followed an in-flight abort. Instead of canceling the program, the X-15 went back to the factory for repair. Three months later Crossfield was flying again.

During the initial ground-testing of the ultimate XLR99 engine in X-15-3 at Edwards, an explosion destroyed the airplane. Nobody was seriously hurt and North American subsequently rebuilt the airplane with an advanced flight control system intended for the stillborn X-20 Dyna-Soar. The program was flying two months later using X-15-1 and the rebuilt X-15-3 went on to become the high-altitude workhorse.

It was the same across the board. When Jack McKay made his emergency landing at Mud Lake that essentially destroyed X-15-2, the Air Force did not cancel the program. Five weeks later Bob White made a Mach 5.65 flight in X-15-3; McKay was his NASA-1. North American rebuilt X-15-2 and the airplane began flying again 18 months later. Jack McKay went on to fly 22 more X-15 flights, although the lingering effects of his injuries shortened his lifetime considerably.

In each case the program quickly analyzed the cause of the failure, instituted appropriate changes, and moved on. Always cautious, never reckless. No prolonged down times. No thought of cancellation. It would not happen that way today. One of the risks when extending any frontier is that you do not understand all the risks.

Paul Bikle, the director of the Flight Research Center, had long warned that the flight program should end when it achieved the design speed and altitude. However, the X-15s provided an ideal platform for follow-on experiments that had little or nothing to do with the design aero-thermo research mission. The temptation was too great, and NASA extended the flight program several years. Bikle knew that eventually the odds would catch up with the program. The day they did, Mike Adams was at the controls of X-15-3, and the consequences were as bad as anything Bikle could have imagined. The crash killed Mike Adams and destroyed X-15-3. Even so, the program made sure it learned from the accident and was flying again less than four months later. This time, however, it would not be for long. Eight more flights were conducted before the program ended when funding expired at the end of 1968.

John Becker, arguably the father of the X-15, once stated that the project came along at “the most propitious of all possible times for its promotion and approval.” At the time, it was not considered necessary to have a defined operational program in order to conduct basic research. There were no “glamorous and expensive” manned space projects to compete for funding, and the general feeling within the nation was one of trying to go faster, higher, or further. In today’s environment, as in 1968 when Becker made his comment, it is highly unlikely that a program such as the X-15 could gain approval.6

Dill Hinley, a former DFRC historian, once opined that “This situation should give pause to those who fund aerospace projects solely on the basis of their presumably predictable outcomes and their expected cost effectiveness. Without

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6 Ronald G. Boston, “Outline of the X-15’s Contributions to Aerospace Technology,” 21 November 1977. Unpublished preliminary version of the typescript available in the NASA Dryden History Office. For those interested in Boston’s original paper, the easiest place to find a copy is in the Hypersonic Revolution, republished by the Air Force History and Museums program. It constitutes the last section in the X-15 chapter. Latter, William H. Dana, Chief, Flight Crew Branch, DFRC, to Las Saegesser NASA History Office, transmitting a copy of the SETP paper for the file. A slightly rewritten (more politically correct) version of the paper was later published as The X-15 Airplane—Lessons Learned (American Institute of Aeronautics and Astronautics, a paper prepared for the 31st Aerospace Sciences Meeting, Reno Nevada, AIAA-93-0309, 11-14 January 1993). Boston listed 1,300°F as the maximum temperature, but Bill Dana reported 1,350°F in his SETP and AIAA papers. Boston also listed the max q as 2,000 psf, but in reality it was 2,023 psf on Flight 1-66-111.

7 Storms, “X-15 Hardware Design Challenges,” pp. 32-33

8 Becker, “The X-15 Program in Retrospect,” pp. 1-2
the X-15’s pioneering work, it is quite possible that the manned space program would have been slowed, conceivably with disastrous consequences for national prestige.” It is certain that the development of the Space Shuttle would have carried a far greater risk if not for the lessons learned from the development and flight-testing of the X-15. Fifty years later, the X-15 experience still provides the bulk of the available hypersonic data available to aircraft designers.9

Perhaps we have not learned well enough.

Dennis R. Jenkins
Cape Canaveral, Florida

ACKNOWLEDGMENTS

Robert S. Houston, a historian at the Air Force Wright Air Development Center, wrote the most frequently quoted X-15 history in 1959. This narrative, unsurprisingly, centered on the early Air Force involvement in the program, and concentrated mostly—as is normal for Air Force histories—on the program management aspects rather than the technology. Dr. Richard P. Hallion, later the chief historian for the U.S. Air Force, updated Houston’s history in 1987 as part of volume II of The Hypersonic Revolution, a collection of papers published by the Aeronautical Systems Office at Wright-Patterson AFB. Hallion added coverage of the last nine years of the program, drawing mainly from his own On the Frontier: Flight Research at Dryden, 1946-1981 (Washington, DC: NASA, 1984) and “Outline of the X-15’s Contributions to Aerospace Technology,” written in 1977 by Ronald G. Boston. These historians did an excellent job, but unfortunately their work received comparatively limited distribution.

I began this history by using these earlier works as a basis, checking the sources, expanding upon them as appropriate, and adding a NACA/NASA and Navy perspective. Amazingly, almost all of the original source documentation still existed in one archive or another, allowing an evaluation of the tone and inflection of some of the earliest material. Although it is largely a new work, anybody who is intimately familiar with the earlier histories will recognize some passages—the original historians did a remarkably thorough job.

Many people assisted in the preparation of this work, and all gave generously and freely, well beyond any reasonable expectation an author might have. Foremost were Betty J. Love, Tony Landis at Dryden, and Dr. Roger D. Launius at the National Air and Space Museum. The surviving X-15 pilots—Neil A. Armstrong, A. Scott Crossfield, William H. Dana, Brigadier General Joe H. Engle (USAF, Retired), Colonel William J. “Pete” Knight (USAF, Retired), and Major General Robert M. White (USAF, Retired)—contributed immensely, and several of them read the manuscript multiple times to ensure that nothing significant was missed or misrepresented. John V. Becker and Charles H. Feltz spent many hours explaining things I probably should have already known, greatly improving the manuscript. Then there are the flight planners—Johnny G. Armstrong,10 Richard E. Day, and Robert G. Hoey. I would have missed many subtleties without the patient tutoring from these engineers, all of whom read and commented on several versions of this manuscript and continued my education well past my two engineering degrees.

There was correspondence with many individuals who had been involved with the program: William P. Albrecht, Colonel John E. “Jack” Allavie (USAF, Retired), Colonel Clarence E. “Bud” Anderson (USAF, Retired), Bill Arnold (RMD/Thiokol, Retired), Colonel Charles C. Bock, Jr., (USAF, Retired), Jerry Brandt, Richard J. Harer, Gerald M. Truszynski, and Alvin S. White. In addition,


10 Officially, Johnny Armstrong (who is now the chief engineer in the Hypersonic Flight Test Team) maintains the AFFTC Hypersonic Flight Test Team Project Files and is, fortunately, something of a pack rat. However, to everybody at Edwards and Dryden, this wonderful collection is simply the Armstrong Memorial Library.
Jack Bassick at the David Clark Company, Stephen J. Garber and Colin A. Fries at the NASA History Office, Michael J. Lombardi at the Boeing Company Archives, Air Force Chief Historian Dr. Richard P. Hallion, Dr. James H. Young and Cheryl Gumm at the AFFTC History Office, and John D. “Jack” Weber at the AFMC History Office all provided excellent support. Friends and fellow authors Gerald H. Balzer, Robert E. Bradley, Benjamin F. Guenther, Scott Lowther, Mike Machat, Michael Moore, Terry Panopalis, and Mick Roth also assisted.

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Special Thanks

I owe a particular mention of Jay Miller, author of the popular *The X-planes: X-1 to X-45*, (Hinckley, England: Midland Publishing, 2001), among many other works. Anybody interested in reading about the other X-planes should pick up a copy of this excellent book. Jay was responsible for the first photograph I ever had published, and published my first book—a short monograph on the Space Shuttle. Somehow, I feel I have him to blame for the quagmire of aerospace history I find myself embroiled in. I truly appreciate the help and friendship from Jay and his lovely wife Susan over the past 25 years or so.

Thankfully, my mother, Mrs. Mary E. Jenkins, encouraged me to seize opportunities and taught me to write and type—such necessary attributes for this endeavor. As for so many things, I owe her a great deal of gratitude, along with my everlasting love and admiration. After listening to my trials and tribulations about this project for a decade, she passed away before publication. I hope she has found the peace and rest she so richly deserves.

A note regarding terminology: In the days before being politically correct became a prime influence on engineering and history, engineers called piloted vehicles “manned” aircraft, and the process of making them safe enough to fly was termed “man-rating.” This work continues to use these terms since they are what were in use at the time.
A New Science

The first 50 years of powered human flight were marked by a desire to always go faster and higher. At first, the daredevils—be they racers or barnstormers—drove this. By the end of the 1930s, however, increases in speed and altitude were largely the province of government—the cost of designing and building the ever-faster aircraft was becoming prohibitive for individuals. As is usually the case, war increased the tempo of development, and two major conflicts within 30 years provided a tremendous impetus for advancements in aviation. By the end of World War II the next great challenge was in sight: the “sound barrier” that stood between the pilots and supersonic flight.

Contrary to general perception, the speed of sound was not a discovery of the 20th century. Over 250 years before Chuck Yeager made his now-famous flight in the X-1, it was known that sound propagated through air at some constant velocity. During the 17th century, artillerymen determined that the speed of sound was approximately 1,140 feet per second (fps) by standing a known distance away from a cannon and using simple timing devices to measure the delay between the muzzle flash and the sound of the discharge. Their conclusion was remarkably accurate. Two centuries later the National Advisory Committee for Aeronautics1

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1 In an unusually far-sighted move, on 3 March 1915 Congress passed a public law establishing an “Advisory Committee for Aeronautics.” As stipulated in the act, the purpose of this committee was “to supervise and direct the scientific study of the problems of flight with a view to their practical solution” and to “direct and conduct research and experiment in aeronautics.”
(NACA) defined the speed of sound as 1,117 fps on an ISO standard day, although this number is for engine convenience and does not represent a real value.²

The first person to recognize an aerodynamic anomaly near the speed of sound was probably Benjamin Robins, an 18th-century British scientist who invented a ballistic pendulum that measured the velocity of cannon projectiles. As described by Robins, a large wooden block was suspended in front of a cannon and the projectile was fired into it. The projectile transferred momentum to the block, and the force could be determined by measuring the amplitude of the pendulum. During these experiments, Robins observed that the drag on a projectile appeared to increase dramatically as it neared the speed of sound. It was an interesting piece of data, but there was no practical or theoretical basis for investigating it further.³

The concept of shock waves associated with the speed of sound also predated the 20th century. As an object moves through the atmosphere, the air molecules near the object are disturbed and move around the object. If the object passes at low speed (typically less than 200 mph), the density of the air will remain relatively constant, but at higher speeds some of the energy of the object will compress the air, locally changing its density. This compressibility effect alters the resulting force on the object and becomes more important as the speed increases. Near the speed of sound the compression waves merge into a strong shock wave that affects both the lift and drag of an object, resulting in significant challenges for aircraft designers.⁴

Austrian physicist Ernst Mach took the first photographs of supersonic shock waves using a technique called shadowgraphy. In 1877 Mach presented a paper to the Academy of Sciences in Vienna, where he showed a shadowgraph of a bullet moving at supersonic speeds; the bow and trailing-edge shock waves were clearly visible. Mach was also the first to assign a numerical value to the ratio that affects both the lift and drag of an object, resulting in significant challenges for aircraft designers.⁵

None of these experiments had much impact on the airplanes of the early 20th century since their flight speeds were so low that compressibility effects were effectively nonexistent. However, within a few years things changed. Although the typical flight speeds during World War I were less than 125 mph, the propeller tips, because of their combined rotational and translational motion through the air, sometimes approached the compressibility phenomenon.⁶

To better understand the nature of the problem, in 1918 G. H. Bryan began a theoretical analysis of subsonic and supersonic airflows for the British Advisory Committee for Aeronautics at the Royal Aeronautical Establishment. His analysis was cumbersome and provided little data of immediate value. At the same time, Frank W. Caldwell and Elisha N. Fales from the Army Air Service Engineering Division, at McCook Field in Dayton, Ohio, took a purely experimental approach to the problem.⁷ To investigate the problems associated with propellers, in 1918 Caldwell and Fales designed the first high-speed wind tunnel built in the United States. This tunnel had a 14-inch-diameter test section that could generate velocities up to 465 mph, which was considered exceptional at the time. This was the beginning of a dichotomy between American and British research. Over the next two decades the United States—primarily the NACA—made most of the major experimental contributions to understanding compressibility effects, while the major theoretical contributions were made in Great Britain. This combination of American and British investigations of propellers constituted one of the first concerted efforts of the fledgling aeronautical community to investigate the sound barrier.⁸

Within about five years, practical solutions, such as new thin-section propeller blades (made practical by the use of metal instead of wood for their construction) that minimized the effects of compressibility, were in place. However, most of the solution was to avoid the problem. The development of reliable reduction-gearing systems and variable-pitch, constant-speed propellers eliminated the problem entirely for airplane speeds that were conceivable in 1925 because the propeller could be rotated at slower speeds. At the time, the best pursuit planes (the fore-runners of what are now called fighters) could only achieve speeds of about 200 mph, and a scan of literature from the mid-1920s shows only rare suggestions of significantly higher speeds in the foreseeable future. Accordingly, most researchers moved on to other areas.⁹

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⁶ Becker, The High-Speed Frontier, pp. 3-5. For more see John William Strutt (the Third Baron Rayleigh), The Theory of Sound, a landmark of acoustics originally published in 1877. An online version is available at http://www.meas ure.demon.co.uk/otokar/troja. mff.cuni.cz/RELATGRP/Mach.htm.

⁷ On 18 October 1917, the U.S. Army established McCook Field outside Dayton as the military aviation research and development site, based largely on its proximity to the American aviation industry (i.e., the Wright brothers). However, within 10 years the facility had become too small and offered no room for expansion. The citizens of Dayton, not wanting to lose the attractive, collected donations and purchased 4,000 acres of land they subsequently donated to the government. The Army dedicated the new Wright Field on 12 October 1927. On 1 July 1951, the portion of Wright Field east of Huffman Dam was redesignated Patterson Field in honor of Lieutenant Frank Stuart Patterson. Patterson Field was the home of Air Force logistics; Wright Field was the home of research and field support. The adjacent Wright Field and Patterson Field were again joined on 13 January 1948 to become Wright-Patterson AFB. However, most development activities continued on the “Wright Field” part of the base, and most contemporary literature (and official correspondence) generally called it Wright Field until the late 1950s.


⁹ Becker, The High-Speed Frontier, pp. 6-7. Surprisingly, one 1924 French document envisioned aircraft flying at Mach 0.8 or more by 1930, as well as the development of some wholly new but unspecified type of propulsion and appropriate new high-speed wind tunnels to support these developments. See the English translation of La Technique Aeronautique, December 1904, by E. Huguerard, “High-Speed Wind Tunnels,” NACA Technical Memorandum 318, 1925.
The public belief in the “sound barrier” apparently had its beginning in 1935 when the British aerodynamicist W. F. Hilton was explaining to a journalist about high-speed experiments he was conducting at the National Physical Laboratory. Pointing to a plot of airfoil drag, Hilton said, “See how the resistance of a wing shoots up like a barrier against higher speed as we approach the speed of sound.” The next morning, the leading British newspapers were referring to the “sound barrier,” and the notion that airplanes could never fly faster than the speed of sound became widespread among the public. Although most engineers refused to believe this, the considerable uncertainty about how significantly drag would increase in the transonic regime made them wonder whether engines of sufficient power to fly faster than sound would ever be available.\(^\text{10}\)

Since the beginning of powered flight, wind tunnels had proven to be useful tools, but it appeared in the 1930s that simulation of the transonic regime was not possible due to the physical characteristics of the test sections. However, the beginning of the Second World War increased the urgency of the research. Therefore, on a spring morning in 1940, John V. Becker and John Stack, two researchers from the NACA Langley Memorial Aeronautical Laboratory in Hampton, Virginia,\(^\text{11}\) drove to a remote beach to observe a Navy Brewster XF2A-2 attempting to obtain supercritical aerodynamic data in free flight over Chesapeake Bay. After it reached its terminal velocity in a steep dive—about 575 mph—the pilot made a pull-up that was near the design load factor of the airplane. This flight did not encounter any undue difficulties and provided some data, but the general feeling was that dividing some data, but the general feeling was that developing an operational-type airplane near its structural limits was probably not the best method of obtaining research information.\(^\text{12}\)

**X-PLANES**

As it happened, John Stack had already considered other alternatives. The idea of a modern research airplane—one designed strictly to probe unknown flight regimes—came in a 1933 proposal by Stack. On his own initiative, Stack went through a preliminary analysis for “a hypothetical airplane which, however, is not beyond the limits of possibility” to fly well into the compressibility regime. Stack calculated that a small airplane using a 2,300-horsepower Rolls-Royce piston engine could obtain 566 mph in level flight—far beyond that of any airplane flying at the time. Ultimately, the NACA did not pursue the suggestion, and it would be another decade before the idea would come of age.\(^\text{13}\)

Ezra Kotcher at the Army Air Corps Engineering School at Wright Field made the next proposal for a high-speed research airplane. In 1939 Kotcher pointed out the unknown aspects of the transonic flight regime and the problems associated with the effects of compressibility. He further discussed the limitations of existing wind tunnels and advised that a full-scale flight research program would be an appropriate precaution. By early 1941 John Stack had confirmed that data from wind tunnels operating near Mach 1 were essentially worthless because of a choking problem in the test section. He again concluded that the only way to gather meaningful data near the speed of sound would be to build a vehicle that could fly in that regime. Again, no action resulted from either Kotcher’s or Stack’s suggestions and determining the effects of compressibility on airplanes remained a largely theoretical pursuit.\(^\text{14}\)

The real world intervened in November 1941 when Lockheed test pilot Ralph Virden died trying to pull a P-38 Lightning out of a high-speed dive that penetrated well into the compressibility regime. By 1942 the diving speed of the new generation of fighters exceeded the choking speed of the wind tunnels then in use. Researchers increasingly supported the idea of an instrumented airplane operating at high subsonic speeds. Those involved do not remember that any one individual specifically championed this idea, but John Stack soon became the chief Langley proponent.\(^\text{15}\)

Interestingly, there was little interest within the NACA in flying through the sound barrier. It appeared that one of the early jet engines could push a small airplane to about Mach 0.9, but the only near-term way to go faster was to use a rocket engine—something that was considered too risky by the NACA.

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11. In July 1948 the word “Memorial” was dropped and the facility became the Langley Aeronautical Laboratory. It would subsequently be renamed the Langley Research Center (LaRC) when NASA came into existence on 1 October 1958. John Stack (1906-1972) graduated from MIT in 1929 and joined the Langley Aeronautical Laboratory as an aeronautical engineer. In 1939 he became director of all high-speed wind tunnels and high-speed airflow research at Langley. Three years later he became chief of the Compressibility Research Division there, was promoted to assistant chief of research in 1947, and subsequently had that title changed to assistant director of the research center. He guided much of the research that paved the way for transonic aircraft, and in 1947 he was awarded the Collar Trophy together with the pilot of the X-1 who broke the sound barrier (by then) Major Charles E. Yeager. He won the award again in 1952 and later won the Wright Brothers Memorial Trophy, among other awards. From 1961 to 1962 he was director of aeronautical research at NASA Headquarters before retiring from NASA to become vice president for engineering at Republic Aircraft Corp. (later part of Fairchild Industries), from which he retired in 1971.


14. Hansen, Engineer in Charge, p. 299. Choking was primarily a transonic issue, since over a small model in the test section could act as an obstruction that prevented the calculated mass of air from flowing through. Some models also produced shock waves that extended almost perpendicular to the flow, reflecting off the tunnel walls and impinging back on the model or instrumentation. Such an effect meant that data from the tests were largely worthless.

The Army, however, wanted a supersonic airplane and appeared willing to accept rocket propulsion. In fact, Ezra Kotcher had listed this as an option in his 1939 proposal, and it became increasingly obvious that a rocket engine represented the only hope for achieving supersonic speeds in level flight in the near future.16

Possible Navy interest in the undertaking also appeared during 1942–1944. However, significant differences of opinion came to the forefront during a 15 March 1944 meeting of Army, NACA, and Navy personnel. The NACA thought of the airplane as a facility for collecting high-subsonic speed aerodynamic data that were unobtainable in wind tunnels, while the Army thought it was a step toward achieving a supersonic combat aircraft. The Navy supported both views, wanting to dispel the myth of the impenetrable sound barrier, but was also interested in gathering meaningful high-speed data. Despite the NACA’s concerns, the Army soon announced its intention to develop a rocket-powered research airplane.17

As John Becker remembers, “The NACA continued to emphasize the assumed safety aspects and relatively long-duration data-gathering flights possible with a turbojet engine compared to the short flights of any reasonably sized research airplane. Furthermore, the turbojet would have obvious applicability to future military aircraft while the rocket propulsion system might not. This apparently irreconcilable difference was easily resolved; the Army was putting up the money and they decided to do it their way.”18

The beginning of supersonic flight research likely occurred when Robert J. Woods from Bell Aircraft met with Ezra Kotcher at Wright Field on 30 November 1944. After they discussed the basic specifications, Kotcher asked Woods if Bell was interested in designing and building the airplane. Woods said yes, and in late December Bell began contract negotiations with the Army to build the rocket-powered XS-1 research airplane.19

Melvin N. Gough, the chief test pilot at Langley, dismissed the rocket-plane concept: “No NACA pilot will ever be permitted to fly an airplane powered by a damned firecracker.” When it became clear in early 1944 that the Army was going to insist on rocket propulsion, John Stack began lobbying the Navy to procure the type of airplane the NACA wanted. The Navy was more receptive to the turbojet-powered airplane, and the Navy Bureau of Aeronautics (BuAer) began negotiations with Douglas Aircraft for the D-558 Skystreak in early 1945.20

These were the beginnings of the cooperative research airplane program. In reality, until the advent of the X-15 there were two distinct programs: one with the Army and one with the Navy. Just because the NACA did not agree with the path the Army had elected to pursue did not mean the Agency would not cooperate fully in the development of the XS-1. The Navy enjoyed the same level of cooperation for the D-558. John Stack noted in 1951 that “the research airplane program has been a cooperative venture from the start…. The extent of the cooperation is best illustrated by the fact that the X-1, sponsored by the Air Force, is powered with a Navy-sponsored rocket engine, and the D-558-1, sponsored by the Navy, is powered with an Air Force-sponsored turbojet engine.”21

WHAT WAS ACHIEVED?

Initially the primary justification for a manned research airplane was the choking problems of the wind tunnels, but, as it turned out, this limitation disappeared prior to the beginning of high-speed flight tests. Although this largely eliminated the need for the X-planes, it is unlikely that the progress in developing transonic ground facilities would have occurred without the stimulus begun by the X-1 and D-558. Clearly, there was an important two-way flow of benefits.

17 Becker, The High-Speed Frontier, pp. 91–92.
18 Ibid. The quote was slightly edited by John Becker during the preparation of this manuscript.
19 Richard F. Hallion, Supersonic Flight (New York: Macmillan, 1972), p. 34; Becker, The High-Speed Frontier, pp. 91–92. Woods worked at Langley during 1928–1929 but he left the NACA and in 1935 teamed with Lawrence D. Bell to form the Bell Aircraft Corporation in Buffalo, New York. The original designation of the X-1 and X-2 was “XS” for “experimental supersonic.” This was subsequently simplified to just “X” for “experimental.”
20 Becker, The High-Speed Frontier, pp. 92–93. Ironically, it was the turbojet-powered D-558-1 that ultimately killed NACA pilot Howard C. Lilly due to engine failure. With further irony, it was the supersonic flights of the rocket-powered X-1 that brought John Stack and the NACA a share of the Collier Trophy.
Stimulated by the problems encountered by the research airplanes during flight, researchers created new ground facilities and techniques that in turn provided the data necessary to develop yet faster airplanes. Comparing the results of flight tests at ever-increasing speeds allowed the wind tunnels to be refined, producing yet better data. It was a repetitive loop.22

The programs proceeded remarkably rapidly, and the first supersonic flights showed nothing particularly unexpected, much to the relief of the researchers. The most basic result, however, was dispelling the myth of the “sound barrier.” The fearsome transonic zone became an ordinary engineering problem, and allowed the designers of operational supersonic aircraft to proceed with much greater confidence.23

When people think of X-planes, record-setting vehicles like the X-1 generally come to mind. In reality, most X-planes investigated much more mundane flight regimes, and there were only a handful of high-speed manned experimental aircraft, built mainly during the late 1940s and early 1950s. Specifically, there were five designs (only three of which carried X” designations) intended for the initial manned assault on high-speed flight: the Bell X-1 series, the Bell X-2, the Douglas D-558-1 Skystreaks, the Douglas D-558-2 Skyrockets, and the North American X-15. Of the five, one probed high subsonic speeds, two were supersonic, and one pushed the envelope to Mach 3. The fifth design would go much faster.24

The X-planes gave aviation its first experience with controlled supersonic flight. On 14 October 1947, Air Force Captain Charles E. Yeager became the first human to break the sound barrier in level flight when the XS-1 achieved Mach 1.06 at 43,000 feet. It took six additional years before NACA test pilot A. Scott Crossfield exceeded Mach 2 in the D558-2 Skyrocket on 20 November 1953. The Bell X-2 proved to be the fastest and highest-flying of the “round one” X-planes and the most tragic, with the two X-2s logging only 20 glide and powered flights between them. Nevertheless, Captain Iven C. Kincheloe, Jr., managed to take one of the airplanes to 126,200 feet on 7 September 1956. Twenty days later, Captain Milburn G. Apt was killed during his first X-2 flight after he reached Mach 3.196 (1,701 mph), becoming the first person to fly at three times the speed of sound, albeit briefly.25

The contributions of the early high-speed X-planes were questionable, and the subject of great debate within the NACA and the aircraft industry. Opinions on how successful they were depend largely on where one worked. The academ-ics and laboratory researchers, and a couple of aerospace-industry designers, are on record indicating the contributions of the X-planes were minimal. On the other side, however, many of the hands-on researchers and pilots are certain the pro-

23 Becker, The High-Speed Frontier, pp. 93-94.
26 Telephone conversations with Scott Crossfield and John Becker, various dates, plus writings in a multitude of books, letters, and memos. The debate is probably never ending and largely moot since what happened has already happened.
27 The XF-86 officially broke the sound barrier in a shallow dive on 26 April 1948. Some sources maintain that this event actually took place slightly before Yeager’s flight, and Scott Crossfield suggests—as do others—that the first Mach 1 dive by an F-86 occurred “within weeks” of Yeager’s first supersonic flight (telephone conversation, Scott Crossfield with Dennis R. Jenkins, 31 October 2002).
28 The XF-91 was hardly a successful attempt, although it did record the “first supersonic rocket-powred flight by a U.S. combat-type airplane” in December 1952. A single General Electric J47-GE-9J engine and four Curtiss-Wright XR327-CW-1 rocket engines powered the aircraft. The Curtiss-Wright rockets were traded for a Reaction Motors XLR91-RM-9 in the modified XF-91A that apparently was never tested.
29 The first flight of an XF-104 powered by a Wright J65-W-6 engine was on 7 February 1956, but this prototype aircraft was only capable of Mach 1.79. The General Electric J79-GE-5 powered XF-104A exceeded Mach 2 on 27 April 1966.
The X-1E, the last rocket-powered X-plane at the NACA High-Speed Flight Station until the arrival of the three X-15s. There is considerable debate over the economics of flying the X-1E given that some jet-powered aircraft could attain the same velocities, but the primary purpose of the X-1E was to maintain a cadre of rocket experience at the HSFS pending the arrival of the X-15. (NASA)

ignited a billion-dollar race to build ever-faster aircraft, and directly affected every combat aircraft design for the next two decades. However, a few aeronautical researchers had always been certain that the sound barrier was simply a challenge for the engineers, not a true physical limitation. The X-1 had proven it was possible for humans to fly supersonically. The next goal was so much faster.

HYPERSONICS

Hypersonic. Adj. (1937). Of or relating to velocities in excess of five times the speed of sound.

Between the two world wars, hypersonics was an area of great theoretical interest to a small group of aeronautical researchers, but little progress was made toward defining the possible problems, and even less in solving them. The major constraint was power. Engines, even the rudimentary rockets then available, were incapable of propelling any significant object to hypersonic velocities. Wind tunnels also lacked the power to generate such speeds. Computer power to simulate the environment had not even been imagined. For the time being, hypersonics was something to be contemplated, and little else.

By the mid-1940s it was becoming apparent to aerodynamic researchers in the United States that it might finally be possible to build a flight vehicle capable of achieving hypersonic speeds. It seemed that the large rocket engines developed in Germany during World War II might allow engineers to initiate development with some hope of success. Indeed, the Germans had already briefly toyed with a potentially hypersonic aerodynamic vehicle, the winged A-4b version of the V-2 rocket. The only “successful” A-4b flight had managed just over Mach 4 (about 2,700 mph) before apparently disintegrating in flight. Perhaps unsurprisingly, in the immediate post-war period most researchers believed that hypersonic flight was a domain for unmanned missiles.

When the U.S. Navy BuAer provided an English translation of a technical paper by German scientists Eugen Sänger and Irene Bredt in 1946, this preconception began to change. Expanding upon ideas conceived as early as 1928, Sänger and Bredt concluded in 1944 that they could build a rocket-powered hypersonic aircraft with only minor advances in technology. This concept of manned aircraft flying at hypersonic velocities greatly interested researchers at the NACA. Nevertheless, although there were numerous paper studies exploring variations of the Sänger-Bredt proposal during the late 1940s, none bore fruit and no hardware construction was undertaken.

One researcher who was interested in exploring the new science of hypersonics was John V. Becker, the assistant chief of the Compressibility Research Division at the NACA Langley Aeronautical Laboratory in Hampton, Virginia.

On 3 August 1945, Becker proposed the construction of a “new type supersonic wind tunnel for Mach number 7.” Already a few small supersonic tunnels in the United States could achieve short test runs at Mach 4, but the large supersonic tunnels under construction at Langley and Ames had been designed for Mach numbers no higher than 2. Information captured by the Army from the German missile research facility at Peenemünde had convinced Becker that the next generation of missiles and projectiles would require testing at much higher Mach numbers.

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Hope for alleviating the compressor problem had first appeared in the spring of 1945 when Becker gained a fresh understanding of supersonic diffusers from

33 Supersonic velocities are usually expressed as “Mach numbers,” a term honoring Austrian mathematician and physicist Ernst Mach, who was the first to assign a numerical value to the ratio between a solid object passing through a gas and the speed of sound through the same gas. The speed of sound varies with atmospheric conditions (temperature and pressure) and hence is different at every altitude on every day. At sea level on a standard day the speed of sound is 761.6 miles per hour. By convention, at altitudes of above 40,000 feet the speed of sound is a constant 660.4 miles per hour.

34 Despite this apparent success, most engineers on the program believed that heat transfer problems would ultimately doom the A-4b; there were no provisions for cooling the airframe, and little was understood about potential heating effects. For further information, see Michael Neufeld’s interview of Karl Werner Dahm, 25 January 1993. In the files at the National Air and Space Museum.


36 The Compressibility Research Division was created in July 1943 as one of the first steps toward breaking the sound barrier. The division included all of the high-speed wind tunnels at Langley and a small section under Arthur Kankiewicz that studied fundamental gas dynamics.


38 Letter, John V. Becker to Dennis R. Jenkins, 29 July 2002.
of the rapid expansion of the air necessary for Mach 7 operation was the large drop in air temperature below the nominal liquefaction value. At the time, there was no consensus on the question of air liquefaction, although some preliminary investigations of the condensation of water vapor suggested that the transit time through a hypersonic nozzle and test section might be too brief for liquefaction to take place. Nevertheless, Kantrowitz, the head of Langley’s small gas-dynamics research group, feared that “real-gas effects”—possibly culminating in liquefaction—would probably limit wind tunnels to a maximum useful Mach number of about 4.5. 42

Nevertheless, Becker had his supporters. For instance, Dr. George W. Lewis, 43 the Director of Aeronautical Research for the NACA, advised Becker, “Don’t call it a new wind tunnel. That would complicate and delay funding,” so for the next two years it was called “Project 506.” The estimated $39,500 cost of the pilot tunnel was rather modest, and given Lewis’s backing, the facility received quick approval. 44

In September 1945 a small staff of engineers under Charles H. McLellan began constructing the facility inside the shop area of the old Propeller Research Tunnel. They soon discovered that Kantrowitz’s predictions had been accurate—the job required more than extrapolation of existing supersonic tunnel theory. The pilot tunnel proposal had not included an air heater, since Becker believed he could add it later if liquefaction became a problem. As work progressed, it became increasingly clear that the ability to control air temperature would greatly improve the quality and scope of the research, and by the end of 1945 Becker had received approval to include an electric heater. This would maintain air temperatures of about 850°F, allowing Mach 7 temperatures well above the nominal liquefaction point. 45

The first test of the “11-inch” on 26 November 1947 revealed uniform flow at Mach 6.9, essentially meeting all of the original intents. An especially satisfying result of the test was the performance of the variable-geometry diffuser. McLellan and his group had devised a deployable second throat that favored mechanical simplicity over aerodynamic sophistication, but was still very effective. The benefit appeared as an increased run duration (in this case an increase from 25 seconds to over 90 seconds). 46

For three years the 11-inch would be the only operational hypersonic tunnel in the United States and, apparently, the world. Several basic flow studies and aerodynamic investigations during this period established the 11-inch as an efficient tool for general hypersonic research, giving Langley a strong base in the new field of hypersonics. Without this development, Langley would not have been

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39 Arthur Kantrowitz and Coleman duPont Donaldson, “Preliminary Investigation of Supersonic Diffusers,” NASA wartime report L713, May 1946 (originally published as LSD20, 1946). Becker was serving as the chairman of the technical editorial committee when he first read the paper.

40 Letter, John V. Becker to Dennis R. Jenkins, 29 July 2002.

41 Hunsaker was chairman of the NACA from 1941 to 1956. Among the notable achievements in a long and accomplished career, his work in aircraft stability was published as NACA Technical Report No. 1 in 1915.


43 In 1919 Lewis became the first executive officer of the NACA; in 1924 he received the title of director of aeronautical research, which he kept until 1947. Lewis died at his summer home at Lake Wicola, Pennsylvania, on 17 July 1948.

44 Letter, John V. Becker to Dennis R. Jenkins, 29 July 2002. The $39,500 estimate contained in the 3 August 1945 memo seems ridiculous by today’s standards. However, it did not include any NACA overhead costs, and construction would take place in NACA shops using NACA personnel. Adding the heater increased the expenditure to over $200,000.


able to define and support a meaningful hypersonic research airplane concept in 1954. Throughout the entire X-15 program, the 11-inch would be the principal source of the necessary hypersonic tunnel support.  

Despite the fact that it was a pilot facility, the 11-inch hypersonic tunnel operated until 1973, resulting in over 230 publications from tests and related analysis (about one paper every 5 weeks for its 25 years of operations). Few major wind tunnels have equaled that record. After it was decommissioned, NASA donated the tunnel to the Virginia Polytechnic Institute in Blacksburg, Virginia.

As the 11-inch tunnel at Langley was demonstrating that it was possible to conduct hypersonic research, several other facilities were under construction. Alfred J. Eggers, Jr., at the NACA Ames Aeronautical Laboratory at Moffett Field, California, began to design a 10 by 14-inch continuous-flow hypersonic tunnel in 1946, and the resulting facility became operational in 1950. The first hypersonic tunnel at the Naval Ordnance Facility, constructed largely from German material captured from the uncompleted Mach 10 tunnel at Peenemünde, also became operational in 1950.

Interestingly, NASA did not authorize a continuously running hypersonic tunnel that incorporated all of the features proposed in the 1945 Becker memo until 1958. Equipped with a 1,450°F heater, the design velocity increased from Becker’s proposed Mach 7 to 12. As it ended up, although the tunnel attained Mach 12 during a few tests, severe cooling problems in the first throat resulted in a Mach 10 limit for most work. The enormous high-pressure air supply and vacuum tankage of the Gas Dynamics Laboratory provided blow-down test durations of 10–15 minutes. Together with improved instrumentation, this virtually eliminated the need to operate the tunnel in the “continuously running” mode, and nearly all of Langley’s “continuous-running” hypersonic tunnel operations have been conducted in the “blow-down” mode rather than with the compressors running.  

### THE MISSILE INFLUENCE

Not surprisingly, during the early 1950s the top priority for the hypersonic tunnels was to support the massive development effort associated with the intercontinental missiles then under development. Initially it was not clear whether the resulting weapon would be a high-speed cruise missile or an intercontinental ballistic missile (ICBM), so the Air Force undertook programs to develop both. Much of the theoretical science necessary to create a manned hypersonic research airplane would be born of the perceived need to build these weapons.

Long-range missile development challenged NACA researchers in a number of ways. The advancements necessary to allow a Mach 3 cruise missile were relatively easily imagined, if not readily at hand. The ballistic missile was a different story. A successful ICBM would have to accelerate to 15,000 miles per hour at an altitude of perhaps 500 miles, and then be guided to a precise target thousands of miles away. Sophisticated and reliable propulsion, control, and guidance systems were essential, as was keeping the structural weight at a minimum. Moreover, researchers needed to find some method to handle aerodynamic heating. As the missile warhead reentered the atmosphere, it would experience temperatures of several thousand °F. The heat that was generated by shock-wave compression outside the boundary layer and was not in contact with the structure would dissipate harmlessly into the surrounding air. However, the part that arose within the boundary layer and was in direct contact with the missile structure would be great enough to melt the vehicle. Many early dummy warheads burned up because the engineers did not yet understand this.

During this time, H. Julian Allen was engaged in high-speed research at Ames and found what he believed to be a practical solution to the aerodynamic heating problems of the ICBM. In place of the traditional sleek configuration with a sharply pointed nose (an aerodynamic concept long since embraced by missile designers, mostly because the V-2 had used it), Allen proposed a blunt shape with a rounded bottom. In 1951 Allen predicted that when the missile reentered the atmosphere,
In 1951, NACA Ames researcher H. Julian Allen postulated the concept of a “blunt body” reentry vehicle for intercontinental missiles. Pushing the shock wave away from the missile body removed most of the aerodynamic heating from being in direct contact with the structure. The reentry profiles developed at NACA Langley used the idea of “sufficient lift,” which were a new manifestation of the blunt-body concept. (NASA)

its blunt shape would create a powerful bow-shaped shock wave that would deflect heat safely outward and away from the structure of the missile. The boundary layer on the body created some frictional drag and heating, but this was only a small fraction of the total heat of deceleration, most of which harmlessly heated the atmosphere through the action of the strong shock wave. As Allen and Eggers put it, “not only should pointed bodies be avoided, but the rounded nose should have as large a radius as possible.” Thus the “blunt-body” concept was born.52

Allen and Eggers verified the blunt-body concept by studying the aerodynamic heating of miniature missiles in an innovative supersonic free-flight tunnel, a sort of wind-tunnel-cum-firing-range that had become operational at Ames in 1949. The researchers published their classified report on these tests in August 1953, but the Air Force and aerospace industry did not immediately embrace the concept since it ran contrary to most established ideas. Engineers accustomed to pointed-body missiles remained skeptical of the blunt-body concept until the mid-to-late-1950s, when it became the basis for the new ICBM warheads and all of the manned space capsules.53

In the meantime, Robert J. Woods, designer of the Bell X-1 and X-2 research airplanes, stirred up interest in hypersonic aircraft. In a letter to the NACA


Committee on Aerodynamics54 dated 8 January 1952, Woods proposed that the committee direct some part of its research to address the basic problems of hypersonic and space flight. Accompanying the letter was a document from Dr. Walter R. Dornberger, former commander of the German rocket test facility at Peenemünde and now a Bell employee, outlining the preliminary requirements of a hypersonic aircraft. The “ionosphere research plane” proposed by Dornberger was powered by a liquid-fueled rocket engine and capable of flying at 6,000 feet per second (fps) at an altitude of 50–75 miles.55 It was apparent that the concept for an “antipodal” bomber proposed near the end of the war by his colleagues Eugen Sänger and Irene Bredt still intrigued Dornberger.56 According to the Sänger-Bredt study, this aircraft would skip in and out of the atmosphere (called “skip-gliding”) and land halfway around the world.57 Dornberger’s enthusiasm for the concept had captured Woods’s imagination, and he called for the NACA to develop a manned hypersonic research airplane in support of it. At the time, the committee declined to initiate the research advocated by Woods, but took the matter under advisement.58

At the 30 January 1952 meeting of the Committee on Aerodynamics, Woods submitted a paper that noted growing interest in very-high-speed flight at altitudes where the atmospheric density was so low as to eliminate effective aerodynamic control. Since he believed that research into this regime was necessary, Woods suggested that “the NACA is the logical organization to carry out the basic studies in space flight control and stability” and that the NACA should set up a small group “to evaluate and analyze the basic problems of space flight.” Woods went on to recommend that the NACA “endeavor to establish a concept of a suitable manned test vehicle” that could be developed within two years. Again, the NACA took the matter under advisement.59

Smith J. DeFrance, an early Langley engineer who became the director of NACA Ames when it opened in 1949, opposed the idea for a hypersonic study group because “it appears to verge on the developmental, and there is a question

54 The NACA received its direction via a committee system. The committees and their subcommittees were composed of representatives from industry, the military, and NACA scientists and engineers. A subcommittee that had the most direct contact with the “real world” might recognize a new area of research and pass a resolution recommending further efforts. The overarching committee would then take up the resolution and, after discussion at a higher level in the food chain, either table it or pass its own resolution. This in turn would pass to the executive committee, which was composed of distinguished members of industry, high-ranking military officers, and government officials appointed by the president. If the executive committee endorsed the resolution, it would direct the NACA laboratories (Ames, Langley, and Lewis) and stations (the Auxiliary Flight Research Station and later the High-Speed Flight Station) to conduct the research. Usually, funding came from the various military services, although the NACA also had a separately appropriated budget.

55 The accepted standard at the time was to report extreme altitudes in statute miles; this equated to 264,000–396,000 feet, almost exactly furnishing the performance ultimately obtained by the X-15.

56 According to Webster’s—antipodal: of or relating to the antipodes; specif. situated at the opposite side of the Earth, or, points on opposite sides of a sphere. The original Sänger concept was that the Silverbird would land on the opposite side of the Earth from where it took off, dropping its bombs midway through Earth orbit.

57 Eugen Sänger, Rocket Flight Engineering, NASA translation TTF-223 (Washington, DC: NASA, 1965). Sänger’s concepts for skip-glide aircraft date back as far as his doctoral thesis of 1928, and formed the basis for several postwar American projects, such as Bom and Robo. His “dynamic-soaring” terminology for this flight path also inspired the name “Dyna-Soar” given to the Step III hypersonic research program, and later the X-20 vehicle.

58 Letter, Robert J. Woods to the NACA Committee on Aerodynamics, “Establishment of a Study Group on Space Flight and Associated Problems,” 8 January 1952. A few weeks later, Dornberger outlined an ambitious version of the aircraft launched from a B-47 and capable of 6,010 fps (4,250 mph) and 564,000 feet. It was, for all intents, a version of the A-4b of A-9 investigated by the Germans at Peenemünde during the war. See a letter from Walter R. Dornberger to Robert J. Woods of 18 January 1952. In the files at the NASA History Office.

59 Minutes of the Meeting, NACA Committee on Aerodynamics, 30 January 1952. In the files at the NASA History Office.
as to its importance. There are many more pressing and more realistic problems to be met and solved in the next ten years.” DeFrance concluded in the spring of 1952 that “a study group of any size is not warranted.” This reflected the position of many NACA researchers who believed the committee should only undertake theoretical and basic research, and leave development projects to the military and industry.60

Further discussion ensued during the 24 June 1952 meeting of the Committee on Aerodynamics. Other factors covered at the meeting included Allen’s unanticipated discovery of the blunt-body concept and a special request from a group representing 11 missile manufacturers.

The NACA Subcommittee on Stability and Control had invited the same manufacturers to Washington in June 1951 to present their ideas “on the direction in which NACA research should move for greatest benefit in missile development.” In this case the weapons in question were more often than not air-to-air and surface-to-air missiles rather than ICBMs. During this meeting, Maxwell W. Hunter, an engineer who was developing the Sparrow and Nike missiles at the Douglas Aircraft Company, suggested that the NACA should begin to explore the problems missiles would encounter at speeds of Mach 4 to Mach 10. Hunter pointed out that several aircraft designers, notably Alexander Kartveli at Republic, were already designing Mach 3+ interceptors.61 For an air-to-air missile to be effective when launched from an aircraft at Mach 3, the missile itself would most probably need to be capable of hypersonic speeds.62

Hunter and Woods repeated their requests during the June 1952 meeting of the Committee on Aerodynamics. In response, the committee passed a resolution largely penned by Air Force science advisor Albert Lombard. The resolution recommended that “(1) the NACA increase its program dealing with the problems of unmanned and manned flight in the upper stratosphere at altitudes between 12 and 50 miles, and at Mach numbers between 4 and 10, and (2) the NACA devote a modest effort to problems associated with unmanned and manned flight at altitudes from 50 miles to infinity and at speeds from Mach number 10 to the velocity of escape from Earth’s gravity.” The NACA Executive Committee ratified the resolution on 14 July. NACA Headquarters then asked the Ames, Langley, and Lewis laboratories for comments and recommendations concerning the implementation of this resolution.63

This resolution had little immediate effect on existing Langley programs, with the exception that it inspired the Pilotless Aircraft Research Division (PARD)64 to evaluate the possibility of increasing the speeds of their test rockets up to Mach 10. Nevertheless, the resolution did have one very important consequence for the future: the final paragraph called for the laboratories “to devote a modest effort” to the study of space flight.65

The concepts and ideas discussed by Dornberger, Hunter, and Woods inspired two unsolicited proposals for research aircraft. The first, released on 21 May 1952, was from Hubert M. “Jake” Drake and L. Robert Carman of the NACA High-Speed Flight Research Station (HSFRS) and called for a two-stage system in which a large supersonic carrier aircraft would launch a smaller, manned research airplane. The Drake-Carman proposal stated that by “using presently available components and manufacturing techniques, an aircraft having a gross weight of 100,000 pounds could be built with an empty weight of 26,900 pounds. Using liquid oxygen and water-alcohol propellants, this aircraft would be capable of attaining Mach numbers of 6.4 and altitudes up to 660,000 feet. It would have duration of one minute at a Mach number of 5.3. By using this aircraft, an aircraft of the size and weight of the Bell X-2 could be launched at Mach 3 and an altitude of 150,000 feet, attaining Mach numbers up to almost 10 and an altitude of about 1,000,000 feet. Duration of one minute at a Mach number of 8 would be possible.” The report went into a fair amount of detail concerning the carrier aircraft, but surprisingly little toward describing the heating and structural problems expected for the smaller research airplane.66

David G. Stone, head of the Stability and Control Branch of the PARD, released the second report in late May 1952. This report was somewhat more conservative and proposed that the Bell X-2 itself could be used to reach speeds approaching Mach 4.5 and altitudes near 300,000 feet if it were equipped with two JPL-4 Sergeant solid-propellant rocket motors. Stone also recommended the formation of a project group that would work out the details of actual hardware development, flight programs, and aircraft systems. Langley director Henry J. E.

60 Memorandum, Smith J. DeFrance, Director, Ames Aeronautical Laboratory, to NACA, subject: Report on Research of Interest to Committee on Aerodynamics, 29 May 1952.
61 In early 1948 Alexander Kartveli at Republic Aviation began designing the Mach 3 AP-44A all-weather high-altitude defense fighter, less than a year after the first XS-1 supersonic flight. Republic sent preliminary data to the Air Force in January 1951, and in September received a phase I development contract for the WS-204A. Although the entire aircraft was extremely futuristic, perhaps its most notable feature was the Wright J87 dual-cycle turbojet engine. The engine installation provided a large bypass duct that fed air directly into the afterburner, allowing it to function as a ramp at high speeds. An 18-month extension of the phase I contract provided further studies of titanium fabrication, high-temperature hydraulics, escape capsules, and periscope sights. The Air Force continued to fund the program despite a variety of technical problems. By July 1954 the program had advanced to the point where the Air Force awarded Republic a contract to manufacture three prototypes. However, technical problems continued, and a low funding level made it difficult to apply sufficient resources to overcome them. In early 1955 the Air Force reduced the program to a single prototype and two flight engines, but little progress had been made by 21 August 1957 when the Air Force canceled the XF-103 and Wright engine entirely. The program had cost $104 million over nine years.
63 The Aircraft Engine Research Laboratory was founded on 23 June 1941 in suburban Cleveland, Ohio. In April 1947 it was renamed the Flight Propulsion Research Laboratory, and a year later it was renamed the Lewis Flight Propulsion Laboratory. When NASA came into being on 1 October 1958, the laboratory was renamed the Lewis Research Center (abbreviated LaRC) to differentiate it from the Langley Research Center (LaRC). On 1 March 1999 it was renamed the John H. Glenn Research Center at Lewis Field.
64 Minutes of the Meeting, Committee on Aerodynamics, 24 June 1952. In the files at the NASA History Office.
65 The PARD was established in June 1946 at the Auxiliary Flight Research Station (AFRS) on Wallops Island, off the eastern shore of Virginia. The group had been set up during World War II to launch “pilotless aircraft” (the military’s name for all guided missiles of the time) to obtain research data on them. On 4 July 1945, the AFRS launched its first test vehicle, a small two-stage, solid-fuel rocket, to check out the installation’s instrumentation. At the end of the war, a typical model weighed about 40 pounds and could attain a maximum speed of Mach 1.4 before it crashed into the Atlantic Ocean. The instrumented models provided telemetry back to the ground during their flights. Despite the fact that PARD launched 396 models from 1947 to 1949, the “dead”IT’S” that the Langley wind tunnels never believed that the operation obtained much useful data. Nevertheless, the PARD continued and soon began launching large-scale models of aircraft on top of its rockets, obtaining data at speeds the wind tunnel operation could only dream of at the time. Many types of aircraft were evaluated for instance, models of the Convair F-102 Delta Dagger helped verify the effectiveness of Richard T. Whitcomb’s area rule principles.
66 Hanssen, Engineer in Charge, pp. 350-351.
Reid and John Stack generally supported this approach, but believed that further study of possible alternatives was required.

Meanwhile, in response to the 1952 recommendation from the NACA Committee on Aerodynamics, Henry Reid set up a three-man study group consisting of Clinton E. Brown (chairman) from the Compressibility Research Division, William J. O’Sullivan, Jr., from the PARD, and Charles H. Zimmerman from the Stability and Control Division. Curiously, none of the three had any significant background in hypersonics. Floyd L. Thompson, who became associate director of Langley in September 1952, had rejected a suggestion to include a hypersonic aerodynamicist or specialist in thermodynamics in the study group. Thompson’s plan was to bring together creative engineers with “completely fresh, unbiased ideas.” The group was to evaluate the state of available technology and suggest possible programs that researchers could initiate in 1954, given adequate funding.

This group reviewed the ongoing ICBM-related work at Convair and RAND, and then investigated the feasibility of hypersonic and reentry flight in general terms. Not surprisingly, the group identified structural heating as the single most important problem. The group also reviewed the earlier proposals from Drake-Carman and Stone, and agreed to endorse a version of Stone’s X-2 modification with several changes. In the Langley concept, the vehicle used a more powerful internal rocket engine instead of strap-on solid boosters, with the goal of reaching Mach 3.7 velocities. Dr. John E. Duberg, the chief of the Structural Research Division, noted, however, that “considerable doubt exists about the ability of the X-2 airplane to survive the planned trajectory because of the high thermal stresses.” The study group released its report on 23 June 1953, and in a surprisingly conservative vein, agreed that unmanned missiles should conduct any research in excess of Mach 4.5.69

Originally, the plan was to have an interlaboratory board review the findings of the study group, but this apparently never happened. Nevertheless, hypersonic specialists at Langley frequently had the opportunity to talk with the group, and heard Brown formally summarize the findings at a briefing in late June 1953. While listening to this summary, the specialists “felt a strong sense of déjà-vu,” especially on hearing Brown’s pronouncement that “the main problem of hypersonic flight is aerodynamic heating.” They disagreed, however, with the group’s conclusion that the NACA would have to rely on flight-testing, rather than on ground-based approaches, for research and development beyond Mach 4.70

Brown, O’Sullivan, and Zimmerman found it necessary to reject the use of traditional ground facilities for hypersonic research because they were “entirely inadequate” in accounting for the effects of high temperatures.71 John Becker later wrote that “much of the work of the new small hypersonic tunnels was viewed with extreme skepticism” because they could not simulate the correct temperatures and boundary-layer conditions. The Brown study anticipated there would be significant differences between the “hot” aerodynamics of hypersonic flight and the “cold” aerodynamics simulated in ground facilities. The study concluded that “testing would have to be done in actual flight where the high-temperature hypersonic environment would be generated” and recommended extending the PARD rocket-model testing technique to much higher speeds. This would also mean longer ranges, and the study suggested it might be possible to recover the test models in the Sahara Desert of northern Africa.72

This was another case of the free-flight-versus-wind-tunnel debate that had existed at Langley for years. Ground facilities could not simulate the high-temperature environment at very high Mach numbers, admitted the hypersonics specialists, but facilities like the pilot 11-inch hypersonic tunnel at Langley and the 10-by-14-inch continuous-flow facility at Ames had proven quite capable of performing a “partial simulation.” Selective flight-testing of the final article was desirable—just as it always had been—but, for the sake of safety, economy, and the systematic parametric investigation of details, the hypersonics specialists argued that ground-based techniques had to be the primary tools for aerodynamic research. Similar debates existed between the wind-tunnel researchers and the model-rocket researchers at PARD.73

Although Langley had not viewed their May 1952 proposal favorably, in August 1953 Drake and Carman wrote a letter to NACA Headquarters calling for a five-phase hypersonic research program that would lead to a winged orbital vehicle. Dr. Hugh L. Dryden, the director of the NACA, and John W. “Gus” Crowley, the associate director for research at NACA Headquarters, shelved the proposal as being too futuristic.74 Nevertheless, in its bold advocacy of a “piggyback” two-stage-to-orbit research vehicle, the Drake-Carman report presented one of the earliest serious predecessors of the Space Shuttle.

68 Letter, David G. Stone to Chief of Research, subject: Preliminary study of the proposal for the flight of manned vehicles into space, 21 May 1952. In the files at the Dryden History Office. The High-Speed Flight Research Station (HSFRS) became the High-Speed Flight Station (HSFS) on 1 July 1954, the Flight Research Center (FRC) on 27 September 1959, and the Hugh L. Dryden Flight Research Center (usually abbreviated DFRC) on 26 March 1975. On 1 October 1981, it was administratively absorbed into the Ames Research Center and its name was changed to the Ames-Dryden Flight Research Facility (DFRF). It reverted to Center status on 1 March 1994 and again became DFRC. At some point between 1954 and 1959, the hyphenation between “High” and “Speed” seems to have been dropped, but no official evidence of this could be found.


70 E. P. Williams, et al., RAND report 174, “A Comparison of Long-Range Surface-to-Surface Rocket and Ram-Jet Missiles,” May 1950. From http://rand.org/about/history/; “On 1 October 1945, General Henry H. “Hap” Arnold and Donald Douglas set up Project RAND (research and development) under special contract to the Douglas Aircraft Company. However, this arrangement was not ideal, and in February 1948 the chief of staff of the newly created United States Air Force wrote to Donald Douglas approving the evolution of RAND into a nonprofit corporation, independent from Douglas. On 14 May 1948, RAND incorporated as a nonprofit corporation under the laws of the State of California. RAND’s charter was remarkably brief: “To further and promote scientific, educational, and charitable purposes, all for the public welfare and security of the United States of America.”

71 Brown et al., “A Study of the Problems Relating to High-Speed, High-Altitude Flight.” The Duberg quote is in Appendix VI.
Chapter 1: A New Science

MILITARY SUPPORT

At the October 1953 meeting of the Air Force Scientific Advisory Board (SAB) Aircraft Panel, Chairman Clark B. Millikan asked panel members for their ideas on future aircraft research and development programs. The panel decided that “the time was ripe” for another cooperative (USAF-NACA) research airplane project to further extend the frontiers of flight. Millikan released a statement declaring that the feasibility of an advanced manned research aircraft “should be looked into.” The panel member from NACA Langley, Robert R. Gilruth, would later play an important role in coordinating a consensus between the SAB and the NACA.77

Contrary to Sänger’s wartime conclusions, by 1954 most experts within the NACA and industry agreed that hypersonic flight would not be possible without major advances in technology. In particular, the unprecedented problems of aerodynamic heating and high-temperature structures appeared to be a potential “barrier” to sustained hypersonic flight. Fortunately, the perceived successes enjoyed by the X-planes led to increased political and philosophical support for a more advanced research aircraft program. The most likely powerplant for the hypersonic research airplane was one of the large rocket engines from the missile programs. Most researchers now believed that manned hypersonic flight was feasible, but it would entail a great deal of research and development. Fortunately, at the time there was less emphasis than now on establishing operational requirements prior to conducting basic research, and, perhaps even more fortunately, there were no large manned space programs that would compete for funding. The time was finally right.78

The hypersonic research program most likely originated during a meeting of the NACA Interlaboratory Research Airplane Projects Panel held in Washington, D.C., on 4–5 February 1954. The panel chair, Hartley A. Soulé, had directed the NACA portion of the cooperative USAF-NACA research airplane program since 1946. In addition to Soulé, the panel consisted of Lawrence A. Clousing from Ames, Charles J. Donlan from Langley, William A. Fleming from Lewis, Walter C. Williams from the HSFS, and Clotaire Wood from NACA Headquarters. Two items on the agenda led almost directly to the call for a new research airplane. The first was a discussion concerning Stone’s proposal to use a modified X-2, with the panel deciding that the aircraft was too small to provide meaningful hypersonic research. The second was a proposal to develop a new thin wing for the Douglas D-558-2. This precipitated a discussion on the “ advisability of seeking a completely new research airplane and possible effects on such a proposal on requests for major changes to existing research airplanes.” The panel concluded that the research utility of the D-558-2 and X-2 was largely at an end, and instead recommended that NACA Headquarters request detailed goals and requirements for an entirely new vehicle from each of the research laboratories. This action was, in effect, the initial impetus for what became the X-15.79

On 15 March 1954, Bob Gilruth sent Clark Millikan a letter emphasizing that the major part of the research and development effort over the next decade would be “to realize the speeds of the existing research airplanes with useful, reliable, and efficient aircraft under operational conditions” (i.e., developing Mach 2–3 combat aircraft). Gilruth further noted that a “well directed and sizeable effort will be required to solve a number of critical problems, by developing new materials, methods of structural cooling and insulation, new types of structures, and by obtaining a thorough understanding of the aerodynamics involved.” Because many of the problems were not then well defined, “design studies should be started now for manned research aircraft which can explore many of these factors during high-speed flight” and which would be capable of “short excursions into the upper atmosphere to permit research on the problems of space flight and reentry.” It was a surprising statement.80

During the late 1940s and early 1950s, the overwhelming majority of researchers thought very little about manned space flight. Creating a supersonic airplane had proven difficult, and many researchers believed that hypersonic flight, if feasible at all, would probably be restricted to missiles. Manned space flight, with its “multiplicity of enormous technical problems” and “unanswered questions of safe return” would be “a 21st Century enterprise.”81

Within a few years, however, the thinking had changed. By 1954 a growing number of American researchers believed that hypersonic flight extending into space could be achieved much sooner, although very few of them had the foresight to see it coming by 1960. Around this time, the military became involved in supporting hypersonic research and development with a goal of creating new weapons systems. During 1952, for example, the Air Force began sponsoring Dornberger’s manned hypersonic boost-glide concept at Bell as part of Project BoMi.82

BoMi (and subsequently RoBo) advanced the Sänger-Bredt boost-glide concept by developing, for the first time, a detailed thermal-protection concept. Non-load-bearing, flexible, metallic radiative heat shields (“shingles”) and water-cooled, leading-edge structures protected the wings, while passive and active cooling systems controlled the cockpit temperature. NACA researchers, including the Brown study group, read the periodic progress reports of the Bell study—classified Secret by the Air Force—with great interest. Although most were skeptical,

77 The NACA actually had two cooperative efforts under way in the early 1950s, and Soulé was involved with both. The first was testing the Bell X-1, X-2, X-6, etc. in cooperation with the Air Force. The other was testing the Douglas D-558 series in cooperation with the Navy.
79 Minutes of the Meeting, Interlaboratory Research Airplane Projects Panel, NACA headquarters, 4–5 February 1954; letter, John W. Crowley to distribution, subject: Request for comments on possible new research airplane, 9 March 1954. The Research Airplane Projects Panel was formed by NACA Associate Director for Research Ous Crowley in September 1949 to coordinate the efforts of Ames, Langley, Lewis, Wallops Island, and the HSFS. Each laboratory reported quarterly to the panel detailing what research was being performed in support of each specific airplane, and the outcome of the research. The panel met in formal session annually. This was different from the Research Airplane Program Committee headed by Langley’s John Stack, which included representatives from the Army Air Forces and the Navy Bureau of Aeronautics.
80 Letter, Robert R. Gilruth to Dr. Clark B. Millikan, subject: Air Force Research and Development Effort for the Next Decade in the Field of the Aircraft Panel, 15 March 1954. In the files at the Air Force Historical Research Agency. John Backer remembers that at the time the consensus was that “space” began where the dynamic pressure was less than one pound per square foot. See the interview of John V. Backer by J. D. Hunley, 3 October 2000, written transcript in the files at the DFRC History Office.
82 BoMi was an acronym for “Bomber-Missile,” and RoBo stood for “Rocket-Bomber.” Both would be consolidated into the HYWARDS program that later evolved into the Boeing X-20 Dyna-Soar.
In response to the recommendation of the Research Airplane Projects Panel, NACA Headquarters asked its field installations to explore the requirements for a possible hypersonic research aircraft. Based on the concerns of the 1952 Langley study group, as well as data from Bell regarding BoMi research, it was obvious that a primary goal of any new research airplane would be to provide information about high-temperature aerodynamics and structures. The missile manufacturers concurred.

In response to NACA Headquarters’ request, all of the NACA laboratories set up small ad hoc study groups during March 1954. A comparison of the work of these different NACA groups is interesting because of their different approaches and findings. The Ames group concerned itself solely with suborbital long-range flight and ended up favoring a military-type air-breathing (rather than rocket-powered) aircraft in the Mach 4–5 range. The HSFS suggested a larger, higher-powered conventional configuration generally similar to the Bell X-1 or Douglas D-558-1 research airplanes. The staff at Lewis questioned the need for a piloted airplane at all, arguing that ground studies and the PARD rocket-model operation could provide all of the necessary hypersonic information at much less cost and risk. Lewis researchers believed that possible military applications had unduly burdened previous research airplane programs, and there was no reason to think anything different would happen in this case.

On the other hand, Langley chose to investigate the problem based largely on the hypersonic research it had been conducting since the end of World War II. After the 11-inch hypersonic tunnel became operational in 1947, a group headed by Charles McLellan began conducting limited hypersonic research. This group, which reported to John Becker, who was now the chief of the Aero-Physics Division, provided verification of several newly developed hypersonic theories while it investigated phenomena such as the shock–boundary-layer interaction. Langley also organized a parallel exploratory program into materials and structures optimized for hypersonic flight. Perhaps not surprisingly, Langley decided to determine the feasibility of a hypersonic aircraft capable of a 2- to 3-minute excursion out of the atmosphere to create a brief period of weightlessness in order to explore the effects of space flight. Hugh Dryden would later liken this excursion to the leap of a fish out of water, and coined a new term: space leap.

Langley’s ad hoc hypersonic aircraft study group consisted of John Becker (chairman); Maxime A. Faget, a specialist in rocket propulsion from the Performance Aerodynamics Branch of PARD; Thomas A. Toll, a control specialist from the Stability Research Division; Norris F. Dow, a hot-structures expert from the Structures Research Division; and test pilot James B. Whitten. Unlike the earlier Brown study group, this group intentionally included researchers with previous experience in hypersonics.

The group reached a consensus on the objectives of a hypersonic research aircraft by the end of its first month of study. Although one of the original goals was to investigate the effects of weightlessness, the members soon realized “that the problems of attitude control in space and the transition from airless flight to atmospheric flight during reentry were at least equally significant.” The group also began to consider the dynamics of the reentry maneuvers and the associated problems of stability, control, and heating as the most pressing research need. However, another objective would come to dominate virtually every other aspect of the aircraft’s design: research into the related fields of high-temperature aero-
dynamics and high-temperature structures. Thus, it would become the first aircraft in which aero-thermo-structural considerations constituted the primary research problem, as well as the primary research objective.\(^{89}\)

Eventually, Becker and the group selected a goal of Mach 7, noting that this would permit investigation of “extremely wide ranges of operating and heating conditions.” By contrast, a Mach 10 vehicle “would require a much greater expenditure of time and effort” yet “would add little in the fields of stability, control, piloting problems, and structural heating.” Considering that no human had yet approached Mach 3, even Mach 7 seemed a stretch.\(^{90}\)

By the end of April 1954, Becker’s group had completed a tentative design for a winged aircraft and an outline of proposed experiments. The group kept the configuration as conventional as possible to minimize the need for special low-speed and transonic developments without compromising its adequacy as a hypersonic, aerodynamic, and structural research vehicle. However, acknowledging what would become a continuing issue; the group did not consider any of the large rocket engines then under development entirely satisfactory for the airplane. In the absence of the rapid development of a new engine, the group hoped a combination of three or four smaller rocket motors could provide hypersonic velocities.\(^{91}\)

At this point Floyd Thompson, by now the associate director at Langley, influenced the direction of the Becker study. He made a suggestion that echoed John Stack’s 1945 recommendation that the Bell XS-1 transonic research airplane use a 12% thick wing that would force it to encounter the compressibility efforts that aerodynamicists were most interested in studying. Since the hypersonic airplane would be the first in which aero-thermal-structural considerations constituted the primary research problem, Thompson argued that the aim of the aircraft “should be to penetrate as deeply as possible into the region of [high aerodynamic] heat and to seek fresh design approaches rather than makeshift modifications to conventional designs.” His suggestion became policy.\(^{92}\)

Wind-tunnel testing began in mid-1954 and continued through the end of 1955 using the basic Becker design. David E. Fetterman, Jr., Jim A. Penland, and Herbert W. Ridyard led the tests, mainly using the 11-inch tunnel at Langley. The researchers noted that previous hypersonic designs had “been restricted mainly to missile types which were not required to be able to land and which, therefore, had relatively small wings or wings of very low aspect ratio.” The researchers concentrated on extrapolating existing data to the Becker design while making sure the concept would be acceptable for a manned aircraft, including the ability to land.\(^{93}\)

One particular feature, however, differed from later concepts. The initial wind-tunnel tests used a design that incorporated relatively large leading-edge radii for both the wing and vertical stabilizer. The large radii were believed necessary to keep the heat transfer rates within feasible limits. Eventually the researchers discovered the beneficial effects of a leading-edge sweep and found materials capable of withstanding higher temperatures. These allowed smaller radii, resulting in less drag and generally better aerodynamic characteristics. Although the baseline design changed as a result, by this time the researchers were concentrating on evaluating various empennage configurations and elected not to change the wing design on the wind-tunnel models to avoid invalidating previous results.\(^{94}\)

While performing the original heating analysis of the proposed reentry from the “space leap,” Becker and Peter F. Korycinski from the Compressibility Research Division ran head-on into a major technical problem. At Mach 7, reentry at low angles of attack appeared impossible because of disastrous heating loads. In addition, the dynamic pressures quickly exceeded, by large margins, the limit of 1,000 pounds per square foot (psf) set by structural demands. New tests of the force relationships in the 11-inch tunnel provided Becker and Korycinski with a surprising solution to this problem: if the angle of attack and the associated drag were increased, deceleration would begin at a higher altitude. Slowing down in the thinner (lower-density) atmosphere made the heat-transfer problem much less severe. In other words, Becker and Korycinski surmised, by forcing deceleration to occur sooner, the increased drag associated with the high angle of attack would significantly reduce the aircraft’s exposure to peak dynamic pressure and high heating rates. Thus, by using “sufficient lift,” the Langley researchers found a way to limit the heat loads and heating rates of reentry. Interestingly, this is the same rationale used 15 years later by Max Faget when he designed his MSC-002 (DC-3) space shuttle concept at the Manned Spacecraft Center.\(^{95}\)

On reflection, it became clear to the Becker group that the sufficient-lift concept was a “new manifestation” of Allen’s blunt-body theory and was as applicable to high-lift winged reentry as to the non-lifting missile warheads studied at Ames during 1952. As the group increased the angle of attack to dissipate more of the kinetic energy through heating of the atmosphere (and less in the form of frictional heating of the vehicle itself), the configuration became increasingly “blunt.” Some form of speed brakes, again in accord with Allen’s concept, could increase drag and further ease the heating problem.\(^{96}\)

Throughout 1954 the heating problems of high-lift, high-drag reentry came under increasing scrutiny from key Langley researchers. However, another problem soon outweighed the heating consideration: making the configuration stable and controllable at the proposed high-angle-of-attack reentry attitude. Because they were venturing into a new flight regime, the researchers could not determine

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90 Letter, Floyd L. Thompson/Langley to NACA, 3 May 1954, enclosing a copy of a memo from John V. Becker titled “Research Airplane Study.” The quotes are from the attached memo.
91 Although it had always been assumed that an drop would be the preferred launch method, the original “Research Airplane Study” did not specifically mention any launch method.
93 A variety of reports came from these tests. See, for example, Jim A. Penland et al., “Lift, Drag, and Static Lateral Stability Data from an Exploratory Investigation at a Mach Number of 6.86 of an Airplane Configuration Having a Wing of Trapezoidal Plan Form,” NACA research memorandum L54A21a, 18 January 1955; Herbert W. Ridyard et al., NACA research memorandum L54A21a, “Static Lateral Stability Data from an Exploratory Investigation at a Mach Number of 6.86 of an Airplane Configuration Having a Wing of Trapezoidal Plan Form,” 15 February 1955; Jim A. Penland et al., “Static Longitudinal and Lateral Stability and Control Characteristics of an Airplane Configuration Having a Wing of Trapezoidal Plan Form with Various Tail Airfoil Sections and Tail Arrangements at a Mach Number of 6.86,” NACA research memorandum L55F17, 15 August 1955.
94 Penland, “Static Longitudinal and Lateral Stability and Control Characteristics.”
96 Hansen, Engineer in Charge, p. 359.
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NACA literature. Calculations based on these findings indicated that at Mach 7 the on normal-force characteristics, and his findings had been lying dormant in the time before, suggested using a thicker wedge-shaped section with a blunt trailing edge. Some thin-section stabilizers then in vogue for supersonic aircraft were used. This was lier X-planes, was that an extremely large vertical stabilizer was required if the consensus, reached by wind-tunnel testing and evaluating high-speed data from ear Becker sought the advice of his 11-inch hypersonic tunnel researchers. The consensus, reached by wind-tunnel testing and evaluating high-speed data from earlier X-planes, was that an extremely large vertical stabilizer was required if the thin-section stabilizers then in vogue for supersonic aircraft were used. This was largely because of a rapid loss in the lift-curve slope of thin airfoil sections as the Mach number increased. In a radical departure, however, Charles McLellan suggested using a thicker wedge-shaped section with a blunt trailing edge. Some time before, McLellan had conducted a study of the influence of airfoil shape on normal-force characteristics, and his findings had been lying dormant in the NACA literature. Calculations based on these findings indicated that at Mach 7 the wedge shape “should prove many times more effective than the conventional thin shapes optimum for the lower speed.” By modifying the proposed configuration to include the wedge-shaped vertical stabilizer, McLellan believed that a reason-
ably sized vertical stabilizer could correct most directional instability.

A new series of experiments in the 11-inch tunnel verified that a vertical stabilizer with a 10-degree wedge angle would allow the proposed aircraft to achieve the range of attitudes required by heating considerations for a safe high-drag, high-lift reentry. Further, it might be possible to use a variable-wedge vertical stabilizer as a means of restoring the lift-curve slope at high speeds, thus permitting much smaller surfaces that would be easier to design and would impose a smaller drag penalty at lower speeds. McLellan calculated that this wedge shape should eliminate the disastrous directional stability decay encountered by the X-1A.

Becker’s group also included speed brakes as part of the vertical stabilizers to reduce the Mach number and heating during reentry. Interestingly, the speed brakes originally proposed by Langley consisted of a split trailing edge; very similar to the one eventually used on the space shuttles. As the speed brakes opened, they effectively increased the included angle of the wedge-shaped vertical stabil-

97 John V. Becker, “The X-15 Project, Part I: Origins and Research Background,” Astronautics and Aeronautics, February 1964, p. 56; letter, John V. Becker to Dennis R. Jenkins, 29 July 2002. The temperatures in the boundary layer at Mach 7 exceed 3,000°F. The 2,000°F “equilibrium” temperature is the surface temperature of the underside of the wing where heat loss due to radiation away from the surface balances the imposed heating. Although the angle of attack was between 11 and 26 degrees, the reentry flight path was generally around –32 degrees, meaning that the airplane was actually flying between 21 and 8 degrees nose-down.

98 The wind-tunnel tests of the X-1A had extended only to Mach 2.


lizer, and variable deflection of the wedge surfaces made it possible to change the braking effect and stability derivatives through a wide range. The flexibility this made possible could be of great value because a primary use of the airplane would be to study stability, control, and handling characteristics through a wide range of speeds and altitudes. Furthermore, the ability to reenter in a high-drag condition with a large wedge angle greatly extended the range of attitudes for reentry that were permissible in view of heating considerations.\textsuperscript{102}

Up until this time, the designers of supersonic aircraft had purposely located the horizontal stabilizer well outside potential flow interference from the wings. This usually resulted in the horizontal stabilizer being located partway up the vertical stabilizer, or in some cases (the F-104, for example) on top of the vertical stabilizer. However, researchers at the HSFS suspected that this location was making it difficult, or at times impossible, for aircraft to recover from divergent maneuvers. The same investigations at Langley that verified the effectiveness of the wedge-shape also suggested that an X-shaped empennage would help the aircraft to recover from divergent maneuvers.\textsuperscript{103}

The Becker group recognized that the change from a conventional “+” empennage to the “X” configuration would present at least one major new problem: the X-shape empennage projected into the high downwash regions above and below the wing plane, causing a potentially serious loss of longitudinal effectiveness. Researchers at Langley looked for solutions to this new problem. By late 1954 they had an unexpected answer: locate a conventional “+” horizontal stabilizer in the plane of the wing, between the regions of highest downwash. This eliminated the need to use an X-shaped empennage, allowing a far more conventional tail section and control surfaces.\textsuperscript{104}

Although it would come and go from the various preliminary designs, the use of a ventral stabilizer was beginning to gain support. Charles McLellan observed, “At high angles of attack, the effectiveness of the upper and lower vertical stabilizers were markedly different. Effectiveness of the upper tail decreases to zero at about 20 degrees angle of attack. The lower tail exhibits a marked increase in effectiveness because of its penetration into the region of high dynamic pressure produced by the compression side of the wing. Assuming the wing is a flat plate and the flow is two-dimensional, the dynamic pressure below the wing increases with angle of attack. Since only a part of the lower tail is immersed in this region its gain in effectiveness is, of course, less rapid, but the gain more than offsets the loss in effectiveness of the upper tail.”\textsuperscript{105}

On the structural front, the Becker study evaluated two basic design approaches. In the first, a layer of assumed insulation protected a conventional low-temperature aluminum or stainless steel structure. The alternative was an exposed “hot structure.” This design approach and the materials used permitted high structural temperatures without insulation.\textsuperscript{106}

![Graph](Image)

Surprisingly, the temperatures expected on the high-altitude “space leap” were significantly higher than for the basic hypersonic research flights. Establishing a design that could withstand the 2,000°F equilibrium temperature was a challenge, and ultimately resulted in the hot-structure concept shown on the lower line of this chart. (NASA)

Analysis of the heating projections for various trajectories showed that the airplane would need to accommodate equilibrium temperatures of over 2,000°F on its lower surface. Unfortunately, no known insulating technique could meet this requirement. Bell was toying with a “double-wall” concept in which a high-temperature outer shell and a layer of insulator would protect the underlying low-temperature structure. This concept would later undergo extensive development, and several contractors proposed it during the X-15 competition, but in 1954 it was in an embryonic state and not applicable to the critical nose and leading-edge regions. However, the Becker group believed that the possibility of local failure of any insulation scheme constituted a serious hazard, as was later tragically demonstrated on the Space Shuttle Columbia. Finally, the problem of accurately measuring heat-transfer rates—one of the primary objectives of the new research


\textsuperscript{103} McLellan, “A Method for Increasing the Effectiveness of Stabilizing Surfaces at High Supersonic Mach Numbers.”

\textsuperscript{104} Becker, “The X-15 Project, Part I: Origins and Research Background,” p. 56-57. Downwash is a small velocity component in the downward direction that is associated with the production of lift, as well as a small component of drag. At hypersonic speed, the flow behind a wing is characterized by a shock pattern. Immediately behind the shock is a region of high dynamic pressure and high downwash, which intersected the lower tail surfaces of the original X-tail concept. The upper tails were in a region of low dynamic pressure and low downwash. This situation had the adverse effect of greatly increasing the yaw (or side-to-side movement) of the lower tails relative to the upper tails, causing directional instability. See McLellan, “A Method for Increasing the Effectiveness of Stabilizing Surfaces at High Supersonic Mach Numbers.”

\textsuperscript{105} McLellan, “A Method for Increasing the Effectiveness of Stabilizing Surfaces at High Supersonic Mach Numbers.”

\textsuperscript{106} Becker, “The X-15 Project, Part I: Origins and Research Background,” p. 56-57. These same trade studies would be repeated many times during the concept definition for the Space Shuttle.
aircraft program—would be substantially more difficult to accomplish with an insulated structure. 107

At the start of the study, it was by no means obvious that the hot-structure approach would prove practical either. The permissible design temperature for the best available material was about 1,200°F, which was far below the estimated equilibrium temperature of 2,000°F. It was clear that some form of heat dissipation—either direct internal cooling or absorption into the structure itself—would be necessary. It was thought that either solution would bring a heavy weight penalty.

The availability of Inconel X and its exceptional strength at extremely high temperatures made it, almost by default, the structural material preferred by Langley for a hot-structure design. 108 In mid-1954, Norris Dow began an analysis of an Inconel X structure while other researchers conducted a thermal analysis. In a happy coincidence, the results showed that the skin thickness needed to withstand the expected aerodynamic stresses was about the same as that needed to absorb the thermal load. This meant that it was possible to solve the structural problem for this transient condition of the Mach 7 research aircraft with no serious weight penalty for heat absorption. This was an unexpected plus for the hot structure. Together with the fact that none of the perceived difficulties of an insulated-type structure (particularly the difficulty of studying structural temperatures) were present, this led the study group to decide in favor of an uninsulated hot-structure design.

Unfortunately, it later proved that the hot structure had problems of its own, especially in the area of non-uniform temperature distribution. Detailed thermal analyses revealed that large temperature differences would develop between the upper and lower wing skins during the pull-up portions of certain trajectories, resulting in intolerable thermal stresses in a conventional structural design. To solve this new problem, researchers devised wing shear members that did not resist unequal expansion of the wing skins. The wing thus was essentially free to deform both span-wise and chord-wise with asymmetrical heating. Although this solved the problem for gross thermal stresses, localized thermal-stress problems still existed near the stringer attachments. The study indicated, however, that proper selection of stringer proportions and spacing would produce an acceptable design that would be free of thermal buckling. 109

The analyses produced other concerns as well. Differential heating of the wing leading edge resulted in changes to the natural torsional frequency of the wing unless the design used some sort of flexible expansion joint. The hot leading edge expanded faster than the remaining structure, introducing a compression that destabilized the section as a whole and reduced its torsional stiffness. To negate these phenomena, researchers segmented and flexibly mounted the leading edge to reduce thermally induced buckling and bending. Similar techniques found use on the horizontal and vertical stabilizers.

107 Ibid. Possible insulators included water, several different liquid metals, air, and various fibrous burl materials. The liquids would require active pumps and large reservoirs, making them exceptionally heavy concepts.

108 Inconel X is a temperature-resistant alloy whose name is a registered trademark of Huntington Alloy Products Division, International Nickel Company, Huntington, West Virginia. It is, for all intents, an exotic stainless steel. Inconel X is 72.5% nickel, 15% chromium, and 1% columbium, with iron making up most of the balance.


X-15: EXTENDING THE FRONTIERS OF FLIGHT

Langley evaluated many materials for the proposed hypersonic research airplane, but the availability of Inconel X and its exceptional strength at extremely high temperatures, made it, almost by default, the preferred material for a hot-structure design. Coincidently, the researchers at NASA Langley discovered that the skin thickness needed to withstand aerodynamic stress was about the same as that needed to absorb the thermal load in the high-altitude mission. (NASA)

Perhaps more worrisome was the question of potential propulsion systems. The most promising configuration was found to be four General Electric A1 or A3 rocket engines, due primarily to the “thrust stepping” this configuration provided. 110 At the time, rocket engines could not be throttled (even today, most rocket engines cannot). Several different techniques can be used to throttle a rocket engine, and each takes its toll in mechanical complexity and reliability. However, a crude method of throttling did not actually involve changing the output of the engine, but rather igniting or extinguishing various numbers of small engines. For instance, in a cluster of three 5,000-lbf engines, the available thrust levels (or “steps”) would be 5,000, 10,000, and 15,000 lbf. Since most rocket engines were not restartable (again, the concept adds considerable mechanical complexity to the engine), once an engine was extinguished it could not be restarted. Thrust

110 The General Electric A1 and A3 engines powered the Hermes A-3, also designated XSSM A-16, which was designed as a tactical surface-to-surface missile capable of carrying a 1,000-pound warhead 150 miles. Project Hermes was the first major U.S. ballistic missile program. It encompassed several different configurations and tested both liquid and solid-fuel rockets, and ramjet propulsion systems. Hermes began in 1944 as an Army effort to study the German V-2 rocket. The project soon led to hardware development, and the first of five Hermes A-1s was launched at the White Sands Proving Grounds on 19 May 1950. The program was canceled on 31 December 1954.
stepping or throttling allowed a much more refined flight profile, and largely defined the propulsion concept for the eventual X-15.111

At this stage of the study, the vehicle concept itself was “little more than an object of about the right general proportions and the correct propulsive characteristics” to achieve hypersonic flight. However, in developing the general requirements, the Langley group envisioned a conceptual research aircraft that would serve as a model for the eventual X-15. The vehicle they conceived was “not proposed as a prototype of any of the particular concepts in vogue in 1954...[but] rather as a general tool for manned hypersonic flight research, able to penetrate the new regime briefly, safely, and without the burdens, restrictions, and delays imposed by operational requirements other than research.”112

Although the Becker group was making excellent progress, their continued investigation of the “space leap” caused considerable controversy. The study called for two distinct research profiles. The first—the basic hypersonic research flights—consisted of a variety of constant angle-of-attack, constant-altitude flights to investigate aero-thermodynamic characteristics. However, the second flight profile explored the problems of future space flight, including investigations into “high-lift and low-L/D [lift over drag] during the reentry pull-up maneuver.” Researchers recognized that this was one of the principal problems for manned space flight from both a heating and piloting perspective.113

This brought yet more concerns: “As the speed increases, an increasingly large portion of the aircraft’s weight is borne by centrifugal force until, at satellite velocity, no aerodynamic lift is needed and the aircraft may be operated completely out of the atmosphere. At these speeds the pilot must be able to function for long periods in a weightless condition, which is of considerable concern from the aeromedical standpoint.” By employing a high-altitude ballistic trajectory to roughly 250,000 feet, the Becker group expected that the pilot would operate in an essentially weightless condition for approximately 2 minutes. Attitude control was another problem since traditional aerodynamic control surfaces would be useless at very high altitudes. To solve this problem, the group proposed using small hydrogen-peroxide thrusters for attitude control outside the sensible atmosphere.

While the hypersonic research aspect of the Langley proposal enjoyed virtually unanimous support, it is interesting to note that in 1954 most researchers viewed the space-flight aspect with, at best, cautious tolerance. There were few who believed that any space flight was imminent, and most believed that manned space flight in particular would not be achieved until many decades in the future, probably not until the 21st century. John Becker remembers that even the usually far-sighted John Stack was “not really interested in the reentry problem or in space flight in general.” Several researchers opined that the space-flight research was premature and recommended it be eliminated. Fortunately, it remained.114

Langley’s work throughout 1954 demonstrated one thing: the need for flexibility. Since their inception, the Brown and Becker groups had run into one technical problem after another in the pursuit of a conceptual hypersonic aircraft capable of making a space leap. Conventional wisdom had provided experimental and theoretical guidance for the preliminary design of the configuration, but had fallen far short of giving final answers. Contemporary transonic and supersonic aircraft designs dictated that the horizontal stabilizer should be located far above or well below the wing plane, for example, but that was wrong. Ballistics experts committed to pointy-nosed missiles had continued to doubt the worth of Allen’s blunt-body concept, but they too were wrong. Conversely, the instincts of Floyd Thompson, who knew very little about hypersonics but was a 30-year veteran of the vicissitudes of aeronautical research, had been sound. The design and research requirements of a hypersonic vehicle that could possibly fly into space were so radically new and different, Thompson suggested, that only “fresh approaches” could meet them. He was correct.

A CONVINCING CASE

After three months of investigations, the Becker group believed that the development of a Mach 7 research aircraft was feasible. Those at NACA Headquarters who followed the progress of their work, as well as the parallel work on hypersonic aircraft concepts at the other NACA laboratories, agreed. It was time to formally present the results to the NACA upper echelon and the Department of Defense.115

The preliminary specifications for the research airplane were surprisingly brief: only four pages of requirements, plus six additional pages of supporting data. As John Becker subsequently observed, “it was obviously impossible that the proposed aircraft be in any sense an optimum hypersonic configuration.” Nevertheless, Langley believed the design would work. At the same time, a new sense of urgency was present: “As the need for the exploratory data is acute because of the rapid advance of the performance of service [military] aircraft, the minimum practical and reliable airplane is required in order that the development and construction time be kept to a minimum.” In other versions of the requirements, this was even more specific: “It shall be possible to design and construct the airplane within three years.” The researchers were nothing if not ambitious.116

On 4 May 1954, Hugh Dryden sent a letter to Lieutenant General Donald L. Putt at Air Force Headquarters stating that the NACA wanted to initiate a new manned hypersonic research aircraft program. The letter suggested a meeting between the NACA, Air Force Headquarters, and the Air Force Scientific Advi-

111 Thrust stepping was not a new idea. The XLR11 used on the X-1 and other early X-planes had four “chambers” that could be started and extinguished individually. This allowed the thrust to be tailored for any given flight to one of four levels. There was an ongoing effort to develop a throttletable engine for the Bell X-2 research airplane. Originally assigned to Bell, the contract was moved to Curtiss-Wright. The resulting engine was the XLR9-5-CW-1 which was continuously variable from 2,500 to 15,000 lbf. Unfortunately, the engine fell significantly behind schedule and proved to be unsatisfactory.
113 Ibid.
115 Letter, Hartley A. Soule to NASA, no subject, 3 June 1954.
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Those in attendance were in general agreement that a new project was feasible. Department of Defense approval for the project would continue studying the NACA proposal, and that data at an earlier time. The meeting closed with an agreement that the military an adequate increment over existing research airplanes, and that a cooperative airplane did not necessarily make it worth building; he wanted further study be possible. However, Langley study became the starting point for further discussions since it was the most detailed available, with John Becker and John Duberg, who was substituting for Norris Dow, leading the discussions.

Those in attendance were in general agreement that a new project was feasible. However, Hugh Dryden, reflecting what John Becker described as “his natural conservatism,” stated that the fact it was feasible to build such a research airplane did not necessarily make it worth building; he wanted further study before deciding. The Navy representative indicated that some “military objective” should be included in the program, but Clark Millikan stressed the need for a dedicated research airplane rather than any sort of tactical prototype. The group agreed the performance parameters discussed by the Langley study represented an adequate increment over existing research airplanes, and that a cooperative program would be more cost-effective and more likely to provide better research data at an earlier time. The meeting closed with an agreement that the military would continue studying the NACA proposal, and that Hugh Dryden would seek Department of Defense approval for the project.

 Unexpectedly, the Office of Naval Research (ONR) announced at the meeting that it had already contracted with the Douglas Aircraft Company to investigate a manned vehicle capable of achieving 1,000,000 feet altitude and very high speeds. The configuration evolved by Douglas “did not constitute a detailed design proposal,” but was only a “first approach to the problem of a high-altitude high-speed research airplane.” Representatives from the NACA agreed to meet with their ONR counterparts on 16 July to further discuss the Douglas study.

THE DOUGLAS MODEL 671

The “High Altitude and High Speed Study” by the El Segundo Division of the Douglas Aircraft Company had been funded by the ONR as a follow-on to the D-558 research aircraft that loosely competed with the Air Force X-1 series. Duane N. Morris led the study under the direction of the chief of the Aerodynamic Section, Kermitt E. Van Every. Although the concept is generally mentioned—briefly—in most histories of the X-15, what is almost always overlooked is how insightful was regarding many of the challenges that would be experienced by the X-15 a few years later.  

By the spring of 1954, when the X-15 approval process began, Douglas had not accomplished a detailed design for a new airplane, but recognized many of the same problems as John Becker and the researchers at Langley. The Douglas engineers also examined peripheral subjects—carrier aircraft, landing locations, etc.—that the initial Langley studies did not address in any detail.

One interesting aspect of the Douglas Model 671 was that the contractor and the Navy had agreed that the aircraft was to have two mission profiles: high speed and high altitude (with the emphasis on the latter). This was in distinct contrast to the ongoing Langley studies that eventually led to the X-15. Although the Becker team at Langley was interested in research outside the sensible atmosphere, there was a great deal of skepticism on the part of others in the NACA and the Air Force. Douglas did not have this problem—the ONR strongly supported potential high-altitude research.

Excepting the Langley work, the Douglas study was probably the first serious attempt to define a hypersonic research airplane. Most of the other companies investigating hypersonics were oriented toward producing operational vehicles, such as the ICBMs and BoMIs. Because of this, they usually concentrated on a different set of problems, frequently at the expense of a basic understanding.
of the challenges of hypersonic flight. The introduction from the Douglas study provides a good background: 122

The purpose of the high altitude study…is to establish the feasibility of extending human flight boundaries to extreme altitudes, and to investigate the problems connected with the design of an airplane for such flights.

The project is partially a result of man’s eternal desire to go higher, faster, or further than he did last year. Of far more importance, however, is the experience gained in the design of aircraft for high-speed, high-altitude flight, the collection of basic information on the upper atmosphere, and the evaluation of human tolerance and adaptation to the conditions of flight at extreme altitudes and speeds.

The design of an airplane for such a purpose cannot be based on standard procedures, nor necessarily even on extrapolation of present research airplane designs. Most of the major problems are entirely new, such as carrying a pilot into regions of the atmosphere where the physiological dangers are completely unknown, and providing him with a safe return to Earth. The type of flight resembles those of hypersonic, long-range, guided missiles currently under study, with all of their complications plus the additional problems of carrying a man and landing in a proper manner.

The study consists of a first approach to the design of a high-altitude airplane. It attempts to outline most of the major problems and to indicate some tentative solutions. As with any preliminary investigation into an unknown regime, it is doubtful that adequate solutions have been presented to every problem of high-altitude flight, or even that all of the problems have been considered. It would certainly appear, however, that the major difficulties are not insurmountable.

The Model 671 was 41.25 feet long (47.00 feet with the pitot boom), spanned only 18 feet with 81 square feet of area, and had an all-up weight of 22,200 pounds. In many respects, it showed an obvious family lineage to the previous D-558s. The fuselage consisted of a set of integral propellant tanks, and dive brakes were located on each side aft, as in most contemporary fighters.

A conventional configuration was deliberately chosen for the study, and no benefits have yet been discovered for any unconventional arrangement. Actually, for the prime objective of attaining very high altitudes, the general shape of the airplane is relatively unimportant. Stability and control must be provided, and it must be possible to create sufficient lift for the pullout and for landing; but, in contrast to the usual airplane design, the reduction of drag is not a critical problem and high drag is to some extent beneficial. The planform of the wing is unimportant from an aerodynamic standpoint at the higher supersonic Mach numbers. Therefore, it was possible to select the planform based on weight and structure and landing conditions. These considerations led to the choice of an essentially unswept wing of moderate taper and aspect ratio. 123

The empennage of the Model 671 was completely conventional and looked much like that of the Mach 2 D-558-2 that preceded it. However, Douglas realized that the design of the stabilizers was one of the greater unknowns of the design. “The tail surfaces are of proper size for stability at the lower supersonic Mach numbers, but there is some question of their adequacy at very high supersonic speeds. Further experimental data in this speed range are necessary before modifications are attempted. In addition, it may be possible to accept a certain amount of instability with the proper automatic servo controls.” 124 Unlike the Becker group, Douglas did not have access to a hypersonic wind tunnel.

Nevertheless, preliminary investigations at Douglas indicated that “extremely large tail surfaces, approaching the wing area in size, are required to provide complete stability at the maximum Mach number of about 7.” 125 Engineers investigated several methods to improve stability, with the most obvious being to increase the size of the vertical stabilizer. However, placing additional area above the fuselage might introduce lateral directional dynamic stability problems “due to an unfavorable inclination in the principle axis of inertia and the large aerodynamic rolling moment due to sideslip (the dihedral effect).” The preferred arrangement was to add a vertical stabilizer and keep the ventral and dorsal units as symmetrical as possible. However, Douglas recognized that a large ventral stabilizer would present difficulties in ground handling and during landing. The engineers proposed that the fin should be folded on the ground, unfold after takeoff, and then be jetisoned just before touchdown. Alternately, Douglas believed that some sort of autopilot could be devised that would allow the use of more conventional-sized control surfaces. 126

Douglas conducted an evaluation of available power plants, and reached much the same conclusions the X-15 program would eventually come to. The desired engine should produce about 50,000 lbf with a propellant consumption of about 200 pounds per second. The only powerplant that met the requirements was the Reaction

124 “Technical Report on High Altitude and High Speed Study,” p. 7. The wedge principle that would play such an important role in the X-15 design was still languishing in the archives, and the Bell X-2 had not provided its own contribution to understanding “high speed instability.”
125 “Technical Report on High Altitude and High Speed Study,” p. 40. “High Altitude and High Speed Study,” pp. 18-19. The eventual X-15 design took a somewhat similar approach, at least for the ventral stabilizer. By the 1970s, of course, augmentation systems were finally beginning to allow inherently unstable aircraft to fly—the Space Shuttle being a prime example.
tion Motors XLR30-RM-2 rocket engine, which used liquid oxygen and anhydrous ammonia propellants. The high (245 lbf-sec/lbm) specific impulse (thrust per fuel consumption) was desirable since it provided “a maximum amount of energy for a given quantity of propellant.” The high density of the propellants allowed a smaller tank size for a given propellant weight, allowing a smaller airframe. However, the researchers worried that since the original application was a missile, it would be difficult to make the engine safe enough for a manned aircraft.\(^\text{126}\)

Douglas had some interesting observations about drag and power-to-weight ratios:\(^\text{127}\)

The function of drag in the overall performance must be reconsidered. The effect of drag is practically negligible in the power-on ascending phase of flight (for a high altitude launch), because of the very large thrust to weight ratio. Throughout the vacuum trajectory, the aerodynamic shape of the airplane is completely unimportant. During the descending phase of flight, a large drag is very beneficial in aiding in the pullout, and the highest possible drag is desired within the limits of the pilot and the structure. In fact, during the pullout it has been assumed that drag brakes would be extended in order to decelerate as soon as possible. However, because of excessive decelerative forces acting upon the pilot, it is necessary to gradually retract the brakes as denser air is entered, until they are fully retracted in the later stages of flight.

For a given propulsion unit (i.e., fixed thrust and fuel consumption), the overall performance of the present design [Model 671] is much more dependent upon the ratio of fuel weight to gross weight that it is upon the minimum drag or the optimum lift-drag ratio. Even though the fuel is expended in approximately the first 75 seconds of flight (a relatively small fraction of the total flight time), the ultimate performance as measured by the maximum altitude is affected to a great extent by small changes in the fuel to gross weight ratio. As an example, an increase in fuel weight/gross weight from 0.65 to 0.70 results in an increase in peak altitude of about 35% for a typical vertical flight trajectory, other parameters remaining constant.

To better understand the nature of the various propellants then available for rocket engines, engineers reviewed numerous reports by the Caltech Jet Propulsion Laboratory, the NACA, and RAND. Only two oxidizers—oxygen and either red fuming or white fuming nitric acid—seemed to offer any increase in performance. Douglas was seeking better propellants than the liquid oxygen and alcohol used in the Reaction Motors LR8, effectively ruling out nitric acid since it was less dense than oxygen. The available fuels were alcohol (CH3OH or C2H5OH), anhydrous ammonia (NH3), hydrazine (N2H4), and gasoline. Alcohol offered no improvement, and hydrazine was too expensive and too difficult to handle safely, narrowing the choice to anhydrous ammonia and gasoline. Interestingly, Douglas ruled out liquid hydrogen because “on the basis of density, hydrogen is seen to be a very poor fuel.” It would be 20 years before the Centaur upper stage would prove them wrong.\(^\text{128}\)

An auxiliary power unit (APU) rated at about 8 horsepower was necessary to support the electrical requirements of the instruments, controls, and radio. Investigation showed that the lightest alternative would be a small turbine generator using hydrogen peroxide or ethylene oxide monopropellant. The Walter Kidde Company and American Machine and Foundry Company were develop-
Engineers spent a great deal of time studying possible flight paths, but “no attempt has been made in the present study to determine an absolute optimum flight path, because of the large number of variables involved.” The designers noted that the airframe and propulsion systems could theoretically support a maximum altitude in excess of 1,130,000 feet; however, based on a conservative pullout altitude of 30,000 feet, the vehicle was more realistically limited to 770,000 feet. The pullout altitude (and the limiting decelerations, which were really the issue) was “directly traceable to the single limiting factor of the presence of a human pilot.” The 770,000-foot, 84-degree profile resulted in a 10-g pullout maneuver, about the then-known limit of human tolerance.132

Some thought was given to using a “braking thrust,” which would allow a small amount of propellant to be saved and used during reentry. Either a mechanical thrust reverser would be installed on the rocket engine, or the airplane would reenter tail-first. This technique would have allowed slightly higher flights by reducing the stresses imposed by the pullout maneuver, although less propellant would be available for the ascent. The designers did not pursue this concept since entering tail-first involved undesirable risks, and the mechanical complexity of a thrust reverser seemed unnecessary, at least initially.133

The theoretical maximum performance was 6,150 mph and 190,000 feet for the speed profile, and 5,200 mph and 1,130,000 feet for the altitude profile (but limited, as discussed above). Landings would be made at Edwards AFB because of its “long runways and considerable latitude in the choice of direction and position of touchdown.” The study noted that there would be little opportunity to

130 “Technical Report on High Altitude and High Speed Study,” pp. 54 and 58. In 1954 calculations of this nature normally were done by hand since general-purpose electronic computers were not widely available, and were quite slow in any case.
control either the range or the heading by any appreciable amount after engine burnout. “Since the airplane must land without power at a specified landing site, it is obvious that it must be aimed toward the landing site at launch.” Douglas estimated that a misalignment of 5 degrees in azimuth at burnout would result in a lateral miss of over 45 miles.135

One of the concerns expressed by Douglas was that “rocket thrust will not be sufficiently reproducible from flight to flight, either in magnitude or in alignment.” Engineers estimated a thrust misalignment of less than one-half of a degree could impart 500 pounds of side force on the aircraft, causing it to go significantly off course. Researchers investigated several possible solutions to thrust misalignment, including using a larger rudder, using the auxiliary reaction control system, installing movable vanes in the exhaust,136 performing gas separation in the nozzle,137 and mounting the rocket engine on a gimbal. All of these methods contained various problems or unknowns that caused the engineers to reject them. Further consideration showed that thrust misalignment was largely a non-issue since early low-speed flights would uncover any deficiencies, allowing engineers to correct them prior to beginning high-speed flights.138

The estimated landing speed was 213 mph, with a stall speed of 177 mph. Engineers accepted this relatively high speed “given the experimental nature of the aircraft and the high skill level of the pilots that will be flying it.” The study noted that the slower speeds were possible if high-lift leading-edge devices were used or the area of the wing was increased. However, the increased weight and/or the resulting complications in the leading-edge cooling system appeared to make these changes undesirable.139

The high-altitude profile would use “flywheels, gyroscopes, or small auxiliary jets” for directional control outside the atmosphere, with Douglas favoring hydrogen peroxide jets in the wing tips and at the rear of the fuselage. Flywheels were rejected because they were too complex (for a three-axis system), and gyroscopes were too heavy. Each of the hydrogen peroxide thrusters would generate about 100 lbf and use 1 pound of propellant per second of operation. The engineers arbitrarily assumed that a 25-pound supply of propellant was required since no data existed on potential usage during flight. A catalyst turned the liquid hydrogen peroxide to steam at 400-psi pressure.140

The projected performance of the airplane caused Douglas engineers to investigate escape capsules for the pilot: “Because of the high altitude and high speed performance of the aircraft, it is believed that all ordinary bailout procedures, such as escape chutes and ejection seats, are of no value to the pilot.” At the time, Douglas believed that ejection seats were only “suitable up to a Mach number of approximately one at sea level, with somewhat higher speeds being safe at higher altitudes.” Instead, the engineers decided to jettison the entire forward section of the fuselage, including the pilot’s compartment, much like the Bell X-2. The total weight penalty for the capsule was about 150 pounds. The study dismissed pressure suits, stating that “it is very doubtful that sufficient pressurization equipment could be carried by the pilot during...ejection...to sustain suit pressurization from the maximum altitude to a safety zone within the earth’s lower atmosphere.” Douglas stated flatly that “an ejection seat or other ordinary bailout techniques will be inadequate in view of the problem of high speeds and high altitudes.” Scott Crossfield would later disagree.141

In order to withstand the reentry temperatures, the cockpit windscreen used two 0.5-inch layers of quartz with a 0.25-inch vented air gap between them. This would keep the inner windscreen below 200°F. A thin sheet of treated glass placed inside the inner quartz layer reduced ultraviolet and other harmful radiation. Although the potential dangers of radiation above the atmosphere were largely unknown, Douglas predicted that little harm would come from the short flights (a few minutes) envisioned for the D-558-3. However, “proper precautions to prevent any one pilot from making too many successive flights in a week or months time interval should be taken...”142

One of the technical innovations of the eventual X-15 program was the “ball nose” that sensed the angle of attack and angle of sideslip during high-speed and high-altitude flight. The Douglas study foresaw the need for a new pitch and yaw sensor “capable of sensing exceedingly low forces or pressures, but capable of withstanding the maximum dynamic pressures encountered during the complete pullout.” However, Douglas thought that “the instrument need not be precise, for it is only to serve as a guide for pointing the nose into the wind at heights where a pilot might otherwise lose all sense of orientation.” Four possible solutions emerged:143

1. A weathervane, either direct or remote-reading
2. A pitch or yaw indicator that measured the relative Mach number or pressure ratio on opposite sides of a symmetrical sphere, cone, or other convenient shape
3. A vane inside a conventional instrument case that indicated the direction of the resultant momentum from two jets of air brought in by a pair of symmetrical external tubes
4. A device similar to the Reichardt gage

Douglas dismissed the first two (although the second one is what was eventually built for the X-15) since they did “not seem very satisfactory.” The external weathervane would need to feature rugged construction to resist the high aero loads and would therefore be too insensitive at high altitudes. Douglas discount-

136 The same technique used by the V-2 and several other early rockets.
137 This involves injecting a small amount of gas along one wall of the exhaust nozzle, causing a flow separation that results in slightly asymmetrical thrust. The solid rocket motors for the Titan III/IV launch vehicle later used the same technique.
140 “Technical Report on High Altitude and High Speed Study,” pp. 42-43. In 1954, manned space flight was still seven years in the future, and no airplane had yet flown above the sensible atmosphere. This made it impossible to guess accurately how much control a pilot would want, or need, at extremely high altitudes.
143 Ibid, pp. 45-46.
Nevertheless, despite the seemingly thorough study, Douglas noted that there were many uncertainties since they were entering previously unknown areas of aeronautical science. Highlighting this, the final report contained statements such as “[t]here is no method available for the calculation of the supersonic, zero-lift, pressure drag of a finite wing with a laminar flow airfoil section” and “no theoretical methods have been devised for the calculation of the theoretical supersonic section drag coefficient of a blunt nose airfoil.” It was all very speculative.

Other areas of concern were calculating (or even understanding) the compressibility effects of turbulent flow at high speeds. The compressibility effects in laminar flow were calculated using factors corresponding to the results of Crocco and Van Driest, but engineers noted that the corresponding correction for turbulent flow was “difficult to determine.” At the time there were a number of different theories for the turbulent corrections, all of which appeared equally valid but led to widely divergent results when extended to higher Mach numbers. The proper choice of a compressibility correction was important because between Mach numbers 3 and 10 the uncorrected skin friction accounted for 40–50% of the total zero-lift drag. Douglas chose to use the Van Driest results that predicted a relatively large decrease in turbulent skin friction as Mach numbers increased, although the engineers noted that the results “may be somewhat optimistic.” These were many of the same problems investigated by John Becker, Charles McLellan, and others at Langley.

According to the Douglas representative at the 16 July meeting with the NACA and ONR, the next step would be a more detailed study that would cost $1,500,000 and take a year to complete. Given that a new joint project was about to be undertaken, the ONR declined to further fund the Douglas study, and the company began to concentrate its high-speed efforts on the Model 684 that would be proposed to the Air Force for Project 1226.

Overall, Douglas anticipated many of the problems that were ultimately encountered during the development of the eventual hypersonic research airplane. It would not have surprised any of the engineers working on the Douglas study that the solutions they proposed for some of the problems were not the ones that were ultimately implemented. Still, they touched on almost all of the pitfalls that would hamper the development of the eventual X-15. It is difficult to say whether Douglas could have done the job better, faster, or cheaper (to use a much later vernacular). It is likely, however, that they ultimately would have succeeded in building a useful research aircraft if the government had continued down that road.

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144 Ibid, pp. 44–46.
145 "High Altitude and High Speed Study," p. 26. At the time, almost no data actually existed on the number or size of micrometeorites, or the likelihood of their striking an orbiting object.