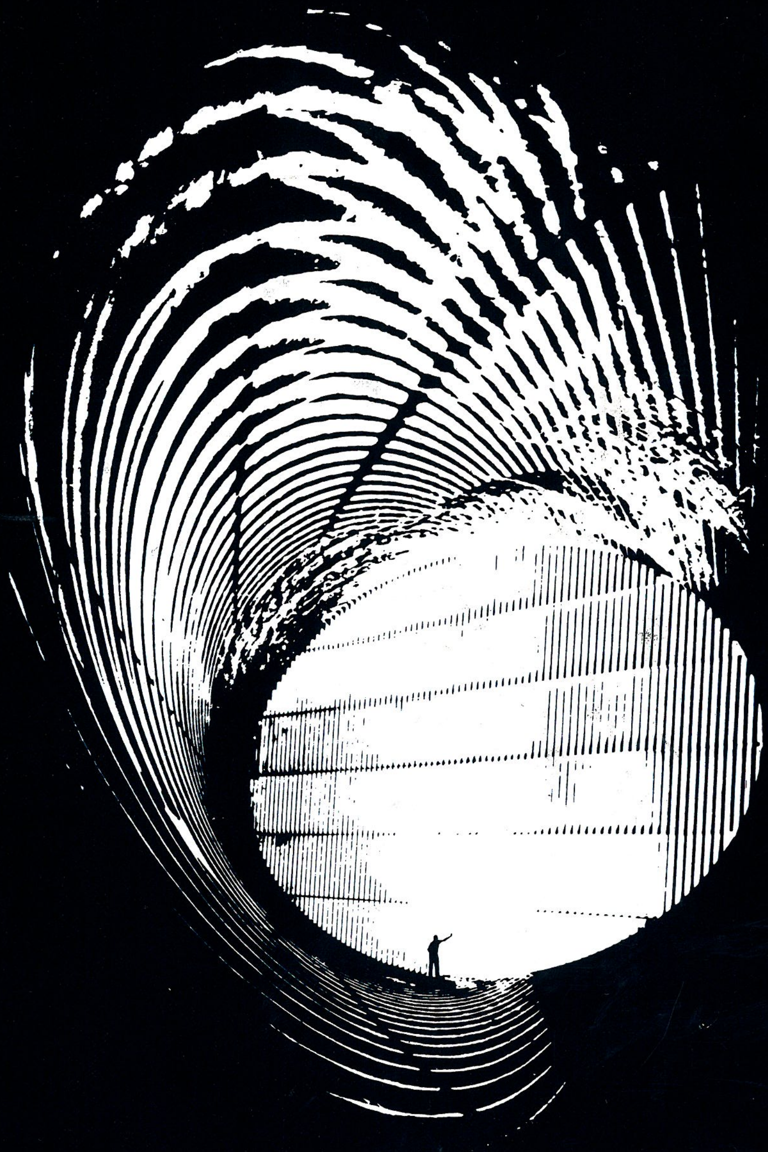
ENGINEER IN CHARGE



A History of the Langley Aeronautical Laboratory, 1917-1958

ENGINEER IN CHARGE

The youthful engineer-in-charge Henry J.E. Reid sits at his desk, April 1928.

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A History of the Langley Aeronautical Laboratory, 1917-1958

James R. Hansen

The NASA History Series

Scientific and Technical Information Office 1987
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NASA maintains an internal history program for two principal reasons: (1) Sponsorship of research in NASA-related history is one way in which NASA responds to the provision of the National Aeronautics and Space Act of 1958 that requires NASA to "provide for the widest practicable and appropriate dissemination of information concerning its activities and the results thereof." (2) Thoughtful study of NASA history can help agency managers accomplish the missions assigned to the agency. Understanding NASA's past aids in understanding its present situation and illuminates possible future directions. The opinions and conclusions set forth in this book are those of the author; no official of the agency necessarily endorses those opinions or conclusions.

On the cover: An NACA engineer inspects the turning vanes in Langley's 16-Foot High-Speed Tunnel in 1951.

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Foreword

Langley has had a remarkable history, not only during three decades as NASA Langley Research Center, but in an earlier period as well: during Langley's four decades as the flagship research facility of the National Advisory Committee for Aeronautics (NACA). Long before spaceflight, Langley Memorial Aeronautical Laboratory (later, Langley Aeronautical Laboratory) began its work incubating the ideas and hatching the technology that made American aviation take off and fly. James R. Hansen here offers us that story.

More than just an outlining of historical facts is to be found here, for Hansen has captured the very culture of Langley. He has done so by illustrating what I see as the four major aspects of the laboratory: people, facilities, program, and customer relations.

People, of course, have always been the most important aspect of this unique place, so it is good to see the people themselves studied so carefully in its first complete history: people like Eastman N. Jacobs, who energetically engineered many of the early programs, and Theodore Theodorsen, whose powers as an applied mathematician and theorist made him sometimes the rival but always the complement of men like Jacobs; or like Max M. Munk, the brilliant and difficult prodigy of Langley's early years; or like Fred E. Weick, an aviation pioneer for most of this century; or like the skilled makers of special instruments, tools, and models who practically invented the various fields of supporting work for aerodynamical research; or like John Stack, Richard T. Whitcomb, Robert T. Jones, Robert R. Gilruth, John V. Becker, and all the other engineers and researchers whose names permeate this book. Hansen is seriously concerned with the motivations, the training, the personalities, the hopes of people who cause aeronautical science and technology to evolve. Indeed the very title of the book, in stating his theme of the practical-minded engineer moving the laboratory's work toward feasible, useful solutions of aviation problems, shows Hansen's respect for the importance of people in the story of the laboratory. No doubt there are many benefits for the rest of us in the work of historians; surely the chance to see and know as individuals the people who preceded us must be one of them.

The people who lead a research institution, and the people who do the work, always to a greater or lesser degree face the same kinds of choices regarding the next two of the four important aspects of Langley: facilities and program. How, for instance, can the allotted budget best be used? How much money will there actually be? How much risk should be taken in building a machine that may not help you learn precisely what you need to learn? How good is the chance that it will help you learn answers to the questions you do not at present even know enough to ask? Should resources be committed to investigation of this or that possibly bright but predictably expensive-to-study idea? How far should you stray from the planned path of a research program to seek for possible extra benefits when they appear attainable?

Engineer in Charge conveys a wealth of Langley's institutional experience in dealing with these kinds of questions about facilities and program. Hansen tells how Langley's first wind tunnel came to have an open circuit—a safe and proven design, but much less useful than the closed-circuit tunnels then coming into their own. The rapid subsequent evolution of wind tunnels, much of which took place at Langley, involved further choices that required commitment of funds and time and effort without certainty of getting the hoped-for results. And always the facilities needed to be stretched to maximize the benefits of the research program. Readers of Hansen's book will all but hear the Langley engineers of a half-century ago saying, if only we can build this or that new tunnel, or try this or that new piece of gear, or get permission to work on such-and-such new technology, we might really get somewhere .... Readers will find themselves watching the evolution of the facilities and program at NACA Langley, from the early quantum improvements in aircraft design to the pre-NASA work that foreran the various space programs.

Hansen also traces Langley's fourth important aspect, its relations with the industrial, scientific, and technical community it was built to serve. While the laboratory has a strong tradition of independent research, it also has a tradition of solving the problem of the moment—of "fighting fires." During my tenure as Langley director, the most striking example of this ability was the work of over 300 Langley engineers and technicians on the space shuttle thermal protection system, the tiles that protect the shuttle from the intense heat of reentry into the atmosphere from space. But there have been many other examples of Langley's ability and readiness to apply concerted effort in overcoming aeronautical development problems. Readers will find them here.

Readers will also find here the background of these customer relations—not only the "what" of Langley's work with the larger aeronautical community of which it has been a part, but the "how" and the "why" as well. While Hansen has defined for himself the primary task of telling Langley's story in terms of Langley itself, he has nonetheless devoted extensive effort to showing how Langley worked with Washington, with aircraft manufacturers, and with the armed services and others. He brings to life such episodes as the old annual manufacturers' conferences, the pre-World War II affairs that were for Langley part business, part public relations, even part fun; he shows how the laboratory coordinated its various efforts for military aviation; he even probes the various ways in which Langley drew on international resources, from individual aerodynamicists in friendly countries to captured research results at the end of the second world war.

The importance of a history such as this book is to better understand the character of an organization and what it will mean to the future. There is a living memory at Langley, an awareness of the triumphs, and for that matter the failures, of the laboratory's past. But a living memory is in most respects an incomplete and anecdotal memory, a mixture of hearsay and hand-me-down impressions, a collection of stories embellished by time and imagination, an awareness of some of the facts, a misunderstanding of others. What is needed is a systematic arrangement of what is known, a synthesis of what is recorded on paper and film with what is remembered by surviving participants—in short, what is needed is a sort of accurate rejuvenation of the living memory.

Langley has only just begun to be called upon by the aerospace community for the things only Langley can provide. NASA has called upon James R. Hansen for an accurate rejuvenation of Langley's living memory. Here it is.

January 1986

Donald P. Hearth
Director
Office of Space Science and Technology
University of Colorado

and *Director (1975–1985)*
Langley Research Center

Acknowledgments

No book published is ever solely the work of the author. This is especially true in the case of *Engineer in Charge*. So many people have helped me with its production, and in so many different ways, that I sometimes think about the book as a community project and about my role in writing it as that of a project leader. This holds true only for the book's strengths, however; its faults and shortcomings are my own.

Gratitude beyond measure goes to Richard T. Layman and Laurence K. Loftin, Jr., for the extraordinary attention and enthusiastic support they gave my work from start to finish. Every NASA center should have a historical program coordinator as bright, conscientious, and interested in serious scholarship as Dick Layman is. Larry Loftin was also a strong booster and confidant. He surrendered countless hours to fielding my questions and responding critically yet cordially to preliminary drafts of my chapters.

NACA/NASA researchers John V. Becker, Richard R. Heldenfels, Axel T. Mattson, and John P. "Jack" Reeder shared several illuminating stories that I would not have known otherwise, since they were not part of the documentation. Along with Loftin, this quartet kept me on track technically. Getting to know these men gave me a far greater appreciation of the special capabilities of engineers.

I would like to thank many other Langley veterans individually for their contributions of historical information: Marie Bird Allen-Burcher, Donald D. Baals, Emanuel Boxer, Clinton E. Brown, D. William Conner, Martin Copp, John E. Duberg, Macon C. Ellis, Jr., I. Edward Garrick (deceased), Vera Huckel, Helen Johnson, Upshur Joyner, Kitty O'Brien-Joyner, James G. McHugh, Jane Moore, W. Hewitt Phillips, Edward C. Polhamus, Joseph A. Shortal, William C. Sleeman, Jr., Hartley A. Soulé, Betty Toll, Virginia Tucker, Catherine Turner, Harold Turner, Sr., Richard T. Whitcomb, and Helen Willey.

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Special thanks also to the talented and hard-working staff of the Floyd L. Thompson Technical Library for being so generous with their time and assistance.

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I am grateful to Elizabeth A. Muenger, Command Historian at the United States Air Force Academy, for permitting me to see preliminary drafts of her book *Searching the Horizon: A History of Ames Research Center, 1940–1976,* NASA SP-4304 (GPO: Washington, 1985).

Michael D. Keller, academic dean at Missouri Valley College in Marshall, Missouri, completed a successful doctoral thesis (University of Arizona, 1967) on the history of NACA Langley to 1946. I am grateful for our friendly phone conversation some time back, and hope to meet him someday to share our Langley stories.

Heartfelt thanks to Steve Corneliussen, my discerning and diligent editor, who read a fat manuscript with enthusiasm and a meticulous eye for errors. His work improved my product immensely.

Finally, I would like to acknowledge a great debt to Peggy, my wife, who listened with only sometimes flagging attention to hours of dissertation about the NACA, and to Nathaniel and Jennifer, our children. I left my family alone on far too many weekends to squeeze in a couple of extra hours of work on *Engineer in Charge.*

January 1986

James R. Hansen
Poquoson, Virginia

Introduction

On the most superficial level the title of this book refers to the officer who first headed Langley Aeronautical Laboratory, the original and, until 1941, the only research center of the National Advisory Committee for Aeronautics. In all of NACA history—1915 to 1958—there were only two such officers: Leigh M. Griffith (b. 1882, d. 1940) and Henry J. E. Reid (b. 1895, d. 1968). Griffith served as engineer-in-charge from 1923, when NACA headquarters created the position, through 1925; Reid succeeded Griffith in 1926, filling the top position at the lab until his retirement from the National Aeronautics and Space Administration (NASA)* in 1960.

For the last twelve years of his career, however, Reid managed Langley not as the engineer-in-charge but as the "director." In 1948 the NACA changed his twenty-two-year-old title in anticipation of Public Law 167 (passed 13 July 1949, 81st Congress, first session). This law authorized the chairman of the NACA to create "ten positions in the professional and scientific service" of the federal government, "each such position being established in order to enable the NACA to secure and retain the services of specially qualified personnel necessary in the discharge of the duties of the Committee to supervise and direct the scientific study of the problems of flight with a view to their practical solution."[1] Before this time Reid's salary as engineer-in-charge was boxed in below $9,000 per annum by civil service criteria which discriminated against engineers in favor of scientists. The decision in 1948 to change Reid's title to director was thus part of a larger NACA scheme to increase annual pay to its top executives, and enhance their social and professional status, by giving them academic-sounding titles like those borne by individuals in the higher grades of the federal bureaucracy. As Vannevar Bush, chairman of the NACA from 1939 to 1941, had once said, a scientist may sell a bill of goods to Congress when an engineer could not get a street car token on Capitol Hill.[2]

Despite the increased pay, Reid was privately reluctant to have his official identity changed after so many years. Like Griffith before him, he was

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* Usage varies on the pronunciation of the names *NACA* and *NASA*. In this book, *NACA* is meant to read as four individual letters ("the N-A-C-A"), while the acronym *NASA* reads as a two-syllable word.

an engineer and proud of it. He had earned a bachelor's degree in electrical engineering nearly half a century before at Worcester Polytechnic Institute in Massachusetts, and much of his work, both before and after becoming Langley's engineer-in-charge in 1926, truly classified as engineering. To have worked in the field of aeronautics in the 1920s and 1930s was to have been a participant in what was indisputably one of the greatest and most rapidly successful engineering adventures in all history. Born into a world without flying machines, his generation had known the airplane at a time when it barely worked, yet lived to see it perform wonders. These achievements, Reid believed, had been largely the result of practical engineering solutions to the outstanding scientific and technical problems of flight. Congress had created the NACA in 1915 "to supervise and direct the scientific study of the problems of flight with a view to their practical solution." But in practice at Langley, the keystone of the organization's charter had rested chiefly in the end of that phrase, "with a view to their practical solution." This meant that aeronautics had been treated not so much as a scientific discipline, but as an area for engineering research and development. Most Americans did not know how significantly the NACA laboratory of which Reid had been in charge had contributed, and was continuing to contribute, to solutions of this sort; most Americans did not even know that the NACA existed. This anonymity frustrated Reid occasionally—though he knew it had certain political advantages. However irritating it had been to hear the unknowing public frequently giving scientists all the credit for accomplishments he felt rightfully belonged to engineers, the irritation never prompted him to question his organization, profession, or identity as the engineer-in-charge.

Neither Reid nor anyone else who knew anything about Langley ever doubted that at nearly all levels of laboratory research activity, not just at the top, it was the engineer who was in charge. Novelist James Michener picked up on this fact in the late 1970s during interviews with veteran NACA employees. In his book *Space*, Michener has a crusty, white-haired wind tunnel jockey scolding a new employee for crediting a scientist with the design of the 16-Foot High-Speed Tunnel: "Scientists are men who dream about doing things," he reprimanded the young man. "Engineers do them. This [tunnel] was designed by engineers, built by engineers, and is run by engineers. You're an engineer, young fellow, and you're to be proud of it."[3] Wind tunnels and other test equipment—which required engineering talents for design, development, operation, and exploitation—formed the laboratory's backbone. By the end of the 1930s, fifteen of the eighteen aerodynamics sections at Langley were named after wind tunnels or other specific experimental setups. The only parts of the lab with names suggesting a theoretical approach to research were the Mathematical,

The mature Henry J. E. Reid, admires Langley's first Collier Trophy in 1955, twenty-six years after the NACA won it for the development of the low-drag engine cowling.

Flutter and Vibration, and Propeller Sound and Noise research sections, all of which belonged to the small and somewhat isolated Physical Research Division.

Langley's penchant for experimental programs was in fact very appropriate for the actual state of aeronautics in the period 1915 to 1930, the years when the NACA developed its own operating style. To a considerable degree, the empirical approach for which the agency became well known seems to have been dictated by the nature of the aircraft design problems confronting the laboratory after World War I and by the inadequacies of theory in addressing them.

The generation of airplane designers responsible for some of the most famous World War I aircraft had used an intuitive, daring empirical engineering loosely connected at best with any theory of aerodynamics or structural integrity. Geoffrey de Havilland (1882–1965), Anthony Fokker (1896–1939), and Gianni Caproni (1886–1957), among others, produced stable and maneuverable airplanes using essentially a job-shop approach. The British designer Thomas Sopwith (b. 1888) never put his early planes through a single stress test. One of his prototypes could be designed, constructed from full-size chalk drawings on the factory floor, and test flown in less than three months.

Curtiss JN4H Jennies prepare to take off from Langley Field for NACA flight research in the spring of 1919.

During the 1920s frail wooden biplanes covered with fabric, braced by wires, powered by heavy water-cooled engines, and driven by hand-carved wooden propellers still ruled the airways. This meant that very large gains in aerodynamic efficiency—perhaps even the biggest payoffs then possible—would follow almost immediately once the aviation establishment possessed correct answers to just a few questions, such as:

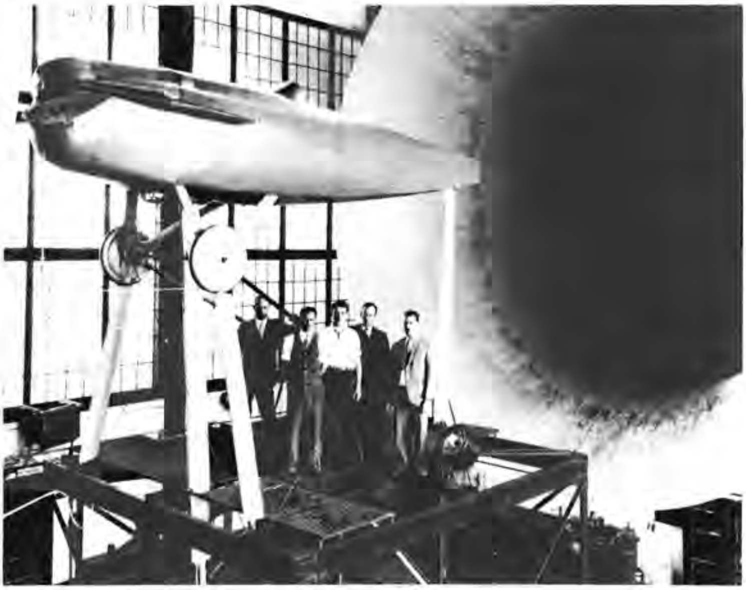
> Can drag be reduced without degrading cooling? If so, how?
> How can wings be shaped to increase lift at low speeds and decrease drag at high speeds?
> How and when do flaps work best?
> How can effectiveness and control force be accurately predicted for ailerons, elevators, and rudders?
> Is it worthwhile to retract landing gear?

In essence, each of these questions was asked as a result of someone's practical concern and thereby fell into the category of applied fundamental research. For the most certain progress toward practical solutions, engineering talents were required, along with wind tunnels and other sophisticated experimental equipment which, at the time, only the federal government could afford. Because it was staffed and managed mainly by engineers and experienced an early proliferation of large and unique test facilities, like the Variable-Density, Propeller Research, and Full-Scale wind tunnels, Langley laboratory was admirably suited to handle the technological problems at hand.

Although the NACA's original laboratory was named after scientist Samuel P. Langley, the hero of most NACA engineers was Orville Wright. This photograph was taken during Wright's visit to Langley in July 1922. Wright was a member of the NACA for nearly thirty years.

On the other hand, the professional disposition of the Langley staff might have assured the victory of empirical approaches regardless of the nature of the aeronautical research problems of the day. George Lewis, the director of research for the NACA in Washington from 1919 to 1947, was, like Griffith and Reid, also an engineer. Though he respected theoreticians and employed a few at Langley, Lewis wanted "his boys" at the lab to look for practical solutions. It should come as no surprise that a laboratory in which engineers prevailed formulated problems in a way that required for their solution just those methods, techniques, and apparatuses in which the engineer himself was especially skilled.

In a famous paper on wing section theory published by the NACA in 1931, Langley physicist Theodore Theodorsen suggested that the laboratory staff sometimes tied the progress of their work so completely to the use of test equipment that the equipment started to use them.[4] For example, while possession of the world's first full-scale propeller research tunnel presented Langley in 1926 with a unique opportunity to explore systematically the potential of dozens of different cowling shapes and arrangements, having this large and costly research plant also obligated the lab's researchers to make full and routine use of the facility.



Nearly every chapter of Langley history involves the design or utilization of wind tunnels. But the lab's story also demonstrates that no research facility, however superb its design, can be productive over the long haul without a staff that learns how to use it wisely and in ways new and different from those originally conceived. One Langley facility whose record benefited greatly from the work of a creative engineering staff was the Propeller Research Tunnel. From left to right, in 1929: Fred E. Weick, PRT section head, Ray Windler, William H. Herrnstein, Jr., John L. Crigler, and Donald H. Wood, assistant section head.

The symbiosis between engineer and wind tunnel would grow so strong over the years that it was often almost impossible for management to put a machine out of business. The closing of some tunnels that had reached the point of diminishing returns—like the Propeller Research Tunnel in the 1940s—was accomplished only by overpowering stubborn defenders. Sometimes even after equipment was formally abandoned, old operators tried surreptitiously to run tests with it. Demolition proved the only sure way to end a tunnel's life.

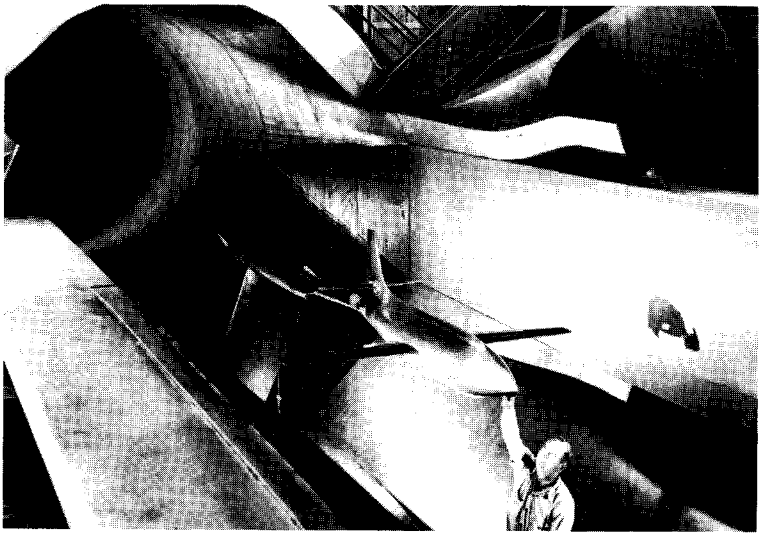
Whether it was the professional disposition of the staff or the nature of the aircraft design problems then confronting it that gave the NACA laboratory its strongest dose of empirical flavoring, engineering was what NACA Langley was all about. Once by the late 1920s it had settled into its niche in the American aeronautics community, it never really intended to do anything else. Langley did little "basic" or "scientific" research in the usually accepted senses of those words; rather, almost every investigation done there, whether "fundamental" or "developmental," aimed at a useful aircraft application.[5]

Though empiricism clearly predominated at the laboratory, a careful study of Langley history also shows that some NACA researchers were more than willing and more than able to resort to theoretical analyses when necessary. Demonstration of this should lead to revision of some current historical interpretations of the NACA, such as the one expressed by Edward Constant II in *The Origins of the Turbojet Revolution*. Professor Constant argued in one chapter of this deservedly prizewinning and influential book that the NACA performed "first-rate empirical work" but accomplished "minor work, or no work at all, on fundamental theory." His interpretation, partly accurate only if one uses a strict construction of the term "fundamental," implied that the NACA accomplished its empirical work without the help of any theory, an oversimplification which the history of several Langley programs—most notably the development of laminar-flow airfoils in the late 1930s (see chapter 4) and of slotted-throat transonic tunnels in the late 1940s and early 1950s (see chapter 11)—shows to be misleading, if not wholly incorrect. The NACA might have been "widely recognized for the excellence of its experimental data and for little else," as Constant stated, but this recognition was based on a popular misunderstanding of the subtle but often necessary interplay between theory, experiment, and design in successful engineering science.[6] In any case, the "little else" had its profound effects upon aviation.

The unwritten rule for the work of any engineer is to bring everything to bear on solving the problem of the moment. This means bending every effort, be it cut-and-try, experimental, theoretical, or any combination of the three. The bias of an engineer against theory is not that of the philosophic doctrinaire; the engineer simply knows from his working experience that theory has its limitations, that there are too many things in life that draw one aside from the charm of a theory, and that facts often murder a theory. Nonetheless all of the better engineers at Langley ultimately realized that they needed some solid theoretical ability—because one never knew when it might be essential. They understood more and more, especially as the time arrived when flow velocities over parts of aircraft approached the sonic regime, that aerodynamic refinement could not go on endlessly on a purely empirical basis. Without some constant theoretical guidance, they would seek answers to too many ill-conceived and unnecessary questions. Motivated by an awareness of this potential danger, several Langley engineers worked hard from the mid-1930s to master such subjects as applied mathematics. This mastery enabled them to make some notable theoretical contributions and to provide valuable consultation to the rest of the staff. Consequently, the theoretical analysis necessary for lifting an experimental series beyond the occasional impasse was usually accomplished in-house.

To say that the NACA employed some researchers with excellent theoretical capabilities is not necessarily to say that it had a sufficient supply of them. Theoretical aerodynamicists were hard to find in the United States. American aeronautical programs by and large produced engineers of the practical sort described earlier. Stanford University's aerodynamics professor Elliott G. Reid, who worked at Langley from 1922 to 1927, complained in the preface to his 1932 textbook on *Applied Wing Theory* that "the average graduate of an American technical school cannot be expected to be very familiar with fluid mechanics, to have a working knowledge of potential theory, or to have facility in the use of either the complex variable or Fourier series" because neither the teachers nor the textbooks were there to instruct about such advanced information or problem-solving methodologies.[7] If more crackerjack theoretical aerodynamicists had been available, the NACA would have hired them. But to hire people and call them theoreticians when they were not really very good at theory would have served no useful purpose. Thus the most influential theoreticians at Langley before World War II were Max Munk and Theodorsen, two accomplished European imports, and the most influential theoretician at Langley after the war was Adolf Busemann, the accomplished German aerodynamicist who had fathered the swept-wing concept.

Without a doubt, the NACA could have benefited from more theoretical capabilities. But it is equally true to say that the NACA could have benefited from more experimental capabilities. The problem for management besides acquiring a sufficient number of talented experimentalists and theoreticians was motivating the two types of researchers to work together productively. I. Edward Garrick (1910–1981), a talented mathematician who worked closely with Theodorsen in the 1930s and 1940s, believed in retrospect that NACA management might have stimulated a more gainful exchange between theory and experiment if fewer researchers had been organized around use of any given experimental facility. If not for this clustering around equipment, Garrick suggested, theory might have come to the aid of experimentation more quickly and less accidentally than it typically did.[8] One can thus wonder, as some veteran Langley researchers now do, whether the NACA might have improved its total performance by forcing direct interaction between different types of research sections more often, no matter how this might have upset those temperamental individuals who headed them. One can also wonder whether the NACA might have achieved more by fostering a few small groups of imaginative individuals possessing



A Langley engineer inspects his installation of a model of the Bell X-1 supersonic airplane in the new slotted test section of the 16-Foot High-Speed Tunnel in March 1951.

both inventive engineering skills and creative theoretical talents, and then encouraging these groups to consider the possibilities lying beyond contemporary technology.

The title of this book suggests a final meaning. Despite the influence of economic, political, and military objectives over NACA programs, Langley engineers enjoyed considerable freedom to advance research as they, not others, wanted. To a great extent, then, they were themselves "in charge" of what they did and how they did it. In this respect one may compare the position of a Langley engineer to that of an architect. Though "dependent upon commissions from patrons for the opportunity to work out his ideas," an architect "can usually design a building which reflects his personal artistic ideals and intentions as well as serving the client's needs."[9] The best architects are also great artists, and so also are the best engineers. It is thus important to address not only the bureaucratic circumstances in which Langley engineers worked, but also the engineers' personal drives and intentions. As historian Arnold Pacey wrote in *The Maze of Ingenuity: Ideas and Idealism in the Development of Technology:*

> Economic or military needs may give the engineer or inventor his opportunity, but they can rarely provide much stimulus to his imagination. To understand where that came from, we must ask what it is that really excites him about technology.[10]

We just may find that what excited Langley engineers most of all was the spirit of adventure and exploration.

* * *

Engineer in Charge is basically a technical analysis of NACA history from the perspective of Langley laboratory to complement Alex Roland's headquarters-centered institutional study, *Model Research.* I first read Roland's manuscript in October 1980; this was six months after he completed the first draft of his book and eight months before I would spend my first day under NASA contract researching Langley history. Quite naturally, his scholarship guided my endeavor greatly. But *Engineer in Charge* will hopefully do more for the reader than gloss Roland's previous work. From the beginning, I tried to move the analysis of NACA history in entirely different directions and to offer new ways of looking at some of the same things. By this I certainly do not mean to say that my intention was to build a rival interpretation between which readers of the two books could choose. Rather, my purpose was to add another dimension to Roland's overall story.

Neither was it my idea to recite in detail all of Langley's technical achievements. There were too many research programs, major and minor, conducted at the lab over too many years for that end to be achieved, even if I had thought it desirable. Instead, my plan was to explore the histories of (1) the most technologically significant research programs associated with the lab, and (2) those programs that, after preliminary research, seemed best to illustrate how the lab was organized, how it worked, and how it cooperated with industry and the military.

In looking back over this book, I can see how informed readers might think that my approach resulted in a somewhat positive distortion of the Langley record. Citing my emphasis on the most technologically significant research—i.e., programs that led to the low-drag cowling, laminar-flow airfoils, wartime drag reduction, supersonic flight, transonic tunnels, the area rule concept, and spaceflight—they might argue that little if any room was left for the many draws and defeats of NACA research (like the NACA Langley "failure" in early jet propulsion research, the subject of chapter 8). They might wonder whether the projects whose histories I have presented are just those that Langley veterans trotted out before me to demonstrate how good they were.

I acknowledge these concerns. Nevertheless I stand by the approach I have taken, for I believe it has led to the most useful understanding of Langley. After some months of preliminary research and oral interviewing, I discovered that there was much more to the supposedly well known projects than what had been published in contemporary newspaper and magazine stories or in George W. Gray's book *Frontiers of Flight: The Story of NACA Research* (New York: Alfred A. Knopf, 1948). And what was known was often misleading. More astonishingly, I found remarkably little agreement even among NACA veterans over how these projects had come about, how they had been conducted and managed, and what they ultimately signified. Moreover, previous historical treatments of these projects (with the exceptions of Roland's book and Richard Hallion's *Supersonic Flight: The Story of the Bell X-1 and Douglas D-558* [The MacMillan Co., 1972]) had not benefited from significant research in Langley's correspondence and research authorization files.

I thus felt the need to clarify important episodes in Langley history that have been imperfectly apprehended, not only to document these episodes more completely but also to put them together, as had never been done before, in the context of an overall thesis—that of engineer-in-charge. I considered examination of outstanding research programs not only a way of giving credit where credit was due, but also a means for institutional and technological case study. This dual function could only be accomplished, however, if the outstanding programs were demythologized and understood, not for what the NACA's publicists said they were, but for what they were in fact.

My intent was not hagiographic; I did not mean to tell a story of heroic engineers and their triumphant research. Nonetheless my book has strong central characters—George Lewis, Max Munk, Henry Reid, Eastman Jacobs, Theodore Theodorsen, John Stack, Robert T. Jones, Robert R. Gilruth, Richard T. Whitcomb, John V. Becker, and Floyd L. Thompson, to name some of the more prominent. I made a real effort to bring these personalities to life. Those men who are deceased I came to know by reading their correspondence and transcripts of interviews made with them while they were alive, and by listening to what friends, colleagues, and even some rivals had to say about them. Most of those still living I was able to meet or at least talk to over the telephone.

By thinking about all of them as the kind of people one might meet and know, naturally I began to like some more than others. My preferences no doubt show up—I liked Max Munk in spite of what I learned about him—but after hearing from NACA veterans who have read the book in manuscript, I believe that the portraits are fair overall. In any case, I doubt that the reader will find any of the portraits "heroic." In fact, my depiction of Stack is somewhat iconoclastic.

Readers should also be aware that my account of Langley's past is what historians of science call internal history. I tell Langley's inside story and do it largely from Langley's own documents. The object is to illuminate the meaning of those often obscure day-to-day in-house practices, procedures, and technical demands that determine so much of the life of any research laboratory. And I also tend to emphasize the laboratory's local rather than its national or international setting. Instead of comparing and contrasting Langley's experiences with those of other research institutions, such as the Naval Research Laboratory or the Bell labs in this country or the National Physical Laboratory in Great Britain, I stay more "at home" to see how the personality of the NACA's oldest laboratory evolved within its own setting in Tidewater Virginia.[11]

This approach has obvious drawbacks. For one thing, it tends to overly parochialize the history. If my object had been to make a complete evaluation of the NACA—what the military, industry, and general public, in both the United States and Europe, thought about the agency's overall research record—then my local focus on Langley might have detracted greatly from my study. Obviously I would have had to give far greater credence, as did Roland, to what outsiders thought of, and how they influenced, Langley. But in any case I trust that the reader will not think I have treated the laboratory as a closed system. The book does analyze Langley's relations with NACA headquarters, the other NACA laboratories that were eventually created, and the NACA's clients, even if not as completely or as theoretically as some readers might prefer.

The manner of my written presentation may not suit all readers, for it is something of a hybrid. Those who do not know the fundamental principles of flight may find parts too technical for their liking; experts in aerodynamics will no doubt find many of my technical explanations simplistic or inadequate. Wanting both the layman and the aeronautical engineer to enjoy my book, I tried to steer a middle course.

Finally, in a book whose theme is the engineer in charge, there should be plenty of pictures to stimulate the mind's eye, a vital organ for creating, understanding, and retaining technology. There are around 300 photographs in this book, an uncommonly high number for what is meant to be a scholarly publication. But I believe, as Mark Twain once wrote: "Dates are hard to remember because they consist of figures; figures are monotonously unstriking in appearance, and they don't take hold, they form no pictures, and so they give the eye no chance to help." For Twain, likewise for me and the Langley engineers I know best, pictures are the thing.

1

Foundations

The parent organization of Langley laboratory was the National Advisory Committee for Aeronautics. Congress established the NACA in 1915 "to supervise and direct the scientific study of the problems of flight with a view to their practical solution." This establishment did not happen easily. It took years of active politicking by dedicated, well-connected scholars and government officers to grease the bureaucratic machinery for the creation of a new federal agency devoted to advancing the state of the art in aircraft design and operation. It also took a world war to convince a skeptical American public that aeronautics was not the province of cranks and dreamers. Finally, it even took a legislative contrivance to get the authorizing legislation through Congress.

Establishing the NACA

The idea to establish a national aeronautical organization having a central research laboratory had been discussed earnestly in April 1911 at the inaugural banquet of the American Aeronautical Society. During this meeting, several members called for the federal government to endorse the idea of creating a national aeronautics laboratory. This laboratory might be directed by the Smithsonian Institution, the members suggested. Whirling arms and other pieces of experimental equipment from Samuel Pierpont Langley's earlier aerodynamical laboratory lay dormant in Washington behind the castle building on the Mall; that lab could be expanded to include wind tunnels, shops, and instrument and model rooms. The prestige of the Smithsonian's secretary could foster the kind of cooperation among scientists requisite to the creation and proper maintenance of an effective advisory body.[1]

Forceful opponents killed the idea, however. Rear Adm. David W. Taylor, chief constructor of the navy, declared that the experimental model basin at his Washington Navy Yard and the Engineering Experiment Station at Annapolis already performed aeronautical research. A civilian laboratory, Taylor charged, would duplicate military work at needless public expense. The admiral's unworkable alternative was for the government to assign all of its aeronautical research to the navy—a proposal not as odd then as it may seem today, considering the admixture of hydrodynamic and aerodynamic theory before 1920. But others also complained. Richard C. Maclaurin, president of the Massachusetts Institute of Technology, argued that the lab should be located at or near a university or technical school, according to the successful European example. Why not at his institute, he implied. Samuel Stratton, director of the National Bureau of Standards, also dismissed the idea of any leading role for the Smithsonian. He felt that his bureau could supervise an aeronautical research facility, just as the National Physical Laboratory in England oversaw the operations of the Royal Aircraft Factory.[2]

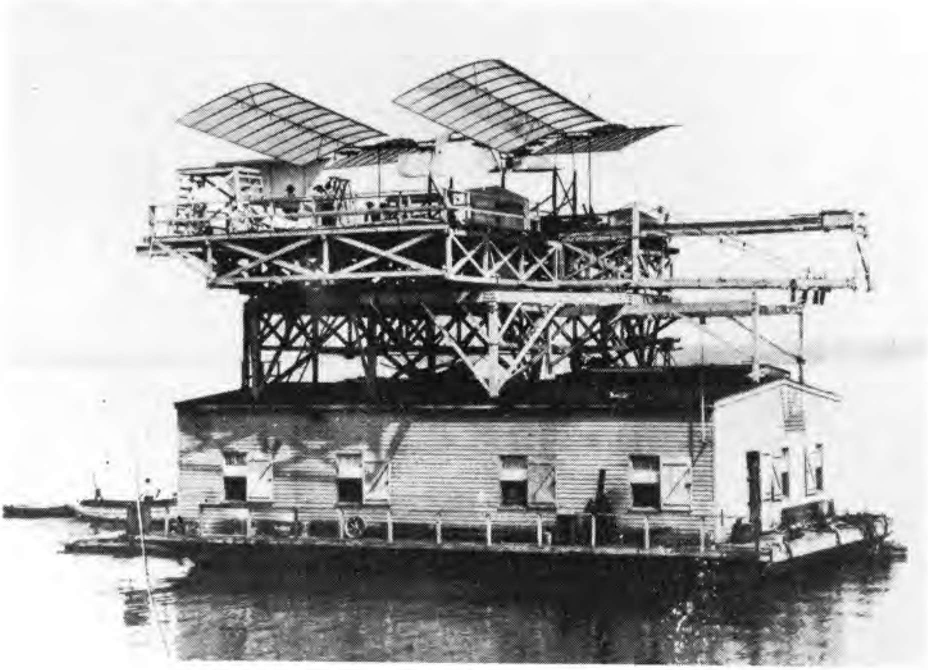
It was paradoxical that bureaucratic politics could stand in the way of creating an aeronautical research organization in America, the country where powered flight had been first achieved. The Wright brothers had made their pioneering first flight at Kitty Hawk, North Carolina, in December 1903. In the succeeding eight years, progress in American aeronautics had been wonderful, due largely to the magnificent work of the Wrights. The first truly practical passenger-carrying airplanes capable of three- to four-hour flights were developed during this short period. In 1909 the Wrights sold their Type A Military Flyer to the army, and the navy expressed interest in launching airplanes from platforms atop its ships. In 1911 Glenn H. Curtiss introduced the first practical seaplane, and the first transcontinental flight took place.

To those close observers of aviation who called for a central aeronautical research laboratory in 1911, however, the direction of aviation progress in the United States seemed "halting, haphazard, and fortuitous."[3] They argued that despite the successes, the leaders of American government were still treating aeronautics as a passing fancy rather than as a new technology which would change the world. In comparison with chemical and electrical research programs, which were helping profits to soar at General Electric, American Telephone and Telegraph, Westinghouse, Du Pont, Eastman Kodak, and other American corporations, some solid aeronautical research projects had stalled shortly after takeoff. In early 1904 the regents of the Smithsonian had closed Samuel Langley's aerodynamical laboratory in response to public criticism stimulated by news reports of the ignominious crash of the professor's full-scale "aerodrome" into the Potomac River. Ironically, this crash had occurred nine days before the Wright brothers' flight, a landmark success, which, in comparison with Langley's ridiculed failure, would be largely ignored by the American press. One reporter, either reacting ignorantly or playing to the ignorance of his reading audience, described Langley as "wandering in his dreams ... given to building castles in the air"; and a congressman from Nebraska charged, in a newspaper article entitled "Fads, Frauds, and Follies Cripple Nation's Finances," that the only thing the Smithsonian ever made fly was government money.[4] Before Wilbur Wright displayed his *Flyer* to astonished and enthusiastic European audiences in 1908, another pioneer American facility had shut down. Albert F. Zahm's wind tunnel at Catholic University in Washington, D.C., an impressive, uniquely instrumented machine for the study of airflow about dirigible hulls, was discontinued for lack of money.[5]

The situation in Europe was different. National traditions there caused scientific activities to be quickly institutionalized; thus governments convinced of the revolutionary importance of aircraft were able to build major aeronautical research programs before the start of World War I. Even as Wright toured Europe, Frenchmen studied resistance of various surfaces in free air at the new Central Establishment for Military Aeronautics at Chalais-Meudon, near Paris, and were about to begin experimenting in Gustave Eiffel's wind tunnels at Champs de Mars and Auteuil. The University of Paris authorized an "aerotechnical" institute in 1912 at St. Cyr. Across the Channel in 1909 the British prime minister appointed an Advisory Committee for Aeronautics with physicist Lord Rayleigh as president. This committee supervised the aeronautical work of the National Physical Laboratory (NPL) and the expensive new Royal Aircraft Factory at Farnborough. The Germans, in their tradition of highly organized applied scientific research, built their major facility at the University of Göttingen.

American aeronautics progressed more slowly than European because it had not yet managed to win the political support necessary for its national organization. Outgoing President William Howard Taft had appointed a commission in 1912 to investigate the sorry situation of American aeronautics, but the lame duck body accomplished nothing.[6] The situation seemed to improve when, a month after the inauguration of Woodrow Wilson in 1913, the Smithsonian Board of Regents authorized the reopening of Langley's laboratory. Charles Doolittle Walcott, secretary of the Smithsonian, even presided over a meeting of "The Advisory Committee of the Langley Aerodynamical Laboratory" on 23 May 1913. Several distinguished men belonged to the committee, including Orville Wright, Albert F. Zahm, Samuel W. Stratton, Glenn H. Curtiss, Capt. W. I. Chambers, USN, and Brig. Gen. George P. Scriven, USA, plus representatives from various departments of government. Sixteen subcommittees were formed on paper. But a

Left, Samuel Pierpont Langley (1834–1906) had more reason to show confidence in his chief mechanic and pilot, bespectacled Charles M. Manly, than Manly did in the Smithsonian's distinguished scientist of flight. The Langley Aerodrome *crumpled into the Potomac River shortly after being catapulted from its position on top of a houseboat, not because of any problem with Manly's power plant design, but because Professor Langley's framework for the tandem wings was too weak. (National Air and Space Museum)*



congressional act of 1910, preventing executive agencies "from requesting the heads of departments to permit members of their respective departments to meet at the Institution and serve on an advisory committee," forced the board to disband the committee and reshut the door to the Langley laboratory. (The elite composition of the committee and the distribution of work among numerous subcommittees nevertheless presaged the first meeting of the NACA in 1915, as well as its subsequent approach to organizing its work.)[7]

Constant pressure from Walcott and like-minded men, the Progressive impulse for economy and efficiency in government, and, above all, the war in Europe, led finally to the creation of an advisory committee for aeronautics. On 3 March 1915, on its last working day, the 63d Congress passed a Smithsonian proposal to create such a body.[8] Though the authorizing legislation slipped through Congress largely unnoticed, success still hinged on compromise. First of all, the Smithsonian proposed only to form a committee to coordinate basic aeronautical research already being done at existing facilities. By not creating a national laboratory, the legislation eased President Wilson's fear that such a facility would endanger American neutrality. The legislation did provide, however, that "in the event of a laboratory or laboratories, either in whole or in part, being placed under the direction of the committee, the committee may direct and conduct research." Second, the Smithsonian would not dominate the program. Rather, the act established a broadly representative unpaid panel, modeled after the British Advisory Committee for Aeronautics, consisting of "two members from the War Department, from the office in charge of military aeronautics; two members from the Navy Department, from the office in charge of naval aeronautics; a representative each of the Smithsonian . . . , of the . . . Weather Bureau, and of the . . . Bureau of Standards; together with not more than five additional persons who shall be acquainted with the needs of aeronautical science, either civil or military, or skilled in aeronautical engineering or its applied sciences." Finally, success rested on legislative contrivance. A friendly House Committee on Naval Affairs attached the NACA's charter as a rider to a naval appropriation bill, greasing the machinery for quick approval. Congress appropriated $5000 to the NACA for fiscal year 1915.[9]

Research by Committee

The National Advisory Committee for Aeronautics was to be a rather simple government agency, as agencies went, with a unique composition and hierarchy. A *Main Committee*, composed of seven government and five private members, would meet in Washington, D.C., semiannually—and occasionally more often—to identify key research problems to be tackled by the agency and to facilitate the exchange of information within the American aeronautical community. This body would be independent, not under any department, but reporting directly to the President, who appointed its members. These members would receive no salaries. A smaller *Executive Committee* of seven members, elected by ballot from the Main Committee for a term of one year, was to act as the real governing body of the NACA.* It would control "the administration of the affairs of the committee," exercise "general supervision of all arrangements for research, and other matters undertaken or promoted by the Advisory Committee," and collect aeronautical intelligence. It also appointed *technical committees*—in effect subcommittees, since the NACA itself was a committee—to provide expertise to the parent committee in one of the larger fields of aeronautical inquiry, such as aerodynamics, power plants for aircraft, or aircraft construction. These technical committees, in turn, created subcommittees of their own to give specialized advice. The Executive Committee also authorized the formation of *special committees*, usually ad hoc, to deal with problems even more specific—for example, the Special Committee on the Design of the Navy Rigid Airship ZR-1 (created in 1923). In later years, the problems giving rise to special committees were often more political or institutional in nature, as in the cases of the Special Committee on the Relation of the National Advisory Committee for Aeronautics to National Defense in Time of War, and the Special Committee on Future Research Facilities (both created in 1938). The composition of the NACA and its Executive Committee changed over the years; however, it should be obvious from the above description that any use of the term *committee* when referring to the NACA must be careful and precise.†

In theory, the committee system belonged to and represented a powerful intelligence network. Each member was chosen because he was thought to possess a special knowledge. Though early NACA policy made it clear that appointees from private life served as individuals and not as representatives

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* Until 1933, members of the Executive Committee "were chosen annually by vote of the Main Committee. The usual practice was to elect all members of the Main Committee who resided in the Washington area and who could devote a reasonable amount of time to Committee work. After 1933, all members of the Main Committee automatically belonged to the Executive Committee, but that did not greatly alter the situation. The Washington members—usually the government members—still dominated the Executive Committee." Alex Roland, *Model Research: The National Advisory Committee for Aeronautics, 1915–1958*, NASA SP-4103 (Washington, 1985), p. 424.

† The author will use the capitalized term *Committee* to refer to the NACA. The Executive Committee and the specific subcommittees will be so identified.

The Main Committee of the NACA met in Washington, D.C., twice a year, the annual meeting being held in October and the semiannual meeting in April. Among the matters discussed at this semiannual meeting on 18 April 1929 was the forthcoming construction of a full-scale wind tunnel and a seaplane channel at Langley. Left to right: John F. Victory, secretary; Dr. William F. Durand; Dr. Orville Wright; Dr. George K. Burgess; Brig. Gen. William E. Gillmore; Maj. Gen. James E. Fechet; Dr. Joseph S. Ames, chairman; Rear Adm. David W. Taylor, USN (Ret.), vice chairman; Capt. Emory S. Land; Rear Adm. William A. Moffett; Dr. Samuel W. Stratton; Dr. George W. Lewis, director of aeronautical research; and Dr. Charles F. Marvin. (One member, Dr. Charles G. Abbot, was absent.)

of the institutions from which they came, it was hoped that each member—private or governmental—would come to NACA meetings with a briefcase of extraordinary personal and professional experiences, connections, sources of information, and reference points. Military officers would arrive with ideas provoked by recent intelligence reports, university professors with word of yet-to-be-published papers announcing new theories or experimental findings. By pooling their information, which was already high-level, and as a group assessing its practical effects for American aeronautics, NACA members would produce a new body of knowledge even better than the sum of its parts. They could then translate this knowledge into effective technical advice and wise government research policy. In this synthesis, many would say, lay the genius of the committee system.

In practice certain committee members fell short of the ideal roles described. Over the years some members were in effect only honorary, some did not understand research, and some just did not put forth a good effort. On the whole, however, the committee system worked. (Decades later, the system was abandoned by NASA as anachronistic, but recently there have been movements to revitalize it.)

During the Great War, the young NACA fulfilled its advisory function, but reached slightly beyond it. The Committee sponsored tunnel tests at the Navy Yard model basin, propeller tests at Stanford University, and cooperated in engine testing and instrument development by the Bureau of Standards. It evaluated aeronautics-related inventions for the War Department and elaborated a plan by which an Aircraft Production Board became a branch of the Council of National Defense. It helped the young aircraft industry in particular, coordinating meetings between manufacturers and the armed services, and bringing order to the procedure by which the military procured aircraft. The Subcommittee on Motive Power worked to stimulate production of a high-performance airplane engine. The NACA's greatest wartime success, however, may have been its mediation of the bitter and complicated patent dispute between the Wright-Martin Company and Glenn Curtiss over the wing-warping technique for lateral control.[10] The cross-licensing agreement that resulted from the Committee's intercession facilitated immediate construction of more and better American combat planes. Critics complained, though, that it also reduced competition in the aircraft industry. At the least, the consolidation of patent rights sacrificed the interests of the small inventor to those of the big corporation.[11]

Until the NACA possessed its own technical staff, wind tunnels, and other experimental facilities, however, its contributions would be limited and its future dubious. One historian has charged that the wartime Committee spent most of its time and energy trying to carve out a permanent niche in American aeronautics and, in fact, paid little attention to calls for immediate service.[12] Yet the NACA never hid its priority. Executive Chairman Walcott conceded in the *Third Annual Report of the National Advisory Committee for Aeronautics, 1917* that the preceding three years of activity had been "preparatory for the more effective service which the Committee hopes to render through its laboratory facilities ... and through the enlarged technical and scientific staff contemplated in connection therewith."[13] Until then, employees could have only the haziest idea of what was expected of them. Leigh M. Griffith, a War

Department engineer detailed to the Committee in 1917 (who would become the first engineer-in-charge of the NACA's Langley Memorial Aeronautical Laboratory) lamented that "until it is known what we are trying to do, it is impossible to formulate any system or build any organization for the doing of that thing."[14] To fulfill the vision of its early proponents and founders—indeed, to complete that foundation—the NACA had to have its own laboratory.

The NACA had to defend its idea for a central laboratory against the old charge that its research activities would duplicate work at existing facilities. This bone of contention carried little meat, for the rival research institution usually in mind—the Washington Navy Yard model basin—was small, largely devoted to development, and "backward in its use of advances in science and engineering."[15] Charles Walcott advised the House Naval Affairs Committee in 1916 that safeguards against duplication were in place. If a problem before the NACA required investigation, he told the congressmen, informed Committee members like the army's chief signal officer (Brig. Gen. George P. Scriven, the first chairman of the NACA and ex officio head of army aviation) and the commander of the Navy Air Service (Capt. Mark L. Bristol) would ascertain whether that investigation should be carried out by the navy, the War Department, the Bureau of Standards, or the NACA laboratory.[16]

In defense of its campaign for a laboratory, NACA leaders also pointed out that no one in the government had assumed responsibility for *civilian* aviation research. Present needs bore on military preparedness, wrote General Scriven in the *Annual Report for 1915*, but "when the war is over there will be found available classes of aircraft and a trained personnel for their operation, which will rapidly force aeronautics into commercial fields, involving developments of which today we barely dream."[17] The Committee needed to be ready with its laboratory to meet this coming civilian challenge.

Building the Laboratory

Lacking money to purchase and develop a site for its laboratory, the Committee circulated the idea of a joint civil-military experimental station. Interservice rivalry, however, defeated the original proposal to combine the aeronautical research of the NACA, the Weather Bureau, and the aviation sections of the armed forces. The idea was unwise at any rate if the Committee intended to maintain autonomy in the future. General Scriven advised his colleagues on the Committee to support a request for $50,000 from Congress, to be included as part of the fiscal 1916 navy budget, to build the lab. Secretary of the Navy Josephus Daniels objected strongly.

A party from the U.S. Army Corps of Engineers surveys the future site of Langley Field in the fall of 1916. (National Air and Space Museum)

Planning an enormous new naval research facility at that moment, Daniels may have been worried about increasing an already fat budget request.[18] Regardless of motive, navy leaders never endorsed a joint laboratory and proving ground, although they did continue to participate in meetings about a mutual site, and did not obstruct the NACA's budget request.[19] On 29 August 1916 Congress appropriated the full $87,000 asked for by the Committee, of which $53,580 was earmarked for laboratory construction.

The Committee's best chance to obtain land for the laboratory was to cooperate with an Army Air Service project. Congress had directed the War Department in 1915 to identify a military reservation to house an experimental facility with airfield. If that quest failed, the secretary of war could authorize up to $300,000 for the purchase of a new site. General Scriven, as head of army aviation, appointed a board of officers, including four members of the aviation section of the Signal Corps, to investigate locations for the proving ground, "agreeing to give the Advisory Committee the benefit of its inquiries and conclusions" and to make any land chosen available to it. The board considered such factors as climate, proximity to industry, accessibility, health of environment, availability of local mechanics and other technicians, character of land for experimental flying, and general location as affecting attack by enemy from land or water.[20]

After considering 15 tracts of land (six in Maryland, four in Virginia, and one each in West Virginia, Tennessee, Ohio, Illinois, and Missouri), board president Lt. Col. George O. Squier informed the NACA of the army's choice—1650 acres in Elizabeth City County, Virginia, just north of the town of Hampton.[21] Following an inquiry to the surgeon general concerning health conditions in the Hampton area and an inspection of the site by one of its subcommittees, the NACA recommended that "this site be obtained for the use of the Government at as early a date as practicable."[22] Stanford University professor William F. Durand, Scriven's successor in 1916 as chairman of the NACA, summarized the rationale behind the Committee's endorsement of the site for "Langley Field"* in the *Annual Report for 1916.* Hampton, close to the Chesapeake Bay but reasonably immune from attack, stood in relative proximity to Washington, D.C. (an overnight steamer ride) and to the shipbuilding and repair industries at Newport News, Norfolk, and Portsmouth. Temperate but changeable climate, plus location alongside a tidal river, permitted experimental flying above both land and water and under nearly all conditions that aircraft would meet in service. The site, the NACA chairman believed, left the door open to a plan for a combined facility, sponsored by the War and Navy departments.[23]

Climate and topography seemed to bless the site, but shrewd Hampton businessmen sold it. Political boss Harry H. Holt, clerk of the court of Elizabeth City County; Hunter R. Booker, president of the Hampton-Phoebus Merchants' Association; Col. Nelson S. Groome, executive officer of the Hampton Bank; and Capt. Frank W. Darling, vice-president of two local banks and head of J. S. Darling and Son, the third largest oyster packer in the United States, saw a chance to revive a dying economy while making a small fortune for themselves. This local elite brought Hampton to the government's attention.

Elizabeth City County had a population of around 5000 during World War I. Until a referendum in December 1914, a significant number of its citizens had earned their livelihood from the liquor industry. Then the

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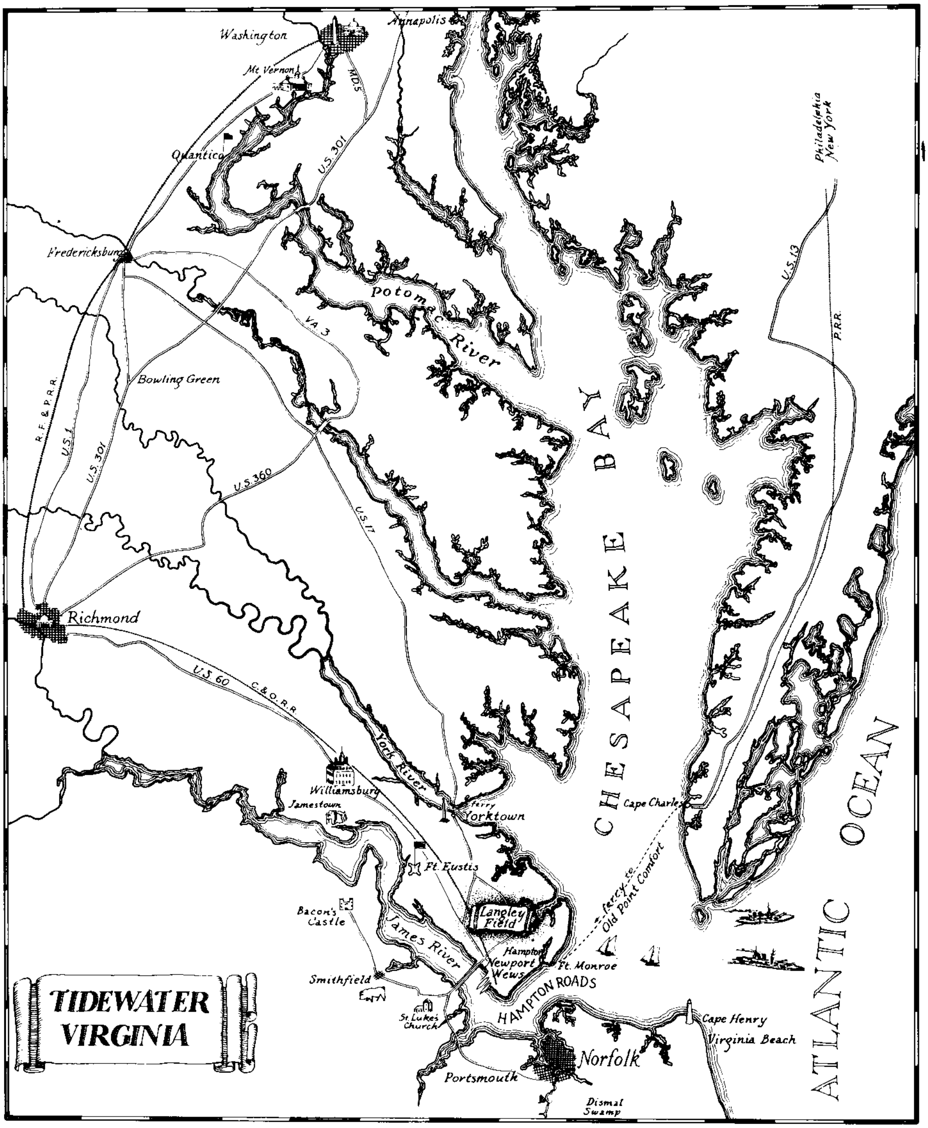
* The idea to christen the new installation "Langley Field," in honor of Prof. Samuel P. Langley of the Smithsonian, appears to have originated either with General Scriven or Lieutenant Colonel Squier. On 13 October 1916 Scriven proposed Langley's name to Walcott, who answered the same day: "The suggestion is a fine one and we can bear it in mind when the field is obtained." In a speech to the annual meeting of the Aero Club of America, held in New York City on 12 January 1917, Squier declared, "If I have any influence in the matter, we are going to call that proving ground on the Atlantic 'Langley Field,' and I cannot conceive of any better monument to the memory of Professor Langley." The NACA's resolution to call its field installation "Langley Memorial Aeronautical Laboratory" was approved at the semiannual meeting of the Committee on 22 April 1920.



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A Langley map of the Tidewater Virginia area from the late 1930s. The bridge at Newport News over the five-mile-wide James River was built in the late 1920s. During World War I, U.S. highways 17 and 60 were primitive dirt roads.

The river harbor at Hampton, Virginia, 1899. (Courtesy of Vintage Virginia Photographs, Inc., Norfolk, Va.)

Commonwealth of Virginia went dry. Harry Holt recalled the parching effects of Prohibition:

> The cutting off of this source of support seemed certain to doom our community. In the city of Hampton alone, hundreds of families emigrated . . . , scores were made jobless, houses were empty and business generally suffered.[24]

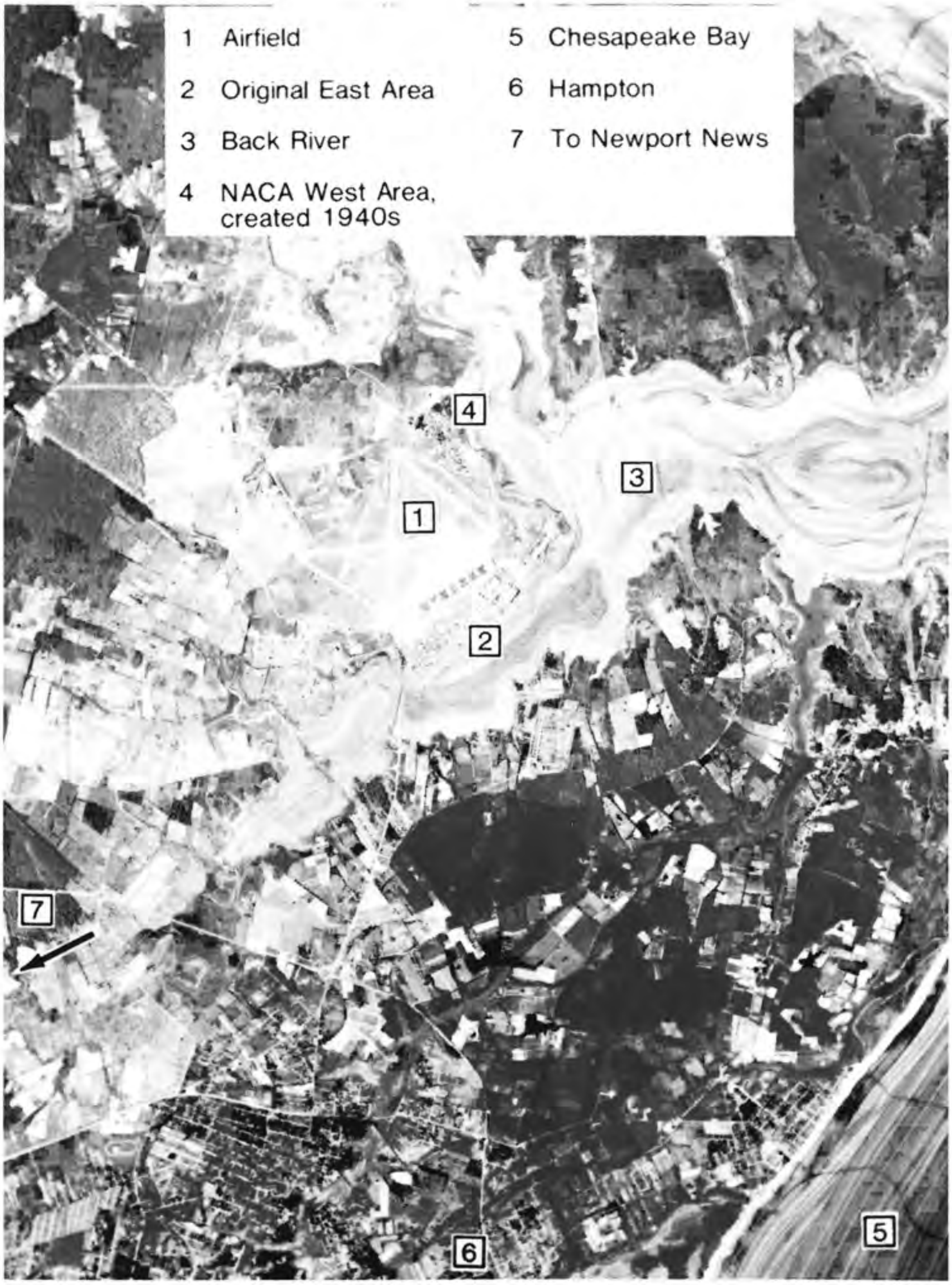
The most severe blow fell upon holders of real estate. Holt approached banker Groome, his closest associate, with news of the government's interest in buying land for an airfield, and assured him that there were "ideal sites in the plantations of the Sherwood, Lambington, Pool, Morefield, Blumfield and Shellbank properties." Quietly, so as not to attract attention to the speculation, the two proceeded to secure cheap 90-day options on large parts of these properties.[25]

Between Halloween and Thanksgiving 1916, a Hampton committee, spearheaded by Holt and Groome, met once at home and once in Washington with Squier's army site selection board. "We had just the right place to offer," Holt recalled in 1935, "and after repeated visits here, and agreements by us to build a railroad line onto the property, a price of $290,000 was finally agreed upon." The entrepreneurs were forced to sink $17,000 into some unexpected purchases of right-of-way—$3000 of which was defrayed by a stock subscription by the Newport News, Hampton, and Old Point Railway—but they had walked off with all but $10,000 of the $300,000 authorized by Congress and the War Department for the land purchase. (In

Oyster baron Frank W. Darling owned one of the plantations, "Sherwood," sold to the federal government in 1916 for construction of Langley Field. In this photo from about 1910, Captain Darling prepares his wife and son for a carriage ride from their magnificent waterfront estate "Little England" along Hampton Roads. (Courtesy of Hampton Center for Arts and Humanities, Hampton, Va.)

fact, the local men received $5645.31 more from the government in 1917 for an additional sale of land on Plum Tree Island in York County north of Langley Field. The army eventually used this marshy property for experimental bomb-dropping, demolition training, and target practice. During and after World War II, the NACA used it for drop-body tests.) A deed, executed 30 December 1916, transferred the land from "H. R. Booker"—the name of one of the Hampton businessmen involved—to the government.

The land gamble paid off handsomely for Holt and confreres, but it also benefited the entire northern shore of Hampton Roads, across from Norfolk and Portsmouth. A small group of men had made about $175 an acre on typical Tidewater fringe land—low-lying land next to shallow water. (By April 1918, in fact, when the Atlantic, Gulf, and Pacific Company concluded dredging a channel in Back River to allow larger boats to dock, it had deposited 1,791,320 cubic meters of fill onto Langley Field at a cost of half a million dollars.) The entire community cheered the venturesome heroes and the expected business boom, and many privately laughed at the government for having bought such a questionable bill of goods. The Newport News *Daily Press* announced a "fine Christmas for the entire Lower Peninsula . . . the future of this favored section of Virginia is made." Public works—road, bridge, and electric railway construction—reverberated around Langley Field for many years to come. Prior to these projects, it



A high-altitude view of Langley Field in the 1950s.

had been "almost impossible to get ... to Newport News, or for that matter, to get anywhere" from Hampton.[26] Many residents were not exactly sure what was going on at Langley Field (even today, many do not differentiate between air force and NASA activities there), but all recognized the life-giving energy of the thousands of federal dollars poured into their midst.

Construction Bottleneck

The chaos of war finally forced the army to abandon its plan to make Langley Field its aeronautical research and development center. Capt. John T. Sloan, building inspector for the War Department, arrived in Hampton on 8 February 1917—the day the Kaiser announced unrestricted submarine warfare within specified blockade zones and President Wilson broke diplomatic relations with Germany—to supervise field construction.[27] American entry into the war a month later upset all schedules. Sloan and Capt. John O. Steger, the original constructing quartermaster at Langley Field, went to France. The J. G. White Engineering Corporation of New York City, the major construction contractor, could not find enough laborers or obtain materials when needed. Too many bosses and too much division of responsibility exacerbated the confusion. To the War Department it seemed that the contractors put up any and every type of structure, without consultation with or authority from the proper government officials, the usual explanation for such structures being that they were only "temporary." Contractors, on the other hand, complained of work-order cancellations, red tape, and improper use of their equipment and supplies by soldiers.[28]

As soon as the United States had declared war, Britain and France hit their new ally with an immediate request for an air armada of 20,000 airplanes and 30,000 engines. In May 1917, the shocked War Department chose the British DeHavilland 4 as the multipurpose battle plane it would build under license; however, a commission headed by Col. Raynaul C. Bolling and Capt. Virginius E. Clark, sent to Paris by the secretary of war to consult with the Allies' aviation experts, determined that not only was the DH-4's standard engine underpowered for the airplane to perform as well as the U.S. Army wanted it to do, but that so too were all other existing American and European engines. The Bolling-Clark group identified the need for development of a new engine for the DH in June 1917, precisely the time that an initial layout plan for Langley Field was being finished by Albert Kahn's architectural firm of Detroit. Within the month, an engineering design of the new "Liberty" engine was ready. By 17 July 1917, the day the NACA broke ground for its first laboratory building at Langley

AVIATION EDITION

THE HAMPTON MONITOR

Featuring the construction of Langley Field and the development of the Newcomb Lifeboat Company

HAMPTON, VIRGINIA, JULY, 1918

A resume of commercial enterprise and military activity of the Peninsula of Virginia brought into its own because of strategic importance during the great European war.

Printed and Published by the HOUSTON PRINTING AND PUBLISHING HOUSE, Hampton, Virginia

In July 1918, the Hampton Monitor *published a special aviation edition featuring the construction of Langley Field.*

Field (and exactly one week before a 640-million-dollar aviation bill became law, the largest appropriation bill for aviation in American history up to that time), Henry Ford and the Packard Motor Company had a prototype Liberty engine running.[29]

Had its dynamometer been ready, Langley could have tested the new Liberty engine. But, in fact, not one permanent building was completed on the post until the end of the summer. Construction of the flying field was an ordeal. One of the first soldiers to arrive there recorded that it was

> Nature's greatest ambition to produce [for Langley Field], her cesspool, the muddiest mud, the weediest weeds, the dustiest dust and the most ferocious mosquitoes the world has ever known. Her plans were so well formulated and adhered to that she far surpassed her wildest hopes and dreams.[30]

One person who experienced the ordeal of constructing Langley Field was Thomas Wolfe. In his autobiographical novel *Look Homeward Angel* (1929), he described how young Eugene Gant (Wolfe in fictional clothing) spent the summer of 1918. From Norfolk

> he went by boat once more to Newport News, and by trolley up the coast to Hampton. He had heard, in the thronging rumor of Norfolk, that there was work upon the Flying Field, and that the worker was fed and housed upon the field, at company expense.
>
> In the little employment shack at the end of the long bridge that led across into the field, he was signed on as a laborer and searched by the sentry, who made him open his valise. Then he labored across the bridge ... staggered at length into the rude company office and sought out the superintendent ....

He was given a job as a personnel checker, a horse to ride, $80 a month, and room and board.

> Three times a day he rode around the field to check the numbers of two dozen gangs who were engaged in the work of grading, levelling, blasting from the spongy earth the ragged stumps of trees and filling interminably, ceaselessly, like the weary and fruitless labor of a nightmare, the marshy earth-craters, which drank their shovelled toil without end. The gangs were of all races and conditions: ... part of the huge compost of America.[31]

Forty-six Langley workers died of influenza between September 1918 and January 1919. So severe was the epidemic that the undertaker who had the contract for burying the government dead was unable to secure enough coffins to take immediate care of the bodies.[32]

The wartime Aircraft Production Board, needing an instant change in the miserable outlook for American aircraft manufacture, as well as a place to test the new Liberty engine, observed the infernal delays in construction near Hampton with great anxiety. Its representatives at the site reported sadly that it would be "a considerable time before the permanent construction at Langley Field [would] be in effective operation."[33] The army had to respond. Pointing a finger at Langley as "the bottleneck of the aircraft program," the army dropped the plan to share an installation with the NACA and reassigned aircraft research and development to the engineering division at McCook Field, near Dayton, Ohio, operational since early 1917. Captain Clark returned from Paris to Washington in August 1917; in October he assumed command of the new aeronautical experimental station at McCook.

It took Albert Kahn's designers through the end of 1917 just to complete the first twenty buildings at Langley Field. But by then Liberty-powered DH-4s were flying regularly above Dayton; a few months later they were in transit to European airfields for action alongside the already battle-hardened fleets of Spads, Sopwith Camels, and Nieuports. Then the army changed its thinking about Langley's mission—the field was now to be used for training pilots, aerial photographers, and observers. After the Armistice in November 1918 (ironically, with the Liberty-powered DH-4 playing no major role in winning the war) construction at the field virtually ceased. The NACA was left in the lurch, "a disappointed tenant having little in common with its landlord."[34] The laboratory would have to make its own way.

But the military did not want an independent NACA presence at Langley. In December 1916, the Committee had asked the army for an official designation of property on which it could build its own laboratory buildings, but the army failed to respond. Air Service commanders wanted to maintain control not only over Langley Field but over all experimentation at the field, including that conducted by the NACA. (Col. Thurman H. Bane, chief of the Air Service Technical Command, opposed the idea of dual military-civilian control so much that he recommended to the army's director of military aeronautics, in January 1919, that all NACA personnel at Langley Field "be subject" to his orders.) The Committee repeated its request three months later, but the army answered that formal assignment of land would be postponed "until [the] work of preparing Langley Field [was] in a more advanced state." The army used the same delaying tactic to ward off similar appeals made by the NACA in August 1917 and December 1918.[35]

The one thing that impressed nearly every visitor to the original LMAL administration building was the mud that surrounded it.

After the war the military could continue to put off the NACA only so long. On 22 April 1919 Acting Secretary of War Benedict Crowell approved an Air Service recommendation that "the portion of Langley Field known as Plot 16 be definitely set aside for use by the NACA for their purposes in constructing laboratories or other utilities necessary in scientific research and experiments in the problems of flight."[36] Although welcomed by the NACA, the army's offer was hardly generous. The NACA had already acquired Plot 16 unofficially in 1917. By the end of 1918, the Committee's first building on this land had been erected, and work there on its first wind tunnel had been started.[37] And there was another problem with this tardy token of army generosity: it was too small to accommodate the building of any living quarters for NACA employees. Suitable housing close to work was for the NACA a major problem that only the military could help to remedy. In July 1919, after repeated requests from NACA chairman Walcott, the army did agree reluctantly to make primitive housing, along with heat, light, and telephone services, available to a certain number of civilian Langley employees. This arrangement, though unpleasant, was better than nothing, but it lasted only a short time. In the fall of 1919, based on a ruling by its judge advocate general, the army informed the NACA that it could no longer furnish Langley's civilian employees with housing or utilities.[38]

Isolation, mosquito bites, flu, inadequate housing, and poor relations with the military—where but Langley Field could things be so bad? This question began to plague one NACA employee after another early in 1919 until feelings against the place festered into a mutiny. The NACA engineer in charge of building and construction, John DeKlyn, complained to Executive Committee Chairman Joseph Ames on 9 July 1919 that "Langley Field can never be an efficient or satisfactory place for the Committee to carry on research work." John Victory, NACA executive secretary in Washington, concurred and recommended that the lab be moved to Bolling Field, a base under construction in the District of Columbia. The Committee, in its *Annual Report* for 1919, formally requested congressional approval of the relocation from Hampton.[39]

Dedication

The reluctance of Congress to change the lab's location and its cutting of the Committee's postwar budget requests forced the NACA to make the best of a bad situation. All too aware that army research at McCook Field was already showing signs of production (including the development of the Sanford Moss turbosupercharger, a siphon gasoline pump, several different leakproof tanks, and fins and floats for emergency water landings), the NACA pushed its workers in 1919 and 1920 to finish an atmospheric wind tunnel, dynamometer lab, administration building, and small warehouse.[40] It hired an executive officer, and preliminary research began—a flight investigation of the lift and drag characteristics of the Curtiss JN4H Jenny airplane.[41] The full-time Langley complement grew to eleven persons: four professionals and seven nonprofessionals. Meanwhile, an inquiry by the Committee revealed that Bolling Field had serious shortcomings of its own.[42]

Formal dedication of the Langley Memorial Aeronautical Laboratory on 11 June 1920 guaranteed that the NACA would remain at Hampton. Ceremonies included an aerial exhibition highlighted by a 25-plane formation led by Brig. Gen. William "Billy" Mitchell, addresses by prominent military and civilian officials congratulating the NACA and giving it best wishes, and a tour and demonstration of the wind tunnel—all of which improved morale. A speech by Rear Adm. David Taylor, a former opponent of the laboratory, greatly bolstered NACA confidence. "One of the party, on approaching the wind tunnel building with me," Taylor asserted, "expressed the thought that the Committee had probably been a little lavish in its expenditures .... I do not agree ... as the building is only a fitting housing for ... the shrine to which all visiting aeronautical engineers and scientists will be drawn."[43] An exaggeration in 1920 of the research significance of the NACA's original tunnel—an almost obsolescent design (see

Langley's first wind tunnel, completed in 1920, was essentially a replica of a ten-year-old tunnel at the British National Physical Laboratory.

chapter 3)—Taylor's overstated prophecy was exactly what the pediatrician ordered for ailing, infant Langley laboratory.

Relations between the NACA and the Air Service seemed to improve immediately. Ten days after the dedication, Dr. Ames sent a warm letter to Col. William N. Hensley, the commanding officer of Langley Field, thanking him for courtesies extended the Committee at the ceremony. "The efficiency of our work at Langley Field," wrote Ames, "depends in the end to a great extent upon the degree to which you give us your support, and I feel that if your cooperation on June 11 was an indication of your attitude toward us, we can rest assured as to the future."[44]

In reality, however, things had not been settled. Colonel Hensley had in fact not even attended the dedication. A few days later, the LMAL senior staff engineer informed NACA headquarters that Hensley had prevented all but one of his officers from attending the ceremonies by issuing "specific orders to remain at their posts until after 5 p.m.," and had called a meeting on 15 June to discuss the "possibility of ousting the NACA from the field," promising "to do everything in his power to bring this about."[45]

World War I was over, but there were still many tough battles left for the NACA to fight.

2

Langley Personality, Formative Years

In some ways, an institution seems to be organic. Its parts live and communicate, develop attributes of survival and adaptation, mature, age, weaken. In a way that cannot be demonstrated objectively, an institution develops a personality. As with a person, heredity and early environment are the critical influences. Because institutions manifest persistent stylistic or expressive traits, generations of Americans can easily and consistently discriminate between the distinct personalities of such similar organizations as the army and marines, IBM and Apple, the New York Yankees and New York Mets, or the University of California–Berkeley and Texas A&M.

Most people involved with American aeronautics between 1917 and 1958 saw a distinct personality in Langley laboratory. Langley's most striking physical feature was its unique collection of wind tunnels, many of which were of unprecedented design and capability. To a few observers, Langley's tunnels might have looked like huge, ungainly, wormlike creatures, washed ashore perhaps after a battle of primordial monsters in the nearby tidal river. But the tunnels were no less fascinating to those whose gaze was less imaginative. Some tunnels might have looked only like big warehouses with jointed appendages and rounded corners, but they were all in fact complicated mechanized marvels, national resources, great and powerful monuments to the modern age.

The impression that stuck in the minds of people who knew Langley best, though, was not only that of the wind tunnels, as impressive as they were, but also that of the human beings who built and operated them. In the 1920s and 1930s, Langley researchers earned an international reputation for finding practical solutions to urgent aeronautical problems. They did it largely through a careful management of technical and bureaucratic details—management which, among other things, turned individual talents into team capabilities and balanced the requirements of laboratory self-sufficiency with those of responsiveness to clients' needs. It was the overall research environment that shaped the perception of most visitors to Langley. Where else but in this tremendously interesting place, most visitors thought, could one find dozens of government employees working so energetically, demonstrating their equipment with such extraordinary pride, and discussing experimental results with outsiders so openly?

The evolution of the Langley personality over the course of the two decades between the world wars is the subject of this chapter.

Management

A committee is better suited to giving advice than exercising control. As the NACA changed from a solely advisory and coordinating body, the need for an executive office staffed by full-time civil servants became clear. Chairmen Charles Walcott and Joseph Ames recognized from the start that someone had to be on the job for day-to-day business in Washington, and that someone had to take charge of Langley Memorial Aeronautical Laboratory in Hampton.[1]

The two individuals who first took firm control of routine NACA affairs were John F. Victory and George W. Lewis. More than anyone else at NACA headquarters, these men left their lasting, if contrasting, impressions on Langley. Needing an office clerk, the Committee hired Victory as its first employee in June 1915, only three months after congressional approval of the enabling act. In fact, Victory had been doing some NACA paperwork earlier as secretary to Committee member Holden C. Richardson, officer in charge of the experimental basin at the Washington Navy Yard. Born in New York City in 1892 and orphaned early, Victory had worked continuously and indefatigably from boyhood. He began his federal service career as a messenger in the Washington patent office at age 16, studying shorthand and typing at a night school (which he later bought and operated). At 18, he recorded proceedings of courts-martial and courts of inquiry for the navy. To help support his younger sisters, he earned extra money on his annual leave days recording congressional hearings. While working for Richardson, he became familiar with some of the basic principles of aeronautical research and cultivated a finesse in public relations. He took a keen delight in showing lady visitors around the Yard, taking them into its wind tunnel, shutting the door, and turning on the breeze.[2] Victory's first task after going on the NACA payroll (at $1200 a year on 22 June 1915) was handling requisitions from NACA contractors, depositing them with the bureau of supplies and accounts. The secretary's lean and tenacious constitution mirrored that of the upstart organization he was joining.

John F. Victory (1892–1974) was the NACA's first employee and the only executive secretary it ever had.

George W. Lewis, on the other hand, was a portly, relaxed, college-educated engineer from a secure family living in Ithaca, New York. He became executive officer of the NACA in November 1919—he was 37—and director of aeronautical research in July 1924. In both capacities, Lewis was subject to general research policies and budgets set by the Committee, which sometimes also issued instructions on specific projects. His responsibility was to implement these policies and report results directly to the Committee. He supervised the preparation of technical papers for publication and their distribution to users in the military services, industries, universities, and various government departments. His hard work, great discretion, and shrewd combination of modesty and forcefulness enabled him to win the confidence and blanket support of most members of the NACA. Over the years, Lewis maintained a happy and successful relationship with them even while criticizing some of their policies and implementing many of his own plans independently. A 1910 master's graduate in mechanical engineering from Cornell University, he possessed considerably more technical competence than John Victory. He had taught engineering at Swarthmore College for seven years (1910–1917), done research on superchargers at the Clarke-Thomson Research Foundation in Philadelphia during the war, and first served the Committee in 1918 as a member of the Subcommittee on Power Plants.[3]

George Lewis's ability to befriend and influence politicians and to circumvent bureaucrats was another of his assets. Originally, the Committee had meant to install him in an office at Langley Field, but Lewis thought it more effective to manage "his boys" from Washington, where he could

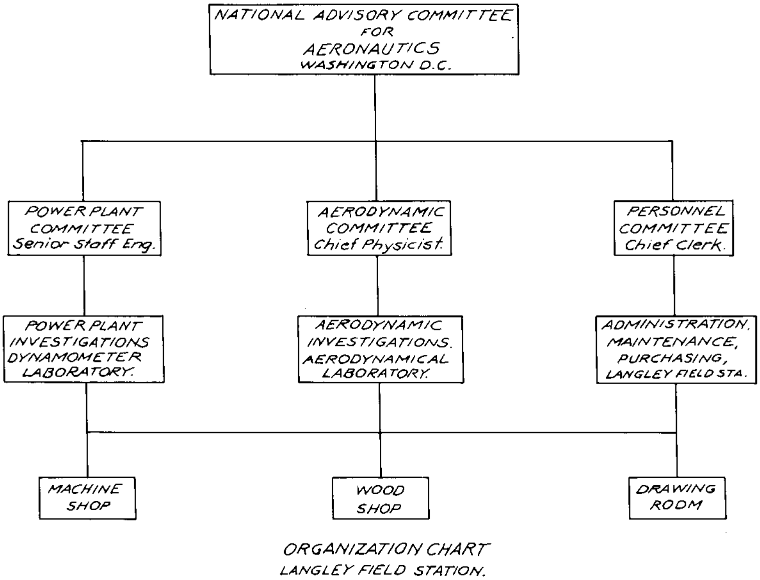
> organization charts rather than from an intimate knowledge of the value of each organization. The difficulty lies in the fact that it is almost impossible to put everything into an organization chart.[6]

Another part of Lewis's distaste for organization charts was managerial. He wanted to ensure research leeway by encouraging his staff to cooperate irrespective of nominal boundaries. When he visited the lab, he often held informal meetings where junior engineers could meet with section heads, division chiefs, and even the engineer-in-charge, to exchange ideas without fear of overstepping formal rank.

This democratic practice had a liberating effect on the staff. Even in the lunchroom, the newest members of the staff could share their ideas with veterans by drawing curves, sketches, and equations in pencil on white marble-top tables. Kitchen attendants wiped away evidence of the lunchroom conversations at the end of the meal, but not until after freewheeling inquiry and expression had run their course, been postponed until noontime tomorrow, or carried over for the walk back to office, tunnel building, or shop.[7]

The earliest organization chart of "Langley Field Station" appeared 11 June 1920, the day of the Langley dedication ceremonies. A draftsman prepared it with standard engineering lettering. The senior staff engineer and the chief physicist, according to the chart, were of equal rank. The Executive Committee in Washington considered the lab's administrative load light enough for Lewis to handle from Washington with a minimum of clerical staff at the lab, and with no on-scene, overall leader there at all. Perhaps such an in-house executive might have implied a degree of independence that the Executive Committee was not yet ready to acknowledge.

The matter of the NACA field executive, his role at Langley, and his relationship to the Washington office had had a troubled history before June 1920. George Lewis had split top research authority at Langley between the senior staff engineer and chief physicist earlier in the year after the stormy departure of John DeKlyn, engineer in charge of buildings and construction. Like Griffith, the first engineer-in-charge, DeKlyn had been employed by the NACA originally in Washington, as a draftsman, before moving down to Hampton. Three years of thunderous construction headaches—aggravated by the general nastiness of life and work at the field during World War I—prompted DeKlyn to campaign in 1919 for moving the NACA facility to Bolling Field. Chronic jousting with the Committee's efficient if fastidious secretary, John Victory, over such petty administrative details as the mechanics of submitting travel vouchers, completely soured his



First organization chart of the LMAL, 11 June 1920. (See also appendix F.)

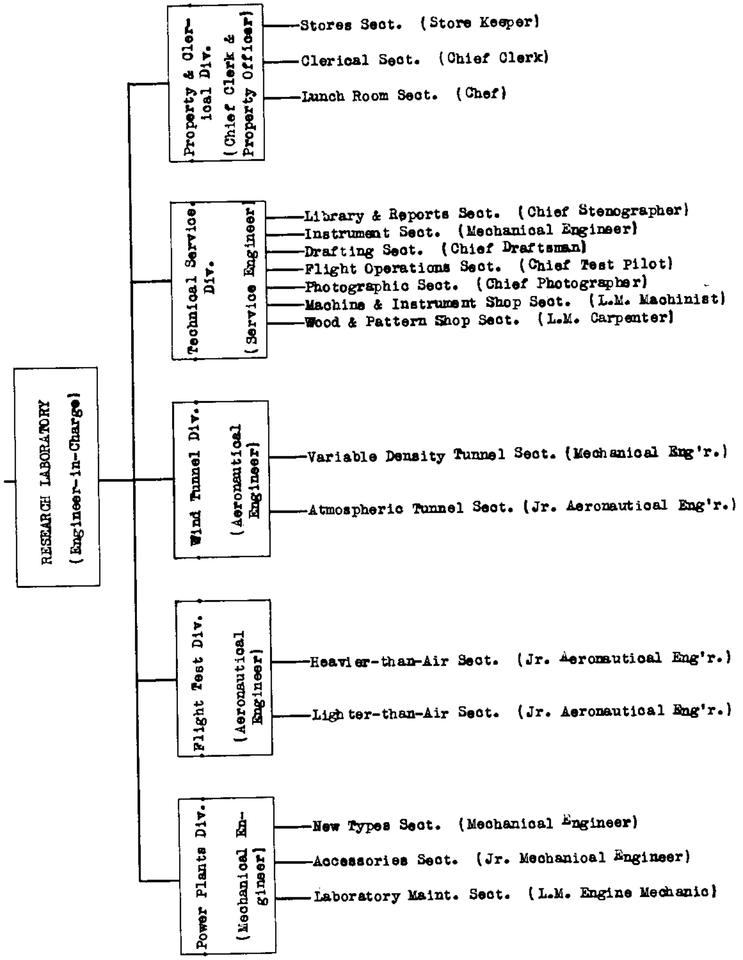
taste for the job. (Some routine correspondence between DeKlyn's staff and Victory's had been mismanaged, and Victory concluded that the Langley staff showed a lack of "courtesy and cooperation" in righting the matter. So the bureaucrat undertook to lecture DeKlyn on interoffice etiquette, even though DeKlyn, an engineer in the professional ranks of the civil service, was above Victory in both salary and prestige.) "Not about to be dictated to by a pompous place-filler in Washington," DeKlyn resigned in February 1920, preempting his dismissal by only a few days.[8] Bureaucratic bickering aside, DeKlyn was a draftsman and a construction engineer, and not a man to assist George Lewis in directing a comprehensive program of aeronautical research.

In 1920 Leigh Griffith, a 39-year-old mechanical engineer from California, became the senior staff engineer. The NACA had hired him some three years earlier as a "technical expert," upon the recommendation of its chairman, William F. Durand, professor of mechanical engineering and aeronautics at Stanford University. A power plants man, Griffith possessed little capacity to direct aerodynamic study. At Langley, his senior rank rested on his age, experience, and ability to manage the then-critical development of a high-performance aircraft engine, especially the testing of superchargers and fuel injection systems. The post of chief physicist was temporarily vacant in June 1920. Edward P. Warner, a Ph.D. in physics who lectured on aeronautics at MIT, his alma mater, had just left Langley

Leigh Griffith, Langley's first engineer-in-charge, far right, receives, from his immediate right to his far right, visiting NACA members William F. Durand, John F. Hayford, and NACA secretary John F. Victory in front of the LMAL administration building in 1923.

for a summer tour of European aeronautical facilities. Warner had commuted to the Hampton facility for brief stints since early 1919 to oversee the Committee's preliminary flight research programs and design of its first wind tunnel.[9] He returned to MIT full time in the fall of 1920 as an associate professor, and another graduate of the prestigious school, Frederick Norton, became Langley's chief physicist. Norton was at something of a disadvantage in dealing with the senior staff engineer, for Griffith was 15 years older, had served the NACA in Washington, and was a friend and engine-research colleague of George Lewis. Norton, on the other hand, knew aerodynamics. He could also claim to be the NACA's first permanent employee at Langley Field, having arrived there in the autumn of 1918.[10]

By the time Langley produced a second organization chart in October 1923, the Executive Committee had already named Griffith the "Engineer-in-Charge."[11] He received the position (which paid the lab's highest annual salary, $4800) in large part because he had taken more executive initiative than had Norton and had formally communicated to the Washington office detailed plans as to how Langley should be organized and managed. Griffith managed a fully operational lab that had grown both vertically and horizontally since 1920 and included 5 working divisions,



Second organization chart of the LMAL, October 1923. (See also appendix F.)

17 sections, and some 60 employees. For the next two years he generally maintained favor with both his staff and NACA headquarters by restricting personal research supervision to his own field of technical competence, engine development; by virtually keeping his hands off the work of the aerodynamics sections; and by effectively handling interoffice relations with Victory.[12]

During 1925, however, Griffith too succumbed to the rigid bureaucratic discipline of the NACA executive secretary. A dispute with Victory over correspondence (over the writing of letters deemed by Victory "unnecessary" and lacking in "etiquette") precipitated a nasty exchange of even less necessary and polite letters. Victory sought to end the matter with a letter to Griffith dated 19 March 1925: "In the further interests of economy and efficiency in correspondence, it is directed that *argumentative* matter, *unnecessary* matter, and *impertinent* or *irrelevant* matter be eliminated from official correspondence in the future." Griffith responded in a handwritten note: "Suggest that you consider the points mentioned ... and rewrite your letter with them in mind." Victory, tough as steel when it came to paperwork, made the note a matter of official record. Finding Victory's attitude "good evidence of ignorance," Griffith soon requested an extended leave of absence, ostensibly to devote himself to pressing family business in California. One Langley employee remembered long after the episode that the Washington office had informed Griffith before his leave began that he could not return to the NACA. Twenty-nine-year-old Henry J. E. Reid, a 1921 graduate in electrical engineering from Worcester Polytechnic Institute who was in charge of Langley's instrumentation research and development, succeeded Griffith as engineer-in-charge. Reid remained at the helm of Langley lab until his retirement from NASA in 1960.[13]

Henry Reid survived for so long—unlike DeKlyn and Griffith—in part because he always understood the idiosyncrasies of John Victory and abided by the secretary's insistence upon a centralized correspondence system. Less than a year after Griffith's departure, Reid instructed Langley secretaries to send

> no letters directly to the Washington office from anyone excepting [Langley Chief Clerk] Mr. [Edward R. "Ray"] Sharp or myself. Anyone wishing to communicate with the Washington office will do so by preparing a memorandum for forwarding by myself or Sharp or shall prepare a letter for my signature.[14]

Employees carried out this close-to-the-vest policy for as long as Reid and Victory shuffled papers between Langley and Washington; that is, for the rest of the NACA's history. Outgoing correspondence was reviewed and revised up through the division level until sanctioned in its final form by the office of the chief of research or its equivalent; then it was signed by the engineer-in-charge. Letters could be taken off the premises only with approval, and no copies could be made without the approval of the head of the lab or his designated agent. Incoming letters to individual researchers were routed directly to them, but only after being opened by the mail clerks. Copies of these letters were made for central files and for distribution to top researchers and research managers in the relevant technical fields both at Langley and in Washington. New personnel discovered that "we don't say that" or "we don't say it that way here at Langley." NACA correspondence policy was so strict that some people worked at the laboratory for 30 or

Langley administrative office, 1927.

40 years without ever sending a work-related letter directly to an outside address.[15] Similar regulations restricted telephone calls.

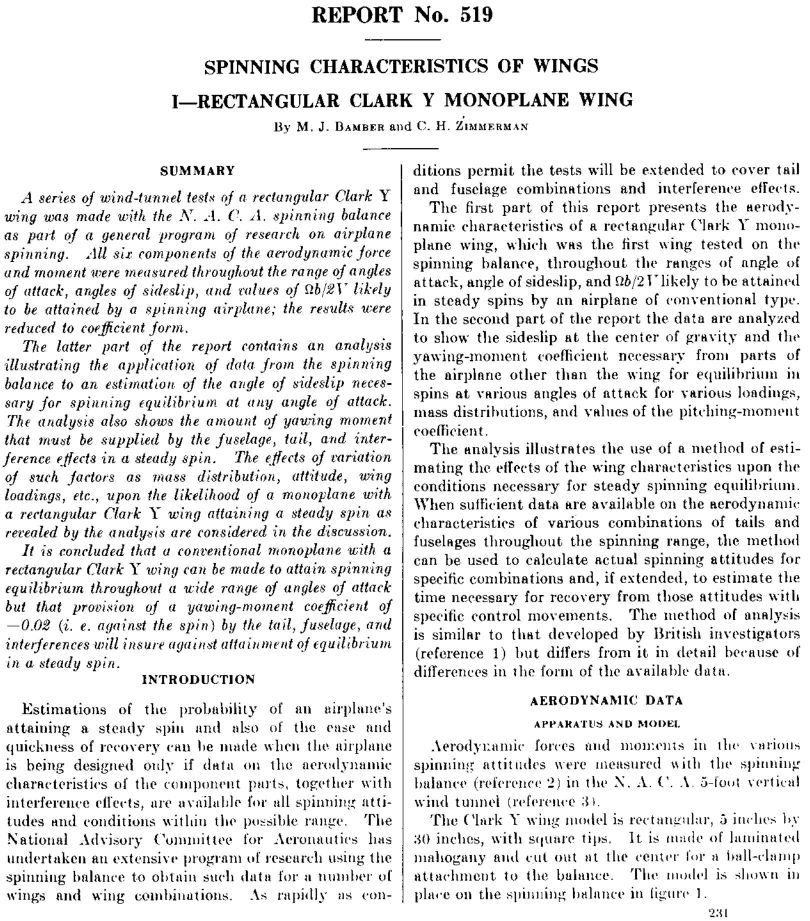
Despite the constraints Victory obligated him to impose on the laboratory, Reid was considered a model supervisor by most thoughtful employees. He usually did not mind qualified personnel going around him with their ideas to Washington, and when he did mind, he did not object in a way that made enemies. Perhaps Reid's greatest strength was his willingness to let young researchers be themselves; he did not try to make them all fit the same mold. This was an essential leadership quality for the man in charge of Langley, the acknowledged center of American aeronautical research in the late 1920s and 1930s, where talented, highly motivated researchers seeking national and international reputations in science and technology needed elbow room in order to produce the results wanted by both the NACA and its many clients. With temperamental individuals rocking the boat for resources, respect, and reputation, Reid deserves great credit for keeping Langley on an even keel.

Because employees viewed Reid as a model supervisor, his (and Victory's) strictness in regard to correspondence was duplicated up and down the organizational line. Nearly all correspondence between sections, for example, required the section head's signature. One section head extended the

LMAL division chiefs confer with the engineer-in-charge in April 1929. Left to right: E. A. Myers, Personnel Division; Edward R. Sharp, Property and Clerical Division; Thomas Carroll, Flight Test Division; Henry J. E. Reid, engineer-in-charge; Carlton Kemper, Power Plants Division; and Elton Miller, Aerodynamics Division.

policy so far as to list himself as an author on a majority of the technical reports from his research group, apparently believing that routine suggestions and reviews of reports justified the claim of authorship.[16]

The system of prepublication editorial review that George Lewis originally instituted for technical papers seems to have been equally strict. Theoretically it worked as follows: After a researcher at Langley had finished a rough draft of a technical paper, an editorial committee—consisting of three or four members of the Langley staff—met to offer constructive criticism of its accuracy, soundness, and clarity. The chairman of that committee—who did not have the authority to kill a report, although his power was tantamount to that if he was a man of real prestige—routed his appraisal of the paper through the author's division chief, stating whether the author had revised it in accordance with the suggestions of the committee. When the report finally reached the editorial office, its content had been rigorously checked and its form properly manicured. All preliminary copies of the paper were collected from editorial committee members and usually destroyed to prevent any use of unrevised work. In selected cases, the laboratory then circulated copies of the paper to members of the concerned NACA subcommittees for further suggestions for revision. After 1941 Langley also sent copies to its sister labs in California and Ohio for comment. Typically, the author received his paper back from reviewers several times in the organization's effort to ensure the validity and credibility of its research publications.



REPORT No. 519

SPINNING CHARACTERISTICS OF WINGS

I—RECTANGULAR CLARK Y MONOPLANE WING

By M. J. Bamber and C. H. Zimmerman

SUMMARY

A series of wind-tunnel tests of a rectangular Clark Y wing was made with the N. A. C. A. spinning balance as part of a general program of research on airplane spinning. All six components of the aerodynamic force and moment were measured throughout the range of angles of attack, angles of sideslip, and values of $\Omega b/2V$ likely to be attained by a spinning airplane; the results were reduced to coefficient form.

The latter part of the report contains an analysis illustrating the application of data from the spinning balance to an estimation of the angle of sideslip necessary for spinning equilibrium at any angle of attack. The analysis also shows the amount of yawing moment that must be supplied by the fuselage, tail, and interference effects in a steady spin. The effects of variation of such factors as mass distribution, attitude, wing loadings, etc., upon the likelihood of a monoplane with a rectangular Clark Y wing attaining a steady spin as revealed by the analysis are considered in the discussion.

It is concluded that a conventional monoplane with a rectangular Clark Y wing can be made to attain spinning equilibrium throughout a wide range of angles of attack but that provision of a yawing-moment coefficient of −0.02 (i. e. against the spin) by the tail, fuselage, and interferences will insure against attainment of equilibrium in a steady spin.

INTRODUCTION

Estimations of the probability of an airplane's attaining a steady spin and also of the ease and quickness of recovery can be made when the airplane is being designed only if data on the aerodynamic characteristics of the component parts, together with interference effects, are available for all spinning attitudes and conditions within the possible range. The National Advisory Committee for Aeronautics has undertaken an extensive program of research using the spinning balance to obtain such data for a number of wings and wing combinations. As rapidly as conditions permit the tests will be extended to cover tail and fuselage combinations and interference effects.

The first part of this report presents the aerodynamic characteristics of a rectangular Clark Y monoplane wing, which was the first wing tested on the spinning balance, throughout the ranges of angle of attack, angle of sideslip, and $\Omega b/2V$ likely to be attained in steady spins by an airplane of conventional type. In the second part of the report the data are analyzed to show the sideslip at the center of gravity and the yawing-moment coefficient necessary from parts of the airplane other than the wing for equilibrium in spins at various angles of attack for various loadings, mass distributions, and values of the pitching-moment coefficient.

The analysis illustrates the use of a method of estimating the effects of the wing characteristics upon the conditions necessary for steady spinning equilibrium. When sufficient data are available on the aerodynamic characteristics of various combinations of tails and fuselages throughout the spinning range, the method can be used to calculate actual spinning attitudes for specific combinations and, if extended, to estimate the time necessary for recovery from those attitudes with specific control movements. The method of analysis is similar to that developed by British investigators (reference 1) but differs from it in detail because of differences in the form of the available data.

AERODYNAMIC DATA

APPARATUS AND MODEL

Aerodynamic forces and moments in the various spinning attitudes were measured with the spinning balance (reference 2) in the N. A. C. A. 5-foot vertical wind tunnel (reference 3).

The Clark Y wing model is rectangular, 5 inches by 30 inches, with square tips. It is made of laminated mahogany and cut out at the center for a ball-clamp attachment to the balance. The model is shown in place on the spinning balance in figure 1.

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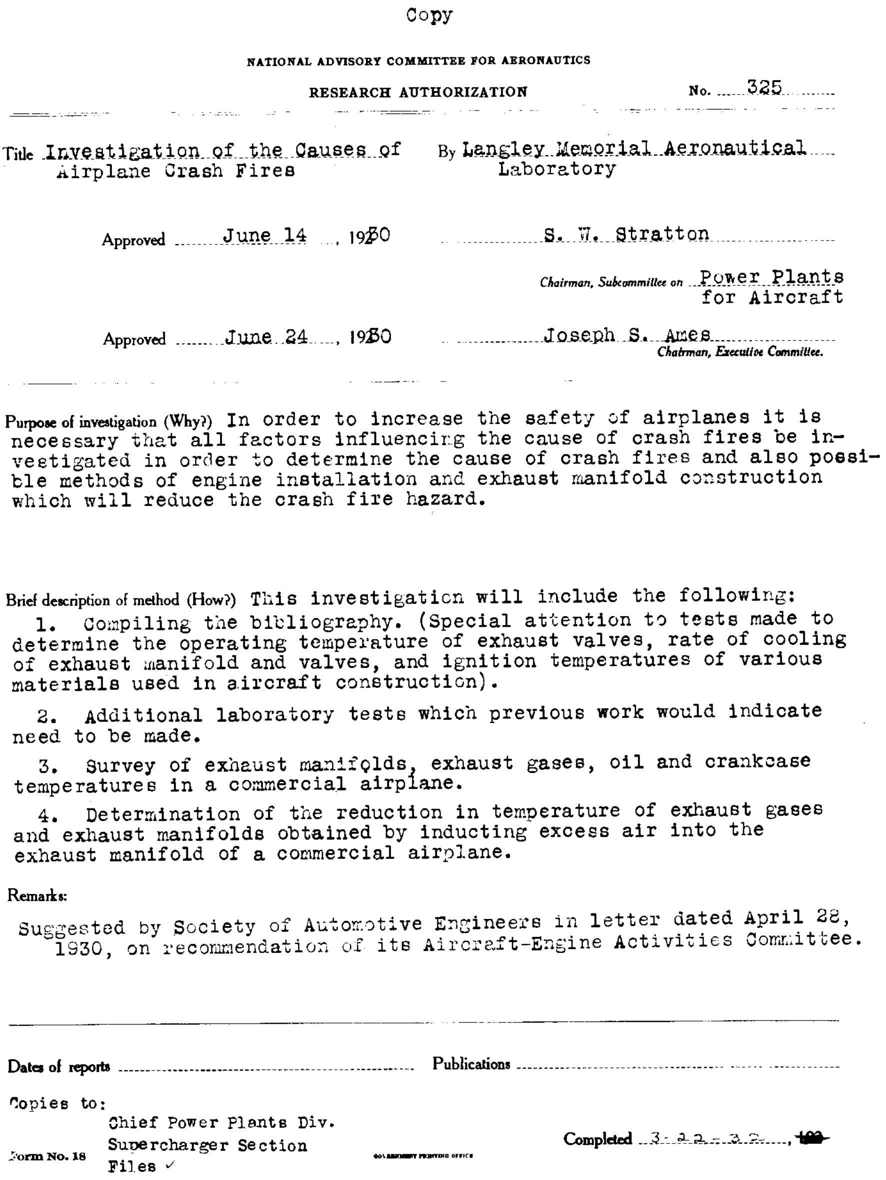
First page of a typical NACA report, 1934.

In practice, however, the system was less strict than in theory. Compliance with reviewer comments varied greatly, depending on the author's inclinations and the attitudes of the editorial committee chairman. Rarely did an editorial committee reconvene, which meant that members other than the chairman seldom saw either the outside comments or the revised report prior to its publication. Moreover, comments from other labs generally had little effect on the finished product. According to one Langley veteran, it was easy to give these comments, which usually came in weeks after the editorial committee meeting, "a polite weasel-worded brush-off."[17]

Research policy was in fact quite lenient. According to written instructions, Langley was supposed to have a *research authorization*, or RA, signed by the Executive Committee chairman for each of its investigations; but, in reality, approval of a research idea was very often just a formality. The Washington office turned down a Langley concept rarely: "Any scheme for research that survived peer discussion [at the lab] and gained section and division approval was likely to be implemented." Sometimes an engineer even went ahead with an idea without formal approval. George Lewis and Henry Reid looked the other way from this "bootlegged" work in the early days because they understood that it sometimes produced as much of value as did the best-prepared programs. Furthermore, the NACA worded its initial RAs using vague general terms like "similitude testing," "controllability testing," or "tests on wings," and kept authorizations operating as long as possible. This practice allowed researchers at Langley the flexibility to do almost anything they wanted under the umbrella of the formal program.[18]

When NACA management was not sure of the urgency of research in a new field or special subject, it went only so far as to give a few of its more talented personnel the freedom to educate themselves in it, to teach its basics to colleagues, and perhaps even to build simple, low-cost experimental equipment. This happened several times at the laboratory—especially before World War II changed research priorities—and sometimes without the approval, or even the knowledge, of headquarters.

After returning from the Volta Congress on high-speed aeronautics in late 1935, for example, Eastman Jacobs, one of the lab's most brilliant section heads, decided that Arthur Kantrowitz, a young physicist from Columbia University, could contribute the most to the NACA by studying the principles of supersonic flow. Jacobs made this decision on his own, in defiance of a cautious NACA management stance against supersonics. A few years later, after both men had concluded that there was no physical prohibition of supersonic flight, Jacobs gave Kantrowitz an open job order to design a small supersonic wind tunnel. With the help of engineers in Jacobs's section, Kantrowitz finished this job successfully in less than



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NATIONAL ADVISORY COMMITTEE FOR AERONAUTICS

RESEARCH AUTHORIZATION

No. 325

Title Investigation of the Causes of Airplane Crash Fires

By Langley Memorial Aeronautical Laboratory

Approved June 14, 1930

S. W. Stratton
Chairman, Subcommittee on Power Plants for Aircraft

Approved June 24, 1930

Joseph S. Ames
Chairman, Executive Committee.

Purpose of investigation (Why?) In order to increase the safety of airplanes it is necessary that all factors influencing the cause of crash fires be investigated in order to determine the cause of crash fires and also possible methods of engine installation and exhaust manifold construction which will reduce the crash fire hazard.

Brief description of method (How?) This investigation will include the following:

1. Compiling the bibliography. (Special attention to tests made to determine the operating temperature of exhaust valves, rate of cooling of exhaust manifold and valves, and ignition temperatures of various materials used in aircraft construction).

2. Additional laboratory tests which previous work would indicate need to be made.

3. Survey of exhaust manifolds, exhaust gases, oil and crankcase temperatures in a commercial airplane.

4. Determination of the reduction in temperature of exhaust gases and exhaust manifolds obtained by inducting excess air into the exhaust manifold of a commercial airplane.

Remarks:

Suggested by Society of Automotive Engineers in letter dated April 28, 1930, on recommendation of its Aircraft-Engine Activities Committee.

Dates of reports .... Publications ....

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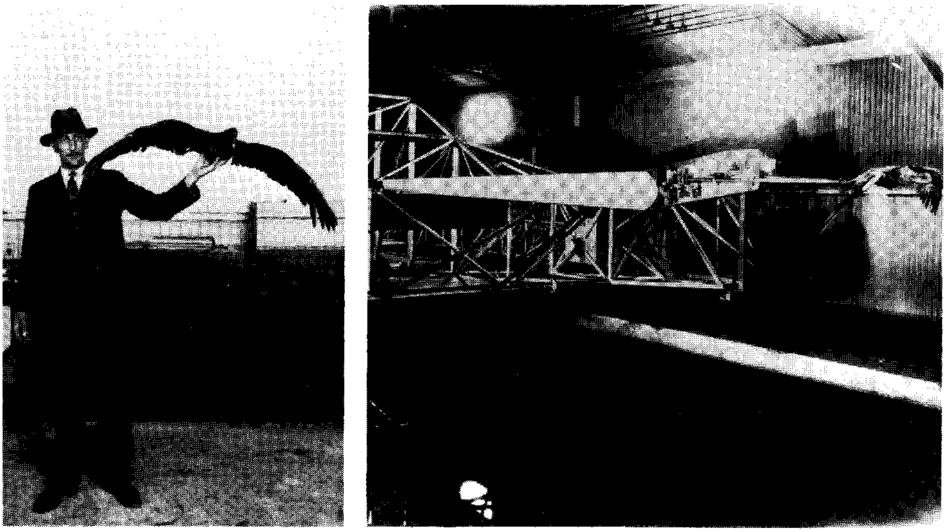
Form No. 18

GOVERNMENT PRINTING OFFICE

Completed 3-22-32, ~~192~~

Example of an NACA research authorization (RA), June 1930.



Langley old-timers refer to unauthorized testing as "bootlegging." Some of their stories about bootlegged tests are apocryphal, however. According to one of these tall tales, a group of employees wondered out loud during a lunchroom conversation in 1932 about the aerodynamic characteristics of some of the birds that flew over Langley. One of the men who took the subject most seriously, Tom Collier (left), shot a buzzard, and froze it with its wings outstretched for unauthorized testing in the NACA towing tank. The test results indicated that the frozen bird could not fly because it was inherently unstable (birds are, in fact, unstable, but this has never stopped them from flying)! The teller of the tale never mentions, however, that tests of soaring birds in the NACA tank had been proposed by Victor Lougheed of the U.S. Navy Bureau of Aeronautics. Moreover, in May 1932, the Virginia Commission of Game and Inland Fisheries had issued a permit for Lougheed "to possess and transport for use in connection with flight investigations, ten sea gulls." At right, one of these gulls is being tested on the carriage of the towing tank.

18 months. An unauthorized order from Jacobs thus led to the pioneering 9-Inch Supersonic Tunnel for the NACA, one of the first supersonic tunnels in the United States.[19]

Special independent studies like those done by Kantrowitz in supersonics during the late 1930s were permissible at Langley as long as they were not too exotic, did not require too many agency resources, and did not draw adverse public attention. One study that the NACA ultimately did cancel for being too far-out involved the first American experiment designed to achieve thermonuclear fusion. Kantrowitz and Jacobs read in a newspaper in 1938 that Westinghouse had just bought a Van de Graaff generator. The two men knew that this huge electrical device, which produced sparks several feet long, was being used in atom-smashing experiments, so they suspected that Westinghouse had bought the machine to begin exploring ways of making nuclear power a reality. While discussing the news item (which led both of them to study the works of Hans Bethe), Kantrowitz and Jacobs got the notion that if a very hot plasma (e.g., an electrically neutral, partially ionized gas) could be confined magnetically, a fusion reactor could be built. Jacobs, who had good rapport with NACA headquarters because of his promising work on laminar-flow airfoils (see chapter 4), managed to get $5000 from George Lewis for construction of a big aluminum torus with a coiled magnetic device whose purpose would be, Jacobs said, to study the potential of atomic power for aircraft. Using the drive motor of the Variable-Density Tunnel as the power supply for the magnetic field, away went Jacobs and Kantrowitz, trying to excite the plasma to a high enough temperature to produce X-rays. But before they could achieve the necessary temperature, Lewis came by the laboratory one day and happened upon the fusion apparatus. Knowing that nonaeronautical experimental equipment of so radical and dangerous a nature was not appropriate for the NACA, Lewis canceled the project on the spot. Jacobs and Kantrowitz* both considered the cancellation a tragedy since experiments with the torus had led to several important discoveries.[20]

The personality, long-term directions, and aspirations of an organization like NACA Langley are seldom revealed by formal policy statements. Goals emerge more often as a set of constraints defining acceptable performance. The NACA correspondence and editorial review policies clearly demonstrate the influence of the strictness of John Victory and George Lewis on the development of the Langley personality. Neither wanted anything

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* During World War II Kantrowitz worked at Langley on airfoil cascades, axial-flow compressors, and the dynamics of gas turbines. Then he began to devote himself more and more to exploring the connection between quantum physics and fluid mechanics. The first connection that he established was in an NACA paper on heat-capacity lag that demonstrated how the vibrational energy of a gas (CO_2) lagged behind changes in temperature occurring in a gas flow ("Effects of Heat-Capacity Lag in Gas Dynamics," Advanced Restricted Rept. L4A22, 22 Jan. 1944. There is an earlier version of this paper by Kantrowitz, dated 8 Dec. 1941, in the Floyd Thompson Memorial Library, LaRC, Code 5070-184. An abbreviated version of it appeared in the *Journal of Chemical Physics,* 14 Mar. 1946, pp. 150–164). Kantrowitz left Langley in 1946 for a professorship at Cornell University. A year later Columbia accepted a revision of his paper on heat-capacity lag as his doctoral thesis. In the 1950s Kantrowitz went to work for the AVCO Research Laboratory in Everett, Massachusetts, where he worked on various ICBM concepts. Later he studied the science of blood clotting with his brother, a famous cardiologist, and designed a series of cardiac assist devices, including an artificial heart. Obviously, it was wise for a man with as many rich and different scientific interests and talents as Arthur Kantrowitz not to restrict his career to aeronautics only, at Langley only.

imperfect to come out of the laboratory and be associated with the Committee. Victory wanted all routine business conducted by the book, down to the smallest detail of epistolary style and grammar. Lewis wanted published reports to be accepted as holy writ. S. Paul Johnston, former editor of *Aviation* (1931–1940) and an employee at NACA headquarters (1940–1942), remembers that Lewis "was so afraid that he would get caught with a mistake in a report that wind tunnel results—all research results—would be hung up down there at Langley until all the *i*'s were dotted and *t*'s crossed and he was damn sure that the results were what they said." Russell G. Robinson, a more veteran headquarters employee, argues that this was not fear on Lewis's part, just extreme insistence on technical integrity. Lewis recognized that only the most highly respected scientific and technical papers could buttress the Committee's public positions.[21]

One can debate the long-term value and significance of such strict controls on the laboratory. They were tough, time-consuming, and occasionally traumatic. Victory's bureaucratic tenacity cost the NACA two senior engineers (DeKlyn and Griffith), but their resignations caused barely a ripple in the flow of research. Lewis's editorial policy may have prevented the prompt publication of an occasional paper on the ground that the lab would "at some later date in the indefinite future be able to check and amplify the work and so make a more valuable report."[22] In terms of institutional behavior, the policies of Lewis and Victory, the long-lived father figures of the lab, seem to have promoted a certain conservatism, a caution against prematurely announcing research results, and a reluctance to embrace for publication research writings and ideas from other than the NACA's rigorously scrutinized sources.

On the other hand, in the constraints imposed upon Langley there was freedom. Lewis's attitude about organization charts, for instance, permitted researchers in the field to communicate through informal "shadow" networks. His editorial policy heightened self-confidence in the NACA product and method of quality control and freed researchers to work creatively on novel ideas without the fear of preliminary reports building up too much industry anticipation of and pressure for future advances. Victory's centralized correspondence system, as instituted in Henry Reid's offices, freed employees from bothersome paperwork. In sum, the organization exhibited throughout its history a delicate blend of careful bureaucratic constraint with research freedom.

The Family

The most vital arena for aeronautical research was the human mind, not the wind tunnel. No facility could substitute for talent and creativity. Without employing enough individuals who possessed "knowledge of the existing state of aerodynamics, experiences in the study of its fundamental problems, and who combine[d] engineering training with profound mathematical knowledge, the rare gift of originality, and demonstrated ability in the conduct of research," the best NACA leaders understood that Langley could not accomplish its assigned duties no matter how good the management.[23]

The first thing that needs to be remembered about the Langley staff in the early years of the NACA was that it was very small. The total complement did not reach 100 until 1925, and the complement of research professionals did not reach that number until 1930 (see the table below). Research members of the various aerodynamic sections numbered only 23 in 1927: 12 in flight research, 6 in the atmospheric wind tunnel, 4 in the propeller research tunnel, and 1 in a prototype ice tunnel. The power plants sections had 16: 7 in engine research and 3 each in fuel injection, supercharger testing, and engine analysis. Four people worked in a physics lab. Between 1927 and 1930—the crucial period when Congress increased NACA appropriations from half a million to over a million dollars—Langley hired 55 new professionals. Through the worst years of the Depression, the Committee was able to get enough money to keep the lab's professional and nonprofessional staff levels steady. In 1936 Langley employed three times the staff it had in 1925, but that expansion still amounted to only 230 more employees. The staff size was such that members from junior engineering aide to engineer-in-charge could know each other personally.

The arrangement and apparel of the Langley staff in the accompanying photograph (p. 43), April 1921, reflect Langley's original social structure. The photograph shows 34 employees: 33 men and 1 woman. Leigh Griffith, senior staff engineer, stands on the loading dock seventh from the left. Posed second to his right (on the other side of white-shirted David Bacon, head of the Variable-Density Tunnel section) is Frederick Norton, chief physicist. Though the two men held equal rank at the time officially, the photographic impression suggests seniority: Griffith over Norton. A mechanic, a physicist, and an engineer stand to Norton's right, with Henry Reid, future engineer-in-charge, at the very end. Kneeling in front of these men are the four members of flight operations. All four had World War I military experience. The moustached man resembling actor Errol Flynn, Growth of Langley Staff, 1919–1939

Fiscal year	Professional	Nonprofessional	Total
1919	4	7	11
1920	12	15	27
1921	12	32	44
1922	18	38	56
1923	23	52	75
1924	36	62	98
1925	39	72	111
1926	44	92	136
1927	45	104	149
1928	60	108	168
1929	79	110	189
1930	100	128	228
1931	102	155	257
1932	111	159	270
1933	110	150	260
1934	109	140	249
1935	111	164	275
1936	138	203	341
1937	149	253	402
1938	166	260	426
1939	204	320	524

Source: "Growth of Langley's Staff," 16 Sept. 1965, Langley Historical Archive, Milton Ames Collection Box 2.

test pilot Thomas Carroll, had served in France teaching air tactics to pilots. Two of Griffith's power plants engineers crouch in front of Norton, Bacon, and Griffith. The five men (two kneeling, three standing) in the middle of the photo wearing coats with vests made up the drafting section. Two men and, standing between them, one woman composed the property and clerical staff. Finally, the right side of the picture shows the technical service employees in rolled-up shirt sleeves and work clothes.

The median age of the Langley staff in 1921 was roughly 28. The professionals especially were young: power plant engineers Gardiner and Ware were 23 and 27, respectively; assistant physicist Brown was 24 and electrical engineer Reid 26. Morgan, head of the drafting room, was 41; his boys called him "Pop." Only two men had significant aerodynamical

LMAL staff, April 1921. Front row, left to right: J. Turon, mechanic; Robert Mixson, airplane mechanic; Fred Hunsecker, airplane mechanic; Thomas Carroll, test pilot; Marsden Ware, power plant engineer; Robertson Matthews, power plant engineer; E. Tasso Morgan, draftsman; Arthur Webster, draftsman; Percy Keffer, patternmaker; Harwood Moore, toolroom attendant; J. D. Shurtleff, toolmaker; Harry Downs, leadingman machinist; Howard Morris, toolmaker; Charles Wolf, mechanic; and Samuel Eakin, mechanic. Back row, left to right: Henry J. E. Reid, electrical engineer (later to become engineer-in-charge); William G. Brown, assistant physicist; Arthur Gardiner, power plant engineer; H. M. Metz, engine mechanic; Frederick H. Norton, physicist; David L. Bacon, mechanical engineer; Leigh Griffith, senior staff engineer (later to become engineer-in-charge); C. H. Masters, draftsman; William C. Morgan, mechanical engineer; Benjamin Bennett, draftsman; Frank Herbert, property officer; A. M. Campbell, stenographer; Joseph McManus, chief clerk; William Adams, carpenter; Edward Raub, toolmaker; Ernest Gay, chief carpenter; John Hanks, mechanic; John Evans, mechanic; and Edward McDonald, fireman.

experience: physicist Norton and engineer Bacon. Norton had done "a little work" in the MIT wind tunnel. This qualified him at age 25—and only three years after his graduation—to be Langley's chief physicist.[24] Twenty-six-year-old David Bacon had worked between 1918 and 1921 for the Gallaudet Aircraft Corporation doing design, development, and a certain amount of research work involving pressure distribution tests on seaplanes in free flight. Hired fresh out of school with a minimum knowledge of aerodynamics and little practical experience of any kind, the majority of these early Langley researchers learned nearly everything on the job. Because they were so young, they had not yet learned that a lot of things just could not be done, so they went ahead and did them.

Members of the technical staff who supported the research effort with various services, such as carpentry and mechanics, or making wind tunnel models and special tools, were older and more experienced. Most of these people came from the immediate vicinity of the lab. The communities

Engineer David L. Bacon (far left) and physicist Frederick H. Norton, escorted Orville Wright, in hat, around the laboratory during his visit in July 1922. To the far right is George Lewis.

of Hampton, Newport News, Portsmouth, and Norfolk possessed a large population of craftsmen and artisans skilled in the operation of machinery, in wood, metal, and concrete construction, in marine and auto repair, in toolmaking, and in the design and uses of various instruments and electrical equipment. The Newport News Shipbuilding and Dry Dock Company, located at the terminus of the Chesapeake and Ohio Railways, was one of the largest firms of its type in the world. Its apprentice programs attracted young workers from as far away as South Carolina and New Jersey. During World War I, the fine harbors of Hampton Roads—where the ironclads *Monitor* and *Merrimack* had fought their famous battle during the Civil War—became the home of America's largest naval base and port of embarkation, their shores lined with naval workshops, shipyards, depots, cantonments, and fortifications. Back at Langley Field another cadre of technicians, including engine mechanics, aerial photographers, and aerial and ground observers, worked for the army. The NACA recruited most of its technical service personnel from such local talent. These craftsmen were prized highly by the professional staff, for they provided the essential support services on which all the research programs depended.

The work of talented mechanics and other technical employees was instrumental to the NACA's success. These three photographs show some of the everyday activities of early LMAL hangar personnel. Left, a mechanic stands on a stool to work on an air-cooled radial engine. Right, metalworkers weld a piece of pipe. Bottom, two mechanics measure wing ordinates on a Curtiss Jenny airplane.

Unlike the nonprofessionals, the majority of professionals came to work at Langley from outside the local community, in particular from the industrialized states of the Northeast and Midwest which had the major engineering schools. This resulted in a clique of New Englanders at the lab who had studied at such places as MIT, Cornell, Yale, and Worcester Polytechnic Institute, as well as a large group of graduates from the University of Michigan.*

These young men had chosen to go to work for the NACA for various personal reasons, most of which centered on an attraction to airplanes and flying. Smith J. "Smitty" DeFrance, one of the Michigan graduates, went to work at Langley in 1922 after completing his degree in aeronautical engineering. He had left college temporarily during World War I to train as an aviator with the Canadian Flying Corps and later, after America's formal entry into the conflict, had flown with the U.S. Army's 139th Aero Squadron. For DeFrance, taking the civil service junior aeronautical engineer's exam that led to NACA employment was simply "a matter of getting a job," as in 1922 there was still a serious postwar recession plaguing the country.[25] Floyd L. Thompson decided to come to Langley because DeFrance, a fellow Michigan alumnus, told him that the Virginia lab was "a good place to work." "He said they have roses for Christmas," Thompson remembered in 1973, and, coming out of a long and snowy Ann Arbor winter, "that impressed me too." Thompson took the qualifying exam, but because he heard nothing of his application for a long time, he also applied for a job as a field representative at the Pontiac plant of General Motors:

> The day came when I got a response from General Motors which said report up to Pontiac for duty, and I was just about to go there when ... I got a letter from Langley ....

Thompson chose Langley over Pontiac because he felt it "was the only opportunity that I knew of anywhere to get into interesting work in aeronautics"—his true passion. (In 1918, Thompson had been a member of the first class of the U.S. Navy's Great Lakes aviation mechanics school, and had then spent a year at Pensacola serving as a member of the first

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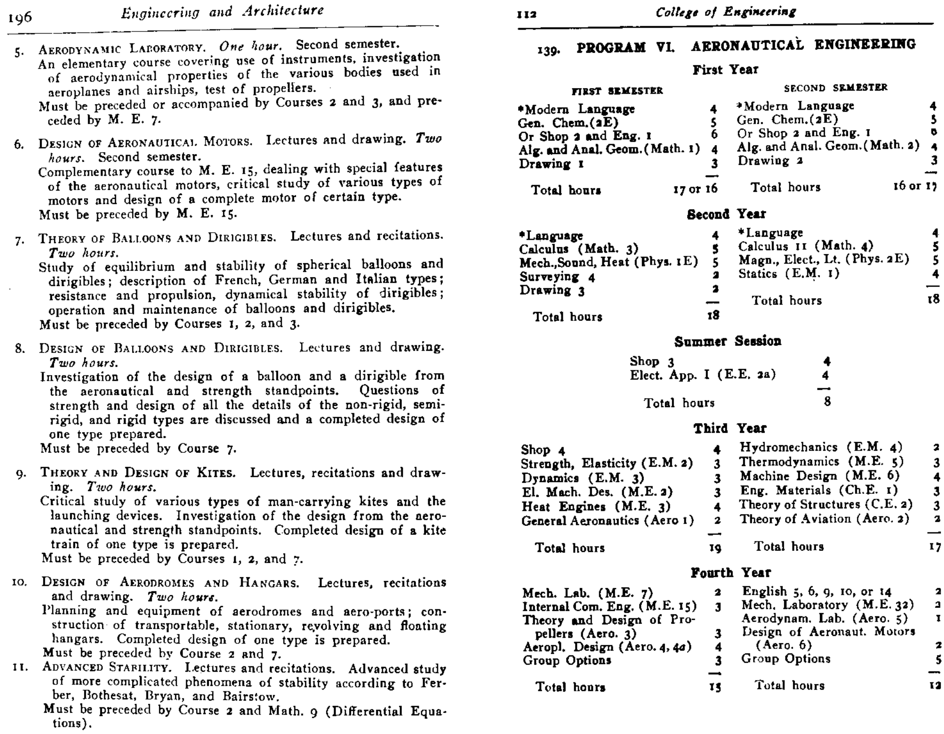
* The New England clique was led by Edward P. Warner, Fred Norton, and John Crowley from MIT; David Bacon from Yale; and Henry Reid from Worcester. The Michigan group included Starr Truscott, class of '09; Robert G. Freeman, class of '21; Clinton H. Dearborn, Smith J. DeFrance, Elliott G. Reid, and Kenneth M. Ronan, class of '22; George J. Higgins, Ernest D. Perkins, and Maurice D. Warner, class of '23; Maitland B. Bleecker, George L. Defoe, and Karl J. Fairbanks, class of '24; Millard J. Bamber, class of '25; Floyd L. Thompson, class of '26; Howard W. Kirschbaum, class of '27; and Robert J. Woods, class of '28.

naval torpedo plane squadron. One of his most memorable experiences in late adolescence was seeing a Larsen monoplane flying from Milwaukee to Chicago.)[26]

The stories of DeFrance and Thompson, besides being indicative of the motives of many others who chose to come to work for the NACA in the early years, are of special interest. These two Michigan aeronautical engineers spent their entire careers with the NACA and NASA, becoming directors of Ames and Langley laboratories, respectively, each leaving his distinctive stamp on the character of his research center.

The great majority of professionals who reported to work at Langley in the 1920s came to the lab with engineering training not specifically designed to prepare them for doing advanced aeronautical research. Only a few had specialized in aeronautical engineering. The University of Michigan, MIT, and New York University had degree programs in aeronautics by 1926, but only three schools—Caltech, Stanford, and the University of Washington, each a continent away from Langley Field—offered any aeronautics option for mechanical engineering students. And even the outstanding education at these few schools had serious limitations, especially in the teaching of aerodynamic theories. According to Stanford's aerodynamics professor Elliott G. Reid, existing textbooks in English on such subjects as airfoil theory were "too advanced, too academic or too condensed for maximum usefulness in the classroom." As the principal text for his presentation of wing theory to graduate students in aeronautical engineering during the early 1930s, Reid was thus compelled to use the NACA's 1921 translation of Ludwig Prandtl's classic 1904 paper "Applications of Modern Hydrodynamics to Aeronautics." Reid found this a "difficult experience" for everyone involved, not only because the NACA's translation of Prandtl's work lacked clarity and precision, but because the translation retained the German aerodynamic symbols and coefficients and also included somewhat superfluous sections devoted to airship hulls and propellers.[27]

Nonetheless, the aeronautical, mechanical, and electrical engineering programs at American universities and polytechnical schools did a relatively good job of preparing young graduates to adapt to advanced aeronautical research. Mechanical engineering was a broad subject in the 1920s, covering nearly everything pertaining to prime movers, generation of power, and manufacturing. It interlocked with all other branches of engineering and dealt with the design, construction, testing, and even sales of machines and mechanical devices, together with the arrangement of the plants in which they were produced. With concentrated reading in aerodynamics and a postgraduate exposure to aircraft, wind tunnels, and aeronautical instruments, there was no reason why bright young mechanical engineers



196 Engineering and Architecture

5. Aerodynamic Laboratory. *One hour.* Second semester.
An elementary course covering use of instruments, investigation of aerodynamical properties of the various bodies used in aeroplanes and airships, test of propellers.
Must be preceded or accompanied by Courses 2 and 3, and preceded by M. E. 7.

6. Design of Aeronautical Motors. Lectures and drawing. *Two hours.* Second semester.
Complementary course to M. E. 15, dealing with special features of the aeronautical motors, critical study of various types of motors and design of a complete motor of certain type.
Must be preceded by M. E. 15.

7. Theory of Balloons and Dirigibles. Lectures and recitations. *Two hours.*
Study of equilibrium and stability of spherical balloons and dirigibles; description of French, German and Italian types; resistance and propulsion, dynamical stability of dirigibles; operation and maintenance of balloons and dirigibles.
Must be preceded by Courses 1, 2, and 3.

8. Design of Balloons and Dirigibles. Lectures and drawing. *Two hours.*
Investigation of the design of a balloon and a dirigible from the aeronautical and strength standpoints. Questions of strength and design of all the details of the non-rigid, semirigid, and rigid types are discussed and a completed design of one type prepared.
Must be preceded by Course 7.

9. Theory and Design of Kites. Lectures, recitations and drawing. *Two hours.*
Critical study of various types of man-carrying kites and the launching devices. Investigation of the design from the aeronautical and strength standpoints. Completed design of a kite train of one type is prepared.
Must be preceded by Courses 1, 2, and 7.

10. Design of Aerodromes and Hangars. Lectures, recitations and drawing. *Two hours.*
Planning and equipment of aerodromes and aero-ports; construction of transportable, stationary, revolving and floating hangars. Completed design of one type is prepared.
Must be preceded by Course 2 and 7.

11. Advanced Stability. Lectures and recitations. Advanced study of more complicated phenomena of stability according to Ferber, Bothesat, Bryan, and Bairstow.
Must be preceded by Course 2 and Math. 9 (Differential Equations).

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139. PROGRAM VI. AERONAUTICAL ENGINEERING

First Year

FIRST SEMESTER		SECOND SEMESTER	
*Modern Language	4	*Modern Language	4
Gen. Chem.(2E)	5	Gen. Chem.(2E)	5
Or Shop 2 and Eng. 1	6	Or Shop 2 and Eng. 1	6
Alg. and Anal. Geom.(Math. 1)	4	Alg. and Anal. Geom.(Math. 2)	4
Drawing 1	3	Drawing 2	3
Total hours	17 or 16	Total hours	16 or 17

Second Year

*Language	4	*Language	4
Calculus (Math. 3)	5	Calculus II (Math. 4)	5
Mech.,Sound, Heat (Phys. 1E)	5	Magn., Elect., Lt. (Phys. 2E)	5
Surveying 4	2	Statics (E.M. 1)	4
Drawing 3	2		
Total hours	18	Total hours	18

Summer Session

Shop 3	4
Elect. App. I (E.E. 2a)	4
Total hours	8

Third Year

Shop 4	4	Hydromechanics (E.M. 4)	2
Strength, Elasticity (E.M. 2)	3	Thermodynamics (M.E. 5)	3
Dynamics (E.M. 3)	3	Machine Design (M.E. 6)	4
El. Mach. Des. (M.E. 2)	3	Eng. Materials (Ch.E. 1)	3
Heat Engines (M.E. 3)	4	Theory of Structures (C.E. 2)	3
General Aeronautics (Aero 1)	2	Theory of Aviation (Aero. 2)	2
Total hours	19	Total hours	17

Fourth Year

Mech. Lab. (M.E. 7)	2	English 5, 6, 9, 10, or 14	2
Internal Com. Eng. (M.E. 15)	3	Mech. Laboratory (M.E. 32)	2
Theory and Design of Propellers (Aero. 3)	3	Aerodynam. Lab. (Aero. 5)	1
Aeropl. Design (Aero. 4, 4a)	4	Design of Aeronaut. Motors (Aero. 6)	2
Group Options	3	Group Options	5
Total hours	15	Total hours	12

The University of Michigan created one of the first and finest aeronautical engineering programs in the country. Left, a page from the university catalog, 1916–1917, showing course offerings; right, the specimen curriculum from the university's 1922–1923 catalog. (Courtesy of Michigan Historical Collections, Bentley Historical Library, University of Michigan–Ann Arbor)

could not turn into insightful, productive aerodynamical researchers. In fact, many did at Langley laboratory.

Electrical engineers were even better prepared for careers in aerodynamical research. Along with instruction in the fundamental applications of electricity, they were trained to develop and use recording instruments like those necessary to measure the forces acting on an airplane in real or simulated flight. Moreover, electrodynamic theory, its symbols and equations, translated nicely into aerodynamic theory. Finally, the effective operation of laboratory machinery—especially the wind tunnels—depended upon electric power and the engineer's ability to tend power systems, generators, transformers, and the like. (Henry Reid, Langley's engineer-in-charge from 1926 to 1960, was an electrical engineer.)

The training of engineers specifically for aeronautics, then, was not the most serious problem. The problem was attracting and keeping a sufficient number of engineers with even the basic requirements. A Langley power plants engineer, on returning from an unsuccessful recruiting trip to Swarthmore College in 1924, reported to the engineer-in-charge that seniors

> had been surfeited with propositions from commercial concerns. Many such representatives had been proselytizing at the college and offering great inducements, especially as regards advances in the sales field with only enough preparatory training to give them a basis for sales talk. One commercial organization had gone as far as to give three separate talks, a week apart, in order to arouse and maintain interest.

The net result was that Langley's recruiter found most of the Swarthmore men either signed up for jobs or practically so and not, therefore, in a receptive frame of mind. The chief disadvantage for the NACA—besides its generally lower starting pay—was the requirement of a civil service examination before appointment, while the degree sufficed for commercial concerns.[28]

The NACA knew that some of its research professionals planned to stay at Langley for only a brief time, like graduate fellows at a university, until they could secure more attractive employment in the aircraft industry. In 1926 George Lewis wrote to Alexander Klemin, New York University aeronautics professor, about the value of an NACA apprenticeship:

> I feel that an engineering graduate who obtains a position with this Committee has an excellent opportunity to extend his theoretical knowledge, and, in particular, prepare himself as a research engineer. The opportunities for advancement are good, as evidenced by the fact that all of the activities at Langley Field are in charge of engineers who are recent graduates. All of the men who have left the Committee and who were in charge of major activities at our laboratory are now in charge of research laboratories.[29]

Lewis and the rest of NACA management accepted the abbreviated length of many tenures grudgingly, however, and embraced those who decided, because the work proved sufficiently interesting and challenging, to stay longer than planned.

The frequency of such resignations in the 1920s constituted a real threat to the operation of the laboratory. A survey of staff service cards shows that no fewer than 37 men of professional grade left Langley between 1920 and the end of 1931 after relatively brief stays on a professional staff that took until 1930 to total 100. The median age of these departing employees was only 28. Fifteen had graduated from the prestigious aeronautical engineering programs at MIT and Michigan. Both the aerodynamics and power plant divisions suffered serious losses of key personnel. Two chief

LMAL staff, August 1926. The number of female employees has grown from one in 1921 to seven. George Lewis and Henry Reid (his mouth and chin not visible) are sitting in the middle of the third row. To Lewis's right is chief test pilot Tom Carroll; to Reid's left is chief clerk Edward R. "Ray" Sharp.

physicists resigned in a period of less than two years (1923-24). This led Leigh Griffith, who himself was soon to leave the NACA, to say, "The aerodynamics research has not been subject to the same detailed guidance that it received previously ... so that the research work in flight and in the two tunnels has not progressed as rapidly as I desired."[30] Resignations seem to have climaxed in 1927 and involved such major figures as Marsden Ware and Arthur Gardiner, Langley's top power plants engineers; Elliott Reid, the head of the Atmospheric Wind Tunnel section; Paul Hemke, physicist; Paul King, test pilot; and Max Munk, the aerodynamicist imported by the Committee from Germany soon after the end of World War I. (Munk's resignation was a special case that will be studied in detail in the next chapter.)

Several events in the mid-1920s stimulated American aviation and created a highly mobile market for aeronautical engineers. Henry Ford started the first regular commercial air freight line (between Detroit and Chicago) in 1925, and in the same year Congress passed the Kelly bill authorizing contract air transport of mail. In 1926 President Coolidge signed the Air Commerce Act, the first federal legislation regulating civil aviation; the Daniel Guggenheim Fund for the Promotion of Aeronautics made its initial university grant; and NACA Langley hosted its first annual manufacturers' conference. Lindbergh flew the Atlantic in 1927. This rapid series of events awakened Americans to the potential of flight, and the aircraft construction industry took off. During this "Lindbergh boom," nearly everyone became interested in flying. As a result, worldwide sales of American-built aircraft shot up from 789 units in 1925 to over 6000 units in 1929.[31]

The resulting stiff competition for qualified aeronautical technologists caused wage wars that cost Langley several of its most promising researchers. Twenty-five of the 37 men mentioned earlier as having left the lab between 1920 and 1931 resigned during the Lindbergh boom. We know the immediate post-NACA employment of fourteen of this group: nine joined industry, four academia, and one the military. We can guess that industry coaxed most of the others also. Two of the Committee's recruits from the University of Michigan in 1924, Karl J. Fairbanks and Maitland B. Bleecker, resigned to take jobs with industry within two years of Langley employment. Bleecker went to work for Wright Aeronautical Corporation in New Jersey, and Fairbanks became a stress analyst for Consolidated Aircraft in New York (and later a technical adviser to the board of directors for AVCO and, during World War II, management coordinator for Brewster Aeronautical Corporation). Robert J. Woods, Michigan class of 1928, resigned his Langley post after barely one year at the lab to take successive jobs with Towle, Detroit, Lockheed, Consolidated, and Bell aircraft companies. With them he made major contributions in the field of military aircraft design, especially for the P-39 Airacobra, and helped to initiate the Bell X-1 supersonic research airplane program. Fred Weick, who had taken a B.S. in mechanical engineering from the University of Illinois in 1922, resigned in 1929 to become chief engineer of the Hamilton Aero Manufacturing Company in Milwaukee. He returned in 1930 and left again in 1936 to fulfill his dream of putting a small private-owner airplane into commercial design. Charles Zimmerman, a 1928 University of Kansas graduate, left Langley in 1937 for a similar reason. After growing increasingly devoted to a "flying wing" concept, he moved from the NACA to Chance Vought. (He returned to Langley in 1948.) Both Zimmerman and Weick had worked on their airplane concepts while at Langley, but could not bring their plans to fruition there.[32]

As serious a problem as the turnover of employees and their transplantation within industry and academia was in Langley's early years, it also had some real advantages. Qualified researchers who remained at the lab advanced more quickly when their superiors left. Richard V. Rhode, for

While employed at Hamilton, veteran NACA engineer Fred Weick made a series of propeller tests with Charles Lindbergh. (This was at Hamilton's west coast factory in Glendale, California, where Lockheed also had a plant.) In early 1930, Lindbergh was pruning his new Lockheed Sirius for an attempt to break the cross-country record from Los Angeles to New York. He wondered whether Weick might find him a propeller that would give his plane a little more speed. After learning what propeller the Sirius had, Weick informed Lindbergh that the most he could hope for would be an increase of about one mile per hour, and that to make the tests accurately would probably take a number of flights. Lindbergh surprised Weick by deciding that the one mile per hour was worth going after. For three days the two aeronautical pioneers flew the Sirius through a series of runs along a speed course that Weick had laid out along a railroad track between Burbank and Van Nuys. The results were quite accurate, but they were a great disappointment to Lindbergh. In earlier speed trials, which had not been carried out with Weick's painstaking accuracy, the Sirius had supposedly attained 177 MPH. The speed obtained in Weick's tests was eight miles per hour less—only 169 MPH. In the final run, with the best propeller and optimum pitch setting, Lindbergh's new plane did reach 170 MPH. From 169 to 170 MPH— this was an increase of one mile per hour, just as Weick had predicted.

In the photo to the left, Weick is the man to the left with hands on hips; Lindbergh is to the right. In the photo to the right Weick is in the rear cockpit, Lindbergh is in the front, and Tom Hamilton is standing.

instance, fell heir to the PW-9 flight loads project—which was in its planning stages between 1926 and 1929—because of key resignations in the flight research section.[33] And though the personnel losses may have retarded the successful execution of a few NACA research projects, the larger American aeronautics effort—the raison d'être of the NACA—probably benefited from them. Langley provided a training ground for some dozens of aeronautical experts at a time when American universities were not stimulating their growth and development. An apprenticeship at Langley seems to have been excellent preparation for other jobs in the design and manufacturing of aircraft and the teaching of aerodynamic principles. Conversely, and probably more importantly, understanding and appreciation of NACA goals and working procedures by former employees definitely facilitated closer contact among the various organizations concerned with aeronautics.

The career of Elliott G. Reid provides an example of this important liaison. Reid began working at Langley in July 1922, one month after graduating from the University of Michigan's aeronautical engineering program. By 1925 he was in charge of research in the Atmospheric Wind Tunnel section. In August 1927 he resigned his Langley post to teach aerodynamics at Stanford University. While teaching at Stanford, Reid maintained a close and cordial relationship with his old friends in the NACA. He and Prof. Everett P. Lesley cooperated on propeller research under contract to the Committee (as well as to the Army Air Corps and the navy's Bureau of Aeronautics). Reid had married a Virginia woman while at Langley and sometimes called at the lab during occasional visits to his wife's family farm. Though he never actually recruited for the NACA, Reid encouraged his students to consider research for the Committee as a career. Numerous Stanford-educated engineers, in fact, went to work at Langley in the 1930s and at NACA Ames lab in the 1940s.[*]

The importance of this liaison can be seen at its highest political level in the career of Edward P. Warner, whose early service with the NACA—he was Langley's first chief physicist—surely had an impact on his later dealings with the Committee. After resigning from the NACA in June 1920, Warner became aeronautics professor at MIT, assistant secretary of the navy in charge of aviation, editor of the journal *Aviation,* adviser to the

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[*] At Langley, this group included H. Julian "Harvey" Allen, Carl Babberger, Ogden W. Bodenheimer, Ralph B. Miller, John F. Parsons, Warren D. Reed, Russell G. Robinson, Francis M. Rogallo, and John B. Wheatley. At Ames, it included George B. McCullough, Henry Jessen, Charles W. Frick, Jr., Ralph F. Hunsberger, and Walter G. Vincenti. On the origins of the Stanford propeller research for the NACA, see Vincenti's "The Air-Propeller Tests of W. F. Durand and E. P. Lesley: A Case Study in Technological Method," *Technology and Culture* 20 (Oct. 1979): 712–51.

Civil Aeronautics Administration, and a leading member of the influential NACA Aerodynamics Committee. Though Warner's role in helping the NACA politically has not been studied thoroughly enough to make definitive conclusions, it is clear that he often used his strong voice to promote NACA research. In the late 1930s, for example, as head of a group responsible for writing the specifications for the Douglas DC-4 transport plane, Warner asked specifically that Langley provide the basic data on stability and control.

Two external factors in the late 1920s and early 1930s helped to ease Langley's major personnel problems: (1) Guggenheim's philanthropic support of aeronautical education at various American universities from 1926 on increased the quantity and improved the quality of the manpower supply, and (2) the Wall Street crash of 1929 brought on the collapse of several of those aircraft manufacturers that were out-competing the NACA for trained manpower. During the Depression that followed, the NACA was better able to select and retain qualified researchers, even when it had to give a few of its college-educated employees nonprofessional ratings and the majority of its veterans minimum professional pay.[34] Langley researcher John V. Becker recalls that upon his graduation from New York University in 1936 his first job offer came from Grumman: $25 a week in the company shop. Becker opted for the NACA, better pay ($38.50 a week), and a chance to work with unique research equipment.[35] As a result of a number of such decisions, the Depression became the golden age of NACA recruiting. In consequence, a larger, better trained, and more stable research staff at the Hampton installation performed aerodynamic research in many ways superior in quality to the NACA product of the earlier decade.

Specialization and Innovation

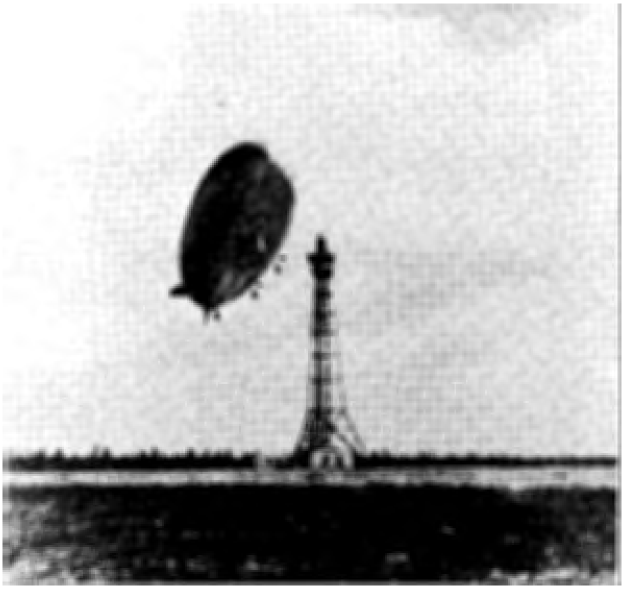
A common predicament among researchers is not knowing in advance whether general knowledge or specific knowledge will prove most valuable in the process of discovery. Faced with this dilemma, many people decide that it is better to look far and wide in pursuit of solutions and new knowledge than it is to focus exclusively on one given object. An individual can become a pure specialist, after all, by staying confined to a chosen field of ignorance.

For the most part, Langley would avoid excessive specialization and succeed as a research institution because it did so. NACA managers as a group must have felt that it would be unwise for Langley to specialize: Since a succession of unpredictable new problems more diverse than those already existing at the end of World War I could be expected to emerge from the embryonic field of aeronautics, practical solutions would likely require the effective assimilation and combined use of many different kinds of scientific and technological knowledge, both specific and general. In any case, the fact of the matter for NACA managers was that Langley had too few personnel to disperse among many specialized duties.

Before its tremendous expansion during World War II, the NACA's staff was large enough to specialize only where the promised rewards were substantial. In the 1920s and 1930s the biggest payoff was in refining the aerodynamics of the airplane, and that quickly became the job that Langley excelled in. Aerodynamics as practiced at the laboratory during the interwar years (and later) was not limited to the usual fields of that discipline, however. Besides the study of the fundamentals of fluid flow, wings, bodies, and propellers, aerodynamics also included a great deal of work in hydrodynamics, meteorology, instrumentation (electrical and otherwise), research equipment and techniques, and, most importantly, in propulsion (e.g., engine cowlings, engine cooling, nacelle placement, air intakes and exits, fuels, friction and lubrication, and noise) and in structures (e.g., loads, vibration, and flutter).

Within the fields of aerodynamics—loosely defined, as above—attention to special subjects waxed and waned. In the 1920s and early 1930s, for example, Langley conducted extensive experimental and theoretical work on lighter-than-air (LTA) craft. The army had assigned its 19th Airship Squadron to Langley Field at the end of World War I. From 1922 on, this outfit was stationed in a large hangar located on the northwest side of the airplane runway. NACA flight personnel assisted the squadron with speed and deceleration runs for several classes of army airships and helped to determine improved takeoff, landing, and docking procedures. The NACA also detailed Langley personnel to assist in the flight trials of the navy's lighter-than-air craft.[36]

As a result of this practical "hands-on" experience, many Langley flight researchers became outspoken advocates of airships. It was not clear at all to them or to anyone else at the time that the airplane would win out over the airship, let alone as totally as it soon did. Airplanes of the early 1920s were slow and small—an aerodynamicist who favored airships over airplanes even went to the bother of "proving" that airplanes larger than those of the day could never be built. LTA advocates believed correctly that airships had enormous unproven capabilities: they were not much slower and could carry many more passengers in far greater comfort than airplanes, most of which still had open cockpits; they were much more forgiving than airplanes during instrument flight; and with their extreme range and low operating cost, they could be used not just as military weapons but also for transportation of heavy commercial and industrial loads.



A camera obscura situated on top of a platform at the edge of the flying field measures the turning radius of the navy dirigible U.S.S. Los Angeles *in 1928.*

Despite these capabilities, the age of the airship ended on 6 May 1937, the day of the *Hindenburg* disaster. The gaseous explosion of Germany's greatest zeppelin killed 36 people—of whom 13 were passengers, the only passengers ever lost in 20 years of commercial travel by airship—and the tragedy became one of the greatest news events of its time. Stark public memory of the big dirigible going down in flames at its mooring at Lakehurst, New Jersey, and of the extraordinarily emotional live reporting of an eyewitness radio announcer, guaranteed the death of LTA flight as the losses of the *Roma, Shenandoah, Akron,* and *Macon* had not.[37]

Though some men at Langley remained interested in solving the problems of LTA flight even after the *Hindenburg* disaster, the NACA knew that further advocacy of comprehensive LTA flight studies would be politically foolish. Langley did use airship models for a brief time in association with a Propeller Research Tunnel program designed to explore improving the drag and propulsive efficiency of aircraft through boundary-layer control.[38] But after this work was completed in 1938, Langley carried out no more research relating to airships. Researchers who had specialized in LTA studies quickly translated their backlog of particular skills and experience to the study of airplanes. This translation happened rather easily, because those who had been most involved in airship research had been forced by the pressures of the busy NACA agenda to remain active all the while in more general aerodynamic testing.

A 1/40th-scale model of the navy airship U.S.S. Akron *being prepared for aerodynamic testing on a ground board at zero degrees of yaw in the Full-Scale Tunnel in 1935.*

The NACA's involvement in the airplane-airship competition contributed more to its understanding of aerodynamics than most people today can imagine. Airship design leaned more heavily on aerodynamic theory than had airplane design because there was little empirical knowledge of airships, since few had been built. Larger and more expensive than airplanes, completed airship structures could not be modified for experimental variations as readily; hence flight testing was extremely limited. At the same time, wind tunnel tests of airships had been less persuasive than of airplanes because of the relatively greater difficulties caused by scale effects.

The history of the NACA's attention to airships demonstrates that there can be a wonderfully productive cross-flow between disciplinary specialties which only the enthusiast or visionary can anticipate. In 1936 Max M. Munk, who had been a technical adviser at NACA headquarters (1921–25) and chief physicist at Langley (1926–27), predicted that

> since airship design draws on the whole domain of aerodynamics and since special airship aerodynamics should contain as its most notable problem the full analysis of airship drag, it seems quite possible that from airship theory may some day come forward such fundamental progress as shall revolutionize our technique of air travel.[39]

In a way, Munk's intuition proved correct: airship theory became extremely valuable when NACA researchers like Robert T. Jones began to extend airfoil theory to the near-sonic and supersonic speed ranges. In 1945 Jones used as the basis for a new slender-wing theory a linear theory formulated by Munk in 1924 for the approximation of specified forces acting on airship hulls. Jones's approximation avoided severe mathematical difficulties in determining the lift distribution of wings—difficulties involving, among other things, the solution of an equation containing a double integral. Near the end of World War II, Jones recognized the indirect value of such a theory for the design of delta (triangular) and swept wings; soon after the NACA's publication of his theory, so did many others. (See chapter 10.)

The NACA mobilized its staff for a special research effort, generally speaking, in one of two ways: either by adding a new unit to the formal organization, or by fostering an unofficial shadow organization that operated perpendicularly to the formal organization's mainly vertical lines of organization. "Small teams or task groups would be set up in these cases, relieved of their normal duties and exempted from normal lines of authority, burdens of paperwork, etc.—that is, freed from the restraints of the large parent organization, while taking advantage of its services and facilities whenever possible."[40] Laboratory management usually chose between the formal and informal response more instinctively than consciously.

The Product of Environment

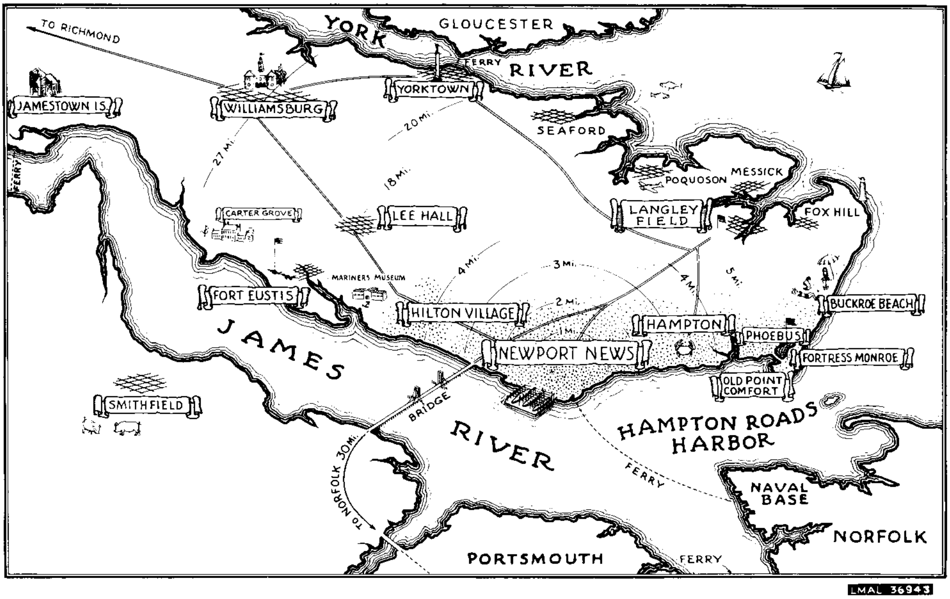
One might have thought that the cosmopolitan character of modern aeronautics called for locating America's research center in the industrialized Northeast, in the nation's capital, or perhaps on the campus of a major university. None of these had happened. The NACA had built its laboratory on flat plantation fields near Hampton, a sleepy and isolated small town on the southwestern shore of the Chesapeake Bay, in an area known to many as "Tidewater" and to a few as "the Asia Minor of Virginia." Airplanes circled over Langley Field, where crops of wheat and alfalfa had recently grown, while provincial watermen farmed oysters in the waters of Back River. The NACA installation, the adjacent army airfield, and the growth of Hampton Roads as a center of American naval power and shipbuilding during and after World War I combined to transform much of the antebellum character of the area. But the area had its effects upon the character of Langley as well.

As stated earlier, research professionals came to Langley largely from great distances beyond Hampton, from the northern states that possessed the major engineering schools. Many of the engineers who left Langley in its formative years resigned discontented, not with the NACA, but with Hampton. In the eyes of the newly arriving northern professionals, the community appeared a cultural backwater, an isolated place surrounded by large bodies of water on three sides and a wilderness of marshes and tall pines on the fourth. With the exception of regular steamboat lines on the Chesapeake Bay and a few river ferries, travel into or away from the secluded peninsula was difficult. Unlike today's citizens, Hamptonians then enjoyed neither a tunnel under Hampton Roads to Norfolk nor bridges spanning the wide James River at Newport News or the York River at Yorktown.

Langley Field itself rested at the northern frontier of Elizabeth City County as an island within an island. A few miles of farms and swampland separated the airfield from the small town. Only bad roads—some made from crushed oyster shells—linked laboratory to living room. The most attractive residential areas were the farthest from Langley, along the boulevards paralleling Hampton Roads, the James River, or Buckroe Beach on the Bay. The old families who lived in these neighborhoods, however, did not welcome newcomers from the North into their midst. One young Langley arrival, after failing to find a room-for-rent sign in a pleasant neighborhood, went to the door of a private home in order to ask advice of its owner. The homeowner, besides informing him that he knew of no rentals, growled that the newcomer was the first "Yankee" ever to come through that gate.[41]

Langley management was very aware of this and other housing problems. It had pleaded with the Army Air Service in the early 1920s to provide suitable on-base quarters, but nothing permanent or satisfactory was ever arranged. A number of the earliest employees slept on cots in the Research Laboratory Building. Several unmarried men herded together in boarding houses, while others slept at hotels. Some NACA recruits even turned down job appointments because they could not find suitable residences for themselves and their families in Hampton. Late in the decade the lab tried to influence some local businessmen to finance the building of new houses and apartments for its employees, and even took surveys on what rent its employees would be willing to pay, how many rooms and what kind of furnishings were required, etc.[42] This effort to motivate the local construction industry met with some success, but the problem of finding satisfactory housing remained severe for Langley employees into the 1950s.

The environment oppressed the newcomer in at least one other way. One of the aeronautical engineers from Michigan (Floyd Thompson) reported to work at Langley in the summer of 1926 in 98-degree temperature and high humidity. People told him that it was unseasonably hot, but the young man subsequently discovered that it was unseasonably hot there almost every year at that time.[43] What was worse, Prohibition was in effect! At first glance anyway, the Langley professional perceived local life as painfully provincial and unfulfilling.



LMAL map of the Hampton Roads area from the late 1930s. The James River bridge at Newport News was completed in the late 1920s. (The map is not drawn to scale.)

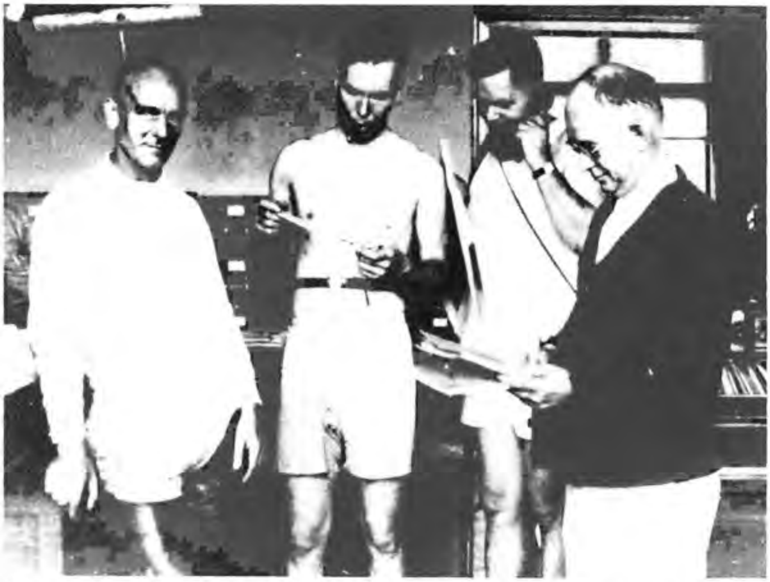
The professionals who stayed on adjusted to their new environment by learning to embellish and enjoy their distinction from the established Tidewater community. They formed an activities association, which sponsored a lunchroom at the lab and maintained a "Shore Camp" for fishing, picnicking, and other vacation activities, and they even created a fraternal social club known as "The Noble Order of the Green Cow."[44] Extracurricular camaraderie translated into an important esprit de corps during workdays. Some freethinkers and loners did not fit too well into this active brotherhood of living, working, and playing together, but the majority seem to have enjoyed it. The intensity of interaction between Langley personnel, distinct from established Hampton society, caused a real sense of family to develop. The feeling became so deeply rooted in the small NACA community that it flourished long after the original population had been assimilated into native life (largely through marriage to local girls). In 1976, 18 years after their parent body went out of existence, over 600 members of the NACA family celebrated a reunion. A larger number attended "Reunion II" in 1982. These conventions of former employees of a defunct agency are rare happenings in the social history of American government.

Langley's staff of young engineers wearing shorts to beat the summer heat of Tidewater in 1930. From left to right: Harvey Herring, Irvin Coates, Warren Weiss, Clindon Glass, W. M. Martin, Ray W. Hooker, W. K. Ritter, Eastman Jacobs, Robert Mixson, John Stack, George Hammeter, Joseph A. Shortal, Kenneth Ward, R. E. Tozier, C. D. Waldron, Charles H. Zimmerman, Gilbert Strailman, Melvin Gough, Everett Johnson, Elton W. Miller, Fred Schultz, Ira H. Abbott, and Addison Rothrock.

The intensity of interaction within what was a small professional complement, numbering only 44 in 1926, led in a few cases to a certain peculiar personality—the so-called "NACA Nut." This "acceptable eccentric" was a technical sophisticate par excellence, who wanted to know the RPM of his vacuum cleaner and asked that his lumber be cut to the sixteenth of the inch. In local lore, this person was dreaded by every hardware and automobile salesman in Hampton and nearby Newport News.

Though most locals regarded the NACA Nut with humor, they were not fond of certain other NACA types. In fact, the reluctance of the local people to accept the strangers may have resulted in part from the "smart-alecky behavior of some of the Langley professionals, who regarded themselves as intellectually superior to the natives." When asked to make a few remarks to the Hampton Rotary Club in 1923, engineer-in-charge Griffith, for example, used the opportunity to tell everyone exactly what he thought was wrong with the town.[45] Eventually Henry Reid, Griffith's successor, developed

The NACA staff held an annual Christmas party in the Langley boat house, December 1928.



NACA Nuts supervise cleanup operations after a 1933 hurricane. From left to right: Walter Reiser, chief of the Maintenance Division; Henry Reid, engineer-in-charge; Ray Sharp, chief clerk and property officer; and Elton Miller, chief of the Aerodynamics Division.

more solid rapport with the Hampton citizenry, largely by being active in community affairs and belonging to many local service organizations.

Those researchers who came to stay at the Hampton facility into the 1930s gradually realized that to work at the NACA laboratory was in fact to be at a cosmopolitan hub of world aeronautics. At Langley, the Committee brought together men from the best engineering schools and fostered their cooperation and intellectual cross-fertilization. (The number of women with professional grades at Langley before World War II can be counted on one hand, with fingers left over.) It gave its researchers a chance to work in the most advanced wind tunnels and supplied them with translations of the most important scientific and technical papers from around the world. The annual NACA-sponsored aircraft manufacturers' conference (see chapter 6) kept them in touch with leaders of the aircraft industry and gave them a regular chance to publicize their work. One engineer who worked at Langley in the 1930s later recalled that

> it wasn't a matter of NACA going out to find out what somebody else was doing. It was a matter of other people trying to find out what we were doing.[46]

The successful research programs at Langley in the late 1920s and 1930s, especially the systematic airfoil and cowling programs (described in chapters 4 and 5) enhanced the public reputation of the NACA and strengthened feelings of satisfaction with Hampton and self-sufficiency at the lab. Here, too, was a source for the family feeling at Langley.

Many Langley veterans say that the laboratory operation ultimately benefited much more than it suffered from physical isolation. Because of its distance from Washington and its strong sense of individual identity, "Langley did not think of itself as part of the federal bureaucracy." The lab thus kept paperwork to a minimum, staff meetings brief, program reviews relatively simple and straightforward, and attention focused on technical and scientific rather than political matters.[47] In other words, employees were better able to concentrate on their real work.

A major part of this real work, and much of the human drama that accompanied it, was carried out in the wind tunnel buildings.

3

The Variable-Density Wind Tunnel

Langley first built its reputation as an outstanding aeronautical research institution on the strength of the variable-density wind tunnel. Max M. Munk, the NACA's German aerodynamicist, proposed this unique and, in some respects, revolutionary piece of experimental equipment in 1921. Two years later Munk's so-called VDT went into operation at the lab. The test results it yielded were so superior to those obtained with any previous tunnel design, especially regarding wing performance, that they made the NACA a world leader in aerodynamic research for at least the next ten years. Aircraft companies, engineering schools, and even foreign research establishments, such as the National Physical Laboratory of Great Britain, sent crews to Langley to study the VDT and return home with ideas for building improved versions of it.

Considering this achievement, it is curious that the history of the VDT involves as much controversy as it does. There is the controversy over credit for inventing the tunnel: Was Munk the true father of the VDT concept, or was it the Russian Wladimir Margoulis, who in 1920 was working as an aerodynamical expert and translator for the NACA's Paris office? Even if Munk does deserve credit as the originator of the design concept, does credit for actually designing a feasible VDT rightfully go to Munk or to the engineering staff at Langley? There is also the controversial "revolt" against Munk at Langley, which, though secondary to the VDT achievement, is important for what it reveals of the Langley personality and for what it suggests about the intercultural transfer of technology. Also somewhat controversial in retrospect are the quality of the tunnel design and the quality of its test results. Was the VDT the total aerodynamic triumph trumpeted in the NACA brochures, or was it in fact riddled by shortcomings? Finally, at the end of VDT history, there is the matter of laminar-flow airfoils (which allowed drag to be reduced and speed to be increased; to be discussed in chapter 4). Was their practical achievement by Langley researchers a reality or a myth?

The Development of Wind Tunnel Technology

Many of the major developments in early aeronautics depended largely on findings achieved through intelligent use of research equipment. The laboratories that pioneered aeronautical research and development possessed a surprising panoply of tools, many primitive but a few highly sophisticated. They included whirling arms, dynamometer cars, water channels (also called towing tanks), engine test beds, as well as flying machines. In a given lab, various technical departments supported the work of this experimental equipment. One department might devise and calibrate pressure gauges, balances, recording dynamometers, chronographs, and the like, while another built and repaired test models. Mechanics tuned engines and maintained drive units. Photographers developed cameras to visualize airflow and techniques to measure aircraft movements in real and simulated flight. Successful research and development required careful planning and management of this intricate, expensive, and bedeviling equipment, with personnel organized into teams.

Within the diversity of facilities, the wind tunnel predominated. Francis Wenham built the first known tunnel at Greenwich, England, for the Aeronautical Society of Great Britain in 1871. The tunnel consisted of a steam-driven fan that blew air through a wooden box 12 feet long and 18 inches square, and open at both ends. All succeeding tunnels shared certain features of the Wenham design: a *drive system* turned a *fan* that produced a *controlled airstream,* the effects of which on a *scale model* mounted in a *test section* of the tunnel were precisely observed. *Balances* and other instruments measured the aerodynamic forces acting on the model and the model's reaction to them.[1] The progressive integration of improved versions of these wind tunnel components rendered all other experimental aerodynamic research tools, with the exception of full-scale experimental aircraft in free flight, secondary or obsolete by the end of World War I. Subsequent advances in aerodynamics have generally been closely linked to the course of tunnel development.

The physical law behind the wind tunnel was not fully understood until the late nineteenth century, though it had been deduced by da Vinci and refined quantitatively by Newton: a fluid flowing past a stationary object produces the same interactions as those that occur when the object moves through the fluid at rest.[2] For aeronautical researchers, this meant that flight conditions could be simulated by holding an aerodynamic surface stationary within an airstream moving at flight velocity. And a tunnel was the ideal place to conduct and observe such simulations. A tunnel was relatively versatile, safe, and economical. A full-size experimental airplane cost a great deal more money than a wind tunnel test model. Reconfiguring as testing progressed cost more at full size, too. And flying experimental planes could cost lives. Moreover, some test conditions could be measured and controlled more accurately on the ground than in flight, and some instruments could be mounted and read more easily, and lasted longer, in a tunnel.

Though the invention and early (pre–Kitty Hawk) evolution of tunnel technology provided vital knowledge of the forces affecting wing surfaces (specifically about the surface area required to support a given weight, as well as the surface's optimum shape), the wind tunnel's full potential was not entirely obvious in aviation's earliest days. Several developments led to recognition of its importance. First, the Wright brothers relied heavily on wind tunnel data to design, build, and fly the first powered manned airplane in 1903. Second, the electric power industry developed a cleaner and more compact motor to replace older steam-driven monstrosities powering wind tunnel fans. Third, between 1908 and 1915, German aerodynamicists at the University of Göttingen leapfrogged earlier designs when they built the first closed-circuit tunnel.

The real significance of the wind tunnel became gradually more apparent beginning with aviation's dramatic event of 1903. Legend depicts the Wright brothers as simple bicycle mechanics whose hard work led to success, but in truth engineering knowledge underpinned their flying achievements. After their 1901 glider tests revealed major inaccuracies in published aerodynamic data, the Wrights turned to the wind tunnel for reliable design information. (Only one tunnel seems to have operated in America before 1900—that at MIT, built to check drag measurements Samuel Langley had made with a whirling arm in his Washington lab.) In their Dayton shop, the Wrights first built a makeshift tunnel from an old starch box and later a more sophisticated wooden one (with a 16-square-inch test section). By testing the lift of each of nearly 200 airfoil models, they obtained much of the critical information needed to build the highly successful 1902 glider and its derivative, the landmark airplane of 1903. The wind tunnel had proved indispensable to the first successful powered flight.[3]

Electric power also contributed to the growth of the wind tunnel's importance. No wind tunnel before 1910 had more than 100 horsepower. Steam engines powered most of the early tunnel drive systems, at relatively low speeds. After the turn of the century, however, electric motors powered more and more of the tunnels at faster and faster speeds. The first tunnel fan driven by electricity in the United States was most likely Albert Zahm's at Catholic University in 1901. Zahm's later 8 × 8-foot tunnel at the Washington Navy Yard attained airspeeds in 1913 of up to 160 miles per hour, equivalent to the diving speed of World War I military aircraft.[4] Cheap and increasingly available, electricity permitted precise adjustment of tunnel speed and reliable performance at higher horsepower in a quieter and cleaner environment. (The availability of electric power was to become a very important factor in the planning and operation of wind tunnels at Langley, especially during the facilities boom of the World War II era.)

Nearly all of the pre–World War I wind tunnels, starting with Wenham's and including the Wrights', had open circuits; that is, they drew air into the test passage directly from the atmosphere and released it back into the environment. The classic examples of the non-return, open-circuit tunnel are those Gustave Eiffel (1832–1923) built in and around Paris in the early 1900s. His 1.5-meter-diameter tunnel at Champs de Mars, completed in 1909, sucked air through a test section at 20 meters per second (roughly 45 miles per hour). Eiffel's later tunnel at Auteuil, built in 1911 and 1912, improved the design. Producing an airspeed of 32 meters per second (roughly 72 miles per hour), it was the last great open-circuit design of the era.[5] (Open-circuit tunnels are still used today, for special purposes.)

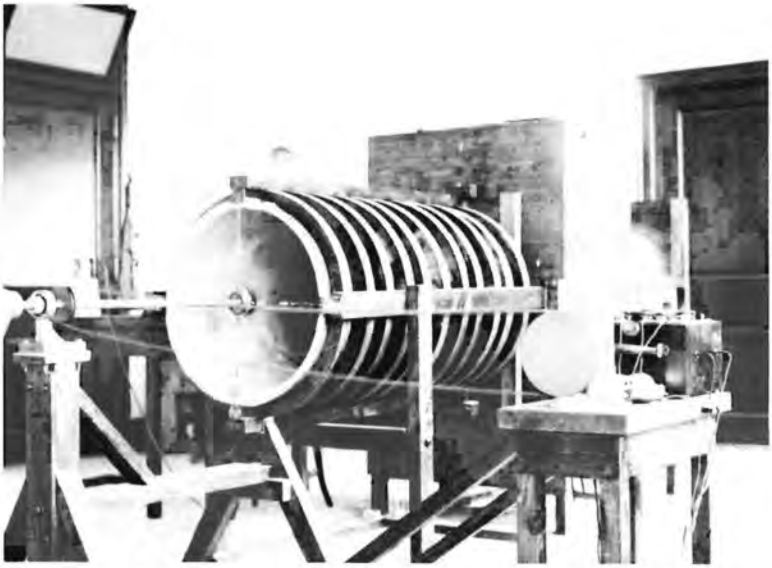
The aerodynamics research staff of the great German physicist-engineer Ludwig Prandtl (1875–1953) changed the direction of tunnel development in 1908, when it finished the first continuous-circuit, return-flow machine at the University of Göttingen. This new tunnel had three inherent advantages over open circuits: first, it reduced power requirements (through partial recovery of the kinetic energy of the air leaving the diffuser); second, by incorporating improved screens and honeycombs, it produced and maintained airflow that was much more uniform than that in open circuits; and third, it permitted pressurization and humidity control. The primary problem peculiar to the closed circuit—turning the airflow 360 degrees—was solved by introducing efficient turning vanes. A settling chamber upstream of the test area in Göttingen's second-generation closed-circuit tunnel, completed in 1916, further dampened airstream turbulence, and a contraction cone at the test section entrance further increased its velocity.[6] Thus, the new closed-circuit tunnels produced faster, smoother, drier, and more reliable airflow than any tunnel had produced before.

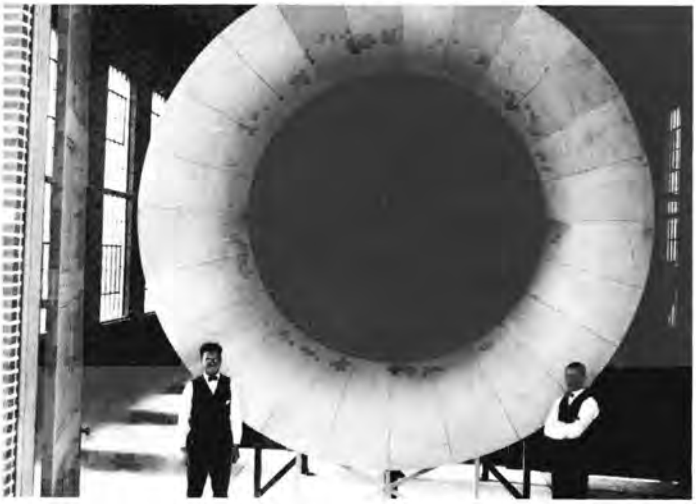
NACA Wind Tunnel No. 1

By the time Langley laboratory came to life in 1920, the closed-circuit tunnel had proved its superiority over the open-circuit type. But the NACA, cautious because its original staff had so little wind tunnel experience, still chose to design its first tunnel with an open circuit. It patterned the design after that of a successful tunnel which had been in operation for some time at the British National Physical Laboratory. The leaders of the Committee apparently felt that it was better to improve the NPL tunnel design and to get some immediate firsthand operating experience with the proven machine than it was for novices in the field to proceed boldly with the creation of newer experimental technology. In fact, NACA engineering personnel were so inexperienced that they were told to construct and operate a one-fifth-scale model of the English tunnel before going ahead with the design of the actual facility.

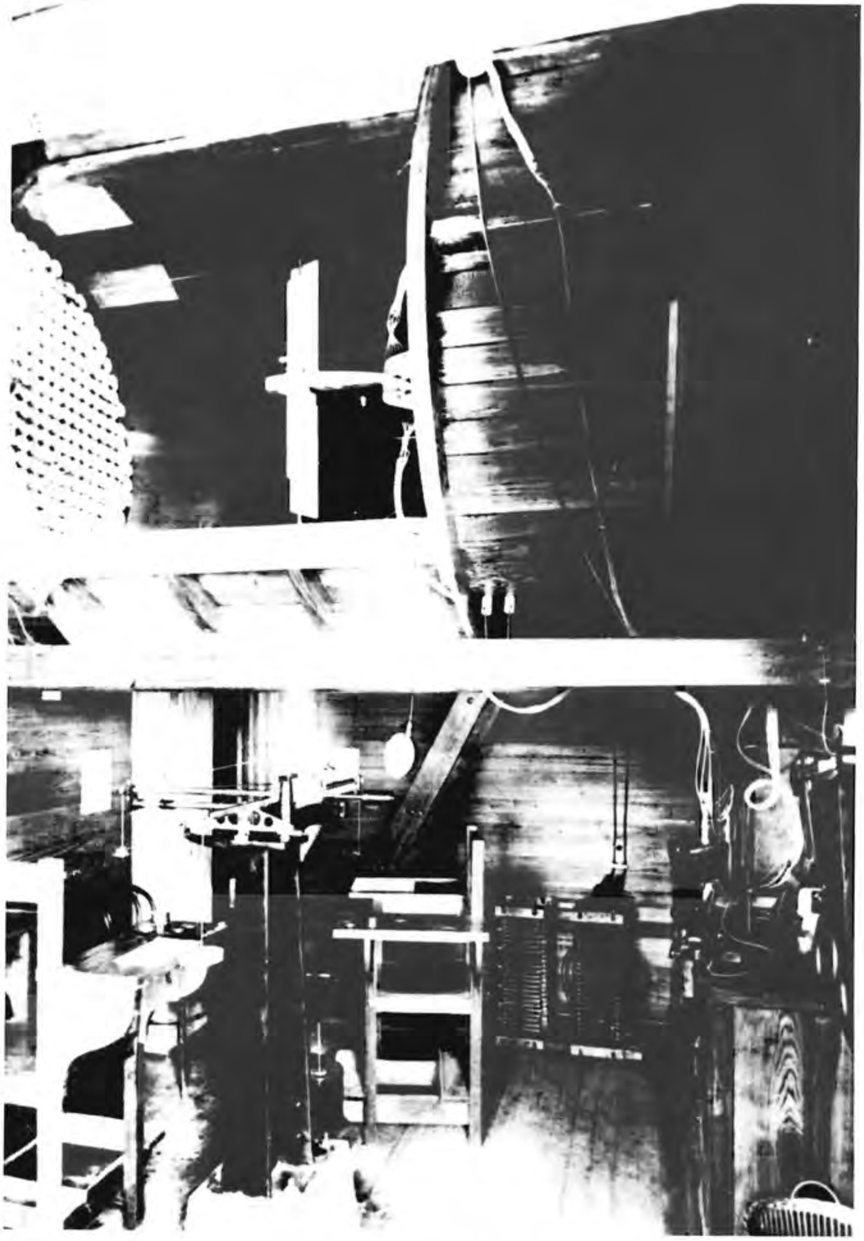
In the fall of 1920, very soon after completing the tunnel that resulted from experience with this model, Langley researchers discovered that results from tests in Tunnel No. 1 could not really be applied to the performance of full-size airplanes. Because the circular test section of the new facility was only five feet in diameter, it was impracticable to use models wider than three and a half feet, or about one-twentieth scale. NACA engineers and other informed aerodynamicists knew how to convert or "scale up" data determined from airflow over such a small object, but systematic testing now made it clear to them that the empirically derived factor customarily used to approximate full scale was largely unreliable.

The problem concerned *Reynolds number.* In the 1880s, Osborne Reynolds (1842–1912) of the University of Manchester had identified this crucial scaling parameter. In a classic set of experiments dealing with the flow characteristics of water through pipes, Reynolds had demonstrated that the responses of an object to that flow depended on the object's size, the speed with which it (or the water) was moving, the density of the water, and the viscosity of the water. He concluded from a mathematical study of the relationship between the flow patterns over a scale model and those patterns over the same shape at actual size that if in both cases a certain flow parameter (the ratio $\rho V d/\mu$, where ρ = density, V = velocity, d = diameter, and μ = fluid viscosity) was the same, the flow pattern in both cases would also be the same. Understanding and using this ratio, known thereafter as Reynolds number, soon became vital to wind tunnel work





Langley's first wind tunnel was a modest open-circuit device which, by the time its construction began in 1919, had been made virtually obsolete by Germany's closed-circuit tunnels. The NACA built a one-fifth-scale model tunnel (top) to give LMAL researchers some design and operating experience before moving on to the real thing. In the bottom photograph, two LMAL mechanics pose near the entrance end of the actual tunnel, where air was pulled into the test section through a honeycomb arrangement to smoothen the flow.



The 5-foot-diameter circular test section and control room of NACA Tunnel No. 1.

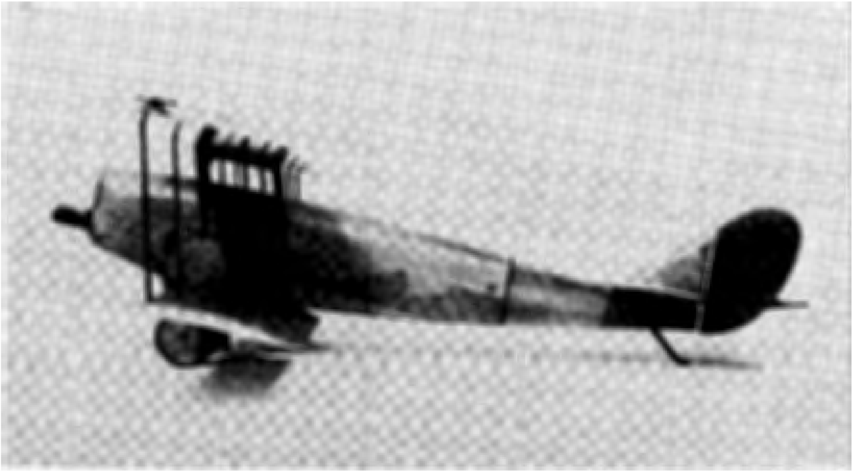
because it provided a rational basis for extrapolating experimental data from scale-model testing. The closer a tunnel's airflow came to producing the value of the full-scale Reynolds number, the closer its test measurements came to indicating the aerodynamic forces of actual flight.[7]

In the older form of atmospheric wind tunnel the Reynolds number usually amounted to only about one-tenth that of actual flight. This limitation was critical in the aerodynamic region known as *maximum lift,* which determines landing speed, and equally critical in the region near *zero lift,* or *minimum drag,* which determines maximum speed. (According to some aeronautical engineers, minimum drag is "mostly fictional" and thus strongly dependent on Reynolds number.)[8] Since NACA Tunnel No. 1 was a low-speed facility which necessarily involved one-twentieth-scale models, the Reynolds numbers of its tests were recognized as being too low by a factor of 20 for comparison with flight performance of the actual aircraft. Though the researchers at Langley knew that it was possible theoretically to increase Reynolds number in their tests by increasing model size, increasing the speed of the airflow, or by increasing the density or decreasing the viscosity of the air, none of these alternatives seemed feasible given the nature of the existing facility.

In 1921 Max Munk, working as a technical assistant in the NACA's Washington office, suggested to the Committee that experimental results comparable to full-scale flying conditions might be realized in a sealed airtight chamber, the air in which would be compressed "to the same extent as the model being tested." His basic idea was simply to achieve higher Reynolds numbers approximating the flight values of contemporary aircraft by using denser air. Specifically, he proposed immediate construction at the LMAL of a variable-density tunnel. This facility, Munk argued, would compensate for the small size of the one-twentieth-scale models by increasing the density of the air in the tunnel up to 20 atmospheres. Though the chief physicist at Langley argued that through flight research his staff could obtain airfoil data at high Reynolds numbers without this expensive new facility, the NACA Executive Committee authorized construction of Munk's compressed-air tunnel in March 1921.[9]

Max Munk

By the time he arrived in 1920 at the port of Boston from his native Germany, en route to Washington, D.C., to confirm his appointment as technical assistant to the NACA, Max M. Munk, just 30 years old, was already a prominent aeronautical engineer. His aptitude in mathematics and the sciences was such that Munk as a young teenager had convinced



A wooden 1/20th-scale model of a Curtiss Jenny for tunnel testing.

A Curtiss Jenny in flight with trailing Pitot-static tube for airspeed calibration, August 1922.

his parents—lower-middle-class Jews from the worldly old Hansa city of Hamburg—that he should leave rabbinical school for German academe. In 1914 he earned an engineering diploma at the Hanover Polytechnical School (where to sound more Germanic he started to use his middle name Max in place of his first name Michael) and in 1917 two doctorates at the University of Göttingen, one in engineering and one in physics.

At the university he had been one of Ludwig Prandtl's most gifted students, assisting Prandtl in his effort to achieve higher Reynolds numbers by using oversized models in the new closed-circuit tunnel. During World War I, a significant number of Munk's analyses of wind tunnel experiments appeared as secret military reports. "Nevertheless," according to Munk, "they were translated in England a week after appearance and distributed there and in the U.S." In his doctoral thesis, "Isoperimetrische Probleme aus der Theorie des Fluges," Munk used shrewd intuitive mathematics to solve the problem of how to make the *induced drag* of a wing (a concept originated by Munk) as small as possible. (He showed that the *minimum induced drag* of an airfoil was obtained mathematically if the distribution of the lift over the span corresponded to an ellipse.) At the end of the war, he worked a short time for the German navy and then became an employee of the airship manufacturing company Luftschiffbau Zeppelin, where he designed a small atmospheric wind tunnel and proposed the design of a much larger (1000 horsepower) one for the testing of large airship models. This incredible facility was never built, but according to Munk's plan, would have produced a Reynolds number equivalent to the flight conditions of a full-size airship by having a 152-kilometer-per-hour (nearly 100 miles per hour) closed-circuit airflow pressurized to 100 atmospheres.[10] An airflow of this speed under such high pressure would have produced a Reynolds number much higher than that produced by any other wind tunnel at that time.

Leaders of the NACA were greatly impressed with what they thought to be the scientific orientation of European aeronautical researchers like Munk and of their parent organizations. Joseph Ames, professor of physics at Johns Hopkins University and chairman of the NACA Executive Committee from 1920 to 1937, wrote in January 1922 that

> aeronautics is no sense a function of an engineer or constructor or aviator; it is a branch of pure science. Those countries have developed the best airships and airplanes which have devoted the most thought, time, and money to scientific studies.[11]

Future NACA member Jerome C. Hunsaker had spent a few weeks in 1913 at Prandtl's Göttingen laboratory as a representative of the U.S. Navy while touring several major European aerodynamic labs. On his return he reported his special admiration for this particular German research organization, where a steady stream of promising young doctoral candidates under an accomplished academic mentor provided the lifeblood of the research effort.[12] Thus after the war and despite its residual ill will, the NACA generally and Hunsaker specifically would be predisposed to listen closely to any request by one of these young aeronautical scientists for employment. According to Munk's own version of his 1920 migration from Germany to the United States, Prandtl had contacted Hunsaker soon after the end of the war about a job for Munk. (Munk was interested in going to America partly because a distant uncle had made a fortune in mining here.)[13] Hunsaker informed Ames of Munk's interest and availability, and Ames persuaded the rest of the Committee, which was then hard pressed for talented aerodynamicists (Edward P. Warner having just resigned as Langley's chief physicist), to offer Munk a position as technical assistant. Munk's employment required two orders from President Woodrow Wilson: one to get a former enemy into the country, the other to get him a job in government.[14] (At the end of the next world war, another special arrangement would bring the German rocket specialists led by Wernher von Braun to work for the American government as part of "Operation Paperclip.")[15]

For six years Munk was stationed in Washington, where he worked mostly on theoretical problems. He contributed theories of flow around airships, and of moments and positions of center of pressure on other aerodynamic shapes. He introduced a significant advance in airfoil theory, in the form of a linearization that permitted the calculation of certain airfoil characteristics in terms of easily identified parameters of the profile. During the six-year period the NACA published over 40 of Munk's papers. His contributions were considered so outstanding by the Committee that in 1925 it published a paper (TR 413) by Joseph Ames entitled "A Résumé of the Advances in Theoretical Aerodynamics Made by Max M. Munk."

Simultaneous Discovery

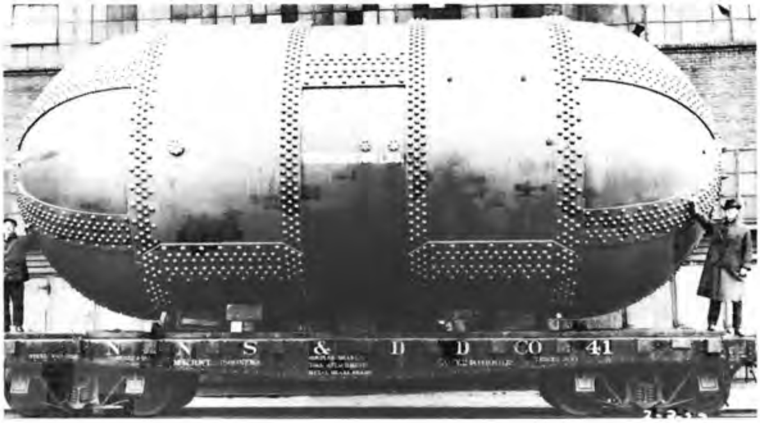
Munk had arrived at his idea for pressurizing air to increase the Reynolds number in wind tunnel experiments at just about the same time that Russian-born Wladimir Margoulis (former collaborator of aerodynamicist Nikolai E. Joukowski) considered the feasibility of a closed-circuit wind tunnel using carbon dioxide as the test medium. Though the ideas of Munk

Munk became an employee of the NACA principally through the efforts of Jerome C. Hunsaker (left), a future chairman of the NACA (1941–1956) who was, at the time of Munk's immigration, chief of design in the navy's new Bureau of Aeronautics, and of Joseph S. Ames (right), executive chairman (1920–1937) and later chairman (1927–1939) of the NACA, seen here, in about 1920, at his desk at NACA headquarters.

and Margoulis were elaborated in different ways, their basic concept was the same—that dynamical similarity between scale models and full-size prototypes could be achieved by using a fluid that had a lower density/viscosity ratio (the ρ/μ term in the Reynolds number). The virtual simultaneity of the two men's thinking has, since the 1920s, prompted some people to question the priority: Who had the idea first, Munk or Margoulis?

Margoulis first proposed using carbon dioxide for wind tunnel testing in his paper "Nouvelle méthode d'essai de modeles en souffleries aérodynamiques," which appeared in the *Comptes rendus de l'Académie des Sciences, Paris* in November 1920. Five months later, the NACA published Margoulis's own English translation of his paper as Technical Note (TN) 52, under the title "A New Method of Testing Models in Wind Tunnels." Munk proposed his idea for a compressed-air tunnel in NACA Technical Note 60, "On a New Type of Wind Tunnel," which appeared in June 1921. Thus, it appears that the first published proposal to increase Reynolds number in wind tunnel experiments by using a fluid of low kinematic viscosity came from Margoulis. On the other hand, Munk had proposed for Zeppelin the design of a pressurized tunnel even before 1920.

The Munk-Margoulis priority issue was not energetically debated in aeronautical circles until the British began to design their own variable-density tunnel at the National Physical Laboratory in the late 1920s;



The tank for the Variable-Density Tunnel arrived at Langley by railway from its manufacturer, the Newport News Shipbuilding & Dry Dock Company, Newport News, Virginia, in February 1922.

Max M. Munk inspects the Variable-Density Tunnel, summer 1922.

then the British-American competition for the lion's share of credit for developing the tunnel concept began. In the twentieth Wilbur Wright Memorial Lecture, delivered in England in May 1932, H. E. Wimperis, vice-president of the Royal Aeronautical Society, claimed that British engineers had extrapolated the variable-density tunnel idea from Margoulis's paper and had put forward a considered design for a compressed-air tunnel before hearing a word about the NACA design suggested by Munk. Spokesmen for the American aeronautical research establishment disputed this British claim. Walter S. Diehl of the navy's Bureau of Aeronautics, for example, wrote: "While it is quite natural for Mr. Wimperis to argue in favor of the British equipment, I get the impression from his lecture that there is a lot of 'sour-grape' background and that he is being unfair to the National Advisory Committee for Aeronautics in his statements and comparisons." In Diehl's mind, there was "no doubt whatever ... that Munk originated the idea" and that the British were trying to steal the credit for Margoulis and themselves.[16]

The Method of Airfoil Research

Though all manner of aerodynamic studies were attempted in the VDT, the facility's primary purpose was to test airfoils. Wing design was one of the most important aeronautical research problems facing NACA Langley in its early years. From the time that Sir George Cayley (1773–1857) had identified the inclined plane as "the true principle of aerial navigation by mechanical means" in the 1830s, aerodynamicists had tried in earnest to know better the complex flow phenomena through which the airfoil generates the lift necessary for flight. In the eight decades of sporadic aeronautical development between Cayley's major work and the establishment of the NACA, they had tried everything from crude cut-and-try to rather sophisticated experiments. All of the successful methods of wing design had been empirical. Cayley had feared that the whole subject of aeronautics was "of so dark a nature" that it could be more usefully investigated by experiment than by theoretical reasoning; thus he had tested various airfoil shapes on the end of a whirling arm. In 1879 the Aeronautical Society of Great Britain had reinforced this commitment to empiricism, opining that mathematics had been "quite useless to us in regard to flying." One of the Society's most prominent members, Horatio Phillips (1845–1924), had conducted primitive wind tunnel tests "of every conceivable [wing] form and combination of forms."[17] The Wright brothers had later used a rough version of experimental parameter variation to determine how much lift and drag could be expected from various wing sections. (Parameter variation has been described as "the procedure of repeatedly determining the performance of some material, process, or device while systematically varying the parameters that define the object or its conditions of operation"; see chapter 5.)[18] During World War I, European research teams at the NPL in England, the Eiffel Institute in France, and Prandtl's laboratory in Germany had refined this method. Their five or six best shapes, plus close derivatives, provided nearly every wing section in use at the end of the conflict.

Ironically, the empirical method had been providing designers with some basic misinformation about wings. Since the tests were made at the low Reynolds numbers then available in the small atmospheric wind tunnels, thin, highly cambered (arched) wing sections seemed to have the most favorable properties. At low Reynolds numbers, airflow over thick sections "separated" early and resulted in unsatisfactory performance.* Furthermore, the Wrights had achieved their successful flight in 1903 with a long, slender airfoil. Convinced that the longest span with the thinnest sections generated the greatest lift, some German designers of propellers even went so far as to make their blades from mere fabric stretched by centrifugal force. Nearly all World War I aircraft, with the important exceptions of some advanced aircraft designed by Junkers and Fokker, employed extremely thin wings requiring for external strength and rigidity a messy conglomeration of wires, struts, and cables.[19]

In its first *Annual Report* to Congress in 1915, the NACA called for "the evaluation of more efficient wing sections of practical form, embodying suitable dimensions for an economical structure, with moderate travel of the center of pressure and still affording a large angle of attack combined with efficient action." The Committee could not carry out this work itself, of course, because Langley laboratory was at that time no more than a dream. The best the NACA could do toward improving wing design was to support wind tunnel tests at MIT, which were under the auspices of the airplane engineering department of the Bureau of Aircraft Production. This experimental program resulted by 1918 in the introduction of the U.S.A. series, the largest single group of related airfoils developed in America up to that time.[20]

The NACA supplemented its support of the MIT wind tunnel program with a laborious effort by its small technical staff in Washington to bring

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* "At small angles of attack the flow has little difficulty in following the surface. As the angle is increased, however, the air finds it increasingly difficult to maintain contact, especially on the upper surface, where it has to work its way against increasing pressure, and it separates from the surface before reaching the trailing edge." Theodore von Kármán, *Aerodynamics* (Ithaca, N.Y., 1954), pp. 46–47.

together the results of airfoil investigations at the European laboratories. In June 1919 the Committee opened an intelligence office in Paris to collect, exchange, translate, and abstract reports, and miscellaneous technical and scientific information relating to aeronautics. Then, through its Committee on Publication and Intelligence, the NACA planned to distribute this information within the United States.[21]

One of the early fruits of this labor was NACA Technical Report (TR) 93, "Aerodynamic Characteristics of Airfoils," a comprehensive and handy digest of standardized test information about all the different airfoils employed by the Allied powers. The report, published in the NACA *Annual Report* of 1920, offered graphic illustrations of the detailed shapes and performance characteristics of over 200 airfoils, as well as four index charts that classified the wings according to aerodynamic and structural properties. The intention was to make it easier for an American designer to pick out a wing section suited to the particular flying machine on which he was working. In retrospect it is plain that many of the plots were totally unreasonable—no doubt because the NACA personnel who interpreted the collected data, like those who made the original tests, did not really understand how and why certain shapes influenced section characteristics as they did. Despite the flaws, however, the effort that went into the preparation of this report and others like it mobilized the NACA staff to manage a solid program of airfoil experiments once research facilities were ready at Langley.[22]

When the LMAL began routine operation in June 1920, the empirical approach was by far the most sensible way to better wings. Wing section theory, as developed before World War I by Europeans Martin W. Kutta (1867–1914) and Nikolai E. Joukowski (or Zhukovski, 1847-1921, director of the Eiffel laboratory during World War I and consultant to the NACA's Paris office after the war), permitted the rough determination of lift-curve slopes and pitching moments, but little else. It was possible to transform from the pressure distribution around a circle, which was known theoretically, to the flow distribution usually measured around an airfoil, and thus create an approximate airfoil shape, but the mathematics required for the transformation was too abstruse for the average engineer. Further, there was no way to measure the practical value of the mathematical formulations other than via systematic wind tunnel testing. Prandtl had refined the Kutta-Joukowski method, but his refinement still allowed only for the rough calculation of wing section characteristics.[23]

Some of the most popular airfoils of the 1920s were produced by highly intuitive methods—cut-and-try procedures based neither on theory nor on systematic experimentation. For the wing section of his successful seaplane, Grover Loening took the top curvature of the Royal Air Force's number 15 wing section and for the underside drew a streamlined curve with a reverse in the center, which enclosed the spars. The net result of this cut-and-try method was so good that Loening, who did not want other people to copy his product, decided not to submit the wing for tests anywhere. Col. Virginius Clark, USA, designed one of the 1920s' most popular airfoils for wings, the Clark Y, simply by deploying the thickness distribution of a Göttingen airfoil above a flat undersurface; he chose the flat feature only because it was highly desirable as a reference surface for applying the protractor in the manufacture and maintenance of propellers.[24]

The cut-and-try method, though successful in the hands of a few talented practitioners, had too spotty a success record. Aeronautical engineers understood that a wide range of effective airfoils would be created only by using some more systematic analytical method involving tests in a significant and reliable wind tunnel.

VDT Testing

From the standpoint of significant and reliable research results, Langley's original atmospheric wind tunnel had been largely unproductive; however, the earliest tests in the new Variable-Density Tunnel, which began operation in October 1922, demonstrated that the NACA's experimental equipment had come of age. Tests in the compressed air of the VDT raised the dynamic scale significantly, validating Munk's design principle and making it possible to estimate full-scale performance more correctly by observing small model wings.

Langley began its first experimental investigation of a series of wing sections in the VDT in 1923. Though the research approach was to be essentially empirical, the idea behind the design of the series derived from a highly intuitive theoretical statement. In the "General Theory of Thin Wing Sections," published by the NACA in 1922, Max Munk had reversed the classic Kutta-Joukowski method. Convinced that contemporary aerodynamicists would fail to produce significantly improved airfoils if they continued to let the wing section be dictated by this mathematical method, Munk decided to "start with a wing section, any technically valuable wing section, and fit the mathematics to the wing section." Even though the method required some simplifying assumptions and did not permit the calculation of maximum-lift coefficients, Munk's idea was still a major breakthrough, if not a watershed in the history of airfoil design.[25] By replacing the airfoil section with an infinitely thin curved line, it permitted the calculation of certain airfoil characteristics (e.g., lift-curve slope, pitching moments, and range without any difficulty from the balance. He then made some minor adjustments and satisfied himself that all rough spots had been smoothed out.

The problem of convincing Munk remained. Weick could not simply tell him about the successful test, so he and the engineer-in-charge agreed to arrange another "first test" for Munk to witness. Reid escorted Munk to the tunnel the next morning. Weick casually said, "Good morning," walked up the ladder, and pulled through the Messenger's prop. Luckily, the engine started on the first try. Weick moved the ladder away, ran the engine through its entire range, and then shut it down. There was no noticeable vibration in any part of the balance. Weick, who had wondered what Munk's reaction would be, later recounted: "He walked toward me with his hand outstretched and congratulated me on the success of the operation."[35] The balance system operated satisfactorily with engines of up to 400 horsepower into the late 1930s, when it was replaced by a new and better one.

The matter of the PRT balance design resolved, Weick later had to deal with Munk over the technical issue of the best propeller blade-section coefficients—the numbers representing the lift, drag, and pitching moment characteristics. Munk thought that the coefficients should be put on the same logical foundation as that on which wing coefficients were based. While Munk's were more precise and elegant, Weick urged the use of coefficients that would be easier for designers to apply. (As an employee of the Bureau of Aeronautics, Weick had authored NACA TN 212, "Simplified Propeller Design for Low-Powered Airplanes," to help people make their own props for home-built aircraft.) One day in Munk's office Weick argued for his viewpoint. Not flinching, Munk—thumbs in the arm holes of his vest—ended the conversation with his version of compromise:

> Mr. Weick, we should agree. We should agree so that when we get up ... we say this is the way the coefficients should be. No one will dare stand against us. We should agree on my coefficients.

At that moment Weick did agree, but, back at work, he continued to use his own coefficients.[36]

Munk's subordinates did what they could during 1926 to work with him and then around him, but they finally rebelled. In early 1927 all of the section heads of the Aerodynamics Division resigned in protest against Munk's supervision: Elton Miller, head of the PRT section; George Higgins, head of the VDT section; Montgomery Knight, head of the Atmospheric Wind Tunnel (AWT) section; and John Crowley, head of the flight test section. Engineer-in-charge Reid, in office for barely a year and already stuck between the devil and the deep blue sea, tried to resolve the crisis by reassigning Munk as his adviser. Lewis tried halfheartedly to pacify Munk by asking him to return to Washington, even though Lewis probably did not want that to happen.[37] But Munk, his pride hurt, refused all options left open for him with the NACA, emphatically refusing to be holed up again in a small office away from research facilities, and resigned. Peace, and the section heads, returned to Langley, but only at the cost of losing one of the best theorists ever to work there.

Complexities

Because a profile of an outsider can help to outline the character of an inside group, just as a clear statement of antithesis clarifies a thesis, further exploration of the question "Why was it impossible for Munk to survive as chief of aerodynamics at Langley?" seems worthwhile. Such exploration might reveal important aspects of the historical personality of Langley laboratory, and might suggest much about the intercultural sharing of technology.

Clearly Munk was unusual in the Langley setting. The first thing that any group of Americans would have noticed about him, once hearing him speak, was that he was a foreigner. No doubt his thick accent and unfamiliar inflections made him seem more eccentric than he really was. What was worse in the early 1920s—a time of rampant nativism—Munk was a German, a "hated Hun," only recently the enemy of the United States and its allies.* In 1921 Frederick Norton, Langley's chief physicist, informed the NACA in Washington that if Munk were to stay at the lab on a regular basis—as he believed (correctly) Munk desired to do—it would be "extremely difficult to fit him into the organization." Army officers at Langley Field, he reported, would not take kindly to the presence of the German.[38]

Like the great majority of Langley researchers, Munk was an engineer—but the professional norms he had learned to value were those of German engineering and Göttingen applied science. Though some Langley engineers understood the importance of theory and were mathematically competent according to the American (not the Göttingen) standard, they saw Munk's

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* I have found no evidence that Munk suffered from any anti-Semitism at Langley; in fact, Munk, during my most recent (20 August 1985) interview with him, refused to admit that this sort of ill will, to whatever extent it existed, had anything to do with his problems at the NACA laboratory.

theoretical orientation as a factor separating him from themselves. George Lewis remarked that Munk's works were "of such a highly scientific character that they are not appreciated by the average aeronautical engineer, and can be appreciated only by those who have a very extensive training in mathematics and physics."[39] Besides believing this personally, Lewis had heard others complain about the highly mathematical character of Munk's works. For instance, in response to Munk's criticism of one prospective NACA report, Langley engineer-in-charge Leigh Griffith had advised the Washington office that "criticism of research reports dealing with actual laboratory results should not be undertaken by theoreticians since the viewpoint of the theoretician is usually so radically different from that of the laboratory research man." The engineer-in-charge tied his unwillingness to accept the judgment of this particular theoretician to Munk's use of foreign criteria. It "is rather unfortunate," he told Lewis, that Munk was "not more familiar with current standard American nomenclature" and was therefore "inclined to criticize terminology not in agreement with his own peculiar ideas."[40]

Not only American engineers but American scientists as well thought that Munk's report writing, both its style and substance, was excessively vague and obscure. After reading the draft of Munk's July 1925 report "On Measuring the Air Pressures Occurring in Flight," Joseph Ames, a physicist, wrote Lewis that Munk's discussion of general problems in the paper was "excellent" but his style "impossible." It was "neither fish, flesh nor fowl." Ames nevertheless recommended that the report be published after extensive editing.[41] However, Walter S. Diehl of the Bureau of Aeronautics (a profoundly influential individual in NACA history; see especially chapter 6) reviewed Munk's prospective report and asserted that "there can be no real argument about the style desirable for a scientific report." It had to be "clear, concise, and without grammatical errors or rhetorical flourishes." "I feel that Munk has carried the matter entirely too far and that he is substituting rhetoric for scientific facts," Diehl charged.* Though he

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* Typically a paper by Munk included very few references to relevant published literature or rigorous mathematical demonstrations which would show readers exactly how he came to his conclusions. His manner of thinking was so highly intuitive that he proceeded in research as if he were the only person working in the field. His collection of technical books (which Munk recently donated to the Langley Historical Archive) is remarkably meager—perhaps indicating the great extent to which he relied on no human being but himself for revelation of knowledge. In "My Early Aerodynamic Research: Thoughts and Memories" (in the *Annual Review of Fluid Mechanics*, 13 [1981], pp. 4 and 6), Munk declared: "Mathematics comes from within .... Undertaking research for the advance of mathematics is more difficult than using established mathematics. It requires more curiosity, diligence, and aimful thinking. The researcher's character in general, I believe, has also much to do with it. The pure in heart shall see." When asked (during an August 1985 interview) how he arrived at the thin

considered the piece of interest and value, Diehl questioned the advisability of publishing it. He suggested holding out for a "conventional report," one in which the observed data—all of it—held center stage.[42]

In addition, Munk showed personality quirks that went far beyond those tolerable even in NACA Nuts.[†] Having internalized the social relations of German academic life, Munk considered himself the absolute master of the division he directed. He intended to set the research goals and, like a German university professor, himself receive whatever credit was forthcoming. A proud genius, Munk was frequently autocratic and arrogant in his dealings with people, treating his men at Langley as graduate students and obliging some of them to attend a seminar on theoretical aerodynamics he conducted in a way that at least two talented young men, Elliott Reid and Paul Hemke, a Ph.D. in physics from Johns Hopkins University, found rude and condescending. (In 1927 Reid and Hemke both resigned from the LMAL, "strongly influenced in their decisions to leave the Committee because of their unpleasant relations with Dr. Munk.")[43] While still learning English by reading Macaulay and Oscar Wilde, Munk had the audacity to offer Langley employees and their wives a night class on English literature. The class met once; Munk's sweeping criticisms of some of the class members' favorite authors and books alienated his audience completely.[44]

In sum, one may interpret the revolt of the Langley engineers against Munk as a clear instance of nonadaptation between different national cultures of science and engineering, or as a case in point showing how "culture shock" may affect technology transfer. American history is full of outstanding examples of skilled European technologists, such as Samuel Slater, Benjamin Latrobe, and John Roebling during the early nineteenth century, emigrating to the United States and successfully transplanting the

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airfoil theory of 1922, for example, Munk answered, "How do such things happen? They are miracles!"

[†] Langley old-timers Harold R. Turner, Sr., James G. McHugh, and Hartley A. Soulé love to tell a tall tale about Munk learning to drive a car; over the years, the story has become a sort of local legend, an extravagantly exaggerated one. When he arrived, the story goes, Munk had never driven an automobile. One of the wind tunnel technicians tried to teach him how to do it right, but Munk thought there was a better way. So, he drew a map of the road between Hampton and Langley Field, figured the exact distances between the curves of the road, calculated the curvatures of the mandatory turns, hung a string down from the top of the steering wheel, and applied numbered pieces of tape to indicate the manipulation of the wheel required for the car to follow each turn. Then, by driving at a predetermined speed, he could, with the help of his map and a stopwatch, make it safely from home to work. According to Soulé, "It was that type of story that caused the local people to assume that everyone from the NACA was a screwball" (Soulé interview with Walter Bonney, 28 March 1973, p. 2 of transcript in LHA).

In the eyes of most aeronautical experts, the overall record of Max Munk (front row, third from left) in aerodynamic research falls at least one big step short of that achieved by fellow immigrant Theodore von Kármán (the short man in a double-breasted coat in the middle), shown here during a visit to Langley in December 1926. Both men were protégés of Göttingen's Ludwig Prandtl. George Lewis, the NACA's director of research, stands to the far right of the photo. Henry Reid, Langley's bespectacled engineer-in-charge, is on the same step as von Kármán, to his right. Fred Weick, PRT head, is in the back row, over Reid's right shoulder. Paul Hemke is to Weick's immediate left. Elliott Reid, the future Stanford University professor, is at the far right of the back row.

new or "hot" technologies of the Old World in the fertile soil of their New World. But these transplantations were not automatic. There were certain forces in American society that resisted technology transfer. We know for example that American clients criticized Latrobe for his stubborn insistence on doing everything to English standards, such as building with expensive stone instead of wood.[45] We know also that the canal builders of western Pennsylvania at first rejected (also mainly for financial reasons) Roebling's advice to substitute wire for hemp in their winch-driven canal cables.[46]

Transfers of technology, like transplantations of living organisms, are difficult, and do not always succeed. You cannot raise cotton in Michigan, after all, or sugar cane in Maine. Alligators will not live in the Bering

Sea. Taken away from native nutrients, confronted with the challenges of a radically different environment, species have to prove adaptable genetically or they die out. The story of Max Munk at Langley should encourage students of American science and technology to be more sensitive to the kinds of attitudes and arrangements on both sides of a transfer that in some circumstances facilitate and in others impede the flow of knowledge between different peoples.

Max M. Munk in his office at Langley, 1926.

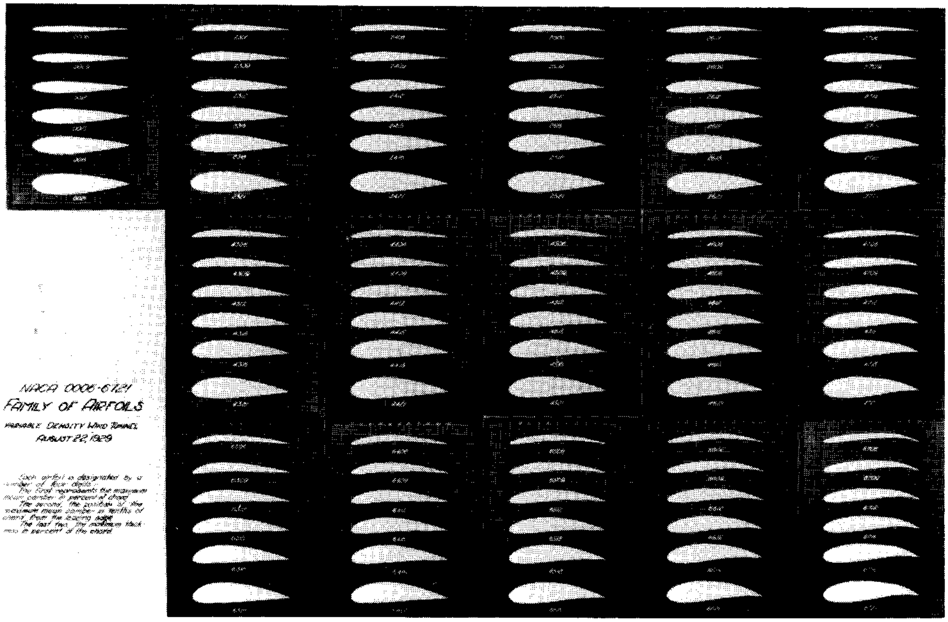
4

With a View to Practical Solutions

Though Munk resigned from the NACA in early 1927, Langley's systematic experimental program to develop improved airfoils in the VDT continued unabated. In 1933 the Committee published "Characteristics of 78 Related Airfoil Sections from Tests in the Variable-Density Tunnel." This report introduced what was to be the VDT's principal achievement as an aeronautical research tool: a second and more significant series of airfoils, the NACA 4-digit series.

The Mind's Eye

In the course of developing the second airfoil series in the late 1920s and early 1930s, the VDT team devised a numerical code—patterned after that used to identify the composition of steel alloys—by which to describe the physical shapes. Like all other aerodynamical laboratories, Langley had until then designated airfoils simply by numbering them in the sequence in which they had been tested (M-1, M-2, M-3, and so on). In the new system, however, four numbers would indicate the airfoil section's critical geometrical properties. The first integer represented the maximum mean camber in percent of the chord; the second integer represented the position of the maximum mean camber in tenths of the chord from the leading edge; and the last two integers represented the maximum thickness in percent of the chord. Thus, airfoil "N.A.C.A. 2415" was a wing section having 2 percent camber at 0.4 of the chord from the leading edge, with thickness 15 percent of the chord. Zeroes were used for the first two integers when the section was symmetrical, as in the case of N.A.C.A. 0015. The laboratory expanded the code to five and then six digits for subsequent airfoil series, and indicated modifications like changes of the leading-edge radius or the position of maximum thickness by adding a suffix consisting of a dash and



By August 1929, tests in the Variable-Density Tunnel had derived the family of airfoils N.A.C.A. 0006 through N.A.C.A. 6721, shown here in cross section.

two more digits, as with N.A.C.A. 23012-64, an outstanding section in the popular 230-series.[1]

This code did not signify much to the man on the street, but to aeronautical engineers it suggested everything important about an airfoil. The NACA's 1933 report on 78 related airfoils, which formally introduced the numbering system, became a classic, a designer's bible. From the mid-1930s on, one could say, for instance, "N.A.C.A. 2415," and an airfoil complete with a camber line, position of maximum thickness, and special nose features would appear in any aerodynamicist's mind's eye. Serving to remind as much as to instruct, the NACA's airfoil report complemented the coded information with graphic illustrations of two independent sets of curves. These curves communicated knowledge basic to an engineer's understanding of the relationships among an airfoil's variables.[2] Graphic representation of airfoil data—the outline of the physical shape reinforced by performance curves and the digital code—gave aeronautical engineers ready access to the wide range of parametric data necessary to their work. The NACA digest gave them a "whole range of wings from which to choose, the way one might select home furnishings or automobile accessories from a catalog."[3] From that catalog, the American aircraft industry picked NACA airfoils that became the wings for some of the best aircraft of their era, including the DC-3 transport and the B-17 Flying Fortress, as well as a number of postwar general aviation aircraft.

Tunnel Turbulence

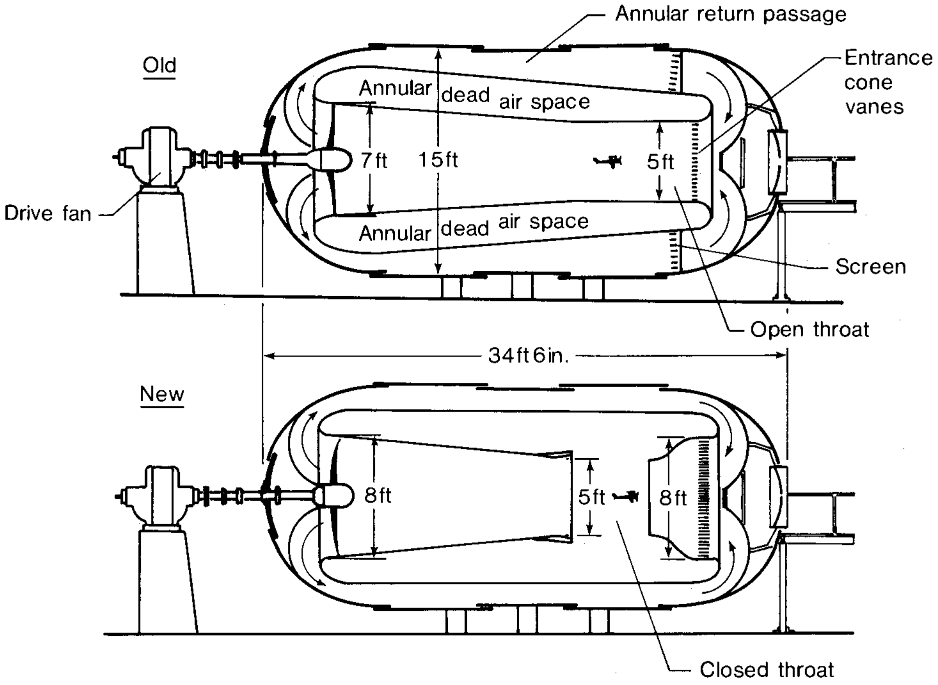
In 1929 C. G. Grey, British engineer and editor of *The Aeroplane,* attended ceremonies at Kitty Hawk celebrating the 25th anniversary of the Wright brothers' flight. During his stay in America, Grey visited the NACA laboratory in nearby Hampton. Upon his return home, he upset his British colleagues by expressing the opinion that "the only people so far who have been able to get something like accurate results from wind tunnel experiments are the workers at the experimental station at Langley Field."[4]

The NACA staff had made a reputation by building and making good use of the VDT and a few other unprecedented facilities. By that time, the Propeller Research Tunnel (PRT) had made its initial contribution to the development of a low-drag engine cowling (see next chapter), research work had begun in an 11-inch high-speed tunnel which used exhaust air from the VDT (see chapter 9), and a giant 30 × 60-foot full-scale tunnel was under construction. Above all, however, it was the VDT, representing the Committee's first bold step in the direction of novel research equipment, which won the NACA its international reputation as a technologically outstanding research organization.

Ironically, though, as useful as it was, the VDT was far from the total aerodynamic triumph trumpeted in the NACA brochures: the compressed-air machine suffered from intense airstream turbulence (small-scale eddies and cross-current swirls) resulting from its small-contraction-ratio, double-return design and relatively inexpensive synchronous drive motor, which followed small but rapid frequency fluctuations. These motor fluctuations made airspeed control a serious concern for tunnel operators.

Langley chief physicist Fred Norton, who had so many problems with Munk over the design and construction of the tunnel, had in fact identified the VDT's basic defect as early as April 1921. In a letter to NACA headquarters, Norton had asserted "the probability that the steadiness of flow in the compressed-air tunnel because of the small room required [to turn the airstream] would be inferior to that in the usual type tunnel, thus considerably decreasing the accuracy of the test."[5]

In spite of these shortcomings, VDT researchers were extremely proud of their facility because they knew that no one in the world had a similar instrument for penetrating the vagaries of scale effects, meaning that everyone else was getting data even less accurate than they were. By the late 1920s, however, the VDT was fast losing its edge over other wind



This diagram, based on an LMAL drawing from 1928, illustrates the lab's plan for correcting the turbulent airflow that had plagued the original VDT. Notice in particular the change from an open-throat to a closed-throat test section.

tunnels. Enhancing the tunnel by rectifying its limitations became critically important to the NACA staff.

In August 1927 a broken light bulb sparked a fire and explosion in the VDT and gave Langley the opportunity to tear the compressed-air machine apart and rebuild it with a closed-throat test section and a new direct-current, variable-speed drive system. But after some five years of sporadic reconstruction, the head of the VDT section, Eastman N. Jacobs, informed the engineer-in-charge that the tunnel's basic design precluded the "possibility of obtaining the steady, constant, and uniform airstream sought in modern wind tunnels."[6]

NACA Langley's growing recognition of the seriousness of turbulence in the VDT was only one good reason to seek funds in 1928 for the construction of a full-scale wind tunnel. Though the Committee continued to believe in the VDT as "a satisfactory means for testing the component parts of an airplane" and, in particular, "for conducting fundamental research on airfoil sections," it also wanted a state-of-the-art facility large enough to permit the testing of actual pursuit aircraft complete with operating engines and slipstream effects. Fortunately for the NACA, Congress decided to appropriate the money for the construction of the Langley Full-Scale Tunnel (FST) in February 1929, just months before the Wall Street crash. Contracting for materials and labor at Depression prices, the laboratory was able by May 1931 to complete what was then the world's largest wind tunnel at a cost of just over one million dollars.[7]

FST vs. VDT Debate

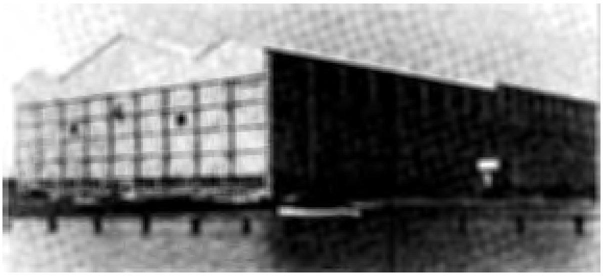
The need to investigate the degree of dynamic similarity between the performance of scale models in small tunnels and the performance of airplanes and components at full scale led the FST section into spirited competition with the VDT group. Preliminary tests in the 30 × 60-foot facility convinced the FST staff that turbulence in the new machine was so "unusually low"—certainly much lower than in the VDT—that its effects could "be neglected in applying the data to design."[8] Then the results of an FST study of the characteristics of several large airfoils of various designs, including some from the NACA 4-digit family, indicated an increase in drag caused by differences in section thickness, a key design parameter, at a rate much less than that predicted by VDT tests. This was a discrepancy that directly affected the choice of wing thickness for the inner sections of airplane wings.[9] As more and more tests in the FST showed good agreement with results obtained in flight, some of the prouder and less circumspect proponents of the FST even went so far as to contend that results from the VDT bore little relation to what really happened in flight and that correct airfoil data could only be obtained from tests on full-scale wings in the FST. VDT defenders, though fully aware by this time of their facility's inherent defects, answered the charges of their peers by asserting that their machine was still the NACA's best cheap means of obtaining a wide range of comparative data on a multitude of related airfoils. FST test specifications called for aircraft and aircraft models that were simply too cumbersome and expensive, they argued, to permit the kind of systematic research programs that had been accomplished in the VDT.[10]

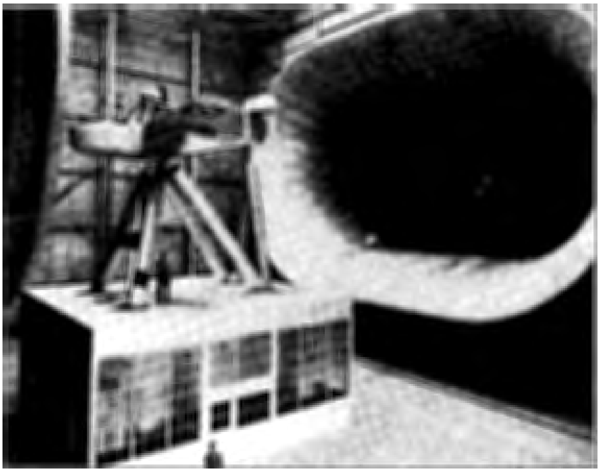
The FST vs. VDT debate continued into the mid-1930s, stimulating members of all LMAL wind tunnel teams to think about the factor of scale and the corrupting effects of turbulence on aerodynamic measurement. In particular, however, the debate seems to have sparked the ingenuity of the VDT team itself, whose work was most in question. Many of these researchers began to look more carefully at the flow phenomena, especially in the *boundary layer*, that might be the source of the consistent errors in their results. (The boundary layer is the thin stratum of air very close to the surface of a moving airfoil in which the impact pressure—that is, the reaction of the atmosphere to the moving airfoil—is reduced because of the air's viscosity. In this layer, which is separated from the contour of the airfoil by only a few thousandths of an inch, the air particles change from a smooth *laminar flow* near the leading edge to a more or less turbulent flow toward the rear of the airfoil. See von Kármán, *Aerodynamics*, pp. 86–91.) To visualize the nature of the airflow around airfoils and other objects, they constructed—next to the other equipment in the VDT building—a small low-turbulence smoke tunnel. Photographs of the smoke flowing around test models facilitated study of the conditions of the boundary layer as they changed from low-friction laminar flow to high-friction turbulent flow. LMAL engineers accelerated their pursuit of a means of removing air from the boundary layer through slots or holes in the wing surface—an effort which dated back to 1926, and which was intended to decrease drag and increase lift by postponing *transition* from laminar to turbulent flow. Work in the smoke tunnel eventually led NACA aerodynamicists to the conclusion that two of the critical factors causing transition, and thus high skin-friction drag, were surface roughness (the rivet heads, corrugations, and surface discontinuities then common in manufactured airplane wings) and pressure distribution on the wing surface.[11]

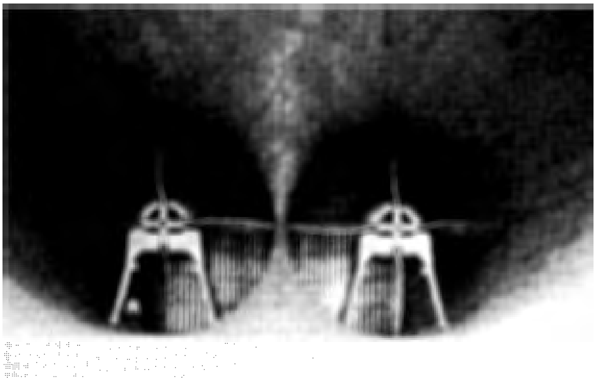
Eastman Jacobs, head of the VDT section, answered the FST challenge to the integrity of VDT results by introducing the concept of "effective Reynolds number." In essence, this was Jacobs's stopgap effort to reproduce the aerodynamic effect that would be obtained in the VDT if it had zero turbulence:

> In a wind tunnel having turbulence, the flow that is observed at a given Reynolds number ... corresponds to the flow that would be observed in a turbulence-free stream at a higher value of the Reynolds number. The observed coefficients and scale effects likewise correspond more nearly to a higher value of the Reynolds number in free air than to the actual test Reynolds number in the free stream. It is then advisable to refer to this higher value of the Reynolds number at which corresponding flows would be observed in free air as the *effective Reynolds number* of the test and to make comparisons and apply the tunnel data at that value of the Reynolds number.

Jacobs figured the effective Reynolds number by multiplying the test Reynolds number by the tunnel's turbulence factor. For the VDT, the turbulence factor was 2.6, the highest of all LMAL tunnels.[12] The concept of the effective Reynolds number, though resting on a slender empirical correlation, was soon used by all the NACA wind tunnel sections, in



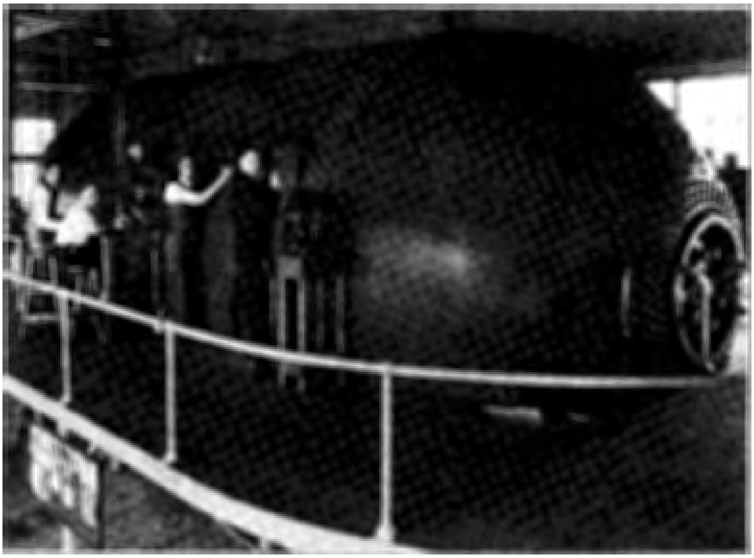




Above, a view of the Full-Scale Tunnel's huge (434 by 222 feet, and 90 feet high) exterior from the Little Back River, October 1930. Center, one of the first tests in the Full-Scale Tunnel was a performance evaluation of the Loening XSL-1 single-engine navy seaplane, October 1931. Below, the FST's enormous twin fan blades.

particular to show the effects of Reynolds number on maximum lift.[13] Some way to compensate for tunnel turbulence was better than no way at all.

The permanent solution Jacobs really wanted to the problem plaguing his work, however, was a new and larger variable-density tunnel with an airstream quality approaching that of the smooth air of free flight. Though



The VDT research team, March 1929. Eastman Jacobs is sitting (far left) at the control panel.

he had wanted it for some time, Jacobs began to campaign in earnest for the new machine after the NACA's introduction in 1934 of its successful 5-digit airfoil series, which had evolved through systematic variation of the nose shape and camber parameters of the better airfoils of the 4-digit family. For the first time since the beginning of the Depression, the Committee was in a relatively good position to secure funds for new construction. In an April 1935 memorandum to the engineer-in-charge reporting the results of a staff conference on ways to increase the speed of airplanes, Jacobs made his idea official. A low-turbulence pressure tunnel, he urged, would greatly enhance the two related lines of research that the VDT team had been long pursuing: development of new airfoils and better understanding of the basic aerodynamic relationship between airstream turbulence, boundary-layer flow, and wing performance. Though asserting that the existing VDT could still provide useful design data and should "probably be maintained for this purpose" and as an air reservoir for the LMAL's 11-inch and 24-inch high-speed tunnels, Jacobs quickly emphasized that the "air stream necessary for the continued investigation of the fundamental characteristics of large scale air flows cannot be obtained in the existing tunnel." Turbulence in the old tunnel did not completely invalidate its results for airfoils like those of the 4- and 5-digit classes, but accurate experiments with airfoils and other bodies that might enjoy low-friction laminar flow could not be expected in the existing facility.[14]

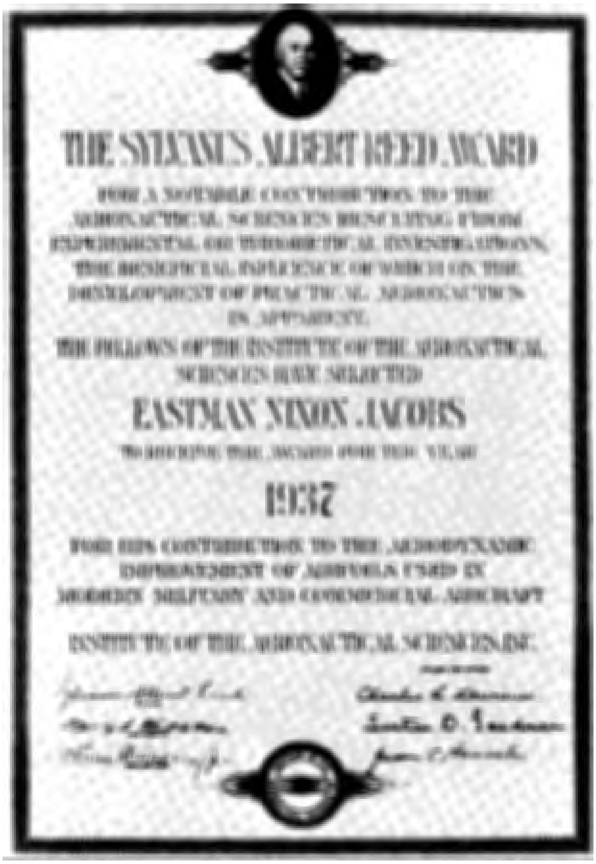
Within two weeks after receiving a copy of Jacobs's proposal for comment, two of Langley's most influential division chiefs sent memos to Henry Reid elaborating their reasons why the NACA should reject Jacobs's idea. Smith J. DeFrance, head of the FST group, questioned whether the knowledge to be gained from the new equipment would warrant the expenditure of money.[15] But it was Theodore Theodorsen, head of the small Physical Research Division, who expressed the most vociferous and historically significant (and ultimately incorrect) objections to the facility Jacobs had in mind:

> I think the variable-density wind tunnels have outlived themselves. I do not think that the variable-density tunnel has led to any fundamental discoveries. They contain a very large amount of turbulence in the airstream, a condition that cannot be avoided.

"What is a new variable-density tunnel to be used for?" Theodorsen asked. "Several years will be required to investigate the tunnel, and then what?" There was "no more need for airfoil testing," the physicist declared, except possibly in connection with some questions about flow conditions in the boundary layer better answered by theoreticians.[16]

The Jacobs-Theodorsen Rivalry

While Eastman Jacobs and his VDT staff had been developing the 4- and 5-digit families using the systematic experimental approach, Theodorsen and his group of more mathematically inclined researchers in the Physical Research Division had been tackling various airfoil problems from the theoretical angle. Though perhaps the greatest contribution of Theodorsen's group during this period was a theory of oscillating airfoils with hinged flaps, related closely to the problem of flutter, the group also provided some very meaningful insight into the relationship between pressure distribution and boundary-layer flow, and hence on wing-section characteristics. In an NACA report published in 1931, Theodorsen had described a "Theory of Wing Sections of Arbitrary Shape," which made it possible, as long as the flow did not separate from the airfoil, to predict the pressure distribution of an airfoil. Starting with an arbitrary airfoil, one changed the closed two-dimensional shape through a conformal transformation almost into a circle; then, by using a rapidly converging series, one transformed the bumpy circle into a true circle about which the flow



THE SYLVANUS ALBERT REED AWARD

EASTMAN NIXON JACOBS

1937

Eastman N. Jacobs, one of Langley's most adventurous researchers. In 1937 Jacobs received the Sylvanus Albert Reed Award for his contribution to the aerodynamic improvement of airfoils.

was known.[17] Though no one at the time thought it reasonable to apply this theory for the purpose of a practical design, the knowledge of the pressure distribution made possible by this clever double transformation later suggested the answer to the riddle of how to shape a laminar-flow airfoil.

The proposed low-turbulence tunnel was not the first issue over which Jacobs, the lab's leading experimentalist, and Theodorsen, the lab's top theoretician, had squared off, and it would not be the last. Beneath the basic difference in their approaches to gaining aeronautical knowledge, there existed a strong personal rivalry and mutual dislike that moved most of their confrontations beyond mere objective disagreement. At Langley both men controlled fiefdoms, and because both men were so valuable to the NACA, George Lewis had permitted the feudal arrangement to flourish. Usually they worked on completely separate activities, but occasionally they had to work together—and then they inevitably clashed.

Theodore Theodorsen, the NACA's Norwegian import, complained that many LMAL engineers were weak in mathematics.

More significant than any hint of personal antagonism in Theodorsen's critique of Jacobs's tunnel proposal was the theoretician's suggestion that Langley's airfoil research had reached an experimental impasse. Though Theodorsen was practical enough to realize that the "imperfect status" of wing theory required designers to make their airfoils "independent of theoretical restrictions," he nonetheless saw the need for the NACA staff to fertilize its experimental routine with a stronger dose of theory. "A large number of investigations are carried out with little regard for the theory," Theodorsen charged, "and much testing of airfoils is done with insufficient knowledge of ultimate possibilities." In his opinion, to discover more advanced airfoils the NACA did not need a new wind tunnel but rather better mathematical and physical understanding of the effects of basic aerodynamic phenomena on wing performance. The implication of his argument was that the experimentalists at Langley had become too interested in and dependent upon equipment for their own good.[18]

Jacobs disagreed totally with the idea that theoreticians could answer the remaining questions about airfoils better than could experimentalists; he also rejected the argument that it was unnecessary and impossible for the NACA or anyone else to build a pressure tunnel having low airstream turbulence. He did not disagree, however, with Theodorsen's notion of theory's general role in successful research. An adventurous man with an expansive outlook on what was possible, Jacobs kept up with and understood the most current theory—though he did not devote much of his own time to its study—and valued its role in creating the fundamental but directly useful technological information expected of the NACA. In fact, he had a broader outlook on what was possible than did many of the more theoretical types. During his long career with the NACA (1925–1944), Jacobs explored several revolutionary aeronautical concepts and sometimes grew impatient with co-workers and bureaucrats who saw too many obstacles in the way of their rapid development. In the late 1920s, he tested the potential of thrust augmentors for jet propulsion (see chapter 8). Ten years later, after a newspaper article led him to read the theoretical papers of Hans Bethe, he and fellow NACA researcher Arthur Kantrowitz attempted to initiate the thermonuclear fusion experimentation described in chapter 2. Colleagues remember "Jake" as the type of man who was always looking for the pot of gold at the end of the rainbow.[19]

As for the development of airfoils by a combination of theory and experiment, in the 1930s no one working in the United States, perhaps even in the world, surpassed Jacobs's ability. Though it is now difficult to pinpoint just when he first considered controlling the boundary layer through body shape or through control of the usual pressures acting along the body surface, the idea for doing so seems to have been germinating in Jacobs's mind since at least as early as 1930. In a memo on airfoil scale effects in November of that year, Jacobs discussed the importance of the relationship between transition and airfoil drag and mentioned the dependence of the transition point on airfoil shape.[20] At the time, he expected that "the possible large drag reductions through prolonging of laminar boundary layers" (that is, through prolonging transition to turbulent flow) would become apparent "as the result of the systematic tests of various airfoil shapes." By 1935, however, he knew this empirical verification would not happen without new turbulence-free testing equipment.[21]

In May 1935, after considering Jacobs's tunnel proposal together with the comments of DeFrance and Theodorsen, engineer-in-charge Reid determined that the project did "not warrant serious consideration by the Committee at this time." George Lewis, in Washington, concurred.[22] The research managers had good reasons to turn down Jacobs's idea for a new VDT. First, other important projects, including the construction of an expensive new tunnel visualized as a super PRT for high-speed propeller research (eventually built at the LMAL as the 19-Foot Pressure Tunnel but which was not really used much for propeller research) were awaiting funding. Second, because the NACA knew that "the desirability of low turbulence in wind tunnels was not widely appreciated," funds for such a facility would be difficult to justify before Congress.[23]

Jacobs Campaigns for Low-Turbulence VDT

In late 1935 Jacobs returned to Langley after representing the NACA in Rome at the Fifth Volta Congress on High-Speed Aeronautics. Now more than ever, he was convinced that Langley had to have a low-turbulence pressure tunnel. During his trip he had visited most of the larger aeronautical research laboratories on the Continent, whenever possible examining new experimental facilities and discussing current work. He found the European nations to be in keen scientific and technological competition, spending "large sums of money building up their research establishments." Though concluding that America's "present leading position" in aeronautical research and development was "not seriously menaced at this time," Jacobs warned that "we certainly cannot keep it long if we rest on our laurels" and fail to develop and modernize our test equipment. At the end of his trip report the Langley engineer reverted to the theme of his memorandum of 26 April 1935: "It is again urged that modern variable-density tunnel equipment be built in this country capable of testing at full dynamic scale for modern aircraft."[24]

Jacobs also brought back some new insight into the nature of the boundary layer. While in England he had spent a weekend at the home of Sir Geoffrey I. Taylor, professor of physics at Cambridge University, who had presented a paper on high-speed flow at the Volta Congress. In long private conversations, Taylor described for Jacobs the substance of his recent work in the statistical theory of turbulence. This theory seemed to indicate that "the transition from laminar to turbulent flow was due to local separation caused by the pressure field."[25] By implication, this result said that transition could possibly be delayed or perhaps avoided by preventing laminar separation—i.e., by using a falling pressure gradient. This would be the mechanism used eventually by Jacobs in his design of laminar-flow airfoils.

Jacobs also had the chance at Cambridge to talk at length with B. Melville Jones, professor of aeronautical engineering, who, like Jacobs, epitomized the researcher who combined theory and experiment for practical purposes. (Jones's classic 1929 paper, "The Streamline Airplane," had provided designers with an idealized goal that served to indicate how much power was being wasted to overcome drag.) Jones reported to his American counterpart that recent British flight work showed considerable laminar flow over the forward regions of very smooth wings where much of the flow was in the falling-pressure region. This encouraged Jacobs greatly. It pointed to the possibility that drag levels achieved by well-designed advanced aircraft could be down to the value of skin friction. Thus, the only remaining opportunity for reducing drag would lie in encouraging laminar flow—something that is still true.[26]

Armed with this new information, Jacobs returned to the LMAL ready to press harder for the construction of his new variable-density tunnel. During his presentation to the NACA's annual manufacturers' conference in May 1936, he advertised his belief that further reduction in drag would have to take place as a result of somehow delaying transition to turbulent flow in an airfoil's boundary layer.[27] At a laboratory conference on boundary-layer control in July, Jacobs argued that "direct control through shape should be placed first on our program" and again urged his colleagues to support his idea for the construction of suitable turbulence-free testing equipment.[28] In the fall, he wrote a paper on "Laminar and Turbulent Boundary Layer as Affecting Practical Aerodynamics," which, in essence, was a plea for the new tunnel.[29]

On 28 May 1937, Jacobs's 13-month campaign for a low-turbulence VDT finally achieved its goal, if in a roundabout way: NACA headquarters authorized the construction of an "icing tunnel." The name was a necessary political subterfuge. George Lewis felt that the NACA could not at the time justify the expense of a new wind tunnel at Langley solely for development of low turbulence. Congressmen simply would not understand the urgency. But the Committee could sell it, he believed, on the basis of icing experiments. Many aircraft crashes traced to icing problems were attracting considerable public attention in 1937; the commercial airline operators were clamoring for useful information on the subject.[30] Here was a way for the NACA to kill two birds with one stone.

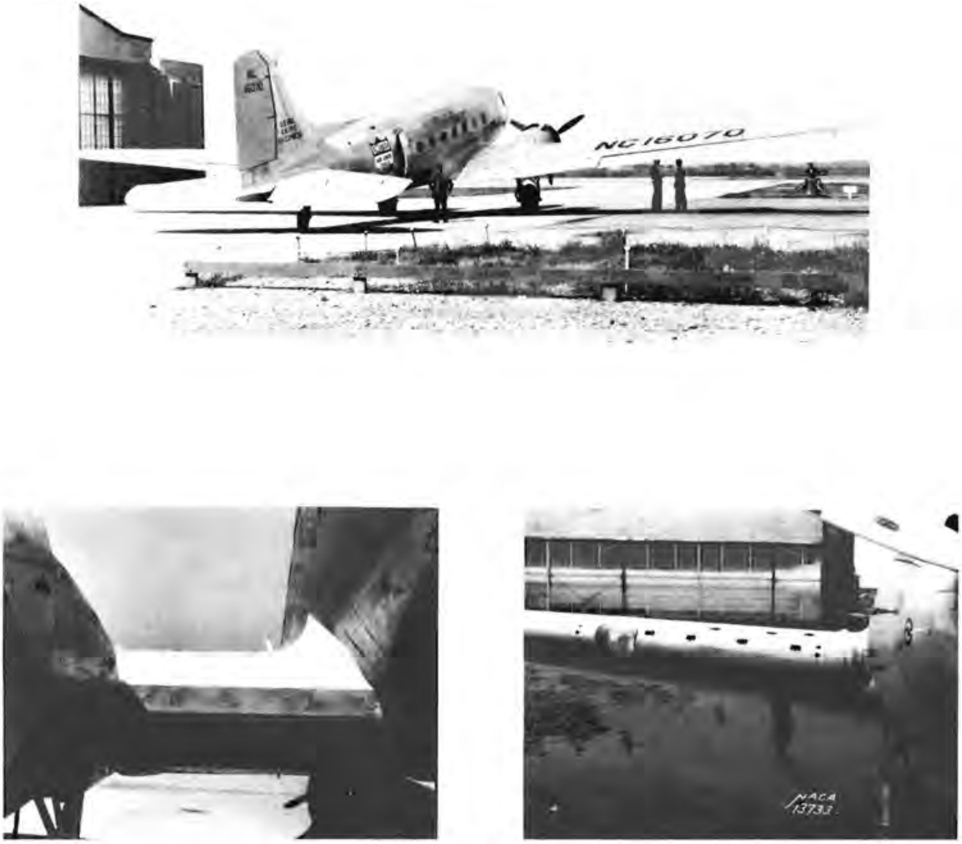
In 1937 a team of Langley researchers headed by Lewis A. Rodert was in fact in the midst of conducting icing research in free flight. The idea was to pipe hot engine exhaust gases through interior passages in model wings—mounted firmly on struts a foot or two above the wing of a test airplane—at the critical altitude where air temperature could cause ice to form. When the plane reached that height, water was sprayed on the leading edge of the model. As the edge quickly coated with ice, heat was piped into the model's interior passages, and a timed camera recorded how long a given amount of heat took to free the surface of its ice coating. This technique worked—that is, it worked in these flight tests in small models. But it raised serious problems of adaptation for full-size flying machines. In particular, since the heat-conducting pipes in an actual airplane had to pass through critical elements of the wing structure (e.g., spars and ribs), the technique threatened to weaken that structure seriously, mainly by adding too much weight. Thus tests using models in a small ice tunnel could not aid the flight program significantly. (In 1938 Rodert's team reduced the weight of the NACA thermal ice-prevention system to a minimum. The army provided funds for Langley to remodel a Lockheed 12 with a wing-and-tail heating system and to send the aircraft up into the clouds seeking ice.)[31]

So Langley never really intended to conduct many icing experiments in the icing tunnel of 1937. LMAL technicians had insulated the walls on the outside of the tunnel with only a wrapping of crude insulation (kapok removed from surplus navy life preservers by members of the Hampton High School football team) and added only the simplest refrigerating equipment (an open tank of ethylene glycol cooled by blocks of dry ice). On one hot summer day in 1938, when everything was ready, enough of the cold mixture was pumped into the tunnel test section to cause some ice to form on the leading edge of an airfoil. Then a method of using an engine's exhaust heat to prevent the ice formation was tested. A perfunctory series of experiments fulfilled the announced purpose of the ice tunnel, and Langley immediately converted it into a low-turbulence tunnel for low-drag airfoil studies. This facility served as a prototype for the pressurized Two-Dimensional Low-Turbulence Tunnel constructed at the LMAL between 1939 and 1941.

In Search of the Laminar-Flow Airfoil

After returning from the Volta Congress in late 1935, Jacobs discussed with the men of his VDT section what he had discussed in England with Taylor and Jones: the idea that continuously decreasing pressure along an airfoil would tend to delay transition from laminar to turbulent flow in the boundary layer. But exactly how and when the implications of this concept were first spelled out for the rest of the lab is not entirely clear. Jacobs now recalls that sometime in 1937 he "rediscovered" the idea, noting that the effect of the pressure gradient on laminar separation had been established previously.[32] In fact, the idea dated back to a paper by Prandtl published before World War I, and had been restated by Theodorsen in his 1931 paper, "Theory of Airfoils of Arbitrary Shape." Whatever its origins, this concept was the underlying criterion for the Jacobs group's imminent preliminary design of laminar-flow airfoils.

Jacobs and his fellow researchers knew that laminar-flow airfoils would have to satisfy several conflicting requirements. They were confident that an application of existing airfoil theory could start the design process by producing shapes with prescribed pressure distributions. (Langley's airfoil experts already had some valuable experience in designing airfoils for a prescribed pressure distribution. In the mid-1930s they had designed the 16-series to have a specific distribution in order to achieve the highest



In late September 1937 Langley performed stalling and icing studies with a DC-3 Mainliner passenger transport (top) belonging to United Airlines. In order to warn the pilot of an approaching stall, the NACA engineers installed sharp leading edges on the section of the wing between the engine and fuselage (left). These sharp edges disturbed the airflow enough to cause a tail buffeting which could be felt by the pilot in his control column. When the pilot felt this buffeting, he knew that his airplane was approaching a stall and needed pilot correction. In order to simulate the effects of ice formation on the DC-3's performance, the engineers cemented pieces of sponge rubber to the forward part of the wings (right), where ice was thought to form most often, and then measured the resulting changes in the plane's climb, cruise, and stalling speeds.

possible critical Mach number.)[33] But they were also quite sure that such a theoretical application would only be the first step. Theory alone could not answer the key design questions, such as: What distribution was needed for laminar flow? What compromises with other kinds of design requirements would have to be made for construction of an effective and practical wing? Answers to these questions would have to be found, the researchers believed, in a comprehensive program of experiments in the low-turbulence tunnel.

The Jacobs group could visualize a virtually infinite number of airfoils with falling pressure distributions—with varying pressure gradients, camber, thickness, and positions of peak suction pressure. A large number of related experimental airfoils would now have to be designed to incorporate the falling pressure feature, together with systematic variations in the other parameters.[34] Obviously this would be a far more difficult task than had been the design of the previous NACA airfoil families, which involved mostly simple, arbitrary, geometrical relationships.

With his commitment to the design of laminar-flow airfoils now overshadowing all of his other work, Jacobs disappeared from Langley Field for a few days to unravel the mysteries of Theodorsen's 1931 airfoil theory and to explore possible ways of reversing its procedure, which had been designed to predict the pressure distribution from a given shape. First, he called over to his house a close friend, Robert T. Jones, a highly intuitive NACA researcher who had taken a few classes at Catholic University taught by Max Munk.[35] Together, Jacobs and Jones decided that Theodorsen's method could not be used in the way desired without adding to the theory. Jones proposed an extension of the theory derived from Munk's thin-airfoil work that seemed to be a way of calculating a shape that would give a desired sequence of pressures, but this also proved too inaccurate.[36]

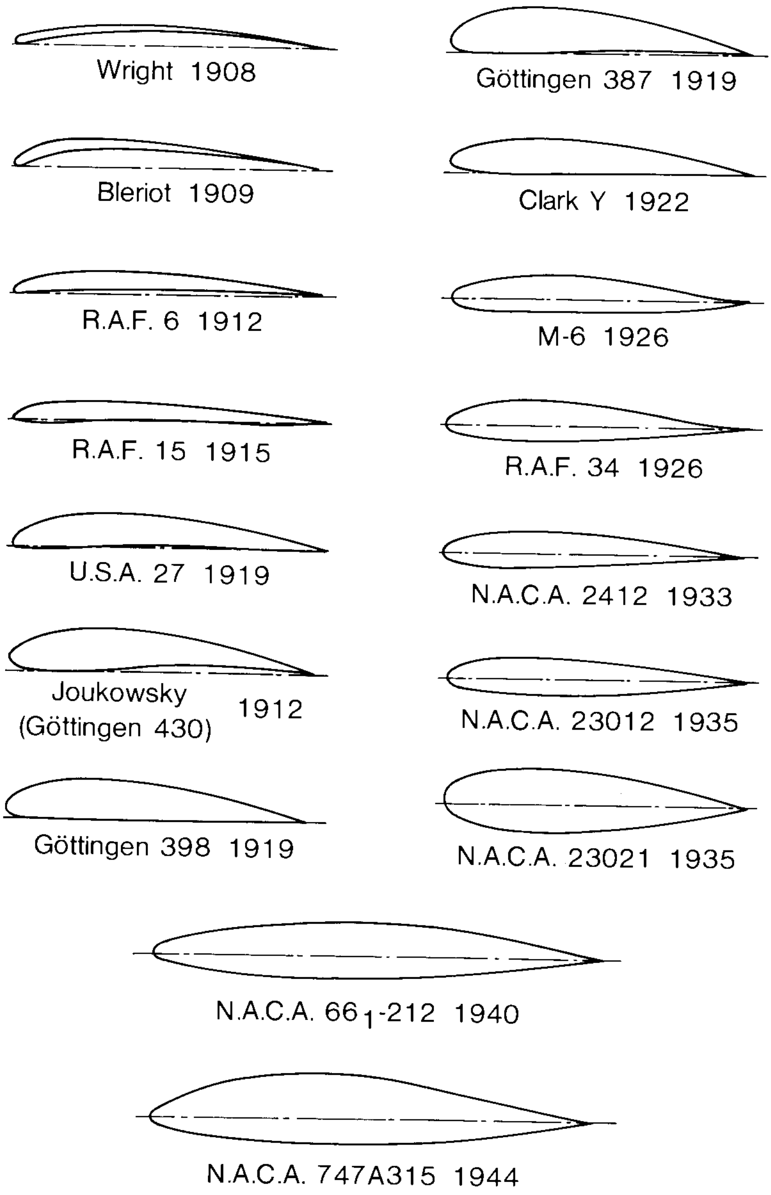
When Jacobs returned to the laboratory from his short working vacation, he challenged his staff to apply Theodorsen's theory in design. H. Julian "Harvey" Allen, one of the brightest members of the VDT staff, came up with one means of inverting the theory based on a linearization that started from a thin Joukowski airfoil. Applicable only to thin sections, Allen's way proved too inaccurate near the leading edge for prediction of local pressure gradients.[37]

No one in the VDT section had any special training in advanced mathematics of the sort required, which prompted a few of the men to approach Theodorsen's Physical Research Division for assistance. According to Ira Abbott, another key member of Jacobs's staff: "We were told that even the statement of the problem was mathematical nonsense with the implication that it was only our ignorance that encourages us."[38] Theodorsen himself went to the trouble of showing that the shapes likely to result from an inversion of his theoretical method would be "unreal," things that looked like figure eights and surfaces that crossed over one another.[39] Encouraged now by hearing this negative peer response, Jacobs stubbornly persisted in directing an all-out effort to devise a satisfactory inversion of the Theodorsen method. (In fairness to Theodorsen, it must be noted that he eventually contributed to solving the problem.)[40]

The breakthrough in this effort came in the spring of 1938, during the construction of the icing tunnel. The inversion, which Jacobs now says he modeled after Isaac Newton's clever method of approximating a square root, consisted essentially of changing a function in small increments in the conformal transformation of Theodorsen's theory. By taking an ordinary wing section, like the N.A.C.A. 0012, and "running it backwards," that is, designing its nose features according to the shape principles of the tail and its tail features according to the nose, Jacobs's team was able to arrive at an approximate shape that had falling pressures over most of the surface.[41] It is impossible to document whether this single spectacular approximation ever took place; the inversion procedure may in fact have been a gradual refinement. Jacobs's role is not in dispute, however; he was the inspiration and driving force behind the entire laminar-flow program.

After verifying its pressure distribution theoretically, Jacobs rushed the manufacture of a wind tunnel model through the LMAL shop. As soon as the new low-turbulence testing equipment was ready for operation, he supervised a test of the new model in comparison with a conventional airfoil. To his delight, "the new airfoil showed a drag on the order of one-half that of the conventional airfoil."[42] The result pleased him for two reasons especially: it provided empirical verification that inversion of the Theodorsen theory worked—something that his rival Theodorsen had called impossible—and it further justified the construction of the controversial new VDT, which he had personally championed through strong opposition.

Inspired by this success, Jacobs and colleagues explored further into the range of shapes theoretically enjoying laminar flows. By combining experimental knowledge with better ways of approximating solutions, they delineated a family of airfoils designated the 2-, 3-, 4-, and 5-series airfoils. Wind tunnel and free-flight work on some of these sections provided good qualitative information about the characteristics to be desired, but, because the mathematics was simply too approximate to show correctly the effects of changing such key parameters as the profile of the section near the leading edge, the work produced no practical airfoils. Much was learned, however,



The historical evolution of airfoil sections, 1908–1944. The last two shapes (N.A.C.A. 66_1-212 and N.A.C.A. 747A315) are low-drag sections designed to have laminar flow over 60 to 70 percent of chord on both the upper and the lower surface. Note that the laminar flow sections are thickest near the center of their chords.

and modified criteria evolved for development of the new 6-series.* In comparison with conventional sections, airfoils from this new series had the point of maximum thickness further aft along the chord. The point was prescribed in order to achieve the type of pressure distribution on the airfoil surface thought necessary for laminar flow.

In June 1939, the NACA distributed an advance confidential report prepared by Jacobs covering his new laminar-flow airfoils and explaining the methods his wind tunnel team had adopted for airfoil and boundary-layer investigations.[43] Though the Committee did not circulate the exact results of this research publicly until after World War II, a copy of the confidential report was sent to the Paris office of the NACA; John Ide burned it along with all of his other files before the Germans overran the city in 1940. German aeronautical engineers had reason to guess at the nature of the development anyway. On the first page of its *Annual Report* for 1939, published in 1940, the NACA hinted:

> Discovery during the past year of a new principle in airplane-wing design may prove of great importance. The transition from laminar to turbulent flow over a wing was so delayed as to reduce the profile drag, or basic air resistance, by approximately two-thirds.

Though admitting that it was still too early to appraise adequately the significance of this achievement, the NACA nonetheless suggested that its continued wing research should in the near future "increase the range and greatly improve the economy" of both military and commercial aircraft.

Beginning in 1940, Langley helped North American Aviation test fly its prototype of the P-51 Mustang, the first aircraft to employ the NACA laminar-flow airfoil.† Though the Mustang's war record confirmed

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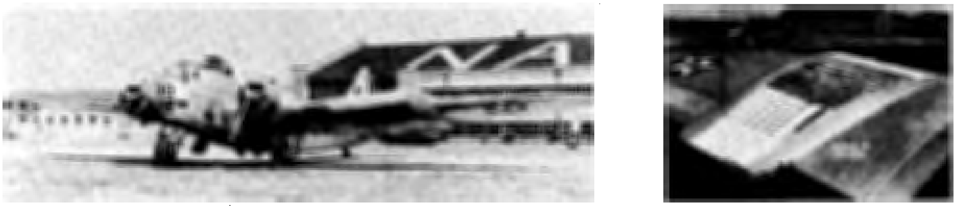
[*] Airfoils belonging to the 6-series were designated by a six-digit code together with a numerical expression of the type of mean line used. For example, in the designation "N.A.C.A. 65,3-218, $a = 0.5$," 6 was the series designation; 5 denoted the chordwise position of minimum pressure in tenths of the chord behind the leading edge for the basic symmetrical section at zero lift; 3 was the range of lift coefficient in tenths above and below the design lift coefficient for which favorable pressure gradients existed on both surfaces; 2 was the design lift coefficient in tenths; 18 indicated the airfoil thickness in percent of the chord; and $a = 0.5$ showed the type of mean line used. When the mean-line designation was not given after the sixth digit, a uniform-load mean line ($a = 1.0$) had been used. Ira H. Abbott, Albert E. von Doenhoff, and Louis B. Stivers, Jr., "Summary of Airfoil Data," TR 824, 1945.

[†] The P-51 had another interesting distinction: it was the first case of an aircraft's actual construction matching its aerodynamic design specifications without adding thickness in building the metal skin. The idea of the Mustang designer, a German perfectionist named Edgar Schmued, was to produce an airplane whose aerodynamic shape was the same as that decided upon by the aerodynamicist—not that shape plus an overcoat of lapped aluminum



In the spring of 1941 Langley installed an experimental low-drag test panel on the wing of a Douglas B-18 airplane. The panel, seen close up at right, was fitted with suction slots and pressure tubes for a free-flight investigation of the transition from laminar to turbulent flow in the boundary layer. The pressure at each tube was measured by liquid manometers installed in the fuselage.

expectations of appreciable improvements in speed and range as a result of the low-drag design, practical experience with this and other aircraft using advanced NACA sections in the 1940s also showed that the airfoil did not perform quite as spectacularly in flight as in the laboratory. Manufacturing tolerances were off far enough, and maintenance of wing surfaces in the field careless enough, that some significant points of aerodynamic similarity between the operational airfoil and the accurate, highly polished, and smooth model that had been tested in the controlled environment of the wind tunnel were lost.[44] Still, despite manufacturing irregularities and the detrimental effects of actual use, the Mustang's modified 4-series section, with its pressure distributions and other features, proved an excellent high-speed airfoil. The delineation of it and other laminar-flow airfoils was thus a great contribution by Langley, even if not exactly to the degree advertised by NACA publicists like George Gray, who claimed in *Frontiers of Flight* that "the shape of this new wing permitted the flow to remain laminar until the air had traveled about half way along the chord." According to Langley engineers who knew what it took in practice to achieve success, Gray's claim was an exaggeration. Because the percentage drag effect of even minor wing surface roughness or dirt increased as airfoils became more efficient, laminar flow could be maintained in actual flight operation only in a very small region near the leading edge of the wing.[45]

Though the NACA's high-speed airfoil work continued to be impressive in the late 1940s and early 1950s, this chapter's examination of its role in

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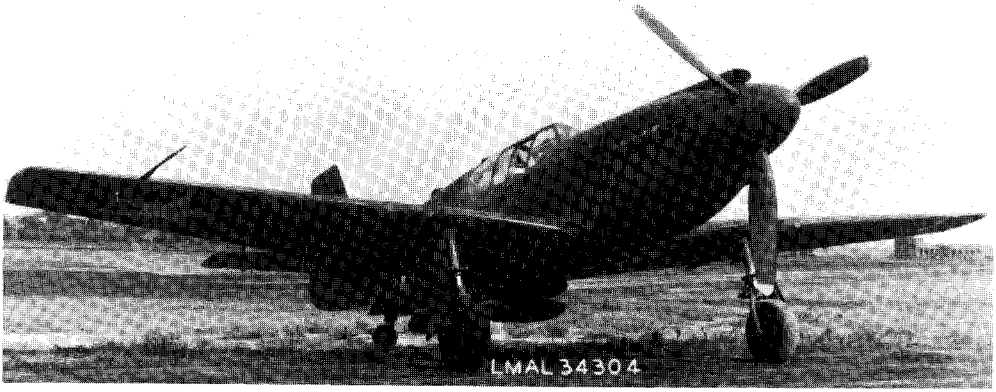
alloy that in places might add up to four sheets of thickness. The Mustang's faithfulness to profile was later exceeded by refined thicker-skinned aircraft like the Lockheed P-80 and F-104. See Richard P. Hallion, *Designers and Test Pilots* (Time-Life Books, 1983), pp. 78–79, 148–151, and Richard Sanders Allen, *Revolution in the Sky: Those Fabulous Lockheeds* (The Stephen Greene Press, 1967).



The North American XP-51 Mustang (shown above, in 1943) was the first aircraft to incorporate an NACA laminar-flow airfoil. In 1946, Langley equipped a P-51B (left) with wing gloves for an investigation of low-drag performance in flight.

the history of the VDT actually ends in 1939 with the cautious public announcement of the laminar-flow airfoil, a dramatic research success. By that time, the Committee's airfoil research had moved a full 180 degrees away from its unsatisfactory course of 20 years earlier, when a very small research staff with very limited technical capability and no operational test facilities of its own had mainly occupied itself accumulating, analyzing, and disseminating European data. Thanks in large part to the VDT and to its enhanced successor, the Two-Dimensional Low-Turbulence Tunnel, a much larger research staff worked at the cutting edge of modern experimental technology by the time of American entry into World War II. One result: wing sections developed by the NACA at Langley became by far the most widely used sections worldwide.

Postscript to VDT History

The history of Langley's variable-density wind tunnel would not be complete without some reference to the ultimate fate of the actual equipment. After replacing it with the Two-Dimensional Low-Turbulence Tunnel in the early 1940s, Langley continued to use the VDT, minus internal test instruments and mechanisms, as a high-pressure air storage tank for small high-speed induction tunnels. In 1981 the Pressure Systems Committee of NASA Langley Research Center closed the VDT pending its inspection and recertification for use as a tank, but Langley lacked the $30,000 needed to ready the old, riveted structure for inspection. In 1984 the National Park Service recommended that steps be taken to safeguard the VDT as a national historical landmark.

The fate of Max Munk, the father of the Langley VDT, should also be noted briefly. After leaving the lab in 1927, Munk seems to have "failed to repeat the brilliant record"[46] he achieved when the VDT and other NACA resources were available to him. He took a job with Westinghouse in Pittsburgh, where he tried to solve a cooling problem in electric motors. Then he worked a year for the American Brown Boveri Electric Corporation of Camden, New Jersey, and another year for the small Alexander Airplane Company in Colorado. In the late 1920s Munk asked the NACA to publish one of his articles, but the Committee rejected it for lacking clarity and rigor. By 1930 hard times had reduced him to writing "pathetic letters"[47] in which he styled himself "the foremost aerodynamic expert of the world" and declared that "all special scientific methods by which aircraft is [*sic*] computed nowadays, most experimental methods, and types of equipment have been originated by me."[48] During the Depression he became a consulting editor for the journal *Aero Digest*, and, in the opinions of George Lewis and others at NACA headquarters, contributed anonymously to its editorial campaign against the Committee.[49] Munk also taught mechanical engineering at Catholic University part time and educated himself in patent law.

It is not widely known that Munk proposed to design another new wind tunnel for the NACA. In July 1939, as Nazi Germany prepared to invade Poland, Munk wrote a letter to NACA chairman Joseph Ames, the man who had arranged for Munk to come to America in 1920 and work for the NACA, saying that he knew how to design "the ideal, the most efficient, most practical, most useful and most impressive piece of equipment" for the study of high-speed airplane problems. He suggested that the NACA might make use of his knowledge by rehiring him as an employee or by special contract.[50]

NACA member Jerome C. Hunsaker, the other principal actor in Munk's immigration to America, answered Munk for Ames, who was very sick, with a one-sentence letter: "I have read your letter to Dr. Ames about a proposed wind tunnel, but unless you can disclose something of the ideas behind it, I don't see how anything can be done about it." Two weeks later George Lewis wrote Munk, suggesting that he "submit for the Committee's consideration general or detailed plans" of the proposed device.[51]

Munk swallowed hard, for he had had a very serious falling-out with Lewis in 1927, and sent the director of research a contrite letter in which he proposed in vague terms the design of a "new type of wind tunnel," the same phrase Munk had used in 1921 to describe the VDT. This new tunnel was to be at least 32 feet in diameter at the throat and have a 20,000-horsepower motor capable of providing 400-mile-per-hour low-turbulence airflow. Munk estimated its cost at $1.5 million.[52]

Lewis, pressed by the heavy schedule of preparing the expanding NACA organization for wartime research and development activities, was slow to act on Munk's proposal.[53] Eventually he did ask Langley's foremost designer of large atmospheric wind tunnels, Smith DeFrance, for a quick appraisal of Munk's idea, to be based on the correspondence. DeFrance reported back that a device of the size and speed suggested could not attain 400 miles per hour with only 20,000 horsepower, and probably would not be of much value to the Committee even if it could. "It is apparent that Dr. Munk has in mind testing single-engine pursuit ships at full-scale and at what he may consider to be full speed," DeFrance asserted. "However," he went on, "from experience at Langley Field, it is safe to say that results obtained at 250 MPH can be extrapolated to 400 MPH, provided compressibility effects are disregarded." As for determining compressibility effects, DeFrance argued that either of the two new 16-foot wind tunnels authorized for construction by the NACA in 1939 (one at Langley and the other at the new Ames laboratory in California) would supply the necessary information.[54]

Munk later submitted a more formal and specific contract proposal for the design of his new tunnel, but in May 1940 the NACA rejected it.[55] Munk then asked Vannevar Bush, who had replaced the ailing Ames as NACA chairman in October 1939, to reestablish his old technical assistant position at NACA headquarters and to appoint him to it. Bush, after looking into the NACA's past problems with the imported aerodynamicist, turned down that idea as well.[56]

Munk had to remain content writing articles for *Aero Digest* and teaching part time at Catholic University. Beginning in 1945, he went to work as a research physicist at the Naval Ordnance Laboratory, contributing reports on the mechanism of turbulent fluid motion. He returned to Catholic

Max Munk at his home in Rehoboth Beach, Delaware, 1981.

full time in 1958, retiring in 1961. In the mid-1970s, the American Institute of Aeronautics and Astronautics (AIAA) honored Munk with one of its awards. In 1977, Munk published a small book at his own expense in which he claims to have provided the proof of Pierre de Fermat's "Last Theorem," which has baffled the mathematical profession for over 300 years.[57] In 1985, 95-year-old Max Munk was still living and still in good health, if with failing eyesight, in Rehoboth Beach, Delaware. He enjoyed discussing with visitors the etymology of words, especially Greek derivatives, and quoted at length from the works of Arthur Schopenhauer.

The consequences of the NACA's first and later rejection of Munk on the quality and vision of subsequent research at Langley laboratory are still a matter of debate. Basic work in theory seems to have declined for a while at Langley following Munk's departure; over the years there would be few researchers at the NACA who spoke the language of higher mathematics. On the other hand, the overall quality of NACA research in the 1930s seems not to have declined but, in fact, to have risen.

There is far less contention about the consequences for Munk himself: the impact of the revolt on Munk's life and career after 1927 was tragic. His notorious departure from the NACA surely slowed the advance of his ideas within the American community of aeronautical engineers. The NACA, which so often in the early 1920s had touted Munk as its most brilliant and productive staff member, now treated him virtually as a nonperson. Many technical reports published by the NACA that by rights should have credited Munk's earlier papers did not reference them at all. As a result, many members of the American aeronautics community supposed that Munk's work was irrelevant or out-of-date. They began to assume (perhaps correctly) that Munk had pretty well exhausted his supply of genius and vision by the time he left Langley. Moreover, they suspected from what they did hear from and about him that he was devoting far too much of his subsequent time and energies to flirting with exotic research topics (such as the Flettner rotor, a strange sailboat-like craft moved by vertical rotating sheet-metal cylinders—a concept which also interested Albert Einstein and Jacques Cousteau) and criticizing the research establishment for not investigating their potential benefits.

Perhaps the bitterness with which Munk remembered the revolt against him at Langley made him think he had something new to prove. Perhaps the hurt and anger did affect Munk's work adversely, if by *adversely* one means that by choosing to explore research problems offering largely imaginary benefits instead of those having the most urgent technological relevance, Munk failed to match the practical brilliance of his NACA contributions. In spite of all the reasons he might have had for holding a grudge against the NACA, Munk might have risen above them further than he did. As Samuel Johnson once said, "A man of genius is seldom ruined but by himself." On the other hand, America with its egalitarian society—with its egalitarian *engineering* society—is not an easy place in which to be a genius.

5

The Cowling Program: Experimental Impasse and Beyond*

One of the more urgent questions facing American aeronautical engineers in the 1920s was how to reduce the drag of radial engines without degrading their cooling. Soon after the end of World War I, the navy had become convinced that the air-cooled engine offered a more practical solution to its aircraft power-plant problems than did the heavier liquid-cooled engine—with its water jacket, radiator, and gallons of coolant—favored by the army. The jarring confrontations of naval aircraft with arresting gear on aircraft carriers resulted in too many cooling system maintenance problems at sea (e.g., loose joints, leaks, and cracked radiators). However, subsequent experience also made it clear to the navy's Bureau of Aeronautics that existing air-cooled designs wasted considerable power: projected into the external airstream for cooling, the finned cylinders of the radial engine caused high drag. Navy engineers attempted to reduce this drag by putting a propeller spinner (a rounded cover) over the hub and covering the crankcase and inner portions of the cylinders with a metal jacket, but this left the outer ends of the cylinders still jutting into the airstream.[1]

In June 1926 the chief of the Bureau of Aeronautics asked the NACA to determine how much a cowling could be extended outward over the cylinders of the radial engine in order to reduce drag without excessive interference with cooling. Less than a year later, during a technical session of an aircraft manufacturers' conference at Langley Memorial Aeronautical Laboratory, representatives from industry also asked the NACA for help in understanding the effects of cowling on the performance and cooling of radial engines.[2] The NACA responded to these requests by authorizing its laboratory, first, to conduct a free-flight investigation of the effects of various

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forms of cowling on the performance and engine operation of a Wright Apache (borrowed from the navy) and, second, to prepare a systematic program of cowling tests in its Propeller Research Tunnel (PRT), a brand new facility that made it possible for the first time anywhere to test full-size propellers and other aircraft components in a wind tunnel.[3]

The results of this research program are well known. In 1929 NACA Langley won its first Robert J. Collier Trophy, an annual award presented by the National Aeronautic Association for the year's greatest achievement in American aviation, for the design of a low-drag cowling.[4] By the mid-1930s the laboratory had designed a family of streamlined cowlings that not only reduced drag dramatically but actually improved engine cooling as well, an accomplishment that confounded the previous engineering intuition that had stuck those finned cylinders directly into the airstream. What is not very well known, however, is the history of the *method* of the NACA's successful cowling research and, more specifically, the fact that the engineers at Langley who used that method so well in the late 1920s and early 1930s eventually met and had to overcome what can only be described as an experimental impasse, a position from which there seemed no empirical way out. That history is the subject of this chapter.

Parameter Variation

The primary method employed by the NACA engineers in their cowling research was *experimental parameter variation*—"the procedure of repeatedly determining the performance of some material, process, or device while systematically varying the parameters that define the object or its conditions of operation."[5] When a complex research problem needs practical solution, and hypotheses are more scattershot than pinpoint because complex understanding is still a distant goal, this technique systematizes the pragmatic researcher's only real choice for a course of action: a combination of brain work, guesswork, and trial and error. By observing the effects of slight changes made one at a time in planned, orderly sequence, he can add progressively to his knowledge about the actual performance of whatever he is investigating. Seeking effects now and saving causes for later, he uses what he does know, circumvents what he does not know, and discovers what will work.

The method is ancient. Greek military engineers varied the parameters of full-scale machines to find the most effective dimensions for their catapults.[6] During the Industrial Revolution, engineers used the method to explore the performance of new construction materials and steam engines.[7] The success of the first powered airplane in 1903 followed application of

Originally the Wright Apache had a propeller spinner over the hub and a metal jacket covering the crankcase and inner portions of its engine cylinders. Configured as a seaplane, the Apache in the photograph at the top prepares to take off from the Little Back River. Lower left, an LMAL test pilot prepares to fly the Apache to high altitude. (In 1929 navy pilot Apollo Soucek set a world altitude record of 40,366 feet in the same type of plane.) Lower right, NACA mechanics install cowling for testing, 1928.

the method's fundamentals by the Wright brothers while testing airfoils in their wind tunnel.[8] The success of Langley laboratory's own airfoil research in its Variable-Density Tunnel, discussed in the two previous chapters, also hinged on parameter variation. The method has been used for so long by so many different types of engineers precisely because it permits solution of a complex problem without a complete understanding of all aspects of the problem.[9]

The growth of the method of Langley's cowling research from 1926 to 1936 and beyond can be divided into four stages: (1) definition of the cowling's parameters, ending in 1929 with the public announcement of a successful low-drag design; (2) 1929 to 1931, encompassing an important series of engine placement and free-flight cowling tests that resulted in a strong identification throughout the NACA with the empirical method; (3) 1931 to 1934, when the laboratory began by outlining a new three-pronged experimental attack on cowling and cooling problems, but ended in an impasse with that attack stalled; (4) 1934 to 1936 and beyond, when a more analytical approach to cowling research began to emerge out of this stalemate to answer some of the basic questions that the empirical approach of the preceding stages had left unanswered. Experimental parameter variation had led to results. Practical use had been made of observed performance effects; now it was time to search beneath them for those causes that had been postponed for later. It was time to go after that distant goal of complex understanding.

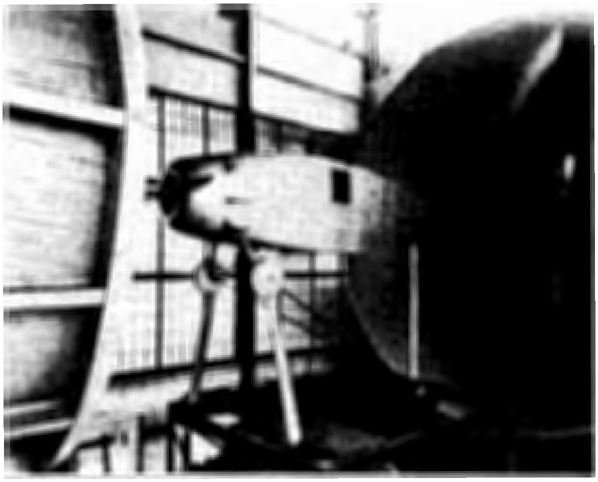
LMAL engineer Fred Weick, the former employee of the navy's Bureau of Aeronautics who had become a principal designer and head of the Propeller Research Tunnel, was already very familiar with parameter variation when he used it as the basis of the first-stage attack. As a senior engineering student at the University of Illinois, Weick had based a paper on variable-pitch propellers on data from "the first aerodynamic study under NACA auspices" to employ the method of experimental parameter variation—data reported by professors William F. Durand and Everett P. Lesley from model air propeller tests in the Stanford University wind tunnel.[10] Later, one of the first things Weick had done after joining the Bureau of Aeronautics in 1924 was "to work out a simple system of blade-element analysis using only a single element ... but obtaining the airfoil lift and drag characteristics by working the analysis backward" from the Durand-Lesley propeller data. In 1926, Weick reported the details of the method in NACA Technical Notes (TNs) 235 and 236.[11]

Recognizing that he should extend the cowling investigation well beyond the range of immediate interest, Weick pinpointed the extremes. Obviously, one extreme was a bare engine with no cowling at all; everyone who

Fred E. Weick, head of the Propeller Research Tunnel section, 1925–1929.

knew anything about aerodynamics assumed that it would have maximum cooling, but maximum drag as well. The value of the other extreme—enclosing the engine completely—no one had anticipated because that form seemed to exclude all possibility of air cooling. For smooth flow around the exterior of the cowl, Weick's team of engineers in the PRT modeled an engine nacelle on the best available airship form. Then the amount of cowling was systematically varied from extreme to extreme, and ten different cowlings for experimentation resulted.[12]

The test program proceeded easily enough—its goal being a cowled engine that would be cooled just as effectively as one with no cowling whatsoever. The PRT team mounted the Apache's J-5 Whirlwind engine in the tunnel, measured the cooling effectiveness of each of the ten cowlings, and investigated their effects on propulsive efficiency. Each experimental shape underwent numerous, systematically planned variations. With the help of Elliott G. Reid, the head of the Atmospheric Wind Tunnel section, who had been studying the effects of Handley-Page wing slots, the team designed a cowl that brought outside air in and around the engine via a slot



By the end of September 1928, tests of cowling no. 10 in the PRT showed a dramatic reduction in drag.

at the center of the nose. The potential of a complete cowl then began to look more enticing. Researchers had to modify the cooling air inlet several times, and had to install guide vanes or baffles to control the air in its passage for a more efficient heat transfer. They also had to design an exit slot that released the air at a slightly higher velocity and lower pressure than it had entered the cowling with, but they finally obtained satisfactory cooling with a complete cowl (called "no. 10") that entirely covered the engine and used slots and baffles to direct air over the hottest portions of the cylinders and crankcase.[13]

To everyone's surprise, the no. 10 cowling reduced drag by a factor of almost three! The results of this first portion of cowling tests at Langley were so remarkable that the NACA made them known to industry at once. In November 1928 the Committee published Technical Note 301, "Drag and Cooling with Various Forms of Cowling for a 'Whirlwind' Engine in a Cabin Fuselage," by Fred Weick. In it, Weick argued that use of the form completely covering the engine was "entirely practical" under service conditions, but warned that "it must be carefully designed to cool properly."[14] The NACA then announced to the press that aircraft manufacturers could install the low-drag cowling as an airplane's standard equipment for about $25 and that the possible annual savings from industry's use of the invention was in excess of $5 million—more than the total of all NACA appropriations through 1928.[15]

With the initial round of wind tunnel investigations completed, Langley borrowed a Curtiss Hawk AT-5A airplane from the Army Air Service, fitted it with the J-5 engine, and applied cowling no. 10 for flight research. The Hawk's speed increased from 118 to 137 miles per hour with the low-drag

The Curtiss Hawk with NACA cowling, November 1928.

cowling, an increase of 16 percent. The results of the instrumented flight tests had enough scatter for Langley to have been justified in claiming a 20-mile-per-hour speed increase instead of 19, but the NACA kept its advertised figure conservative.[16]

Effectiveness of the cowling was demonstrated to the public almost immediately. In February 1929 Frank Hawks, who was already famous for his barnstorming and stunt flying, established a new Los Angeles–to–New York nonstop record (18 hours, 13 minutes) flying a Lockheed Air Express equipped with an NACA cowl that increased the aircraft's maximum speed from 157 to 177 miles per hour. The day after the feat, the Committee received the following telegram:

> COOLING CAREFULLY CHECKED AND OK. RECORD IMPOSSIBLE WITHOUT NEW COWLING. ALL CREDIT DUE NACA FOR PAINSTAKING AND ACCURATE RESEARCH. [signed] GERRY VULTEE, LOCKHEED AIRCRAFT CO.[17]

A few months later, the NACA won its first Collier Trophy, for the greatest achievement in American aviation in 1929. This pleasant recognition not only promoted the cowling's economic value and justified the NACA's decision to build the PRT; the award was also timely support for the NACA's request for money to build a full-scale tunnel.[18]

A second stage of systematic cowling research had begun in late 1928—even before the public acclaim—and involved tests with several different forms of cowling, including individual fairings behind and individual hoods over protruding cylinders, and a smaller version of the new complete cowling, all mounted on an open-cockpit fuselage. The researchers at Langley also performed drag tests with a conventional engine nacelle and

The record-breaking Lockheed Air Express with NACA cowling, 1929.

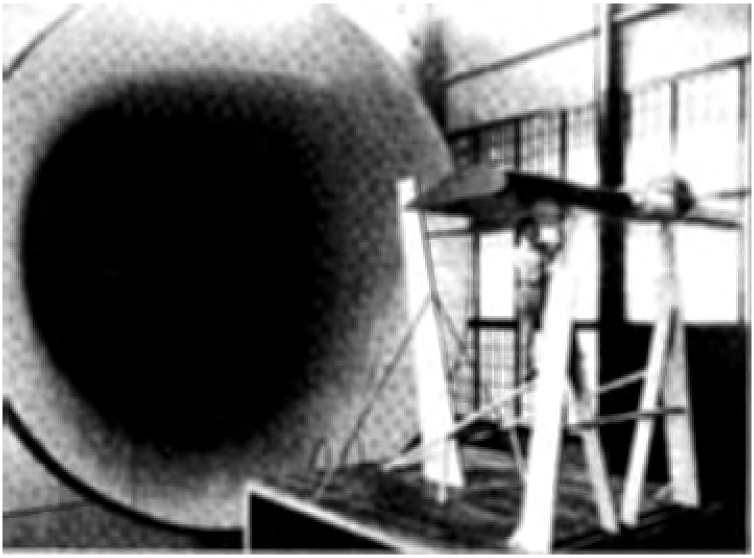
President Herbert Hoover presents the Collier Trophy to Joseph Ames, chairman of the NACA, in 1929. Three years later, as part of his plan to increase efficiency in government, Hoover would sign an executive order to abolish the NACA. (See next chapter.)

with a nacelle having the new complete design. The individual fairings and hoods proved ineffective in reducing drag, and it was found that for a smaller body as opposed to a fuselage with larger cabin, the complete cowling reduced drag more than twice as well as the conventional cowling did. Data from the AT-5A flight tests confirmed this conclusion.[19]

The LMAL flight crew installs an experimental low-drag cowling on the Fokker trimotor, 1929.

In 1929 Langley mounted its low-drag cowling on the engines of a Fokker trimotor. When comparative speed trials proved extremely disappointing, the engineering staff started to wonder how the position of the nacelle with respect to the wing might affect drag. In the case of the Fokker (as well as the Ford) trimotor, the original design location of the wing engines was slightly below the surface of the wing. As the air flowed back between the wing and nacelle, and the distance between them increased toward the rear of the nacelle, the expansion required was too great for the air to flow over the contour smoothly. The PRT team tried fairing-in this space, but achieved only a small improvement.[20]

Nevertheless, the lab's systematic, empirical approach soon yielded its dividend. With the help of his assistants, Fred Weick laid out a series of model tests in the PRT with NACA-cowled nacelles placed in 21 different positions with respect to the wing—above it, below it, and within its leading edge. The resulting data on the effect of the nacelle on the lift, drag, and propulsive efficiency of the airplane made it clear that the optimum location of the nacelle was directly in line with the wing, and with the propeller fairly well ahead. Although their primary emphasis was on drag and improved cooling, the tests at Langley also confirmed that a complete cowling of the radial engine, if situated in the optimum position, could in some cases actually increase the maximum-lift coefficient.[21] The NACA transmitted

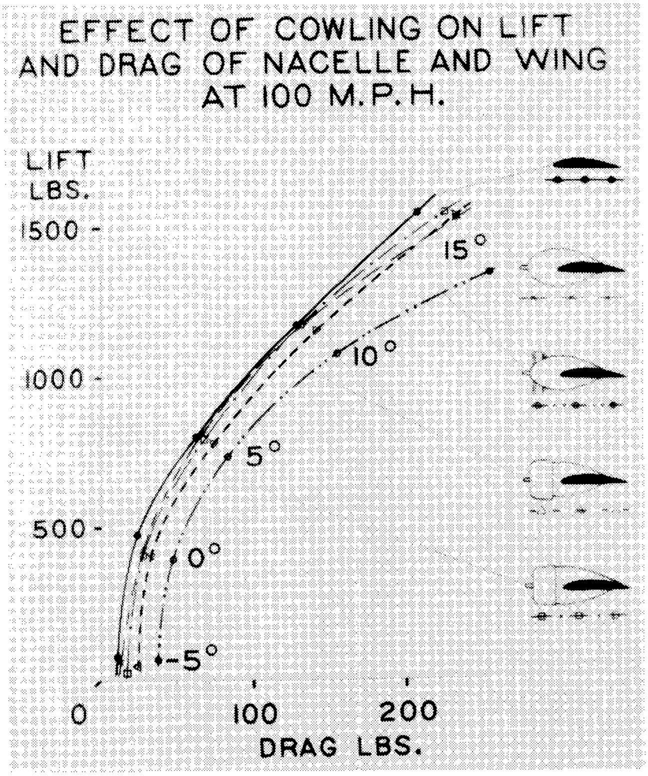


An engineer in the PRT tests the aerodynamic effects of nacelle position with respect to the wing, May 1930.

these results confidentially to the army, navy, and industry. (This private transmission was very significant: it gave U.S. industry several months lead time over European aircraft builders.) After 1932 nearly all transport and bombing airplanes with radial, wing-mounted engines—including the DC-3, the B-17, and many other famous aircraft of the era that followed—used the NACA cowling and located the nacelles with reference to the NACA data.

Momentum or Inertia?

The cowling was winning so much respect in the late 1920s and early 1930s that the NACA seemed to have gradually identified itself more and more with the systematic experimental approach that had been the basis of that successful research. In 1930 the head of the Aerodynamics Division, Elton W. Miller, reported to engineer-in-charge Henry Reid that "an effort is being made throughout the Laboratory to conduct every investigation in as thorough and systematic a manner" as the cowling program.[22] The following year, George Lewis in Washington told Reid to frame and hang in his office or along the corridor of the LMAL administration building a copy of a quotation from a recent speech by President Hoover in praise of Thomas Edison:



The NACA used this chart at its manufacturers' conference in May 1930 to demonstrate the advantageous effects of various cowlings on the lift and drag of a nacelle-wing combination.

> Scientific discovery and its practical applications are the products of long and arduous research. Discovery and invention do not spring full-blown from the brains of men. The labor of a host of men, great laboratories, long, patient, scientific experiments build up the structure of knowledge, not stone by stone, but particle by particle. This adding of fact to fact some day brings forth a revolutionary discovery, an illuminating hypothesis, a great generalization or practical invention.[23]

Clearly the pattern of work behind the cowling—the NACA's greatest public success to date—was contributing to a clearer sense of institutional identity and mission.

At least one contemporary observer saw this identification with systematic engineering as unflattering to Langley laboratory. Frank Tichenor, the outspoken editor of the journal *Aero Digest* who had hired Max Munk, labeled the NACA cowling "a development rather than an original work" and misjudged it as being far less effective than the Townend ring, a rival concept developed simultaneously by Hubert C. Townend at the British National Physical Laboratory.[24] Though the NACA can perhaps be criticized for trying to take too much credit for industry's adoption of the cowling, one must underscore the truth that the NACA never really claimed to have invented the cowling. It professed neither conceptual originality nor revolutionary development. What the NACA did claim—and what seems beyond dispute—is that the PRT permitted engineers to work with full-scale cowled engines. Better experimental equipment had led to more comprehensive and more useful data. It is not so clear in retrospect, however, whether the NACA's commitment to the pattern of experimental parameter variation for the next stage of cowling research signified technological momentum, or technological inertia.

The third stage of cowling research, 1931–1934, began at Langley when many more aircraft manufacturers decided to adopt the NACA design as standard high-performance equipment. A few companies did rather well with their applications of the NACA no. 10 cowling, especially those that put a series of adjustable flaps around the circumference of the metal jacket in the hope of better regulating the release of used air. (Those that tried to encourage more cooling flow by employing larger exit openings failed, however, sometimes to the point of nullifying the external drag advantage.) With the development of twin-row engines such as the Pratt and Whitney R-1830 of 1933 and 1934—with one row of cylinders *behind* the other—whole new problems arose.[25] This situation challenged Langley to obtain more trustworthy data on the general aerodynamic properties of the proven NACA design. Practical results had been obtained from experimental parameter variation, and they had been used profitably. Now it was time for a clearer understanding of them, so that still more results could eventually be achieved.

Three major branches of the laboratory became involved in the ambitious program. The Power Plants Division worked to improve the efficiency of radial engine cooling by varying such engine parameters as pitch, width, thickness, and shape of the fins. The 7 × 10-Foot Wind Tunnel section, using small models, sought the best possible cowling arrangement for necessary cooling with minimum drag by streamlining the front and rear openings, changing the size of the nacelle, and altering the camber of the cowling's leading edge. The PRT team was then to verify the results of the tests made by the other two groups. Full-scale propeller-cowling-nacelle units were to be tested under conditions of taxiing, takeoff, and level flight.[26]

Though the first two parts of the program advanced without much difficulty, the PRT tests—the final and most important part—ran into major problems soon after starting in 1933: the 100-mile-per-hour tunnel could simulate only the climb speeds of the cowled engine being used (a borrowed Pratt and Whitney Wasp); the obsolete shell-type baffles employed to deflect cooling air toward the hottest parts of the engine were too loose for the NACA researchers to work with effectively;[27] and, more importantly, certain anomalies that no one at the lab could explain plagued the cowling drag measurements. Together these problems contributed to a growing "maze of contradictory data" about cowlings. Despite five years of NACA experimentation and three years of general industrial flight test experience, American aeronautical engineers felt a "general suspicion" that there was "something mysterious or unpredictable determining the efficiency of engine cowling."[28]

Analytical Help

To move beyond the paralyzing confusion of this experimental impasse, Langley's cowling research needed some analytical help. It was eventually provided by the head of the laboratory's small Physical Research Division, Theodore Theodorsen. A Norwegian-born engineer-physicist with a trigger mind and tremendous power of concentration, Theodorsen had already seen in Langley's pattern of airfoil testing in the VDT the need for experimental routine to be fertilized with a stronger dose of theory (as the terms of his opposition to Eastman Jacobs's idea for a new low-turbulence VDT, outlined in the previous chapter, plainly showed). In the curious introduction to his seminal 1931 report on the "Theory of Wing Sections of Arbitrary Shape"—curious at least in an NACA report for stating a bold personal opinion and implicitly taking part of the parent organization to task—Theodorsen had asserted that

> a science can develop on a purely empirical basis for only a certain time. Theory is a process of systematic arrangement and simplification of known facts. As long as the facts are few and obvious no theory is necessary, but when they become many and less simple theory is needed. Although the experimenting itself may require little effort, it is, however, often exceedingly difficult to analyse the results of even simple experiments. There exists, therefore, always a tendency to produce more test results than can be digested by theory or applied by industry.

What Theodorsen believed the NACA needed in order for it to move beyond the impasse temporarily blocking the progress of its experimental cowling program was more attention to the "pencil-and-paper" work that could lead to a complete mathematical and physical understanding of the basic internal and external aerodynamics of the different cowling shapes.[29] And what this meant in terms of the history of Langley's method of cowling research was

Theodore Theodorsen, head of the small Physical Research Division, prepares to give a conference talk on the physics of a four-blade propeller in 1945.

a turning away from experimental parameter variation, and toward that distant goal of complex understanding.

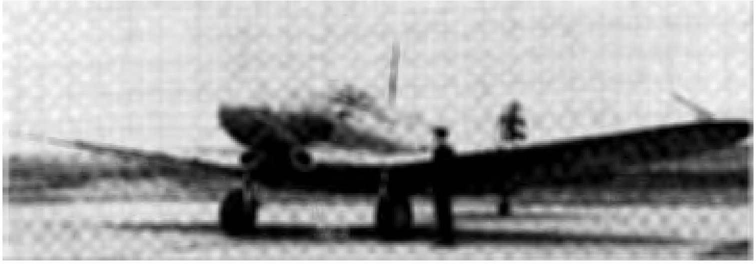
Theodorsen first perceived new cream to be skimmed off the top of the old cowling and cooling investigation while serving on the editorial committee that reviewed the draft report on the tests of the full-scale propeller-cowling-nacelle units in the PRT. After pointing to the blunt afterbody of the nacelle as the probable source of the anomalies that had been observed in the drag data, he suggested to his colleagues that the stalled cowling program could be completed as planned (and his resolution of the drag anomalies verified) by a new, more comprehensive and analytical full-scale investigation. Its aim, underscored Theodorsen, would be both to improve basic understanding of the obscure cooling mechanisms of the cowled engine and to put the understanding of the relationship between internal flow and drag on a more rational basis. The provocative suggestion was adopted; the engineer-in-charge transferred most of the cowling work and some of its key workers to Theodorsen's division.[30]

Previously the PRT research team had focused almost entirely on the net or overall effect of the cowling on drag and engine temperatures. What Theodorsen now proposed was to investigate the fundamental flow involved. In part, the approach of Theodorsen's new cowling research team still followed that of experimental variation. The Wasp engine having proved inadequate as part of the test bed, they built a full-scale wind tunnel model with a dummy engine, which had one cylinder heated electrically. Numerous combinations of more than a dozen nose shapes, about a dozen skirts, six propellers, two sizes of nacelles, and various spinners were tested. But hoping to produce a detailed handbook by which designers could better understand the actual functioning of the NACA cowl, they also included extensive measurements of pressure in both the external and internal flows.

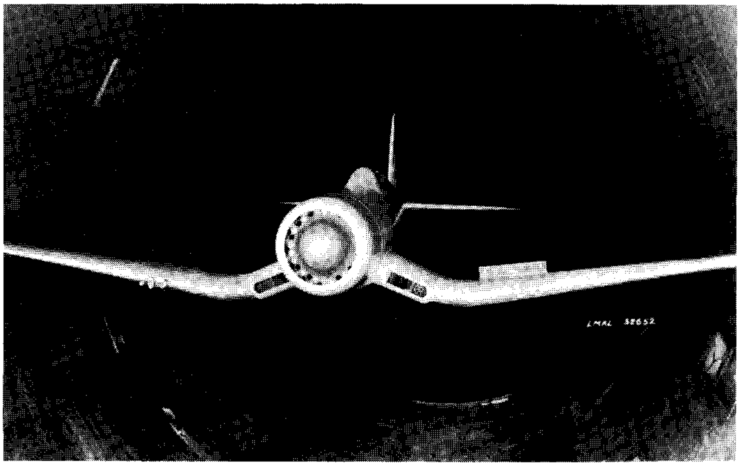
Langley's revised cowling program thus remained primarily experimental, but it now also allowed quantitative analysis and computation of these flow pressures. This quantitative analysis, which had been lacking in the previous work, eventually produced some new NACA cowling designs, but more importantly it provided solid answers to virtually all the remaining questions about the fundamental principles of the cowling and cooling of radial engines.[31] It demonstrated conclusively that the early NACA designs had been "quite haphazard and often aerodynamically poor" and had cooled the engine successfully only by a crude excess of internal flow and internal drag (a conclusion that Vought engineers had apparently arrived at on their own, earlier, on behalf of Pratt and Whitney and its R-1830 engine).[32] Designers of future cowlings, like airfoil designers, would have to be much more sensitive to such subtleties as the ideal angle of the cowling's leading edge attack on the local airflow. The work even demonstrated as fact something that everyone had unconsciously assumed to be physically impossible when the cowling research began in 1926: a proper engine cowling could, by making the enclosed baffled engine act in essence as a ducted radiator for cooling, lower operating temperatures more than could full exposure of cylinders in the airstream. With an understanding at once basic and advanced, the national aeronautical establishment could now begin to focus on more specific, higher-speed applications of cowlings, work that would be essential to the design of military aircraft used by America and her allies during World War II.

Evaluation of the Research Method

The history of the NACA's cowling research from 1926 to 1936 celebrates a victory but also demonstrates an important general point about research: No matter how practical or otherwise advantageous any one method may be, it always has some disadvantages. Systematic parameter variation had enabled the researchers at Langley to delineate a cowling that significantly reduced the drag of a radial engine without degrading its cooling, but because initial success came rather quickly and easily, they did not have to understand exactly *why* the cowling worked. When questions and doubts



In the summer of 1939 the NACA tested an experimental cowling and cooling system on Northrop's A-17A attack plane.



A model of Vought's F4U-1 Corsair with high-speed cowling was tested in the LMAL 8-Foot High-Speed Tunnel in April 1943.

arose, and data seemed contradictory and mysterious, the original empirical method was unable to proceed. Only then did Theodorsen design the research program whose goal was an understanding that went far beyond the mere collection of overall performance data on a variety of promising but arbitrary shapes. The cowlings that resulted from the Theodorsen program did not beat the earlier shapes as regards external drag (which is only a weak function of cowl shape), but with the tight baffles, small exit areas, and low internal drag made possible by the NACA's new criteria of understanding, the total drag of Theodorsen's shapes was dramatically less.

* * *

Historians have tended to treat the NACA cowling as a magical piece of tin wrapped around an engine and producing fantastic results. As a result, they have failed not only to appreciate the systematic character of the laboratory work which made the initial design breakthrough possible, but also to pick up on the later work by Vought and Theodorsen which made the important breakthrough in understanding possible. The success of the cowling was not due to magic. Nor was it the result of simple cut-and-try or advanced theory demonstrating its ultimate superiority over empiricism. Rather, the cowling was the product of fruitful engineering science: a solid combination of physical understanding, intuition, systematic experimentation, and applied mathematics.

Ultimate success in research is never inevitable, however. Without the help of Theodorsen or someone else with comparable analytical and mathematical talents, cowling research at Langley might have remained indefinitely at the point of impasse.

6

The Challenge of Teamwork

Langley managers understood the American aeronautics community as a team of four members: the universities and technical schools, which prepared future scientists and engineers for work in aeronautics; the NACA, which produced and distributed the new research results that made progress in aeronautical technology possible; the aircraft manufacturing and operating industries, which used research results in design, production, and routine flying of aircraft; and the military, whose requirements for advanced aircraft constituted the most acute challenge to the manufacturing industry and the NACA.

As the government's civilian member of the team, however, the NACA had to operate within perhaps the most challenging environment in the American aeronautics community, where teamwork was all too often the euphemism for political hardball and the push and shove of powerful interest groups. In such an arena the NACA could easily strike out if its managers had not prepared their players to stay away from wild pitches. The purpose of this chapter is to summarize the most serious political threats facing the NACA and Langley before World War II and, more importantly, to analyze major aspects of Langley's working relationship with its two major clients, the aircraft manufacturing industry and the military.

Surviving Political Threats

Gossamer wings had kept the NACA aloft in a turbulent atmosphere from the start. In 1915 Congress had established the Committee not, after all, because of any groundswell of public opinion, but rather to satisfy a few persistent advocates of such a step. Many of its supporters had opposed the construction of Langley laboratory, believing that it would duplicate work at existing government facilities. Those few Americans who had learned about the NACA during World War I had supported it as an instrument of national security and industrialized armed force; they perceived it after the Armistice as just another military branch in need of demobilization.[1]

Money matters plagued the NACA from its inception. After secretary John Victory's $1200 salary was subtracted from the first year's budget in June 1915, the Committee was left with only $3800. Fortunately, the comptroller decided that Congress had meant for $5000 to be immediately available—meaning until 30 June—and that a brand new $5000 was to be made available on 1 July. Six years later, this problem arose again. During the second session of the 67th Congress, the House Independent Offices Subcommittee asked whether the enabling act preempted the House's right to appropriate to the NACA any funds beyond that original $5000. With the 1915 decision as a precedent, the subcommittee resolved the question in the NACA's favor.[2] The NACA wisely kept its budget requests modest; until 1930, none exceeded a million dollars a year (see appendix C). Nonetheless, getting appropriations was always tricky business. Langley's construction was funded through legislative contrivance as part of a naval appropriations bill. This tactic, which had also been used to make the Committee's establishment possible in the first place, was followed for a few years. But after the Bureau of the Budget was created in 1922, the NACA had to fight the same battles for money and live by the same budget cycle as other branches of the federal government. Legislation regulated how the agency spent its money and transferred its funds from one account to another.[3]

As soon as World War I ended, a series of political maneuvers threatened the NACA's existence as an independent body. The Committee seems to have provoked the first threat by helping to draft and then support legislation in Congress that, if passed, would have "stopped just short of giving [the NACA] control over all aeronautical and aviation activities of the federal government."[4] House Bill 14061, introduced by Julius Kahn on 13 May 1920, provided for "the establishment of a Bureau of Aeronautics in the Department of Commerce, in charge of a Commissioner of Air Navigation whose duties will comprise the licensing of aircraft, pilots, and airdromes, the designation of flying routes, cooperation with the States and municipalities in the laying out of landing fields, and, in general, the promotion of all matters looking to the advancement of commercial aviation." All rules and regulations formulated by the new commissioner of air navigation were to be submitted to the NACA for consideration, criticism, and recommendation to the secretary of commerce. House Bill 14137, introduced by C. F. Hicks on 19 May 1920, offered an alternative to the Kahn proposal. Hicks would not only have created a Bureau of Aeronautics in the Department of Commerce, where the NACA would have had broad advisory responsibilities, but his bill would also have given the NACA the authority to consider "questions of policy regarding the development of civil aviation, with particular reference to education, preliminary training, commercial production of aircraft, establishment, elimination, and consolidation of flying fields and air stations" and "to recommend to the heads of the departments concerned the [transfer] of aircraft and aircraft equipment and accessories from one department to another for the civil uses of the Government." The Committee was also to "consider and report upon any question dealing with aviation referred to it by the President or by any of the departments, and ... initiate, report, and recommend to departmental heads desirable undertakings or developments in the field of aviation." Each department would "furnish the said Advisory Committee such information as to its aviation activities as may be requested."[5]

The purpose of both bills was to coordinate the government's multifarious aeronautical and aviation activities. Through its support of the legislation, the NACA offered to assume the major coordinating functions. This was a bold and risky step by an inexperienced agency ostensibly devoted to advice, not executive control, and the Committee barely survived the swift storm that blew up. Waving the old red flag of overlapping and duplicated effort in government, Senator William E. Borah (Rep., Idaho) introduced a joint resolution to abolish the NACA and to transfer its equipment to the Bureau of Standards and its land and buildings to the War Department.[6] Preoccupied with other business, Congress failed to act on the proposal (or on the Kahn and Hicks bills); however, in its pursuit of more control over civil aviation, the NACA had angered some old enemies and made some new ones.[7]

One of the most vociferous opponents of the NACA during the debate in the mid-1920s over national aviation policy was Brig. Gen. William "Billy" Mitchell, USA. Even before the dedication of the LMAL in June 1920 (a ceremony in which he had participated), Mitchell wanted to abolish the NACA. He wrote to the military attaché at the American embassy in Paris:

> It is difficult to handle this National Advisory Committee in any way. It does no good here nor any place that I can see.[8]

As a guest at a meeting of the NACA Executive Committee on 27 January 1921, Mitchell proposed that the Air Service buy all of the NACA buildings at Langley Field and move the research operations to Col. Thurman Bane's "Arsenal of Aeronautics" at McCook Field.[9] In 1925, before the House Select Committee of Inquiry into Operations of the United States Air

Service, the general argued that the NACA spent "large appropriations of money for matters that could be handled far better in a central engineering department." Seeing in the NACA a major obstacle to his idea for a separate air force, Mitchell again advocated its abolition.[10] After an inspection of Langley laboratory, however, the congressional committee found Mitchell's criticisms excessive.[11]

From 1926 to 1930 the NACA's public situation was as secure as at any time before World War II. Appropriations for the agency rose from $470,000 in 1925 to $1.3 million in 1930 (see appendix C). Mitchell's court-martial in October 1925, the publication of a favorable report on NACA activities two months later by President Coolidge's Aircraft Board, and the Lindbergh boom contributed to an overall improvement. So did the NACA's decision to keep out of the spotlight.

The critics of the NACA ended their sabbatical as soon as the Depression arrived, launching an attack on the Committee the equal of any that had come before 1926. Though it had a good reputation in government circles for fiscal responsibility—even for turning back unspent money to the Treasury—the NACA now had to convince skeptics that its efforts gave an adequate return for the precious dollar spent. In the December 1930 editorial "Why the NACA?" Frank Tichenor, editor of *Aero Digest*, portrayed the Committee as just another self-righteous and unenterprising federal bureau. He derided Langley laboratory as a second-rate organization, trapped in red tape; its staff, though working in what Tichenor called the world's largest, most expensive, and most modern facilities, had been unable to contribute "one research project of scientific value, and only few of technical value." "If the results of the NACA could be computed in dollars and cents," the editor chided, "the Committee would long ago have been bankrupt." In a March 1932 editorial entitled "Take Politics Out of Research," he in fact calculated the cost of an NACA research paper:

> The main results of the NACA's experimental research for the year [1931] is [*sic*] laid down in 13 technical papers. Attributing the [year's] entire expenditure [of roughly $1.4 million] to them we find their cost to have been in excess of $100,000 each. ... The world never has known more costly current literature than that.

With this absurd upbraiding of the NACA for "doing only one thing well," spending money, Tichenor urged Congress to "merge the NACA laboratories with those of the Bureau of Standards, with those of the Army Research Department at Wilbur Wright Field, or with those of the Naval Aircraft Factory at Philadelphia."[12]

On the heels of this editorial campaign came the most serious direct threat to the NACA's existence in its first two decades. On 9 December 1932, as part of his plan to reduce expenditures and increase efficiency in government by eliminating or consolidating unnecessary or overlapping federal offices, President Hoover signed an executive order to abolish the NACA—just as he had wanted to do a few years earlier as secretary of commerce. The original resolution that had become the Air Commerce Act of 1926 contained a provision insisted on by Secretary Hoover calling for the transfer of the NACA to the Department of Commerce. Though the provision was eventually removed from the bill, Hoover continued to believe in its wisdom. As a lame duck president, he finally acted on that belief.[13]

In its January 1933 editorial "Perhaps Farewell, Lewis and Victory," *Aero Digest* applauded Hoover's action. Editor Tichenor said that the NACA had ceased to be a research body and had become "an advertising club, a rest home, a comfortable refuge for the two who have controlled it."[14] This war of words against the Committee's director of research and executive secretary was so personal and bitter that the NACA staff thought that it saw *Aero Digest* employee Max Munk's hand providing the ammunition.[15]

The NACA responded to Hoover's order by soliciting the support of its most influential friends. Chairman Joseph Ames appointed a dozen men prominent in military and civil aviation (including Maj. Gen. Benjamin Foulois, chief of the Air Corps; Rear Adm. William A. Moffett, chief of the Bureau of Aeronautics; Edward P. Warner, editor of *Aviation*; Harry F. Guggenheim; and Orville Wright) to a Special Committee on the Proposed Consolidation of the National Advisory Committee for Aeronautics with the Bureau of Standards. As might be expected, they expressed strong opposition. Though not a member, Charles Lindbergh wrote a letter supporting the committee's report. He argued that the present, with its "rapid development in technical improvements and applications of aircraft to American commerce," was not the time "to make any move which would impair the efficiency" of the NACA.[16]

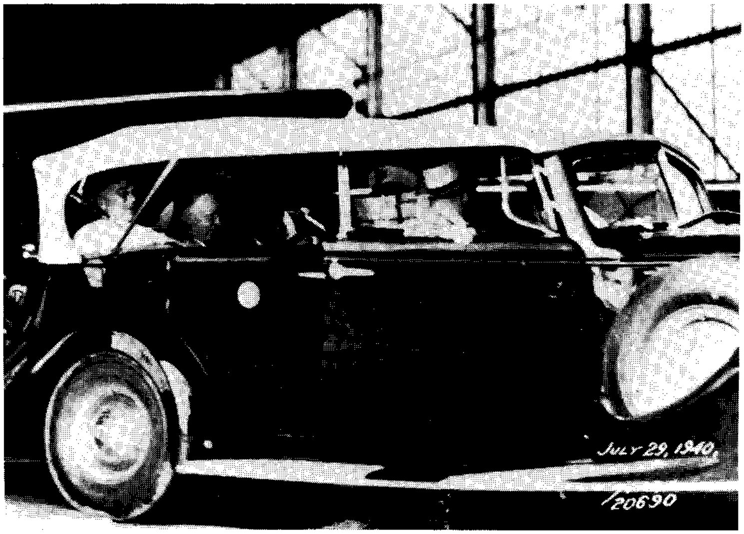
In January 1933 House Democrats voted unanimously to kill Hoover's mergers and left readjustment of the federal establishment to the new Roosevelt administration. In the heady days of the New Deal, critics of the NACA found little opportunity to threaten it with abolition. Budgets once again became the Committee's most serious political concern, Congress having refused in 1931 and 1932 to appropriate to the Committee a single penny for new construction. In 1933 and 1934, however, the NACA managed to get from the Public Works Administration nearly three-quarters of a million dollars for new construction at Langley. Part of the money was used to rehire some personnel. In 1936, the NACA's general purpose budget rose by more than 50 percent to over one million dollars for the first time since 1932. An article in the previously censorious *Aero Digest* related that the NACA was a "non-political organization of aeronautical experts" and that its research findings were based "not upon guesses or political expediency, but upon fact."[17] NACA Langley had survived the Depression.

The NACA's continuous existence from 1915 to 1958 as an independent organization of the federal government testifies not only to real merits in research but also to skill in the art of survival. Though the Washington office insulated the laboratory as much as possible from playing this political game, Langley had to be discreet in dealing with its clients. In particular, it had to respond effectively to calls for service from the aircraft industry and from the military.

Satisfying Industry

During World War I the NACA worked to stimulate the nation's aircraft industry. Between the wars, the Committee continued to give due consideration to the problems of business firms involved in designing, building, and operating aircraft. As matters of policy, the staff at Langley laboratory not only regularly investigated research questions peculiar to commercial aviation, but carried out its military-related programs in such a way as to make their results applicable to civil purposes. Industry could use idle research facilities for proprietary tests upon payment of the costs involved plus 100 percent. Excepting proprietary information, the NACA generally made its research findings known to all companies at the same time. When a test program suggested results of immediate interest to aircraft manufacturers prior to the publication of a formal report, the Committee issued the data to industry in advance. All technical reports were distributed to industry free of charge.[18]

The NACA's sustenance of the aircraft industry between the world wars was hardly carte blanche, however. To avoid any suspicion that it belonged to or sanctioned an aviation trust, the NACA in its first year of existence had decided that industry should have no direct representation on the Main Committee and only limited membership on the subcommittees. This decision reflected the NACA's acceptance of an earlier piece of advice from the assistant secretary of the navy, the young Franklin D. Roosevelt. Endorsing the House resolution behind the establishment of the NACA, Roosevelt had argued in 1915:



President Franklin D. Roosevelt visited Langley Field on 29 July 1940.

> The departments of the Government most interested in the development of aeronautics will be the ones that will be coordinated by the advice of this committee, individually carry out the work required, and be responsible for the expenditures of money appropriated by Congress. Therefore, the representatives of the Government should always have the controlling interest in the activities of the proposed committee. The interests of private parties must be more or less commercial and influenced by such considerations.[19]

As a result of such Progressive ideas, the aircraft industry acquired only incidental NACA membership—the consequence of the sporadic appointment of individuals who happened to be associated with industry. Such persons, the NACA asserted, were always selected on the basis of their unquestionable qualifications, and did not represent industry.[20]

Industry spokesmen occasionally challenged the NACA policy—especially in the early 1920s when manufacturers hoped for federal aid to the depressed aircraft market and bitterly opposed the NACA's unwillingness to advocate a separate air service.[21] The NACA responded by approving in principle industry's frequent representation on future ad hoc subcommittees, organized under the standing subcommittees—which themselves had significant informal industry representation—to consider specific problems. In 1936, for example, the NACA created a Special Committee on Problems of Transport Construction and Operation and convened a conference of airplane pilots to discuss the handling characteristics and piloting techniques of large transport planes. The membership of both included representatives of the principal American airlines, as well as representatives of the air services of the army and navy, the Bureau of Air Commerce, the Weather Bureau, and the NACA itself. Former Langley chief physicist Edward P. Warner, editor of *Aviation* and consultant to Douglas Aircraft Company, chaired the committee.[22]

Annual Aircraft Engineering Conferences

On 24 May 1926, 15 months after the Kelly bill had authorized the contract air transport of the U.S. mail and four days after the Air Commerce Act had assured small but consistent appropriations for the development and procurement of military aircraft, the NACA convened the first of what collectively became its most significant response to industry's request for service: the annual aircraft engineering conferences. Convinced that the advent of commercial aviation would generate a new series of aerodynamic problems, the Committee collected various representatives of the aircraft manufacturing and operating companies for a one-day, by-invitation-only tour of Langley laboratory. The meeting was intended to allow the LMAL technical staff to ascertain "the problems deemed of most vital importance" so that the NACA could incorporate them "as far as practicable" into its research programs. Held through 1939 (with the exception of a postponement in 1938 due to the extensive prewar construction of new research facilities), the conference became a regular conduit through which industry could make requests of the NACA. Discontinued by World War II, the conferences resumed in 1946 under a slightly different format.[23]

The way Langley organized and conducted the annual conference illustrates much about the organization's situation within the American aeronautics community. A combined technical meeting and public relations extravaganza, the conference provided an opportunity for the NACA to highlight its recent accomplishments before captains of industry and high-ranking military officers (groups whose members "seldom had time to read NACA technical reports"), to exchange information with the other leading minds in American aviation, and to bang its big drum before congressmen and other public officials (who "had neither the time nor the qualifications to read the technical reports and judge whether the agency's output justified its appropriations").[24] The event grew from a modest and relaxed affair in 1926, when the Committee sent out only 38 invitations, into a highly staged pageant that took weeks of preparation by the Langley and Washington office staffs. By 1936 the spectacle took two days, the first day's session "for executives and engineers of the aircraft and operating industries, and Government officials," the second for "personnel of the governmental agencies using aircraft, representatives of engineering societies, and members of professional schools."[25] Over 300 people attended each session, including a number of aviation writers who reported fully on the laboratory's presentations in newspapers and journals.

NACA Langley's handling of the annual conference reflected the shrewd political and administrative talents of its two executive officers, John Victory and George Lewis. Under their personal supervision, the event became the NACA's rite of spring. Victory was basically responsible for making sure that all the guests enjoyed themselves. All of the "important people" he gathered in Washington the day before the meeting, and in the late afternoon he escorted them aboard a steamer for a leisurely overnight trip down the Chesapeake Bay to Hampton. Mid-May was chosen to increase the chance for excellent weather. Cruise director Victory assigned cabins in a way that would facilitate easy exchanges of business and cordial conversation. He must have succeeded in creating a relaxing atmosphere despite "running around wearing his annual worried look." After the 1939 junket, an executive of the Curtiss Wright Corporation was reported as having successfully defended "the championship for having the wildest pajamas on the boat."[26] After docking at Old Point Comfort early in the morning, conference participants breakfasted in grand style at the Chamberlin Hotel with its view of Hampton Roads, and then proceeded to Langley Field some five miles away via automobile caravan (55 cars as early as 1930) escorted by Hampton motorcycle police. Victory seemed to be everywhere during the tour, smoothing over any rough spot that might appear (such as an uninvited guest), overseeing the place cards for lunch, and staging the group photograph. His most cherished moments, however, came after the conference adjourned, when participants were back at the Chamberlin for cocktails on its spacious verandas and for dinner, and later during the return steamer voyage to Washington.[27] (One menu at the Chamberlin during the Depression included fruit cocktail, celery and olives, essence of tomatoes, crabcakes mornay, half a broiled chicken, baked stuffed potato, fresh green peas, lettuce and tomato salad with French dressing, walnut ice cream and cake, and demitasse; the cost to the NACA was 85 cents a meal.)

The part of the program influenced most directly by George Lewis began with the morning's first technical session at Langley Field. After welcoming speeches in the post theater by the air base commander and the NACA chairman, Lewis heard his engineer-in-charge and his chief of aerodynamics summarize the laboratory's major investigations of the past

On the north shore of Hampton Roads, Old Point Comfort (top) and the Chamberlin Hotel (bottom) in the 1950s. In the hotel photograph, notice the moat of Fort Monroe to the right. (Courtesy of the Newport News Daily Press*)*

"Doc" Lewis, about 1935.

year. At 10 A.M. sharp the tour began. The visitors, organized into color-coded groups for compatibility of membership, were taken on a strict schedule through the various wind tunnels, shops, the hangar, and along the flight line. Lewis himself escorted one of the groups. At each location, a thoroughly prepared engineer demonstrated some current work in terms that Lewis and Victory had judged during rehearsals to be suitable for both expert and layman. No pains were spared in helping the visitor to visualize tests and understand results.

After lunch—originally in the base officers' club, but in later years in the Full-Scale Tunnel—key staff members, such as the heads of propeller and power plants research, offered more technical reports within special conferences, answered questions, and entertained comments. Here was industry's opportunity to recommend to the NACA new wind tunnel and free-flight tests. Though it was pretty hard to get the first individual to stand up in a large crowd of peers—and competitors—to suggest what the NACA should do in the future, the meetings stimulated plenty of ideas. Records show that conference guests from 1926 through 1939 offered hundreds of suggestions for research. At the 1935 conference alone, the NACA staff heard 72 different ideas.[28]

As years went by, however, less and less time was made available for audience participation. For weeks prior to the meeting, management supervised the preparation of talks and demonstrations by the LMAL staff

The annual industry conference grew from a small, modest affair into a large, orchestrated pageant. At the first conference in 1926 this photo was taken on the steps of the administration building. Those attending were, from left to right, (1) John F. Victory, NACA; (2) R. W. Brewer, Pitcairn Aviation, Philadelphia; (3) Andrew J. Fairbanks, LMAL; (4) William B. Stout, Stout Airplane Co., Dearborn, Mich.; (5) Thomas Carroll, LMAL; (6) A. E. Larsen, Pitcairn Aviation; (7) Harold F. Pitcairn, Pitcairn Aviation; (8) Lt. Ernest W. Dichman, Materiel Division, Air Corps, McCook Field, Dayton; (9) Jones (possibly either Charles S., Curtiss Flying School, Garden City, N.Y., or Ernest La Rue, Chief, Air Information, Aeronautical Branch, Department of Commerce); (10) Charles F. Pape, Hall-Aluminum Aircraft Corp., Buffalo; (11) J. S. Bray, Allison Engineering Co., Indianapolis; (12) Charles W. Lawrance, Wright Aeronautical Corp., Paterson, N.J.; (13) Hugh L. Dryden (with hat), Bureau of Standards; (14) Herbert V. Thaden, Thaden Metal Aircraft Corp., San Francisco; (15) William F. Joachim, LMAL; (16) Waldemar A. Klikoff, Aircraft Development Corp., Detroit; (17) Karl Arnstein, Goodyear Tire and Rubber Co., Akron; (18) Charles F. Marvin, Chief, U.S. Weather Bureau; (19) Joseph Ames, NACA chairman; (20) J. B. Johnson, Materiel Division, McCook Field; (21) George Lewis, NACA; (22) Henry Reid, LMAL; (23) Marsden Ware (behind), LMAL; (24) Elton W. Miller, LMAL; (25) Max M. Munk, LMAL; (26) A. E. Nesbitt, Aviation Corp., New York City; (27) John W. Crowley, Jr., LMAL; (28) Arthur Gardiner, LMAL; (29) Smith J. DeFrance, LMAL; (30) Charles Ward Hall, Charles Ward Hall Inc., New York City; (31) W. G. Brombacher, Bureau of Standards; (32) Fred E. Weick, LMAL; (33) Theodore P. Wright, Curtiss Aeroplane and Motor Co., Long Island; (34) Lt. Walter S. Diehl, BuAer, Navy Department; (35) Temple N. Joyce, Curtiss Aeroplane and Motor Co., Long Island; (36) George J. Higgins, LMAL; (37) Edward P. Warner, Assistant Secretary of the Navy for Aeronautics; (38) Walter Reiser, LMAL; (39) Capt. Holden C. Richardson, BuAer; (40) Edward R. Sharp, LMAL; (41) Lyman J. Briggs, Bureau of Standards; (42) Maj. Leslie MacDill, Materiel Division, McCook Field; (43) unknown; (44) Donald G. Coleman, LMAL; (45) Paul Hemke (behind), LMAL; (46) Mitchell (full identity unknown).

Attendees at the 1934 annual conference assembled for a group picture in the Full-Scale Tunnel because it was the only place large enough to hold them all. The airplane mounted in the FST is the Boeing P-26A Peashooter.

as if the boys were "putting on a parade for their parents."[29] Lab engineers worked long and hard on their presentations until nearly everyone was satisfied that they were near perfection. In 1937 the chief of aerodynamics wrote the engineer-in-charge:

> If it is desirable that all speeches and demonstrations be ready for rehearsal at an earlier date than heretofore, it is my suggestion that the date be set some time in advance so that everyone can work to it. I think the best way to bring our preparations to a completion at an earlier date is to stop tunnel operation at an earlier date and concentrate on our conference preparations rather than to try to keep the tunnels running and make preparations at the same time.[30]

Even before 1939, when the NACA formally changed the name of the conference in response to military requirements brought on by the start of World War II, most Langley employees already considered it an "inspection."

Such an approach worked at times against spontaneity. Eastman Jacobs remarked in a 1939 memo:

> A pretense should at least be made to giving the guests an opportunity to make suggestions and to get ideas off their chests. Very few will respond, but they will leave with the impression that we would have been glad to hear from them.[31]

The well-rehearsed NACA engineers made the most of their time in the national spotlight, sometimes reducing the time for visitor input. This domination of conference sessions reflected a management decision. In 1931 George Lewis had informed Langley that too much time in past meetings had been consumed by the presentation of suggestions. "We [in the Washington office] are trying in so far as possible to obtain in writing all the suggestions of the manufacturers' representatives as to future research problems to be undertaken by the Committee," said Lewis, and the lab must "cut down the time allowed [for them] as much as possible."[32]

However limited the give-and-take within the program, the NACA conference initiated a year-long discourse within the American aeronautics community. Companies that were reluctant to offer their most profitable ideas for research and development in the presence of competitors frequently wrote to the NACA proposing tests, and many followed up on an idea expressed during the conference by later sending a representative or even a team of consultants. Nearly all of these visits were friendly, though some of them could be troublesome. A few weeks after the 1934 conference, Langley's chief of aerodynamics reported to the engineer-in-charge that a recent visit by a man from Chance Vought demonstrated the "need for more definite rules" regulating visitors. The manufacturer's representative arrived at Langley Field on a Saturday morning (when all employees worked until noon), spent about an hour with the chief getting information that would be needed in arranging a definite test program, and then bothered a member of the chief's staff at his home in the afternoon. The following Monday the Chance Vought man spent six and a half hours tying up two of Langley's best men (Fred Weick and John Stack) and "not by any means" did he confine himself to the "questions which he [had previously] mentioned." The purpose of his visit, in the chief's estimation, was "not so much to clear up hazy points regarding our reports or the information given out at the conference" as to obtain additional data that might help in connection with the design on which his company was then working.[33]

Since it was strict NACA policy to avoid giving commercial advantage to any one company or to obligate itself to any firm, Langley had to try as best it could to fend off these occasional attempts to use it as a consulting service. Usually this meant tightening the visitation rules. By 1938, one rule in the NACA's "General Information for Laboratory Guides" provided that "the research problems of the Committee shall not be discussed with visitors at the Laboratory except upon specific authorization from the Engineer-in-Charge or a division chief," and another stated that "unless visitors have a letter from the Washington office authorizing their obtaining technical information and data, [they] shall not be given information on any of the researches of the Laboratory, except where such information is published."[34] At least one industry representative complained years later that by the late 1930s it was not even possible to watch tests being made on proprietary articles belonging to his own company.[35] World War II, however, required that the NACA loosen its rules; during the national emergency, getting the job done "took priority over concern for fairness in dealing with competitive companies" (see next chapter).[36]

In the weeks following an annual conference, the NACA staff gave serious consideration to the merits of every suggestion made by the visitors. Langley forwarded written comments on each idea to the Washington office, which in turn sent its recommendations to relevant NACA subcommittees. Even those questions that had already been answered during the meeting were considered as serious requests and given formal review.

From the hundreds of suggestions at the conferences from 1926 to 1939, the NACA authorized only 15 new research projects (see table 1). That comes to just over one research authorization (RA) per conference. Why so few? There are at least two explanations to consider. First, because the NACA's initial research authorizations had broad titles, Langley could often carry out tests suggested during a conference under RAs already in effect. Second, most suggestions for research that surfaced at a conference reflected someone's desire for a solution to a specific, and often private, problem of current aircraft design or operation. These ideas for NACA research thus involved refinement of what Edward Constant II, historian of the turbojet revolution, has called "normal technology"; that is, technology that evolves slowly, incrementally, and in accordance with a community of practitioners' ruling paradigm.[37] The NACA rejected some suggestions as technically unsound and turned down some others because they would require detailed work on someone's proprietary design, and therefore were not problems appropriately to be undertaken by the Committee. In evaluating most ideas, however, it concluded that there was "sufficient information already at hand," that "this question has been covered to a reasonable extent," or that "work on this project is in progress." (For examples of these typical conclusions, see table 2.) Since it was Committee policy to carry out all major tests requested by the military, even to authorize an investigation of a special or proprietary device if the army or navy or a large number of manufacturers were interested in it, Langley at any one time had in mind most of the problems that were important to the aircraft industry.[38]

Table 1

NACA Research Authorizations Resulting from Suggestions at Annual Manufacturers' Conferences 1926–1939

RA	Title (Source of idea)	Date approved
215	Effect of Cowling and Fuselage Shape on the Resistance and Cooling Characteristics of Air-Cooled Engines (Several individuals)	22 June '27
252	Mutual Interference of Airplane Parts and Effect of the Use of Fillets (Charles Ward Hall, Charles Ward Hall, Inc., New York City)	28 June '28
253	Effect of Position of Propeller with Reference to Wings (Col. Virginius E. Clark and W. H. Miller, Curtiss Aeroplane and Motor Co., Long Island, New York)	28 June '28
283	Investigation of Maneuverability and Control of Commercial Type Airplane (E. P. Warner, Asst. Sec. of the Navy for Aeronautics)	22 Mar '29
285	Study of High-Speed Cowling as Ignition Shielding of Air-Cooled Engines to Aid Radio Reception (Airways Div., Dept. of Commerce)	22 Mar '29
325	Investigation of the Causes of Airplane Crash Fires (Soc. of Automotive Engineers)	24 June '30
418	Investigation of Landing Characteristics of an Autogiro (Aeronautics Br., Dept. of Commerce)	8 June '33
472	Aerodynamic Characteristics of W-1 Airplane with Slot-Lip Aileron and New Type of Flap (Bur. of Air Commerce)	14 June '35
476	Investigation to Determine the Handling Characteristics of an Airplane in Flight Following Failure of One Engine (Douglas Aircraft Co.)	14 June '35
509	Preliminary Study of Control Requirements for Large Transport Planes (E. P. Warner)	14 Jan '36
510	Investigation of Airplane Tail Surfaces (Consolidated Aircraft Co.)	3 Mar '36
542	Detailed Investigation of Balanced Control Surfaces (Consolidated Aircraft Co.)	22 Oct '36
660	Investigation of Flying Qualities of Lockheed 14 Airplane with Special Reference to Stability, Controllability, Stall, and Vibration (Air Safety Board, Civil Aero. Adm.)	13 Mar '39
699	Flight Investigation of Control and Handling Characteristics of a Light Airplane (CAA)	15 Sept '39
703	Study of Airline Operating Conditions of Wright 1820-Series Engines in DC-3 Airplanes (Air Safety Board, CAA)	19 Oct '39

Source: NACA research authorization files, Langley Historical Archive (LHA).

Table 2

NACA Responses to Selected Suggestions for Research Made by Representatives of the Douglas Aircraft Co. at the Annual Manufacturers' Conference in 1935

Suggestion to study	NACA response
Problem of a transport airplane taking off and clearing an obstacle at the edge of a field	Problem already being studied as part of the NACA's research program on propellers and high-lift devices; covered by existing RA
Problem of landing over an obstacle and stopping in the shortest possible distance	Already being studied as part of program on high-lift devices; covered by existing RA
Lateral control at low speeds, particularly in connection with blind landings	Already being studied as part of program on use of high-lift devices; covered by existing RA
Handling characteristics of airplane in flight following failure of one engine	Recommended for authorization by Executive Committee; RA 476
Investigation of proper design of vents and scoops for various purposes on body and wings of airplanes	A particularly suitable problem for laboratory study; work covered by existing RA
Ice formation in carburetors and on airplane parts such as ailerons	Work covered by existing RA
Effect of airport contours on accelerations during taxiing	Research outside the scope of NACA functions
Development of retractable landing gear that will operate very quickly	Outside the scope of NACA functions
Problem of landing with a side wind, particularly in regard to proper proportions of the vertical surfaces and the dihedral of the main wing	Though already part of an NACA research program, more attention should be paid to the problem
Investigation of propellers, preferably at full scale, with higher pitches and with three or more blades	Covered by existing RA

Sources: "Suggestions for Aerodynamic Research, 10th Annual Aircraft Engineering Research Conference, Langley Field, Va., May 22, 1935," A197-1, LaRC Central Files; Fred Weick to files, "Discussion with Dr. W. Bailey Oswald during His Visit to the Lab on May 27, 1935," 29 May 1935, A197-1.

No one at Langley in 1926 needed to be told that the cowling problem was important, for example. It had been obvious to most aeronautical engineers for years. The NACA had only deferred action until Langley's new Propeller Research Tunnel became available. So the conference request for cowling studies simply provided the official justification for the NACA's authorization of a new research project.[39]

The annual aircraft engineering conference at Langley Field allowed the NACA to solidify its place in the American aeronautics community. As a public institution, the Committee and its laboratory faced the challenge of promoting teamwork in national aeronautics while dealing with competitive economic interests, professional rivalries, and political tensions—forces that sometimes threatened the NACA's role as an autonomous federal agency. The conference informed (and entertained) important people in the various fields of aviation, and advertised NACA research. The response of most visitors was positive. In his written evaluation of the 1939 conference, one LMAL engineer noted that "spontaneous comments on the work of the laboratory were invariably favorable—occasionally to the point of absurdity."[40]

Though the annual conference kept NACA Langley in touch with the needs of industry, and allowed manufacturers' representatives to obtain firsthand information on the Committee's research facilities and results and to advance suggestions for future research, the conferences did not make the NACA captive to commercial interests. Considering the polished, public-relations finesse with which the NACA executed the conference proceedings, the limited time for questions and answers during the formal program, and the regulations for follow-up visits, it is hard to see how the meetings could have furthered any exploitation of the NACA by industry.[41]

Relations with the Military

The annual aircraft engineering conferences did not cause LMAL programs to slide toward commercial, as opposed to military, applications. After all, the U.S. government had first supported aeronautical research and development during World War I as an instrument of national defense and industrialized armed force. The NACA's organic legislation and the funds to build the LMAL had been approved by Congress as riders to naval appropriation bills. Until 1919, the NACA budget had been part of the navy's request. (Some critics had even called the NACA "The *Naval* Advisory Committee for Aeronautics.") Committee headquarters was located in a wing of the old Navy Building, and its laboratory was on an army base.

The lab's location confused the public and caused LMAL officers "no little ... inconvenience ... in our transaction of business by correspondence." In 1925, for instance, the lab's chief clerk and property officer complained to George Lewis about the practice of addressing all government communications intended for the LMAL to the "Officer in Charge, Langley Field," or "Commanding Officer, Langley Field." Apparently this was the invariable practice with the navy, and a common one with the army. "It is evident that the Committee is confused with the Army," the clerk reported, "probably as a result of no instructions having ever been issued covering the independence of the two, and the distinction that should be made in addressing them." The lack of distinction, with uncertainty as to the real recipient of a letter or package, was an administrative nuisance:

> Not infrequently the Army holds property intended by the shipper for the Committee, merely because of it being addressed to the Commanding Officer, Langley Field, ... who will receive the property and demand a memorandum receipt before delivering it to us. In such cases we become accountable to both the Langley Field authorities and the shipper for the same item of property.

NACA headquarters worked to remedy the problem by instructing other agencies to address mail intended for NACA Langley to "Engineer-In-Charge, N.A.C.A., Langley Field," but Langley's correspondence files and property records after 1925 continue to furnish hundreds of instances of this nuisance.[42]

There were other minor problems associated with the everyday sharing of Langley Field by the NACA and the army. One aspect of the lab's operation that routinely irritated military personnel in the 1920s and 1930s was the noise caused by the "blowing down" (rapid release of pressurized air to achieve high speed) of the VDT, by the diesel submarine engines of the PRT, and by the two powerful 4000-horsepower drive motors of the FST. According to base adjutants who periodically complained, not only did the noise interrupt sleepers, but it also destroyed the ambience of the officers' club.[43]

Of course a trifling problem like noise from wind tunnels did not in the long run really harm NACA-military relations, which especially in the period between the two world wars were generally close, constant, and cordial. NACA policy was to carry out expeditiously all major research investigations requested by the military: whereas proposals from civilian sources were sent to appropriate subcommittees for review, military requests went directly to the Executive Committee for action. And although the NACA tried not to ask for military funds to carry out the projects, in the early 1920s it did get some money to pay for them. For example, to cover the cost of research authorization 46, "Investigation of Small Oscillations in Steady Flight" (approved in June 1921), the engineering division of the Army Air Service transferred $1500 to the Committee; and to cover the cost of RA 97, "Investigation of the Landing Speed of a TS Airplane" (approved in October 1923), the navy provided $24,000. By the mid-1920s, however, the NACA included such funds in its own budget requests.

Military expressions of support and praise for the NACA's independent aeronautical research provided the Committee with its strongest political testimony. In a letter sent to the Bureau of the Budget in 1922, Gen. Mason M. Patrick, chief of the Air Service, asserted that the army depended upon the NACA to solve "the more difficult problems" in aeronautics. Because the basic job of its aircraft engineering divisions was to assist procurement offices in selecting the best possible aircraft and accessories, the military concentrated on design and applications, while depending on the NACA for "fundamental research."[44] In response to a request in January 1933 from the chairman of the Senate Committee on Appropriations for his view on President Hoover's order to abolish the NACA, Charles F. Adams, the secretary of the navy, argued that if the NACA were abolished, "the Navy would be deprived of the benefit of organized counsel with leading scientists and would be forced to conduct independently the researches in aeronautics deemed necessary for the development of naval aircraft." Both the Navy and War departments strongly opposed the NACA's abolition or transfer to another agency of government, including to the military departments themselves.[45] John Victory, the NACA's executive secretary, regularly tapped the fount of incoming correspondence for these endorsements. According to historian Alex Roland, he

> would mark the appropriate passage, often lifting it entirely out of context, and direct a secretary to "card" it. From these excerpts Victory compiled over the years a 3 × 5 card file that stacked up over two feet high. In it were compliments for every occasion, which could be selected and quoted for any purpose ....[46]

So important were these endorsements to the survival of the NACA that George Lewis once remarked that "if the NACA ever sets itself aside from the Army and Navy, it is a dead duck."[47]

In the first years of Langley's operation—when the NACA was just beginning to learn what it was going to take to survive public controversy, and when its research for the most part lacked specific military or commercial purposes—the Executive Committee authorized most laboratory projects without any background justification. Between 1920 and 1925, a period when the adolescent military air services were still relying on World War I

Table 3

Military Requests for Research Work by NACA Langley
1920–1941

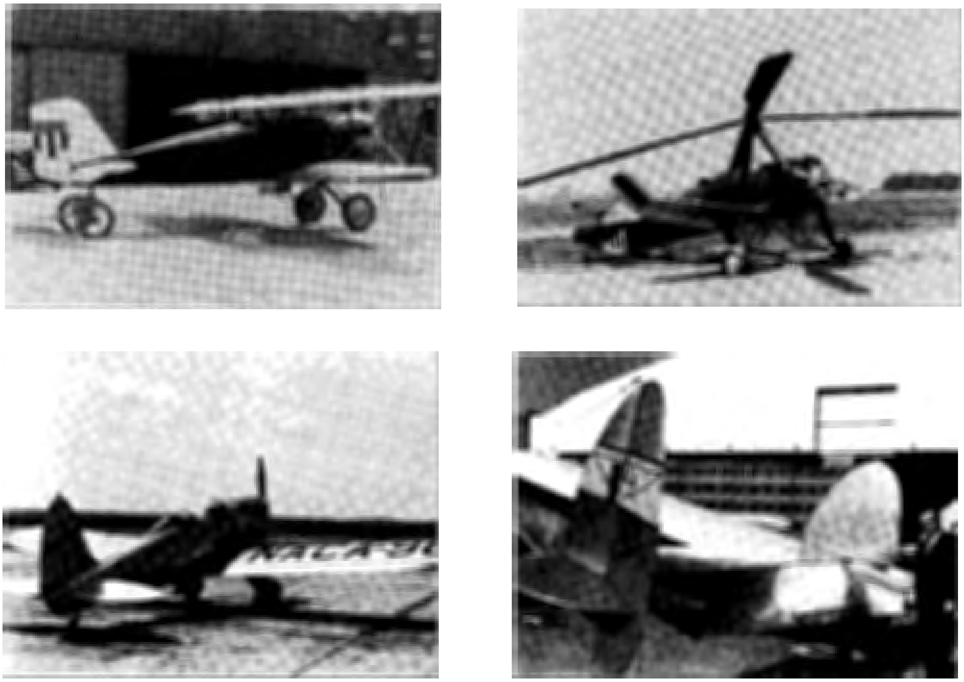
Period	New RAs assigned to LMAL	Work requested by:			Military requests as % of new RAs
		Army	Navy	Total	
1920–25	94	8	17	25	27%
1926–30	92	15	25	40	44%
1931–35	118	12	47	59	50%
1936–39	172	38	70	108	63%
1940–41	162	83	59	142	88%
Total	638	156	218	374	59%

Source: NACA research authorization files, LHA. Nearly all of the more fundamental aerodynamic investigations of the NACA were undertaken at Langley; however, some investigations were also assigned, especially in the period 1920–25, to the Bureau of Standards, the Forest Products Laboratory, the Weather Bureau, the engineering division of the Army Air Service, the navy, and to various universities.

aircraft, the NACA cited military requests as justifications for only 25 of Langley's 94 new RAs (table 3). After fluctuating in the late 1920s, the number of military requests then rose steadily with the explosion in new aircraft types under development. With the approach of World War II, the number skyrocketed. As the army and navy relied increasingly on the NACA for help with specific aircraft, the NACA seems to have rightfully used "military necessity" more and more as the justification for its programs.

Borrowed Airplanes

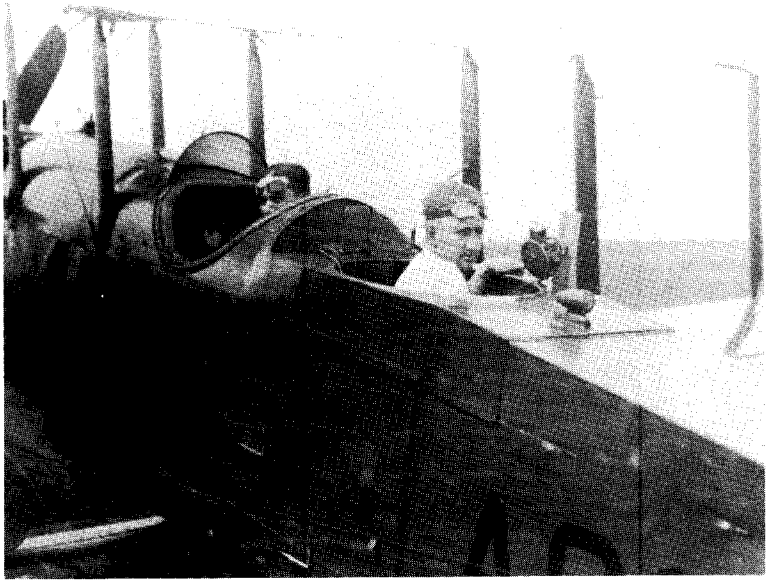
The NACA never owned many aircraft. Modest budgets, congressional suspicion of the Committee's need to own aircraft, and the increasing availability of military aircraft for loan when American production picked up around the time of the army and navy five-year plans in 1926 restricted the number of aircraft owned by the NACA. In 1924 it ordered its first airplane—a Boeing PW-9 pursuit plane built with especially strong tail surfaces and fuselage for use in a systematic investigation of pressure distribution.[48] Subsequently, George Lewis testified before a congressional subcommittee that the purchase was necessary because the services could not provide an aircraft of the special construction required for the



Among the few aircraft owned and operated by the NACA at Langley Field in the 1930s were a Boeing PW-9 pursuit plane (top left), a Pitcairn autogiro (top right), a Ryan ST sportplane (bottom left), and a Lockheed 12 (bottom right). In the photo of the Lockheed 12 (NACA aircraft no. 99), test pilot Mel Gough is pointing out a third vertical fin which Langley installed on the airplane to test the prevention of rudder lock in sideslips.

research.[49] (The NACA had requested an appropriation for the purchase of the plane after the order for it had been placed.) In 1928 the NACA bought a Fairchild FC-2W2 five-passenger monoplane with an enclosed cabin and detachable wings for testing a family of airfoils, and in 1931 came the first autogiro, a Pitcairn PCA-2. Other airplanes eventually owned by the Committee included two Fairchild 22s, a Stinson Reliant, a Ryan ST sportplane, a Piper Cub, and two Lockheed 12s. Though all were ostensibly purchased for research, several also served as transportation.

Nearly all the flying machines tested at Langley throughout its history came on loan from the army and navy. The first experimental work at the laboratory in the spring of 1919 involved flying two of the army's Curtiss JN4H Jennies to determine the degree to which their actual flight behavior at various altitudes differed from that predicted in wind tunnel tests at MIT. The NACA borrowed the biplanes from the flight line at



In the early years the flight research team was usually made up of a test pilot (in this case, Thomas Carroll, front cockpit) and an engineer (John W. "Gus" Crowley, Jr.).

Langley Field, where they were being used to train pilots and observers in gunnery, aerial photography, bombing, and communications. As test pilots the Committee used military aviators.[50] The next year it hired its first test pilot, but even after the NACA no longer had to rely on military test pilots, it still needed the routine assistance of the Langley Field base operations and flight control departments.[51] By the end of 1923 the services had transferred 17 airplanes to LMAL. Thirteen came on temporary assignment from the army, including five Jennies, a Thomas-Morse MB-3 pursuit plane, a British-designed SE-5A, a captured German Fokker D-VII, a French SPAD VII, and two DeHavillands, a DH-4 and a DH-9. Four were transferred by the navy: two Vought VE-7 trainers, a Douglas DT-2 torpedo plane, and a Curtiss TS-1 seaplane. As with the hundreds of other aircraft that were to be at Langley in the next 35 years (see appendix E), the NACA conducted comprehensive aerodynamic investigations with some of these airplanes and used others as test beds for various innovations (like superchargers and high-speed cowlings). And over the years laboratory personnel also made brief evaluations of a considerable number of aircraft that were at the military field temporarily.

LMAL's earliest civilian test pilots: Thomas Carroll (top), Paul King (left), and William McAvoy (right). King was the son of a United States senator from Utah.

The decision to lend a military airplane to NACA Langley was often informal and personal. Most naval aircraft in the 1920s and 1930s came through the good offices of Lt. Comdr. Walter S. Diehl, the officer in charge of liaison with the Committee at the Bureau of Aeronautics in Washington. A construction corps engineer who in his insistence on remaining a technical man refused throughout his career to pursue promotions via sea duty, Diehl often approached his superiors at BuAer with the news that the NACA wanted to borrow a certain type of airplane for an investigation at Langley. Because he met regularly with George Lewis and his assistants in the Washington office (Diehl's office was also in the Navy Building) and frequently visited Langley, he always knew exactly what the NACA was doing and what it wanted to do in the future. If he could pass on the Committee's assurance that the laboratory would make immediate use of the aircraft in question and that the proposed research had a good chance of producing data valuable to the general or specific development of naval aircraft, Diehl usually received permission to process the necessary papers.

Besides arranging the loan of aircraft, Diehl was also the NACA's best means of getting navy support for the authorization of a new research program or the permanent transfer of equipment and spare parts. In return for such support—and because his supervision was friendly and occasional and did not put the staff to the trouble of preparing replies and discussions—the NACA seems to have permitted him on-the-spot authority to terminate any navy-requested test that in his opinion had run its productive course.[52]

Dozens of the aircraft borrowed by the NACA came to Langley directly from the manufacturer's production line. Often naval machines were experimental types that came via the Anacostia and Norfolk air stations. Though the army sent the NACA many aircraft from Bolling Field near Washington, D.C., and its aircraft engineering division at McCook (later Wright) Field in Dayton, most loans came from the local flight line at Langley Field, typically from operational squadrons.

The LMAL could keep most borrowed airplanes for only a specified period, usually several weeks. Some it possessed for an undetermined course of research or on permanent transfer. On the majority it could make modifications and install special equipment as long as the aircraft was restored to the original configuration before being returned to the owner. On a few it could make no changes whatsoever or, conversely, could make whatever permanent alterations and additions it saw fit. This latter category consisted mostly of older aircraft for which the services had no more use.

Since the airplanes came from various sources under varied arrangements, lots of paperwork and other chronic bureaucratic headaches were unavoidable. Officers in charge of keeping track of military belongings, usually junior assistants, spent much time revising schedules. Because the laboratory frequently underestimated the amount of time it would need to keep an airplane, the schedules were sometimes unrealistic. Flight research required the development, installation, and calibration of many sensitive instruments and other special equipment, and it was very difficult—especially in the early years of Langley's operation—to estimate the time necessary for the work. More often than not, once an aircraft was available, some bright researcher would think of an additional, interesting way to use it. In 1929 LMAL test pilot William H. McAvoy felt personally responsible for the failure to return an aircraft according to the agreed-upon timetable. He complained to the engineer-in-charge that "it has been quite embarrassing for me to continually ask [BuAer] for more time ..., particularly in view of the fact that I did not know of the various requests that were to be made for further work in conjunction with its use." It seemed to the pilot that the NACA had been guilty of "false pretense." Practically all of Langley's flight research investigations, McAvoy argued, required considerably more time than originally estimated.[53]

The NACA's executive officers sometimes aggravated this situation by reducing the time estimated by the men in the field. In the same year that McAvoy complained, chief test pilot Tom Carroll questioned the Washington office for cutting his carefully thought out estimate for tests of a Fokker C-2A monoplane transport from one month to two weeks. He recommended that all future loans be accepted "for the duration of the research at the discretion of the Committee."[54] This might have made life easier at Langley, but it flew in the face of the NACA's idea of considerate service to clients.

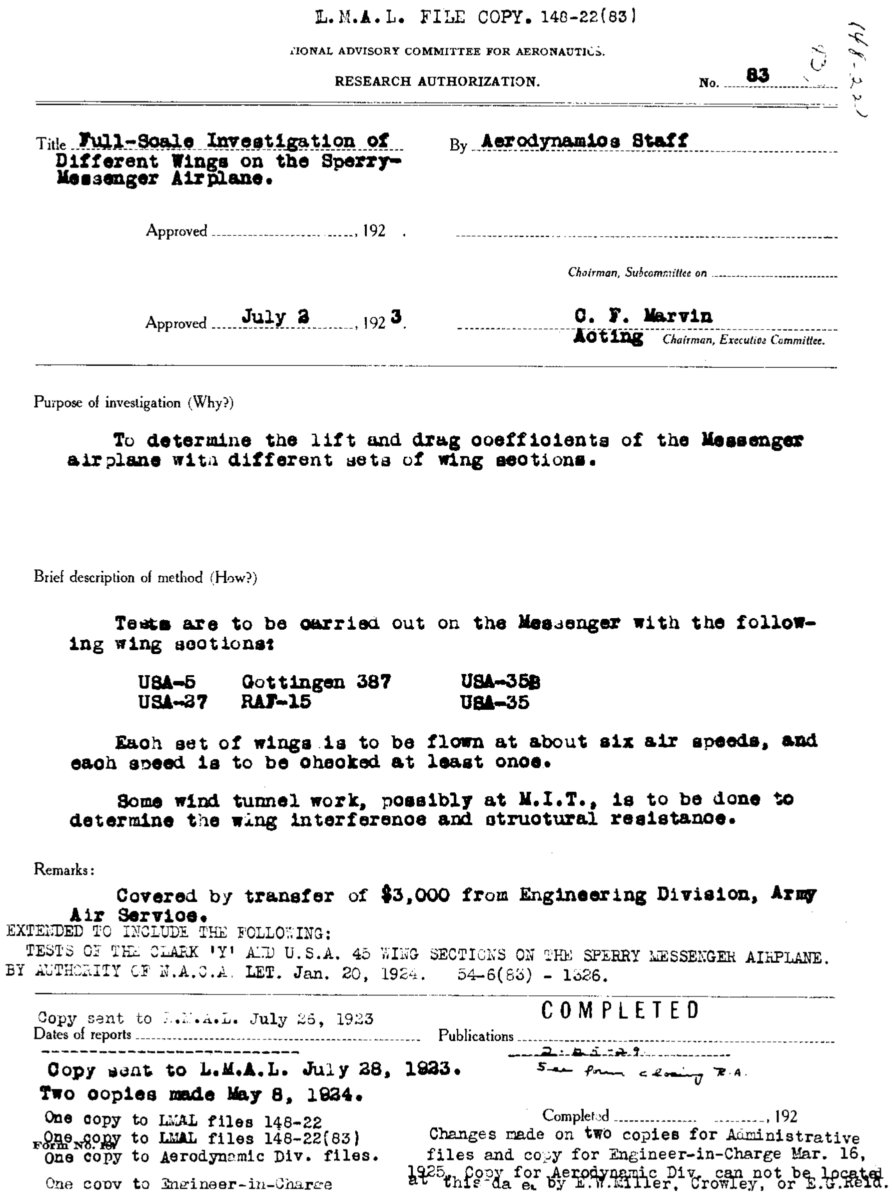
The Case of the Sperry Messenger

One of the earliest test programs requested by a branch of the military to be undertaken by the LMAL involved the loan of a Sperry Messenger, a small biplane the army had procured to replace motorcycles for certain liaison uses. The engineering division of the U.S. Army Air Service at McCook Field near Dayton provided the aircraft. Approved for research by the NACA Executive Committee in July 1923, RA 83, "Full-Scale Investigation of Different Wings on the Sperry Messenger Airplane," set a precedent. It was the first of many RAs in NACA history to cite work on a specific type of aircraft in its title. Before the NACA closed the file in February 1929, RA 83 would cover the job orders for nearly six years of occasional free-flight and wind tunnel testing, only a small part of which was directly relevant to the original purpose of the research. With this background in mind, a case study of RA 83 not only demonstrates the laboratory's handling of a borrowed military airplane, but also sheds light on important details of research administration and the working association with the military on a particular project.

The Lawrance Sperry Aircraft Company, Farmingdale, Long Island, delivered a Sperry Messenger to the Air Service at McCook Field in early November 1922. Soon thereafter the army engineering staff initiated a set of tests to determine the biplane's lift and drag characteristics when equipped with each of six interchangeable sets of wings. The manufacturer had built the wings after the then-popular R.A.F. 15, Göttingen 387, U.S.A. 5, U.S.A. 27, U.S.A. 35, and U.S.A. 35B airfoil sections, which were shapes of varying camber and thickness. By early 1923, the engineers at McCook had acquired considerable information on the airplane's performance as it compared to design calculations (including the results of three-foot-model tests of the Messenger's propeller in the Stanford University wind tunnel), but they possessed very little reliable information correlating the free-flight and tunnel performance of the airplane when using the different wings. Wanting to determine more correctly which of the six sets gave the best aerodynamic performance, the Air Service formally requested the NACA in February 1923 to conduct tests on the Messenger.[55]

After receiving the request, the first thing that NACA headquarters did was ask Langley several questions: What work does the military request entail exactly? Can it be done? Does it require special instruments or equipment? How soon can the laboratory start on this work? How long will it take? Does its scheduling seriously interfere with work in progress? How much will the entire program cost? In sum, Washington was asking Langley how and when it could do the work, not whether the proposed research was of fundamental value or whether the LMAL staff wanted to do it. George Lewis had already told the engineer-in-charge that the lab would carry out at least part of the research on the Messenger airplane before the end of the current fiscal year.[56]

Foremost in the minds of the men who considered Langley's responses to these questions was the additional workload on the small aerodynamics staff (less than 20 men in the wind tunnel and flight-test divisions combined). "The actual work of carrying out the tests will be considerable," the chief physicist warned, "as each set of wings will have to be flown at about six air speeds, and each speed will have to be checked at least once." He thought that the effects of wing interference and structural resistance on the calculation of lift and drag coefficients for each wing section called for some wind tunnel work, but added that this could be done at MIT. However, the development, installation, and calibration of a special inclinometer and



L.M.A.L. FILE COPY. 148-22(83)

NATIONAL ADVISORY COMMITTEE FOR AERONAUTICS.

RESEARCH AUTHORIZATION. No. **83** (83) 148-22

Title **Full-Scale Investigation of Different Wings on the Sperry-Messenger Airplane.** By **Aerodynamics Staff**

Approved ...., 192 . ...

Chairman, Subcommittee on ....

Approved **July 3**, 192**3**. **C. F. Marvin**

Acting Chairman, Executive Committee.

Purpose of investigation (Why?)

To determine the lift and drag coefficients of the Messenger airplane with different sets of wing sections.

Brief description of method (How?)

Tests are to be carried out on the Messenger with the following wing sections:

USA-5	**Gottingen 387**	**USA-35B**
USA-27	**RAF-15**	**USA-35**

Each set of wings is to be flown at about six air speeds, and each speed is to be checked at least once.

Some wind tunnel work, possibly at M.I.T., is to be done to determine the wing interference and structural resistance.

Remarks:

Covered by transfer of $3,000 from Engineering Division, Army Air Service.

EXTENDED TO INCLUDE THE FOLLOWING:
TESTS OF THE CLARK 'Y' AND U.S.A. 45 WING SECTIONS ON THE SPERRY MESSENGER AIRPLANE.
BY AUTHORITY OF N.A.C.A. LET. Jan. 20, 1924. 54-6(83) - 1326.

COMPLETED

Copy sent to L.M.A.L. July 26, 1923

Dates of reports .... Publications ....

2-25-29

See form closing R.A.

Copy sent to L.M.A.L. July 28, 1923.

Two copies made May 8, 1924.

One copy to LMAL files 148-22
One copy to LMAL files 148-22(83)
One copy to Aerodynamic Div. files.
One copy to Engineer-in-Charge

Form No. [illegible]

Completed ...., 192

Changes made on two copies for Administrative files and copy for Engineer-in-Charge Mar. 16, 1925. Copy for Aerodynamic Div. can not be located at this date by E.W.Miller, Crowley, or E.G.Reid.

The NACA authorized testing of six different wing sections for the army's Sperry Messenger airplane, July 1923.

The army's Sperry Messenger airplane with variable-camber wings, 1926.

airspeed head would require several weeks before tests could begin. If Langley was going to take on this project, the physicist argued, "either some of our personnel or else some part of our present program must be abandoned."[57] The engineer-in-charge reinforced that opinion. He wrote George Lewis that the flight operations section was busy supplying Joseph Ames with data on the performance of the Fokker D-VII—information that the chairman of the Executive Committee planned to use later that same year as part of his Wilbur Wright Memorial Lecture in London. Therefore, the NACA "should not promise to get out any Messenger results before the end of the summer."[58]

On 2 July 1923 Charles F. Marvin, chief of the U.S. Weather Bureau and acting chairman of the Executive Committee, signed research authorization 83. For the brief description of the purpose and method of the investigation called for on every RA form, NACA officers in Washington lifted phrases directly from Langley's evaluation of the research request. To cover the cost, the engineering division at McCook transferred $3000 to the NACA. Two weeks later Langley received a duplicate copy of the signed RA. Its engineer-in-charge then had the authority to approve job orders and its chief clerk the means for paying costs.

During the remainder of the summer, McCook conducted a preliminary investigation of the airplane's performance. Mechanics calibrated the small three-cylinder, 60-horsepower Lawrance engine and, in turn, attached the first three sets of wings to the fuselage. After each assembly, the flight test section investigated the plane's high- and low-speed characteristics. In early November, the maintenance section crated all six sets of wings and shipped them, along with the plane's freshly painted fuselage, overhauled engine, and flight log, to Langley Field. A few weeks earlier, the Air Service had sent its final outline of proposed tests to the NACA.

The Air Service plan called for NACA Langley to test fly the Sperry Messenger, equipped alternately with each of the six sets of wings, with power on and power off at five different speeds. To check the accuracy of the full-scale data, the Air Service asked the lab to test a one-tenth-scale model in its atmospheric and variable-density wind tunnels. After reading the proposal, the head of the wind tunnels division reported to the Washington office that the costs of all the tests, as outlined by McCook, would exceed the army's original transfer of funds by at least $9500. He believed that Langley could curtail costs and still get meaningful results by encouraging McCook to continue the study of the biplane in free flight with different wings. The NACA could then ignore those of unsatisfactory performance. He also wondered, though, whether it might not be wise for the LMAL staff to conduct the research as requested, regardless of the cost; the program was especially important because it involved, for the first time, "both coordination between three different sections of our own organization and the maintenance of requisite contact with McCook Field."[59]

Up to that time, members of the Langley and McCook organizations had felt vaguely as if they were rivals. Air Service personnel remembered their difficult and unproductive working association with George de Bothezat, a temperamental Russian aerodynamicist whom the NACA had recommended;[60] NACA employees recalled with some irritation that the army had agreed to share experimental facilities with them at Langley Field and had then reneged in 1918, transferring its aircraft development programs to Ohio. McCook's engineers had worked successfully on the development of the Sanford Moss turbosupercharger, a siphon gasoline pump, several different leakproof tanks, and fins and floats for emergency water landings, all before the dedication of the LMAL in June 1920. Later, they built the first high-speed tunnel in the United States and used the acquired data to design reversible and variable-pitch propellers.[61] The Langley staff had a hard time matching these contributions until the VDT began operation in late 1922.

A point of friction between NACA Langley and the Army Air Service surfaced almost immediately after the Sperry Messenger research was authorized. The chief of McCook's airplane section wanted to send the designer of the six sets of wings to Hampton for two or three weeks to assist in rigging the wings and to watch test procedures. The McCook official assumed that "both parties can benefit by having him stay on the job as long as we can spare him from here," especially as the designer was one of the engineering division's "most capable men, but quiet and unassuming."[62]

But even with that assurance, Langley did not like the idea of a McCook engineer under foot during the tests. The LMAL engineer-in-charge could only hope that "some unforeseen circumstance" would prevent the visit. George Lewis realized that the attendance of the military representative meant that the NACA would "have to use more care and judgement in estimating when we can undertake the investigation and the time required to complete it," but also understood that the army had "a perfect right" to request such attendance since it was paying for the research.[63]

Unpacking the various parts of the Sperry Messenger in November 1923 uncovered another problem. Although the airplane was supposed to arrive complete and ready for easy assembly with each set of wings, the Langley crew discovered that the Air Service had shipped a heterogeneous assortment of parts, some of which had never been checked. Half of the wings—the U.S.A. 5, U.S.A. 35, and U.S.A. 35B—had never even been fixed to the fuselage! This situation necessitated more work at Langley than had been scheduled. Moreover, the propeller sent by McCook was old and "by no means comparable with the model." Langley asked for a new propeller with more exact and predetermined characteristics. Only when these details were worked out could flight research begin.[64]

Agitated by the problem of assembling the Messenger, Langley researchers soon were questioning the very methodology of aerodynamic research at McCook. The head of Langley's wind tunnels division found the McCook Field report "Determination of Airplane Drag Characteristics in Free Flight" so full of errors that he doubted the overall value of the army's proposed outline of tests on the Messenger. He reported that

> the sample tests on a VE-7 and a DH-4 airplane are surprising to us because they show the latter to have a higher lift/drag ratio than the former. Our information on these two machines shows the condition to be quite the opposite and we can not believe the McCook Field flight tests show the true characteristics of these two airplanes. We do not see how a test of this nature can be of any value unless done with considerably greater care and accuracy than seems to have been used in this report.

The division head brought this criticism to the attention of his engineer-in-charge because the earlier work at McCook was the basis for many details of the proposed Messenger research that Langley was about to commence.[65] In a letter to George Lewis covering the memo, the engineer-in-charge related that this was not the first case of problems referred to the laboratory by one of the military services being "partially covered by erroneous and misleading reports." The mistakes thus put on record constituted "an obstacle which must be cleared away before the organization is in position to properly appreciate and value the research work we do for them."[66]

The director of NACA research understood even better than his technical staff at Langley that this matter had to be handled with discretion. If the NACA officially criticized McCook for faulty research methods and erroneous reports, the army "would be seriously antagonized," even though it might eventually admit that the criticisms were justified. If, on the other hand, the NACA did not report its suspicions about the value of the Air Service work, it would have nothing on record to show that its research team had noticed the errors; worse, there would be no way to justify deviating from the research agenda planned by the Air Service. Lewis brought this delicate issue to the attention of certain members of the Committee. He also sought the advice of his good friend in the navy's Bureau of Aeronautics, Lieutenant Commander Diehl. After reading the McCook report in question and Langley's critique of it, Diehl concluded that "the errors passed over at McCook Field appear serious in this kind of work." As the army results were "certainly questionable," he recommended that the staff at Langley "be allowed to devise and use their own methods."[67]

After giving the problem this private airing, Lewis advised the engineers at Langley to ignore the faulty McCook report, which would mean not even mentioning it in the bibliography that would accompany the final NACA report. Though someone might one day take this omission as an indication of the NACA's unfamiliarity with the relevant literature, Lewis believed that this approach to the quandary best freed the hands of the laboratory staff to conduct the Messenger tests properly. It was also the best way to avoid mutual embarrassment and to keep the army cooperating with the NACA on friendly terms.[68]

This matter resolved, another arose: McCook stalled the research by failing to understand the stringent requirements for models to be tested in the VDT. In 1923 Langley was learning something every day about the operation of its newest facility, but aerodynamicists outside the organization, though they could easily understand the principle behind the tunnel's revolutionary design, could also easily remain largely ignorant of its details. Models had to be made of metal, preferably duralumin, to withstand the tunnel's high dynamic pressures and the test section's powerful vibrations. Wooden models could break up, especially at high angles of attack, sending splinters and other debris flying through the tunnel. Moreover, early tests in the VDT had confirmed that models had better replicate the exact geometry of the full-scale body. Tests with simplified models, such as had sufficed in earlier tunnels, would not produce reliable data.

Elton W. Miller inspects damage to a wing setup in the Variable-Density Tunnel, August 1924.

The engineering division at McCook did not adequately appreciate these things. After having been informed earlier of the special requirements of VDT models, McCook had agreed to provide a model of the Messenger with interchangeable duralumin wings. This it did do—but during a visit to Dayton in January 1924, the head of the VDT section learned that the Air Service's model was nonetheless "utterly inadequate," that it was being built with little or no regard for the other structural features that Langley had specified. "It seems improbable," he reported, "that the wings as they are now being put together will hold up in the Variable-Density Tunnel." He advised his engineer-in-charge that some of the model's major defects could be corrected in Hampton, but questioned whether the Committee should have to pay for army mistakes.[69] Later that month the NACA extended the scope of RA 83 to meet McCook's request to include tests of the Clark Y and U.S.A. 45 wings. When these models arrived at Langley a year later, they had been made of wood! As late as September 1925, the head of the

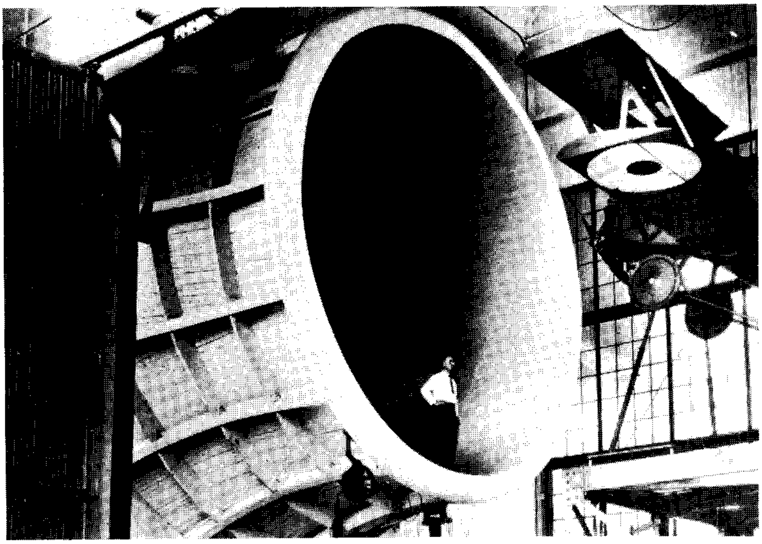
VDT requested approval of a job order for the manufacture of duralumin wings to replace unsatisfactory ones sent by the Air Service.[70]

In any event, tunnel testing had to await the completion of preliminary tests of the Messenger airplane in free flight. In March 1924 a Langley test pilot succeeded without much difficulty in taking the biplane, equipped with the U.S.A. 5 wings, to 2000 feet, but a few weeks later, with the R.A.F. 15 wings, he could barely get it off the ground. The engineer-in-charge reported to Washington in late April that if the other wings showed no better performance, the lab would be unable to execute more than one-half of the contemplated program. He even suggested transferring the research to the Fokker D-VII because that airplane was in much better flying condition than the Messenger and because the lab already had an accurate duralumin model of it, which was being tested successfully in the VDT. Lewis answered that "it would be rather bad form for the Committee to make any definite recommendations" relative to the Messenger until Langley had made a more serious effort to conduct investigations with it, especially as McCook had reported "no difficulty of any nature" in flying the airplane with the R.A.F. 15 wings.[71]

Reporting Test Results

The NACA expected research authorization 83, like all other RAs, to lead to the publication of technical papers. The first was Technical Note (TN) 223, "Determination of the Lift and Drag Characteristics of an Airplane in Flight," a report by Maurice W. Green in August 1925 that announced the preliminary results of Langley's glide tests with the Messenger. Then, in 1926, the NACA published Technical Report (TR) 225, "The Air Forces on a Model of the Sperry Messenger Airplane without Propeller," by Max M. Munk and Walter S. Diehl. The TR series was the top of the Committee's report hierarchy, "the rock to which the NACA anchored its reputation."[72] The NACA intended for TR 225 not only to satisfy McCook's request for specific information about the performance of the Messenger, but also to make a lasting contribution to the body of aeronautical research literature. An advance copy was sent to the engineering division of the Army Air Service for comments and recommended changes. Only then was the report sent to the Government Printing Office for printing and binding in the Committee's *Annual Report* to Congress. Finally, the NACA distributed separate copies of the TR to a long list of academic, industrial, and military subscribers.

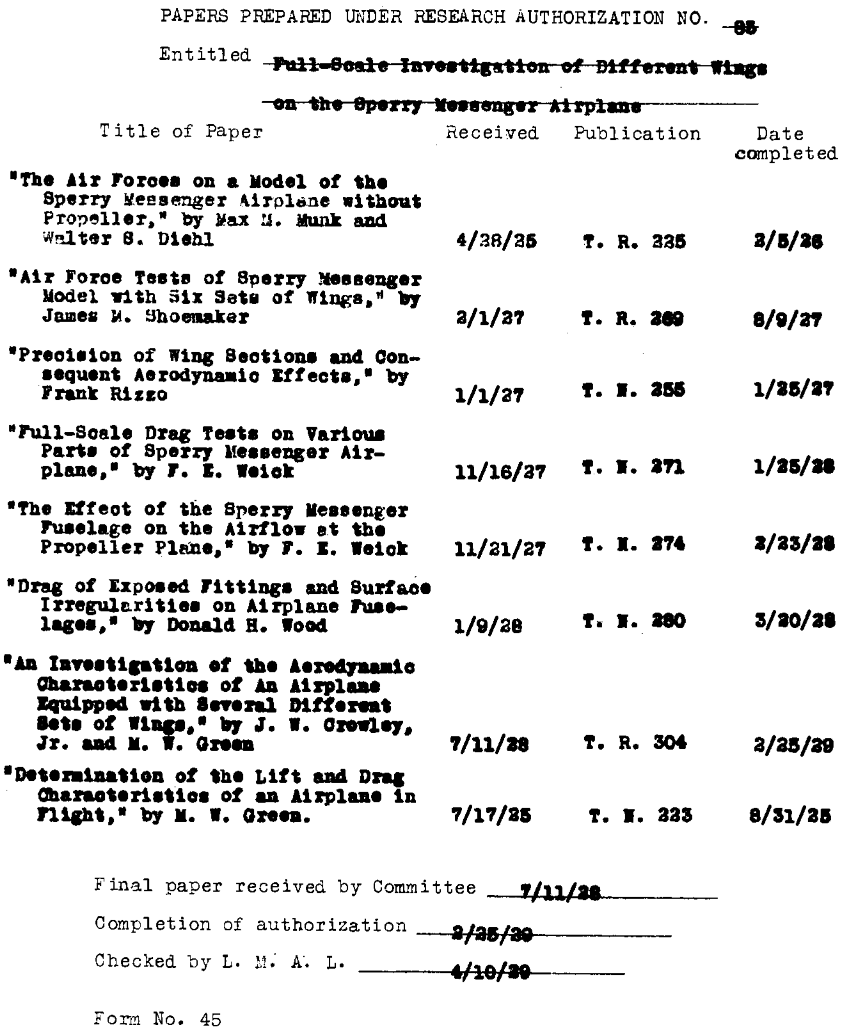
Even more than addressing the original purpose of the Messenger research—to ascertain experimentally which wings gave the biplane its best



The Sperry Messenger mounted for testing in the PRT, 1927. Standing in the exit cone is Elton W. Miller, Max M. Munk's successor as chief of aerodynamics.

performance—the real purpose of TR 225 was to advertise the Variable-Density Tunnel as a research tool of enormous potential. After testing a one-tenth-scale model of the Messenger (equipped with U.S.A. 5 wings) without propeller, Munk and Diehl declared that the NACA's VDT was "admirably suited for studying the scale effect and obtaining information which is necessary in an interpretation of the results obtained in atmospheric wind tunnels at low values of the Reynolds number." Though the research on the Messenger had not progressed far enough to allow complete comparison between model and full-scale machine, the authors concluded on the basis of the data at hand that airfoil characteristics were "affected greatly and in a somewhat erratic manner" by variations of the Reynolds number and that "the more exact a model is made, the more exactly will the test data obtained in the variable-density wind tunnel agree with the full-scale." Knowing all about Langley's nagging problem of getting McCook to provide suitable models for testing in the VDT, Munk and Diehl could not put too much emphasis on the "unsoundness" of testing with simplified models.[73]

Just as the multiple purposes of the TR went beyond the Messenger's aerodynamic problems, much of the testing done by NACA Langley from 1926 to 1929 under the cover of research authorization 83 had little to do with the purpose of the research as originally expressed. (In this regard, RA 83 was not unique.) In June 1927, for example, engineers assigned to the Propeller Research Tunnel asked for permission to mount



PAPERS PREPARED UNDER RESEARCH AUTHORIZATION NO. 85

Entitled Full-Scale Investigation of Different Wings on the Sperry Messenger Airplane

Title of Paper	Received	Publication	Date completed
"The Air Forces on a Model of the Sperry Messenger Airplane without Propeller," by Max M. Munk and Walter S. Diehl	4/28/25	T. R. 235	3/5/26
"Air Force Tests of Sperry Messenger Model with Six Sets of Wings," by James M. Shoemaker	2/1/27	T. R. 269	8/9/27
"Precision of Wing Sections and Consequent Aerodynamic Effects," by Frank Rizzo	1/1/27	T. N. 255	1/25/27
"Full-Scale Drag Tests on Various Parts of Sperry Messenger Airplane," by F. E. Weick	11/16/27	T. N. 271	1/25/28
"The Effect of the Sperry Messenger Fuselage on the Airflow at the Propeller Plane," by F. E. Weick	11/21/27	T. N. 274	2/23/28
"Drag of Exposed Fittings and Surface Irregularities on Airplane Fuselages," by Donald H. Wood	1/9/28	T. N. 280	3/20/28
"An Investigation of the Aerodynamic Characteristics of An Airplane Equipped with Several Different Sets of Wings," by J. W. Crowley, Jr. and M. W. Green	7/11/28	T. R. 304	2/25/29
"Determination of the Lift and Drag Characteristics of an Airplane in Flight," by M. W. Green.	7/17/25	T. N. 223	8/31/25

Final paper received by Committee 7/11/28

Completion of authorization 2/25/29

Checked by L. M. A. L. 4/10/29

Form No. 45

The document that concluded the business of every research authorization file was a list of technical papers written under that RA.

the Sperry Messenger, minus wings and propeller, at zero pitch in their new facility's mammoth 20-foot test section to determine the drag of the airplane's detailed parts. Theretofore, drag measurements had been limited by the sizes of the available tunnels to tests on relatively small models that replicated few of an airplane's complicated shapes, such as landing gear, engine cylinders, and tail surfaces. After taking up the matter with the Executive Committee, George Lewis extended the scope of RA 83 to cover the new work. Under this administrative umbrella Langley produced several research papers, including: TN 255, "Precision of Wing Sections and Consequent Aerodynamic Effects," January 1927, by Frank Rizzo; TN 271, "Full-Scale Drag Tests on Various Parts of the Sperry Messenger Airplane," January 1928, by Fred Weick; TN 274, "The Effect of the Sperry Messenger Fuselage on the Air Flow at the Propeller Plane," January 1928, by Fred Weick; and TN 280, "Drag of Exposed Fittings and Surface Irregularities on Airplane Fuselages," March 1928, by Donald H. Wood. The last paper grew out of observed differences between the drag of the Messenger fuselage in the PRT and that of its model in the VDT. Believing that the difference could be explained by investigating the drag of various small parts, the chief of aerodynamics requested job order 862 under RA 83 to measure the effects of turnbuckles, wire fittings, certain unfaired struts, rudder and elevator horns, pulleys, bolt heads, and nuts in Langley's 6-Inch Wind Tunnel at an airspeed of 100 inches per second. The engineer-in-charge approved the request on 3 November 1927.

These follow-on research efforts demonstrate Langley's good use of a research authorization and of a wind tunnel to go beyond stated purposes. Such latitude in research management and innovation in the use of research equipment were basic ingredients in NACA Langley's long-term success. (Note that the NACA cowling, the most important contribution of the Propeller Research Tunnel, was only indirectly related to the study of propellers.)

The NACA usually published a final technical report tying together the loose ends of a research authorization and announcing its conclusions. TR 304, "An Investigation of the Aerodynamic Characteristics of an Airplane Equipped with Several Different Sets of Wings," July 1928, by John W. Crowley, Jr., and Maurice W. Green, completed the work of RA 83.[74] Unlike the earlier reports prepared under the RA, TR 304 specifically addressed the purpose of the research as requested by the Army Air Service in 1923—comparison of the lift and drag characteristics of the full-scale Sperry Messenger with different sets of wings of commonly used airfoil sections. In contrast to all but the earliest report prepared under RA 83

(TN 223), the authors of TR 304 were flight researchers—not wind tunnel engineers—flight testing, after all, having been the principal mode of aerodynamic investigation called for originally by the engineering division at McCook Field.

Langley had tested only four of the six sets of wings before condemning the Sperry Messenger as "structurally unsafe" and discontinuing flight investigations with it. Nevertheless, the authors of TR 304 claimed that the results were clear. The thin R.A.F. 15 wings gave the airplane its lowest maximum lift and lowest minimum drag and the thicker Göttingen 387 wings gave the greatest maximum lift and highest minimum drag. (They found the U.S.A. 5 and U.S.A. 27 wings to be quite similar to each other in all respects.)[75]

There was no criticism of the army in TR 304, and NACA editors would have deleted it even if the Langley authors had cared to include it. However, the authors did manage to question discreetly the operating presupposition of the entire study as requested by the Air Service: the results of the Sperry Messenger tests "emphasize one fact which it is believed is not sufficiently appreciated," declared the LMAL flight researchers, "and that is, that with the exception of the change in maximum lift, the use of different reasonably good airfoil sections in themselves can not be expected to greatly change the performance of an airplane." Airplane drag consisted of induced, parasite, and profile drag of the body, tail surfaces, and wings, they reported, and the refinement of the section shape improved only the wing profile drag.[76] Without mentioning the method by name, the authors implied that parametric variation of model airfoil shapes in wind tunnels was a better way to find the best wing for any particular application.

Significance of RA 83

The history of RA 83 is the story of a precedent. It demonstrates how NACA Langley handled the first of many military requests for developmental testing of a particular airplane. It also exemplifies the routine of opening up, conducting, administering, and finally closing out a research program for a client. Together, the precedent and the example suggest some important points about NACA Langley's cooperation with the military in aeronautical research and development.

First, it was essential for clients to understand all of the NACA's detailed requirements. This was especially true in the case of the Sperry Messenger program because the army's engineering division at McCook, which had to provide the critical test apparatus—the model wings—was unfamiliar with the special aerodynamic conditions inside Langley's VDT.

As a result, more than two years passed before McCook provided LMAL with suitable duralumin models.

Better liaison between the NACA and the military might have prevented this and similar problems. Whereas Lieutenant Commander Diehl visited Langley often to discuss naval problems, the army had not established a channel by which to stimulate regular, fruitful exchange of ideas and know-how between its aircraft engineering staff and the NACA research team. Only when the pace of developmental testing accelerated with the approach of World War II did the army try to follow the navy's example. In March 1939 Maj. Carl Greene, chief of the engineering division of the Air Service Technical Command, and his civilian aeronautical engineer Jean Roché moved from Wright (formerly McCook) Field to Langley. Their new job was to provide more regular liaison between the applied research and development activities of the Air Corps and the more basic research of the NACA. Besides funneling information to appropriate Air Corps offices, the occupants of "Greene House" across from the LMAL Administration Building enabled the army to keep up better with the detailed requirements of the laboratory's research methods, facilities, and programs. To complete the conduit, the NACA later created its own liaison office at Wright Field.[77]

The history of RA 83 also demonstrates how cooperation between institutions with complementary functions and regular mutual business can be hampered. The NACA's policy of honoring all military requests for research placed Langley in a dilemma. Doubting the correctness of the army's procedures—the basis of the proposed tests of the Sperry Messenger—the LMAL staff either had to execute the flawed proposal or find some means to do useful testing in spite of the dubious military objective. Either way, the military engineers needed to be led—gently—to appreciate the value of the Committee's independent research process. The latter option demanded the more circumspection, especially in the language of its research reports, if relations between the NACA and the services were to remain cordial. In its internal memos Langley criticized many things about McCook Field, but these opinions were never aired officially.[78]

Many Langley old-timers have suggested that NACA-army relations between the two world wars tended to be less productive than NACA-navy relations. They believe that into the 1930s heirs of Billy Mitchell continued to want the removal of "those civilians" from Langley Field and the transfer of NACA research equipment to McCook. In this view the navy, having no similar designs, supported the NACA and achieved happier results.[79] Diehl believed in retrospect that the navy's approach to the airplane had to be less "emotional" and more akin to and dependent on the more "scientific" approach of the NACA than the army's did, because of the special technical requirements of carrier aircraft. In the 1920s the navy

> didn't know what kind of an airplane it would take to use on [a carrier]. It took ten years of ... hard work before we had [real] carriers and [real] carrier airplanes. And all the time we were calling on the NACA for help [in] measuring something, getting more stability and control, getting [better] data on the lift you could get out of wings, trying to improve the lift, trying to improve the structure, getting a lighter structure, reducing the drag.[80]

Table 3 may support Diehl's appraisal: between 1920 and 1935 the navy requested NACA research more than twice as often as the army did.

With the arrival of Greene and Roché to establish the Materiel Command Liaison Office at Langley in 1939, the army's understanding of the requirements of the NACA operation generally improved and the number of army requests for research increased dramatically. This closer tie to the army may have exacted a cost from Langley's research independence, however. At least in the beginning, the Langley staff strongly preferred the navy's occasional and informal style of liaison, reflected in the visits of Diehl, to the omnipresent army officers who regularly requested up-to-the-minute data sheets and curves and unpublished reports. World War II demanded such close liaison, however, as well as a change in the focus of NACA publications from polished TRs to quick, confidential bulletins. When the Japanese bombed Pearl Harbor in December 1941, most everyone at NACA Langley was grateful that the army liaison office had already been operating at the lab for over two years.

Finally, the life of RA 83 demonstrates that investigations resulting from those many research authorizations based on military requests aided more than the development of military aircraft. Under the cover of RA 83, Langley made a number of investigations that had little to do with the army's original intent. The lab went beyond the development of the Sperry Messenger to pursue those aspects of the research problem that could make innovative use of new research equipment, the VDT and PRT. The NACA's research on the little biplane furthered the broader interests of the American aeronautics community, both military and civilian, by revealing two fundamental points: that airfoil characteristics were affected greatly by variations in Reynolds number, and that in order for VDT test data to reliably predict actual performance at full scale, tunnel models had to be made very exactly. In a limited sense, since its research involved a comprehensive program of coordinated tunnel and flight tests of a series of "research wings," Langley's experience with the Messenger even helped to prepare the NACA for its vital role in the famous transonic research airplane programs of the late 1940s and 1950s. Thus by the time the Bell XS-1 was conceived during World War II, the NACA laboratory already had acquired some valuable experience on a specially constructed flight research configuration.

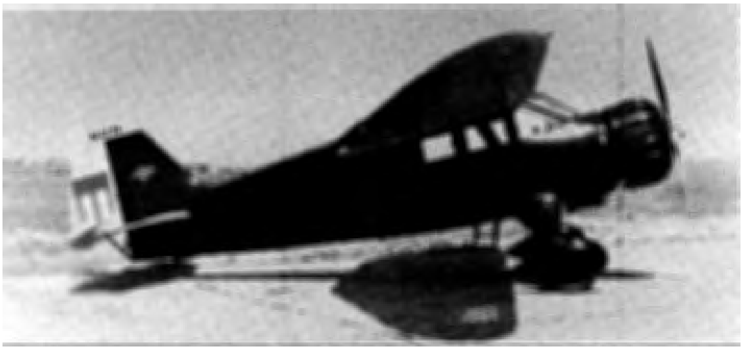
One explanation for the breadth of Langley's research contribution before World War II is that until military and commercial aircraft began to diverge in the mid-1930s—the military pursuing higher speed and altitude, the commercial emphasizing efficient operation and safety—there was no particular competition between military and commercial requests for NACA research. Earlier military and commercial airplanes did not differ greatly with respect to performance, wing loading, airspeed, and so on. At the NACA's first aircraft engineering conference at Langley in 1926, research on the effects of cowling on the drag, cooling, and propulsive efficiency of the new radial air-cooled engines had been requested by nearly everyone attending, including representatives of the army and navy, the Department of Commerce, and manufacturers. The low-drag NACA cowling that resulted (and which won the Collier Trophy in 1929) was used in all branches of aviation. Langley also performed some tests with no special civil applications and some with no obvious military applications, but most of the systematic programs—on airfoils, propellers, high-lift devices, alleviation of the flight hazards of airframe icing, and determination of the nature and magnitude of gust loadings that occur in storm systems, for example—applied fairly equally to all fields. The momentum for most of these broad programs was generated internally by the LMAL staff.

This assertion of in-house momentum is supported by the contents of dozens of Langley RA files besides RA 83. RA 204, for example, which called in 1927 for work on the "Control of Airplanes at Large Angles of Attack," contains a report on a November 1936 conference on stability research that exemplifies how a broad research program of the NACA was driven internally by laboratory researchers.[81] Fred Weick, the assistant chief of the Aerodynamics Division, was the meeting's main speaker; after dividing the stability problem into its most important components for his colleagues, he recommended that "all available data be used to obtain statistical information for preparation of empirical rules and for development of possible theoretical relationships." Weick suggested further that "the present program be extended to include a study of the effects of gusts," and then he opened the floor for discussion. During the course of the animated conversation that followed, Hartley Soulé, one of two representatives at the meeting from the Flight Research Division, pointed out the advisability of developing a series of charts with which the longitudinal stability characteristics of any airplane might be readily estimated. Robert T. Jones of the 7 × 10-Foot

Atmospheric Wind Tunnel section then suggested that "full-scale tests could be made to measure various individual stability derivatives." Soulé, who had previous experience in making such measurements, reacted to Jones's suggestion by warning that "such tests should be made only as check tests because of the difficulty and the time required." John Crowley, also of the Flight Research Division, added that the lateral stability of several airplanes should be measured "as a basis for comparing actual and estimated lateral stability characteristics." It is especially important to note that at the end of this 45-minute conference Weick stated in very strong and clear terms that it was advisable for the NACA "to obtain Army and Navy approval of *our* [author's emphasis] stability research program so that it will not be crowded out by urgent Army and Navy tests." Thus, military support was quite often merely the device used by NACA researchers to ensure that the generalized research program which they had developed would be conducted on an equal priority with development tests requested by the military.

Just before World War II, NACA Langley rightfully placed more and more emphasis on the testing of particular military aircraft and, as a result, found itself increasingly limited as to what it could do to meet broad commercial needs. In May 1939, for example, the Committee replaced its 13-year-old practice of the annual manufacturers' conference with an "inspection," a classified technical meeting intended exclusively for military representatives and a few delegates of their chosen contractors.

One civil aviation program which became increasingly directed toward military aircraft as World War II approached involved determination of satisfactory flying qualities. In 1935 Edward P. Warner, the original chief physicist at LMAL who was then working as a consultant for the Douglas Aircraft Company, asked the NACA to help him specify the stability and control characteristics to be built into the DC-4 transport. Up to this time, pilot impressions had been the only measure of what constituted good flying qualities in relation to the mission performance and operational suitability of an aircraft. In December 1935 the NACA Aerodynamics Committee, which was chaired by Warner, approved what became RA 509, "Preliminary Study of Control Requirements for Large Transport Airplanes." The purpose of this investigation was to determine "what specific qualities pilots desired, so that they could be numerically specified in future design competitions." A team of LMAL flight researchers under Hartley A. Soulé started this work in 1936 with a Stinson cabin monoplane. Langley instrumented the airplane so that its response characteristics, following known control inputs from the test pilot, could be measured, related to design parameters, and correlated with the pilot's qualitative evaluation of the ease and precision with which he maneuvered the plane. Soulé's team continued this effort using "all

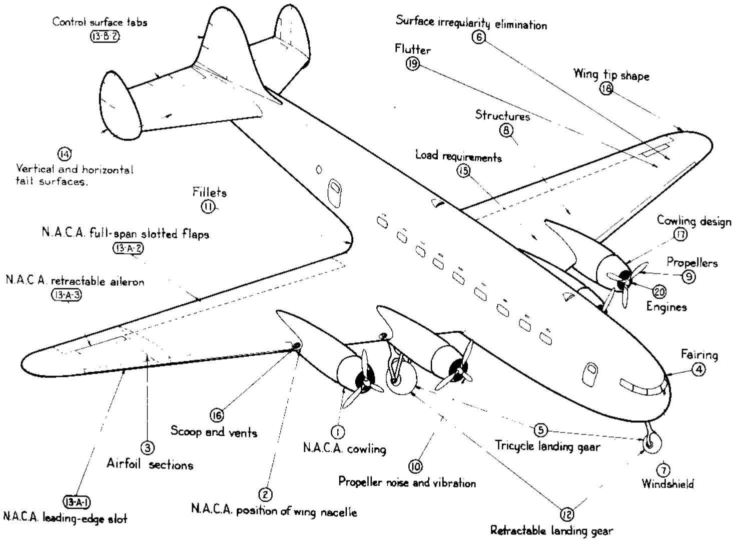


In 1936 LMAL used this Stinson Reliant SR-7, which was owned by the NACA, for a preliminary study of control requirements for large transport airplanes.

airplanes that could be obtained for the purpose" until 1941, when it was ready to specify numerical requirements for the longitudinal and lateral stability and control characteristics and the stalling characteristics of 12 different airplanes, large and small.[82]

In 1942, the U.S. Army and Navy revised the NACA's preliminary specifications to meet their immediate requirements and asked Langley to continue validating and upgrading handling requirements specifically for military aircraft. According to Soulé, "this was fortunate, as many more airplanes were made available and a broader view taken than would otherwise have been possible." By the end of World War II, the NACA had measured the stability and flying qualities of 60 aircraft, and military and civil aircraft handling requirements had been standardized. This effort foreshadowed the extensive work that would be undertaken in the field over the next three decades leading to the uniformly utilized rating system of the present day.[83]

In the history of American aviation, the development of advanced civil aircraft has always depended to a large extent—at least until quite recently—on the availability of technology generated by military research and development. Many parts of the Douglas DC-3 commercial transport, including air-cooled radial engines, retractable landing gear, and controllable-pitch propellers, derived from military-sponsored R&D. A 1972 study by the air force on R&D contributions to aviation progress began by pointing out that more than eight out of ten of all the commercial jet airliners then operating in the free world were designed and built in the United States, and that one of every four of those American-built craft traced its lineage to a single military bomber program.[84]



The design formula for propeller-driven aircraft recommended by the NACA in 1939 looked very much like the later configuration of the Douglas DC-4E. Ironically, plans for the DC-4E went nowhere.

Ironically, however, working successfully with industry and the military before World War II on ever more refined propeller-driven aircraft may have cost the NACA some of its chances to explore more fully some of the more revolutionary ideas in aeronautical science and technology. Successful teamwork depended upon a consensus, and the NACA's clients were interested in optimizing shapes and structures that could fly at speeds up to 200 miles per hour without falling to the ground in pieces. One NACA engineer has written that "it would have been quite impossible in the prewar period to have any major support from the military, industry, or from Congress for research and development aimed at such radical concepts as the turbojet, the rocket engine, or transonic or supersonic aircraft," and another has commented that "it is certain that if the NACA had had the foresight to do research on the turbine engine in the decade before World War II, the agency would have met with such technical ridicule and criticism about wasting the taxpayers' money that it would either have had to drop it or have been eliminated."[85]

A review of suggestions for NACA research made at the annual aircraft engineering conferences and of military requests for NACA tests seems to confirm that these insider testimonies are not mere rationalizations. With management concentrating on ways to satisfy the immediate demands of the American aeronautical establishment, Langley researchers could do less than they might have wanted to further understanding of exotic aeronautical ideas. On the other hand, the NACA was not beyond putting things over on Congress—like the "icing" tunnel. If members of the laboratory staff had really wanted to make exploratory studies of jet propulsion or another radical concept, they might have found some way to do it.

Luckily, the failure of NACA researchers and other American engineers to understand the potential of the turbine engine as quickly as a few men in Germany and Great Britain did made little difference in the practical outcome of World War II. The timing of the turbojet revolution was such that the NACA's systematic, evolutionary approach to aviation progress was vindicated. Research done at Langley in the fields of subsonic aerodynamics, stability and control, loads, propulsion, and structures—that is, research on the practical aeronautical problems of the day—contributed significantly to the design of the military aircraft essential to the Allied victory.

7

The Priorities of World War II

Of all the events that have affected the course of Langley history in the past seventy years, only two have caused major trauma. The *second* was the Sputnik crisis, induced in October 1957 by the Soviet Union's launching of the world's first artificial satellite. This crisis was indeed traumatic: Sputnik not only triggered the demise of the NACA and the birth of NASA, but it also triggered what future historians might very well call the "space technology revolution"—something that at present has all the appearances of becoming perpetual. The *first* was World War II, and in certain ways this trauma changed Langley more significantly, and more totally, than did even Sputnik.

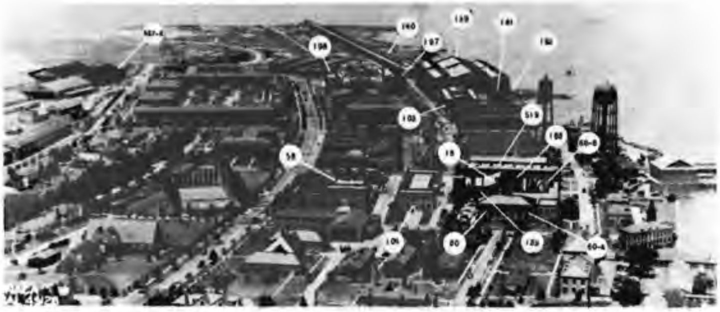
Before World War II, Langley and its parent organization, the NACA, were in some ways obscure operations. There were congressmen who did not even know that the NACA existed. The war altered this status dramatically. First, the laboratory grew much larger. In 1938, the total LMAL staff numbered only 426; by 1945 the size of the staff, in order to meet the increased workload, had swelled to over 3000. With wartime expansion came added organizational complexity and greater fragmentation of personnel. In 1935, employees belonged to one of only six different research divisions, and they worked in one of a dozen buildings on a few acres surrounded by army property; ten years later, employees worked in 18 divisions located either in the old "East Area" or in a large new "West Area" separate not only from the active parts of the military installation but also, by a few miles, from the other base of NACA operations. Beyond that, dozens of LMAL employees moved away to the NACA's new installations, the Ames Aeronautical Laboratory (AAL) at Moffett Field, California, and the Aircraft Engine Research Laboratory (AERL) in Cleveland, Ohio. The staff that remained was less uniform: a large number of women worked there for the first time, many of them doing a "man's job." Also, Langley's fiscal posture changed dramatically. Between 1940 and 1945 lab expenditures amounted to more than twice (approximately $33 million) what they had been in the first twenty years of LMAL history combined (approximately $14 million).

Finally, the priorities of American involvement in the war dictated a change in emphasis at Langley from general to specific testing. This change did not compel every engineer and scientist on the staff to forsake basic research, but it did mean that researchers assigned to the major wind tunnels and to the Flight Research Division had to spend the majority of their time preparing for, conducting, and reporting on tests of specific aircraft configurations. In the minds of NACA clients, managers, and most employees, the main responsibility of Langley laboratory during the crisis was to refine the high-performance combat aircraft being readied for production and to communicate accurate component data and other useful test information to military contractors as quickly as possible.

The NACA Perceives European Threat

In 1936, NACA reports of European aeronautical activities grew urgent. The Committee's intelligence officer in Paris, John Jay Ide, reported that the French had just completed a full-scale wind tunnel at Chalais-Meudon; the Italians had built an entire city, Guidonia, which they planned to devote almost exclusively to high-speed aeronautical research; and the Germans, traditionally strong in applying the science of aerodynamics, were in the midst of what appeared to be a major revitalization of their country's aeronautical resources. As a result of Nazi support, there would soon be five major regional stations for aeronautical research and development in Germany: three in the west at Aachen, Braunschweig, and Göttingen; one in the south at Stuttgart; and a central establishment, the Deutsche Versuchsanstalt für Luftfahrt (DVL) at Aldershof near Berlin.[1] George Lewis visited the DVL in the late summer of 1936 while touring various aeronautical installations in Russia and Germany; the place looked to him "like a construction camp" being readied for experiments "with every conceivable device." He estimated that between 1600 and 2000 well-trained employees were working there, compared with only 350 at Langley.[2]

Despite this comparison, Lewis still considered Langley "the single best and biggest aeronautical research complex in the world."[3] The lab possessed an unparalleled array of experimental facilities, led by the VDT, PRT, and FST. Lewis knew what advances in the design of state-of-the-art aircraft had been and could still be achieved from test programs conducted in these tunnels, and he also knew that a full-speed (500-MPH) companion to



EAST AREA

58	Administration Building	198	19-Foot Pressure Tunnel
104	Service Building	60	Atmospheric Wind Tunnel
519	Utility Building	60-A	Rectangular High-Speed Tunnel
60-B	Two-Dimensional Low-Turbulence Pressure Tunnel	191	8-Foot High-Speed Tunnel
		140	Tank No. 1
188	Two-Dimensional Low-Turbulence Tunnel	140	Tank No. 2
		140	Dynamic Model Shop
139	Full-Scale Tunnel	192	East Substation
103	Propeller-Research Tunnel	18	24-Inch High-Speed Tunnel
537-R	Flight Research Laboratory	197	East Shop

WEST AREA

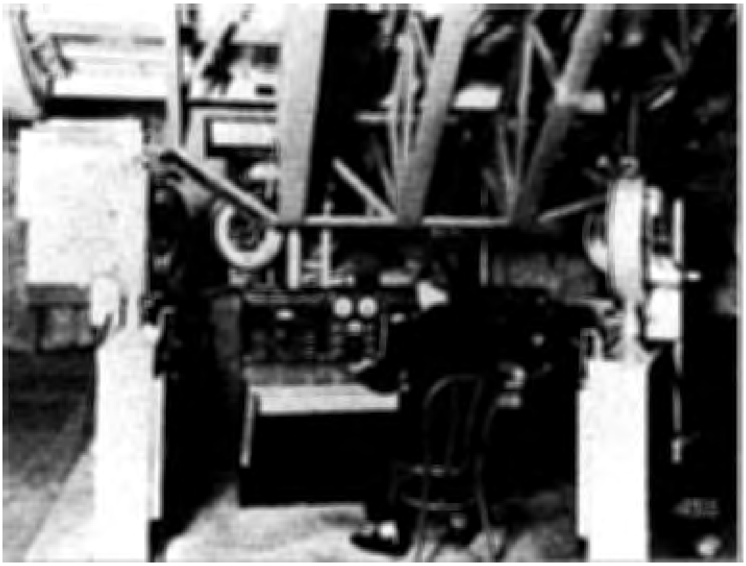
1146	Stability Tunnel	1212	300 MPH 7- by 10-Foot Tunnel
1146	16-Foot High-Speed Tunnel	1212	High-Speed 7- by 10-Foot Tunnel
1148	Structures Research Laboratory	1212	7- by 10-Foot Tunnels Laboratory
1194	West Shop	1219	Aircraft Loads Laboratory
1152	Power Plant	1218	Gust Tunnel
1192	Impact Basin	1220	Aircraft Loads Calibration Laboratory
1191	Model Supersonic Tunnel	1225	Sheetmetal Shop
1221	Induction Aerodynamics Laboratory	1213	Electrical Building
1229	Physical Research Laboratory	1215	New Heating Plant
1195	Warehouse	1224	Lumber Storage Shed
1230	Instrument Research Laboratory		

Langley's original east and new west areas, 1945.

the low-speed Full-Scale Tunnel had just become operational at Langley in March. The director of research and other NACA officials believed that this facility, conceived by VDT section head Eastman Jacobs in November 1933 and later named the 8-Foot High-Speed Tunnel, would "make possible the use of great speeds with safety, and thus give the United States a decided advantage over other nations."[4]

Lewis also knew that the NACA had just received over one million dollars, thanks to a June 1936 deficiency appropriation, for construction of a new pressure tunnel at Langley for high-speed propeller research. The basic idea behind the aerodynamic design of this super PRT, which came from Smith DeFrance in February 1936, was to overcome scale effects. DeFrance's experience in the FST had told him that "the most satisfactory size" of a wind tunnel for general use was one with a throat dimension of 20 to 25 feet. In a tunnel of this size, wherein compressed air traveled at speeds up to 200 miles per hour (as compared to the 118-MPH atmospheric current of the FST), not only could models be large enough to incorporate minor construction details, but tests could be conducted at a Reynolds number high enough to reduce the scale effect "to a negligible quantity." Though this Reynolds number (9 million) was approximately the same as that obtained in the FST, the cost of operating the smaller tunnel would be considerably less, DeFrance argued, because of its need for less electric power and the greater ease it allowed in making and changing setups. The NACA's primary political justification for immediate construction of this tunnel was to handle on "a production basis" the increasing demands for complete-model testing which Langley had been receiving in the mid-1930s from industry.[5]

Notwithstanding the excellent record of existing LMAL facilities and the promise of its new ones, NACA leaders understood the danger of complacency. In March 1936 they formed a Special Committee on Aeronautical Research Facilities, chaired by Rear Adm. Ernest J. King, the influential chief of the Bureau of Aeronautics. It was congressional respect for a recommendation of King's panel for additional experimental facilities that led to the NACA's deficiency appropriation in June.[6] This appropriation did not prevent Lewis, upon returning from Europe in September, from warning people that the technological edge enjoyed for the last several years by the NACA would come to an abrupt end if Congress did not allocate funds to increase manpower and build new test equipment beyond that already approved. Specifically, he wanted Langley's permanent complement raised immediately to 500 employees and a low-turbulence VDT for the development of higher-speed, lower-drag wings. In 1937 the NACA managed to get the Congress to authorize funds for this facility, but only by packaging it



The test section and the control console (bottom) of the 8-Foot High-Speed Tunnel were housed in the thick concrete igloo in the middle of the photograph at the top.

as an icing tunnel (see chapter 4). The Langley complement did reach 500, but not until 1939—when Germany invaded Poland and plunged Europe into war.

Expansion

The NACA had tried to prepare itself for this turn of events. In October 1936 it created a Special Committee on Relation of NACA to National Defense in Time of War, chaired by Maj. Gen. Oscar Westover, chief of the Army Air Corps. This committee took nearly two years to issue a report, but when it did, in August 1938, the idea to build a second NACA laboratory was put forward in strong terms. A second lab was necessary somewhere in the interior of the country or on the west coast, the committee report argued, both to disperse the government's aeronautical research facilities so that they would not be vulnerable to a single attack, and to relieve "the congested bottleneck at Langley Field."[7] Research teams at the LMAL were admittedly "working under high pressure" and managing to satisfy the increasing number of requests for specific configuration testing only "at the expense of interfering with or neglecting the more fundamental scientific long-range investigations that in the end mean much to the advancement of American aeronautics."[8] The argument for a second laboratory was soon strengthened by highly publicized reports from resolute Charles Lindbergh, who was touring Europe in October 1938, that Germany was "far ahead" of the United States "in military aviation."[9]

In December 1938, a Special Committee on Future Research Facilities under the chairmanship of Rear Adm. Arthur Cook, chief of BuAer, Navy Department, recommended the construction at Langley Field of several new facilities in which investigations of the special characteristics and problems of military airplanes could be made. One of these facilities was for structures research, a field made vital by the increases in size and speed of aircraft and by the increasing complexity of their metal construction. Another was a new tunnel to study spinning, which, as evidenced by the loss of several new aircraft such as the Grumman XF3F, was still a much-misunderstood phenomenon. The committee also advocated immediate construction at Langley of two high-speed tunnels, one (with a 16-foot-diameter test section) to investigate the cowling and cooling of full-size engines and propellers, and the other (with a 7 × 10-foot test section, the same size as that of the atmospheric wind tunnel operating at Langley since 1930) to study stability and control problems. All three of these facilities were eventually located a few miles away in the new West Area granted to the NACA by the War Department in 1939.[10]

The special committee also named Moffett Field in Sunnyvale, California (38 miles south of San Francisco), as the best site for a second NACA laboratory. Moffett Field, a naval airship station used since 1938 by the Army Air Corps as a training base, met all the general requirements set down by the site selection committee, including adequate electric power supply, which had been a chronic problem at Langley.[11] Location near the growing west coast aircraft industry was the deciding factor in preferring it, however.[12]

The NACA quickly endorsed its panel's choice of Moffett Field and recommendation for new facilities at Langley, and appealed to the Congress for construction funds. George Lewis testified before a House subcommittee that Langley was being forced, by lack of personnel and facilities, to neglect 49 authorized projects. He quoted engineer-in-charge Reid as saying, "Right now, we have enough work to keep our present staff busy for 2½ years."[13] Though the Langley items experienced no difficulty in clearing either the Congress or the Bureau of the Budget, the Sunnyvale installation ran into some trouble in the House Appropriations Committee, headed by Congressman Clifton A. Woodrum of Roanoke, Virginia. Woodrum was "not opposed to seeing funds for the expansion of the NACA pour into Langley Field, within his own state, but he was a little more circumspect about the advisability of sending such funds all the way across the country."[14] Eventually the NACA pacified Woodrum, and its entire expansion package was authorized—on 9 August 1939, just days before the Nazis rolled into Poland. The following spring, the NACA named the Moffett facility "Ames Aeronautical Laboratory," in honor of Joseph Ames, charter member of the Committee and its recently retired chairman, who was then near death.

A Special Survey Committee on Aeronautical Research chaired by Lindbergh followed up on the authorization for a second NACA laboratory with a declaration, in October 1939, that time was running out for America to catch up with European nations in engine development. Britain, France, and Germany possessed faster and more versatile fighter aircraft, Lindbergh said. They did largely because their engine manufacturers had been able to afford to develop superior liquid-cooled power plants capable of high-altitude flight. Because the geography of America was different, requiring flights of greater distances, U.S. manufacturers had concentrated instead on refining fuel-efficient air-cooled engines. Beyond industry, American facilities for research on aircraft power plants were totally inadequate, Lindbergh lamented. This inadequacy was partly a consequence of the NACA's agreement in 1916 to leave engine development to the engine manufacturers. It was now essential for the NACA to reverse this hands-off policy, he said. His committee called for the creation of a third NACA laboratory geared solely to solving the problems of high-speed aircraft propulsion.[15]

On 26 June 1940 Congress authorized construction of the NACA's "Aircraft Engine Research Laboratory" (renamed the "Lewis Flight Propulsion Laboratory" in 1948, in memory of George Lewis who died in July of that year) at a site near the Cleveland municipal airport. As with the Ames lab, the key personnel of this facility were to be drawn from Langley.

Langley now had two junior siblings. The NACA foresaw the three laboratories working together as a family, one member devoted to engines and two to aerodynamics. Ames and Langley might duplicate each other's programs only to the extent that duplication, competition, and cross-fertilization were productive. People at Langley saw themselves as part of the "mother laboratory," sharing talent and experience with daughter facilities. For a short time some employees at Ames and Lewis felt subordinate to Langley because its practices, policies, and opinions were so well established. By the end of the war, however, most people at the new labs felt distinct and confident enough in the capabilities of their own organizations to view "Mother Langley" as a peer and, on occasion, as a rival.[16]

Drag Cleanup

While the various ad hoc committees formed by the NACA from 1936 through 1939 helped to organize the political support necessary for the addition of new research staffs and facilities, they barely addressed the question of what the NACA was supposed to do with them once it had them. That responsibility the special committees left to the main, executive, and technical committees and to the research staff at Langley.

In April 1938 these bodies all heard a loud cry for help: the navy was unhappy with the 250-mile-per-hour flight test performance of its new experimental fighter, the Brewster XF2A Buffalo. The Bureau of Aeronautics wanted the staff at Langley to look for "kinks" or "bugs" in the plane's general design and to determine, in only one week's time, "what drag reduction may be expected from changes that can readily be incorporated in the event that this type is put into production." The NACA readily agreed, and even before a formal research authorization was transmitted to the lab, the navy flew an XF2A-1 to Langley Field for tests in the Full-Scale Tunnel.[17]

The FST team acted quickly to satisfy the navy's urgent request. Its engineers mounted the XF2A-1 on the balance of the 30 × 60-foot wind tunnel and put the airplane through a meticulous drag cleanup investigation.

In 1938 Langley mounted the navy's Brewster XF2A-1 Buffalo in the Full-Scale Tunnel for drag reduction studies.

At the end of five busy days of tunnel tests, the FST team concluded that Brewster had in fact overlooked the aerodynamic importance of several small but highly significant details of the Buffalo's design. The landing gear, exhaust stacks, machine-gun installation, and gunsight all projected outside the smooth basic contour of the aircraft in such a way as to produce unacceptably high drag. By modifying the XF2A-1 in these and several other minor particulars, it reported, the top speed of the prototype could be increased by 31 miles per hour to 281, more than a 10 percent improvement in performance.[18]

The XF2A set two precedents. It was the first airplane to use the NACA's new 230-series airfoils. All high-performance American military planes built through the end of World War II, with the exception of the P-51 Mustang, employed an airfoil from this efficient series.[19] Second, Langley did such an outstanding job reducing the drag of the Buffalo that the army and navy were soon sending all of their new prototypes to the lab for drag cleanup. Between April 1938 and November 1940 the LMAL gave 18 different military prototypes thorough goings-over in the FST to see if the airplanes could be bettered in any particular (see the table below).

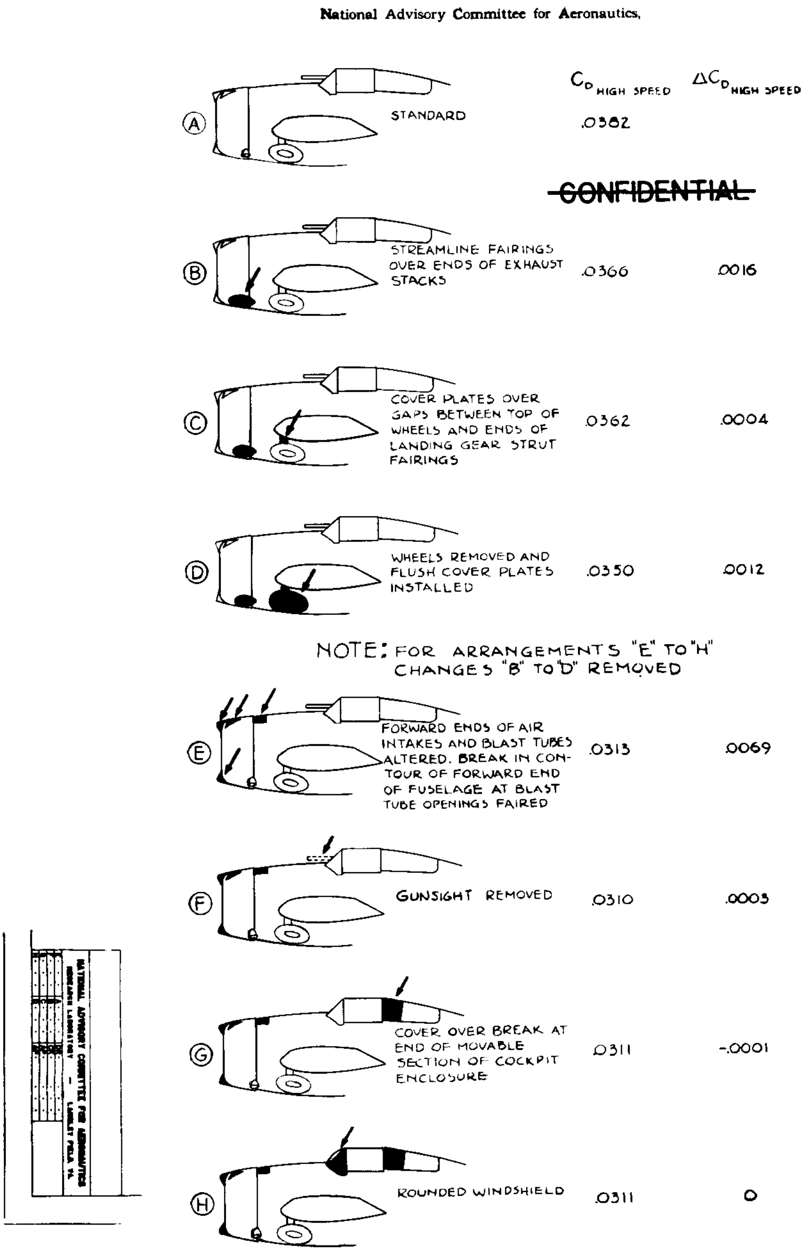
Langley Drag Reduction Program
April 1938–November 1940

RA no.	Date	Airplane
603	June 1938	Brewster XF2A-1 Buffalo
606	June 1938	Grumman F3F-2
607	June 1938	Grumman XF4F-2 Wildcat
633	August 1938	Vought-Sikorsky SB2U-1 Vindicator
635	August 1938	Curtiss XP-37
636	August 1938	Curtiss P-36A Mohawk
637	August 1938	Curtiss XP-40 Kittyhawk
646	December 1938	Douglas XBT-2
647	December 1938	Curtiss YP-37
672	June 1939	Seversky XP-41
674	June 1939	Bell XP-39 Airacobra
695	September 1939	Curtiss XP-42
698	September 1939	Grumman XF4F-3 Wildcat
709	November 1939	Curtiss XP-46
739	May 1940	Republic XP-47 Thunderbolt
746	September 1940	Chance Vought XF4U-1 Corsair
796	October 1940	Brewster XF2A-2 Buffalo
797	October 1940	Curtiss XSO3C-1
811	November 1940	Consolidated XB-32 Dominator

Source: Langley research authorization files, Langley Historical Archive (LHA), Hampton, Va.

This program of specific configuration tests was of unprecedented proportions for the NACA laboratory, and Langley fulfilled its responsibility systematically. Following the classic style of the successful cowling and airfoil series research programs, the FST team perfected a method of experimental parameter variation. First, engineers examined the airplane in detail, identifying those of its external features most suspected of causing unnecessary drag. They then made the airplane as aerodynamically clean as possible, by carefully removing protuberances like the radio antenna and using putty or tape to cover holes and leaks and to reshape irregular surfaces such as the cockpit canopy. Following this, they mounted the plane in the test chamber, and measured its drag at various wind speeds.

In this faired and sealed condition, the airplane naturally proved to have less drag than the original body, but it was impossible for this pristine

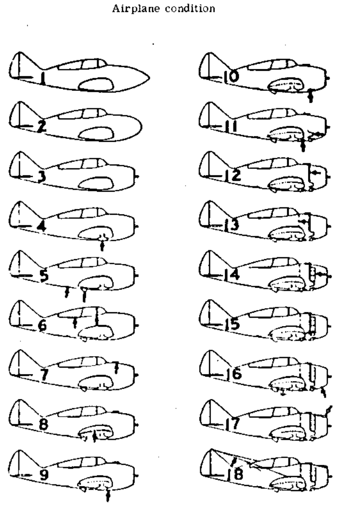


		C_{D} HIGH SPEED	ΔC_{D} HIGH SPEED
Ⓐ	STANDARD	.0382	
Ⓑ	STREAMLINE FAIRINGS OVER ENDS OF EXHAUST STACKS	.0366	.0016
Ⓒ	COVER PLATES OVER GAPS BETWEEN TOP OF WHEELS AND ENDS OF LANDING GEAR STRUT FAIRINGS	.0362	.0004
Ⓓ	WHEELS REMOVED AND FLUSH COVER PLATES INSTALLED	.0350	.0012

NOTE: FOR ARRANGEMENTS "E" TO "H" CHANGES "B" TO "D" REMOVED

		C_{D} HIGH SPEED	ΔC_{D} HIGH SPEED
Ⓔ	FORWARD ENDS OF AIR INTAKES AND BLAST TUBES ALTERED. BREAK IN CONTOUR OF FORWARD END OF FUSELAGE AT BLAST TUBE OPENINGS FAIRED	.0313	.0069
Ⓕ	GUNSIGHT REMOVED	.0310	.0003
Ⓖ	COVER OVER BREAK AT END OF MOVABLE SECTION OF COCKPIT ENCLOSURE	.0311	-.0001
Ⓗ	ROUNDED WINDSHIELD	.0311	0

LMAL chart of the test arrangements for the Brewster XF2A-1 Buffalo. The two columns of numbers show quantitatively the effects of the configuration variations.



Condition number	Description	C_D ($C_L = 0.15$)	ΔC_D	ΔC_D, percent[a]
1	Completely faired condition, long nose fairing	0.0166		
2	Completely faired condition, blunt nose fairing	.0169		
3	Original cowling added, no airflow through cowling	.0186	0.0020	12.0
4	Landing-gear seals and fairing removed	.0188	.0002	1.2
5	Oil cooler installed	.0205	.0017	10.2
6	Canopy fairing removed	.0203	-.0002	-1.2
7	Carburetor air scoop added	.0209	.0006	3.6
8	Sanded walkway added	.0216	.0007	4.2
9	Ejector chute added	.0219	.0003	1.8
10	Exhaust stacks added	.0225	.0006	3.6
11	Intercooler added	.0236	.0011	6.6
12	Cowling exit opened	.0247	.0011	6.6
13	Accessory exit opened	.0252	.0005	3.0
14	Cowling fairing and seals removed	.0261	.0009	5.4
15	Cockpit ventilator opened	.0262	.0001	.6
16	Cowling venturi installed	.0264	.0002	1.2
17	Blast tubes added	.0267	.0003	1.8
18	Antenna installed	.0275	.0008	4.8
	Total		0.0109	

[a] Percentages based on completely faired condition with long nose fairing.

Experimental parameter variation of drag sources on the Seversky XP-41 airplane, summer 1939. (From Paul L. Coe, Jr., "Review of Drag Cleanup Tests in Langley Full-Scale Tunnel," NASA TN D-8206, 1976.)

shape, with essential parts covered up or removed, actually to fly. The wind tunnel workers returned the plane to its service condition item by item and evaluated the change in drag caused by each action. In the case of the cleanup tests of the Seversky XP-41 in late 1939, for example, Langley studied the drag of the airplane in 18 different configurations. The data indicated that the changes in drag values corresponding to the steps of the cleanup process were generally small, amounting to only a few percent of the total drag coefficient and thus involving only small speed changes. Taken together, however, increments like these often resulted in impressive gains in total performance.[20]

The NACA did its best to help industry realize these dramatic increases of speed in production aircraft. This effort can be seen clearly in Langley's cleanup of the Bell XP-39 Airacobra, eleventh in the series of military planes subjected to the NACA operation. Bell's chief engineer Robert J. Woods (a former LMAL employee in Eastman Jacobs's VDT section) had designed the unconventional plane—its power plant amidships, at the center of gravity, and its cannon in the nose—as a 400-MPH fighter. At Wright Field in the spring of 1939, the unarmed XP-39 prototype (with a turbosupercharged Allison engine, rating 1150 horsepower) flew to a maximum speed of 390 MPH at 20,000 feet. The aircraft reached this speed, however, with a gross weight of only 5550 pounds, thought to be about a ton less than a heavily armored production P-39. That meant that the existing aircraft, when normally loaded, would have a hard time exceeding 340 MPH. Still, the test performance impressed the Air Corps enough for it to issue a contract, three weeks later, for 13 production model YP-39s. Gen. Henry H. "Hap" Arnold, desperate for a new fighter, hoped that the speed of the airplane could be increased to over 400 MPH by cleaning up the drag. On 9 June 1939 he formally requested NACA approval for immediate testing of the XP-39 in the Full-Scale Tunnel.[21]

Actually Langley had received the XP-39 from Wright Field three days before Arnold's request, which had been put in writing on 6 June to satisfy NACA headquarters. On 8 June, Robert Woods and other representatives from Bell arrived at Langley to see the NACA's experimental setup and witness the initial round of tests. For the next two months the FST team systematically investigated the airplane's various sources of drag. On 10 August, Lawrence D. Bell, president of the Bell Aircraft Company, visited Langley to discuss the test results obtained to date. Bell was shown preliminary data from the FST indicating that the prototype in a completely faired condition had a drag value of only 0.0150 compared to 0.0316 in the original form. This meant a maximum increase in speed, if all the NACA's suggestions for drag improvement were met, of 26 percent. The NACA realized, of course, that not all of the changes to the configuration studied in the FST were feasible for the production aircraft. Fifteen days later, the head of the FST team reported that by cuffing the propeller at the point where it met the hub, streamlining the internal cooling ducts of the wings, lowering the cabin six inches, decreasing the size of the wheels so that they could be completely housed within the wing, and removing the turbosupercharger and certain air intakes, the speed of the XP-39 airplane for a given altitude and engine power could be increased significantly. Extrapolating from the same weight airframe to a more powerful (1350-horsepower) engine with a geared supercharger, he estimated that the top speed attainable with the aircraft might be as high as 429 MPH at 20,000 feet. The FST head did not know precisely how much additional air would be required to cool the bigger engine, but he did believe that even if this increase was very large, it would not prohibit the plane from obtaining at least 410 MPH.[22]

Bell incorporated enough changes recommended by the NACA to improve the speed of the airplane by about 16 percent. These changes included installation of an engine that could be equipped with a gear-driven supercharger but had only 1090 horsepower—60 horsepower less than the

The army's Bell P-39 Airacobra in flight over Langley Field, 1943. The pilot of this airplane sat on the front end of the gearbox with the engine behind him and the propeller shaft passing underneath his legs. The P-39 was one of the first military airplanes fitted with a tricycle landing gear.

engine which had driven the unarmed XP-39 to 390 MPH at Wright Field in the spring of 1939 (and 260 horsepower less than that used hypothetically by the FST head in his paper study). The Air Corps then resumed flight trials. The less powerful aircraft, redesignated XP-39B, weighed some 300 pounds more than the original, and without the turbosupercharger flew to a maximum speed of 375 MPH at 15,000 feet in the first trials. Both the Air Corps and Bell expressed satisfaction with the NACA results. In January 1940 the Air Corps told Bell to finish the production of the first series of YP-39s without turbosuperchargers. (The Bureau of Aeronautics called the NACA report on the XP-39B the "worst condemnation of turbo supercharging to date.")[23] Soon thereafter Lawrence Bell informed George Lewis that

> as a result of the wind tunnel tests at Langley Field, we are getting extraordinarily satisfactory results. From all indications the XP-39 will do over 400 m.p.h., [even] with 1150 H.P. All of the changes were improvements and we have eliminated a million and one problems by the removal of the turbosupercharger. ... The cooling system is the most efficient thing we have seen. The inlet ducts on the radiator are closed up to 3% and the engine is still over cooling. ... I want to convey to you personally and your entire organization ... our very deep appreciation of your assistance in obtaining these very satisfactory results.[24]

The top speed of the Airacobra never came anywhere near 400 MPH during this second round of flight trials—for that matter, no version of the P-39 ever would. However, the plane showed reasonable stability and roll rates and maneuverability at low altitudes—attributes that were not due to NACA drag testing—which meant it would be useful in ground support as a strafer and fighter-bomber.[25]

The Army Air Corps seems to have left the problem of increasing the speed of the XP-39 to over 400 MPH to Langley. On 6 February 1940, General Arnold's office advised the NACA to make any modifications its staff thought necessary "which do not involve structural change to the airplane." NACA headquarters responded with word that "the entire investigation should be carried out in flight" at Langley Field. At first, this appeared possible: during a telephone conversation with George Lewis on the morning of 28 February, General Arnold said that if the NACA felt the best way to increase the speed of the Airacobra to over 400 MPH was to make flight tests with the airplane at Langley, Langley "should do that and, if necessary, get a pilot from Wright Field."[26]

However, the Air Corps, Bell, and the NACA soon agreed that "these tests could be better conducted first in the Full-Scale Tunnel."[27] In early March the XP-39B was flown to Langley from Bolling Field, where it had undergone performance tests, and was again mounted immediately in the FST. Within a few weeks the FST team finished another systematic drag investigation, this time concentrating on internal flow problems. Little more could be recommended to improve the airframe, however, because within the poorly designed ducts were structural members for the wings which could not be altered without some basic reconstruction of the aircraft.[28] A flight test program followed (at Wright, not Langley, Field).

"In order to provide for the possibility of additional tests being requested by the Air Corps," George Lewis notified Langley to keep the research authorization (no. 674) covering drag cleanup of the XP-39 open.[29] For the next several months, Langley sent representatives to both Wright Field and the Bell plant in Buffalo to make sure that the major modifications called for by the FST analysis (such as the installation of propeller cuffs and wheel well covers, the latter being "the most likely possibility for large drag reduction") were being carried out properly.[30] In September 1940 the first YP-39, having incorporated most of the suggestions called for by the NACA, flew, top speed 368 MPH at 13,300 feet. Deliveries of the first production model P-39s, which were very similar to the service-test YP-39, began four months later. In 1941 the United States sent nearly 700 Airacobras to Great Britain and the Soviet Union under Lend-Lease. After the Japanese attack at Pearl Harbor, the Air Corps rushed P-39 units into action in the South Pacific.

Because these P-39s flew well below 400 MPH, with a slow rate of climb and a low ceiling, Bell asked the NACA for another round of tests in the FST. Langley answered that it seemed "unlikely that further tests in the Full-Scale Tunnel would result in any other suggestions than those already made as a result of the tests of the experimental model."[31] LMAL engineers did suggest two ways to boost the aircraft's speed by modifying the exhaust stacks for auxiliary thrust, but neither earned much support.[32]

The first unarmed P-39 prototype had flown 390 MPH, faster than any subsequent P-39, but 10 miles per hour slower than Bell advertised. The maximum speed of the production P-39D was only 368 MPH. Thus to assert that NACA drag testing helped the airplane to pick up speed may not appear to make sense: how could it make sense when, in spite of the NACA improvements, the production model flew slower? The answer to this riddle is weight. The army added a new and bigger power plant and heavier armor plate to the production model. (The XP-39E would weigh nearly 9000 pounds!) Based on drag coefficients from the FST, it seems that the NACA drag cleanup recommendations improved the speed of the airplane by a dramatic 16 percent.[33] In other words, if the P-39 had not gone through drag testing, it would have been slower than it ultimately was.

The drag reduction program required precisely the kind of systematic wind tunnel work that Langley did best. The lab had derived its original families of airfoils in the VDT, and its first low-drag cowlings in the PRT, according to the method of experimental parameter variation; similarly, it cleaned up the drag problems of the American military aircraft that fought World War II. Here again, as in the other two cases, the NACA engineers were demonstrating how the correct design of small details improved the performance of an aircraft. The significance of this work should not be underestimated: by pointing out ways for these aircraft to gain a few extra miles per hour, the NACA effort might often have made the difference in performance between Allied victory and defeat in the air. Moreover, the program also had an impact on the shape of postwar technology. Specialists in the analysis of engine cooling and duct design—like physicist Kennedy F. Rubert, who had worked as an integral part of the FST drag reduction team during the war—formed the nucleus of a new Induction Aerodynamics Laboratory at Langley in 1946. In this facility, researchers investigated the aerodynamics of subsonic and supersonic internal flows, concentrating on solving such basic problems as the optimum method of inducing air and supplying it to high-speed conventional and jet engines.

Meeting Manpower Needs after Pearl Harbor

From 1939 to 1941, as the drag reduction program picked up speed in preparation for direct American involvement in World War II, Langley increased its total manpower from 524 to 940 employees. Though 416 more employees in two years' time constituted unprecedented growth for the NACA staff—especially considering that the LMAL was simultaneously losing personnel to help organize and operate new laboratories at Moffett Field and Cleveland—this expansion was minor in comparison with what happened after Pearl Harbor. On 6 December 1941 Langley still had fewer than 1000 employees, and of those, fewer than 300 were professionals. On V-J day in August 1945, the lab had more than 3200 employees, including more than 800 professionals. In a span of less than four years of war, personnel more than tripled.

Accomplishing an expansion of this magnitude during national mobilization for war was a prodigious, uphill task which constantly threatened to become a Sisyphean labor. By the end of 1941 the NACA had already tapped deeply into the already short supply of American aerodynamicists, engineers, technicians, and mechanics in order to get ready for its expanded role during a war. Once the nation began fighting, everything about that involvement operated to reduce the supply of desirable personnel even further.

The NACA's biggest personnel problem was the drain of qualified men to selective service. In 1938 the Special Committee on Relation of NACA to National Defense in Time of War had counseled the government to keep all NACA employees working in civilian status as employees of an "essential industry" in the event of war. The idea was to preserve NACA effectiveness by keeping personnel from jumping to jobs in manufacturing to avoid the conscription that would probably take place.[34] The committee had advised against blanket deferment. It said that deferment arrangements satisfactory to both the NACA and the armed services could be made promptly with the proper authorities if and when war came.

President Roosevelt incorporated this advice into his mobilization plan of 1939. Unfortunately, when the United States entered the war two years later, deferments for NACA personnel were not as easy to come by as had been anticipated in 1938. LMAL personnel officers had to travel to Richmond at least once a month to negotiate for the deferment of "essential" employees with the director of the state board on a case-by-case basis. Their efforts were not always successful: Langley lost more employees to military induction in certain months of 1942 and 1943 than it was able to recruit.[35] John Victory appealed to selective service director Lewis B. Hershey for special consideration of the NACA's unique role in the war effort, but nothing agreeable to both parties was worked out until early 1944. According to Victory, one idea offered by Hershey in 1943 was militarization of all the NACA's physically able males; however, everyone working for the committee from the director of research in Washington down to the junior engineering aide at Langley opposed being blanketed into military service. In a military organization, they feared, rank insignia would ultimately count more than what an individual knew about solving research problems.[36]

Ironically, it was military service in the end that enabled the NACA to circumvent the unsympathetic selective service policy. On 1 February 1944, the army and navy agreed to a plan which, when approved ten days later by President Roosevelt, called for the induction into one of the armed services of all draft-age NACA employees. According to the scheme (which was modeled after one devised by the army in 1943 to take care of employees of the civilian flight training schools under contract to the Air Corps), all eligible employees at Langley would join the Air Corps Enlisted Reserves (ACER) and then be placed immediately on inactive status under the exclusive administrative management of the NACA. (Those holding reserve commissions resigned them to become members of the ACER.) When a Langley man was called for induction, he was given a letter prepared by the NACA personnel office, which he took with him to the induction station in Richmond. This letter indicated the procedure for inducting and placing essential NACA employees in the ACER. The man spent 24 hours in the state capital undergoing a physical examination and completing forms and other induction measures. Then he recited an oath of induction that included a reference to his assignment to the ACER on an inactive status, and went home. He received no military training, never wore a uniform, and spent absolutely no time on active duty. Through rigorous compliance with this system, Langley was able to retain most of its professional staff for the remainder of the war. Following the surrender of the Japanese in August 1945, all NACA members of the ACER were granted honorable discharges.[37]

So the army-navy-NACA plan of February 1944 resolved the problem of keeping essential employees at Langley. On the negative side, though, the contrivance at the heart of the plan naturally upset some Hampton citizens who saw sons and brothers being drafted, sent to war, and killed.[38] Also, the plan came much too late to resolve Langley's other wartime personnel problem, the lack of engineering, technical service, and administrative laborers in sufficient numbers to meet the dramatically increased post–Pearl Harbor workload.

EXTRA **EXTRA**

LMAL BULLETIN

LANGLEY MEMORIAL AERONAUTICAL LABORATORY

Issue 10, Vol. 3 Langley Field, Virginia, February 29, 1944 Page 1

ANNOUNCE ARMY-NAVY PLAN REGARDING

ESSENTIAL DRAFT ELIGIBLE EMPLOYEES

H. J. E. Reid, Engineer-in-Charge, today announced a plan of the War and Navy Departments by which draft eligible personnel of the Langley Laboratory who hold essential jobs may be approved for inactive reserve status in the Army Air Corps and returned to their duties at LMAL.

The plan, known as the Joint Army-Navy NACA Plan, is similar to that used last year in the case of the Civilian Flying Training Schools under contract to the Army Air Forces. The plan as approved by the President is a result of joint action by the War and Navy Departments and was made necessary by an insufficiency of qualified personnel to carry on aeronautical research for the Army and Navy.

Mr. Reid explained that as each essential employee of the Laboratory is called for induction into military service, he will be given a letter for delivery at the induction station. That letter, which will be prepared at the Laboratory, will indicate the procedure for inducting and placing in the Air Corps Enlisted Reserve (ACER) those essential employees who are inducted into military service under the Selective Service System. When such employees are placed in the ACER, they will return to duty at the Laboratory.

After the plan goes into effect, it will supersede the present replacement schedule system which calls for occupational deferments. Deferments on other grounds will not be affected by the plan. Under the plan, each essential employee who is inducted and placed in the ACER will remain on his job at the Laboratory for so long as the Army and Navy deem it expedient for the welfare of the nation.

Mr. Reid announced that further details regarding the plan will be explained within a few days to those persons affected.

In a special February 1944 edition of the LMAL Bulletin, *the engineer-in-charge announced a joint army-navy-NACA plan that placed essential lab employees who were eligible for the draft into the Army Air Corps Enlisted Reserve.*

The lab had launched a vigorous recruitment campaign reaching far beyond traditional sources of new employees as soon as the war started. It sent scouts up and down the Atlantic coast to recruit anyone who looked even marginally qualified for the dozens of vacant positions in research, technical service, and administration. Automobile mechanics along the Maryland Eastern Shore were persuaded to leave their garages and come to Langley to work on aircraft engines; blacksmiths from the mountains of western Virginia agreed to try their hands at aircraft sheet metal work; and loom fixers who had been working in North Carolina textile mills proved to be effective wind tunnel mechanics. Recruits from other locales whose raw skills translated less directly into useful NACA work were placed into a new LMAL apprentice program, taught by the lab's own journeymen. By the end of the war this program had graduated nearly 400 men for the difficult work expected at Langley from draftsmen, machinists, metalsmiths, toolmakers, model makers, and the like.[39]

LMAL personnel officers utilized the talents of the hometown population more fully also. They hired hundreds of boys for part-time work as shop assistants, messengers, and model makers, and encouraged mathematics and English teachers from the local school systems to capitalize on their summer vacations by taking positions as computers and technical report editors. Many of the teachers chose to stay on at Langley permanently because their new jobs with the government were more interesting, paid better, had more fringe benefits, and related more concretely to the war effort than teaching school.[40]

Women in numbers came to work at Langley for the first time, many of them to do jobs formerly done only by men. Before the war the lab had never employed more than 100 women at one time, mostly for traditional office functions as secretaries, stenographers, typists, mail sorters, payroll and file clerks, telephone operators, and receptionists. The exception to the rule was Pearl I. Young (1895–1968), the NACA's first female professional. A Phi Beta Kappa graduate in physics from the University of North Dakota, Young reported to work at Langley in April 1922. Her first assignment was in the Instrument Research Division, where she worked side-by-side with Henry Reid, the future engineer-in-charge. In the late 1920s, when she suggested the need for a technical editor at Langley, she was promptly given the job. In this position she published a *Style Manual for Engineering Authors* (1943) which was consulted frequently by employees both at Langley and at the other NACA centers.

In some respects, Pearl Young led the way for working women at Langley.* By war's end, nearly 1000 women worked at Langley—practically one-half of the nonprofessional staff and one-third of the entire staff. The majority of the women continued to do the quiet, unspectacular jobs involved in keeping the wheels of the government laboratory running smoothly through the welter of paperwork, but many of them rolled up their sleeves, donned shop aprons, and pitched in to do whatever work had been made necessary for them by the war. Women set rivets, operated spray guns and welding irons, polished wind tunnel models, and drove buses and trucks around the field. Others served as technical illustrators and draftsmen. One woman drove the towing carriage in the hydrodynamics research tank. The Structures Division, which operated its own training school, assigned women to take strain-gauge measurements. Female computing units (one of them made up entirely of black women) were added to several of the individual wind tunnel staffs. These distaff units took over most of the slide rule work and curve plotting formerly done by the engineers.[41] Only a few women held engineering posts, and they were not assigned to the wind tunnels. A number of females with the rating of "minor laboratory apprentice" were used, however, as mechanics' helpers to relieve hard-pressed junior engineers of many duties associated with tunnel operation and laboratory procedure. On the whole, the women who worked at Langley during and after World War II could not advance as far or as fast up the civil service ladder as could even some men with inferior talents; nonetheless, most of them still believe today that the NACA's treatment of women was better than the treatment of women by many other contemporary employers.[42]

With the army's cooperation, Langley also recruited a number of returning servicemen, mostly for technical service occupations. At regional

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* In 1943 Pearl Young moved from Langley to the Aircraft Engine Research Laboratory in Cleveland. She stayed there until 1947, when she accepted a position teaching engineering physics at Pennsylvania State University at Pottsville as an assistant professor. In 1958 she returned to the Cleveland laboratory (now NASA Lewis Research Center) to do special bibliographical work on the spectroscopy of plasmas. After retiring from NASA in 1961, she taught physics for two years at Fresno State College in California.

Young loved flight. Her most memorable experience was the trip she made to Europe in 1936 aboard the *Hindenburg*; she was one of only 50 passengers making the first west-to-east flight of the great German airship. Her favorite hobby was aeronautical history. Starting in 1947, she gathered a wealth of material for a biography of the French engineer and aeronautical pioneer Octave Chanute. She also collected and indexed information on Francis Wenham, the builder of the first wind tunnel, Ferdinand von Zeppelin, and Alphonse de Penaud. These materials are preserved as part of the aeronautical collection at the Denver (Colorado) Public Library.

redistribution centers (in Atlantic City, Miami, and Santa Monica, California), army orientation programs gave these men an idea of the reassignments available to them, including jobs with the NACA at Langley, Ames, and the Flight Propulsion Laboratory in Cleveland. If a man thought he was qualified for one of these jobs, he was interviewed by an army classification officer who had lists of the available occupations at the three NACA laboratories and of the necessary qualifications for them. If the classification officer felt that the applicant was qualified for a particular position, he referred him to the NACA representative at the orientation center, who, if satisfied, requested that the serviceman be sent to one of the laboratories for an interview. If the lab's personnel officer then decided to offer an appointment, the NACA sent a letter to the army stating that fact and requesting that the applicant be transferred from active duty to reserve status at the lab.[43]

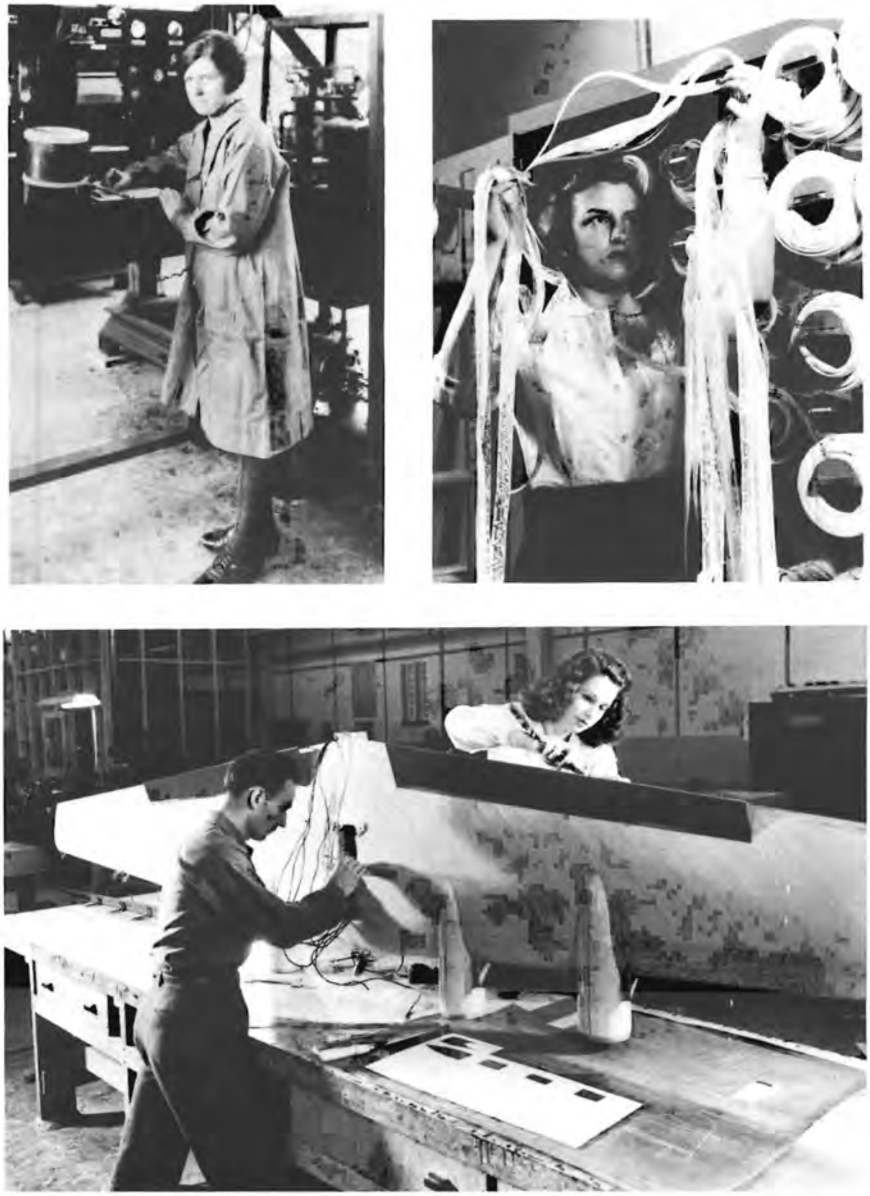
Irreversible Changes

According to one man who worked at Langley before, during, and after the war, the influx of thousands of new and different employees caused certain "irreversible changes" in the Langley personality:

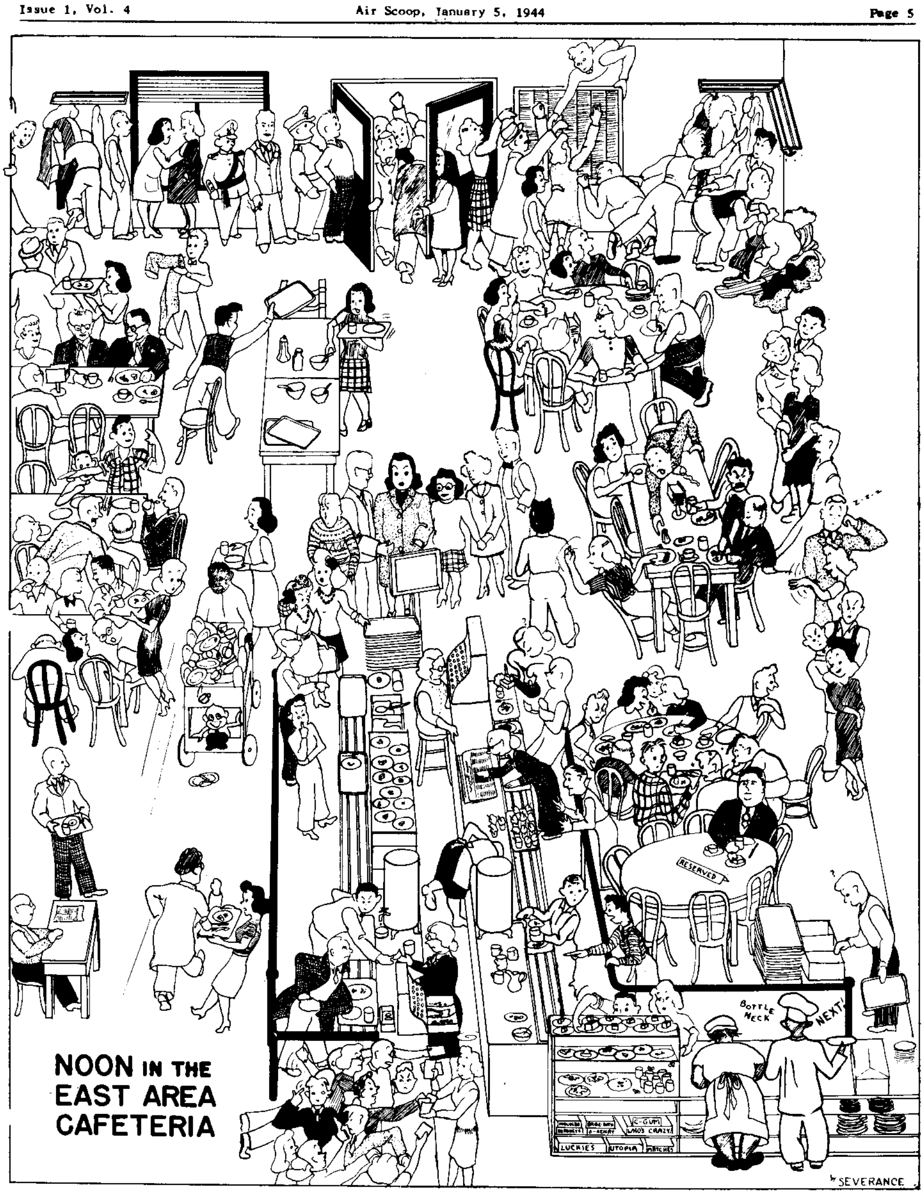
> The selective standards which had provided the exceptional talent of the [1920s and 1930s] had to be abandoned. Both the quality and the per capita yearly output of reports declined. Not a few of the newcomers hinted openly that immunity from the draft was the reason they had come. The increased wind-tunnel testing of specific military designs provided convenient undemanding assignments for the less-talented new engineers. The term "wind-tunnel jockey" was coined during the war and is still used today to describe inveterate tunnel operators.

In the 1920s and 1930s the entire professional staff was so small that everyone had known each other on a first-name basis, gathered together in one room for meetings and lunch, and partied as a group. Now there were "hordes of weak performers ... who were OK for the double-shift testing [of specific aircraft] needed during the war" cluttering up the buildings, shops, and cafeteria.[44]

Lab veterans seem to have snubbed or ostracized only those new employees who proved themselves technically incompetent. "We gave every newcomer the benefit of the doubt," the same man recalls, "at least until their limitations had become unmistakable. A minority proved to be good researchers, and quite a few with marginal technical qualifications and abilities were retained because of their likable personalities, loyalty, and reliability as team members." Competent newcomers and those well-liked



The contributions of women to NACA research began well before World War II, but did not truly expand until after 1941. Pearl I. Young (top left) works in the instrument research laboratory, about 1929; an unidentified female handles rolls and rolls of computer tape; another woman helps to finish the wing of a flying boat model in the dynamic model shop, 1944.



Issue 1, Vol. 4 Air Scoop, January 5, 1944 Page 5

The LMAL cartoonist captured the hectic wartime lunchroom for the Air Scoop *in January 1944. Notice that there is one reserved table. The drawing reflects much about the times.*

persons who then worked at the lab long enough to become one of the old crowd relate today that during the war Langley veterans indeed went out of their way to make new men and women feel welcome and to assimilate them into the lab's business and social life.[45]

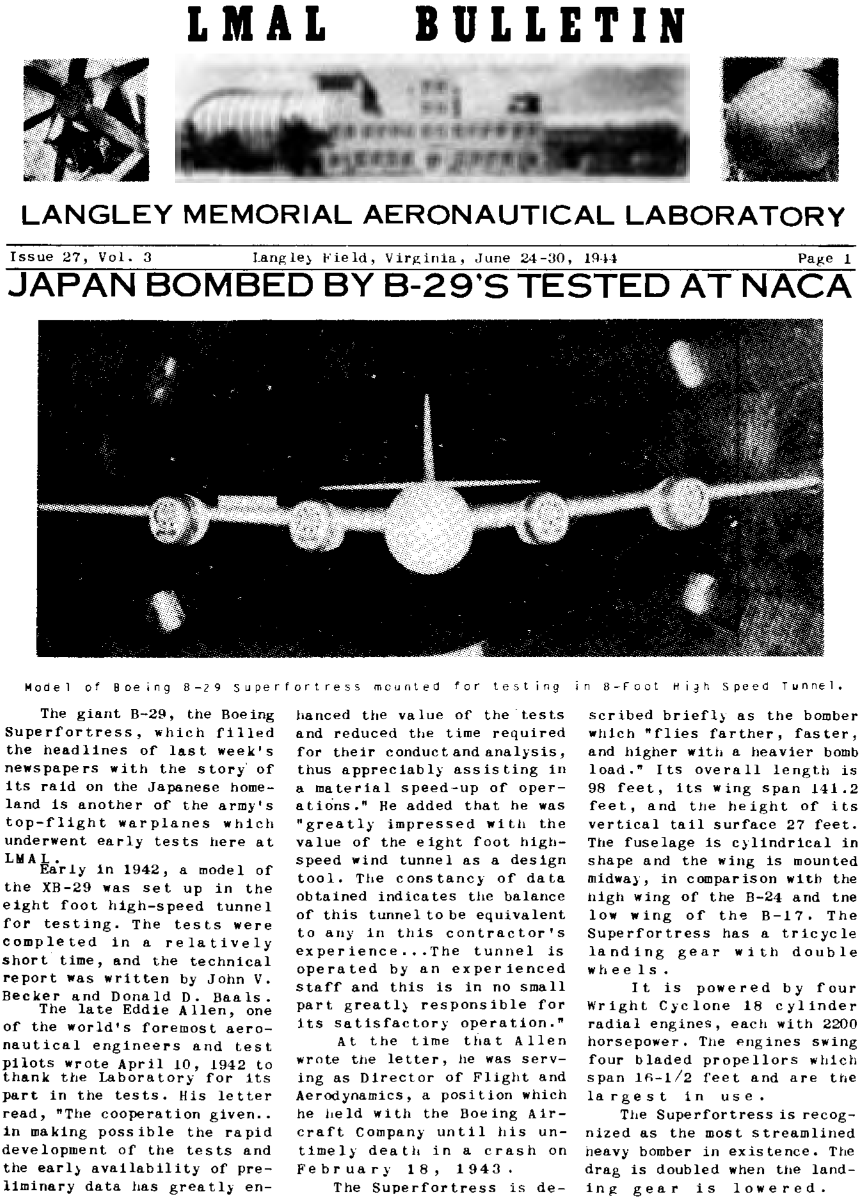
The assignment of laboratory personnel to buildings in east and west areas and their parceling into a great number of sections and branches did tend to undermine the unity of prewar staff, however. Some of the old bases of influence were broken up. In February 1944, for instance, management dissolved Eastman Jacobs's Air-Flow Research Division and divided its personnel between various sections of the Full-Scale Research Division and of the new Compressibility Research Division.[46] Compensating for this fragmentation in the life of Langley was a new organ of communication between employees, the *LMAL Bulletin* (renamed the *Air Scoop* in 1944). A survey of the articles, photographs, and illustrations in this in-house newspaper shows that what pulled together heterogeneous staff members more than anything else was awareness of their organization's special responsibility in winning the war.

This wartime responsibility required certain changes in traditional NACA policies and practices. A major change occurred in the NACA's relationship with industry. Before 1939, the laboratory had endeavored to protect itself from becoming a consulting service. NACA policy had not allowed representatives from industry to serve as such on the Main Committee. Also, as shown in chapter 6, prewar Langley tried hard to remain impartial in its dealing with different companies, instituting strict visitation rules and refusing to release advance information—and even some information that companies considered proprietary—to individual interests. Also, the lab purchased outright virtually all equipment necessary for the conduct of test programs, or borrowed the equipment from one of the military services; it would not accept free of charge the ownership of any company's equipment or products.

With the outbreak of war, however, the task of perfecting America's combat airplanes required the NACA to loosen its policies and practices in relation to industry. In 1939 George J. Mead, recently retired vice-president for engineering of the United Aircraft Corporation, became vice-chairman of the NACA and chairman of its Power Plants Committee. This was "as close as the NACA had yet come to placing an industry representative on the Main Committee or in the chair of one of the main technical committees."[47] Though there was no causal link to Mead's appointment, Langley and the other NACA centers soon "became overrun by large numbers of industrial scientists, engineers, and technicians who witnessed tests relating to the designs of their companies, actively assisted in the conduct and planning of such tests, talked with a new freedom with NACA employees about research-in-progress, received much advance information of a very tentative character, and sometimes used every possible opportunity to spy out what was being done for their competitors."[48] According to NACA statistics, there was an average of 45 industry representatives present at Langley each day in 1943; this compared to a daily average of less than three there during a twelve-month period in 1935 and 1936.[49] Keeping happy these clients, who were under pressure to meet the needs of the military, while avoiding conflicts of interest, was a tall order for the LMAL staff, the newest members of which were unfamiliar with the old ways of successfully conducting such subtle business. Thanks largely to careful NACA management, which was handled almost entirely by men trained in the old ways, a degree of decorum satisfactory to the NACA was preserved.

This change in the way the NACA served industry affected its publications practices. Before the war, nearly all NACA reports could be given wide circulation because they were not restricted by military security classification. Though its clients even then wanted the NACA to share its test results and other new information as rapidly as possible—so that they could put it to good use in aircraft design—the NACA could, in relative terms, take its time writing, editing, and distributing publications because there was no great national urgency. During the war, of course, requirements changed; in response to them, the Committee developed several new publications series, including the Advance Confidential Report (ACR), Advance Restricted Report (ARR), Confidential Bulletin (CB), Confidential Memorandum Report (CMR), Restricted Bulletin (RB), and Restricted Memorandum Report (RMR). Some reports were stamped "Secret," meaning that the content and very existence of the paper was to be made known to the absolute minimum number of people who in connection with official duties had of necessity to be informed. Also, secret reports were to be transmitted from one authorized person to another by hand or by registered mail, locked in the most secure space available at all times when not in use, and disposed of only by being burned in the presence of an authorized witness. These precautions included all preliminary drafts, stenographic notes, stencils, work sheets, and carbon copies. Though the distinction between confidential and restricted papers was fuzzy, each type was meant to relay quickly, effectively, and privately—to selected parties in industry and the services—information considered vital to carrying on the war effort in the air.[50]

Thus another change brought on by the war was stepped-up laboratory security and increased recognition of the need for protecting national scientific and technological information. Until the mid-1930s, Langley



LMAL BULLETIN

LANGLEY MEMORIAL AERONAUTICAL LABORATORY

Issue 27, Vol. 3 Langley Field, Virginia, June 24-30, 1944 Page 1

JAPAN BOMBED BY B-29'S TESTED AT NACA

Model of Boeing B-29 Superfortress mounted for testing in 8-Foot High Speed Tunnel.

The giant B-29, the Boeing Superfortress, which filled the headlines of last week's newspapers with the story of its raid on the Japanese homeland is another of the army's top-flight warplanes which underwent early tests here at LMAL.

Early in 1942, a model of the XB-29 was set up in the eight foot high-speed tunnel for testing. The tests were completed in a relatively short time, and the technical report was written by John V. Becker and Donald D. Baals.

The late Eddie Allen, one of the world's foremost aeronautical engineers and test pilots wrote April 10, 1942 to thank the Laboratory for its part in the tests. His letter read, "The cooperation given.. in making possible the rapid development of the tests and the early availability of preliminary data has greatly enhanced the value of the tests and reduced the time required for their conduct and analysis, thus appreciably assisting in a material speed-up of operations." He added that he was "greatly impressed with the value of the eight foot high-speed wind tunnel as a design tool. The constancy of data obtained indicates the balance of this tunnel to be equivalent to any in this contractor's experience...The tunnel is operated by an experienced staff and this is in no small part greatly responsible for its satisfactory operation."

At the time that Allen wrote the letter, he was serving as Director of Flight and Aerodynamics, a position which he held with the Boeing Aircraft Company until his untimely death in a crash on February 18, 1943.

The Superfortress is described briefly as the bomber which "flies farther, faster, and higher with a heavier bomb load." Its overall length is 98 feet, its wing span 141.2 feet, and the height of its vertical tail surface 27 feet. The fuselage is cylindrical in shape and the wing is mounted midway, in comparison with the high wing of the B-24 and the low wing of the B-17. The Superfortress has a tricycle landing gear with double wheels.

It is powered by four Wright Cyclone 18 cylinder radial engines, each with 2200 horsepower. The engines swing four bladed propellors which span 16-1/2 feet and are the largest in use.

The Superfortress is recognized as the most streamlined heavy bomber in existence. The drag is doubled when the landing gear is lowered.

From the LMAL Bulletin, *24–30 June 1944.*

From the Langley Air Scoop, *13 April 1945.*

employees showed little concern for such things. Involvement in very few research programs required security clearances, meaning that very few people were prevented by the lack of identification badges from entering wind tunnel buildings or other facilities. Beginning in June 1937, the Committee began to tighten security regulations, announcing that foreign visitors to Langley would be allowed only to see the lab's exterior—though this did not stop five members of the Japanese Imperial Army from making an inspection tour in July.[51] However, visitors were often given sanitized tours in which they saw nothing of real technological significance.

Major steps to increase security and protect secrets came only after the attack at Pearl Harbor. In January 1942 the LMAL engineer-in-charge told his division chiefs to maintain constant surveillance for "probable fifth column activities such as agitation, propaganda, espionage, sabotage, and actual physical attack."[52] Placards were posted around the lab warning employees of the dangers of loose talk. After the FBI informed the Office of Naval Intelligence that "an outside confidential source" had overheard "young boys employed at the National Advisory Committee for Aeronautics, Hampton, Virginia" discussing, at the Langley Sweet Shop, "laboratory tests on new types of planes," the engineer-in-charge instructed the head of the apprentice program to deliver a talk to the boys on the dangers of discussing any of their work, including that of their model airplane clubs, in public places.[53] In 1943 the Committee published an information pamphlet entitled "Don't Talk." This pamphlet listed ten rules, including: "The research on and the results of each project at the Laboratory are to be discussed only with those members of the staff who are connected with the subject," (rule no. 1); "Do not leave material of a Confidential or Restricted nature unattended in an exposed place while you are on duty," (rule no. 5); "Data and photographs obtained in connection with the Committee's research activity shall not be taken from the Laboratory nor shown outside the Laboratory without the specific permission of the Engineer-In-Charge," (rule no. 6); and "Technical information and data shall not be released in the form of a paper to be presented to an engineering or technical society if the information has not been released in an unclassified Committee publication that is issued before or at the same time as the paper is presented to the society," (rule no. 8). An NACA pamphlet warned workers that pursuant to federal laws regulating the disclosure of information affecting national defense, personnel found in violation of these rules were subject to a $10,000 fine and liable to 30-year imprisonment or the death penalty.[54] There is no evidence of any arrests for spying at Langley during the war, but LMAL security officers did complain frequently to NACA management about careless lapses by employees in abiding by minor security practices.[55]

In *Science in the Federal Government: A History of Policies and Activities to 1940*, historian A. Hunter Dupree observed that "many of the characteristics of the wartime research effort were in fact permanent changes in the government's relation to science."[56] Langley history seems to support Dupree's observation. Life at the NACA laboratory was changed significantly by the war: research was performed now not only at Langley but also at sister laboratories in California and Ohio; the size of the LMAL staff itself exploded from fewer than 500 to over 3000 members; women became vital members of the research and technical divisions; expenditures on the order of millions of dollars a year became established; in many of the wind tunnels, testing of production prototypes and production models themselves predominated, as opposed to the testing of purely experimental designs; and security regulations became more strict. By inheriting these changes, postwar Langley would be in several basic respects more like the wartime laboratory than like the laboratory of the 1920s and 1930s.

This was especially true for Langley's formal organization. In the 1920s and 1930s organization charts were drawn up only occasionally and with only a few simple divisions. By 1945, though, rapid expansion of the LMAL

Air Scoop

RESEARCH PIONEER SUCCUMBS

DR. LEWIS' CAREER VARIED, COLORFUL

Effective September 1, 1947, Dr. George W. Lewis resigned as Director of Aeronautical Research and was appointed Research Consultant to the Committee. He was succeeded as Director of Aeronautical Research by Dr. Hugh L. Dryden.

Dr. Lewis assumed the direction of the Committee's research activities in 1919 with the title Executive Officer, and in 1924 he was appointed Director of Aeronautical Research. He was awarded the Daniel Guggenheim Medal in 1936 for "outstanding success in the direction of aeronautical research and for the development of original equipment and methods." In 1937 he was appointed by President Roosevelt as plenipotentiary delegate of the United States to the Inter-American Technical Aviation Conference in Lima, Peru. He delivered the Wilbur Wright Memorial Lecture before the Royal Aeronautical Society in London in 1939 on the subject, "Some Modern Methods of Research in the Problems of Flight." He was awarded the Spirit of St. Louis Medal of the American Society of Mechanical Engineers in 1945. He has been active in scientific and engineering societies, and has served on a number of boards of award for aeronautical medals, contests, and trophies. He has been honored by election to the National Academy of Sciences, by the award of two degrees of Doctor of Engineering, and by a Life Membership in the National Aeronautic Association. He served as an official emissary of the United States Government in inspecting aeronautical research activities in various European countries, notably Germany and Russia, before the war.

DR. GEORGE W. LEWIS
1882-1948

At a meeting of the NACA Executive Committee held on August 11, 1947, at which the resignation of Dr. Lewis as Director of Aeronautical Research was announced, the members unanimously adopted a testimonial to Dr. Lewis "with our heart-felt thanks for all he has done for the Committee" and congratulating him for his long period of "exceptionally meritorious service to his country."

Air Scoop, July 16, 1948
Issue 28, Vol. 7

LAB PAYS TRIBUTE TO NACA OFFICIAL

Dr. George W. Lewis, 66, Research Consultant to the NACA and director of the Committee's research activities from 1919 to 1947, died Monday, July 12, at his summer home at Lake Winota, Pennsylvania.

Funeral services for the world-reknowned figure in aeronautical research were conducted Wednesday in Pennsylvania. The Laboratory paused a moment in silent tribute Wednesday to Dr. Lewis, who made frequent visits to Langley in the course of his official duties.

Dr. Lewis is survived by his widow, six children, and six grandchildren. A son, Leigh K. Lewis, is employed at the Laboratory in the West Mechanical Engineering Section.

Born at Ithaca, N.Y., in 1882, Dr. Lewis was educated at Cornell University. He received the honorary degree of Doctor of Science from Norwich University in 1934 and the honorary degree of Doctor of Engineering from the Illinois Institute of Technology in 1944.

During his service with the NACA, Dr. Lewis recruited and trained the research staff from a handful of workers into the present force of approximately 6,000 employees. He planned and carried through the unique research facilities at Langley, Moffett Field, and Cleveland, and has led an outstanding technical staff, from whom have come a succession of advances in aeronautical science.

Langley pays tribute to George Lewis, 16 July 1948.

Hugh L. Dryden (left), George Lewis's successor as the NACA's director of research, arrives with John F. Victory (center), the NACA's executive secretary, for a tour of the LMAL. Welcoming Dryden and Victory is engineer-in-charge Henry Reid.

staff and physical plant regularized and complicated the charts. By the end of 1956, less than a year before the Russians put *Sputnik 1* into orbit, Langley's 3300 employees were organized into 19 divisions, 50 branches, and 100 sections. Between March 1958 and November 1963, there were at least 28 different NASA organization charts (9 proposed and 19 authorized), more than NACA Langley produced in its first 28 years of operation.

Langley lost a major link with its past in July 1948: George Lewis died. Lewis had been sick for most of the war but pushed himself stubbornly from NACA laboratory to laboratory, overseeing the details of research. He resigned as director of research in September 1947. Before he left office, however, he said to Ira Abbott, his assistant: "I have given my life to the NACA. I want you to promise me that it will never become simply another Government agency interested chiefly in its own preservation and bureaucratic growth."[57] Succeeding Lewis was Hugh L. Dryden, former director of the National Bureau of Standards. Dryden did not have Lewis's zest for sitting down daily with politicians and military leaders to deal with the nitty-gritty of research appropriations and procurements, but he was scientifically sharper than the former director. Under Dryden's more formal and less paternal management, Langley researchers would extend their vision beyond the subsonic aeronautics of Lewis's era to the supersonic, hypersonic, and space frontiers.

8

Exploring Unknown Technology: The Case of Jet Propulsion

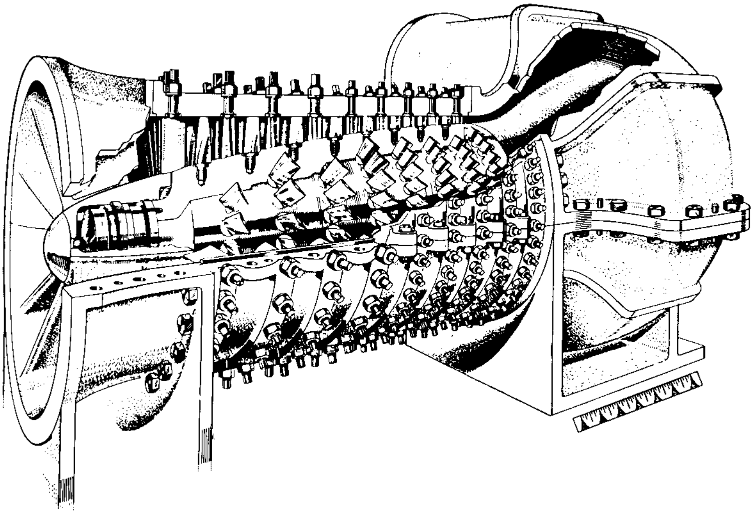
Langley's wartime mission was essentially not much different from its earlier peacetime mission: to find practical ways for American aircraft to achieve improved performance, i.e., higher speeds and altitudes, longer range, more maneuverability, and better handling characteristics. The pace of its quest had to be much more frenetic, of course. Though all aircraft used by the United States in combat were designed to the same basic formula (internally braced, all metal monoplane, equipped with retractable landing gear, wing flaps, controllable pitch propeller, and enclosed compartment for the crew), they differed widely and significantly in terms of their aerodynamic details. It was thus essential to refine aircraft on a case-by-case basis as problems arose.

Rarely did the army, the navy, or a manufacturer already know the design problem that needed fixing when it sent an aircraft to the NACA laboratory; in most cases, a prototype would be sent to the lab with instructions for the NACA to determine the aircraft's characteristics and to fix problems if the staff found any. In this way Langley researchers solved various problems in specific configurations. For instance, they recommended a modified tail arrangement and antispin device on the Vought F4U-1 and a new elevator for the Curtiss SOC-1.[1] Tests in the Full-Scale and 8-Foot High-Speed tunnels and in different tunnels at Ames lab in California led to the development of a simple but effective wing flap which, when deflected, increased lift just enough to make recovery from a high-speed dive possible (see next chapter).[2] Tests in the LMAL towing tanks and impact basin led to the development of a "hydroflap" to aid in ditching.[3] These are just a few examples of specific refinements to aircraft recommended by NACA Langley. In all, Langley tested 137 different airplane types between 1941 and 1945, representing more than half of all the types contracted for by

Data from tests of over 300 different models in Langley's free-spinning tunnels enabled the NACA by the end of the war to establish tail design requirements for satisfactory recovery from high-speed dives.

the army and navy during the war and including virtually all types that actually saw combat service.[4]

But the design of advanced-performance bombers and fighters involved more than mere aerodynamic refinement based on existing knowledge—among other things, it also required new understanding of high-speed phenomena. By 1939 flying speeds had increased to the point where the fastest aircraft were encountering a unique set of potentially dangerous aerodynamic phenomena known as *compressibility effects.* Previously, designers had created aircraft on the assumption that the air flowing over the wings and other surfaces was essentially incompressible, like water; though compressibility was always present in air, it was negligible until speeds approached sonic. Thus as aircraft evolved and their speeds increased, engine cowlings, canopies, fuselages, and especially wings and propellers were subject at high speed to a sharp rise in drag and loss of lift, with resulting changes in pitching moments. These compressibility effects limited the speed of aircraft and caused buffeting, control surface flutter, shifts in trim, and other dangerous changes in stability and control characteristics. In some cases, aircraft flying into the compressible regime became completely uncontrollable, could not recover, and crashed.[5]



Eight-stage axial-flow compressor designed by Eugene Wasielewski and Eastman Jacobs, 1938.

To achieve safe high-speed flight, the NACA began to consider some new technologies such as lighter-weight materials, stronger structures, and radically different types of engines for application in airplane design. Not all of these considerations led to immediate—or even to successful—application. (Langley's successful overall response to the "compressibility crisis" is analyzed in the next chapter.) In 1938, for example, Langley engineers Eastman Jacobs and Eugene Wasielewski (a power plants expert formerly employed by Allis-Chalmers) explored the potential of a technology—unproven in aeronautics—that they thought might help to solve some of the high-speed problems: they designed an axial-flow compressor, an unconventional piece of machinery that compressed an engine's intake air by sending it through a series of rotating and stationary (stator) blades which were concentric with the axis of rotation. The practical purpose of the axial-flow compressor was to be part of a piston-engine supercharger application (that is, part of a device for sending pressurized air into the engine cylinders to increase thrusting power). That the design posed some ultimate, mind-boggling problems for an airfoil researcher was Jacobs's personal reason for undertaking such a project. To work effectively, each one of the compressor's dozens of rotary and stationary blades had to be designed perfectly, according to airfoil theory, and put into a cascaded series that fed the flow output of one stage of blades into the input of the next stage.[6]

Though the initial round of tests demonstrated the high performance potential of the new compressor, Jacobs was left with "serious doubts about the axial design when the blades of the test machine were destroyed during a run in which the compressor stalled." Believing incorrectly that this accident was caused by some inherent structural weakness which would prevent success, Jacobs abandoned the project. Wasielewski and other members of the LMAL engine research staff continued to refine the design, however. Though the application to a piston-engine supercharger proved a failure, showing no real advantage over the contemporary General Electric–Moss supercharger, Langley's preliminary reports on the overall efficiency of its compressor seem later to have persuaded American manufacturers selecting compressors for jet engines to favor axial designs (wherein the direction of the airflow into and out of the compressor is *parallel* to the longitudinal axis of the engine) over centrifugal designs (wherein the direction of the airflow out of the compressor is *perpendicular* to the longitudinal axis).[7]

A much more notable failure to develop a radically new technology for high-speed aviation at Langley during the war was an attempt between 1941 and 1943 to create a hybrid system of jet propulsion. (Jet propulsion is a means of moving an aircraft forward by sending rearward at high velocity a stream of flowing gases, like sending a balloon across the room by letting go of its neck.) This system, like that developed by Italian engineer Secondo Campini in the 1930s and applied in the early 1940s to a Caproni airplane, was based on the principle of the *ducted fan.*[8]

Langley's version of the Campini ducted-fan propulsion system was supposed to complement conventional propulsion in the following way: Air was admitted through a frontal inlet into a duct, where the air slowed to practically stagnation pressure. A fan inside the duct, driven by the aircraft's conventional radial piston engine, then boosted the pressure and passed the air into a combustion chamber (located at the region of highest boosted pressure) where the exhaust gases (i.e., heat) from the piston engine vaporized gasoline for a primary fire. In turn this primary fire vaporized gasoline that was flowing over the main boiler section, igniting a secondary fire. Heated gases from this fire then escaped, accelerating through the constriction of a high-speed nozzle, where the thermodynamic energy accumulated during the various phases of compression and heating added to the driving thrust. This thrust, Langley engineers thought, could be harnessed for assisted takeoffs and emergency high-speed dashes by combat aircraft. (Today, such a system is called an afterburner.) If the jet was shut down, the aircraft could then revert to the conventional power plant, making it capable of long cruising flight.[9] Langley's ultimate goal was to apply the Campini system to either a military aircraft or to a small experimental aircraft designed specifically for the purpose of high-speed research. Although this goal was not achieved, the lab did prove the system feasible.

Until March 1943, when the work was officially canceled, Langley's testing of the Campini engine had the full support of a special NACA committee on jet propulsion and the polite tolerance of the military services. But during all of the time it worked on the Campini engine, the laboratory was unaware that another type of jet engine, the gas-turbine or turbojet,* was rapidly becoming a reality—and one that had greater feasibility and more potential than the ducted fan. The world's first jet aircraft, Germany's Heinkel 178, had flown for the first time on 27 August 1939; the experimental airplane was powered by a turbojet engine, designated the S-3, designed for Heinkel by Hans Von Ohain. The S-3 engine produced about 1100 pounds of thrust—which was just enough power to make the flight of the small airplane successful. But the NACA lab knew nothing about this top secret German development until 1944. Nor did the lab hear anything concrete about concurrent turbojet developments in Britain until the summer of 1943—even though American military leaders knew a lot about them much earlier. In April 1941 Air Corps chief Hap Arnold found to his astonishment during a tour of England that the British were not simply planning to develop gas turbines, but were actually preparing to flight-test a turbojet-powered aircraft, the Gloster E 28/39, which flew successfully the following month with an engine designed by Frank Whittle. Arnold made arrangements with the British to bring engine blueprints, and eventually a prototype of the Whittle WIX engine itself, to the United States. The

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* *Turbine* is derived from the Latin word *turbo*, meaning whirlwind. The earliest turbojets achieved their thrusting power by sucking air into the front of the engine, compressing it, and feeding it into a chamber (or chambers) where the compressed air was mixed with fuel and was ignited by spark plugs. The hot gases which resulted from this combustion were piped to a vane of airfoils, whose rotation ensured an aerodynamically smooth flow of gas at all engine speeds, and thence expelled into the buckets of the turbine wheel, whose whirlwind action increased the velocity of the already rushing exhaust. The gases were then forced out of the power plant at high velocity (full throttle, at over 2000 feet per second) through a nozzle or tailpipe. The turbojet was long considered impractical; engine experts felt that the necessary compressor and turbine equipment would be too heavy for an airplane and would generally burn too much fuel to be cost-effective. In the 1940s and 1950s, however, the jet proved to have several major advantages over the conventional reciprocating engine, including the elimination of the propeller with all its inherent limitations, the capability of burning almost any type of available fuel (but usually kerosene), and the power to drive aircraft to supersonic speeds. For an introduction to the technical details of the turbojet and other reaction engines, see C. N. Van Deventer, *An Introduction to General Aeronautics*, 3d ed. (American Technical Society, 1974), pp. 200–215.

British, who had just experienced the shocking forced evacuation of their troops from Dunkirk and felt that invasion of England by the Germans was imminent, shared their discovery with Arnold on the condition that he treat it as a top military secret. Upon his return, Arnold assigned the General Electric laboratory at West Lynn, Massachusetts, the task of imitating the prototype engine. Soon thereafter, in the early summer of 1941, the Army Air Corps placed a top secret order with Bell Aircraft Corporation for construction of an experimental jet aircraft. In September 1941, Maj. Donald Keirn carried, manacled to his wrist, a set of design drawings for the Whittle engine from London to G.E. at West Lynn. Thirteen months later the Bell XP-59A, powered by two Whittle I-A "Superchargers" (called that in disguise) developed by G.E., flew successfully at Muroc Dry Lake in California with full armament. Because of the tight lid of secrecy put by Arnold on all jet propulsion developments, Langley had not even an inkling of these important events until after an NACA representative to Bell's plant on the west coast heard rumors about them in May 1942.[10]

This lack of information put Langley engineers searching for a practical means of jet propulsion for aircraft at a serious disadvantage. What was clear to Arnold in the spring of 1941—that the potential of turbojet technology clearly outstripped that of the ducted fan—was not clear to them until the summer of 1943, when the military began to bring the NACA more into its confidence about secret jet propulsion programs, and when Eastman Jacobs, leader of the Campini project, returned from England with knowledge of British developments.

This chapter illustrates an episode in Langley history in which the engineers had every reason to think they were in charge of the technological situation, when in fact they were not. Though their combustion tests were successful in the sense of showing the general feasibility of the Campini system, their overall program was a failure. The failure was lack of knowledge—not about ducted fans, but about turbojets.

Conventional Wisdom

To trace the sources of Langley's interest in a ducted-fan jet propulsion system, it is first necessary to summarize the NACA's earlier analyses of the potential of jet propulsion for aircraft. In 1923 the Committee published a paper by Edgar Buckingham of the Bureau of Standards which declared that jet propulsion for aircraft was practically impossible. From his analysis of the thrust produced by an exhaust of burning compressed air in a combustion chamber, Buckingham determined that there was "no prospect whatsoever that jet propulsion ... will ever be of practical value, even for military purposes." Even at the highest flying speed anyone then had in view—250 miles per hour—a jet-propelled aircraft could not come close to matching the efficiency of an airplane equipped with a piston engine and propeller. The jet's fuel consumption would be far too excessive, he argued, largely because the weight of the compressor machinery would have to be so great. Buckingham calculated that the fuel consumption of a jet would be four times that of a conventional engine producing equivalent thrust. He assumed that aircraft turbines would have to be huge and heavy, similar to industrial turbines then being used in blast furnaces and boilers, to withstand the high temperatures and attendant high pressures. Buckingham's error was in this and other assumptions, not in his subsequent analysis.[11]

Throughout the 1920s and early 1930s Langley researchers accepted Buckingham's conclusions as their own; as a result, they did little to investigate the possibilities of a jet power plant for aircraft. The LMAL had one comparatively small research division devoted to engine research, but the outlook of its members was "slaved so strongly to the piston engine because of its low fuel consumption that serious attention to jet propulsion was ruled out."[12] What little research the lab did do on the subject was done by aerodynamicists, an interesting historical fact, given that the turbojet revolution happened elsewhere largely as a result of investigations made by aerodynamicists, not propulsion experts.[13]

In 1926 and 1927 Eastman Jacobs and James Shoemaker experimented with "thrust augmentors" for jet propulsion at Langley. The idea behind the program was to increase the mass of the airflow involved in the propulsion process by equipping a conventional gasoline engine with a special device that admitted additional external air and caused it to mix with a primary jet. Jacobs and Shoemaker thought that the momentum and energy relationships involved in this process would permit some augmentation of aircraft thrust. Although preliminary data from a specially built test apparatus indicated that it was possible to increase the thrust of a jet by using suitably designed high-speed nozzles, results warned that the maximum increase of thrust was too small, considering the dimensional and weight limitations of conventional aircraft, to achieve a worthwhile net benefit.[14]

During this same period, the NACA received a proposal from one Lt. Sidney P. Vaughn, an obscure supply corps officer stationed at the naval air station at Pearl Harbor, for research on a gas-turbine jet engine for aircraft. The Committee sent a copy of Vaughn's proposal to Langley; Carlton Kemper and William Joachim, two power plants engineers, reviewed it. Both of their evaluations were negative. Kemper concluded that the proposed jet was "impracticable" because "the use of a turbine installation with the present low efficiency, excessive weight and high speed would give an installation having a lower overall efficiency than the present internal combustion engine and propeller." Joachim's remarks were equally conservative. He listed as the chief factors preventing favorable comment:

> (1) Thermodynamically impossible to compress sufficient air by a fuel jet ... to provide proper combustion.
> (2) Practical impossibility of combustion air-fuel ratio control under all conditions of flight.
> (3) Practical impossibility of maintaining the weight of the power plant and discharge jet ducts to as low a weight as present engine with propeller.

Eastman Jacobs also read Vaughn's proposal, and though his evaluation was not entirely positive, neither was it closed-minded. He said that "the question of whether or not this system is of practical value cannot be answered without a consideration of the efficiency of the system." Because there were no experimental data available which were strictly applicable to the proposed system under consideration, Jacobs recommended, in March 1928, that "some simple tests" be made to furnish "definite information about the efficiency of such a device."[15]

Henry Reid agreed not with Jacobs but with the engine experts. Because no form of jet engine at the time seemed to offer any theoretical advantages over the conventional gasoline engine, "with the possible exception of [in an application for] very high-speed military airplanes," the LMAL engineer-in-charge advised NACA headquarters that the laboratory "should not undertake further investigations of jet propulsion at this time."[16] Headquarters concurred. In the early 1930s America's young airlines were trying to achieve reliable flight at 100 to 200 miles per hour. The NACA had more pressing problems than the development of aircraft for 500-MPH flight at 30,000 feet (where these aircraft would be required to fly to obtain this high speed). Headquarters authorized only three jet propulsion investigations in the early 1930s, all at the instigation of the Bureau of Standards, which carried out the work. Results sustained Buckingham's pessimism.[17]

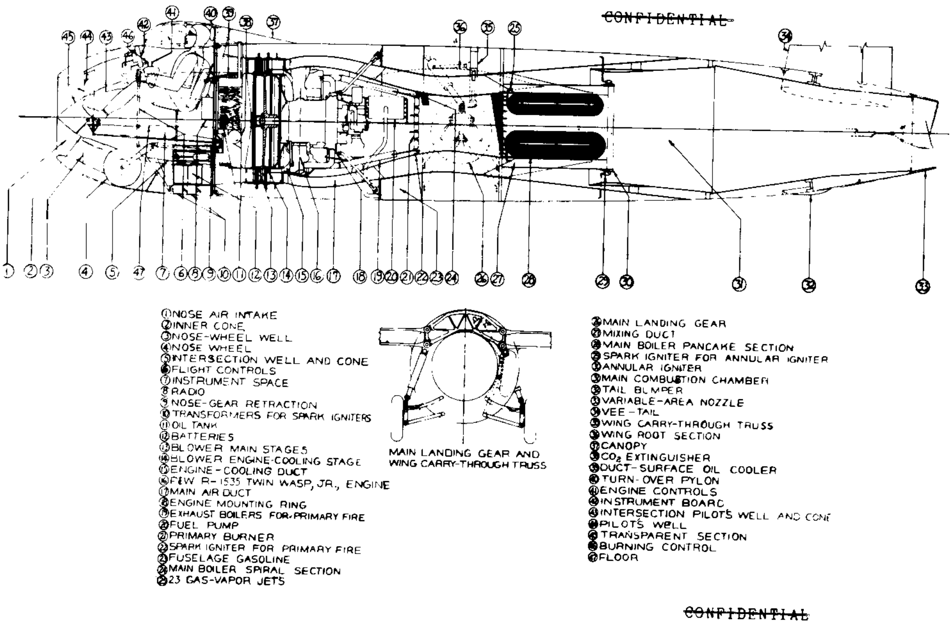
Reevaluating Buckingham's Conclusion

In January 1939, two years after Frank Whittle first bench-tested a jet engine, Eastman Jacobs wrote up a job order covering an analytical reevaluation of Buckingham's authoritative 1923 report on jet propulsion for aircraft. Albert E. Sherman, a member of Jacobs's airflow research

In the spring of 1939 Langley's Power Plants Division tested a Pratt and Whitney 1340 engine to determine the amount of thrust that could be obtained by projecting the waste gas rearwards through short exhaust stacks.

staff, was to rework the old problem in terms of the 500-MPH speed range then being approached by high-performance airplanes. The engineer-in-charge signed the job order immediately. Besides knowing that the navy had asked the NACA in July 1938 to determine how the exhaust system of a radial engine should be designed to obtain the maximum jet reaction from its waste gases, the engineer-in-charge had just heard from George Lewis that "the Army Air Corps [was] particularly interested in the development of some form of jet propulsion apparatus to be used for assisted take-off."[18]

On 11 April 1940, a conference was held in Henry Reid's office to discuss calculations reported by Sherman in his paper "Jet Propulsion for Aircraft at Subsonic Speeds"; present at the meeting were Reid, Elton Miller, Carlton Kemper, chief of the engine research staff, and Benjamin Pinkel, his assistant chief, plus Jacobs and Sherman. The six men agreed that jet propulsion now seemed to offer "the possibility of high enough power outputs with little machinery" to make a new experimental investigation desirable. They also agreed that, at the high air velocities through the combustion chamber, to burn gasoline satisfactorily constituted one of the most definite problems. The trailing flame, in particular, might be dangerous.[19] Since these men were not yet aware of the rapid progress of



Eastman Jacobs's drawing of experimental jet propulsion airplane, 1942.

the turbojet in England and Germany, they were considering jet propulsion in the absence of the turbine component.[20]

Jacobs and Sherman proposed studying a ducted-fan system that used only dynamic pressure (that is, the pressure was not boosted by a fan) for compression and the *Meredith cycle* for thrust. (In 1936 Frank W. Meredith had pointed out in England that not all of the waste heat of a piston engine had to be lost when transferred to the cooling airflow of a radiator. If the pressure at the exhaust of the radiator tubes was higher than the free static pressure of flight, some of the dissipated heat could produce a small thrust.)[21] Because this propulsion concept was a hybrid whose success depended only upon a creative modification, rather than a replacement, of the traditional aircraft power plant, it seemed a logical choice for the NACA to study. The results of Sherman's preliminary investigation indicated that an airplane having a ducted-fan system installed in conjunction with a good reciprocating engine would be capable of "truly high" power—power that could be used for short bursts of speed in combat, and for assisted takeoffs. When not using jet power, the airplane could revert to its piston-engine power plant, making it capable of great cruising range.[22]

Construction of a jet propulsion test bed called the "Jeep" got under way as soon as George Lewis gave authority. Lewis had been no great believer in the future of jet or rocket propulsion (like JATO, or jet-assisted takeoff, a system developed later that used auxiliary jet-producing units, usually rockets, for additional thrust); however, he had a tremendous faith in the talents and intelligence of his Langley staff. In 1940, for example, Lewis expressed his opinion to the laboratory that jet propulsion for assisted takeoff was inherently "inferior" to the catapult method. Even Benjamin Pinkel, head of the LMAL engine analysis section, tried to convince the director of research that jet propulsion offered "many advantages" over the catapult method. Jet-propelled aircraft could take off "simultaneously and in rapid succession" in contrast to the "inherent limited capacity and slowness" of the catapult, Pinkel advised Lewis; moreover, catapults required special apparatus easily put out of commission.[23]

Lewis chose not to press his point. He might not always agree with every person at the lab on technical matters, but he was certainly tolerant and imaginative enough to know that bright members of his field staff should not be discouraged from pursuing ideas in which they believed strongly, as long as those pursuits did not take too much attention away from work on the higher-priority items of the NACA, and as long as they did not cost much money. On 22 April 1940 Lewis sent a letter giving Langley his personal approval to start building the combustion test rig.[24]

There was no formal assignment of construction funds for the Jeep, so the Langley engineers had to build it mostly out of cheap sheet iron—which made it almost impossible to make the ducting system very efficient. To drive the rig's simple one-stage compressor, they scrounged a spare Pratt and Whitney aircraft engine (rating 450 horsepower) from the naval air station in Norfolk. The responsibility for designing the combustor fell to Carlton Kemper, who had been in charge of Langley's Power Plants Division since 1931. Kemper turned key aspects of this design over to Ben Pinkel.

With very few people inside the NACA or even at Langley knowing anything about this project, and with nothing yet known about the revolutionary progress of European turbojet development, none of these NACA engineers felt any real sense of urgency to expedite their jet propulsion research. While the necessary large-scale equipment was slowly being built in late 1940 and early 1941, Sherman conducted some small-scale experiments consisting of a series of qualitative observations of fuel burning under various windstream conditions. These preliminary experiments gave useful information only about the best methods to be tried later with the large-scale apparatus, if that apparatus was ever completed.[25]

Vannevar Bush (center) visited Langley on 21 October 1938—just months before becoming the NACA chairman. Henry Reid stands to Bush's right; George Lewis is to his left.

The Durand Special Committee

In early 1941 something dramatic happened to quicken the tempo and expand the purpose of Langley's work on the jet propulsion burner rig. On 25 February Vannevar Bush, chairman of the NACA, received a letter from General Arnold reporting that the Germans were making "considerable progress" in jet propulsion, "both as a means of assisting take-off and as a primary power plant." (This was two months before Arnold discovered that surprising British progress on the Whittle engine.) "In particular," Arnold's letter stated, "the Heinkel 280 is reputed to have been built and tested with experimental rocket motor installations for assisting take-off. Its predicted climb and ceiling will, if obtained, [make] obsolete existing fighter aircraft." Arnold's letter to Bush continued:

> For the last three years, the Air Corps has subsidized, through a contract with the National Academy of Sciences, a continuous, though limited, program of jet propulsion research which has been carried out at the California Institute of Technology. Due to limited facilities and personnel, no practical results are indicated for at least one or possibly two years.

In view of the new urgency for an early solution of the "rocket problem," Arnold advised Bush that "further investigation by a large group of able scientists is immediately needed." The most significant point about this letter according to historian Virginia Dawson (who discovered the letter in the National Archives in 1984) is that Arnold addressed it not to NACA Chairman Bush, but to National Defense Research Committee (NDRC) Chairman Bush (as Bush also then was). In paragraph three Arnold made it clear that the army wanted the NDRC, not the NACA, to take over the research on "the general questions dealing with jet propulsion and rocket motors." While the army "realized that any application of rockets as a means of assisted take-off for aircraft ... is properly a function for investigation by the National Advisory Committee for Aeronautics," Arnold felt that "the general questions dealing with jet propulsion and rocket motors, i.e., fuels, efficiencies, weights, basic materials, etc., could properly be investigated by the National Defense Research Committee." His argument was that "the basic problem of rocket propulsion ... has to do essentially with National Defense"; it was not "exclusively restricted to aircraft in its application."[26]

Dawson has also found Bush's response to this urgent request by the Air Corps chief, who on 10 March did not yet know about the Langley project. This document shows that Bush agreed with Arnold "entirely in regard to the importance of this subject and the need for immediate steps to evaluate." It shows that he recommended, however, that, since the problem of aircraft propulsion fell "outside the scope of the NDRC," the "best way to do this [was] by setting up a special committee in the NACA organization."[27]

A week later, after taking up the issue with Rear Adm. John H. Towers, chief of the Bureau of Aeronautics and a prominent member of the NACA, Bush expanded the scope of a recently constituted NACA subcommittee on auxiliary jet propulsion. Then on 24 March he established a Special Committee on Jet Propulsion. To head this committee, Bush called from retirement Prof. William F. Durand, a member of the NACA since its beginning and the independent dean of American engineers.[28]

For Durand, to be 82 years old at the time of his appointment as chairman of the Special Committee on Jet Propulsion was only a matter of counting birthdays. After retiring from teaching at Stanford University in 1924 at the statutory retirement age of 65, Durand had stayed extraordinarily active. Besides continuing as a vigorous member of the NACA, he assumed editorial charge of the Daniel Guggenheim Fund's series of monographs by recognized authorities on aerodynamic theory. He wrote parts of three volumes himself, and translated all of the articles written in French, German, and Italian. This work, which would have done credit to a man less than half his age, kept him fully abreast of current foreign and NACA research.[29]

Prof. William F. Durand, ca. 1955. Durand died in August 1958, at the age of 99.

Until Bush informed him about it by a letter dated 18 March 1941 (six days before the announcement of the formation of the special committee), Durand knew absolutely nothing, however, about new jet propulsion research at Langley. Sherman's March 1940 report on "Jet Propulsion for Aircraft at Subsonic Speeds"—the only paper yet written dealing with the lab's critical reevaluation of Buckingham's conventional wisdom—had not been published in any form because Eastman Jacobs had believed the report's findings too preliminary.[30] And all Durand learned about the Langley Jeep and the project in general before assuming the chairmanship of the new special committee on jet propulsion was that it had "attracted the favorable attention of Dr. Bush." In his 18 March letter to Durand (also found by Dawson), Bush reported that "the group at Langley Field and Jacobs, in particular, have been very active in developing one jet propulsion scheme in which I have acquired a large amount of interest and perhaps even enthusiasm, for it seems to have great possibilities and I cannot find any flaw in their arguments."[31]

In order to judge firsthand what role this Langley project might play in America's belated effort to develop a jet-propelled aircraft, Durand visited Langley. He made the visit in late March 1941, even before the inaugural meeting of his special committee. Escorted by engineer-in-charge Reid and director of research Lewis, the professor inspected the unfinished burner rig and talked at length with Jacobs and Sherman. Impressed with the potential of what he saw and heard, Durand asked Reid to have his staff prepare for him a memo with drawings illustrating how the test stand could become more nearly a mock-up of a proposed airplane.

At the first meeting of the special committee in April, Durand used this material to describe the Langley project. He told the panel that he felt a ducted-fan system could be incorporated into a military aircraft in a relatively short time and could give it a performance in some ways superior to that of any existing propeller aircraft. The committee reacted to its chairman's message by recommending, in its first resolution, that the NACA laboratory turn the rig into a nonflying model of a jet propulsion system.[32] Thereafter, Langley would conduct its project in strict accordance with instructions given by Durand.

At this time, because of the changed and expanded scope of the research, Langley began to reexamine the character of the whole project. Jacobs, in particular, argued that various parts of the test rig had to be rebuilt and new machinery added into the system if results were to be applied to the design of an airplane that really could be flown. After all, the original purpose of the equipment had been only to demonstrate, first, how well the burning of fuel could be controlled in a high-speed propulsive duct and, second, whether the addition of heat to the burning fuel mixture actually produced the large increases in thrust predicted by Sherman. Now, without even having finished the equipment to make these preliminary investigations, his staff was being asked to transform a raw test apparatus into a geometrically accurate mock-up of an actual aircraft system.[33]

In the summer of 1941 the Durand committee reacted to military intelligence reports of British and German turbojet developments by recommending that the U.S. military services let contracts to Allis-Chalmers and Westinghouse for the development of contrasting turbojets and to General Electric of Schenectady, New York, for a turbo-propeller. The army apparently did not inform the NACA, however, that a Whittle engine had been sent to the G.E. lab in West Lynn, Massachusetts, for development of a prototype; nor did the army tell the NACA when it placed an order with Bell for the XP-59A. The NACA first heard about this aircraft in May 1942—six months after the army let the contract—when one of its representatives reported hearing rumors of a "Buck Rogers" project at Bell.[34]

Ignorant of the jet propulsion projects which the Air Corps was just starting up, the Langley staff worked through 1941 to complete the Jeep. In this effort the staff had the support of the Durand committee. By February 1942 the Jeep was operational, if not always successful. The machine was awesome. It consumed three gallons of gasoline per second and exhausted a gigantic blowtorch that "impressed, and often terrified" spectators and NACA employees alike.[35] In fact, many of the machine's early test runs failed simply because the man (usually Jacobs himself) handling the introduction of the main flow of gasoline into the combustion chamber, who was understandably nervous about his responsibility, either jerked the fuel valves open or backed them off too suddenly.[36]

By the end of the spring, however, most of the Langley test crew had overcome their fears about working with such volatile equipment. By vaporizing the fuel before mixing it with the combustion air, Jacobs had tried to restrict the main fire to an "intense, small, and short annular blue flame burning steadily in the intended combustion space." His idea had been that the blue flame indicated, and was thus necessary for, high combustion efficiency. It is uncertain whether this technique ever succeeded totally. By ensuring proper conditions, however, Jacobs did demonstrate that "a blanket of cool air" could be maintained between the hot gases and the walls of the test stand. Besides reassuring the men involved that their lives were not in danger, this knowledge meant that an airplane having the propulsion system would probably not burn to pieces or explode from its own energy.[37]

Eventually the Jeep did demonstrate efficient combustion as the result of a liquid injection system designed by K. K. "Nick" Nahigyan of the LMAL engine analysis section. By late July 1942, the basic features of the system appeared promising enough in the ground mock-up to merit instructions from the Durand committee for a design study of an actual flight article, a research airplane incorporating the Campini system.[38]

By the time it received those instructions from Washington, the Jacobs staff had in fact already been investigating the aerodynamic and thermodynamic characteristics necessary for such an airplane for at least three months. In the course of this study, Jacobs and Sherman had parted company in strong personal disagreement over how the ducted fan should be applied. In March 1940, Sherman had argued that "the application of jet propulsion to the cooling ducts of fast military or racing airplanes for auxiliary emergency purposes appears interesting enough to warrant immediate experimental investigation." This argument for what later became known as an afterburner had appeared in the draft of the report which Sherman now said Jacobs had "suppressed."[39]

According to Sherman's plan for the Martin B-26, Langley was to fair out and extend the nacelle afterbody, seal all the air intakes, and arrange for the exhaust to be discharged from the modified nacelle at the tail opening. Then the lab was to fit exhaust boilers to the engine and install jet burners in the forward part of the nacelle afterbody, which would be provided with a light inner heat-resistant duct extending from the burners to the tail opening.

In early 1942 Sherman had still believed that "the actual flight application of jet propulsion to the cooling ducts of some of our existing fighter ships" was desirable, "if only for morale and research purposes." The application "could be done in only a few months," he emphasized, "as is indicated by the information that I have already acquired experimentally." Specifically, Sherman had recommended that a twin-engine, high-speed medium bomber—a Martin B-26 Marauder in Langley's test flight fleet—be modified to include the auxiliary jet propulsion system, which the Langley Jeep had proved to be "attractive in all respects." With the afterburner, he had predicted that the top speed of the B-26 could be raised in an emergency from 350 to 400 miles per hour, "with the fuel consumption increased by the order of only 300 percent." The jet's average temperature would be approximately 1700 degrees Fahrenheit, but since the unburned portions of the air would be directed along the walls for cooling, there was no reason to fear overheating.[40]

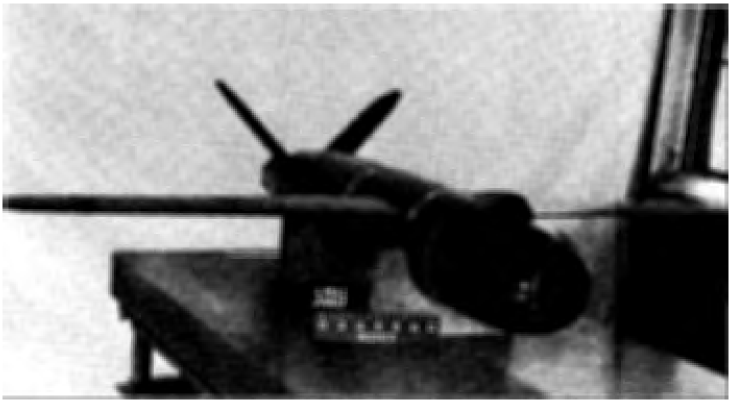
Because he knew that Jacobs had a much different application in mind, Sherman had executed an end run around his boss and sent copies of his proposal directly to engineer-in-charge Reid and chairman Durand. At the same time he had requested a transfer from airflow research to another division where he could spend more time on his own developing a 12-inch portable combustion tunnel for testing his afterburner idea.[41]

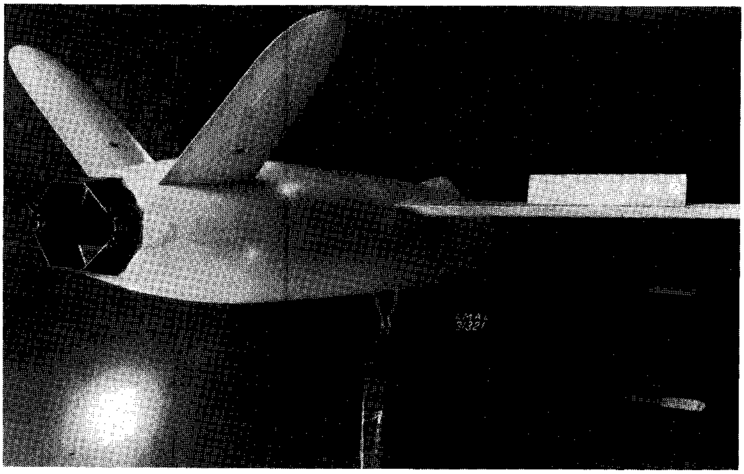
Jacobs's Concept for a Research Airplane

The jet propulsion application Jacobs had in mind showed more boldness and less concern for precedent than even Sherman's idea: an out-and-out NACA research airplane that not only could test the Jeep concept for propulsive capacity but that also could explore the frontier of high-speed, perhaps even transonic, flight. Though it was NACA policy to stay clear of designing aircraft, the idea of a special high-speed experimental plane had been gaining steam at Langley since 1941. At one of the first meetings of the Durand committee on 22 April 1941, Jacobs had said that with the NACA's new family of laminar-flow airfoils it would be possible to attain approximately the speed of sound in flight.[42] And only in flight tests could knowledge of high speeds be gained, for wind tunnel tests in those days provided little if any reliable data at Mach numbers between 0.7 and 1.3 (that is, between speeds 30 percent below and 30 percent above the speed of sound). Later in the same year John Stack, who had worked for Jacobs in the VDT section for most of the 1930s and was soon to become head of Langley's new Compressibility Research Division, suggested to George Lewis a concept for a transonic research airplane (see next chapter).

With Sherman going his separate way toward a specific military configuration, Jacobs put together an experimental jet airplane design team consisting of young engineers Macon C. Ellis, Jr., and Clinton E. Brown. This team quickly decided that it was advisable, for experimental purposes, to keep the airplane small. On the other hand, to obtain conclusive results, the team realized that the plane had to be large enough to carry a pilot and instruments, and of sufficient dimensions and power so that these items would not exert a marked adverse effect on its flight test performance. Most importantly, the designers concluded that in view of the problems connected with the development of radically new airplane types, it was unwise "to complicate and retard the fundamental development by numerous considerations of adaptability to military requirements" or to hamper a project intended primarily to develop the possibilities of an experimental system by "unnecessarily making components, such as a gas turbine-prime mover, which themselves must be treated as experimental," part of the NACA program.[43]

By July 1942, the team of Jacobs, Ellis, and Brown had finished preliminary plans for a specific configuration of the experimental jet-propelled airplane. The design featured a modified Pratt and Whitney R-1535 radial engine, an advanced type of nose inlet and high-speed cowling, a cylindrical fuselage, a high-shoulder wing (derived from an NACA low-drag airfoil section), a V-tail, and a cockpit for one pilot, as well as a version of the





V-tail model of Langley's proposed jet propulsion airplane, December 1942. In the bottom photograph, the model is mounted in the 7 × 10-Foot Tunnel.

Jeep. Because of uncertain values of drag caused by compressibility effects at speeds over 550 miles per hour, the designers could not pinpoint a prediction of the maximum speed of their creation. They did believe, however, that because the same engine in ground tests produced three times the thrust thought necessary to reach Mach 0.75, their small airplane could reach 600 miles per hour. This meant that it "could have really barreled into the transonic region."[44]

For a late July 1942 meeting of the Durand committee at NACA headquarters in Washington, Jacobs brought with him a short reel of film made during a recent test of the Jeep. This film demonstrated achievement of efficient combustion in the Jeep through liquid injection of the fuel. After showing the film, he recommended that arrangements be made with the military services, either jointly or separately, for the immediate construction of his research airplane. Jacobs called such a step "the quickest and most direct approach" to the application of jet propulsion to military aircraft and asked for permission to test the stability and control characteristics of a scale model in the LMAL 7 × 10-Foot Tunnel.

It was the decision of the Durand committee, however, that Langley extend its jet propulsion work along both Jacobs's and Sherman's lines of research in definite accordance with military objectives made necessary by the war:

> First, design studies should be made of applications representing actual military airplanes meeting certain stated minimum requirements [Sherman's way]. Second, the detailed design of the experimental airplane should be continued [Jacobs's way] in order to investigate more thoroughly any possibility of using the experimental airplane itself for military operation.

Durand asked that Langley send him a short progress report about this parallel work each week. Proceeding further with definite plans for procurement would wait until the results of both design application studies were compared.[45]

In late September 1942 Durand notified George Lewis that "there is nothing in particular that we, as a committee, can do with regard to the [jet propulsion] projects in the hands of the industrial companies," but that the committee could help the project at Langley. In this letter Durand admitted feeling "a little anxious about Jacobs's work, due to the fact that the Committee is directly interested in that particular project in the sense that its success or failure will react directly on the reputation of the Committee—at least in conjunction with this particular work." Durand told Lewis that he would be "very grateful ... if you will feel entirely free to represent me in connection with this work and guide Jacobs and his collaborators as may seem best to you."[46]

The Campini Project Dies

A few weeks later the Durand committee visited Langley for the express purpose of witnessing a demonstration of the Jeep. Stationed safely some 200 yards behind the monstrous burner, the visiting dignitaries watched Jacobs's experienced crew ignite the engine. Stable combustion was still not automatic inside the apparatus, however, and the test run fizzled. The failed test was a great disappointment to everyone, especially to those aware that just two weeks earlier at Muroc, Bell test pilot Robert Stanley had flown the XP-59A with Whittle engine successfully. The Durand committee returned to Washington gravely concerned over the delays caused by the fickle apparatus and over the loss of innumerable gallons of precious gasoline. (According to Jacobs's staff, much gas had been stolen from the Jeep for use in private automobiles as a result of wartime rationing.)[47]

Though some members of the research team were growing increasingly dubious about the Campini system, Jacobs continued to have his staff work hard on the Jeep into the first months of 1943.[48] Full-scale burning tests resumed. Albert Sherman analyzed the potential of an afterburner in a Bell P-39 airplane.[49] Systematic tests of the stability and control characteristics of Jacobs's aerodynamic configuration were made in the 7 × 10-Foot Tunnel, followed by a round of drag tests on the fuselage, tail surfaces, and central wing portions of the scale model at higher Reynolds numbers in the Two-Dimensional Low-Turbulence Pressure Tunnel.[50] Though formal work on Jacobs's engine by the engine analysis section ended in December 1942—when the entire LMAL Power Plants Division moved to the new Aircraft Engine Research Laboratory (AERL)—NACA correspondence indicates that Nick Nahigyan in Cleveland continued making tests related to the liquid injection system for Jacobs's jet airplane into early 1943.[51]

Virginia Dawson thinks that Nahigyan's continuation of work on the Jacobs engine might have been kept a secret from everyone at the AERL except Edward R. "Ray" Sharp, the field manager. Not even Ben Pinkel, Nahigyan's boss, seems to have known about it. Pinkel recalled in a 1984 letter to Dawson that his engine analysis section completed all work on the Jacobs combustor before the division's move to Ohio in December 1942. According to him, all of the section heads of the LMAL Power Plants Division had been called into Reid's office just before their transfer. There George Lewis informed them that the "top military echelon" had instructed the NACA "that the war would be fought with five reciprocating engines, namely, the Wright 1820 and 3350, the Pratt and Whitney 1830 and 4460, and the Allison V 1710, and that all work on jet propulsion [could] be stopped in order that all effort [could] be directed toward those reciprocating engines." In Pinkel's mind, then, there was "no useful purpose for further work on Jacobs' engine following our successful demonstration of principle" to the Durand committee in July 1942.[52]

The proscription of further jet propulsion work may have applied to the AERL but not to Jacobs and his staff at Langley, whose overall airplane design was unknown to those power plant engineers brought in periodically to assist on specific engine components. Records show how strongly committed to the Campini system Jacobs and the NACA as a whole remained even after the transfer of the Power Plants Division to AERL. During a visit to Wright Field in January 1943, Jacobs advised Air Corps officials that the Durand committee still supported the experimental airplane idea, as did NACA chairman Hunsaker and director of research Lewis. Jacobs argued that the best way of obtaining the flight article, the research airplane, would be "to have an airplane company appointed to work on the design of the airplane with [the NACA] and then take over the construction." Upon returning to Langley, Jacobs reported that "Wright Field was convinced that flight tests should be made, but was apparently not certain as to how the work should be prosecuted." He believed that "Wright Field would be inclined to build a complete military version rather than building with the least expenditure of effort and time a purely experimental flight article."[53]

The Langley jet propulsion project died in March 1943 when the military services turned down the NACA's request to construct Jacobs's research airplane:

> The Army Air Forces and the Bureau of Aeronautics have had the occasion to study the characteristics of several [jet propulsion] schemes and combinations now under consideration. As one result of these studies, it appears inadvisable at this time to build an airplane of the type recommended [by the NACA]. This conclusion is based primarily on weight and fuel economy when compared with more highly developed types.

Certain features of the NACA Jeep interested the services enough for their representatives to recommend jointly that "further investigations be made in order to explore the possibilities of increasing the ratio of thrust to weight." If the NACA could show this ratio to be comparable to those of other types of jet engines, or if the ducted-fan scheme could be modified to show "compensatory advantages," there would then be "ample justification for reconsidering the proposed design application." And since wind tunnel data in the transonic range were not available, and conventionally powered test aircraft were too slow to perform the needed research in actual flight, the services advised the Durand committee, finally, that "a jet-propelled airplane now under construction" should eventually be made available to the NACA. The airplane being referred to was most likely the XP-80, the first U.S. airplane conceived from the beginning for turbojet propulsion, the design of which had just gotten under way for the Air Corps at Lockheed's plant in Burbank, California.[54]

Langley received word from NACA headquarters on 23 March 1943 to hold the jet propulsion project in check pending further action. A few weeks later, the lab also learned that neither service was interested in Sherman's speed booster application. In May Sherman resigned "to go into another business." Jacobs's immediate reaction was to write a memo in which he charged bluntly that the army and navy were making a big mistake. In concluding that the NACA's proposed experimental airplane was deficient in thrust relative to its weight, the services were comparing "uncertain development projects with a conservative straightforward engineering design which has been partly tested and could be readily constructed by straightforward means and for which we have every reason to expect large gains in performance." The services should do the developmental work only "after the experimental airplane has served its purpose and while the system is being developed for application to a military airplane." In response to a suggestion by Durand that a lighter burner unit might satisfy the military requirement, Jacobs stated that "any gain here would be relatively small." The power-to-weight ratio could not be increased markedly by decreasing the weight of the structure. That feat could only be accomplished by "going over the entire airplane and ruthlessly sacrificing other things." Jacobs doubted whether such alterations in the end would even appear desirable. At any rate, his experimental airplane already showed "ample thrust for its weight." There was no good reason to make the design "less practical or conservative" in order to gain a better ratio of thrust to weight.[55]

Jacobs's reaction, especially the phrase "uncertain development projects," implies that he and his colleagues at Langley were still unaware of how far along G.E. and others had come in achieving a successful gas-turbine form of jet propulsion for aircraft. They apparently did not know that the U.S. military was well on the way to having its first jet fighters, with the Bell XP-59 having flown in 1942 with a full armament system and the Lockheed XP-80 under development. The only explanation for Langley's lack of knowledge about these things in the spring of 1943 is that General Arnold and others had been keeping the NACA in the dark. Historian Alex Roland believes that this was in fact the case:

> Part of the story was simply that the services put an unprecedented lid of secrecy on all jet-propulsion development. Not only did this policy shut out the NACA more completely than ever before from developments in military aviation, but it also prevented manufacturers from freely exchanging information on their projects. In fact the two sections of the General Electric Company working on ... the separate jet projects did not know that the other team existed.[56]

Drag cleanup of the Bell P-59, America's first jet airplane, in the Full-Scale Tunnel, May 1944.

In June 1943 NACA chairman Jerome Hunsaker complained privately:

> The idea that they [the British] are supplying "us" everything they have [about turbojet development] does not apply to NACA but may apply to the services.[57]

Roland explains these facts by suggesting that Arnold had lost faith in the NACA. A less controversial explanation is that Arnold and other military leaders felt that it was more dangerous to confide top secret information to the civilian research agency, and thus increase the number of potential leaks, than it was to keep the NACA research staffs ignorant of military programs to develop jet aircraft. This policy helps to explain the army's polite tolerance of the LMAL Campini project in 1941 and 1942.

Other documents dug out by Dawson suggest that Durand knew about the secret military developments just about as they happened, but could not share his timely information with the NACA staff. On 27 February 1942 the chairman of the Special Committee on Jet Propulsion had asked Maj. Gen. Oliver P. Echols, USAAC, for permission to circulate test results among G.E., Westinghouse, and Allis-Chalmers, the three companies secretly and independently involved in jet engine development. "Of course," Durand had added, "this request has no relation whatever *to the particular project sponsored by the Army, now being carried on by the General Electric organization, and of English origin* [emphasis added]. It relates solely to the projects which have been developed as a result of meetings of the Jet Propulsion Committee."[58]

When the Bell XP-59A, powered by two G.E. I-A Whittle turbojet engines, made its first successful flight test at Muroc Dry Lake, California, on 1 October 1942, Durand had been there to see it. He wrote four weeks later to Colonel Keirn, the army officer at Wright Field who had brought the plans of the Whittle engine from England a year earlier:

> I was very sorry that you could not be with us in California. The performance was indeed very interesting, and I am very much indebted to you for your kindness in facilitating my visit to Muroc .... It really begins to look as though a definite start has been made along the lines we have been thinking about so long.[59]

Jacobs and a few other privileged members of the Langley staff heard about what Durand saw only in the summer of 1943 when the military changed the Whittle project from secret to confidential.

Since the policy of secrecy regarding jet propulsion research and development in America contrasts sharply with the policy of a relatively free flow of information within the British aeronautics community,* the wisdom of Arnold's keeping the NACA ignorant of U.S. military programs can be debated. What seems beyond debate, however, is that researchers at Langley laboratory, fully supported and regularly monitored by the Durand committee but with far too little knowledge of the overall turbojet revolution and its security problems, worked through 1941, 1942, and the first three months of 1943 on a questionable system of propulsion that seemed to cancel out the advantages of both the pure propeller and the pure jet.

This fact raises a provocative but unanswerable set of "what if" questions: What if the research staff at Langley had known more about the most recent developments in turbojet technology? Would they still have worked so hard on a jet propulsion system based on the principle of the ducted fan? Would they have made major changes in the system? Would they have abandoned the concept behind the system altogether? Would they still have proposed the construction of a small experimental airplane, or would they have embraced the idea of a complete military version? If the army had shared its intelligence with the NACA through proper and

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* In Britain, the Gas Turbine Collaboration Committee made sure that British agencies pooled their resources and avoided unnecessary or duplicated efforts. See H. Roxbee Cox, "British Aircraft Gas Turbines," Ninth Wright Bros. Lecture, *Journal of the Aeronautical Sciences* 13 (Feb. 1946): 53-83.

secure channels, would the NACA have leaked the information or simply have improved it?

There are good reasons to think that Jacobs and most other NACA engineers would have turned away from work on the ducted fan much earlier if they had known more about the progress of turbojets. Jacobs today recalls thinking about the potential of gas turbines in the late 1930s, during the period when he was working with Wasielewski on the axial-flow compressor. He and his colleagues had even thought about using a gas turbine to drive the compressor of the Campini system.* But they decided against this on the technical judgments (erroneous as they turned out, but not farfetched at the time) that the material and fluid-mechanical problems of the turbine were too intractable and that the extra machinery would simply weigh too much for practical application to an aircraft.

In the summer of 1943 Jacobs journeyed to Great Britain, where he spent two weeks in London, one week at the Royal Aircraft Establishment in Farnborough, and a day at Cambridge. At one of these places, he saw aerial photographs of German aeronautical centers which showed scorch marks on the ground thought to be the tracks of jet exhaust. Jacobs also found out a good deal more than what he already knew about the Whittle engine and its applications.[60]

With this new insight into the state of foreign turbojet technology came a new argument from Jacobs in protest of the military's decision to kill his experimental airplane idea: in subsequent written and oral reports to the NACA about his visits, Jacobs observed that the British were making the same mistake as that already made by the U.S. military in turning down the NACA research airplane proposal; that is, "the mistake of applying the new power plants to more or less conventional airplanes rather than giving careful consideration to essentially new extreme-performance types" made possible through the use of new power plants. He concluded that both in England and the United States "the development of the jet power units themselves had progressed beyond the development of suitable airplanes to employ them." If a system as new as the turbine engine were incorporated without accompanying changes in the principles of the airplane's aerodynamic design, there would be such an imbalance between power plant and airframe, Jacobs feared, as to make both propulsion and aerodynamics ineffective. What was needed, Jacobs emphasized, was a unified cooperative program among the military services, aircraft

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* In principle, the only difference between the ducted fan and the turbojet—aside from the dividing-up of the fluid stream in the former (which is not really essential and would be introduced into later turbojets anyway)—was that one drove the compressor with an internal-combustion engine and the other drove it with a turbine.

manufacturers, and the NACA, "with a view toward producing quickly extreme-performance airplanes of several types to be developed around existing units and suitable to exploit to the full the capabilities of those existing jet-propulsion power plants."[61]

In using this last phrase, Jacobs was almost certainly thinking of turbojets of the kind he had seen in England. As a result of his growing awareness of the advanced state of the new engine technology, Jacobs now shifted his attention away from the power plant problem, which had preoccupied him on the Campini project, to the problem of fully utilizing the overall potential of jet aircraft. This was the natural thing for an aerodynamicist to do. But not once in 1943 did Jacobs confide in his airflow research staff anything about his knowledge of British turbojets; for an NACA researcher accustomed to the free exchange of information at least among his own staff, this was most unnatural.

He never did acknowledge that his ducted-fan research airplane concept was unworthy of additional consideration. In a personal letter to George Lewis written in England, Jacobs related that the people at the Royal Aircraft Establishment

> profess to agree with me that the Army and Navy are short-sighted in not backing our project to have constructed the N.A.C.A. jet propulsion airplane. I really think that they believe that we should go ahead with the experimental airplane as the best and perhaps the only means of obtaining reliable data and experience in the high Mach number range.

Lewis agreed with Jacobs, telling his engineer in July 1943, "I have always felt that if a jet-propulsion device was to be considered at this time for a single-engine airplane, and if range was an important factor [which it was not], your particular scheme offered the best opportunity of answering the requirements."[62]

Turbojet Revolution Upheld

This chapter is meant to enrich historical understanding of the NACA's failure to discover jet propulsion, not to explain it away. Nothing can do that. One of the main points of Edward Constant's book *The Origins of the Turbojet Revolution* is that the turbojet revolution began with the vague feeling of a few farsighted European *aerodynamicists* that something anomalous was about to block the further progress of aviation to higher and higher speeds and that only a radically new type of power plant such as the turbojet could resolve the anomaly and ensure progress. Since *aerodynamics*

America's first generation jet planes at Langley. Clockwise, from top left: Lockheed P-80 Shooting Star, November 1946, the first fully operational U.S. jet fighter; Vought F7U Cutlass, December 1948, the navy's tailless twin-jet fighter; North American B-45 bomber, November 1949, powered by four jet engines; and Republic F-84 Thunderjet, October 1949.

was the NACA's forte, failure here to presume the anomaly before it became an actual problem appears all the more glaring.

The NACA soon made up for the failure, however, by having its three labs shift focus from the old to the new technology and come with skills and alacrity to the aid of American turbojet development. Subsequent history proves that it was this type of flexibility, not the more radical type that first led certain Europeans to explore turbojets seriously, in which the NACA research staffs excelled.

NACA senior staffers in Ohio, California, Virginia, and Washington, D.C., received their first briefings on the General Electric and Bell turbojet projects in late 1943. At Cleveland, Colonel Keirn swore AERL team leaders to secrecy and then showed them a special test cell designed at West Lynn. The army delivered a G.E. I-A turbojet to the AERL in early 1944 under heavily armed guard. Tests were then made confidentially in the Altitude Wind Tunnel, which was built for the study of liquid-cooled engines under altitude conditions, to check G.E.'s refinements of the Whittle I-14, I-16, and I-40 engines. Members of the AERL staff also collaborated with manufacturers in the design of combustion chambers for proposed new engines.

The postwar turbojet revolution affected the Cleveland laboratory more completely and directly than it affected either of the other two NACA labs.[63] When the war ended in August 1945, the AERL underwent a sweeping reorganization known by insiders as "The Big Switch." As John Sloop, who headed the group at this lab working on ignition problems during the war, described it: "Overnight, research emphasis shifted from piston engines to jet engines (turbojet and ramjet) with some work on rockets." In the process, the name of the institution was changed to the Flight Propulsion Laboratory (and in 1948 to the *Lewis* Flight Propulsion Laboratory), the entire work force was reassigned into four new divisions, and top administrators lost or gave up their posts. The Big Switch caught everyone by surprise, especially the lower-level supervisors. Sloop recalled going home one night "deeply engaged in writing a report on spark plug fouling to find in the morning that [his] desk was in another building and [he] was now officially engaged in rocket engine cooling research."[64]

Though the AERL was most directly affected, Ames and Langley had also become actively involved in developing the technology of jet aircraft starting in 1944. As one would expect, the contributions of these two laboratories mainly involved aerodynamic analysis of turbine engines, compressors, and complete jet aircraft configurations. In May 1944, for example, Langley put the Bell P-59 through drag cleanup in the FST.[65] Swept wings, with new flap systems and other high-lift devices, and narrow streamlined compressor units that could be buried cleanly into the wing or fuselage were some of the NACA aerodynamic developments of the mid- and late 1940s that proved crucial to the ultimate success of the turbojet revolution. With conventional configuration the speed of aircraft would have jumped from the 350- to 450-MPH to the 450- to 550-MPH range because of jet propulsion, but beyond this plateau jet aircraft could not have gone without the breakthroughs in transonic aerodynamics.

It was to this purpose—solving the transonic problem—that NACA Langley had been directing considerable research energy since the late 1930s.

9

The Transonic Problem

Throughout World War II, Langley and the other NACA laboratories faced a perplexing research policy problem: the need to balance refinement of conventional aircraft technology with a strong enough dose of basic research into the high-speed frontier of future flight. In 1941, American combat planes encountered the dangers of compressibility for the first time. When diving vertically to terminal velocity, they penetrated far into the transonic region, where the effects of such aerodynamic phenomena as shock waves on moving bodies were largely unknown.* Lockheed test pilot Ralph Virden's fatal loss of control in November of that year during a dive test of the P-38 Lightning, an extremely high-powered and aerodynamically clean fighter plane, dramatized the need for a more complete understanding of the essential characteristics of transonic flight.[1] The military necessity of solving the many difficult problems associated with the development of jet engines and guided missiles and rockets underscored this need. This combination of factors—the waging of world war and the emergence of compressibility and other problems of high-speed aerodynamics—challenged

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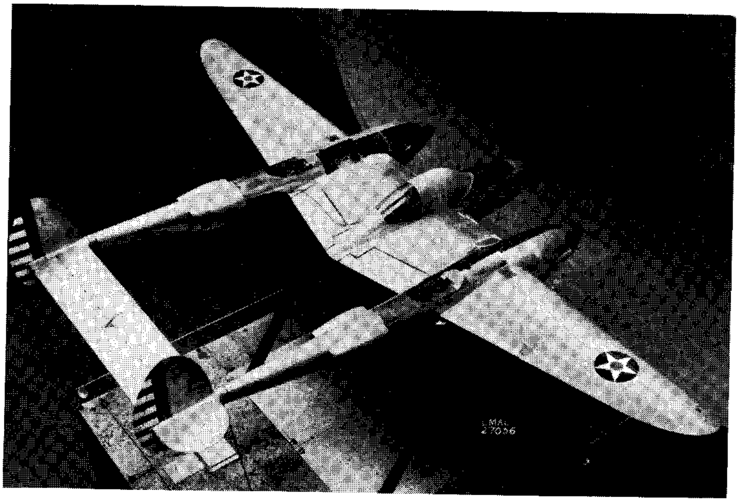
* The transonic range begins for a particular aircraft when the flow over any part of the aircraft's wing exceeds the speed of sound, or Mach 1. At the point when this speed is reached, at what is known as the *critical Mach number*, there are no adverse aerodynamic forces. But as the critical Mach number is exceeded, a shock wave, or major pressure disturbance, forms on the top of the wing (at 90 degrees to the airstream) and propagates through the surrounding air at sonic speed. Because it forces the wing to encounter a mixture of subsonic and supersonic conditions, this shock presents serious problems for the aircraft (see further in text).

Another term that will appear in this chapter is *supercritical*. This refers simply, as it did in the 1940s, to any speed beyond the critical Mach number. In the 1960s, Richard T. Whitcomb of NASA Langley introduced a more restrictive meaning of the term with his invention of a "supercritical" airfoil to delay the drag rise that accompanies transonic airflows. Thus *supercritical* has also come to mean airfoil operations in the speed region between the critical Mach number and drag-rise Mach number. For an introduction to the principles of high-speed flight, see Van Deventer, *An Introduction to General Aeronautics*, pp. 108–128.



Test of Lockheed P-38 Lightning in Full-Scale Tunnel, December 1944.

Langley not only to create a whole new wind tunnel and flight research capability but also to move away as quickly as possible from the first duty of its wartime assignment, which was essentially to help industry catch up with and consolidate the many diverse gains of pre-1939 NACA research.

In spite of their parent organization's heavy commitment to cleaning up specific military configurations, many of Langley's more experienced and restless aeronautical researchers somehow managed to find the time and resources during the war to respond to this challenge. In the summer of 1942, for example, at the same time that Eastman Jacobs was attempting to develop the ducted-fan jet propulsion system (see the previous chapter), he and Arthur Kantrowitz were also busy designing and constructing the NACA's first supersonic wind tunnel. This tunnel, which had a small 9 × 9-inch test section, was built with the approval of NACA headquarters to serve as a partial model for a larger supersonic tunnel authorized for construction at the new Ames laboratory in California. Though it had a water condensation problem, the Jacobs-Kantrowitz model supersonic tunnel provided Langley aerodynamicists timely education and experience in the fundamental phenomena of supersonic flow. In 1944 and 1945 Langley engineers changed this pilot facility to closed-circuit, dry-air operation. This conversion immediately preceded the NACA's major drive for large supersonic tunnels, which began in 1945.[2]

But the deepest probe into high-speed aerodynamics at Langley during the early part of the war was made by John Stack's wind tunnel groups.

In December 1941, a few weeks after test pilot Virden lost his life test-diving the P-38, Stack's 8-Foot High-Speed Tunnel (HST) group began an investigation of the stability and control problems of Lockheed's new airplane using one-sixth-scale models.[3] At about 450 miles per hour, shock waves formed on the upper surface of the P-38's wings. This formation of disturbed airflow—which was not unique to the P-38—made it very difficult, and sometimes impossible, for a pilot to recover the plane from a steep dive. Either controls stiffened up so much from the resulting loss of both lift and downwash on the tail that he could not pull out, or, as had happened in Virden's fatal case, violent buffeting and strong downward pitching motion tore the plane's tail off.[4]

In March 1942, after less than four months of tests in Langley's 8-Foot HST, Stack's engineers reported that they had an answer to the P-38's dive-recovery problem: a wedge-shaped flap installed on the lower surface of the aircraft's wings. They said that their tunnel tests showed that wings having this flap would retain enough lift at high speeds to enable a pilot to pull the plane out of steep dives.[5] Langley then turned the dive-recovery program over to its sister facility in California—Ames Aeronautical Laboratory at Moffett Field—where the flap idea could be proved sound to nearby Lockheed more expeditiously than at faraway Langley. Further tests in Ames's new 16-Foot HST did prove the idea sound: NACA-style dive-recovery flaps eventually saw service not only on the P-38 but also on the P-47 Thunderbolt, the A-26 Invader, the P-59 Airacomet (America's first jet), and the P-80, the first U.S. airplane designed (by Lockheed) from the beginning for turbojet propulsion.[6]

Stack's 8-Foot HST group carried out the dive-recovery program to prevent failures of precious combat aircraft in dangerous high-speed maneuvers. But there was another purpose as well: the Stack group did this work as part of a more basic program to develop a new family of high-speed airfoils. At the time that the P-38 was experiencing its most severe troubles, Stack and his closest associates were discussing the validity of calculations showing conventional airfoils to have improved lift and moment performance when operated inverted with negative camber—meaning with a curved-in or caved-in camber. (This was contrary to the conventional design, which added different degrees of outward curvature from the chord line.) However, all the Langley engineers involved in these conversations dismissed this as an unthinkable approach to solving the P-38's problems. Their educations and working experiences as aerodynamicists told them that positive camber was inherently beneficial to the lifting power of airfoils because such curvature caused the comparatively low pressures on the top side of the airfoil necessary for great lifting force. This lesson of conventional subsonic

Republic P-47 Thunderbolt, 1946. The dive recovery flaps are barely visible underneath the wings.

aerodynamics was so deeply ingrained in the engineers that they dismissed the new high-speed airfoil data as an impractical aerodynamic curiosity. One engineer remarked that pilots would reject the inverted wing because it would make them think that they had to fly their airplanes upside down.[7] The imaginative critic was being facetious, of course, because he realized that pilots would be willing to fly the new "upside-down" wing rightside up—especially if the wing helped them to pull out of supercritical dives.

Beyond the sniping at pilot mentality, though, Langley's work on the P-38 embodied an early attempt to obtain an airfoil with truly supercritical performance. The broader goal of the research program was to discover the new airfoil shapes that would make propellers and wings capable of flight at speeds of 500 miles per hour and beyond. At such velocities, which seemed so fantastic at the time, a plane would be flying into the mysterious transonic region. Tests in the lab's 12-inch and 24-inch induction-jet high-speed tunnels had already shown that even on approaching such speeds, air "bunched up" unpredictably on the upper surface of an airfoil. This bunching-up or *burble* caused an airfoil to lose its lifting power and controllability.[8]

In July 1943, the NACA directed Langley to investigate routinely this and other major impediments to safe and efficient flight at high speeds by creating a new Compressibility Research Division at the lab. This division incorporated all the ground-based high-speed research sections then at Langley, including the 8-Foot HST section, the 16-Foot HST section, the model supersonic tunnel section, and a small section under Arthur Kantrowitz involved with the study of fundamental gas dynamics. Five months later, George Lewis formed a special four-man panel (consisting of Stack, H. Julian Allen of Ames, Abe Silverstein of the Aircraft Engine Research Laboratory in Cleveland, and, as chairman, Russell G. Robinson of NACA headquarters) to coordinate NACA high-speed research.

The "Sound Barrier"

In 1935, a newsman asked British aerodynamicist W. F. Hilton what problem he was working on in the National Physical Laboratory's newest high-speed wind tunnel. Pointing to an airfoil drag plot, Hilton replied, "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, all the leading English dailies misrepresented Hilton's response by coining the phrase, "the sound barrier."[9]

The men who specialized in high-speed aerodynamics at Langley and elsewhere during this era knew that no actual physical barrier existed. But they did realize that flying at sonic velocity required not only finding a set of yet unimagined practical solutions to a number of tremendously adverse and perhaps ineradicable aerodynamic and power plant problems, but also overcoming some major inhibitions against assaulting what was commonly held to be an impenetrable barrier. Realization of these problems did not stop aeronautical engineers from trying to add a hundred miles per hour or more to the maximum practical speed of contemporary aircraft by refining design techniques, carefully streamlining aerodynamic shapes, and improving the power and efficiency of aircraft engines. It did prevent all but a few farsighted individuals, however, from considering flight at speeds approaching and going beyond that of sound.

Indeed Stack and Jacobs, his section head, had worked on high-speed aerodynamic problems from very early in their NACA careers. In 1927 they constructed an 11-inch induction-drive high-speed wind tunnel whose airstream was provided by a rapid blowdown from the VDT. Though certainly not meant to explore transonic flight, tests in the 11-inch tunnel provided the Langley engineers with important formative experience in high-speed aerodynamics. Compressibility phenomena and their ill effects on the performance of airfoils became a major new concern.

In 1933 Jacobs, who as an amateur astronomer was familiar with various optical systems, suggested that Stack try to make compressibility

During her tour of Langley in November 1928, Amelia Earhart had part of her raccoon fur coat sucked into the 11-Inch High-Speed Tunnel. To her left are Henry Reid and Col. Jacob Wuest, Langley base commander.

phenomena visible by using schlieren photography, a method first used by the Austrian scientist Ernst Mach (1838–1916) for visual observation of supersonic flow. Using a schematic drawing of the method from Robert W. Wood's *Physical Optics*, found in the Langley library, Stack constructed a crude schlieren device. Though the quality of the first photographs was poor, the results were nonetheless sensational:

> Shock waves and attendant flow separations were seen for the first time .... Visitors from all over the Laboratory, from Engineer-in-Charge Reid on down, came to view the phenomena.*

In 1934 Langley equipped its new 24-Inch High-Speed Tunnel (built with $10,000 provided by the Public Works Administration) with an improved version of the schlieren photographic system.

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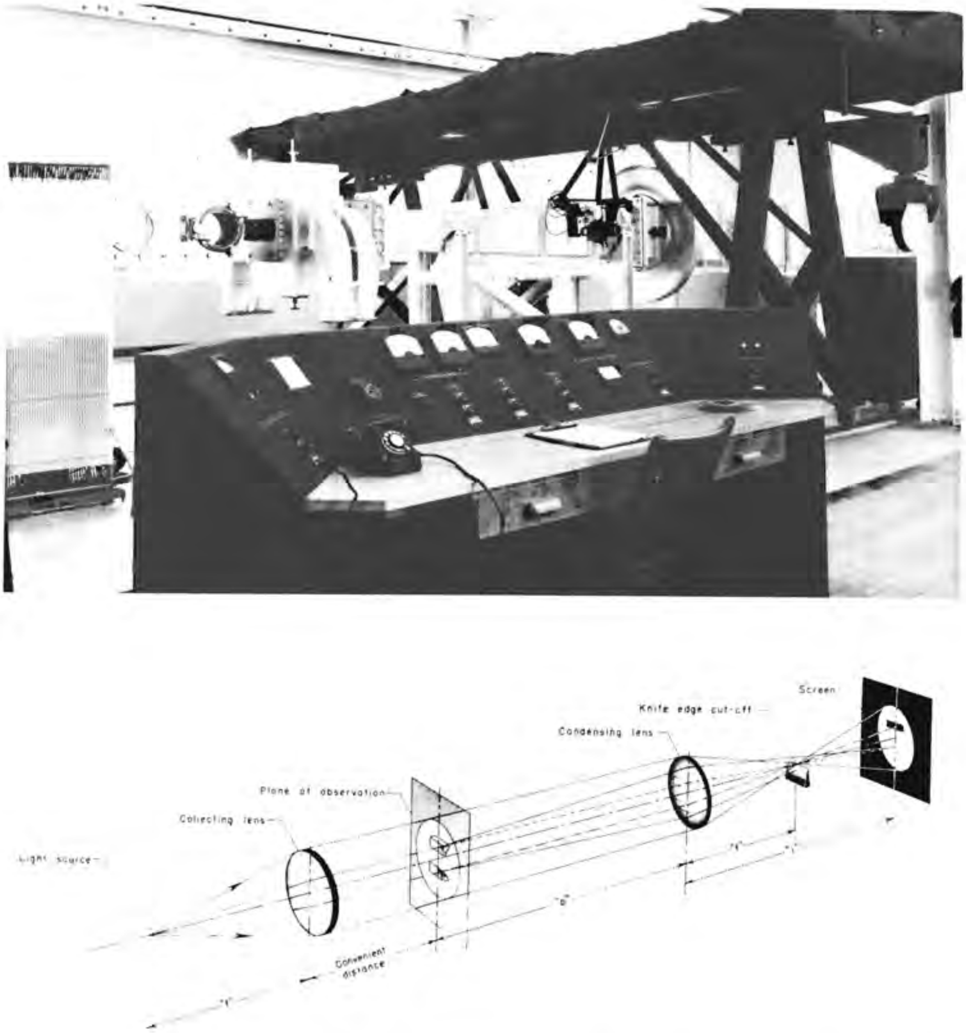
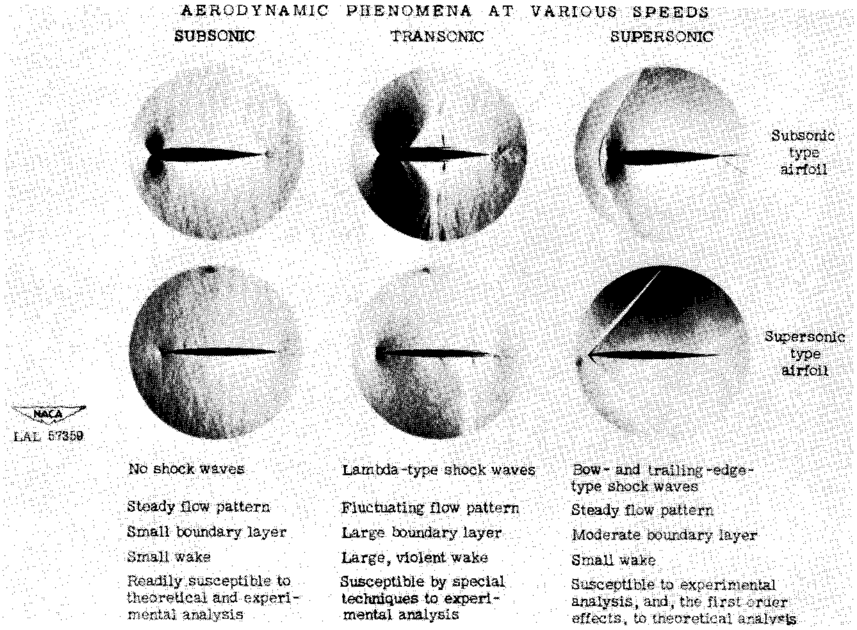
* John V. Becker, *The High-Speed Frontier*, p. 16. According to Becker, Theodore Theodorsen, Langley's ranking theoretician, viewed the schlieren photos skeptically, proclaiming that what appeared to be shock waves was really an optical illusion. At a banquet of the LMAL staff in 1936, Stack played the role of Theodorsen in a skit making his proclamation of the illusion.



Wind tunnel researchers used schlieren photographic systems to visualize high-speed aerodynamic phenomena affecting airfoil performance. In the photograph, a general view of Langley's 9-Inch Supersonic Tunnel, showing the control desk, the test section, and, just to the right of center, the schlieren apparatus. (Schlieren systems continue to be used today.)

Jacobs used the data from the first tests in this new tunnel as the basis of a paper he was preparing for the Volta Congress on High-Speed Aeronautics (in Italy). In this important paper, Jacobs tried to describe what was actually happening in the compressibility burble (i.e., the region



Schlieren photographs showing shock waves, wakes, and other aerodynamic phenomena affecting a low-speed and a high-speed airfoil at subsonic, transonic, and supersonic speeds.

of disturbed flow generated behind a shock wave). He also attempted to determine whether the critical Mach number could be increased by changing the shape of the airfoil in minor ways. What Jacobs actually did in this paper was derive the relationship between the local Mach number on an airfoil at high speed and the suction pressure on the airfoil at low speed. This derivation enabled aerodynamicists to estimate the critical Mach number from the low-speed pressure distribution on the airfoil. They could then improve the high-speed performance by making shape changes determined on the basis of simple incompressible theory or low-speed tests.[10]

In 1933 and 1934 Stack conceptualized on his own initiative an experimental aircraft for compressibility research and published a drawing of it in the *Journal of the Aeronautical Sciences*. He concluded from his paper study that a small propeller-driven monoplane, powered by a 2300-horsepower Rolls-Royce engine and equipped with wings designed in accordance with the NACA's new high-speed airfoil sections, might fly to a maximum 566 miles per hour. Though the NACA apparently never considered helping Stack to find a developer for the airplane, the optimistic results of his paper study convinced many people at Langley that the potential for flying at speeds far in excess of 500 miles per hour was there. To realize that

John Stack as an MIT student in 1927, and (larger photograph) as head of Langley's Compressibility Research Division in 1944.

potential, however, the first thing Stack felt that aircraft designers needed to have from the NACA was a much more complete understanding of the basic aerodynamic phenomena in the transonic region.[11]

Leaders of the aerodynamics staff at Langley like John Stack and Eastman Jacobs understood the physics of fluid flow well enough to know that though "barrier" was the wrong word, there did exist a definite set of transonic flight problems. There were flight and propulsion ingredients to this problem-set, and there was a major wind tunnel ingredient. (In demonstrating how the resistance of a wing shot up "like a barrier" as it approached sonic speeds, had not Hilton pointed the English newspaper reporter to an airfoil drag plot drawn from a series of tests from a wind tunnel?) A limbo in the state of tunnel technology existed just below and just above the speed of sound, preventing fruitful research until there were practical solutions to difficult questions: Why did strange things happen in Langley's own tunnels as airflows approached Mach 1, the velocity of sound? Why could one get Mach 1 in an empty high-speed tunnel but not in one with a model installed in the test section? Why did the airflow in the 8-Foot HST always tend to choke up in the tunnel's throat at some Mach number, generally above 0.7, that was lower the larger the model size and its blockage effect? Why did this still happen no matter how fast NACA mechanics made the driving fans turn? Why did shock waves form off the test model, reflect off the tunnel wall, and thereby inhibit accurate measurement of flow characteristics and behavior around the model? Since the model supersonic tunnel was capable of producing airstream speeds in excess of Mach 2, why did it also choke when slowed down to produce transonic airspeeds?

Though these questions plagued aerodynamicists in the late 1930s, physicists had actually known the fundamentals of the choking problem and the identity of what was now called the "sound barrier" long before that time; in fact they had known them even before history's first wind tunnel had been built in the 1870s. In the 1830s, French scientists Wantzel and Saint-Venant had derived mathematically that a gas flowing through the narrowest part of a constricted duct could not exceed sonic velocity no matter how much additional driving pressure was exerted. To later scientists, this finding did not mean that supersonic flow was unattainable in channels; it meant only that the channel area had to be expanded or diverged to accommodate the increased volume required by the flow as it accelerated above Mach 1 downstream of the throat. In the late 1880s, Swedish physicist-mathematician De Laval applied this principle to achieve supersonic velocities in the convergent-divergent nozzles of his steam turbines.[12]

In the 1920s, Americans L. J. Briggs and Hugh L. Dryden obtained supersonic flow during experiments at the Edgewood (New Jersey) Arsenal with a small free-jet apparatus having a convergent-divergent nozzle. Results indicated that radical changes in the behavior of air happened as the speed of its stream approached that of sound.[13] Later in the decade, Eastman Jacobs and John Stack found during test runs of Langley's original high-speed induction tunnel that higher Mach numbers could be reached with an open throat than with a closed throat. Throughout the 1930s Stack and colleagues explored the nature of the tunnel choking problem. Among other things, they made a detailed study of the blockage effect caused by the presence of a model in the test section. Until aircraft reached much higher flying speeds, the choking problem did not demand a practical solution, however; at low airspeeds, the choking effect was small and accurately correctable.

In the spring of 1940 William J. Orlin, an independent-minded engineer in Stack's 8-Foot HST section, developed a small water channel to visualize the process of the tunnel choking problem by hydraulic analogy. Though this little facility provided some valuable insight into the dynamics of a choking fluid, its operation suggested no solution to the problem of choking.[14] By December 1941 it was clear to Stack and his wind tunnel engineers that no one was likely to solve this problem for some time—if ever. No one was making any significant progress in the theory of transonic flows. Thus they envisioned only one alternative to boarding up the now vitally important transonic region and closing it off from study: a specially instrumented full-scale transonic research airplane.

Stack Gets Go-Ahead

Stack sold Langley management on the technical merits of the transonic research airplane idea in the spring of 1942. Then, though he and everyone else at the lab knew very well that the NACA charter did not allow construction of a complete airplane—and that the NACA budget could not finance such an enterprise even if it did—he brought the idea before George Lewis, director of NACA research. Lewis did not care for the research airplane idea on principle, but characteristically, he tried not to react too negatively. He liked Stack: not only was Stack a rugged individualist, a "man's man," with self-assured ways and ambitious ideas, but he had also proved over the last 15 years to be one of the NACA's best researchers. One question Lewis asked Stack playfully, before talking with him more seriously about the present strain of military research and the inevitable problem of getting such a project off the ground, was whether people might interpret the NACA's unprecedented desire for a research airplane as an admission of some basic failure on the part of all those expensive wind tunnels and their champions at Langley. He left Stack with the idea, however, that some low-priority, back-of-the-envelope estimates to identify the most desirable design features of a transonic airplane could not hurt anyone, providing they did not distract from more pressing business.

Though he knew that this go-ahead by no means implied Lewis's general approval to develop and procure an airplane, Stack immediately assembled a special team of NACA researchers to work out the design requirements of a transonic research aircraft. Stack selected engineer Milton Davidson and junior engineering aide Harold Turner, Jr., to make preliminary layouts and performance estimates. By the early summer of 1943 Davidson and Turner finished a preliminary design of a small turbojet-powered plane capable of flying safely to high subsonic speeds, from Mach 0.8 to 1.0.[15] (Turner had also helped Eastman Jacobs design his proposed research airplane, the one described in the previous chapter, but this new design was far different.)

Stack wasted no time in sending news of his pet project to friends and selected acquaintances inside the aeronautical branches of the army and navy.

Military personnel who learned about Stack's transonic research airplane idea had solid reasons to be cautiously interested in it. Ezra Kotcher, the chief of aeronautical research of the Air Service Materiel Command at Wright Field, felt, too, that the speed of sound was only a wind tunnel and mental barrier. In 1939, Kotcher (a 1928 graduate in mechanical engineering from the University of California) had himself suggested the construction of a transonic research airplane to be powered by either a gas turbine or rocket propulsion system.[16] Because it came before news of the British and German successes with turbojets, General Arnold had rejected Kotcher's suggestion. Several things had happened since 1939, however, to change Arnold's attitude: the army had found out about the Whittle engine; at Arnold's instigation, secret development of an American turbojet had begun; the Guggenheim Aeronautical Laboratory at the California Institute of Technology (GALCIT) had completed a series of experiments with JATO (jet-assisted takeoff) rockets for army ordnance;[17] and, perhaps most importantly, two of the army's newest high-speed combat aircraft—the Republic P-47 Thunderbolt and the Bell P-39 Airacobra—had experienced fatal tail failures during high-speed dives. This frightening epidemic was hitting navy airplanes just as hard: for example, the horizontal tail of its Curtiss SB2C Helldiver was fluttering so badly in high-speed maneuvers that the tail was breaking off.[18] Together, these developments were making answers to compressibility problems an urgent necessity.

Although the loss of life and aircraft heightened both army and navy interest in a possible NACA high-speed research airplane, few officers in either service responded to Stack's proposal immediately. Kotcher, for one, was too busy coping with buzz bomb problems (the army had just assigned him the formidable job of copying the German pulsejet-powered V-1 robot missile). One military man who did actively advocate support for Langley's concept was Capt. Walter Diehl, USN. A longtime friend and close working associate of the NACA, Diehl argued in late 1943 during meetings with the chief of BuAer's structures branch that a transonic research airplane was the only way to convince people that the "sound barrier" was "just a steep hill."[19]

The first time that the military actually recognized and formally discussed Stack's idea, however, was at a conference between service and NACA personnel held at Langley laboratory on 15 March 1944. On that day, two meetings—one chaired by Captain Diehl and the other by Col. Carl Greene, the permanent liaison officer at Langley from the Materiel

Command—dealt with the development of a possible transonic research aircraft. During these meetings the NACA put its weight behind Stack's proposal of a joint NACA-military program to develop and construct a transonic research airplane. According to NACA spokesmen, the purpose of this airplane would be to collect the aerodynamic data needed for transonic flight that could not be obtained in any wind tunnel.[20]

Langley's visitors could not reach agreement with NACA representatives that day, however, over the goals of a transonic research airplane, let alone over its basic design features. Army spokesmen conceived of the airplane more "as a major developmental step toward higher operating speeds extending upward through Mach 1," and navy representatives were inclined to view it "as a means of dispelling the myth of an impenetrable barrier and providing needed high-speed data."[21] Some of the differences of opinion were quite outspoken. For example, Maj. Gen. Oliver P. Echols, assistant chief of staff at Air Corps headquarters, questioned the wisdom of procuring in wartime a nonmilitary research airplane, especially if the army was going to pay for most of it. By the end of the meeting, two things were frustratingly clear to Stack and his colleagues at the NACA: the competitiveness of the services was going to make it very difficult to win joint army-navy cooperation in the development of a single experimental vehicle; and the NACA had better find some other reliable method to collect transonic data.

Stopgap Methods

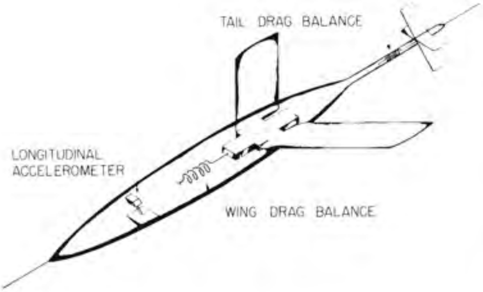
Although the Langley staff slowly came to believe that Stack's transonic airplane should perform the ultimate experiments, it supposed at first that an area as mysterious as the transonic speed region had to be attacked from every possible theoretical and experimental approach. After attempting some theoretical studies, however, the staff chose to treat the problem of transonic flows as essentially an experimental problem. In its opinion, transonic flows involved too many unknown physical principles and complex mathematical relations to recommend the theoretical approach.

In 1942 John W. "Gus" Crowley and Floyd L. Thompson of the lab's flight research section suggested that the NACA improve understanding of the aerodynamic phenomena occurring at high speed by mounting a wing model on a bomb-shaped missile and dropping this body from an airplane flying at great altitude. (The Germans had already tried this technique in 1941, but it is unclear whether Crowley and Thompson knew about this previous experience.) After some preliminary tests, Langley temporarily abandoned this *drop-body* technique. Not only did the lab lack adequate radar and radio telemetering equipment to measure the high-speed flow around the test model accurately, but it was also just too much of a chore to build the big test model (made from a piece of pipe 12 feet long and 10 inches in diameter, filled partly with concrete), carry it up to 40,000 feet, and then find and salvage the model after it had sunk several feet into the muddy bombing range at Plum Tree Island near Langley Field.[22]

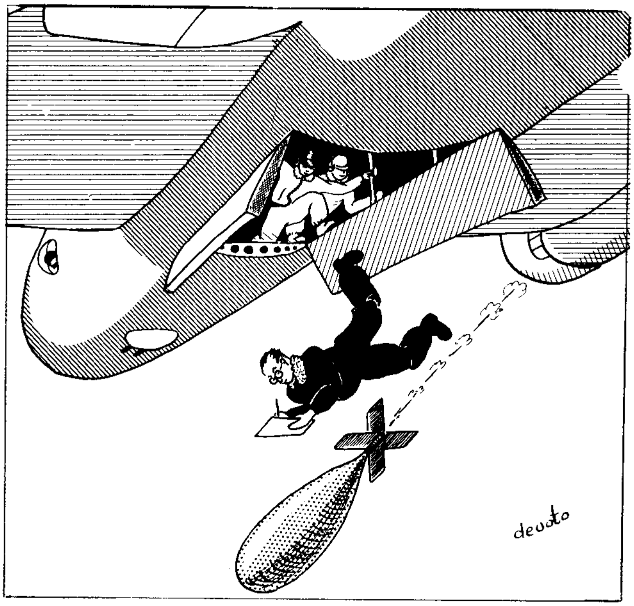
In December 1943 the NACA revived the falling-body idea in response to a proposal for a joint American-British effort on the transonic research problem made by William S. Farren, director of the British Royal Aircraft Establishment. During his visit to Washington that month to give the annual Wright Brothers Lecture, Farren described how the RAE, too, had experimented with dropping weighted bodies from high altitudes. He indicated that such investigations could now be carried out very effectively thanks to tremendous advances in the development of radar and other electronic telemetering devices.[23] Farren's message convinced the NACA Executive Committee that this type of research should be tried again at Langley. On 25 March 1944—ten days after the conference dealing with Stack's transonic airplane concept—the committee approved research authorization 1224, a confidential "Investigation of Aerodynamic Characteristics of Free Bodies at High Mach Numbers."

Langley began new falling-body tests with a Boeing B-29 Superfortress borrowed from the army, which it equipped with the navy's most advanced SCR-584 radar tracking unit. The large airplane carried the test missile to 30,000 feet and then released it. Ground observers received radio signals relayed from instruments inside the body. These instruments, which were developed just for this test program by Edmund C. Buckley of Langley's Instrument Research Division, measured the forces on the body as it dropped at velocities sometimes equaling or exceeding that of sound.[24] Though the speed range and therefore the data were limited by the maximum operational altitude of the B-29, and though Theodore von Kármán of Caltech raised the possibility of errors due to "acceleration effects," NACA engineers considered these data reliable enough to estimate the drag and power requirements of a transonic airplane.[25] (By dropping identical models of varying weight, the NACA later proved that the acceleration, or virtual mass, effects were negligible.)

In 1944, an NACA engineer devised a second alternative method of transonic research whose value was perhaps even greater. Robert R. Gilruth, the young engineer from the University of Minnesota in charge of Langley's flight research section, had noted as early as 1940, during dive tests of the Brewster XF2A-2 airplane, that the transonic flow fields developing on wings in actual flight were 10 to 20 times larger than those predicted by wind tunnel tests on models. More recently, he had heard pilots who had put



The high speeds of diving aircraft and missiles during World War II took the NACA by surprise, for there were no wind tunnels in its laboratories capable of exploring transonic aerodynamics. One of the stopgap methods developed by Langley after 1944 to acquire meaningful data near Mach 1 involved dropping instrumented bodies (top) for free falls from high altitude. The first aircraft used by the NACA to drop its instrumented bodies was a Boeing B-29 Superfortress (bottom), the type of aircraft that dropped the first atomic bomb. In the early 1950s, the NACA used a North American F-82 Double Mustang (just below) for its drop-body tests. A test body is shown mounted beneath the Double Mustang's center wing section.

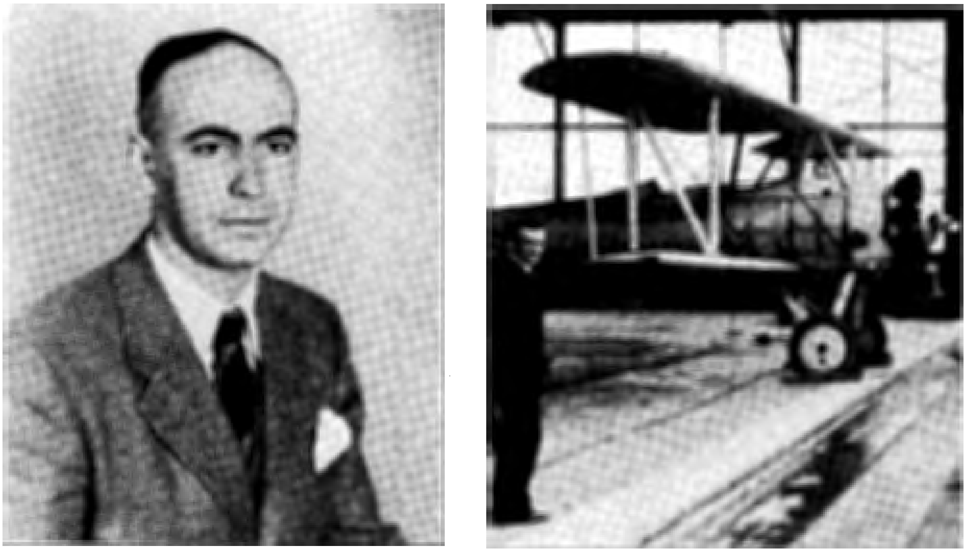


".....I know, but Boy, you should see the swell data we get!"

From the Langley Air Scoop*, 25 October 1946.*

the new North American P-51 Mustang into dives report that they could actually see, if the sunlight was just right, the shadowy edges of shock waves cutting across the streamlines of their airplane's wings. Though this sleek little fighter plane was diving at speeds only to about Mach 0.75, naked-eye observation of this phenomenon by pilots suggested to Gilruth that the flow of air over a portion of the P-51's wing was moving quite smoothly through the speed of sound to low supersonic speeds! Although admitting that a number of Langley's wind tunnel researchers were far more expert than he was in the physics of airflow, the head of the flight research section believed that the supersonic flow region that existed on wings at supercritical (but still subsonic) flight speeds could be used as a test environment. This "flying wind tunnel" would have one great advantage over tunnels on the ground: it would not have walls to constrict and distort the airstream.[26]

In his first application of the *wing-flow* technique, Gilruth mounted a small airfoil model perpendicular to the upper surface of a P-51D's wing. He placed the model vertically just above the Mustang's wing, making sure, in order to generate uniform flow for valid testing, that it rested in that part of the supersonic flow region where the induced velocity was most constant. An NACA test pilot then flew the Mustang to the desired



After extensive flight experience as an engineering observer in the late 1930s, Robert R. Gilruth (left) had a keen appreciation for the pilot's side of the man-machine relationship. In particular, he had learned a great deal from Melvin N. Gough (right, 1936), Langley's chief test pilot, who took great pains to educate Gilruth about airplane handling characteristics.

altitude and dived the plane as rapidly as he safely could, which was to about Mach 0.81. As the speed of the plane in its dive increased, airflow around the small wing-mounted model passed from subsonic through the transonic region to supersonic velocities on the order of Mach 1.4. A small balance mechanism fitted within the P-51's gun compartment and tiny instruments built into the mount of the model recorded the resulting forces and airflow angles. (Because diving the airplane to high speeds was a dangerous maneuver, Langley developed an advanced system of rapid-response instrument surveillance to indicate whether the pilot was headed for trouble in handling the airplane's controls. Specialists in stability and control cleared each test flight that was to go to a previously unexplored high speed, and specialists in reading and interpreting the instruments indicating control handling qualities constantly advised the pilot whether he should proceed with the test once it had started.)[27]

The first reaction of the majority of Langley's wind tunnel groups to Gilruth's wing-flow technique was negative: "There was great consternation amongst the wind-tunnel people in why a young upstart could come along [with a solution] when all their wind tunnels had" failed.[28] Some of Gilruth's best friends were completely against using the method. Engineers in the

Gilruth based his small-model wing-flow technique on the physical fact that air above the wing of a high-speed airplane, like the P-51 Mustang, went quite smoothly and uniformly through the speed of sound.

16-Foot HST group, for example, pointed skeptically to its "many obvious problems" and "impurities"—the nonuniformities of the flow field, the unknown effects of the wing boundary layer, the problem of wing shock passage over the test model, and the very low test Reynolds numbers.[29]

Gilruth agreed with but discounted the technical content of most of these criticisms by confronting the method's critics with two rhetorical questions: Was not the collection of any usable transonic data preferable to the collection of none? Had technical problems and impurities comparable to those handicapping the wing-flow method ever stopped wind tunnel researchers from experimenting with new ways of doing things? Of course not, Gilruth knew. The young engineer tried to reassure the wind tunnel specialists by telling them that in applying the wing-flow method his flight researchers were not using just any airplane wing. They were modifying the wing surface experimentally to meet all the aerodynamic conditions essential for valid testing.*

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* There were four surface conditions necessary for the wing-flow method to be valid: (1) the chord-wise velocity gradient had to be sufficiently small; (2) the velocity gradient normal to the airplane wing had to be sufficiently small for a distance somewhat exceeding the span of the test model; (3) the boundary layer on the airplane wing had to be sufficiently

Gilruth's first wing-flow test results—which the NACA kept secret—impressed Langley's wind tunnel groups.[30] Besides demonstrating airflow trends that conformed to expectations, they gave the most systematic and continuous plots of transonic data assembled by the NACA up to that time. This success not only prompted Langley to put an entire series of wings of various thicknesses through the wing-flow examination, but also eventually helped to confirm its opinion that supersonic flight required a thin wing.

About one year after starting to use the drop-body and wing-flow techniques extensively, NACA Langley began to develop a third stopgap method of acquiring transonic data: *rocket-model* testing. Conducting research on "pilotless aircraft" (the military's name for all types of guided missiles) was not at that time new to the laboratory, however. From the Executive Committee's authorization of a test of the General Motors "flying bomb" in the Full-Scale Tunnel in June 1941 to the time it approved rocket-model testing in early 1945, Langley had worked, in one way or another, on practically every guided missile project started by either service, including the testing and development of glide, shrouded, and buzz bombs, gliding torpedoes, and various types of interceptor missiles.[31] In December 1944, acting engineer-in-charge John Crowley organized a "Special Flying Weapons Team" to oversee all missile research at Langley.[32] (Henry Reid was at the time in France with the Alsos Mission, a secret group sent by the secretary of war to the European theater to identify and collect valuable scientific research information abandoned by the retreating German army.)

Along with its support of the military's top secret guided missile projects, Langley began an ingenious program of more basic aerodynamic tests. From the remote Atlantic coast beaches at Wallops Island, some distance from Hampton off the Eastern Shore of Virginia, a small team of researchers launched rocket models weighing about 40 pounds, of which about 50 percent was the weight of the rocket motor and about 20 percent the rocket fuel. These rocket models shot up into the air to a maximum velocity (at an altitude of only 2000 feet) of about Mach 1.4, continued upward, and then splashed into the Atlantic Ocean. The useful portion of the rocket's flight terminated at about 15,000 feet, meaning that the data were obtained in relatively dense air where the Reynolds number was high. Originally the researchers tracked the models and determined

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thin so as not to affect the flow at the juncture of the model and the main wing; and (4) the normal shock that occurs on the main wing had to be sufficiently far back so that the pressure rise through the shock acting back through the boundary layer could not affect the flow at this same juncture. See Robert R. Gilruth, "Résumé and Analysis of NACA Wing-Flow Tests," unpublished paper presented at the Anglo-American Aeronautical Conference, Sept. 1947, copy in Langley Central Files (LCF), A184-9, "High-Speed Research."

The NACA's first year of operations at remote Wallops Island, Virginia, resembled the activities of advanced military bases in the Pacific during World War II.

their speed, drag, and control effectiveness by using the immediately available telemetering instrumentation previously developed for the falling-body tests; later, they adapted a Doppler radar system which, for certain research purposes, made it unnecessary to place complex instruments inside the test body. In the spring of 1945, Congress approved the NACA's request for a supplemental authorization for a permanent rocket launch facility at Wallops. The purpose the NACA had in mind for this facility was not only to support the military's ballistics projects but also to help define the basic airplane wing and fuselage configuration best able to fly in and through the transonic range.[33]

The rocket-model test was a challenging technique for NACA flight researchers to perfect. It required them to acquire and apply new knowledge about how to measure, transmit, and record accurate test data during the few fleeting seconds of a flight which changed speed, altitude, and model attitude rapidly. By comparison, the aerodynamic goals of the initial test flights were relatively simple, such as to trace the minimum drag curve throughout the transonic region for a variety of representative test objects. Beyond the exploratory flight testing for which the rocket models were suited originally, however, the new technique made it possible in some later cases to employ systematic parameter variation. Beginning in the summer of 1945, a succession of rocket models were launched at Wallops, each model identical to the next except for one geometrical feature.

Though wind tunnel groups at Langley knew that the rocket-model technique was not well suited for advanced aerodynamical research involving extensive pressure distributions, flow surveys, boundary-layer measurements, and flow visualization, they did not criticize the rocket-model technique—as they had the wing-flow technique—on scientific and technical grounds. The wind tunnel groups realized that the new flight technique was largely free of the impurities of the other stopgap transonic techniques and thus constituted exactly what was needed by the NACA at the time. They credited Robert Gilruth and his principal assistants for coping energetically and ingeniously with the inherent problems of the technique, and credited Edmund C. Buckley and his group for devising the indispensable tiny flight instruments.

A rocket model is fired from Wallops in 1949.

But wind tunnel groups did eventually criticize rocket-model testing for interfering with tunnel programs. Each firing required the sacrifice of a precious test model, many of them having expensive instruments inside. Though Langley employed its own shop staffs to build these models and incorporate the instruments, wind tunnel proponents complained, especially after the June 1946 conversion of the "Auxiliary Flight Research Station" at Wallops from a subordinate unit of Langley's flight research department into a separate "Pilotless Aircraft Research Division" (PARD), that the "voracious appetite" of the rocket-model specialists was resulting in "a major slowdown" in the production of their own necessary test models and instruments. In the years 1947, 1948, and 1949, PARD expended no fewer than 386 models. Wind tunnel personnel told themselves that this expenditure was "roughly equivalent to the requirements of perhaps 10 major wind tunnels." Privately, they also said that the practitioners of the rocket-model testing technique tended, partly out of necessity, to be "as much interested in making the rocket models to do more things accurately as they were in the research problems." This tendency was apparent to them in that a majority of PARD reports discussed and analyzed the performance of specific model configurations without shedding much new light on the underlying flow processes—which were, after all, the main object of rocket-model studies in the first place.[34]

Though they could not totally dispute the charges, PARD employees objected strongly to implied criticism of the value of rocket-model testing in comparison with the value of wind tunnel testing. Though rocket-model testing appeared expensive because of the loss of complex and costly models to the depths of the Atlantic Ocean, a single test provided enough important data, they said, to establish the key flight parameters. Thus, the dollar-for-dollar return on the NACA's investment in rocket-model research at Wallops at least matched that provided by the wind tunnels.[35]

However stridently the individual research groups may have debated the scientific, technical, and budgetary validity of the new drop-body, wing-flow, and rocket-model techniques, the internal debate never overshadowed the commitment of Langley's research staff as a whole to exploring every avenue of transonic research. As NACA engineers and scientists, they knew that there existed, between the study of fundamental fluid mechanics and the large-scale testing of specific ideas, a range of problems for which either wind tunnel or free-air methods of research were most suitable. They knew also that every particular method had advantages and disadvantages. Thus they concluded that the peculiarities of the individual problem should dictate the choice of method. History bears out the truth of this conclusion: the early years of the rocket-model program at Wallops (1945–1951) showed that Langley was able to tackle an enormously difficult new field of research with innovation and imagination.

Back in March 1944, before these alternative, free-flight methods of transonic research had been established, no one understood the need for flexibility in research method better than John Stack. If the frustrating interservice rivalries and differences of opinion that surfaced at the Langley conference that month made it seem impossible for the army and navy to cooperate in a high-speed research airplane program, then the NACA should try a new approach: it should try to persuade one of the services, or each of them individually, to procure its own transonic research airplane.

10

Defining the Research Airplane

John Stack had first conceived a high-speed research airplane in 1933, but his paper design had been merely an object for theoretical performance evaluation. He had wanted to explore how fast an imaginary airplane with all known favorable features could go when due allowance was made for the adverse effects of compressibility on drag and propeller performance.

With the coming of the compressibility crisis by 1940 and the growing recognition that there was some barrier preventing the acquisition of useful transonic data in existing wind tunnels, Stack began to campaign privately for NACA and military support for an actual airplane for high-speed research. By 1944, however, there were engineers, like Ezra Kotcher at Wright Field, and even some of Stack's colleagues at Langley, who had competing ideas for the requirements of a high-speed research aircraft. Though many of the particulars of Stack's research airplane concept would provide a solid foundation for the design of what became the Bell XS-1, the first plane to fly supersonically, some of its particulars would not be accepted and others would undergo major compromise.

Working for Procurement

Stack worked first on his contacts in the army. Citing the primary role the army had played in procuring the P-59 Airacomet, the first American turbojet plane, he pressed Col. Carl Greene and his assistant Jean Roché of the Air Materiel Command liaison office at Langley Field to persuade their superiors to develop a transonic research aircraft; within a few weeks a delegation from the Materiel Command, which included Ezra Kotcher, traveled from Wright Field to Langley to renew discussions with the NACA about the requirements of such an aircraft. At the first of two meetings in mid-May 1944, Kotcher reported the results of Wright Field's "Mach 0.999

study," the principal objective of which was to compare the theoretical performance of turbojet and rocket airplanes at high Mach numbers.[1] He told Stack and his colleagues that the experience of the P-59 proved that turbojets could not propel aircraft to transonic speed, while the results of the Mach 0.999 study indicated that rockets could. Kotcher then showed the Langley engineers a rough drawing of a rocket plane Wright Field had in mind.

Stack responded to Kotcher's message by repeating a long-held NACA opinion: the application of rockets to airplanes was too unsafe. Stack knew that Melvin Gough, Langley's chief test pilot, had privately issued the edict, "No NACA pilot will ever be permitted to fly an airplane powered by a damned firecracker!" He let Kotcher know that the majority of Langley test pilots had opposed the idea of the transonic research airplane in the first place; they had felt that they were being asked to risk their lives because wind tunnel personnel were unable to do the necessary work on the ground. Now pilots were going to be asked not only to sit in the cockpit of a radically new airplane, atop a heavy load of explosive fuels, but also to rely on *only* a rocket to keep them aloft! No pilot in his right mind would want to fly this plane, Stack said. (It is not clear whether Stack or his associates knew anything yet about the experience of the Germans with the ME 163 rocket plane.) Furthermore, a rocket plane simply could not meet research needs as well as a turbojet. Because it could not stay in the air as long, it could not gather the kind and volume of systematic data that everyone required. Lastly, Stack argued that the performance of an experimental rocket aircraft surely would not be as applicable to the future development of aviation as that of the turbojet. At the end of this conference, however, it was agreed that the NACA would continue its separate study for the design of a transonic airplane and, upon completion, transmit a report about it to the army for comment.[2] Stack held back submitting his design to Wright Field until 10 July, and then it still incorporated a turbojet rather than a rocket engine. The purpose of his airplane as he conceived it was to collect transonic data (space was provided for 400 pounds of research instrumentation), rather than to fly supersonically.

At another round of meetings at Langley on 13 and 14 December 1944, army representatives—many of whom Kotcher had persuaded personally to support his idea of a rocket-propelled transonic airplane—rejected the NACA's proposal for a turbojet as too conservative.[3] The Stack team had designed an airplane to fly in the speed range from Mach 0.8 to 1.0, with a typical high-speed dash velocity of Mach 0.85 (650 MPH); the army wanted a plane that could fly supersonically to about Mach 1.2 (800 MPH). This apparently irreconcilable difference of intent was resolved easily: the army

Melvin N. Gough started his NACA career in the Propeller Research Tunnel. After taking flight training and becoming a reserve navy pilot in the late 1920s, he transferred from the PRT to the flight test section. He soon became one of the country's most accomplished experimental test pilots.

"was putting up the money and they decided to do it their way."[4] One week after the meetings at Langley, the army started negotiations with the Bell Aircraft Company to procure a rocket plane. Bell immediately called together a design team headed by Robert Stanley, a California Institute of Technology aeronautical engineering graduate who had been the pilot of the first American turbojet, the XP-59. Under project designation MX-524, Stanley's team began development of the "Experimental Sonic-1" aircraft, or "XS-1" for short.[5]

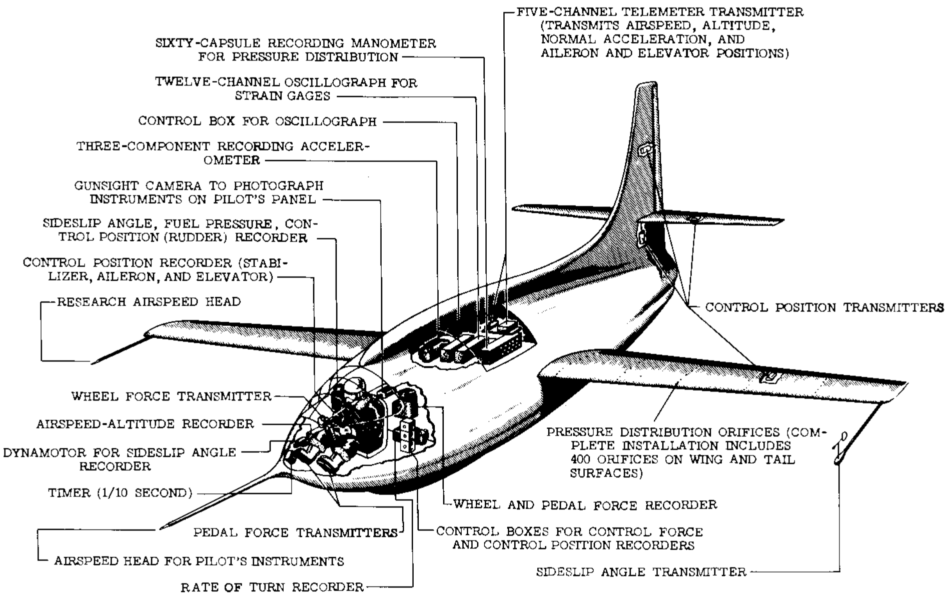
Stack did not give up the idea of procuring the kind of transonic research airplane he wanted. In fact, as soon as Kotcher made it clear to him in the summer of 1944 that the army was going to insist on a rocket plane, he had contacted the navy. He wrote letters and telephoned various friends and acquaintances in the Bureau of Aeronautics, telling them that the rocket plane the army was procuring would probably not survive many flights. With the help of George Lewis and Capt. Walter Diehl (Lewis's good friend), Stack arranged to detail his engineer Milton Davidson to

Washington to work with personnel in BuAer's aviation design research branch on specifications for a transonic research aircraft.[6]

Since the navy had done very little in the way of research airplane studies up to this time, it was more ready than the army had been to accept the NACA's advice and general guidelines. In September 1944, a BuAer engineer (Abraham Hyatt, a Marine Corps officer and an aeronautical engineering graduate of the Georgia Institute of Technology) formally proposed that the navy procure a high-speed research airplane capable of meeting both military and NACA research requirements. Though the navy blueprint proposal called for some details different from those already set out by the Stack team (for example, side inlets instead of a nose intake, so as to free the nose for an armament installation), it basically matched the NACA's conservative design: the plane would take its power from a turbojet, not rocket, engine; the plane would take off from the ground and land under its own power; the plane would have good enough low-speed handling characteristics that data gathered from its flight test program could be applied directly to the design of future navy aircraft; and, finally, the plane would have a maximum velocity not exceeding the speed of sound. Together, these details would make an airplane far different from the XS-1 being planned by the army.

In late December 1944 Davidson informed Langley that the navy had taken the first step toward procurement of this airplane: BuAer had shown a representative of the Douglas Aircraft Corporation, one of the prime contractors for naval aircraft, a preliminary specification of the proposed experimental plane and asked him whether Douglas would be interested in working on it. Apparently the representative had immediately taken the offer back to his company's main office in California, Davidson reported. The report was accurate. By the first weeks of 1945, Douglas engineers were busy considering the design criteria for what would become "Douglas Model 558, High-Speed Test Airplane," the "D-558" for short. BuAer made it clear to its contractor from the start that the navy "was only interested in obtaining an airplane which met with the full approval of the NACA."[7]

Thus by early 1945 the development of two different transonic research airplanes was under way in the United States: the rocket-powered XS-1, being built by Bell under Army Air Forces sponsorship, and the turbojet-powered D-558 being built by Douglas under navy sponsorship. Though researchers at Langley would actively assist in the development and flight testing of both airplanes, they would have reason to prefer helping with the D-558. It was most like the research airplane they wanted.



NACA RESEARCH INSTRUMENTATION IN XS-1 ROCKET AIRPLANE

By December 1944 the NACA had determined that the XS-1 rocket plane should carry roughly 500 pounds of research instrumentation and auxiliary equipment within a space no larger than nine cubic feet.

The Bell XS-1

As soon as the Army Air Forces decided to procure an experimental rocket-powered aircraft, Langley researchers helped Bell engineers to determine the vehicle's basic design criteria. In December 1944, they estimated the instrument requirements for the XS-1: 370 pounds of instruments and 130 pounds of auxiliary equipment (wiring and tubing), all to fit within a space of nine cubic feet.[8] This estimate would form the basis for the package of instruments eventually installed in both the XS-1 and D-558-1. In January 1945, they finished calculation of the load requirements of the airplane: a load factor of 18g, or 50 percent higher than the usual load factor of fighter aircraft. (With a load factor of 18g, the aircraft could accept the stresses and strains of aerodynamic forces equivalent to 18 times its own weight.) Stack suggested this figure because he wanted a wide margin of safety for the plane's first flights.[9]

One of Langley's most important recommendations for Bell's design of the XS-1 was its call for a thin wing section to minimize the buffeting, loss of lift, and control problems that the experimental aircraft would probably experience at supercritical speeds. Langley thought long and hard before making this recommendation, but not because its research staff lacked knowledge about the effects of wing thickness ratio on transonic performance. By early 1945 the staff knew from Hugh Dryden's earlier work at the Bureau of Standards, from preliminary data from Gilruth's wing-flow tests (described in the previous chapter), and from a recent report of their own high-speed airfoil group that "airfoils of large thickness ratio should not be used at high Mach numbers because of radical adverse changes in their characteristics at supercritical speeds." The shock-stall effects were just too severe.[10] Langley engineers disagreed sharply, however, over whether Bell should *deliberately* design wings to throw the XS-1 most quickly into the troubling region of deep shock stall, from Mach 0.75 to 0.90. There were two schools of thought on this question at the lab, one led by John Stack and composed mostly of wind tunnel people, and the other led by Robert Gilruth and made up primarily of his fellow flight researchers.

Stack and his followers advocated a wing section of average (12 percent) thickness. They did so for reasons that Stack made clear in late 1944 in a handwritten note to himself in preparation for a conference with army personnel about its transonic airplane designs:

> 1. 12% wing questioned
>   (a) A good thinner wing for higher speed
>   (b) Note flight further into supercritical region with 12% than with thinner wing—primary purpose of aircraft is to get far into supercritical region
>   (c) Unconventional landing arrangements demand good [maximum-lift coefficient]—less than 12% [thickness-chord ratio] gives poor [maximum-lift coefficient]
>   (d) Unknown or uncertain loading at supercritical *M* demands wing having great strength for first flights—Basic load data obtained would then permit precise design of structurally more difficult thin wings[11]

In sum, Stack wanted Bell to choose a thick wing because it would force the research airplane to encounter exactly those drastic flow changes occurring at critical Mach numbers that aerodynamicists were most interested in studying and correlating with wind tunnel results. The research benefits would be greater.

Gilruth and his followers strongly opposed Stack's point of view. They opposed it, not because as proprietors of the NACA's wing-flow method they possessed some knowledge that Stack and his wind tunnel engineers did not have about thin wings retaining their lift at transonic speeds, but because they had a different concept of the airplane's safety requirements. Gilruth believed that Bell should design the XS-1, the first aircraft to penetrate deeply into the supercritical zone, with every feature it knew could contribute to the airplane's safe operation. "If you put a thick wing on it," Gilruth warned, "it's bound to have problems." On the other hand, if you put a thin wing on the XS-1 (he suggested using wings as thin as five percent thickness-chord ratio), not only would you have a safer airplane, but you might be able to fly through the speed of sound with it.[12] Ironically, Gilruth's conservative concept of the safety requirement was leading him to consider the possibility of the XS-1 flying supersonically, while Stack's adventurous attitude toward that requirement was keeping the airplane he had in mind to speeds well below Mach 1.

Before the NACA could recommend to Bell a thickness ratio for the wings of their airplane, Langley management had to resolve this disagreement between the Stack and Gilruth groups. Resolutions of this sort were essential to the success of the lab, for it was an organization of people from many diverse disciplines. The assistant chief of research, Floyd Thompson, with nearly 20 years of broad experience in NACA flight testing and understanding of many different fields, had the responsibility of assessing the contradictory recommendations given to him by his specialists. Thompson talked at length with both groups of engineers, studied all the relevant data collected by them, and made his decision: Gilruth was right; the XS-1 needed to have a thin wing.

Stack pushed for a compromise: perhaps the research airplane could have two sets of wings, one not quite as thin as Gilruth wanted and the other not quite as thick as Stack wanted. Thompson and the rest of Langley management concluded that splitting the difference was a good idea. It was doubtful that Bell could fabricate a wing as thin as five percent with the desired overstrength load factor of 18g anyway. Between March and July 1945, the NACA decided to advise Bell to build two sets of wings, one eight percent thick and the other ten percent thick.[13] Bell followed this advice. The company built the XS-1 to fly first with the thin wing, but later, in order to provide the data the wind tunnel people wanted, to fly with the somewhat thicker wing.*

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* "As it turned out, the most important region for comparison of flight and tunnels was from Mach 0.9 to 1.1, and thinner wings served as well as a thicker one would have. The region of deep shock stall, Mach 0.75 to 0.9, [the study of] which Stack advocated, proved relatively unimportant from the correlation standpoint." Becker, *The High-Speed Frontier*, pp. 97–98.

In 1965, at a history meeting of the American Institute of Aeronautics and Astronautics (AIAA), Stack acknowledged the correctness of Langley's thin-wing decision as if he had

At first meeting many people underrated Floyd L. Thompson (1898-1976). But Thompson knew how to get his people to do their best work. In the opinion of most Langley veterans, the better one got to know Thompson, the more one appreciated him.

Wing thickness seems to have been the only design criterion for the XS-1 about which any members of the Langley research staff seriously disagreed. Both Stack and Gilruth wanted Bell to design the airplane's horizontal tail using a thinner airfoil section than it used for the wings, for they knew that if the wing and the tail had the same section thickness, both surfaces would reach the critical Mach number at the same time. The simultaneous experience of high drag rise of the wing and other compressibility effects from the tail could easily cause the pilot to lose control of the plane and crash. Stack and Gilruth also insisted that Bell make the horizontal tail

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agreed with it at the time. "We knew it should have a thin wing," Stack told his audience. (Draft of Stack's statement at AIAA History Committee session, San Francisco, Calif., 28 July 1965, p. 6, in "John Stack, Special Collection," Langley Historical Archive.)

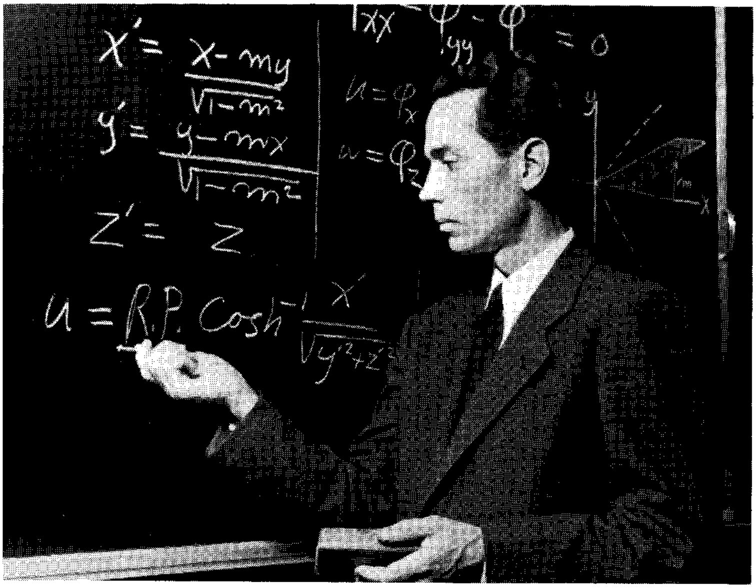
surface all-moving—that is, make the entire horizontal stabilizer adjustable by the pilot in flight. They realized that at subsonic speeds a pilot could ordinarily retain control of his aircraft, if a problem arose, by moving the elevator on a fixed horizontal stabilizer. At transonic speeds, however, they feared that this type of control probably would not be possible. At Langley's suggestion, the NACA also advised Bell to put the adjustable stabilizer high on the vertical fin of its airplane. This position, the laboratory staff had said, would keep the control surface safely above the wing wake.[14]

In early 1945, there was a virtual consensus at Langley—and at the Army Air Materiel Command—on one other basic design feature of the XS-1: no one wanted the NACA to advise Bell to design the transonic research airplane with anything but a conventional straight wing. This was true even though one of the lab's best aerodynamicists had explored a "new" theory suggesting that an aircraft could penetrate the sound barrier more easily if its wings were swept backwards.

Jones's Swept-Wing Concept

Robert T. Jones was an extraordinary aerodynamicist who made important contributions to NACA research without having completed a college education. As a boy in his hometown of Macon, Missouri, Jones read all the aviation magazines available on the local stand. His favorite was the journal *Aviation*, which carried technical articles by eminent aeronautical engineers and notices of forthcoming NACA technical reports. Jones ordered copies of many of the NACA reports from the Government Printing Office for ten cents each, and even received some free simply by writing NACA headquarters in Washington. He perplexed many of his high school English teachers by writing essays for them on aeronautical subjects.

Jones attended the University of Missouri for only two semesters before taking a job rigging wings on airplanes for a flying circus that gave aerial shows at county fairs across the Midwest. In 1929 he took a manufacturing job with the Nicholas-Beazley Airplane Company in Marshall, Missouri, helping to build its new Barling NB-3, a low-wing cantilever monoplane of metal construction (except for fabric covering). Then came the Depression, the collapse of the Nicholas-Beazley company, and a succession of different jobs, in various towns, broken up by periods of unemployment. In 1933 he got a job operating an elevator in a government building in Washington, D.C. At night he took classes in aeronautics at Catholic University taught by former Langley chief of aerodynamics Max M. Munk.



Robert T. Jones used the Lorentz transformation (i.e., a mathematical relation connecting the space and time coordinates of an event) to solve the critical problem of wing sweep in supersonic aerodynamics.

In 1934 the Public Works Administration opened up a number of temporary scientific positions in the federal government. On the recommendation of his hometown congressman, Jones secured a nine-month appointment at Langley laboratory. The NACA made him an "assistant scientific aide" and assigned him to the 7 × 10-Foot Wind Tunnel section, where he soon proved to have exceptional talents, particularly for addressing theoretical problems pertaining to airfoils and to aircraft stability and control. For the next two years Langley managed to keep Jones by arranging for a series of temporary and emergency reappointments. It could not promote him to even the lowest professional or engineering grade, however, because to rate that grade, civil service regulations said that an individual had to have a college degree. In 1936 the lab finally found a way to keep him permanently, and to pay him what he was worth: it gave him the next grade above the lowest professional grade—for which the academic requirement, though presumed, was not specifically mentioned. A few years later Jones became head of the stability analysis section.[15]

While John Stack worked to win the military services over to his idea of the transonic research airplane, Robert T. Jones was busily engaged in studying the aerodynamic configuration of guided missiles. By the end of

1944, Jones had finished designing an experimental air-to-air missile for the Army Air Forces (the JB-3 or NACA "Tiamat") and was in the midst of studying the potential of a proposed glide bomb having a low-aspect-ratio delta (triangular) planform.[16] This unconventional planform had been brought to Jones's attention in August 1944 during a meeting at Langley with Roger W. Griswold, president of Ludington-Griswold of Saybrook, Connecticut, a manufacturer of flying weapons. In 1942 Griswold's company had built a wind tunnel model of a dart-shaped missile conceived by Michael Gluhareff, a Russian émigré who was chief of design for the Vought-Sikorsky Aircraft Division of the United Aircraft Corporation; now, in 1944, Griswold was using the results of Vought-Sikorsky tunnel tests with the model to convince the AAF and the NACA that the new missile should be developed. At their Langley meeting, Griswold showed Jones data plots predicted for the Gluhareff model on the basis of Ludwig Prandtl's lifting-line theory, a mathematical theory involving a series of physical assumptions that made the problems of lift and drag accessible to analysis.[17]

Jones knew that Prandtl's 25-year-old theory of lift was applicable to bodies with high aspect ratio but that it did not work for bodies—like Gluhareff's dart-shaped missile—with low aspect ratio. Jones was intrigued by the prospect of the new missile, however, and, as soon as Griswold left Langley, he began to study its unconventional shape on the basis of a new theory of his own making. This theory, developed by Jones especially for the lifting characteristics of slender delta wings, resulted in formulas and analytical solutions that were very simple, and in some key respects similar to those derived for flow around airships in 1924 by Max Munk, his mentor at Catholic University, and to those derived for supersonic flow around projectiles and other slender bodies in 1938 by Hsue-Shen Tsien of Caltech's Guggenheim Aeronautical Laboratory. For the moment, though, Jones chose not to pursue publication of his theory. He thought the theory "so crude" that "nobody would be interested in it," especially since it was based on incompressible flow at very low subsonic speeds. He placed it in a drawer of his desk and temporarily forgot about it.[18]

One day early in 1945, while playing with the highly sophisticated mathematics of potential flows at supersonic speed,* it dawned on Jones that he was obtaining the same simple formulas with compressible flow equations as he had derived from his crude lifting theory for incompressible flow. He now recalled that Professor Tsien had reported finding that certain

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* In the theory of fluid mechanics, a *potential flow* is a type of fluid motion in which the rotation of the fluid element is zero (or irrotational). This type of flow is also called *vortex-free flow.* The term *potential* derives from the mathematical concept of the velocity potential. See Theodore von Kármán, *Aerodynamics*, pp. 36–39.

slender projectiles exhibited no influence of compressibility when revolving at high speed. Jones immediately got his old paper out of his desk drawer and incorporated the compressible flow equations into it. To his growing wonderment, he discovered that for very slender wings there seemed to be no compressibility effect, no effect of Mach number.

Jones sought a physical explanation for the total lack of compressibility effects on the theoretical performance of slender wings. After performing a series of complicated calculations, he recognized that the physical explanation was related to the effect of *sweepback* on the lift of large-span wings. This effect, Jones remembered, had been noted by Munk in 1924 in a paper published by the NACA dealing with the stability of wings. In this paper, Munk had stated that in level flight, only the component of velocity normal (that is, perpendicular) to the planform's leading edge was "effective for the creation of lift."[19] This statement by Munk—namely, that the air force on a wing depends on the normal component of velocity—was the first statement of the basic effect of sweepback made by anyone, and it was surely more than a coincidence that it was Jones, Munk's prize student, who now recalled it. Though Munk had made this statement for the purpose of comparing the relative effect of dihedral and sweepback on airplane stability in incompressible (low subsonic) flow—and thus not in connection with high Mach number effects—Jones now had good reason to suspect that Munk's principle could be incorporated meaningfully into his slender-wing theory. The result was a new theory that covered the entire sweep range from zero to 90 degrees, and was not limited just to very slender wings.

Jones guessed that his sweep theory would show that the effective Mach number would be much less than that of the flight Mach number even for moderately swept and thick wings. He did not realize how much less the effective Mach number could be until he tried sweeping the leading edge of a slender wing back behind the Mach cone, the idealized cone-shaped zone of disturbance that theoretically emanates from a body moving through the air (or any other fluid medium) at supersonic speed. The effective Mach number of the highly swept wing then appeared to be in the astonishing range of three to five times less than that of straight-wing planforms. The sweep smoothed out the sharply bending streamlines of supersonic flow that otherwise would have affected the wing adversely. This enabled a purely *subsonic* type of flow to exist on the wing's surface, a phenomenon which worked to eliminate the wave drag and compressibility shock of high-speed flight almost entirely. Jones now had a physical explanation for the missing compressibility effect shown by the mathematics of his theory.

At the time Jones did not know that Adolf Busemann, a German aerodynamicist who would come to work at Langley after World War II,

Adolf Busemann, the German aerodynamicist who first expressed the advantages of wing sweep in a 1935 theoretical paper, came to work at Langley in May 1947 as a result of Operation Paperclip.

had introduced the idea of sweeping wings to diminish the wave drag at supersonic speeds ten years earlier, in a paper he presented at the Volta Congress on High-Speed Aeronautics in 1935.[20] (Busemann had kept the wing ahead of the Mach cone, however, so that the cross-flow was still supersonic.) Jones's colleague and close friend, Eastman Jacobs, had attended the meeting in Italy but did not remember the "arrow-wing" concept—one of many highly theoretical ideas in Busemann's paper—as anything important. Neither did Theodore von Kármán or Hugh Dryden, the only other American representatives at the Volta meeting.[21]

Jones discussed his sweep concept first with Langley's other theoreticians, and with supersonics expert Arthur Kantrowitz in particular; then he brought it to the attention of his division chief, Hartley Soulé. In mid-February 1945 he outlined his concept for Jean Roché, civilian liaison officer at Langley for the Air Materiel Command, and described it for Ezra Kotcher. (At the time Jones was working with Kotcher to help the army copy the German V-1 missile.) In his conversations with both Roché and Kotcher, Jones tried to make clear his belief that sweep benefits were progressive—that is, that the adverse effects of compressibility were reduced as the sweep angle of the wing increased—and that these benefits were not limited to the very slender wings of his original theory. He advised the army engineers that wings designed for flight at supersonic speeds should be swept back to an angle that would assure that the component of velocity normal to the wing's leading edge was less than the critical speed of the airfoil sections. On 5 March 1945, he sent a memo to Gus Crowley, Langley's chief of research, announcing that he had

> recently made a theoretical analysis which indicates that a V-shaped wing traveling point foremost would be less affected by compressibility than other planforms. In fact, if the angle of the V is kept small relative to the Mach angle, the lift and center of pressure remain the same at speeds both above and below the speed of sound.

Jones asked Crowley to approve tests of experimental wing shapes "designed to minimize compressibility effects."[22]

Jones's articulation of his theory was still in raw form, however; he would not finish a formal report on his theory until late April.[23] Then the report ran into trouble in Langley's in-house editorial committee. Theodore Theodorsen, head of the Physical Research Division, chaired this committee. Theodorsen had serious reservations about the publication of Jones's paper; he felt that parts of the presentation were too intuitive and asked that Jones clarify the "hocus-pocus" with some "real mathematics." More importantly, Theodorsen was sure that supersonic flow was so completely different in nature than subsonic flow that it was most unlikely to be accompanied by the subsonic flow that Jones predicted on a wing traveling at supersonic speeds. He called Jones's insight into the potential of swept wings "a snare and a delusion."[24] At the end of his committee's deliberations, Theodorsen insisted that Jones take the part about sweep theory out of his paper.[25]

NACA management supported the judgment of Theodorsen and his editorial committee and withheld publication of Jones's report until the sweep theory was confirmed experimentally.[26] This confirmation did not take very long. Even before Jones had finished the first draft of his controversial report, Robert Gilruth's flight research section had started a series of wing-flow and drop-body tests to verify the favorable effects of sweepback on wing drag predicted by Jones. By the end of May 1945, results from these free-flight tests validated the swept-wing concept in convincing fashion: they showed a reduction of wing drag by a factor of almost four.[27] Shortly thereafter, Macon C. Ellis and Clinton Brown verified this dramatic reduction of drag by testing a section of wire at a large angle of sweep in Langley's model supersonic tunnel.[28]

Engineer James N. Mueller tests models of various swept and delta wings in the 9-Inch Supersonic Tunnel, October 1946.

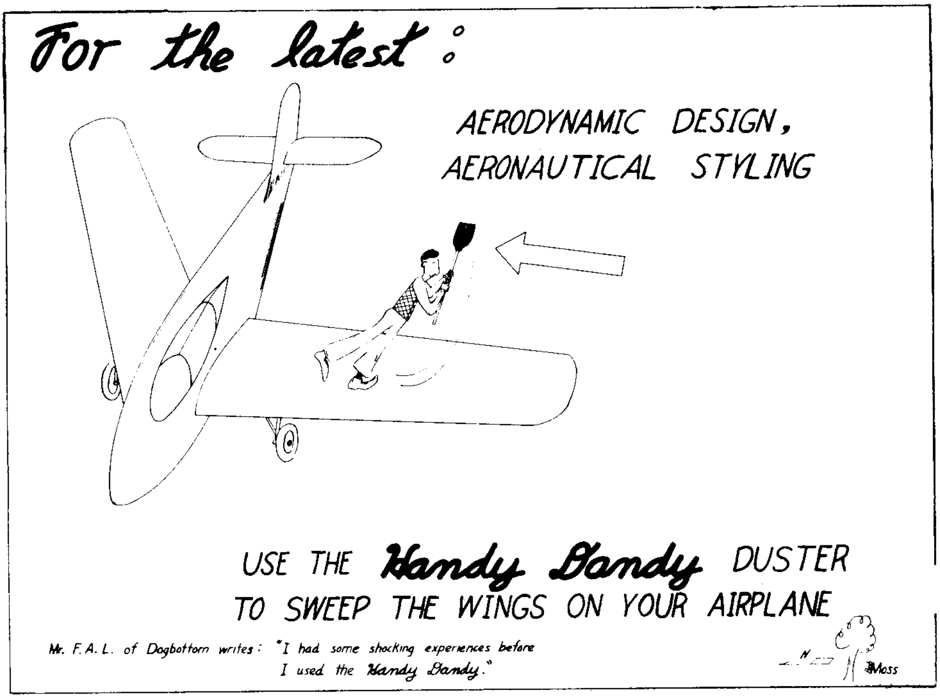
In early June, Langley transmitted Jones's report to NACA headquarters for publication. In the transmittal letter, engineer-in-charge Reid stated that "Dr. Theodore Theodorsen [still] does not agree with the arguments presented and the conclusions reached and accordingly declined to participate in editing the paper."[29] On 21 June, the NACA issued Jones's report, "Wing Plan Forms For High-Speed Flight," as a Confidential Memorandum Report (CMR L5F21), part of a series the Committee's executive officers prepared chiefly for the information of the army or navy. Before the paper was published, Jones's colleague at Langley, Robert Hess, found an overlooked copy of Busemann's earlier paper (a British translation dated April 1942) in the LMAL library, and Jones included a reference to it.[30] Three weeks later, the NACA reissued Jones's paper as an Advance Confidential Report (ACR L5G07), a type of publication the NACA sent not only to both services and to its own subcommittee members but also by registered mail to those members of the aircraft industry who had signed secrecy agreements with the services and who had a "need to know."[31]

Verification of sweep theory and publication of Jones's report came too late for the XS-1. On 10 March 1945, the Army Air Forces had notified the NACA that it was awarding Bell a contract to develop the rocket-powered research airplane with straight wings. By this time, all three parties involved had known something about Jones's theory that sweeping a wing would probably alleviate compressibility shock and generally improve performance, but they would not have changed their minds about the design in any case. There was no proven reason for them to recommend changing from conventional straight-wing planforms to swept wings, an almost completely unknown quantity. Flight test research with full-scale aircraft had to proceed cautiously and conservatively. They were doing enough bold things with the XS-1 as it was. Five days later, the Materiel Command at Wright Field had held the first design review of the XS-1. No one seems to have made any mention of sweep theory.[32]

Frustrations

Because John Stack was in Europe at the time, Langley had sent Stack's top assistant John V. Becker, who was head of the 16-Foot High-Speed Tunnel section, to the XS-1 design review as its representative. At Wright Field Becker found that Bell had accepted all of the NACA's ideas for the design of the airplane except for Stack's longstanding recommendation for a more conservative power plant (turbojet) and speed range (Mach 0.8 to 1.0). Because Bell seemed to be planning for the XS-1 to take off from the ground rather than to be launched from the air, Becker reported that the proposed design was acceptable to the NACA. In climbing by itself up to the Mach 1.2 supersonic cruising speed that the army specified, through the Mach 0.8 to 1.0 speed region the NACA most wanted to know about, such an aircraft would provide realistic data on a full range of flight considerations.[33]

Two months after the design review at Wright Field, however, Bell opted to change the research airplane to air launch: a specially configured B-29 would carry the XS-1 to an altitude of 30,000 feet and then release it for flight. Though there was disagreement among Bell engineers over the wisdom of this decision, the company made the change because, after technical deliberations, it saw no way for the airplane to achieve the supersonic speed required by the army if it had to take off and climb from the ground.[34] The rocket engine would simply consume too much of the precious fuel allotment. This was true even though two-thirds of the gross weight of the airplane was to be in fuel, which would have been an extraordinarily high proportion for any nonrocket military aircraft.[35]



From the Langley Air Scoop, *18 July 1947.*

This change from ground takeoff to air launch further dampened Langley's enthusiasm for the XS-1. According to the lab's experts, air launching was a cumbersome method and the second major violation of the NACA's basic notion that a research airplane should operate as conventionally as possible (the first violation having been the use of rocket propulsion). Moreover, air launching also meant that in all probability the little rocket plane would never be operated out of Langley, a busy flying field close to highly populated areas—if the XS-1 came loose accidentally, without a pilot, from the B-29 in flight, the resulting crash could kill many people. At another field, the NACA would not be able to manage the program of flight tests for the XS-1 as directly as it wanted.[36]

Langley objected to the evolution of Bell's XS-1 from another standpoint besides the launch mode. Because Bell believed that the unavailability of the complex new rocket fuel pumps (then being developed by Reaction Motors of Pompton Lakes, New Jersey) called for by the original design would probably hold up flight tests of the transonic airplane, it decided in April 1945 to redesign the first XS-1 with pressurized fuel tanks of some simpler type already existing. Though the company's design team realized that use of pressurized tanks instead of the new pumping units would reduce the duration of the airplane's maximum thrust by approximately 3 minutes, from 5.4 to approximately 2.6 minutes, and thus force a reduction in the plane's cruising altitude, it judged that "it would be better to have an airplane which would enable preliminary flights to be made at a reduced altitude, rather than to have an airplane on the ground awaiting a pumping unit."[37]

John Stack reacted strongly when he heard about Bell's revised plans. In a memorandum to Langley's chief of research, he warned that the transonic airplane under development "may prove quite unsuitable." Stack noted that everyone had agreed at the initiation of the project that five items were the basic requirements of the research airplane:

> a. speed greatly in excess of the critical
> b. duration at full power for complete observations in level flight at steady conditions
> c. take-off, flight, and landing with self-contained power units
> d. flexibility to permit changing of all principal components such as wings, tail surfaces, canopies, etc.
> e. space for adequate instrumentation

These requirements had since been sacrificed to the point where the project was now

> falling short of basic requirements *b*, *c*, and probably *e*. As a consequence of the failure of this project to fulfill basic requirement *b*, it will also fall short on basic requirement *a*. This is so because the fuel supply is adequate only to get the airplane to 35,000 feet, leaving no fuel for the test run. While it is true that the airplane can be flown at lower altitudes, it is only at the high altitudes approaching 35,000 feet that the airplane meets basic requirement *a*.

Although he agreed completely with Bell's view that it was best to get an airplane flying as quickly as possible, Stack wanted the NACA to remind everyone that the "basic purpose of all of this work," as he had originally conceived it, was to obtain in actual flight compressibility data that could not be acquired in wind tunnels in certain speed regions. Bell could not let a little thing like the present unavailability of the correct rocket fuel pump destroy the basic purpose of the entire project. Stack recommended that "a much larger effort be devoted to the development of this pump, an effort that is as large as the project demands." He urged that the army be asked to call in engineering organizations other than Reaction Motors to help develop the pump, if necessary.[38]

For the rest of 1945 Langley did whatever the army or its contractor asked it to do to help complete the design of the XS-1 and ready it for test flights. It did these jobs well, as was expected of the NACA, even though the XS-1 was far from the research airplane that it wanted. The laboratory oversaw the design and preparations for installing the XS-1's instrument panel. By the end of the year, wind tunnel tests provided data reliable enough for the lab to predict the rocket plane's flying characteristics up to about Mach 0.90 for the low-angle-of-attack conditions which were of most significance for the XS-1 and D-558 flights. Wing-flow data took these NACA predictions up to about Mach 0.93.[39]

Army criticism of the NACA for not releasing Jones's sweepback theory earlier made giving this assistance a somewhat unpleasant task. In May 1945 a special intelligence unit of the U.S. Navy had discovered among the countless abandoned documents of the aerodynamical laboratory at Göttingen solid evidence that the Germans had been aggressively studying for some time the advantages of sweepback in designs of their jet-propelled aircraft.[40] The army heard news of this startling finding at least as soon as did the NACA.[41] Some of its leaders thought that here was another example, like the turbojet revolution, of the NACA failing to keep the United States on a par with Europe in aeronautical development.

In October 1945 Brig. Gen. Alden R. Crawford, chief of the Production Division of the AAF, asked Jerome Hunsaker, the NACA chairman, why there had been no mention of Jones's theory during the first XS-1 design review at Wright Field the preceding March or during follow-up visits of Air Materiel Command personnel to Langley later that spring. Applying 20/20 hindsight, Crawford indicated that the NACA might have announced its vital new information in time to change the design of the XS-1 from straight to swept wings. Because such a change at this time "must delay the project and increase the cost to the Government," Crawford lamented, now the only thing the Air Forces could do was contract with Bell for the development of new XS airplanes with swept wings (which it did in December 1945).

The NACA knew that its organization could not justly be held responsible for the XS-1's conventional wing planform; after all, the Materiel Command had made the decision for straight wings, not the NACA. Moreover, R. T. Jones *had* described his theory for both Jean Roché and Ezra Kotcher by the time of the first design review in March. Floyd Thompson (the LMAL assistant research chief who had arbitrated the original Stack-Gilruth difference over XS-1 wing thickness) prepared for Hunsaker a polite but taciturn answer to General Crawford's letter. To have recommended changing the XS-1 in March 1945 from straight to swept wings could have been a "blunder of the greatest magnitude," Thompson wrote. "Not only was experimental evidence lacking [especially about the low-speed characteristics of the swept wing] but our best theoretical minds were divided as to the validity of the theory."[42]

The Douglas D-558

Douglas proposed to build for the navy six D-558 transonic research airplanes initially. Each aircraft would be powered by a General Electric TG-180 turbojet engine and equipped with alternative wing and nose duct configurations; maximum speed would be about Mach 0.90. In phase two of the program, Douglas would change two of the planes to Westinghouse 24C turbojets plus supplementary rocket propulsion units. These modified aircraft would gather aerodynamic data from Mach 0.89 to about Mach 1. In phase three, Douglas would use results acquired during phases one and two to construct a combat version of the D-558. Douglas estimated the total cost of the three-phase program to be just less than $7 million.[43]

This proposal was not what Douglas originally had in mind. In February 1945 company representatives had submitted a proposal to the navy for just one airplane. This airplane was to be designed around an available turbojet unit capable of delivering, with the help of supplementary rocket propulsion units, a maximum thrust of 3000 pounds for 40 seconds. It would reach Mach 0.9 in level flight and Mach 1.0 after a 25-degree dive from 35,000 feet.[44] With only a few minor modifications, this airplane was to be adaptable as a navy fighter. Its development could thus lead to volume production and considerable profits to the contractor. In most essentials, it was the same plane Douglas later proposed for development during phase one.

The NACA had objected to Douglas's original proposal for this research airplane in very strong terms. Its spokesmen argued that a true research airplane should not be compromised for military or volume production requirements. In meetings with navy officials, they called Douglas's idea for a research airplane "wholly inadequate," "a half-way measure" that would result in an airplane which would "be obsolete by the time it was built." Milton Davidson, John Stack's colleague on special assignment to BuAer's airplane design research branch, reported to his superiors that he had fully outlined the NACA transonic research airplane specifications during meetings with Douglas representatives in early February. What the NACA desired, Davidson said he had explained, was an airplane that would "take off, climb to operational altitude [20,000-foot minimum, 35,000-foot maximum], operate for 10 minutes at a velocity near the critical speed at altitude, have a 2-minute burst at maximum thrust, and return to the airport with power." The airplane Douglas suggested building would be deficient in duration and amount of power and fuel to meet these requirements. Davidson also indicated that he had made clear to the representatives that the plane had to have adequate space for a sizable package of NACA recording instruments; the airplane Douglas proposed did not have enough such space. On many occasions since the first round of meetings at BuAer, he had gone over their preliminary engineering sketches of high-speed research airplanes, enumerating the changes necessary to make the D-558 satisfactory. Apparently Douglas had chosen to ignore NACA advice, Davidson concluded.[45]

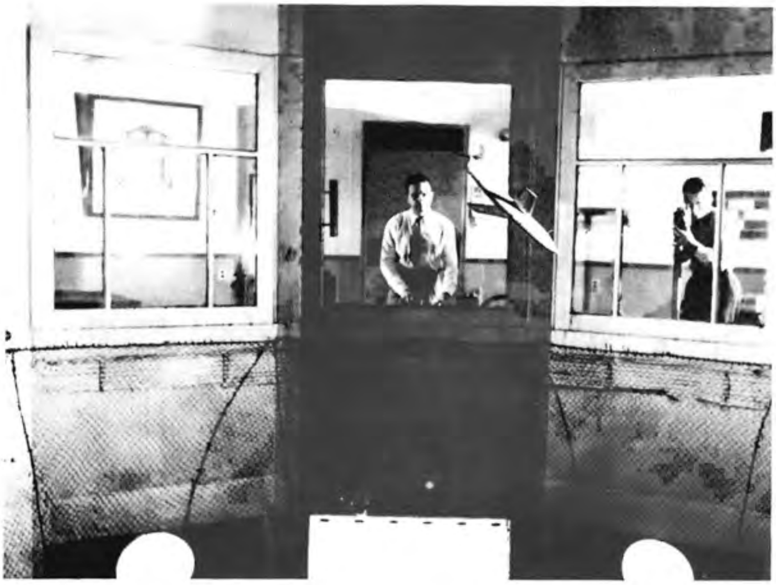
The navy had supported the NACA's objections to the original Douglas proposal. Captain Diehl told Douglas representatives at a meeting at BuAer on 23 February that he thought "the NACA had spent a good deal of time studying the problem, and since the NACA was in the best position to know what was wanted in a research airplane," Douglas's airplane proposal should "measure up to NACA specifications." Four days later, Comdr. Emerson W. Conlon, head of BuAer's structures department, opened another meeting with Douglas representatives by stating that the NACA would have to "heartily approve of any airplane" before its procurement by the navy.[46] This double-barreled NACA-navy criticism quickly led Douglas to the decision to commit itself to a new design proposal, the three-phase plan that the company ultimately submitted in April.

BuAer quickly approved Douglas's preliminary designs for the phase one and phase two aircraft and outlined a development program that guaranteed, among other things, the NACA's role in the management of the flight tests and immediate access to at least one of the airplanes: Douglas test pilots would fly the D-558-1 to acquire data applicable to the design of combat aircraft, and NACA test pilots would fly it to gather fundamental aerodynamic information about air loads, stability and control, flutter, and engine performance at high Mach numbers.[47] Although John Stack, in particular, had some serious reservations about the adequacy of Douglas's phase one aircraft, especially in comparison with the proposed Bell XS-1 and the German ME 163, he soon became satisfied that the phase two program would result in the transonic research airplane he wanted.[48]

Douglas held the first mock-up inspection of the D-558 at its main office in El Segundo, California, from 2 to 4 July 1945. As its representatives, the NACA sent Stack, Thompson, and Gough from Langley, as well as Milton Ames, a technical assistant assigned to NACA headquarters, and H. Julian "Harvey" Allen from Ames laboratory. The five NACA representatives made all of the various sources of their dissatisfaction with the Douglas design known during the first day of the inspection. Among other things, they suggested that Douglas needed to increase the size of the space allotted for NACA recording instruments, generally enlarge the fuselage, change the design of the cockpit canopy and the side inlets, and improve the ducting of the nose inlet. Douglas concurred immediately. Following the ideas agreed upon by everyone during the technical meetings of the first day, the company prepared new drawings of the mock-up as modified. During the last day of the inspection, the NACA delegation got Douglas and navy spokesmen to agree to support the NACA's development of an afterburner unit at its engine research lab in Cleveland. The application of this afterburner, they argued, could probably provide the phase one research airplane with enough additional thrust to permit flight "at extremely great supercritical speeds." Langley and NACA headquarters representatives flew home to the east coast satisfied that they had finally gotten Douglas to commit itself to making the extensive changes that were necessary to make the D-558 into an adequate transonic research airplane.[49] At a second D-558 mock-up inspection held 14 to 17 August 1945, NACA representatives found that Douglas had indeed made the canopy and inlet changes in accordance with the requirements they had outlined at the meeting five weeks earlier.[50]

Even before the first mock-up inspection in early July, John Stack had talked to Captain Diehl about Langley's experimental confirmation of R. T. Jones's sweep theory. The head of Langley's Compressibility Research Division thought it might be wise, considering recent developments, for the navy to ask Douglas to incorporate a 35-degree swept wing on one of its D-558s. Both Stack and Diehl realized that swept wings for the phase one airplane made no sense; it could not be powered by the proposed power plant to a high enough Mach number for the performance of swept wings to be fully evaluated. They also knew that the navy would want Douglas to proceed cautiously, with straight-wing configurations, until there was absolutely no doubt in the minds of the experts that sweep was the best way to go when designing an airplane wing for high-speed flight. They agreed, however, that wind tunnel evaluation of swept wings should be included immediately in the D-558 program for possible incorporation into the design of the phase two aircraft. Soon after the first inspection at El Segundo, the Langley High-Speed Panel, which Stack chaired, asked NACA headquarters to arrange for permission to incorporate swept wings on the model of the D-558 that was to be tested in the lab's 8-Foot High-Speed Tunnel. This request was supported at a joint army-navy-NACA research meeting at the NACA's Washington office on 13 July 1945.[51]

In early August couriers arrived at the Bureau of Aeronautics in Washington and at the Douglas company in El Segundo with microfilm of the captured German swept-wing reports. BuAer shared and analyzed



Models of the D-558 were tested in the 8-Foot High-Speed Tunnel (top) in June 1947 and in the 20-Foot Spin Tunnel (bottom) five months later.

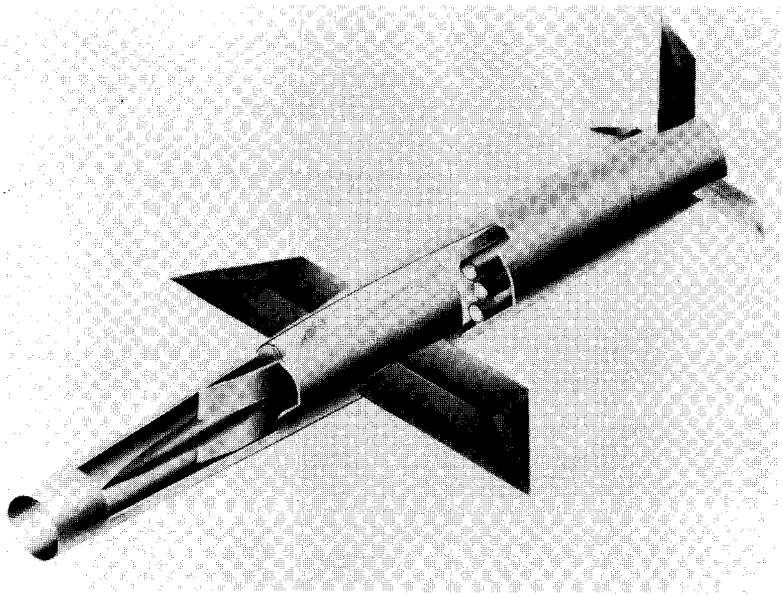
the new information with the NACA almost immediately. Their common evaluation of this microfilm led to a joint request at the second mock-up inspection for Douglas to initiate a study for the design of a D-558 with swept wings. The two men responsible for capturing and microfilming the papers at Göttingen for the navy (L. Eugene Root and A. M. O. Smith) were Douglas employees. The company embraced the navy-NACA request for a design study of a swept-wing D-558 with both turbojet and rocket power for development during phase two and gave the NACA the job of specifying many of the design requirements, including complete responsibility for high-speed wind tunnel testing.[52]

While Langley tried to use its influence to get the navy and its contractor to accelerate the development of the swept-wing phase two aircraft, the laboratory staff continued to aid development of the straight-wing D-558-1 in every way it could. Stack encouraged the dozens of engineers, scientists, technicians, and mechanics involved in carrying out Langley's comprehensive high-speed research program to extend themselves in every way to meet the needs of the D-558 project quickly and successfully.[53] This group included the staff of his own 8-Foot HST section, who were kept busy testing the aerodynamic characteristics of scale models of the D-558 configuration. It also included many personnel of the Flight Research Division, who were using the wing-flow method to test D-558 models mounted on the wings of a P-51 Mustang, and most of the personnel of the Spin Tunnel section, who had modified a scale model of the Bell XS-1 to simulate the spin behavior of the D-558.

Feasibility of a Supersonic Ramjet

Throughout the early development of the Bell XS-1 and the Douglas D-558, Langley engineers displayed their long-term preference for air-breathing propulsion units over rockets. This preference can also be seen in a project designed by a team of engineers in the lab's 9-Inch Supersonic Tunnel section to study the feasibility of powering a small airplane to Mach 1.4 with a ramjet engine.

By the spring of 1944 the Campini jet propulsion system had disappeared from Langley's list of research interests, even though the system's champion, Eastman Jacobs, seems never to have formally acknowledged that it was unworthy of additional consideration. During the summer, Jacobs moved to the Aircraft Engine Research Laboratory in Cleveland after the NACA dissolved the Air-Flow Research Division and made him, its former chief, a "consulting engineer."[54] (He would remain at AERL for only a short time before retiring from government service to do independent con-



The supersonic ramjet conceived by Ellis and Brown died in a Langley committee in late 1945.

sulting work in California.) Before leaving Langley, however, Jacobs had encouraged the two-man staff of the 9-Inch Supersonic Tunnel section—Clinton Brown and Macon C. Ellis, Jr.—to investigate the potential of a new ramjet propulsion unit. Another simple type of jet engine, this unit consisted of a specially shaped tube or duct open at both ends. It required no mechanical compressor. The forward motion of the engine shoved all the air necessary for combustion into the duct and compressed it. In the engine, the compressed air passed through a specially designed chamber, or diffuser, and mixed with fuel; together the fuel and air burned rapidly. Exhaust gases then issued as a propulsive jet from a rear opening.[55]

In December 1945 Ellis and Brown finished a report which showed to the NACA's satisfaction that a small ramjet research aircraft was feasible. Accelerating through the transonic region would require rocket boosters, but once the airplane flew to the speed of sound the ramjet could take over and power it for a short distance (about 60 miles) at a supersonic speed of Mach 1.4. Ellis and Brown envisioned either airplane tow to altitude or air launch as the ramjet's takeoff mode—a plan not without a certain irony, given Langley's opposition earlier in the same year to the plan to air launch the XS-1, and given Langley's overall commitment to developing research airplanes that could operate as conventionally as possible.[56]

Langley managers, at John Stack's instigation, briefly considered advising one or both of the military services to add the ramjet aircraft to the fleet of transonic research airplanes under development, but there was no ramjet engine under development at the time: the engine Ellis and Brown had assessed was hypothetical. This meant that the ramjet proposal "had virtually no chance of support" outside the NACA, especially with the designs of the XS-1 and D-558 now well under way. Stack, still strongly committed to the idea of operating a research airplane as conventionally as possible and apparently satisfied with the direction of the D-558 program, let the supersonic ramjet aircraft proposal die at home in conference.[57]

Flight Tests of the XS-1

Bell completed construction of the first XS-1, without the rocket motor, in December 1945—the month of the Ellis-Brown ramjet proposal. Under terms of its contract with the Army Air Forces, the company now had to test the XS-1 to the speed of Mach 0.8 before official acceptance. The AAF and the NACA had determined even before the delay in completing the rocket motor that Bell pilots should first fly the new airplane through a series of glide tests. These glide tests would identify quirks in the air launch method and address the feasibility of operating the rocket plane from conventional flying fields (like Langley) near population centers. In November 1945 the army had selected isolated Pinecastle Field in central Florida as the site of the glide tests. It was the NACA's understanding that these preliminary flight tests scheduled for Pinecastle with the unpowered airplane were to determine the feasibility and safety of operation from Langley Field.[58]

The NACA sent two Langley engineers, Walter C. Williams and Gerald M. Truszynski, to Pinecastle to join Bell test pilot Jack Woolams and the B-29 launch crew for glide tests of the XS-1. Williams, a 1939 graduate in aeronautical engineering from Louisiana State University, had worked with Stack as early as 1942 on research aircraft studies. Recently, as a member of the flight research section, he had been responsible for advising NACA pilots about how far to push the P-51 in dive tests for transonic wing-flow data. At Pinecastle Williams monitored flight test preparations and supervised on-the-spot analysis of the resulting glide path data. Truszynski, a 1944 graduate in electrical engineering from Rutgers, had been designing radar and telemetry equipment in Langley's Instrument Research Division. At Pinecastle he took charge of the radar tracking equipment.[59]

The XS-1 glide test program at Pinecastle lasted about three months, from January through March 1946, while Bell readied the second XS-1 for powered-flight trials. The aircraft showed itself to be aerodynamically sound, with good low-speed handling qualities, and the air launch method proved practicable, but problems landing the XS-1 safely at Pinecastle demonstrated the inadequacies of a conventional airfield for operating the plane.[60] These last two test results erased the NACA's vestige of hope that some method of ground launching would be found for the XS-1, making it possible for the aircraft to fly from Langley Field.

During the final days of glide testing at Pinecastle, the AAF chose its flight base at Muroc Dry Lake in southern California as the site where the powered tests of the XS-1 would be made. In the opinion of the army, Muroc Dry Lake was the best possible location for several reasons: a flight test base was already there, complete with facilities and a contingent of military personnel; America's first turbojet aircraft, the Bell XP-59A, had flown for the first time at Muroc; the weather was usually excellent; an enormous stretch of desert and dry lake provided more than adequate space for emergency landings; and the remoteness of the site removed the worry and danger of overflying and crashing into populated areas.[61]

The NACA endorsed the AAF choice for the XS-1 test site and, in late September 1946, detailed a group of 13 engineers, instrument specialists, and technical observers from Langley laboratory to Muroc on temporary assignment.[62] Hartley Soulé, chief of the Stability Research Division and project manager for the research aircraft program at Langley, designated this group the "NACA Muroc Flight Test Unit" with Walter Williams, veteran project engineer for the XS-1 glide tests at Pinecastle, unit leader. Williams, who reported to Soulé, was authorized by Langley's engineer-in-charge "to make all necessary contacts and decisions for the NACA ... at Muroc."[63] The assignment given to this special unit was to supervise the complete instrumentation of the second XS-1, gather and analyze all possible data during the period of its powered test flights, and more generally to try to make sure that NACA research interests were considered in planning and carrying out the in-flight program.

From the beginning, different people had different purposes in mind for the XS-1. Stack wanted the aircraft to collect as systematically as possible the detailed transonic data unobtainable *in the wind tunnel*, whereas Gilruth and Thompson, more in line with the thinking of the AAF, wanted to design a good high-speed aircraft and to get that aircraft to fly supersonically as quickly as possible so that it could serve as a prototype of an operational supersonic aircraft. Both the AAF and the NACA had recognized early in the XS-1 development period that these purposes, and the methods for achieving them, were contrasting and in certain ways even contradictory, but they had agreed to coordinate their plans so that the research aircraft could be built and flown for their mutual purposes.

Veteran flight researcher Hartley Soulé managed the NACA Muroc Flight Test Unit from his office at Langley. (The model in this photograph was used in tuft survey research. By observing the reaction of the little pieces of cloth, or tufts, attached in various places on the wing, a researcher could tell whether the flow over the surface was smooth or disturbed.)

When Langley detailed the special flight test unit to Muroc in September 1946, it was seriously concerned about Bell's intention to make the acceptance tests of the XS-1 airplane in as short a time as possible. Though the lab recognized that Bell's test program lived up to the legal requirements of the army-Bell contract for the XS-1, it worried that the flight tests required of the company would cover only demonstration of the "limiting conditions." "The mere flying of the airplane to a Mach number of 0.8 and making an 8g pull-out is not considered suitable preparation for the research flying," Langley emphasized. The program its staff had outlined for the acceptance tests of the XS-1 included "systematic exploration of the stability and control characteristics and structural loading at successively higher speeds up to a Mach number of 0.8." The lab had based its program on the understanding that "before asking anyone to proceed with the extremely hazardous flying above a Mach number of 0.8 everything would be done to make certain that the airplane was satisfactory in all aspects in the speed range up to Mach 0.8." The acceptance-test program was thus the NACA's means of assuring itself that the airplane's subcritical characteristics were satisfactory.

Since the likely level of such assurance seemed too low, Langley informed NACA headquarters that it did not want its pilots to undertake research flying in the XS-1 following the limited acceptance tests at Muroc proposed by Bell. It recommended that the army shift part of the flight test program originally included in the acceptance phase of the contractor program to the NACA research program phase. That way Bell could receive its payment for the airplane as quickly as it wanted without lowering the safety and overall value of the research airplane program.[64]

Bell began flying the XS-1 number two at Muroc on 11 October 1946. Two months later, on 9 December, Bell test pilot Chalmers H. "Slick" Goodlin flew the airplane with its rocket power successfully engaged for the first time. (The company had selected Goodlin to fly the plane in September after Jack Woolams was killed in the crash of a P-39 Airacobra that had been modified to compete in air races.) On 8 January 1947, during a buffet-boundary investigation, Goodlin reached Mach 0.8 at 35,000 feet, the speed and corresponding altitude required by the contract before the AAF would accept delivery of the aircraft. Three months later Bell began flying the XS-1 number one, which had been out of action since its last glide flight at Pinecastle in March 1946 in order to have a rocket engine installed. In mid-May Bell successfully put number two through a required 8g pullout and final airspeed calibration flight. After a total of 21 powered flights (14 by number two and 7 by number one), the contractor program was complete; both the AAF and the NACA were satisfied that the experimental airplanes were airworthy. Now the XS-1s belonged to the military. It was up to AAF flight engineers and test pilots to "break the sound barrier" and to do it in as few flights as possible.[65]

Concurrently with the beginning of the AAF's accelerated transonic flight program, the NACA got ready to conduct its own series of flight tests with the XS-1. The AAF had agreed informally early in the development program to lend the NACA a finished XS-1 for a separate series of flight tests. According to the terms of the agreement—which was completed at an NACA-AAF conference at Wright Field on 30 June 1947—the NACA would use XS-1 number two. It would furnish fuel, maintenance, and a flight crew for the experimental airplane, while the army would furnish the same for the launch B-29.[66] In March 1947 the NACA Muroc Flight Test Unit had prepared a more complex instrument package for installation in number two, as was necessary for making a thorough examination of the airplane's flying characteristics and loads. In late May, after the last flight in Bell's

The Bell XS-1 in flight over Muroc, California, 1947.

contractor program, Walter Williams wrote Melvin Gough at Langley that "we want to fly it [what Gough two years earlier had called 'that damned firecracker that no NACA pilot will ever be permitted to fly'] at the earliest possible date because everyone is quite anxious to get going."[67]

Williams's special unit could not fly the XS-1, however, until the NACA received the number two plane and got it ready—and for a time in the long hot desert summer of 1947 it seemed that neither thing would happen very soon. In early June, the airplane the NACA was going to get was damaged seriously in a freak on-the-ground accident and had to be ferried back via B-29 to Bell's hangar in Buffalo, New York, for repairs. When number two returned to Muroc in July, progress on it was slow because of the intense level of activity on number one. The preparation of the army plane required so many of the mechanical crew that there were usually none left for the NACA plane.

Gough's prediction of 1945 was coming back to haunt NACA personnel. In August 1947, while World War II combat ace Capt. Charles E. "Chuck" Yeager took up number one for more glide and the first accelerated power flights, the two NACA pilots on the scene—Herbert Hoover from

Langley and Howard Lilly from the Aircraft Engine Research Laboratory in Cleveland—had to be satisfied with taking number two through a series of ground runs. Through the first three weeks of September Walter Williams tried "stalling the Army off as much as possible until [the NACA could] get the NACA tests underway."[68] When the NACA airplane was finally ready for its acceptance test on 25 September, the NACA pilots were not. Since NACA management had thought it imprudent for its pilots to take up number one on pilot familiarization flights—and thus risk doing any damage to it—neither Hoover nor Lilly had gotten checked out in the XS-1. Thus the task of flying number two through its NACA acceptance test fell to Captain Yeager.

Supersonic Flight

In a preflight planning session on the morning of 14 October 1947, the NACA advised Yeager to take the rocket plane on its ninth powered flight to a maximum speed of Mach 0.97. Walter Williams and De Elroy Beeler emphasized for Yeager's sake that it would be unwise to go any faster until a complete examination of the data obtained from the previous flights was completed. They warned him to exceed Mach 0.97 only if absolutely certain that it was safe to do so.[69] Yeager ordinarily did not like NACA "eggheads" trying to "dictate" the planned speed of his flight—he recalls attending "highly technical NACA preflight planning sessions and postflight briefings" and not knowing "what in the hell" Walter Williams was talking about. After NACA briefings Yeager usually sat down with fellow army pilot Jack Ridley to "decide whether or not we wanted to stick with [the NACA] recommendation." Invariably they determined to fly faster than the NACA engineers wanted them to. (Yeager has written that the NACA was "so conservative that it would've taken [him] six months to get to the barrier" if he had followed the NACA's instructions exactly.) The way he felt that morning, though, hurting from a broken rib suffered two days earlier in a fall from horseback, a speed of Mach 0.97 was at that moment all he thought he would care to handle.[70]

At about 10:00 A.M. Yeager got into the launch B-29, the rocket plane shackled in its bomb bay, for the approximately 20-minute climb to altitude. At 5000 feet Yeager climbed down the transfer ladder into the tiny cockpit of the XS-1. At approximately 20,000 feet, NACA radar cleared the B-29 to let loose the XS-1. Sixty seconds later, at 10:26 A.M., Yeager's plane dropped free. What followed was the first manned supersonic flight in history.

Though Langley laboratory got word of Yeager's achievement immediately, it did not find out the details of the sensational flight until it received a letter from Walter Williams more than a week later. Williams described the flight in measured, technical language as part of his regular bimonthly report to Gus Crowley, Langley's chief of research:

> In flight 9, the pilot started a four-cylinder climb at 20,000 feet; as he approached 35,000 feet, he shut down two cylinders. The climb continued to 42,000 feet. As the altitude and Mach number increased, the pilot moved the stabilizer at Mach numbers of 0.83, 0.84, 0.88, and 0.92. At the top of the climb, the pilot turned on a third cylinder and pushed the nose down a little; a rate of descent of about 500 feet was noted. The airplane then accelerated to a Mach number of 0.98.
>
> At this Mach number, the needle of the Mach meter took an abrupt jump past $M = 1.0$ and went against the peg, which is a distance equal to about 0.05 in Mach number past 1.0. The pilot reported that the elevator seemed more effective at this speed than at $M = 0.94$ to 0.95. Aileron control appeared good throughout the speed range. The pilot reported no buffeting beyond an indicated Mach number of 0.92. He did report that the right wing dropped between an indicated Mach number of 0.88 and 0.90, as in previous flights.
>
> When the Mach number went off the scale, the pilot shut down all cylinders and jettisoned fuel in a climb. At 45,000 feet, an unaccelerated stall was made which appeared normal to the pilot. The descent from 45,000 to 35,000 feet was made at a Mach number of 0.7 so that a pressure altitude survey could be made.
>
> Preliminary NACA data work-up indicates that a Mach number of 1.06 was reached, taking in account the calibrated error in static pressure and assuming no error in total-head. Evaluation of all data from these flights is in progress and preliminary data will be issued.[71]

There was nothing in the tone of Williams's letter to suggest the fears and inhibitions that had been blocking the work of aeronautical researchers and aircraft designers since 1935 when Hilton inadvertently coined the term, and the concept, of the "sound barrier." Williams made not even an oblique reference to the concept of the "sound barrier" in his letter. In the public mind, however, news of Yeager's flight—once it was finally announced some weeks later—meant only that the awesome sonic barrier had finally and miraculously been pierced.*

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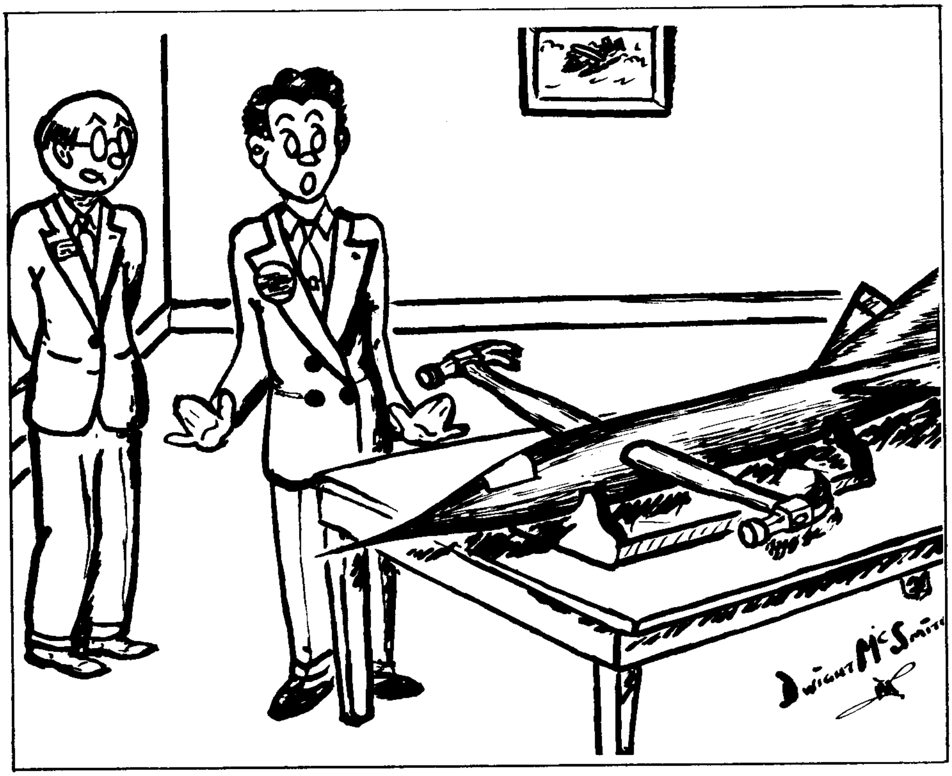
* It is illuminating to compare Williams's dry technical report with General Yeager's colorful and exciting account of the epic flight published years later in his autobiography—for it sheds light on why Williams and Yeager had such a hard time communicating. But Yeager's outspoken reminiscences shed even more light on the differences and personal frictions between test pilots who are engineers (like most of those employed by the NACA), who try always to fly precisely, systematically, and after meaningful data, and pilots like Yeager who are accustomed to "living dangerously and flying the same way." At Muroc in the late 1940s a real grudge apparently grew up between these two types of pilots. According to Yeager's autobiography, the NACA "wasn't thrilled" with the army's selection of him as



"....And with this we hope to break the Sonic Barrier."

From the Langley Air Scoop, *June 1947.*

The day before Langley received this report from Williams, one of its own test pilots, Herbert Hoover, had initiated the NACA program on the XS-1 with a familiarization glide flight in the number two airplane. On landing, however, Hoover misjudged the height of the airplane and made several contacts with the ground, the last of which caused the nose wheel to collapse, before skidding to a stop on the desert runway with damage to the landing strut.[72] Repairs and bad weather kept the NACA airplane grounded for the next seven weeks.

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the XS-1's test pilot: "The NACA team [at Muroc] thought I was a wild man," a macho fighter jockey with no education and no real experience in flight research, whose cockiness might very well lead to tragic mistakes. Yeager, who remembers being treated with some condescension, calls the NACA pilots "the most arrogant bunch" at Muroc; "there was nothing worthwhile that a military pilot could tell them .... I rated them about as high as my shoelaces." See Yeager and Leo Janos, *Yeager: An Autobiography* (Toronto and New York: Bantam Books, 1985), pp. 129–131, 180–183.

During this interim from October to December 1947, test pilot Howard Lilly made the first two NACA test flights of a D-558-1, the navy airplane built by Douglas and so much favored by John Stack over the Bell XS-1. D-558-1 number one had arrived at Muroc with a company test team for the contractor program in April; at the end of summer, D-558-1 number two, the aircraft planned for extensive NACA service, arrived at the California site. It was number two that Lilly flew in November 1947. The NACA's systematic flight tests of the XS-1 number two began on 16 December 1947 when Herbert Hoover became the first NACA pilot to fly a rocket plane. He reached Mach 0.71. By the end of January 1948, Hoover had made six more powered flights in the XS-1, working the speed up to Mach 0.925, and Howard Lilly had checked out in the plane. On 10 March, Hoover achieved the NACA's first supersonic flight. Three weeks later, Lilly repeated Hoover's supersonic performance.

Between the time of these first civilian supersonic flights in early March 1947 and the time of the NACA's replacement by the National Aeronautics and Space Administration in the summer of 1958, the NACA made in the neighborhood of 100 research flights either in the XS-1 number two or in one of its sister ships in the X-series. In the same period, the NACA made nearly 300 flights in the D-558-1 or D-558-2, the latter being the first research airplane with swept wings.[73] But this chapter's examination of Langley's role in this genesis and development of the transonic research airplane program ends with December 1948. At that time, the National Aeronautic Association selected John Stack of Langley laboratory to share in its 1947 award of the Robert J. Collier Trophy, the association's annual prize for the greatest achievement in American aviation. In a ceremony at the White House, President Harry Truman presented the Langley engineer the award citation, which read:

> To John Stack, Research Scientist, NACA, for pioneering research to determine the physical laws affecting supersonic flight, and for his conception of transonic research airplanes; to Lawrence D. Bell, President Bell Aircraft Corporation, for the design and construction of the special research airplane X-1; and to Captain Charles E. Yeager, U.S. Air Force, who, with that airplane, on October 14, 1947, first achieved human flight faster than sound.[74]

In accepting his citation, Stack insisted that he should not have been singled out for a share of the Collier award. The NACA's contribution to the supersonic flight of the XS-1, he said, had been a team effort.[75]

Langley test pilot Robert Champine (in X-series pressure suit) lands the D-558-2 Skyrocket number two at the NACA High-Speed Flight Station in California after completing a stability and control investigation at Mach 0.855, 7 December 1949.

Ironies

It was ironic that Stack won a share of the Collier Trophy, commonly rated the highest honor in American aviation, for his part in the success of the Bell XS-1. Supersonic flight had not even been Stack's original interest;

his idea for the research airplane program had been only to get information in the transonic speed range. He had opposed the army's decision to build a bold new air-launched rocket plane designed especially for the purpose of pushing through the speed of sound. He had favored a rather conservative, turbojet-powered airplane designed to take off from the ground, as airplanes had always done, and explore the high-speed frontier from Mach 0.8 to 1.0. It was the Douglas D-558 that had actually followed Stack's concept. It was the development of the D-558, not the XS-1, that Stack had most encouraged NACA researchers to advance.[76]

After the successful supersonic flight of the XS-1 in October 1947 the NACA said nothing to indicate what Stack's real position on the development of the rocket plane had been. Rather, it emphasized the cooperative nature of the entire research airplane program. During a public presentation in June 1949 Stack said: "The research airplane program has been a cooperative venture from the start among the Air Force, Navy, the airplane manufacturers, and the NACA. The extent of this cooperation is best illustrated by the facts 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."[77] Stack repeated these two sentences in speech after speech in the late 1940s and early 1950s.[78] As NACA spokesmen reiterated Stack's message, people believed that the research staff at Langley laboratory had in fact planned from the beginning for the XS-1 and D-558-1 to be complementary research vehicles, with the idea that the army plane would push through Mach 1 to supersonic flight while the navy plane simultaneously studied the transonic region from Mach 0.8 to Mach 1.[79]

In reality, of course, Stack had argued strongly from 1944 to 1946 that the rocket plane the army was procuring from Bell was unsafe and in important ways unsuitable for studying the intractable transonic speed region. This attitude eventually produced two further ironies. First, it was the conservative, slower-speed D-558-1 turbojet preferred by Stack, partly for safety reasons, that killed NACA pilot Howard Lilly in May 1948 due to engine failure during a *ground* takeoff. The faster air-launched XS-1s had a good safety record at Muroc. Second, it was the D-558-1, not the XS-1, that ended up the greater anachronism. Shortly after the NACA began testing its D-558, service airplanes like North American's F-86 Sabre flew in the Mach 0.8 to 1.0 speed range that the NACA most wanted to explore. The NACA could have instrumented one or more of these service planes as it did the D-558-2 at Muroc and could have conducted extensive transonic flight research using them. As Stack's associate John V. Becker wrote in *The High-Speed Frontier*, "If the D-558-1 could have been promoted in the

A technician prepares dynamic models of the Bell X-1E and the Vought XF-8U Crusader for wind tunnel testing in 1957. The Crusader was then the navy's fastest aircraft—maximum speed Mach 1.75 at 35,000 feet.

early forties, it would have been timely. But coming into the flight picture as it did in 1947, it was unnecessary."[80]

There was another reason why the D-558-1 was unnecessary by 1947. Not only were certain service airplanes flying fast enough to be instrumented for transonic flight research, but NACA engineers had discovered a variety of ways (see next chapter) to circumvent the problem of wind tunnels choking just below and just above the speed of sound—the problem, then thought to be insoluble in the short term, that had led Stack and his associates to the idea of the research airplane program in the first place.

Although it was ironic that John Stack shared the 1948 Collier Trophy for the supersonic flight of the rocket-powered XS-1, Stack and the NACA certainly deserved recognition. Supersonic flight depended unquestionably upon their prior successes. Almost singlehandedly, the Langley engineer had initiated the research airplane program and had sold it to military services heavily preoccupied with fighting a world war. As has been shown, this was not an easy accomplishment: the army did little with Stack's initial proposal other than to put it in a desk drawer at Wright Field. After the Bell XS-1 was in procurement, NACA ideas (including some from Stack) and new research information (provided by LMAL research teams led by Stack)

EDWARD BURKS

HANS KROPF

HANS KROPF

SCIENTIST: John Stack, for the past 20 years a government research scientist with the National Advisory Committee for Aeronautics, is the first of the three men who share the award of the Collier Trophy for the achievement of human supersonic flight. It was because of Stack's awareness of the absolute necessity for ever superior aircraft, and his intensive study of problems of supersonic flight that a workable program for the construction of a research plane came into being.

MANUFACTURER: Lawrence D. Bell, president of Bell Aircraft Corporation, was awarded the contract by the Air Force to design and build the plane evolved from Stack's scientific presentation of supersonic flight. Bell has a reputation for taking on the unusual, the unconventional and what some called the impossible. The ship he designed and built was the Bell X-1 which, before delivery, was tested in 21 flights at a speed slightly less than that of sound.

PILOT: Captain Charles E. Yeager, USAF, was chosen from the nation's finest test-pilot talent as the man to fly the plane pioneered by Stack and built by Bell. Deemed "a natural airman, if there is such a thing," on October 14, 1947, Yeager became the first man to fly faster than the speed of sound. It is for the combined achievement of these three men in their successful penetration of the transonic barrier that the Collier Trophy for 1947 has been awarded.

The Collier Trophy

For Flight Beyond the Speed of Sound

By **FREDERICK R. NEELY**

For bringing about the achievement of human supersonic flight, John Stack, Lawrence D. Bell and Captain Charles E. Yeager, USAF, win America's highest aviation award

AMERICA'S highest aeronautical honor, the 37-year-old Collier Trophy, was presented by President Truman at the White House Friday, December 17th, to the three men adjudged most responsible for the attainment of human supersonic flight. The trophy is awarded annually by a committee selected by the National Aeronautic Association for "the greatest achievement in aviation in America, the value of which has been demonstrated by actual use during the preceding year." It will be shared equally for the ensuing year by:

John Stack, career government research scientist of the National Advisory Committee for Aeronautics "for pioneering research to determine the physical laws affecting supersonic flight and for his conception of transonic research airplanes."

Lawrence D. Bell, president of Bell Aircraft Corporation, "for the design and construction of the special research airplane X-1."

Captain Charles E. Yeager, U.S. Air Force, "who, with that airplane, on October 14, 1947, first achieved human flight faster than sound."

To those three men goes the honor of playing the major roles in an achievement which the Collier Trophy committee termed "the greatest since the first successful flight of the original Wright Brothers' airplane."

All three have been outstanding in their contributions to the vitally important science of supersonic flight—flight that is faster than sound, the speed of which at sea level, with a temperature of 59 degrees and in still air, is 761 miles an hour. However, at altitudes ranging between 40,000 and 100,000 feet, the speed of sound is reached at only 663 miles an hour. This is due to the fact that at such high altitudes the temperature is almost constantly 67 degrees below zero and sound travels more slowly in cold air. At just what altitude Capt. Yeager flew is as much of a secret as the actual supersonic speed he attained.

The problem that confronted Stack, Bell and Yeager was not so much that of flying faster than sound as it was successful flying at speeds between 600 and 900 miles an hour—the transonic range.

Aeronautical scientists were in grave doubt as to just what took place when conventional aircraft entered the transonic range in high-speed dives. They knew that both plane and pilot were kicked around unmercifully for seconds that seemed like centuries and that both were completely out of control. Badly and naturally frightened, the pilots were unable to bring back detailed scientific reports on the phenomenon, and they were usually unwilling to repeat their flights.

Wind tunnel tests with small-scale models revealed that the flow of air over a plane in the transonic range was partly subsonic and partly supersonic. Because of this, the conventional planes (usually fighter types) took on an extremely inconsistent and erratic behavior. But the tunnel findings were not conclusive and since supersonic tunnels large enough to mount a full-scale airplane are prohibitive in cost the scientists concluded they needed a special research airplane equipped with instruments capable of measuring and automatically recording all of the forces acting upon an airplane in transonic flight.

This was where John Stack came in. It was natural that he should have conducted the research phase for he had been working on the fundamental problems of high-speed flight in the wind tunnels and laboratories of the NACA at Langley Field, Virginia, since 1929, shortly after he had joined the government's great aeronautical research es-

Collier's for December 25, 1948

Who, me? John Stack (left) looks surprised to hear that he had won a share of the Collier Trophy for his work on the Bell XS-1 with Lawrence D. Bell (center) and Capt. Charles E. Yeager (right), since it was the development of the Douglas D-558, not the XS-1, that Stack had most wanted to encourage. The page is from Collier's, *25 December 1948.*

contributed greatly to the airplane's rapid development. NACA personnel, overcoming vested interests and the "not-invented-here" syndrome (their own and others'), became enthusiastic about cooperating with the military and its contractor to improve the chances of the experimental rocket plane. The cooperation that resulted supplied the American aircraft industry with the data base it needed for the safe and efficient design of the transonic and supersonic aircraft the U.S. military now wanted.

In the end, the research airplane program seems to have furthered the cause of the NACA almost as much as the NACA furthered the cause of the research airplane program. The transonic problem stimulated the development of important new free-flight and ground-based test techniques: the wing-flow, drop-body, and rocket-model methods. Working on the XS-1 and D-558-1 provided Langley researchers with a focus and a goal that were needed after the end of World War II. Winning the Collier Trophy in 1948 for the supersonic flight of the XS-1 (by then designated the X-1) and again in 1952 for the invention of the slotted-wall transonic wind tunnel bolstered the reputation of the NACA and boosted the morale and self-confidence of all NACA employees, at Langley and elsewhere. This was timely therapy after the criticism they had suffered at the end of the war by news of the American "failure" to seize the practical usefulness of the turbojet as quickly as the rival British and German aeronautics communities had.[81]

For good or bad, involvement in the ambitious research airplane program required the NACA to become more complex organizationally, to do more intra-agency planning, and to formalize some of its methods of management. Planning and monitoring the flight-testing of the XS-1 and D-558 at Muroc was not a small or simple task, especially when it entailed supervision from a mother laboratory some 2500 miles away from the engineers and equipment doing the work. Concern for proper management led the NACA in 1948 to create a special research airplane projects panel and in 1949 to establish a larger NACA High-Speed Flight Research Station (HSFRS) at the California air force base. Langley continued to manage this station until 1954, when NACA headquarters decided to make it an autonomous field installation, the NACA High-Speed Flight Station (HSFS). In 1958, this installation became NASA's Flight (later Dryden) Research Center.

11

The Slotted Tunnel and the Area Rule

Many of our greatest technological artifacts are themselves fine art: the Great Pyramid at Giza, the Chartres cathedral, the Brooklyn Bridge. The engineers who designed these structures applied scientific principles, but they also used practical skills, cleverness in contrivance, and an innate sense of aesthetics. Until the Industrial Revolution, western societies recognized and appreciated the vital role of art in their technologies. With the arrival and widespread use of awesome new machines like the steam engine in the eighteenth century, however, more and more people in Europe and America began to assume, incorrectly, that the only thing that was going into engineering design and invention was science—a type of knowledge theretofore considered too complex, abstract, and even dangerous for the average person to comprehend, let alone command. Although science and mathematics together played an increasingly important role in engineering from the Italian Renaissance in the fifteenth century on, in truth artistic creativity continued to fix many of the outlines and fill in many of the details of our material surroundings through the Industrial Revolution.[1] It was just harder to spot amidst all the operations of modern applied science. Today the mind's eye remains one of the most essential organs of technological creation. Visual conception and imagination help to shape everything from the next model Buick Skylark to the next generation IBM personal computer.[2]

Aerodynamic research also involves artistry. The mind's eye made important contributions to the success of various major programs at the NACA laboratory, particularly the design of the airfoil and cowling families in the late 1920s and 1930s. After World War II, the most outstanding examples of artistry at Langley involved the design of the first *transonic tunnel*, whose key component was a *slotted-throat test section*, and the discovery of the *area rule*, a new concept in the shaping of high-speed aircraft. These two achievements by Langley researchers were products of intelligent guesswork, reasoning by intuition, and cut-and-try testing as much as products of numerical systems analysis, parameter variation, or theory. Both the slotted tunnel and the area rule derived largely from pictures in the mind. In a book about engineers in charge, this chapter will explore how visual images charged engineers.

Model Support

After calling for a transonic research aircraft in the early 1940s, Langley researchers had continued to grapple doggedly with the choking problem of their wind tunnels. They persisted even after procurement of the XS-1 and D-558-1 was assured in 1945. It would have been folly for them to have done otherwise, since there was no assurance that the research airplane program was going to provide the unique kind of new data about transonic aerodynamics that the military services, the aircraft manufacturers, and the NACA itself required. Moreover, John Stack and his associates were die-hard wind tunnel advocates anyway, by nature predisposed to go after the choking problem of the conventional closed-throat tunnel, the problem that had led to the concept of the "sound barrier" in the first place. In the minds of Stack's team, the research airplane was a stopgap superior to drop bodies and rocket models, but a stopgap nonetheless; they would have preferred a solution to the transonic impasse involving some discovery about the imperfect nature of their own precious ground-based type of facility.[3]

The first way that Langley researchers discovered to minimize the tunnel choking problem was the *small-model technique.** By early 1944 choking data correlated from hundreds of previous tests in the lab's various high-speed tunnels made it clear that the range of choked-out airflow speeds was primarily a function of the ratio of the cross-sectional area of the test model to that of the tunnel. Experiments demonstrated that if the lab reduced the size of its models correctly to one-tenth of one percent of the tunnel throat area, its high-speed tunnels would still choke, but at approximately Mach 0.95 instead of 0.80. The range of speeds unobtainable in wind tunnels would be substantially narrower.[4]

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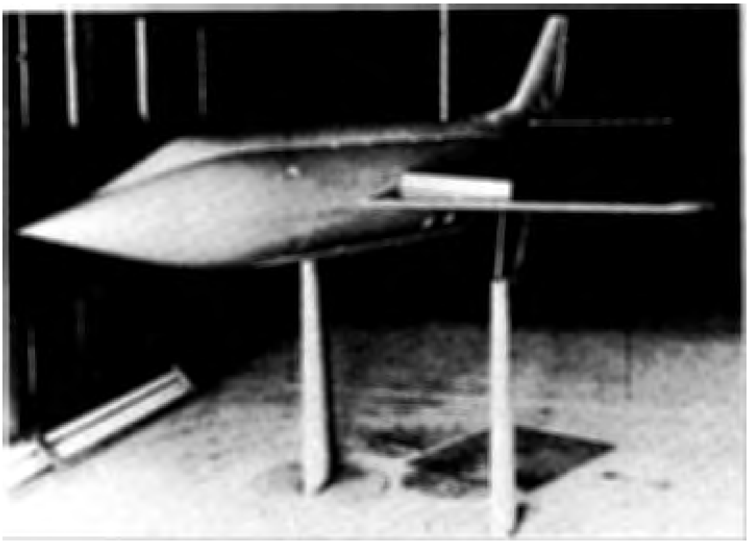
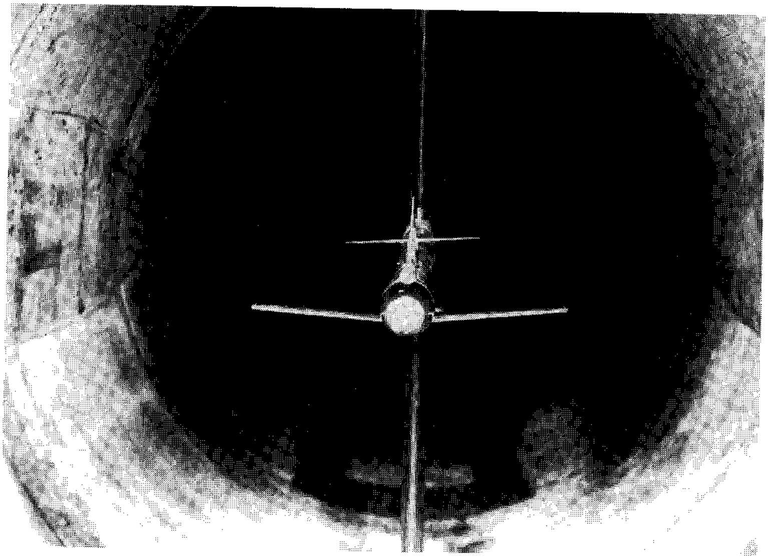
* Langley veteran John V. Becker recalls the evolution of the small-model technique and other major innovations in transonic wind tunnel technology during the 1940s as part of *The High-Speed Frontier: Case Histories of Four NACA Programs* (Washington, D.C.: NASA SP-445, 1980), pp. 98–118. This chapter extends Becker's highly technical story, based on oral testimony of other key participants in the developments and on further research into the archival record.



The strut system used conventionally into the 1940s to support models in large wind tunnels disturbed the airflow so much that many test results were questionable, especially at higher Mach numbers. In this photograph from August 1946, a strut-supported model of the Bell XS-2 is being tested in the 7 × 10-Foot Tunnel.

Langley's experts knew that it was necessary to come up with a correct model support system if the choking range of tunnels was to be narrowed in actual practice with the small-model technique, since, when a smaller model was used, the struts used conventionally to support a model in a test section would contribute more to the choking of the airstream than would the small model itself. These struts were large, asymmetric, and usually attached directly to the forward part of the model surface; they caused local accelerations and changes in the alignment of the flow relative to the model that could not be corrected by any known method of determining support interference. In sum, this meant that test data at the higher Mach numbers were questionable.

Even before the advantages of the small-model technique were verified experimentally and expressed in an NACA report, John Becker, head of the 8-Foot High-Speed Tunnel section, was working to develop a new model support system that would eliminate the interference effects and thus permit wind tunnel testing at higher Mach numbers. In 1943, Becker's division chief, John Stack, had gotten the NACA's approval to repower the 8-Foot HST from 8000-horsepower to 16,000-horsepower drive for operation at higher subsonic speeds. (Langley had designed the tunnel in 1934 for Mach numbers approaching 0.8.) Becker knew that the conventional strut support



Center-plate support for a model of the Douglas D-558 reduced airflow blockage during this test in the 8-Foot HST, June 1947.

system would not work in the repowered tunnel because of its choking limitation. After considering a number of alternative types of new support arrangements, Becker thought to suggest symmetry as the key to a practical solution. In the summer of 1944, he went to Stack and told him about his idea for a *center-plate* support. This support would consist simply of a long thin vertical plate mounted across the tunnel diameter and attached to the floor and ceiling of the test section, Becker said. Wing models would be mounted in the plate's plane of symmetry, half spans protruding from each side, to reduce blockage of the airflow.

Stack decided to have the new type of model support installed in the 8-Foot HST while it was shut down for repowering. When this tunnel began operations with its new 16,000-horsepower drive in the spring of 1945, it had a center plate. Langley now had a ground-based facility that provided reliable data to above Mach 0.9. The first models tested on the center plate of this facility represented wing and tail configurations under consideration by the Army Air Forces as design components for its first high-speed jet bombers.[5]

The center-plate support proved particularly useful for studying the high-speed aerodynamic forces and pressures affecting isolated wings; it proved unadaptable, however, for investigating the performance of wing and body combinations and complete aircraft configurations. What was needed to investigate the performance of these more detailed shapes was a *sting* support system. With this system there was less interference: the model was supported from behind by a rod protruding forward from a vertical strut downstream of the test section, instead of from below by a strut intruding in the airstream of the test section.

Langley had tried stings before 1944, but it had done so for reasons other than to increase the Mach number at which a wind tunnel choked. But these stings contributed just as much to flow blockage as the conventional strut supports did, if for a different reason. Beginning in late 1944, a group of engineers in the 8-Foot HST led by Eugene C. Draley (Becker had since become head of the 16-Foot HST) began designing a new sting support system. Their specific intention was to eliminate the source of the choking problem of the earlier stings: the large strut extending to the tunnel walls downstream of the model. After intensive study and several false starts, Draley's group arrived at a solution: move the strut farther downstream into the diffuser section and install a specially contoured insert or liner within the tunnel's existing walls to create a new closed-throat section ahead of the strut. These two changes compensated for blockage and resulted in the production of a more uniform flow. Langley used an early version of its new sting support system in the spring of 1946 to test models of the XS-1 and D-558-1 in the 8-Foot HST, thus enabling the NACA to provide extremely important and reliable performance data for speeds up to about Mach 0.92 a year before flight testing of the research airplanes began at Muroc.[6]

Langley's small-model technique and its center-plate and sting support systems were only two episodes in the NACA's movement during the period 1942 to 1947 toward bridging the transonic gap in ground-based research capabilities. There were others. In late 1944 Langley engineer Coleman duPont Donaldson invented the Annular Transonic Tunnel, a ring-shaped passage with a single-bladed axial fan that was driven to very high speed by a series of electric motors—in actuality, more of a whirling arm than a tunnel. This facility began operation in early 1947, and, though serious questions soon arose about the quality of its test results, it made an immediate impact by providing the first pressure distributions ever measured on an airfoil at Mach 1.[7]

A few months before Donaldson's invention, another group of Langley engineers was exploring the utility of a crude but remarkable tunnel modification known as the *transonic bump* in the 300-MPH 7 × 10-Foot Tunnel. In truth, the bump was used in a way similar to Gilruth's wing-flow technique, the controversial free-flight test method that some of Langley's more die-hard wind tunnel personnel had rejected for so long as unscientific: a carefully shaped wooden bump or wave about a foot high was placed on

A sting-supported model of the Bell XS-2 was tested in the 9-Inch Supersonic Tunnel in July 1947.

the floor of a wind tunnel and the test model mounted in the region of the bump predicted to experience supercritical flow, just as in Gilruth's method a model surface was mounted in a precise location for airflow on an actual aircraft wing. As air flowed over the bump, it accelerated to transonic speeds even though the speed of the main airflow remained subsonic. Results thus gave "a qualitative indication of the type of effects encountered at transonic speeds, and fairly reliable indications of trends."[8] The principal disadvantage of the bump test, like that of the wing-flow technique, was its low Reynolds number. Nevertheless, NACA researchers used the method until a better ground-based method was devised. Most configurations of the early X-series of aircraft, as well as of the D-558, went through tests on the bump.

Achieving a Transonic Tunnel

In 1946 Langley physicist Ray H. Wright conceived a way to do transonic research effectively in a wind tunnel by placing slots in the throat of the test section. The concept for what became known as the slotted-throat or slotted-wall tunnel came to Wright not as a solution to the chronic transonic problem, but as a way to get rid of wall interference (i.e., the

Airflow over the bump in the 300-MPH 7 × 10-Foot Tunnel reached the speed of sound.

mutual effect of two or more meeting waves or vibrations of any kind caused by solid boundaries) at subsonic speeds.

For most of the year before Wright came up with this idea, he had been trying to develop a theoretical understanding of wall interference in the 8-Foot HST, which was then being repowered for Mach 1 capability. Wright had received this special individual assignment because as a member of John Stack's research division, which was "populated almost entirely by engineers," he had proved himself "an indispensable consultant on matters mathematical and theoretical."[9] In 1939 and 1940, for example, Wright had determined the critical speeds of a large number of existing airfoils and bodies from their low-speed pressure distributions.[10] This determination helped Stack's group contribute to the design of the 16-series, a new family of soon-to-be-celebrated NACA airfoils with higher critical speeds.*

The problem of wall interference facing Wright in 1945 was as old as wind tunnel technology itself. From the time Francis Wenham had built the first primitive tunnel in 1870, aerodynamicists had questioned exactly how airflow confined within solid wooden or metal walls could be

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* The 16-series airfoil sections were actually derived from the 1-series low-drag sections, which were developed by the Eastman Jacobs team and first described by Jacobs in his 1939 Advance Confidential Report on laminar-flow airfoils (also published as Wartime Report L-345).

simulating the actual conditions of flight in free air. The distance between these walls and the scale-model aircraft under investigation was at most only a few feet. Real aircraft disturbed the surrounding air to distances several times the scale of that dimension. Soon experts had discovered that it was impossible, because of the proximity of the solid walls, for airflow to stream naturally over and near the models. The walls strangled the flow streamlines, producing misleading aerodynamic results. Some experimenters had tried to prevent wall interference effects by making the test models smaller, reducing them from five percent to one percent of the test section area. But as with later use of the small-model technique at Langley, reduction in model size often raised the choking speed but also lowered the Reynolds number, thereby actually increasing the discrepancy between the environments of simulated and real flight. Some had also tried getting rid of the walls altogether—as in the small open jets devised in the 1920s by Briggs and Dryden at the Edgewood Arsenal (mentioned in chapter 9). But not having walls just distorted the streamlines in other ways.[11]

In attacking the wall interference problem, Wright benefited not only from the collective knowledge and experience of the engineers working around him, but also from his own hard work, good intuitions, and artistic perspective. This combination caused Wright to wonder whether "since the interference velocities due to ... walls are of opposite signs with free and solid boundaries, opposite effects might be so combined in a slotted tunnel as to produce zero blockage."[12] Theoretical methods were available for making wind tunnel wall corrections at Mach numbers well below the choking value. These methods were available for both closed- and open-throat tunnels. Wright's contribution would thus be in combining the corrections for the different types of throats in such a way as to eliminate the need for any correction at all.

Such an idea dated back to theoretical papers by Prandtl and Glauert in Germany during the 1920s. Stack and Jacobs had tested it at Langley in 1929 and 1930—by partially blocking an open throat with large models to reduce airstream choking—on the way to their final closed-throat configuration of the 11-Inch High-Speed Tunnel. Considerable work on the problem was done by the British, Italians, Japanese, and Germans during World War II. Most noteworthy was the work by Carl Wieselberger in Germany. In 1942, Wieselberger suggested a specific configuration with 46 percent of the perimeter open (via two wide longitudinal slots) as a means to reduce the blockage effect in certain German high-speed tunnels.[13]

NACA researchers did not find out about this work until Maj. Antonio Ferri arrived at Langley in September 1944 from the Italian aeronautical

After fighting the Nazis as chief of a partisan brigade, Antonio Ferri brought important new information to Langley in 1944 about current German and Italian research in high-speed aerodynamics.

research center at Guidonia, where, until the fall of Mussolini's government one year earlier, the young doctor of engineering had been in charge of the *Galleria Ultrasonora* (supersonic tunnel).* Besides reporting on Wieselberger's studies, Ferri brought papers to America covering recent tests he had conducted in a tunnel whose sides were 43 percent open. Together, this information showed that "the Italians had already succeeded in obtaining airfoil force data [in this semi-open tunnel] ... up to about Mach 0.94, and the Germans to about 0.92."[14]

Ferri's first job at Langley was to complete his tabulation of all the relevant Italian airfoil tests at speeds approaching Mach 1. When he finished in 1945, the NACA published his analysis as Wartime Report L-143; it demonstrated for the first time in America that "partly open arrangements could be used effectively."[15] In the following months, Langley tried to apply the Italian's semi-open principle, but the experimental configuration

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* Ferri held a doctorate in engineering from the University of Rome (1936). After the collapse of Mussolini's government in September 1943, Ferri had organized a band of partisans which fought the Nazis and Italian Fascists. In July 1944, when Allied forces took control of the Macerata province in which his partisan brigade (the "Spartaco") was operating, Ferri was contacted by an agent of the U.S. Army's Office of Strategic Services (OSS). He signed an agreement to work for the U.S. and to put all information in his possession at the country's service. Among the documents he gave to the OSS were numerous top secret technical reports, both Italian and German, which he had taken from Guidonia before it was seized by the Germans. Soon after his arrival in the U.S., the War Department assigned Ferri to act as an aeronautical consultant for the NACA at Langley Field, where he stayed (at the engineer-in-charge's special request) until 1950, when as an American citizen he chose to begin a teaching career at the Polytechnical Institute of Brooklyn.

experienced large pulsations. The lab would achieve a successful version of Ferri's arrangement in 1948, but in 1945 and 1946, when Ray Wright was working to achieve zero wall interference in the 8-Foot HST, none of Langley's high-speed tunnel experts were yet sure that the concept of the semi-open tunnel was valid.[16]

Besides understanding the problem of flow pulsation, Wright knew that Ferri's and Wieselberger's semi-open schemes could not work for the 8-Foot HST for at least two other reasons: (1) the 8-Foot HST was a much larger facility—Ferri's tunnel was only 1.31 × 1.74 feet—and, if semi-open, would require considerably more power than was available; and (2) the test section of the 8-Foot HST was circular, not rectangular as were Ferri's and Wieselberger's. Finding the degree of openness and exact slot design required by the circular 8-Foot HST for zero blockage would take a completely different solution.

Mathematical Analysis and First Test Programs

Knowing that the excess power required by slots tended to be proportional to the open area, Wright specified for analysis a configuration with ten narrow slots instead of the two wide slots of both the Wieselberger and Ferri configurations. It is important to note that in attacking the problem, his main weapon was applied mathematics—the same tool used by Theodorsen in the 1930s to lift the cowling program beyond its experimental impasse. Much later Wright would concede, during a conversation with colleague John Becker, that "a systematic experimental attack [i.e., the method of parameter variation] might have been equally effective."[17] However, considering the key role of Theodorsen's applied mathematics when parameter variation had stalled in the cowling development, Wright's use of the term *might* should be underscored.

As a result of a long series of tedious calculations, Wright discovered the optimum peripheral openness of the 8-Foot HST to be about 12 percent, or some 30 percent less open than the schemes of Wieselberger and Ferri. This delighted him because it meant that less additional power would be required. Wright reported his findings to Eugene Draley, his section head, in the late summer of 1946. Draley encouraged Wright to test his theory experimentally—the response Wright expected, as he was accustomed to pleasing engineers who wanted things demonstrated empirically. First, however, he was to report his findings to the division chief, John Stack.

Stack received Wright's report enthusiastically. Starting from his experience in 1929 and 1930 working with different configurations of the

11-Inch High-Speed Tunnel, he had been aware that closed-throat and open-throat tunnels had opposite characteristics in proceeding up toward Mach 1. But he had not since considered seriously how to design a high-speed tunnel half-open or half-closed and really make it work. Now, Wright was giving him good mathematical reasons to think that there was some way to do it.

Stack possessed an open mind toward innovation; his attitude was usually "Let's try the damn thing and see if we can make it work." According to teammate Mark Nichols, Stack

> had an intuitive feeling for things that were important, and he was always very interested in all of the technical matters. He'd have some young engineer ... come and give him a story and, no matter how scrambled the story, he'd look for a gem of new information or a new idea. And he had the faculty for picking these things out of nowhere and then pushing them. He'd say, "This is important, now, what can we do to develop this idea?"

Stack's enthusiasm would then infect those around him. He would lead, but those who followed would be made to feel confident that they were just as vital to ultimate success as he was.[18]

After hearing Wright's concept, Stack worked up a full head of steam. He informed NACA research director George Lewis of the development, discussed its major implications with him, and then proceeded to build a test program. There is no documentary evidence, however, that Stack thought of slots in the wall of a tunnel's test section in 1946 or early 1947 as a solution to the transonic problem. After all, Wright did not suggest it, or apparently even consider it, as such a solution; for him slots were just a means by which to get zero wall interference at high subsonic speeds. The same seems to have been true for Stack.

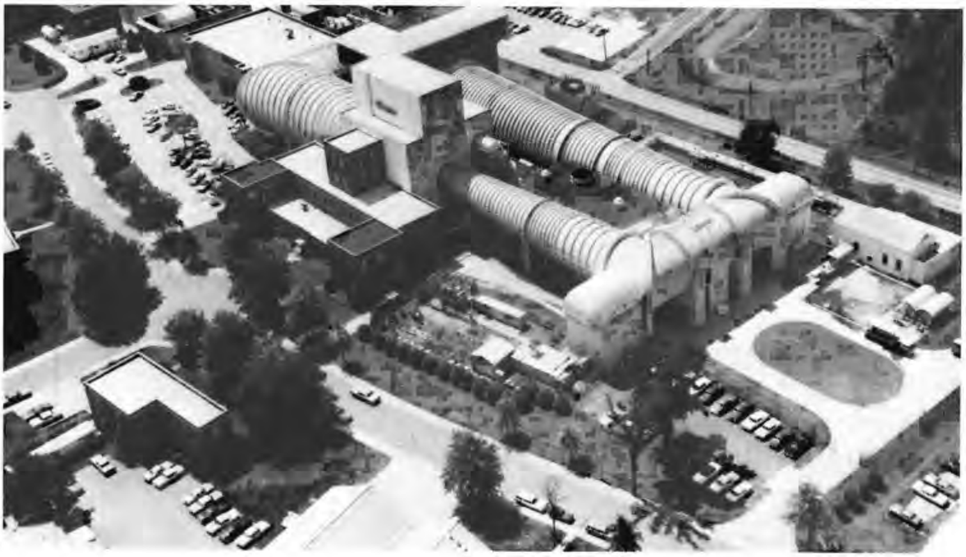
The first team of researchers to get involved in testing a slotted-throat tunnel configuration worked not in the 8-Foot HST but in the 16-Foot HST section. "It was quite easy for us to add a test program for Wright's circular 10-slotted arrangement," recalls John Becker, head of the 16-Foot HST, because "for some time we had been investigating blocking corrections" in small circular "parasite" test sections operated off the 16-Foot HST. (These parasite sections operated at speeds up to Mach 1.6 by sucking outside air through a long diffuser into the low-pressure test chamber of the 16-Foot HST. For details, see Becker's *High-Speed Frontier*, pp. 76–78 and 100–101.) Vernon G. Ward, the man who had been conducting the blockage-correction study for the 16-Foot HST, was assigned as project engineer for the experiments.

Accidental Discovery, Deliberate Debate

In the first test runs, which took place in early 1947, the slotted tunnel operating off the 16-Foot HST achieved a maximum speed, before choking, of Mach 0.97. Then, one day, one of the engineers wondered what would happen if he took the model out of the parasite test section and turned up the power of the driving fan. What happened excited this curious engineer and then excited everyone else at Langley who found out about it: the small experimental tunnel went up to and through the speed of sound just as beautifully as anyone could have imagined. (Which member of Stack's staff turned up the power is still uncertain, but Richard Whitcomb relates that it was definitely not Ray Wright.)[19]

At a meeting of Langley's General Aerodynamics Committee on 25 July 1947, Stack reported the unexpected success, discussed its implications, but mentioned no specific plans to install a slotted throat in any research tunnel. (The General Aerodynamics Committee was for the most part an informal discussion group of Langley physicists and engineers. It met once a month to take up major aerodynamic issues.) At that moment Stack was in fact leading his men in the 8-Foot HST section (Draley, Wright, Axel Mattson, Richard Whitcomb, and others) through the design of a 12-inch slotted-wall section to test Wright's concept, but he was keeping plans to himself until the right time—when slotted-throat designs were proven effective beyond a reasonable doubt and funds were available for converting both the 8-Foot HST (maximum speed Mach 0.75 before the repowering) and the 16-Foot (maximum speed Mach 0.70 before repowering) into transonic tunnels.

Stack faced "a very strong current of disbelief" at Langley about the efficacy of using slotted throats. The new tunnel design was known to involve problems for which no one yet had answers: "power requirements, the details of slot shaping, ... the quality of slotted tunnel flow, model size limitations, possible combinations of wall divergence and slots, shock reflection problems above Mach 1, slots versus porous walls, etc."[20] The strongest expressions of disbelief came from two of Langley's purer theorists, Antonio Ferri and Adolf Busemann, both of whom had arrived only recently from Europe. (Ferri's arrival from Italy and its impact on NACA history have already been mentioned. Busemann, whose "arrow-wing" theory was discussed in chapter 10 in relation to R. T. Jones's concept of a swept wing, came to work at Langley in early 1947 after having worked at the Luftwaffe laboratory near Brunswick, Germany. He was brought to this country soon after the end of the war as part of Operation Paperclip.)[21]



The 16-Foot HST began operations on 5 December 1941, two days before the Japanese attack at Pearl Harbor. During the war, the tunnel tested various air-cooled aircraft engines, cooling systems, high-speed propellers, and even the shapes of the first atomic bombs. This was the first tunnel to receive the NACA's authorization for installation of a slotted-throat transonic test section (in 1947), but the second actually to get it done (in 1950). The picture to the left shows the vanes that turn the airflow around one of the tunnel's corners.

Busemann and, in particular, Ferri felt sure that Wright's theory was wrong and that, if Stack continued charging forward with plans to convert the test sections of both the 8-Foot and 16-Foot HSTs into slotted throats, both he and the NACA would end up appearing foolish. Ferri took his case to at least one colleague, John Becker, who recalls that Ferri

> conceded that slots could be used to reduce the blockage effect, but to have zero blockage at Mach 1 was physically unlikely except for very small models. He felt that many mathematicians and physicists who had an understanding of transonic theory would regard any NACA claim of valid data at Mach 1 for sizable models as absurd.

Unless someone could persuade Stack to at least use "some words of qualification when discussing slotted tunnels," Ferri advised, the NACA's international reputation would be permanently blemished. Becker's own opinion as head of the 16-Foot HST section was that more definitive answers to outstanding questions should be pursued in the model tunnel program "before any commitment was made to incorporate slots" in either the 16- or 8-Foot HST.[22]

The slotted tunnel was the only item on the agenda for the September 1947 meeting of the General Aerodynamics Committee. Ray Wright and Vernon Ward had been asked by Samuel Katzoff, committee chairman, to begin the meeting by presenting their most recent results briefly, which they did "in rather modest terms." Stack, who had only grudgingly agreed to attend,

> made a late entrance and sat down at the head of the table with a belligerent look on his face. Clearly it said, "Anyone who wants to argue about the slotted tunnel will have to take me on."[23]

After Wright and Ward finished their report, Busemann and Ferri had their chance to comment. Busemann reiterated his earlier conclusion that from the standpoint of theory an approach better than Wright's ten discrete slots was one involving a "homogeneous boundary" in which the slots were uniformly distributed about the periphery.[24] Ferri's point, that slots could be used to achieve zero blockage but only with very small models, was lost on many participants "through a combination of poor English [whether he said 'subsonic' or 'supersonic,' people heard 'soup-sonic'] and extreme politeness."[25]

John Stack was a hard-charging, persuasive man whose attitude toward unproven technology was usually "Let's try the damn thing and see if we can make it work."

It is not clear whether Stack totally understood both points, because when both theoreticians had finished, Stack said in essence:

> OK, I get your point. So, there is a unique situation exactly at Mach 1. We'll make this thing work at Mach .995. We'll make it work at Mach 1.005. And we won't give a damn about that little thing in between.[26]

Those who knew him best readily attest to the fact that Stack could be stubborn, and in this instance he had definitely already made up his mind. He was going to have his engineers roll up their sleeves and club away at the problem until it was solved. They were not to let the infinitesimal point in the middle stand in their way. A slotted tunnel would be built.

As head of the Compressibility Research Division, Stack was in a position to block internal opposition from those who knew the most about high-speed aerodynamics. His division had grown tremendously in size and importance in the early postwar period as a result of the shifting emphasis from subsonic to supersonic flight. Three major new facilities—the 4-Foot Supersonic Pressure Tunnel, the Gas Dynamics Laboratory, and the Induction Aerodynamics Laboratory—had been added to those already under his supervision. Considering his experience, management responsibility, and great personal dynamism (some have compared him to a bull in a china shop), few subordinates risked opposing Stack once he had made up his mind.

Moreover, Stack's prestige and influence within the NACA were now approaching a zenith. In one month the XS-1 would break the sound barrier, an achievement for which he would share a Collier Trophy. George Lewis, who had only just retired as director of research, remained on with the NACA as "research consultant"; Stack was one of his favorite boys. Hugh Dryden, Lewis's successor, would not have the same paternal feelings for Stack, but as slotted-throat tunnel development was unfolding, the two men were enjoying a honeymoon period.*

In the fall of 1947 Stack had to decide which of the big tunnels to convert to a slotted throat, and then sell the idea to NACA headquarters. Only a hard-charging, persuasive man like Stack, willing to keep an idea alive at a time when most other experts would have preferred to kill it, could have accomplished this as quickly as he did. He decided to convert the 16-Foot HST first. This decision paved the way for quick approval by headquarters, which had just approved funding for a 60,000-horsepower repowering project that could be broadened to include conversion of the walls of the test section. (In fiscal year 1947 Langley had requested and gotten approval for 35,000-horsepower repowering. See appendix C. It should be noted that repowering was doubly relevant to this slotted-throat conversion, not only in terms of budgetary scheduling convenience, but because even a tunnel whose walls were 12 percent open required about twice as much fan power as one with solid walls.) Total conversion would require a special assignment of additional funds, however. On 10 January 1948, Langley submitted a formal "Description and Justification for Slotted Test Section" prepared by a member of the 16-Foot HST staff for consideration as part of the NACA's fiscal year 1949 budget request. Stack defended the justification vigorously and in person before top management in Washington. Management soon approved, but in doing so made clear that it could take two to three years to procure all the money needed for total conversion.[27]

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* Stack and Dryden were individuals of conflicting backgrounds and personalities—Dryden, a scientist, an introvert from a proper New England Protestant family; Stack, an engineer, the extroverted son of a first-generation Irish Catholic immigrant who had settled as a carpenter in the factory town of Lowell, Massachusetts. In October 1957, the differences between the two men came to a head: at an NACA dinner party hosted by NACA chairman James H. "Jimmy" Doolittle, Stack called Dryden an old fogey—loud enough that most everyone in the room could hear; Dryden never forgave him for that. See Roland, *Model Research*, p. 292, and Walter A. McDougall, ... *The Heavens and the Earth: A Political History of the Space Age* (New York: Basic Books, Inc., 1985), p. 165.

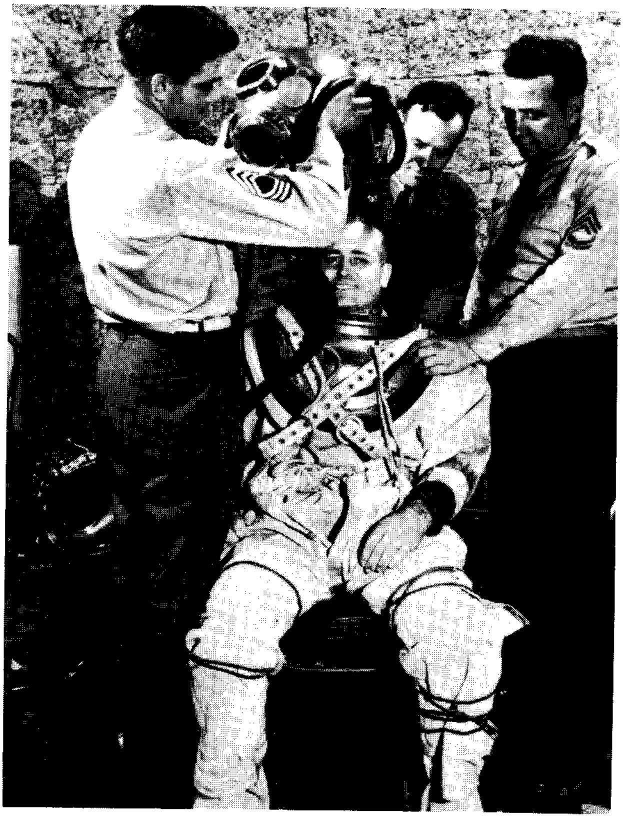
The entire 16-Foot HST staff was now "under heavy pressure to come up with the additional data" needed for an effective design. Stack assumed "personal supervision on a daily basis for the many interrelated slotted tunnel activities, ranging from expediting work on models in the shops, to working with the detail designers of the 16-foot section, and dealing as always with funding and approval problems." By holding frequent meetings, he not only made sure that researchers abided strictly by his schedules but also that they maintained as much enthusiasm for the project as he did himself.[28]

A few researchers came cautiously to Stack with objections and alternatives to the slotted throat. Before a million dollars or more was spent modifying and perhaps ruining a proven research facility, they wanted him to make sure that the slot design was perfected. Antonio Ferri seems not to have been among them, however. After Stack, his division chief, had made the decision to convert the 16-Foot HST, Ferri went to him and asked if there was anything he could do to help. Stack admired this loyalty; the Italian soon became one of the most trusted members of the team and a close personal friend.

Surprising Announcement

In the spring of 1948 Stack announced at Langley that the test section of the 8-Foot HST would also be converted to a slotted throat. This news stunned critics and defenders of the slotted throat alike. Researchers in the 8-Foot HST had been focusing their attention recently on using the new closed throat together with the new sting support system for research at Mach 1.2 and "had given little thought to the next step." Stack was discouraged, however, with preliminary results in the reconfigured facility: the Reynolds numbers of the tests were lower than desired. Moreover, he was "very impatient at the prospect of two or three years of procurement time before the 16-foot tunnel would be operable." Convinced that alteration of the 8-Foot HST would be cheaper and quicker because less complex technically, especially with all fabrication and installation being done in-house, he waved off all protests from his men for more time to study the problem. Before long it was clear to everyone inside the NACA that Stack had in fact transferred top priority for a slotted throat from the 16-Foot to the 8-Foot HST.

Sometime in late 1948, the 8-Foot HST went through the speed of sound with a slotted throat, but the flow was awfully rough and uneven. Now engineers had to get down to the nitty-gritty and come up with the exact slot configuration for smooth transonic flow. Wright's theory guided their



Because the slots he was designing opened directly into the 8-Foot HST's hazardous igloo-shaped test chamber, where high levels of pressure, temperature, and noise would be encountered, Ray H. Wright had to don a diving suit before venturing into the test section.

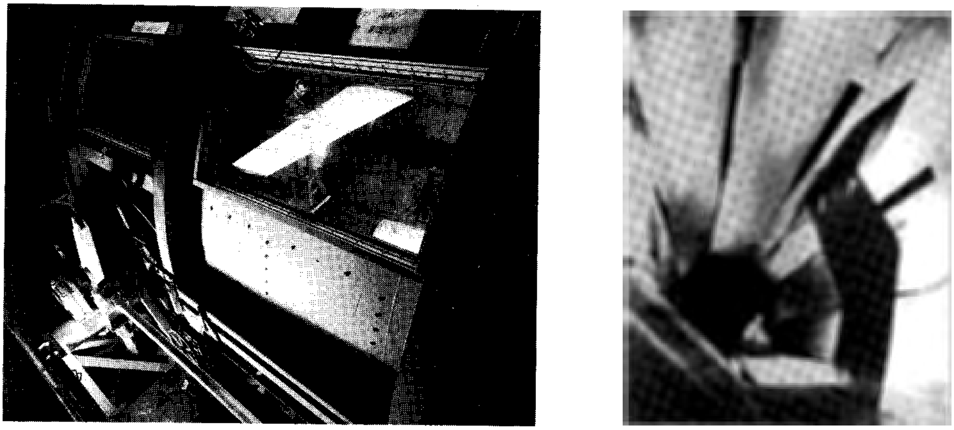
pursuit, as did design data from tests in the 12-inch model slotted section operated off the 16-Foot HST, but neither source of information sufficed. What was needed was creative use of the mind's eye and the touch of a sculptor. This artistry was provided by physicist Ray Wright and engineers Virgil S. Ritchie and Richard Whitcomb. By shaping the slots meticulously and continually by hand over a span of seven months, this trio refined the details of the slotted throat until smooth transonic flow distributions were finally achieved.[29]

The 8-Foot HST began regular transonic operation for research purposes on 6 October 1950. Just three months later, the 16-Foot HST also became operational with a slotted throat for transonic research. What made this short turnover time possible, according to Becker, head of the tunnel section, was not only the immediate exchange of critical knowledge about the proper shape of slots from the 8-Foot to the 16-Foot HST design groups, but also the 16-Foot group's separate pursuit of improved slot technology, which had continued even after Stack had given conversion of the 8-Foot HST priority.

Despite limitations, the slotted tunnels became "best practice" in transonic research almost immediately. By the end of 1950, in fact, Langley engineers were busy planning a completely new slotted tunnel. The design of this facility, which became operational in early 1953 as the 8-Foot Transonic Pressure Tunnel, remedied three of the problems that had been plaguing the operation of the converted tunnels: (1) high humidity and fog, caused by the need to draw outside air into the main airstream for cooling purposes, (2) high turbulence, and (3) low Reynolds numbers.[30]

A problem left unremedied in Langley's slotted-throat transonic tunnels was "the inability of the slots to alleviate significantly the reflection of pressure disturbances from the solid regions of the walls." The tunnels did not choke going through Mach 1, but test data "often exhibited significant discrepancies when compared to free air."[31] In the early 1950s, engineers at NACA Ames Laboratory in California designed a new type of transonic tunnel section to alleviate this reflection problem. Instead of slots, they incorporated a "mesh of holes" in the test section wall. Placed inside Ames's repowered 16-Foot HST (which had been built, like its twin at Langley, in 1941), this ventilated or porous tunnel began routine operations in late 1955 as the 14-Foot Transonic Tunnel. Tests in it helped solve the transonic stability problems of various missiles.[32] For all practical purposes, however, the porous wall tunnel at Ames did not solve the problem which had been plaguing the operation of the slotted tunnels at Langley—for it eliminated the reflection at only one specific Mach number.

The military services and their contractors had been following Langley's slotted-throat developments closely since at the latest the fall of 1948, when they received the NACA's first confidential report on the transonic tunnel test sections.[33] In December 1948, for example, Air Materiel Command headquarters sent several representatives to the laboratory to discuss the possibility of using a slotted throat in a ten-foot wind tunnel at Wright-Patterson AFB near Dayton, Ohio (formerly McCook Field, and later Wright Field). After meeting with Stack, Draley, Wright, and Ward, however, Bernhard Goethert, the air force's leading scientific brain at the meeting, concluded that because the NACA "was embarking on a program to obtain power measurements of large slotted throats," it would be unwise for military engineers to embark "on a systematic series of investigations on slotted-throat power considerations themselves." Rather, they should wait until the NACA's power requirements were available.[34] This conclusion, which Air Materiel Command seems to have endorsed, pleased everyone



The test section of the 16-Foot HST before (left, 1945) and after (right, 1951) installation of slotted walls. The 1945 photo shows the test section as seen through a window from the outside. (For another view of the 16-Foot HST's slotted test section, see the last picture in the Introduction.)

at Langley. After all, though Stack had committed both the 8-Foot and the 16-Foot HST staffs to the slotted-wall concept, a successful design for research use had not yet been achieved.

By the spring of 1949 rumors about Langley's transonic tunnel developments had circulated widely among those in the American aeronautics community who had not even been entrusted with the NACA's first confidential report. At its annual inspection held that May at Langley, the NACA had tried to divert attention from the slotted throat by having Stack deliver a talk in which he emphasized the Annular Transonic Tunnel. But the camouflage did not fool anyone, especially when, in the following month, Hugh L. Dryden, the NACA's new director of research, "took the unusual step of requesting other organizations to follow the Committee's policy of assigning a confidential classification to all information relating to the development of transonic wind tunnels."[35] Clearly Dryden did this in deference to the military, which was then planning new supersonic fighters and bombers that would have to fly through the mysterious transonic region.

Classification meant that the NACA researchers responsible for the slotted throat had to sacrifice the personal advantages of quick and open publication. In 1950 when researchers at the University of Southern California reported work on their own slotted test section, Langley engineers reacted defensively. Eugene Draley complained in a memo that

> this work, to date, is apparently being done without any security classification, which is in violation of the security principles established by the NACA on this type of work. It is therefore recommended that steps be taken to correct this situation ... not only because of the primary considerations associated with basic security requirements, but also because these results are being reported without any reference to any work by the NACA. The inference created by such work is that the whole subject and idea of slotted throats has been originated at the University of Southern California, which is very far from the truth.

In accusing USC researchers of misappropriating credit, Draley failed to mention that security regulations would have in fact prohibited the academics from making reference to anything they might have known about Langley's slotted-throat work. (Apparently USC did not know anything about it anyway.) He closed the memo by recommending that NACA headquarters "check into this matter and call the [USC] group's attention to the existence" of the NACA classified reports.[36]

Though outsiders continued to report independent work with slotted and porous test sections, Langley researchers soon got all the credit due them—Stack and his associates won the Collier Trophy for 1951 (Stack's second in four years) for developing the slotted wall. Two years later, with the principles of transonic wind tunnel design "widely known through independent research" both inside and outside the United States, Dryden withdrew his request "for special treatment for information relating to transonic wind tunnels." All thirteen previously published NACA reports on the slotted throat were subsequently declassified and announced in the usual manner.[37] In its *Annual Report* for 1954, the NACA admitted that advertising the potential of the Annular Transonic Tunnel at the 1949 inspection had been subterfuge.

In contemporary press releases, the NACA claimed that Langley's development of slotted-throat transonic tunnels gave the nation a two-year lead over all other nations in the design of supersonic fighters and bombers. This bold claim, as Becker points out in *The High-Speed Frontier* (p. 117), was based on projected good use of the area rule, a new concept in the shaping of high-speed aircraft. Ironically, considering his vivacity in the slotted-throat program, Stack would not be one of the area rule's staunch defenders.

Whitcomb's First Clue

Ever since his arrival at Langley in 1943, Richard T. Whitcomb, a 1943 graduate in mechanical engineering from Worcester Polytechnic Institute in Massachusetts, had worked under Stack in the 8-Foot HST section. Management had tried to place him in the Instrument Research Division, but the young engineer made it clear that he wanted to work in aerodynamics. The building and testing of model airplanes, the mania of his boyhood, still fascinated him. The sandy-haired, blue-eyed engineer quickly established a reputation as a wunderkind, the rare engineer who was not only quite capable mathematically but also possessed a powerful intuition and unusual artistic talent for cut-and-try techniques. Though the 8-Foot HST did not go transonic until equipped with the slotted throat in 1950, it had been able to get up close to the speed of sound (up to Mach 0.95) after its repowering in 1945. Thus Whitcomb started to get a feel for transonic aerodynamics at least five years before starting the research investigation that led directly to his conception of the area rule.

During this seminal period Whitcomb conducted research on the biggest problem facing the designers of supersonic aircraft—the large increase in drag (associated with the formation of shock waves) that occurred at transonic speeds. He knew, first from laboratory wind tunnel tests and then from the flight of the Bell X-1 (no longer called X*S*-1), that small, lightweight rocket-powered configurations with limited missions could overcome the transonic drag problem; he knew also, however, that for operational turbojets, which would have to be considerably heavier than the X-1, the problem would be critical. If flying up to and through Mach 1 took gradual acceleration because of high drag, there would be insufficient fuel left for the aircraft to sustain supersonic flight for long after achieving it.

In July 1948, after analyzing all available transonic information from NACA ground facility and free-flight (including Wallops Island rocket-model) tests, Whitcomb submitted a proposal for wind tunnel tests of a swept wing and fuselage combination. Fairly substantial progress in reducing transonic drag rise had been achieved by using sweepback and optimizing the shapes of fuselages, and he felt that with proper arrangement and shaping, the drag-producing disturbances caused by the wing and fuselage might be made to counteract each other.[38]

In late 1949 and early 1950 Langley tested models incorporating Whitcomb's sweptback wing and body combination at high subsonic (Mach 0.95) and low supersonic (Mach 1.2) speeds. Results indicated very little favorable effect in reducing the drag. In fact, the results showed significantly higher total drag than transonic theory predicted for the drag of the wing and the drag of the body combined. Stymied, Whitcomb decided that he needed to know more about the fundamental nature of flow at transonic speeds before truly fruitful work on the major design problem of supersonic aircraft could begin.[39]

As soon as the slotted-throat section was placed inside the 8-Foot HST, Whitcomb and his colleagues employed "every available tool" to study in detail what happened in the flow field around wing and body combinations at transonic speeds. These tools included (1) the tunnel *balance*, which had been used by wind tunnel researchers for years as the standard means of measuring the aerodynamic forces on a model (i.e., lift, drag, and pitching moments); (2) *orifices* sensitive to pressures at various points on the surface of a model from whose measurements one could calculate local velocities; (3) *tuft surveys*, involving little pieces of cloth attached in various places on a model surface, by which observers could tell if the flow was smooth or disturbed; and (4) *schlieren photographs*, a method for seeing shock waves (discussed earlier in chapter 9). None of these four methods were new; for instance, researchers had used tufts on some of the earliest airplanes flown at Langley. By using these available tools together, however, the 8-Foot HST group began to understand that drag patterns at transonic speeds were "completely different than anything that anybody had ever predicted" theoretically.[40]

The schlieren photographs were most startling. Besides showing the well-known shock wave that formed where air was pushed aside to make way for the nose of a high-speed projectile, the photos indicated two "fascinating new types" of shocks—one that had apparently built up as the fuselage and wings began pushing more air out of the way, and another near the trailing edge of the wing. In comparison with the size of the wing and body combination being studied, the disturbed area of air was now understood to be much larger than previously conceived. Whitcomb wondered if the sharp rise in drag occurring in transonic flight was caused by losses from the strong extra shocks. After all, this was the first time that these particular disturbances in the transonic flow field had been observed.

Whitcomb had his first clue to the area rule, but he did not yet know what to do with it. The conventional way to design high-speed aircraft was to follow Ernst Mach's advice. In the late nineteenth century Mach had shown that bullet-like shapes produced less drag in flight than any other known shape. Although no controlled, manned aircraft would attain that streamlined ideal—they required wings and a tail—designers of the first generation of supersonic aircraft still tried to mimic that shape as much as possible. As Richard P. Hallion, historian of supersonic aircraft, has explained: "They gave the fuselage a pointed nose, then gradually thickened the body—that is, increased the cross-sectional area—until the fuselage reached its maximum diameter near the middle." Only at the tail end did the designers begin to decrease the diameter of the fuselage.[41] This was the rule of thumb.

In November 1951 Langley put a systematic series of wing and body combinations through tests in the 8-Foot HST; models included swept, unswept, and delta wings, and bodies with various amounts of curvature in the region of the wing. The goal of the program was to evaluate the magnitude of the drag caused by the interference of the two shapes at transonic speeds. Results led Whitcomb to two important new ideas: (1) that variations in the shape of the fuselage, even small ones, could lead to pronounced changes in the drag of the wing, and (2) that in determining transonic drag, the drag of the wing and the drag of the body could not be considered separately; rather, the combination had to be considered as a whole, as a mutually interactive aerodynamic system.[42]

Eureka!

Beginning in college Whitcomb had made it his practice to leave some time each day just for thinking. As has often been the case in discovery of the unknown, it was precisely this type of freewheeling, looking-out-the-window contemplation that led him to the area rule. One day late in 1951, while sitting at his desk trying to figure out *why* the shock waves were so different than anyone had expected, suddenly in his mind's eye he "saw" air passing over a body at transonic speed in a different way. A moment more of this creative visualization and ... "Eureka, I've got it!"* He perceived that the ideal streamlined body for supersonic flight was not a function of the *diameter* of the fuselage alone, as the old rule of thumb had it; what really caused transonic drag rise was the *total cross-sectional area* of the fuselage, wings, and tail. Since wings added most to this area, designers could reduce drag significantly by tucking in or narrowing the fuselage where the wings attached and then expanding the fuselage at their trailing edges.[43]

What opened his mind's eye was a physical analogy made a few weeks earlier by Adolf Busemann during an in-house technical symposium. Busemann, who had been working on theoretical aspects of transonic flow for some time, had told the Langley crowd to work "as pipe-fitters." Aerodynamicists were accustomed to working with streamlines and streamtubes, the German scientist had reminded his audience. A common approach to theoretical analysis of airflow problems was to isolate the streamtube (i.e.,

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* The eureka phenomenon derives its name from an exclamation (*heurika*, meaning "I have found [it]") attributed to the ancient Greek engineer Archimedes (ca. 287–212 B.C.) on his discovery, while in his bath, of the method of determining the relation of weight to volume. Comic strips depict the eureka experience as a light bulb turning on over an inventor's head.

Richard T. Whitcomb examines a model based on his transonic area rule in the 8-Foot HST, April 1955.

bundle of streamlines) containing the object under study. Wind tunnel researchers should also visualize transonic problems in this way, Busemann said. Picture a problem in transonic aerodynamics, he urged, as "uniform pipes going over the surface of the configuration" being studied.[44]

That is exactly what Whitcomb was doing when he exclaimed "Eureka"—visualizing all the pipe lines affecting his swept wing and body combination. His mental plumbing job involved tinkering inside a transonic streamtube having a diameter greater than the wing span of the aircraft model. While imagining how the streamlines deviated as they passed across the nose, along the body, and finally up over the wings before reverting back to normal paths downstream, he got the idea that if air could be displaced less violently, the waves and drag would diminish, enabling his plane to pass more easily through the transonic zone. Specifically, he thought to pinch the waist of the fuselage so that streamlines which otherwise would be brushed aside sharply would have more room. The same amount of air had to get out of the way to make room for the plane, he knew, but if the plane had a trimmed-down "wasp waist," the air would not be displaced in such violent shock patterns.

After jumping up from his desk, Whitcomb took Busemann's concept of what was happening aerodynamically at high speeds, as well as "every available bit of [transonic] data that [had] ever been gotten before, by drop tests and so forth," and compared the two sources of information with the pile of unexplained results from the slotted tunnel. As Whitcomb recalls, "It's been said the proof of a new theory is whether it will [explain] all of those pieces of information that you've been trying to fit together, and it did."[45]

His colleagues, in particular Stack, his boss, were not so sure. Still, Stack allowed him to present his area rule at the next meeting of Langley's elite technical seminar—perhaps, as one NACA veteran believes, so Busemann and the lab's other great mathematicians could prove it wrong.[46] Busemann, however, did just the reverse:

> At the end of [Whitcomb's 20-minute] presentation there was silence. Finally, Adolf Busemann stood up. Turning to his colleagues, the pioneer of sweptwing technology remarked, "Some people come up with half-baked ideas and call them theories. Whitcomb comes up with a brilliant idea and calls it a rule of thumb."[47]

With Busemann defending the rule, the skeptics retreated at least temporarily. Stack reacted characteristically: he told Whitcomb to go prove it.

Verification and Application

The basis of the area rule concerned the cross-sectional areas of a wing and body combination. If these areas obeyed the rule by having the proper relationship to each other, the resulting shape should enjoy minimum transonic drag. To verify the rule experimentally, Whitcomb designed models with variously pinched waists and tested them in the 8-Foot HST. By the end of April 1952, enough data indicated that "very significant reductions in drag could be obtained by contouring the fuselage" for him to start writing a formal report confirming the theory.[48] The NACA published this paper, "A Study of the Zero Lift Drag Characteristics of Wing-Body Combinations Near the Speed of Sound," as Research Memorandum L52H08 in September 1952, and made it available immediately to American industry on a secret basis. By that time, however, at least one aircraft manufacturer—Convair—already had heard something about the area rule.

Recent data from Langley's now transonic 8-Foot HST had suggested that Convair's YF-102, a fighter-interceptor being readied for air force service in defense of the continental United States, could not fly supersonically as planned—the transonic drag was higher than expected. Naturally this information disturbed Convair greatly. The company's designers had given the plane a bullet-shaped fuselage, knife-edge delta wings, the most powerful jet engine in existence (the Pratt and Whitney J-57), and everything else that was currently thought essential for sustained supersonic flight. The company's production managers had set up an assembly line in San Diego for the manufacture of hundreds of F-102s. Now NACA test results indicated that Convair's best-laid plans for an honest-to-goodness supersonic fighter had been insufficient. The company had good reason to fear that the air force might cancel the contract.

In mid-August 1952, less than a month before the publication of Whitcomb's report, a visiting team of engineers from Convair witnessed the questionable performance of a scale model of the YF-102 in Langley's 8-Foot HST. Someone asked Whitcomb what might be done to reduce the air resistance, and in response he described his surprising discovery of a new rule of thumb concerning transonic drag.

The historical records do not make clear what if anything John Stack said to the Convair representatives about the area rule or anything else. However, a few subsequent memos from Stack's division suggest that Stack, either for fear of transonic theory or some personal reason, wanted the whole area rule business "put to bed."[49] This suggestion is supported by the recollections of several Langley veterans who knew Stack quite well. The company men flew home to California, taking the scale model with them for study. However, they were not totally convinced either by the theory or by the NACA's interpretation of the tunnel data that the YF-102 as originally contoured could not go supersonic in level flight.

Over the next several months Whitcomb worked with Convair to apply the area rule to the YF-102 configuration—which apparently means that Stack's skepticism stopped short of obstructionism. New wind tunnel tests, which began in May 1953, indicated far less drag but left room for improvement. Three months later, after more area-rule-based modifications, Whitcomb traveled to San Diego to help the company's aerodynamic department finalize its recontouring of the airplane. In October the NACA reported that Convair's modified aircraft, later designated the YF-102A, met the air force specifications for supersonic flight.[50]

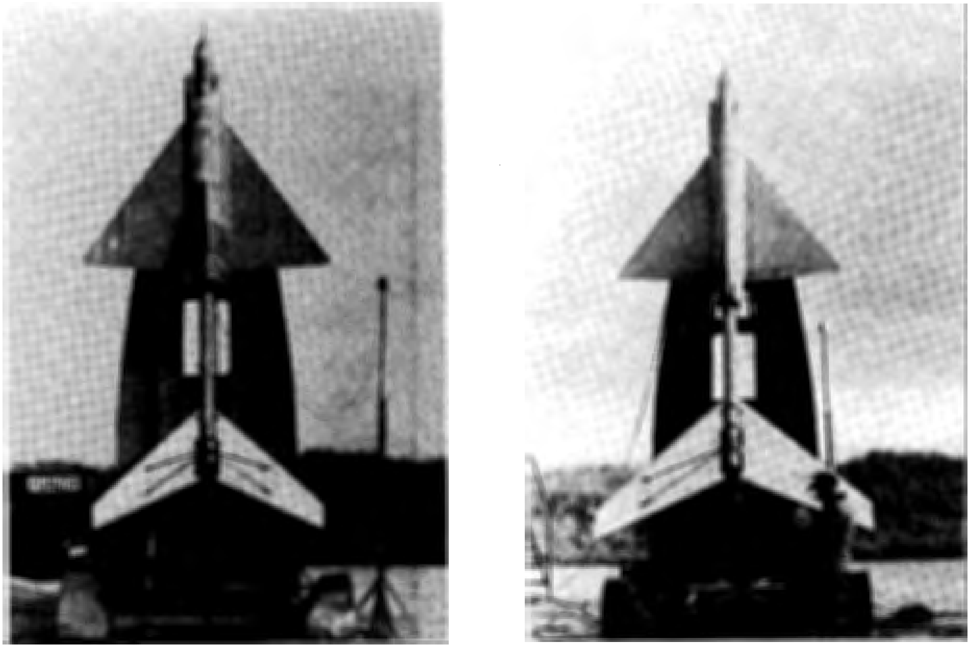
Still hoping that the YF-102 might fly supersonically, Convair had continued all the while producing the prototype. In late 1953 and early 1954 the plane was test flown, but its performance mostly confirmed the

An area-rule-based model of the Convair F-102 being readied for testing in the shop of the 8-Foot HST, November 1953.

NACA's pessimistic wind tunnel evaluation. Hating to delay receipt of the new aircraft but wanting it really to be supersonic, the air force called a halt to Convair's assembly line and advised the company to retool immediately for manufacturing the YF-102A.

Because Convair's designers had, with Whitcomb's help, already redesigned a model according to the area rule, it took less than seven months for a new prototype to be built. Besides the wasp-waist, the YF-102A was given a sharper nose and canopy, tail fairings, and a more powerful version of the J-57 engine. During a flight test five days before Christmas 1954, the YF-102A "slipped easily past the sound barrier and kept right on going."[51] The area rule had helped to increase the plane's top speed by an estimated 25 percent. Delighted with the superior performance, the air force eventually contracted with Convair for over 1000 F-102As. The advanced version of this model, designated the F-106 Delta Dart, flew as a vital part of the continental defense arsenal into the early 1980s.[52]

Whitcomb's discovery of the area rule was particularly timely. His eureka experience occurred at the very moment that virtually all military fighters aimed at sustained level supersonic flight seemed doomed to remain just below Mach 1 because of the incapability of the jet engines of the time to overcome the tremendous drag rise. No matter how skeptical Stack or anyone else might have been about the new theory, aircraft manufacturers stuck in this quandary had little choice but to explore the theory's potential



At Wallops Island in 1953, Langley's Pilotless Aircraft Research Division (PARD) tested rocket-powered models of the delta-winged Convair F-102 before (left) and after (right) application of the area rule.

applications. In large part their future depended upon it. Convair faced up to the problem, and so did Chance Vought (which redesigned its F-8U carrier-based interceptor according to the area rule), Grumman, and eventually Lockheed (in April 1956, its area-rule-based F-104 Starfighter was the first jet to exceed Mach 2 in level flight).

Convair may have heard about the concept first, but Grumman built the first area-rule-based aircraft to fly supersonically. Just two weeks after receiving a copy of Whitcomb's September 1952 report, Grumman, under contract to the navy for a supersonic carrier-based fighter, sent a delegation to obtain further information. In February 1953, five months before his trip to Convair, Whitcomb visited the Grumman plant at the company's request to see the final design layout of the area-rule-based F9F-9 Tiger before slotted-tunnel and rocket-model tests at transonic speeds. By the end of the summer, results confirmed that this layout would have low enough transonic drag for supersonic speeds in level flight. So Grumman built the plane. On 16 August 1954, the flashy white F9F-9 Tiger (later to be called the F11F-1) "breezed through sonic speed in level flight without the use of an afterburner, the first time this had been done."[53] Convair's improved F-102, the F-102A, took four more months to match the achievement.

One month later, a story on the successful application of the area rule to the Grumman Tiger appeared in *Aero Digest.* In the eyes of the military and the NACA, the editor was in violation of that journal's written commitment to withhold publishing anything about this work until the veil of secrecy had been lifted officially. The editor had upset what had theretofore been considered "an object lesson in how a military scientific secret can be kept effectively to gain a time advantage over international competitors."[54] This transgression would have been serious even if the age had not been that of Joseph McCarthy. The war might have been cold, but the United States was then engaged in hot technological competition with the Soviet Union for supremacy in the air.

One year later, in September 1955, the NACA released news of the area rule. The basic idea had been applied so widely among manufacturers and others that it made no sense to persist with secrecy. Articles praising the new aircraft design, dubbed for visible reasons the "Coke Bottle" and the "Marilyn Monroe," soon flooded the aviation journals and newspapers. Bold-faced headlines read: "NACA Formula Eases Supersonic Flight" (*Aviation Week*, 12 Sept. 1955); "Idea Called 'Major Key' To Supersonic Flights" (Newport News, Va., *Daily Press*, 12 Sept. 1955); "25 Per Cent Increase Made in Plane's Speed Beyond the Sonic Barrier" (*Daily Press*, 12 Sept. 1955); "The Area Rule: A Universally Applicable, Rule-of-Thumb Law for Transonic Design" (*Flight*, 30 Sept. 1955); and "How We're Beating the Russians through the Sound Barrier" (*Look*, 13 Dec. 1955).

Five weeks after the public announcement, the National Aeronautic Association awarded Whitcomb its coveted Collier Trophy for the greatest achievement in aviation in 1955. The citation read: "Whitcomb's area rule is a powerful, simple, and useful method of reducing greatly the sharp increase in wing drag heretofore associated with transonic flight, and which constituted a major factor requiring great reserves of power to attain supersonic speeds." As evidence of the value of the area rule, the citation asserted that the concept was being used currently in the design "of all transonic and supersonic aircraft in the United States."

Whitcomb continued to refine and extend his basic concept, not only for the design of supersonic bombers but also hopefully for future commercial jets. For several years he worked full bore, first under Stack and then under Laurence K. Loftin, Jr., on the problems of designing a supersonic transport to fly beyond Mach 2—that is, an SST. Eventually he left the frustrating new field and returned to transonics, where he knew he could make things pay off. Shortly thereafter, at Loftin's instigation, he began a new research project on the airfoil characteristics of a new vertical takeoff (VTO) design being studied by the Ling-Temco-Vought Company. In 1965, as a result of this investigation, Whitcomb conceived the *supercritical wing*, an airfoil shape whose primary attribute was improved performance at high subsonic speeds but which also proved to have excellent high-lift characteristics because of its rounded leading edge and sharply down-curved trailing edge.[55]

The Engineer as Artist

Some analysts qualify Whitcomb's discovery of the area rule by mentioning that the concept was *implicit* in earlier theoretical works. Alex Roland, for example, in *Model Research* asserts that Whitcomb merely "provided the engineering data that turned [the rule] into useful applications ... and pointed out the adjustments needed to get the [Convair F-102] through the sound barrier."[56]

It is true that flow studies over the wing root of a sweptback wing and fuselage combination had led the German aerodynamicist Kuchemann to design a fighter plane with a tapered fuselage as early as 1944. The American intelligence teams that discovered this development tagged it the "Kuchemann Coke Bottle." Also, beginning in 1946, two British researchers, G. N. Ward of the University of Manchester and W. T. Lord of the Royal Aeronautical Establishment, had taken a mathematical approach to the transonic drag problem that, seen in retrospect, could have provided clues to the area rule. In the late 1940s, so did a doctoral thesis by Wallace D. Hayes at Caltech.[57] But none of the forerunners recognized the potential of what they had. Perhaps because they reduced everything mathematically—which involves thinking with symbols—Ward, Lord, and Hayes had failed to *see*, as Whitcomb would, how to bring the physical elements together in a new aerodynamic combination. Whitcomb did not conceive the area rule by *reading* Hayes's theory, even if (despite his denials) he had already read the young man's Ph.D. thesis. He conceived it independently, thanks to a highly individual nonverbal process of thinking that involved seeing shapes and changing them, not interpreting symbols.

Whitcomb's introspective style of creativity was uncommon at Langley. Though he had a conservative, shy personality, he was a radical in the laboratory. In some respects, management did not know exactly how to deal with him. The best idea any of his supervisors came up with was to leave him alone except to help him through those administrative duties distracting him from what he really wanted to be doing. The best thing for a freewheeling mind, after all, was an open road.

12

Hypersonics and the Transition to Space

On 13 June 1944 Germany responded to the Allied invasion of Normandy by launching its first "Velgeltungswaffe Ein" (or "Vengeance Weapon No. 1") missiles against England, followed by its first strike of V-2s on London in September. Because they flew at speeds of up to Mach 5 (3400 miles per hour), the V-2 missiles were invulnerable to interception by even the fastest fighter planes. When the Allies captured the Baltic town of Peenemünde in the summer of 1945, technical experts discovered, among the various V-2 test facilities, a "super-supersonic" wind tunnel, which, though small (0.4-meter diameter), was operational—on an intermittent-flow basis—to Mach 5, as well as a larger, continuous-flow "super-supersonic" tunnel, which was under construction for a speed ten times that of sound. Nowhere else in the world were there high-speed tunnels like these two. Nazi engineers had built them for the purpose of testing long-range ballistic missiles, two of which (the A-9 and A-10) were planned for the aerial bombardment of the eastern United States.[1]

Though there was early debate inside the NACA and elsewhere about whether ballistic missiles would ever amount to much in a military sense, the psychological effect of news of Germany's technically astounding V-2s falling on English civilians, and the fear of the same thing happening to people in the United States, made it urgent that the American aeronautics establishment explore the awesome potential of the new technology. Langley laboratory responded in 1944 and 1945 by setting up three new groups to study the problems of guided missiles and rockets: (1) the Special Flying Weapons Team, (2) the Auxiliary Flight Research Station at Wallops Island, to provide free-flight data from rocket-propelled test models (in 1946, this station became nerve center of Langley's Pilotless Aircraft Research Division, or PARD), and (3) a new supersonics branch, to explore the many new compressible-flow problems brought to light by early design studies of supersonic aircraft.

By 1945 it had become usual in discussing compressible flows to subdivide the fields into subsonic, transonic, and supersonic regimes. These divisions were logically derived from the type of differential equation that described each regime. Experimental research in compressible flows followed such division naturally, not only because experimenters furnished the physical guidance for theoretical development, but also because in this particular field the principal experimental tool, the wind tunnel, had major limitations in the transonic regime.

There was no such clear-cut division yet between the supersonic and hypersonic ranges of flight. Generally speaking, aerodynamicists considered speeds above Mach 5 as hypersonic, since this was the supersonic speed at which aerodynamic heating seemed to become vitally important in aircraft design. Nor was there a clear-cut division between the experimental technologies of supersonic and hypersonic aerodynamics. Though it was clear to everyone by 1945 that subsonic and supersonic wind tunnels had to be designed very differently,* no one was yet sure whether supersonic and hypersonic tunnels could be designed similarly.

An 11-Inch Pilot Tunnel

One Langley researcher who was exploring the gray area between the supersonic and hypersonic speed ranges was John V. Becker, assistant chief of John Stack's Compressibility Research Division. On 3 August 1945 Becker proposed the construction of a "new type supersonic wind tunnel for Mach number 7.0." Though a few of the smaller supersonic wind tunnels then in existence in the United States were capable of a maximum test Mach number of about 4.0, Becker reminded his chief of research that the large supersonic wind tunnels now under construction at Langley and Ames had been designed for a maximum Mach number of only about 2.0, with provision, in the case of the Langley tunnel, for future modification to permit Mach number 3.0 to be attained. Considering what was known about Germany's ballistic missile program at Peenemünde, these plans were grossly inadequate, Becker declared. Since it was plain that all of these

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* There are three important engineering design differences between subsonic and supersonic wind tunnels. First, the test section of a supersonic tunnel is placed *downstream* from the narrowest part of its circuit instead of *at* the narrowest part, as is the case in a subsonic tunnel. Second, supersonic tunnels require powerful multistage compressors capable of increasing air pressure very dramatically in order to compensate for the large energy losses in the air circuit. Third, the air inside the circuit of a supersonic tunnel must be kept cleaner, that is, freer from oil, dust, and water vapor. See Donald D. Baals and William R. Corliss, *Wind Tunnels of NASA*, NASA SP-440 (Washington, 1981), pp. 49–50.

John V. Becker and his 11-inch hypersonic tunnel. The first successful run of the small facility, which was built in the shop of the old Propeller Research Tunnel, was in November 1947.

American tunnels would "be used, to a large extent, to develop supersonic missiles and projectiles of types which [had] already been operated at Mach numbers as high as 5.0," it appeared to Becker that there was "a definite need" for equipment capable of hypersonic test Mach numbers.

As the basis of his proposed design, Becker extrapolated from what he already knew about the proper design of supersonic tunnels. He knew, for example, that a Mach 7 tunnel would have to be fed with air through a smaller throat that expanded into a much larger test section, because the air beyond the nozzle diaphragm had to be accelerated so much more to go hypersonic.[*] He also knew that the power requirements for continuous-flow operation of a Mach 7 facility would be enormous. Equally enormous, he realized, would be the costs of the necessary compressor equipment.[2]

This knowledge, and the uncertainty that enveloped it, pointed Becker towards a *blowdown* type of tunnel with a four-foot-square test section "sufficiently flexible in design to permit easy modification." Mach 7

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[*] This was a more radical version of the *expanding nozzle principle*—which was the physical basis for originally achieving supersonic flow in a large wind tunnel. The energy pumped into a tunnel's airstream by a powerful multistage compressor, and stored in the forms of compression and heat energy, was converted thermodynamically to kinetic energy by the severe constriction and then sudden expansion of a tunnel's circuit. This conversion produced supersonic flow once the airstream had passed the point of smallest cross-sectional area. To provide for a range of test airspeeds, engineers designed the nozzle so that its shape could be varied systematically. Since the development of large supersonic tunnels in the 1940s, they have done this by using such things as interchangeable block nozzles and flexible nozzle walls. See Baals and Corliss, *Wind Tunnels of NASA*, pp. 50–52.

flow would be moved through the circuit of this tunnel, the Langley engineer suggested, by supplying air from a 50-atmosphere pressure tank and exhausting it through the tunnel into a vacuum tank. (Thus the name *blowdown.*) With high pressure on one side and very low pressure on the other, tunnel operation could be maintained for about 90 seconds. Becker advised the NACA to construct a pilot model with an 11-inch test section and to operate it for a period of time to test the design before building the actual tunnel.[3]

Some at NACA headquarters and at Langley had reservations about building Becker's hypersonic tunnel. Jerome Hunsaker, the NACA's chairman, did not see the practical urgency of such a facility at the time, and Arthur Kantrowitz felt that wind tunnels could not go beyond Mach 4 or 5 because of their serious liquefaction (condensation of oxygen plus nitrogen) problem. But since the initial cost of the proposed pilot facility was relatively low ($39,500, compared with $350,000 for the actual tunnel), the opposing forces yielded to persuasion and the small 11-inch hypersonic tunnel was approved for construction.[4]

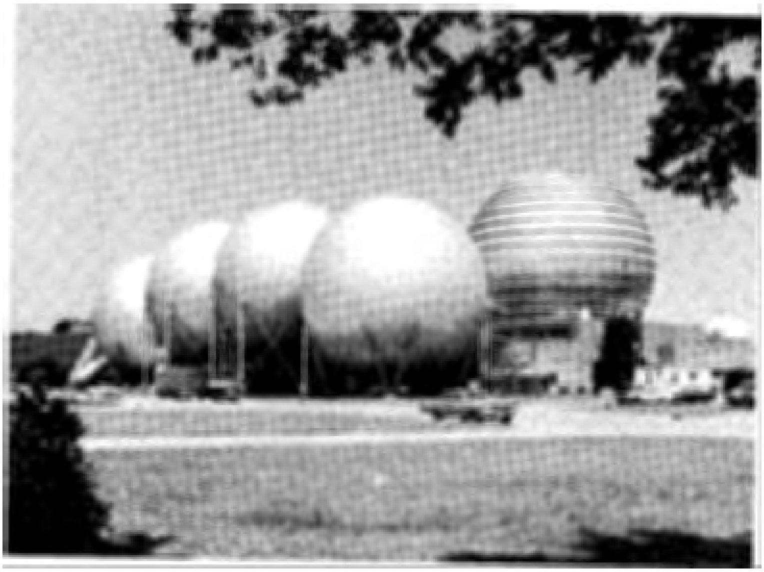
Becker immediately got together a design group headed by C. H. McLellan to do the job. This group soon discovered the truth of what Kantrowitz had foreseen: the job required more than extrapolations from supersonic tunnel design. During preliminary studies in the pilot facility, as tunnel air accelerated from supersonic to near-hypersonic velocities (at about Mach 4.5), and the air's latent heat transformed into energy of motion, the temperature in the test section dropped so low that the air in it liquefied. Experience with condensation in Langley's 9-Inch Supersonic Tunnel suggested that considerable "supersaturation" would probably exist in the air of a hypersonic tunnel; that is, the air would be far more dense with water vapor than normally. But no one knew for sure. (Some of Langley's theoreticians argued, in fact, that liquefaction should not even be occurring and would not occur once the tunnel achieved hypersonic airflow.) This uncertainty as to what would actually happen in hypersonic airflow led Becker's team to incorporate an electric heater in front of the test section of the small pilot tunnel. In November 1947 the 11-inch tunnel operated satisfactorily up to a speed of Mach 6.9—the first operation of a hypersonic tunnel in the United States.[5] Thanks to the heater, the air temperature in the tunnel's settling chamber was high enough above saturation values to prevent liquefaction of the expanding air in the nozzle. The heater also enabled Langley's hypersonics experts not only to study the effects of heat in connection with the condensation phenomenon, but also to study heat transfer (i.e., the exchange of heat by radiation, conduction, or convection within a substance and its surroundings), knowledge of which was vital in the design of supersonic aircraft and missiles. Engineers responsible for testing in the 11-inch tunnel soon developed an accurate technique of measuring heat transfer which they applied in a host of basic studies and configuration analyses.[6]

Experience with the 11-inch tunnel suggested to Langley engineers that hypersonic tunnels using the intermittent blowdown scheme were preferable to the continuous-flow type tunnel, which, because of the necessary compressor equipment, would be extremely costly.[7] With sophisticated recording instruments, short-duration test runs were sufficient. The 11-inch tunnel would itself achieve a remarkable record. Built as a pilot model for Becker's planned larger hypersonic tunnel—which was built some fifteen years later as the Continuous-Flow Hypersonic Tunnel—it operated for twenty-five years until 1973 when it was finally dismantled and given to the Virginia Polytechnic Institute in Blacksburg, Virginia, for educational uses. At least 230 publications resulted from tests and related analyses in the 11-inch tunnel—or about one paper every five weeks for the twenty-five years. Few major wind tunnels designed for data production can equal this record.

Though the 11-inch tunnel was a great success, Langley management knew that there were too many basic aerodynamic, heating, and fluid-mechanical problems present in the hypersonic speed range to attack them all systematically in the single research facility. In 1947 John Stack proposed the design of an additional hypersonic facility that was radically different. Stack's idea was to use a single large spherical vessel (some hundred feet in diameter) with an array of blowdown jets located underneath. On demand, hot pressurized air could be parceled out in short bursts from this central source to individual test cells of small size (20 inches in diameter).[8] Different teams of Langley researchers could then conduct diverse experiments without tying down a tunnel for days or weeks.

As preliminary design studies progressed, Langley engineers found it more feasible and economical to reconfigure Stack's concept into a "farm" of many small high-pressure tanks—some of them salvaged from submarines. This Gas Dynamics Laboratory came into operation in 1951. It contained several different supersonic and hypersonic nozzles which together were capable of covering the speed range from Mach 1.5 to Mach 8.0. Work in this laboratory ranged from routine testing of scale-model aircraft components to esoteric basic studies in magneto-plasma-dynamics (the study of the interaction between a magnetic field and an electrically conducting fluid).[9]

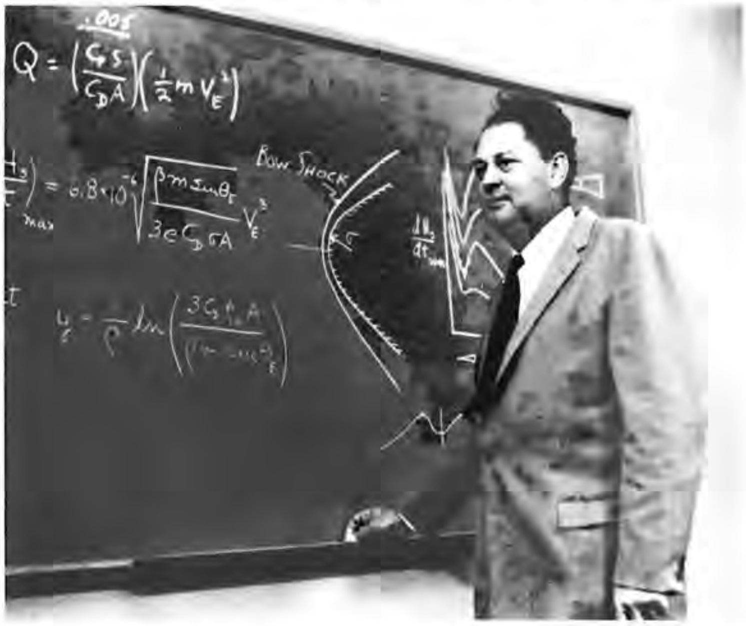
The first priority of hypersonics research at Langley in the late 1940s and early 1950s was to solve the major problems of the various long-range missiles then being developed by the American military and its contractors.



In the Gas Dynamics Laboratory, completed in 1951, researchers explored basic aerodynamic, heating, and fluid-mechanical problems in the speed range from Mach 1.5 to Mach 8.0.

This was true also at Ames, where important missile-related research also began after the war.[10]

Long-range missile development challenged NACA researchers in a number of ways. A successful intercontinental ballistic missile would have to be accelerated to a speed of 15,000 miles per hour at an altitude of perhaps 500 miles and then guided to a precise target thousands of miles away. Sophisticated and reliable propulsion, control, and guidance systems were thus essential, as was the reduction of the structural weight of the missile to a minimum. Moreover, some method had to be found to handle the new and complicated technical problem of aerodynamic heating. As one of these nuclear-weapons carriers arched over and slammed back into Earth's atmosphere, the air around its nose—which carried the warhead—heated up to tens of thousands of degrees, hotter than the surface of the sun. The part of this heat generated outside the boundary-layer surface by shock-wave compression, and which was not in contact with the missile structure, dissipated harmlessly into the surrounding air; but the part that arose within the boundary layer, and which was in contact with the missile structure, was great enough to melt the missile. Many dummy warheads burned up because they were unprotected from the effects of aerodynamic heating.



In 1951 H. Julian "Harvey" Allen, chief of high-speed research at the NACA's Ames laboratory, predicted that the aerodynamic heating problems of certain missiles and reentry vehicles could be avoided by changing their nose shapes from sharp to blunt.

In 1951 Harvey Allen, top man in high-speed research at Ames and former Langley employee in Eastman Jacobs's VDT group (1936 to 1940), found a practical solution to the serious aerodynamic heating reentry problem of the ICBM. In place of the traditional sleek rifle-shell configuration with a sharply pointed nose, an aerodynamic concept long since firmly implanted in the minds of missile designers, Allen proposed a "blunt-body" shape—familiar to us all now because of the rounded bottom side of the Mercury, Gemini, and Apollo space capsules, but a strange idea at the time. The blunt shape, when reentering the atmosphere, would force the buildup of a powerful bow-shaped shock wave, Allen predicted. The shape of this shock would deflect heat safely outward and away from the structure of the missile.[11]

Allen and his associate Alfred J. Eggers verified the blunt-body concept by studying the motion and aerodynamic heating of miniature missiles in an innovative supersonic free-flight tunnel, a sort of wind tunnel-cum-firing range which had become operational at Ames in 1949. Their report on these tests was published in August 1953 as a classified Research Memorandum;[12] however, industry did not pick up on the blunt-body idea very quickly. People accustomed to pointed-body missiles remained skeptical of the revolutionary blunt-body principle until the late 1950s, when the principle became crucial for missile design and for the design of the future blunt reentry capsules of the Mercury, Gemini, and Apollo programs.[13]

In June 1952 the NACA Aerodynamics Committee recommended that Ames and Langley laboratories increase their emphasis on hypersonics research. This recommendation was partly a response to hearing first word of Allen's unanticipated discovery of the blunt-body concept; it was also partly a response to a special request from a group representing eleven guided missile manufacturers. The NACA Subcommittee on Stability and Control had invited this group to Washington in June 1951 to present its ideas "on the direction in which NACA research should move for greatest benefit in missile development." During the meeting, a representative of the Douglas Aircraft Company (which was busily engaged in the development of the Sparrow and Nike missiles) suggested that, because of the contemplated increase in the speed of interceptor aircraft to Mach 3, the NACA should begin immediately to explore the problems missiles were bound to encounter in the speed range from Mach 4 to Mach 10.[14]

Most importantly, however, the recommendation reflected new interest in hypersonic aircraft stirred up in the NACA by a recent letter to the Committee from Robert J. Woods, designer of the X-1, X-2, and X-5 aircraft for the Bell Aircraft Corporation. In a letter of 8 January 1952, Woods, a former Langley employee (1928 to 1929), proposed that the Committee direct some part of its organization to address the basic problems of hypersonic and space flight. Accompanying his letter was a document from Walter Dornberger, formerly commander of the German rocket test facilities at Peenemünde and now employed by Bell, outlining the design requirements of a hypersonic aircraft. Dornberger was still intrigued by an elaborate concept for an "antipodal" rocket plane which had been proposed near the end of the war by his colleagues Eugen Sänger and Irene Bredt. This "winged V-2," according to the Sänger-Bredt study, would skip in and out of the atmosphere to drop its payload and land halfway around the world.[15] Dornberger's enthusiasm for the Peenemünde concept had captured Woods's imagination. As a final recommendation, the Bell engineer called for the NACA to define and seek to procure a manned research airplane capable of penetrating the hypersonic flight regime.[16]

The June 1952 recommendation by the NACA Aerodynamics Committee to accelerate exploratory hypersonics investigations "had little immediate effect on existing Langley programs, with the exception that it inspired the PARD to evaluate the possibilities of increasing the speeds of their test rockets up to Mach 10."[17] But the recommendation did have one very important consequence for the future. In the final paragraph of the recommendation, the NACA called for its laboratories "to devote a modest effort" to the study of the speed range *beyond* Mach 10 to the speeds of *space* flight.

The Brown-Zimmerman-O'Sullivan Study

In response to the recommendation of the Aerodynamics Committee to begin exploring concepts for high-altitude hypersonic flight, Langley management set up an ad hoc three-man study group. The group consisted of Clinton E. Brown, chairman, from the Compressibility Research Division; Charles H. Zimmerman, from the Stability and Control Division; and William J. O'Sullivan, Jr., from PARD. Curiously, none of the three had any significant background in hypersonics. Floyd Thompson, who became associate director of Langley lab in September 1952, had rejected a suggestion to include one of the lab's few hypersonic aerodynamicists or specialists in "hot structures" in the study group. Thompson's plan was to bring together creative engineers who could bring to the subject "completely fresh, unbiased ideas." Brown, Zimmerman, and O'Sullivan quickly educated themselves in hypersonics, asking Langley's experts for help when they needed it. The study group met periodically for the next several months. In late June 1953, Langley circulated internally the group's report, "A Study of the Problems Relating to High-Speed, High-Altitude Flight."[18]

Langley had asked Brown, Zimmerman, and O'Sullivan to assess hypersonic problems and to develop research program ideas, but the trio chose to go further. After reviewing the potentialities of hypersonic systems at speeds up to orbital, the three researchers—all of whom had read the Woods-Dornberger documents—had become especially interested in defining a manned research airplane capable of penetrating the hypersonic flight regime, as well as in the commercial possibilities of that type of plane for long-range transport. The hypersonic airplane would be designed to fly to the limits of the atmosphere, then be boosted by rockets into space, returning to Earth by gliding under aerodynamic control. Rand and Convair had by this time done some preliminary studies of "boost-glide" rockets in connection with their development of ICBMs;[19] however, the scheme of the Brown group to incorporate such a system into an experimental airplane was one that no one had yet explored.

Originally, the NACA's plan was to have an intercenter board review the findings of the Brown study group, but this was never carried out. Langley's hypersonics specialists did get a chance to talk frequently with Brown, Zimmerman, and O'Sullivan, of course, and in June 1953 to

The Brown hypersonic study group of 1952: Clinton E. Brown (top left) had worked with Eastman Jacobs during World War II on the Campini jet propulsion system and with Macon C. Ellis, Jr., in the Model Supersonic Tunnel studying the feasibility of the supersonic ramjet. Langley had recently named him head of the supersonic aerodynamics section in the Gas Dynamics Branch. Charles H. Zimmerman (center) had supervised tests during the mid-1930s in Langley's spin tunnel and had designed the lab's first free-flight tunnel. In 1937 he moved to a job with Chance Vought, where he was in charge of developing the F5U short-takeoff-and-landing (or STOL) aircraft. Zimmerman returned to Langley in 1948. The field of his greatest aerodynamic expertise was the stability of radically new and different aircraft. William J. O'Sullivan, Jr., (right) had worked at Langley since 1938. In the early days on Wallops Island, he had taken on the job of developing PARD's rocket motor capabilities and rocket firing procedures.

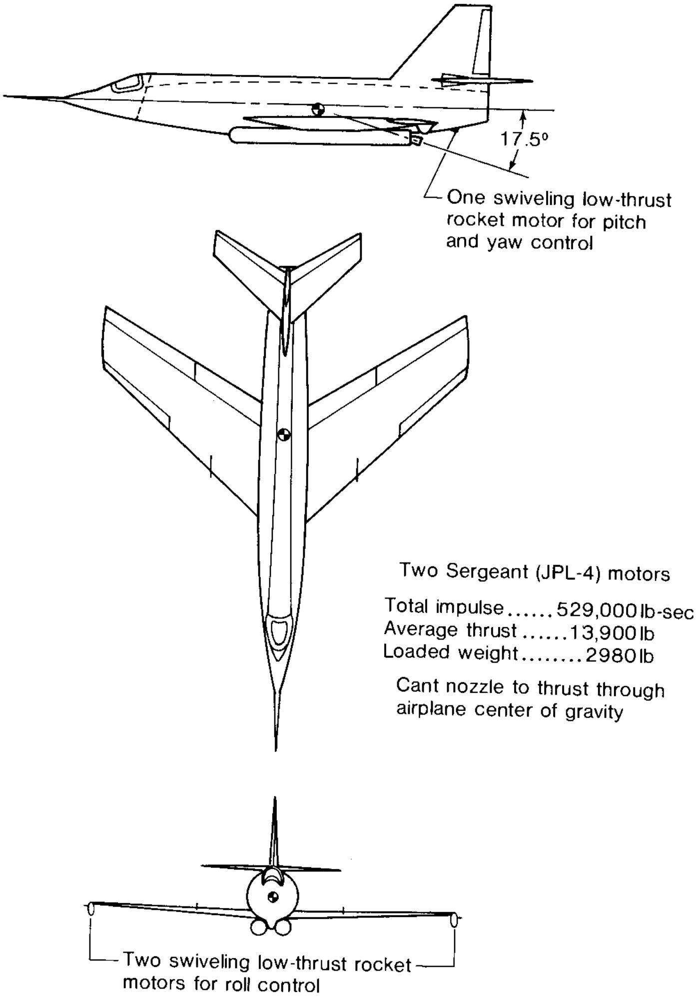
hear Brown formally summarize his group's findings. When listening to this summary the specialists "felt a strong sense of *déjà-vu*," especially at Brown's pronouncement that "the main problem of hypersonic flight is aerodynamic heating." They disagreed, however, with the group's conclusion that the NACA would have to rely on flight testing, rather than on ground-based approaches, for research and development beyond Mach 4.[20]

Brown, Zimmerman, and O'Sullivan had 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 temperature.[21] Anticipating significant differences between the "hot" aerodynamics of hypersonic flight and the "cold" aerodynamics of ground experimental technology, they "indicated that testing would have to be done in actual flight where the true high-temperature hypersonic environment would be generated." According to John V. Becker, "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. To do this, Brown's study group recommended extending the rocket-model testing technique of the PARD at Wallops Island to much higher speeds. Perhaps, they suggested, it would be possible to recover the test models in the Sahara Desert of northern Africa.[22]

Here, again, was a case at Langley of free-flight versus wind tunnel advocacy, similar to the debate that occurred in 1944 and 1945 over Gilruth's development of the controversial wing-flow technique. Ground facilities could not simulate the high-temperature environment of flight at very high Mach numbers, admitted the hypersonics specialists, but wind tunnels like the pilot 11-inch facility at Langley and the 10 × 14-inch continuous-flow facility at Ames had proved quite capable of "partial simulation."[23] Selective flight testing of the final article was desirable—just as it always had been—but, for the sake of safety, economy, and systematic parametric investigation of details, the hypersonics specialists argued, ground-based techniques must remain the primary tools of aerodynamic research.

Concepts for a Hypersonic Research Airplane

In January 1952 NACA headquarters sent copies of the Woods-Dornberger documents to its different research staffs. At the NACA High-Speed Flight Station (HSFS) at Edwards Air Force Base (formerly Muroc) in California, these documents stimulated an unsolicited proposal for a large



David G. Stone's proposed modifications to the Bell X-2 for flight into space, May 1952.

supersonic airplane which would launch, at Mach 3, a small manned second-stage vehicle to accelerate to hypersonic speeds.[24] At Langley, responsibility for evaluating these papers was given to David G. Stone, head of PARD's Stability and Control Branch. Within a few months Stone also submitted an unsolicited proposal for a hypersonic test vehicle. His idea was to equip the Bell X-2 research airplane with reaction controls and add two droppable solid rocket motors as boosters.[25] With such booster rockets, Stone claimed, an air-launched X-2 could be flown at a speed of about Mach 4.5 to orbital altitude.

Before formally submitting the findings of their study group to the NACA in July 1953, Brown, Zimmerman, and O'Sullivan had carefully examined Stone's research airplane proposal as well as the one from the HSFS for a supersonic carrier. The three men concluded that the Stone proposal was the more practical, and they endorsed it for further engineering study. This study proceeded rather leisurely for the next several months until October 1953, when the Aircraft Panel of the Air Force Scientific Advisory Board, of which Langley's Robert Gilruth was a member, pronounced that "the time was ripe" for looking into the feasibility of procuring a manned hypersonic research airplane.

In response to this pronouncement and to news of progress on Stone's proposal to modify the X-2 for hypersonic flight, Hartley A. Soulé, chairman of the interlaboratory NACA Research Airplane Panel, called for a meeting to be held in Washington on 4 and 5 February 1954. During this meeting, Soulé's panel (which consisted at the time of Charles J. Donlan from Langley, Lawrence A. Clousing from Ames, Walter Williams from HSFS, W. Fleming from Lewis, and Clotaire Wood from NACA headquarters) rejected Stone's idea. The X-2 was too small to use for hypersonic research, the panel declared. What was needed, it said, was a completely new and larger vehicle built specifically for hypersonic research extending into the upper atmosphere and into space itself.[26]

In the late 1940s and early 1950s the overwhelming majority of aeronautical engineers thought very little about manned space flight. Creating an efficient and safe supersonic airplane was difficult enough for them. Hypersonic flight, if it proved feasible at all, they thought, 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."[27] In just a few short years, however, thinking changed. By 1954, a growing number of American aeronautical experts felt that hypersonic flight extending into space could be achieved much sooner, though very few of them had the foresight to see it coming, as it actually did, by 1960. The military had gotten involved in supporting future-directed hypersonic research and development. In 1952, for example, the air force had decided to sponsor a study of Dornberger's manned hypersonic rocket-launched glider concept at Bell (Project BOMI). This study advanced and improved the Sänger-Bredt concept by developing, for the first time, a detailed "hot structures" concept. Non-load-bearing flexible metallic radiative heat shields ("shingles") and water-cooled leading-edge structures were to protect the wings while passive and active cooling systems would keep cabin temperature within human tolerance. NACA research sections, including the Brown study group, read the periodic progress reports of the

Bell study—classified secret by the air force—with great interest.[28] In response to the recommendation of Soulé's Research Airplane Panel, NACA headquarters told its field installations to explore the requirements of a possible hypersonic research airplane. In addition to answering questions about stability, control, and piloting, which had been the concerns of previous research airplane designers, this vehicle would be designed to fulfill a major new objective: it would have to provide new information about high-temperature aerodynamics and structures.[29]

NACA headquarters' directive prompted each installation to establish a special group of researchers to investigate different systems. A comparison of the work of these different NACA groups is illuminating 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 to 5 range. The HSFS group at Edwards suggested a larger, higher-powered conventional configuration generally similar to the Bell X-1 or Douglas D-558-I research airplanes it was familiar with. The staff at the Lewis Flight Propulsion Laboratory in Cleveland recommended *against* a new manned research aircraft, arguing that hypersonic research could and should be done by expanding the Wallops Island rocket-model technique. It reminded the NACA that previous research airplane programs had been unduly burdened by anticipated military applications; there was no reason to think that anything different would happen in the case of a cooperative hypersonic research aircraft program.[30]

The intentions and conclusions of Langley's hypersonic aircraft study group of 1954 (the successors of the Brown committee) differed substantially from those of the groups at the other three facilities. The original intent of the Langley group was to determine the feasibility of a hypersonic aircraft capable of a short (two- to three-minute) excursion out of the atmosphere into space. The idea was to create a brief period of weightlessness in order to explore its effects on space flight. Hugh Dryden, NACA director of research, would later liken this excursion to the leap of a fish out of water.[31]

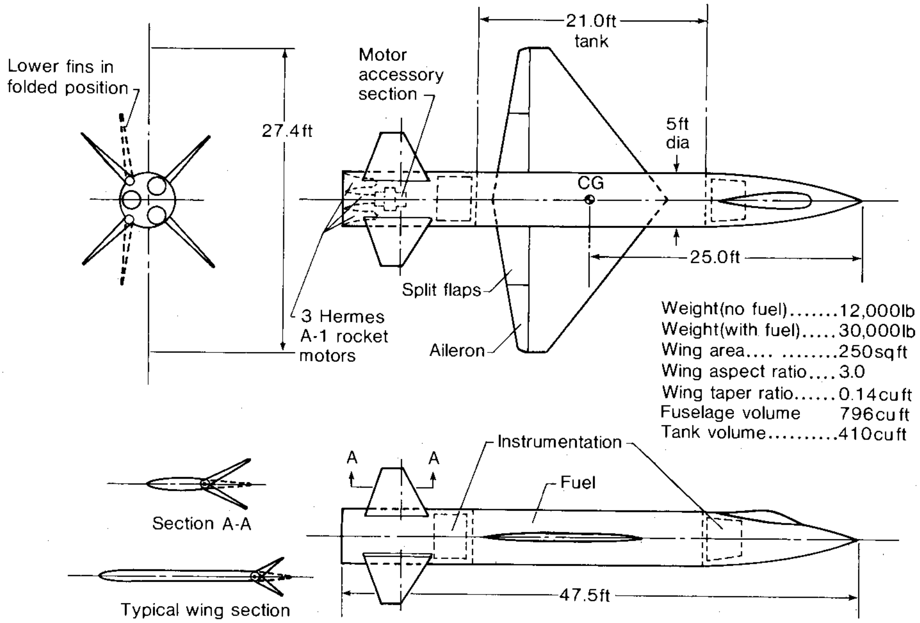
Langley's ad hoc hypersonic aircraft study group consisted of John V. Becker, chairman, chief of the Compressibility Research Division and principal designer of the lab's pilot 11-inch hypersonic tunnel; Maxime A. Faget, a specialist in rocket propulsion from the Performance Aerodynamics Branch of PARD; Thomas A. Toll, a configuration and control specialist from the Stability Research Division; Norris F. Dow, a "hot structures" expert from the Structures Research Division; and James B. Whitten, test pilot. Unlike the Brown study group, this group obviously included some researchers with previous experience in hypersonics.

Becker's group reached a consensus on the objectives of a hypersonic research aircraft by the end of its first month of study. Though study of the effects of weightlessness was the group's original goal, 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." Becker, Faget, Toll, Dow, and Whitten each began to consider the dynamics of the reentry maneuvers (and the associated problems of stability, control, and heating) as the most pressing research need.[32]

By the end of April 1954, Becker's group finished a tentative design of the winged aircraft it had in mind, as well as an outline of proposed experiments. The group had kept the configuration as conventional as possible—on the grounds that it would minimize the need for low-speed and transonic research and development—without endangering its adequacy as a vehicle for the aerodynamic and structural experiments contemplated for hypersonic flight. In the absence of the rapid development of a major new engine, propulsion to hypersonic speed was to be provided, according to the tentative design, by a combination of three or four smaller rocket motors. (None of the larger missile engines then under development was thought by the Becker group to be satisfactory.) Launch of the aircraft would be by the proven air-drop method developed at the HSFS for the XS-1 and refined during the flight test programs of the subsequent research airplanes.[33]

At this point Floyd Thompson, Langley's associate director, significantly influenced the direction of the Becker study. He made a suggestion that echoed John Stack's 1945 recommendation that Bell's XS-1 transonic research airplane have a 12 percent thick wing that would force it to encounter exactly those drastic compressibility problems that aerodynamicists were most interested in studying. Given that Thompson had opposed Stack's 1945 idea (see chapter 10), the similarity of his own 1954 idea seems ironic: 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] heating and to seek fresh design approaches rather than makeshift modifications to conventional designs." His suggestion became policy. Only the best available state-of-the-art materials could be used in the design of the aircraft, however, if procurement time was to be kept reasonably short.[34]

While performing the original heating analysis of the proposed aircraft's reentry from space, Becker and co-worker Peter F. Korycinski from the Compressibility Research Division ran head-on into a major technical problem. At Mach 7—the critical speed coming back from orbit—reentry at low angles of attack appeared impossible because of disastrous heating



Preliminary design configuration for a hypersonic research airplane by Becker's pre-X-15 study group, April 1954.

loads. (The dynamic pressures would quickly exceed by large margins the limit of 1000 pounds per square foot set by structural demands.) New tests in the 11-inch hypersonic tunnel of the force relationships provided Becker and Korycinski with a clue to a solution of this problem: if the proposed hypersonic vehicle's angle of attack and associated drag were increased, deceleration would begin at a higher altitude. Slowing down into the thinner (low-density) atmosphere would make the heat transfer problems 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 time of exposure to peak dynamic pressure and high heating rates. Thus, by using "sufficient lift," the Langley researchers had found a way to limit the heat loads and heating rates of reentry.[35]

On reflection it became clear to the Becker group that the "sufficient lift" idea was a "new manifestation" of Harvey Allen's blunt-body principle and that Allen's principle was as applicable to high-lift winged vehicle reentry as to the nonlifting missile cases he had studied at Ames in 1952. As the group increased the angle of attack of its vehicle in order 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 more and more "blunt." Some form of dive-brake structure could also be employed, again in accord with Allen's concept, to increase drag and further ease the heating problem generated by high lift-drag ratio, the group suggested.[36]

Throughout 1954 the heating problems of high-lift, high-drag reentry earned more and more consideration from key Langley researchers. Another problem outweighed the heating consideration, however: making the configuration stable and controllable in the necessary high-angle-of-attack reentry attitude (which was 11 to 26 degrees above horizontal, meaning that the descending craft's nose would be pointing upward by that amount). In the first stage of its design study, Becker's group came up with a vehicle concept that was really "little more than an object of about the right general proportions and the right propulsive characteristics to achieve hypersonic performance."[37] The planners did not know the exact hypersonic and control properties of such an arrangement; no one in aeronautics knew. Nor did anyone else know, for that matter, whether a structure could even be found that could survive the anticipated air temperatures (estimated at approximately 4000 degrees Fahrenheit) affecting a winged vehicle during reentry. On the other hand, everyone did know that before the NACA would propose the procurement of a radical new research aircraft, it had to have solid answers to the stability and control question.

The NACA's High-Speed Flight Station had forewarned Langley of the difficult problems of hypersonic stability. In December 1953 Maj. Chuck Yeager, USAF, pushed the Bell X-1A far beyond its normal transonic speed range to a speed of about Mach 2.5. (Wind tunnel tests of the X-1A had extended only to Mach 2.) As the experimental aircraft approached this speed, it developed large and completely uncontrollable lateral oscillations which nearly proved disastrous. While Yeager tried frantically to regain control, the airplane dived for over a minute, losing nearly 11 miles of altitude. At subsonic speed, the plane finally went into a spin from which Yeager managed a recovery. At Langley, this incident led to a systematic wind tunnel reinvestigation of the stability characteristics of the X-1A. By mid-1954 findings indicated that the life-threatening directional difficulties of Yeager's plane were almost certainly caused by the loss of lifting effectiveness of the X-1A's thin stabilizing surfaces as overall speed advanced higher within the supersonic regime.[38] (In September 1956, air force test pilot Capt. Milburn G. Apt would be killed in a crash of the X-2 rocket plane into California's Mohave Desert. The cause of this tragedy was similar to the cause of Yeager's 1953 near-disaster in his X-1A.)

The Becker group faced a hypersonic stability problem that was a number of times more severe than that of the X-1A—after all, it was designing an airplane not for Mach 2.5 but for Mach 7! Preliminary calculations based on data from the new X-1A tunnel tests indicated that the hypersonic configuration would require a vertical tail the size of one of the wings to maintain directional stability; but a tail of that magnitude was impractical. Stumped by this huge problem, Becker sought the advice of his 11-inch hypersonic tunnel researchers. One of them, Charles H. McLellan, suggested changing the thin airfoil section of the tails, used conventionally in the design of surfaces for supersonic aircraft since the mid-1940s, to a thicker wedge-shaped section having a more blunt leading edge. Some time before, he had made a special study of the influence of airfoil shape on *normal-force* characteristics; his findings had been lying dormant in the NACA literature. The calculations based on the findings of the previous study that McLellan now made for Becker indicated that, at Mach 7, the wedge shape "should prove many times more effective than the conventional thin shapes optimum for the lower speed."[39] By modifying the configuration in only this one detail, McLellan felt that the anticipated directional instability could be avoided.*

A new experimental program in the 11-inch tunnel verified the predicted effectiveness of McLellan's scheme. It indicated that a tail with a large (ten-degree) wedge angle would expand the ability of the proposed aircraft to achieve the range of attitudes (required by heating considerations) for a safe high-drag, high-lift reentry; furthermore, it suggested that a variable-angle X-shaped tail would help this (or any other) higher-speed supersonic airplane to recover from *divergent* maneuvers (i.e., those that caused deformation of lifting surfaces or other bodies as a result of aerodynamic loads being greater than elastic restoring forces, thus producing instability).[40]

In deciding to add the X-tail to its configuration, the Becker group recognized that the design modification itself would present at least one major new problem: the wedges of the experimental X-tail projected into the high *downwash* regions both above and below the wing plane, causing a potentially serious loss of longitudinal effectiveness.† Wind tunnel tests

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* McLellan had outlined the findings of his original study in an "Investigation of the Aerodynamic Characteristics of Wings and Bodies at a Mach Number of 6.9," a paper he presented at an NACA conference on supersonic aerodynamics held at Ames laboratory in early 1950. A version of this paper appeared in the October 1951 edition of the *Journal of the Aeronautical Sciences* (vol. 18, no. 10, pp. 641–48).

† *Downwash* is a small velocity component in the downward direction which 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

began immediately at Langley to solve this new problem. In a few months, by late 1954, the lab had an engineering answer: locate the horizontal tail somewhere else besides far above or well below the wing plane—the locations which had been used conventionally in transonic and supersonic designs. Experimental data said to place the horizontal tail *in* the plane of the wing, between the regions of highest downwash.[41] (In the final design of the X-15, North American Aviation would place the horizontal tail just slightly below the wing plane.)

Up to this point in 1954, the history of Langley's work to develop the concept of a hypersonic research vehicle primarily demonstrated one thing: the need for flexibility. Since inception, the Brown and Becker groups had run into one major technical problem after another in the pursuit of a hypersonic aircraft capable of a "space leap." Conventional wisdom had provided experimental and theoretical guidelines for preliminary design of the configuration, but had fallen far short of giving final answers. The conventional wisdom of transonic and supersonic aircraft design had dictated that a horizontal tail surface be located far above or well below the wing plane, for example, but that wisdom was apparently wrong for hypersonic conditions. Ballistics experts committed to sharp-nosed missiles had continued to doubt the worth of Allen's blunt-body principle, but they too were wrong. Conversely, the instincts of Floyd Thompson, who knew very little about hypersonics but who was a 30-year veteran of the ups and downs of aeronautical research, had been sound. The design and research requirements of a hypersonic vehicle which could possibly fly into space were so radically new and different, Thompson had suggested, that only "fresh approaches" could meet them.

The North American X-15

By the end of June 1954, after three months of long and pressured work days, the Becker group reached a stage where it felt it could make a convincing case for the feasibility of a Mach 7 research aircraft. Those at NACA headquarters who followed the progress of their work, as well as of the parallel work on hypersonic aircraft concepts being done at the other NACA centers, agreed. It was time for the military to listen to a unified NACA presentation.

Representatives of the navy and the Scientific Advisory Board of the air force assembled at NACA headquarters on 9 July 1954 for what became

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the yaw (or side-to-side movement) of the lower tails relative to the upper tails, causing directional instability. See Charles H. McLellan, "A Method for Increasing the Effectiveness of Stabilizing Surfaces at High Supersonic Mach Numbers," NACA RM L54F21, Aug. 1954.

the first of many presentations on the possible new research vehicle. Hugh Dryden opened the meeting by outlining why he thought a hypersonic aircraft was now desirable. Hartley Soulé, chairman of the NACA's Research Airplane Panel, then reviewed the history of the cooperative research airplane programs in the most favorable terms possible, and Walter Williams, chief of the High-Speed Flight Station, summarized recent activities at Edwards Air Force Base. When Soulé and Williams finished, Becker and John E. Duberg—chief of the Structures Division, who was substituting for N. F. Dow—presented the results of the Langley study. The meeting concluded with agreement that the NACA should circulate a document setting forth the proposed details of a Mach 7 airplane to appropriate parties in the military and industry.[42] Three months later, on 5 October, the NACA Aerodynamics Committee met in executive session at the High-Speed Flight Station. (It had met in regular session the day before at the Ames lab, Moffett Field, California.) The purpose of the executive session was to come to some final decision about the desirability of a manned hypersonic research airplane.

The session at the desert air base more or less followed the plan of the earlier Washington meeting, but the atmosphere was in more ways than one hotter than that of the Washington meeting. First, De Elroy Beeler of the HSFS staff discussed some of the more general results obtained previously with the various research airplanes. Then Milton B. Ames, the committee's secretary, distributed copies of a secret document entitled "NACA Views Concerning a New Research Airplane." Langley's associate director, Floyd Thompson, reminded the Aerodynamics Committee of the major conclusion expressed by the Brown-Zimmerman-O'Sullivan study group in June 1953: that it was impossible to study certain salient aspects of hypersonic flight at altitudes between 12 and 50 miles (such as "the distortion of the aircraft structure by the direct or indirect effects of aerodynamic heating" and "stability and control at very high altitudes at very high speeds, and during atmospheric re-entry from ballistic flight paths") in wind tunnels or other laboratory equipment. The high-velocity rocket program at Wallops Island could investigate aircraft design and operational problems to about Mach 10, the study had admitted, but this program of small unmanned flights was not an "adequate substitute" for full-scale manned flights. Having concluded that the Brown group was right, and that the only way known to solve these problems quickly was by using a manned aircraft, Thompson said that various NACA laboratories then had undertaken to examine the feasibility of designing and constructing such an airplane now. Trying to prevent an internal fight, he explained that the results from Langley to be presented during this executive session, and which were contained in the document

Ames had just distributed, though "generally similar" to the other NACA studies (which they were not), were more detailed than those of the other labs (which they were).[43]

Walter Williams and HSFS test pilot A. Scott Crossfield followed Thompson's introduction with an outline of the performance required for a new research airplane and a discussion of some of the more important operational aspects of the plane. At that point Becker and N. F. Dow took over with a detailed presentation of their group's intensive six-month study. Lively debate followed this presentation. Most members of the Aerodynamics Committee strongly supported the idea of the hypersonic research airplane. This group included Robert J. Woods, Bell's representative on the committee, who in the summer of 1954 had led one of the first industry teams to Langley to find out about the concept of the Becker group, and Clark B. Millikan of the California Institute of Technology, who emphasized the importance of obtaining flight experience, especially about the effects of the "no-gravity" condition on the pilot.

However, Clarence L. "Kelly" Johnson, Lockheed's representative, opposed any extension of the manned research airplane program. Johnson argued that experience with research airplanes from the D-558-II through the X-3 types had been "generally unsatisfactory" in that the aerodynamic designs were actually behind tactical aircraft designs by the time research flights could be performed.[44] He felt that a number of research airplanes had developed "startling performances" only by using rocket engines and flying essentially "in a vacuum." These flights had mainly proved the bravery of the test pilots, Johnson charged. A great deal of data on stability and control at high Mach numbers had surfaced as a result of the test flights, Lockheed's chief engineer admitted, but aircraft manufacturers could not use much of this information because it was "not typical of airplanes actually designed for supersonic flight speeds." He recommended that instead of building a new manned airplane, an unmanned vehicle should first be constructed to obtain data on the structural temperature and the control and stability aspects of the proposed craft. If it were subsequently decided that the aeromedical problems were "predominant," Johnson said, a manned research airplane could then be designed and built. The airplane should be constructed in such a manner that it could be used as a strategic reconnaissance airplane.[45]

Various members of the NACA took issue with Johnson. Williams recalled that as early as 1947 the X-1 airplanes had made both climbing and level flight runs to about Mach 1.5 up to altitudes of some 55,000 feet. He pointed out that it took tactical airplanes from five to seven years longer to achieve flights at speeds and altitudes of this magnitude. Gus Crowley, the

NACA research contributed significantly to the development of the seven most important experimental aircraft designs of the 1950s. Parked (clockwise, from lower left) on the checkered tarmac at Edwards Air Force Base in California are: the Bell X-1A, the Douglas D-558-1, the delta-winged Convair XF-92, the Bell X-5 (the first aircraft to use a variable-sweep wing), the Douglas D-558-2, the Northrop X-4 (with no horizontal tail surface), and (in the center) the dart-shaped Douglas X-3.

associate director for research at NACA headquarters, explained in response to Johnson that the NACA had developed its proposal convinced that the new research airplane should be based on the "X-1 concept." This was "to build the simplest and soundest aircraft that could be designed on currently available knowledge and put into flight research in the shortest time possible." In comparing manned research airplane operations to unmanned, automatically controlled flights, Crowley noted that the X-1 and other research airplanes had made hundreds of successful flights, experiencing on numerous occasions excessive loading and buffeting and equipment malfunctioning. In spite of these difficulties—which, Crowley readily admitted, had occasionally put a plane out of control—research pilots had landed the aircraft successfully an overwhelming number of times. It was the human pilot that permitted further flights exploring the conditions experienced. In Crowley's opinion, automated flight could not be depended upon in similar cases.[46]

In summary, most of those present at this executive session of the Aerodynamics Committee believed that there were "no known limits in flight to which we will or can take human beings," that guided missiles would never eliminate the need for manned aircraft, and that recent studies showed that they were "so close to the achievement of the performance proposed by the NACA that we should proceed to accomplish these objectives in the shortest possible time," presumably within the next two years. After some further discussion, the Aerodynamics Committee passed a resolution:

> WHEREAS, The necessity of maintaining supremacy in the air continues to place great urgency on solving the problems of flight with man-carrying aircraft at greater speeds and extreme altitudes, and
>
> WHEREAS, Propulsion systems are now capable of propelling such aircraft to speeds and altitudes that impose entirely new and unexplored aircraft design problems, and
>
> WHEREAS, It now appears feasible to construct a research airplane capable of initial exploration of these problems,
>
> BE IT HEREBY RESOLVED, That the Aerodynamics Committee hereby endorses the proposal of the immediate initiation of a project to design and construct a research airplane capable of achieving speeds of the order of Mach number 7 and altitudes of several hundred thousand feet for the exploration of the problems of stability and control of manned aircraft and aerodynamic heating in the severe form associated with flight at extreme speeds and altitudes.

Kelly Johnson was the only person to vote "Nay." Sixteen days after the meeting at Edwards Air Force Base, Johnson sent a "Minority Opinion of Extremely High Altitude Research Airplane" to secretary Milton Ames with a request (that was honored) that it be appended to the majority report.[47]

With a strongly worded endorsement of the proposal from his prestigious Aerodynamics Committee in hand, Hugh Dryden immediately conferred with air force and navy management on how best to move toward procurement. Quickly the three parties agreed that detailed technical specifications of the proposed aircraft, with a section outlining Langley's plan, should be produced mutually by the end of the year for formal presentation to the Air Technical Advisory Panel of the Department of Defense.

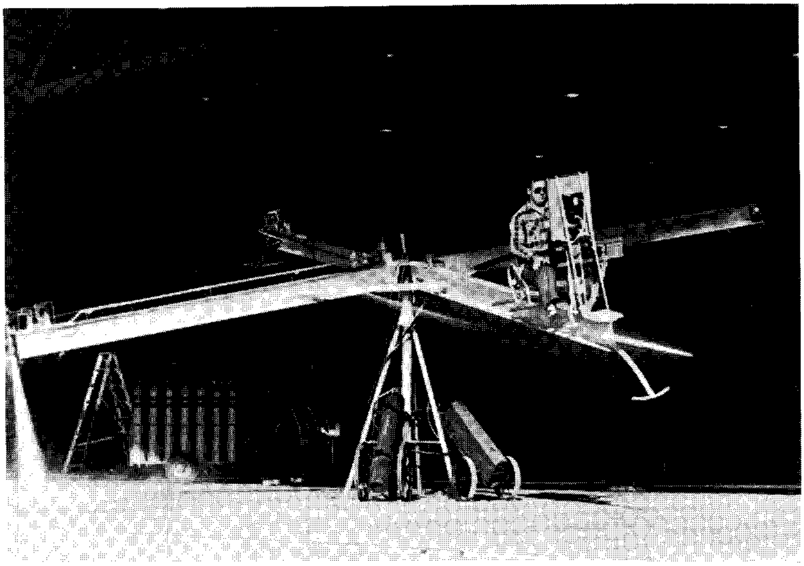
On 14 December 1954, a team of NACA researchers and managers made this formal presentation to the Department of Defense panel. The panel approved the specifications and gave the NACA technical control of the project, but stipulated that the panel would have to be given the chance to review the design proposals submitted by industry. Just before Christmas the NACA, the air force, and the navy signed a "Memorandum of Understanding" setting up a new "Research Airplane Committee" to assume the responsibility for technical direction of the "X-15" project. On

The aerodynamics of air launching the North American X-15 being investigated in the High-Speed 7 × 10-Foot tunnel, about 1957.

30 December, the Air Materiel Command invited aircraft manufacturers to a bidders' briefing to be held at Wright-Patterson Air Force Base on 18 January 1955. At this briefing the NACA and the military informed potential contractors of the design and operational requirements of the hypersonic airplane.

North American Aviation was awarded a contract in September 1955 to develop the X-15, and another in June 1956 to build three prototypes. The Reaction Motors Division of Thiokol Chemical Corporation received the contract for development and production of the rocket engines. The original X-15 made its first flight, a powerless glide, in June 1959, 11 months after the dissolution of the NACA and the establishment of NASA. NASA's flight test program of the X-15 began in March 1960. One of the first NASA pilots to fly the plane was Neil Armstrong, who, within the decade, would be the first man to walk on the moon.[48]

Between June 1959 and October 1968 the three X-15 aircraft involved in the joint NASA-air force-navy aeronautical research program made a total of 199 flights. Until the first orbital flight of the space shuttle *Columbia* in 1981, the X-15 held the altitude and speed records for winged aircraft, with flights as high as 67 miles, and a maximum speed of 6.7 times the speed of sound, or 4518 miles per hour. According to John Becker, the pioneering X-15 reentry systems, their derivatives, and the X-15's reentry flight experiences "led directly" to the systems and techniques employed later in the shuttle. Though the public relations literature surrounding the impressive successes



Attitude control simulator for X-15 studies at Langley, 1958.

of the winged shuttle has quite rightly emphasized the development of the reusable ceramic tile heat-protection system, the enormous boosters, and the automatic flight-control systems, Becker believes that too little has been said about the shuttle's aerodynamic design features and reentry operation modes, established by the NACA some 20 years before the shuttle's first orbital flight. The shuttle's reentry characteristics—the transition from the reaction controls used in space to aerodynamic controls, the use of high angles of attack to keep the dynamic pressures and the heating problems within bounds, and the need for artificial damping and other automatic stability and control devices to aid the pilot—are "similar in all important respects" to those of the X-15 conceived at Langley.[49]

Project HYWARDS

As Langley researchers began wind tunnel and structures testing of the X-15 in early 1956, they could take great satisfaction in the knowledge that NACA headquarters had pushed their radically new research airplane concept through the complex machinery of procurement as fast as they had found solutions to its difficult hypersonic design problems. One can imagine, then, how surprised the NACA researchers were in March 1956 when they heard rumors that the air force had established Project HYWARDS (an acronym for *hy*personic *w*eapon *a*nd *R* and *D* *s*ystem). The goal of Project

HYWARDS was to design and procure a successor to the X-15 capable of a speed of about Mach 12.[50]

Although Langley's hypersonics specialists were busy in the spring of 1956 supporting the development of the X-15, Project HYWARDS also deserved high-priority attention. Floyd Thompson immediately set up another ad hoc interdivisional study group. Though larger, it was patterned directly after the successful pre–X-15 group.* Becker again acted as chairman. As a starting point, he decided to focus attention, for analytic purposes, on a design speed of Mach 15. Though none of the group was sure that Mach 15 would prove feasible, everyone believed that "it was about the lowest speed for which an attractive military boost-glide mission could be defined."[51]

The HYWARDS study group at Langley issued its first formal report in mid-January 1957. The most important recommendation in this report was to raise the design speed to 18,000 feet per second, or about Mach 18! The group had learned in the course of its heating analysis that

> at this speed boost gliders approached their peak heating environment. The rapidly increasing flight altitudes at speeds above Mach 18 caused a reduction in heating rates; at satellite speed, of course, on the outer fringe of the atmosphere, the heating rates became negligible.

The step from X-15 to HYWARDS would thus be an enormous one—from Mach 7 to at least Mach 15 and possibly as high as Mach 18. In many areas, especially in high-temperature, internally cooled structures, the researchers would have to confront enormously complex developmental problems.[52]

Becker's new group proposed the design of an advanced boost-glider prototype. In at least two respects the configuration it suggested differed importantly from the form of previously proposed boost gliders, as championed by Bell, which employed midwing arrangements. (That is, the fuselage crossed both above and below the wing.) Heating analyses carried out principally by Korycinski and Becker himself had revealed "major advantages" for a restyled configuration having (1) a delta wing with a flat bottom surface and (2) a fuselage crossing the relatively cool shielded region on the top

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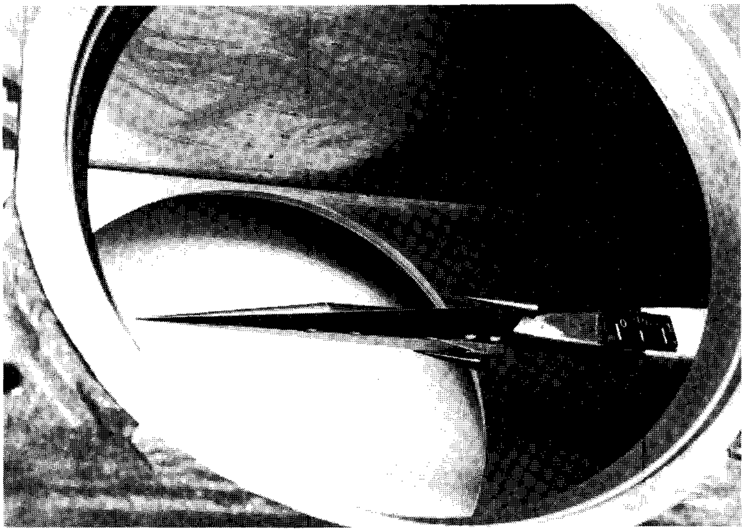
* Members of the HYWARDS study group at Langley were: John Becker, chairman (also leader of the heating analysis subgroup); Max Faget, propulsion and configuration; L. Sternfield and Frederick Bailey, stability, control, and piloting; Israel Taback, instrumentation, range, and navigation; Roger Anderson and Paul Purser, structures and materials; Philip Donely, loads and flutter; A. Vogeley, operations and X-15 coordination; Peter Korycinski, heating. As the work progressed, a number of other specialists were added, notably: Paul Hill, configuration and propulsion; and Eugene Love and Mitchel Bertram, configuration, aerodynamics, stability, and control.



In 1957 Langley tested its HYWARDS design in the 11-inch hypersonic tunnel.

(or lee) side of the wing. The flat-bottomed wing design had "the least possible critical heating area for a given wing loading," which translated into the need for "least circulating coolant, least area of radiative shields, and least total thermal protection in flight."[53] Here was the first clear delineation by the NACA or anyone else of design features that could significantly alleviate the aerodynamic heating problems of hypersonic flight, "space leap," and reentry. In the future, designers would incorporate these basic features in the air force's Dyna-Soar (a program whose intent was to combine all post-1953 feasibility studies on a boost-glide research vehicle into a single plan) and NASA's space shuttle.

A Technical Debate with Ames Laboratory

In the course of supporting HYWARDS, Becker's study group became engaged in a debate with a parallel group of researchers at Ames. A glimpse of this debate reveals specialists inside one overall organization arriving at different solutions to the same technical problem, and management mediating the consequent disagreements and rivalries. Results of the debate show how and why it is sometimes beneficial for two laboratories to work simultaneously but separately on the same problem.

The Langley study shed some new and surprising light on the requirements of lift-drag ratio *(L/D)*, an important gauge of the aerodynamic efficiency of wings at different angles of attack, for hypersonic gliders.* Becker's group knew that regarding aircraft range at ordinary speeds this factor was as important as the weight and propulsion factors. But at the near-orbital launch speed required for "once-around" or global range, the group found theoretically that the glider weight would be carried initially almost entirely by the centrifugal force produced by the launch. Considering this, the group perceived that aerodynamic L/D lost most of its importance. Thus, for global range, the study showed that a certain glider design with low L/D would require only about three percent higher launch velocity than a design with L/D four times higher. Of course Becker and colleagues wanted to capitalize on the enormous configurational, weight, and heating benefits of the high-lift, high-drag glide trajectory mentioned previously. But it made sense to strive for high L/D only for short ranges up to 2000 or 3000 miles. For the intermediate range proposed for the Langley glider (1/4 global range, 70 percent of orbital speed), about half of whose weight would be carried by centrifugal lift, they judged that an intermediate design well below the ultimate high L/D would be best.[54]

Not everyone inside the NACA at first agreed with the conclusion of Becker's study group. When HYWARDS became a high-priority research item in the spring of 1956, Ames had also set up a study group. The motivations and findings of this group—headed by Harvey Allen and Al Eggers—were apparently quite different from those of the Langley group.† The Ames group was more intrigued by the possibilities for combining aerodynamic bodies—wing and fuselage, in particular—to produce beneficial interference effects. (This interest was perhaps stimulated by the great success of Richard Whitcomb's area rule for transonic design; see chapter 11.) In the mid-1950s a number of Ames aerodynamicists were deeply involved in improving the performance of supersonic configurations through favorable interference (the type that occurs when the pressure field of an underslung conical fuselage impinges on a wing). This involvement may have affected the outlook of the Allen-Eggers study group, for its members seemed to have

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[*] Since the ratio of drag to lift (D/L) is expressed in very small fractions, it is customary to plot the *reciprocal* of D/L (i.e., that by which the given quantity is multiplied to produce unity; as, the reciprocal of x is $1/x$) instead of D/L itself. This reciprocal, the lift-drag ratio (L/D), is commonly called "L over D." Typically, the shape of the L/D curve is such that its maximum value occurs at the same angle of attack as where the D/L curve has its minimum value.

[†] The other members of the Ames study group included Robert Crane, Glen Goodwin, and Lawrence Clousing.

worked hardest to identify a hypersonic boost-glide system that made use of favorable interference. In any case, the resulting Ames proposal called for a Mach 10 vehicle designed for the highest conceivable hypersonic lift-drag ratio. The Ames perception of the importance of high L/D, a perception directly at odds with Langley's, was that it would optimize aerodynamic efficiency and thus allow the boost-glide vehicle to achieve a greater range than a ballistic vehicle for a given initial boost velocity.[55]

Ames and Langley tangled over the technical issues of Project HYWARDS on 14 and 15 February 1957 at the first meeting of the NACA "Round III" Steering Committee on New Research Airplanes. (The specific job of this subcommittee was to study the feasibility of a hypersonic boost-glide research airplane. Round III was considered the third major research airplane program, the X-1 and D-558 series being the first and the X-15 the second.) Langley spokesmen had two central objections to the Ames proposal besides the matter of high L/D. First, in keeping with its penchant for favorable interference effects, Ames had the fuselage crossing the high-pressure lower surface of the wing, the hottest region in the wing flow field. This location would increase aerodynamic efficiency, but it also required additional thermal protection, increasing the weight of the airplane. Second, and more importantly, Langley spokesmen questioned the low design speed of Mach 10 recommended by Ames, which was, in the opinion of Becker's study group, almost 50 percent less than the minimum velocity required for an attractive boost-glide mission. They were especially upset when advocates of the high-L/D approach suggested that the Ames vehicle would have a range advantage of some 1300 miles *if launched at the same speed* as the Langley vehicle (about Mach 18). Simple engineering calculations showed that the weight penalty associated with higher L/D would, for equal systems, nullify this range advantage.[56]

The distance between the distinctively different design configuration philosophies of Ames and Langley on HYWARDS can perhaps be explained by a single fact about the NACA organization: Langley had a Structures Research Division that kept the aerodynamicists at the Virginia lab informed about trade-offs required by high-temperature structures and heat protection considerations; Ames did not. "Thus the Ames emphasis on high-L/D in the hypersonic research airplanes was simply a reflection of their established primary research interest [aerodynamics] rather than any special understanding or analysis of the real-life trade-offs that must be made between high-L/D, structural weight, and, especially for hypersonic aircraft, heat-protection-system weight."[57]

Resolving the debate between the Ames and Langley study groups was up to NACA management at the two labs and in Washington. In the interests of interlaboratory peace and cooperation, all three units opted for compromise. The HYWARDS team at Langley wrote a report for headquarters, for example, analyzing both the Langley and Ames vehicles in positive terms as essentially the results of alternative approaches: "low heating" (Langley's) and "high *L/D*" (Ames's). Langley management and an officer in headquarters edited the report for impartiality, while Becker and members of his HYWARDS study group summarized its contents in presentations at Langley, at Ames, and at headquarters in May 1957, and at the Pentagon in July. Because of strong residual differences over how to configure HYWARDS, the NACA held an interlaboratory Round III meeting at Ames from 16 to 18 October 1957. Both working-level personnel and upper management attended. Again, compromise was the order of the day. The Langley and Ames study groups were ready to agree that it was "foolish" to be so "vociferously wedded" to present configurations. Each side knew that its own configuration fell far short of optimum. For its part, the Langley team recognized that it had simply selected "reasonable but arbitrary" values for some vital design factors. For example, it had originally determined the coolant requirements by merely assuming a particular wing loading and skin temperature.* The Langley team also now revealed that the complex internal coolant system it had planned for its glider configuration was "a highly undesirable complication," made necessary by the lack of a superior high-temperature material (which the Langley structures people dubbed "unobtainium").[58] Considering the fact that the aircraft system it recommended would require new developments in every area of applicable technology, the team's forecast that the system could be developed and ready for flight in five years or less was far too optimistic.

During his summary presentation at Round III on 16 October, Becker made exactly these points, if in a way that still meant to show the errors of the Ames high-*L/D* approach. To do so, he predicted certain dramatic effects on the performance of the Langley glider that would result from

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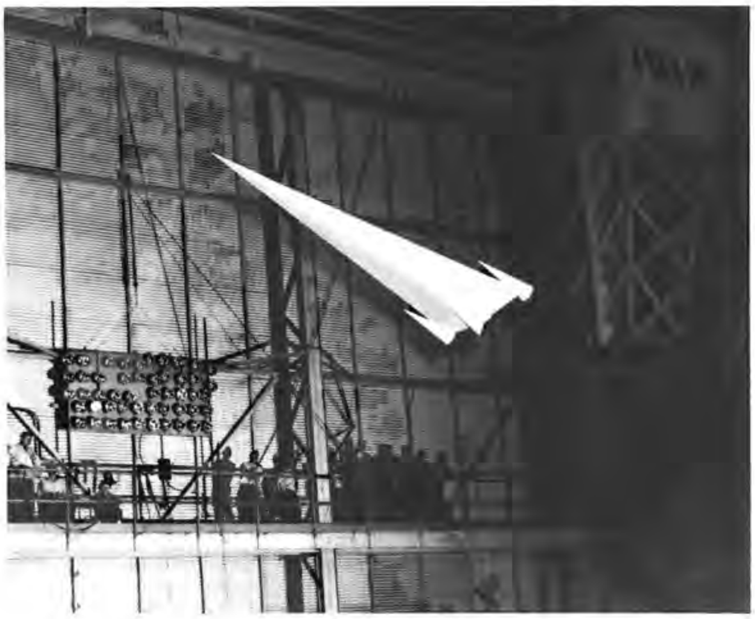
* Two months before the Round III meeting at Ames, Becker and Korycinski had initiated a systematic parametric analysis of the coolant requirements of the Langley glider. Preliminary results were very exciting, for they indicated that if the glider employed a flat-bottomed wing designed for a particular loading and maximum lift, and if the glider were then operated at a specific high angle of attack (about 45 degrees) to produce a specific reentry attitude, the need for surface coolant would be virtually eliminated. This conclusion—which was reported in a 1959 confidential paper (see Becker and Korycinski, "The Surface Coolant Requirements of Hypersonic Gliders," NASA Memo. Rpt. 1-29-59L, April 1959)—eventually helped to make it possible to design the metallic DS-1 vehicle of the Dyna-Soar Project without skin coolant. The space shuttle enjoys the same privilege because of its advanced ceramic tiles (see P. A. Cooper and P. F. Holloway, "The Shuttle Tile Story," *Aeronautics and Astronautics*, Jan. 1981, 19:24–34).

reductions in wing size and wing loading. He demonstrated that by using a wing that was 40 percent smaller, the range of the glider would be increased from 4700 to 5600 nautical miles. Decreasing the size of the wing also reduced the L/D by about 14 percent, but Becker emphasized that the associated 4000-pound reduction in glider weight more than compensated for this L/D loss. The head of Langley's pre–X-15 and HYWARDS study groups concluded that "we should concentrate *not* on increasing L/D by every known means, but rather on seeking *optimized* configurations," which meant, generally speaking, much smaller wings than those called for by high-L/D designs.[59] The Ames people seem to have accepted Becker's ideas with little question. Perhaps they realized by Round III that there were no quick and easy solutions to the enormous technical problems of heat protection in very high L/D design.

Sputnik

Langley and Ames had a more compelling reason, however, to compromise over their different HYWARDS glider configurations than some new technical consensus over the optimum L/D or over structural heating requirements. The first man-made satellite to orbit the Earth—the Soviet Union's *Sputnik I*—was moving overhead. Since Sputnik was launched on 4 October 1957—only twelve days before Round III began—Americans had been huddling near radios and televisions straining to hear the "beep-beep-beep" of the distant satellite. What they heard from the satellite alarmed them, but what they heard *about* the satellite bothered them even more. The Soviet achievement embarrassed American scientific and technological prestige, the politicians were beginning to say, and it posed a new communist threat to national security.[60]

Although the Main Committee took no official notice of it at its annual meeting on 10 October, Sputnik had captured the minds and imaginations of some within the NACA. Many attending Round III "felt mounting pressures" to solve the critical reentry problem of the ballistic vehicle and even to take on satellite research. Robert R. Gilruth, for example, recalls watching the sunlight reflecting off *Sputnik I* as it passed over his home on the Chesapeake Bay: "It put a new sense of value and urgency on the things we had been doing." Langley and Ames had been studying the problems and potentials of *lifting bodies*—that is, wingless bodies capable of generating lift—since the early 1950s. Theoretical and experimental results from ICBM research demonstrated very clearly by October 1957 that ballistic operation—throwing a vehicle into the upper atmosphere or into space rather than flying it there and back—minimized both the launch



Comparison of equal-weight systems

Glider		
S_W	1174 sq ft	587 sq ft
W_o	20,700 lb	16,700 lb
W_{fuel}	45,000 lb	49,000 lb
W_{tot}	65,700 lb	65,700 lb
W_{tot}/W_o	3.17	3.93
T/W_o	2.76	3.43
ΔV	9400 ft/sec	11,150 ft/sec
Booster		
W_{tot}	274,300 lb	274,300 lb
ΔV	8600 ft/sec	8600 ft/sec
System		
W_{tot}	340,000 lb	340,000 lb
V	18,000 ft/sec	19,750 ft/sec
L/D	4.2	3.8
Range	4700 nm	5600 nm

At the NACA's Round III meeting at Ames laboratory in October 1957, John V. Becker used a chart like this one to show how Langley's hypersonic glider (to the right in the chart) could achieve increased range by using a smaller wing to reduce the lift-drag ratio from 4.2 to 3.8. Above, a model based on Langley's concept of a hypersonic glider was test flown on an umbilical cord inside the Full-Scale Tunnel in 1957.

energy required and the reentry heat load. High reentry deceleration rates and the necessity of an uncontrolled parachute landing still handicapped the ballistic vehicle, but at least NACA labs had found a way to greatly alleviate the deceleration problem by designing, according to Allen's blunt-body principle, a wingless body with small L/D which was capable of significant lift.[61]

During Round III, the Ames and Langley groups studying hypersonic gliders agreed that Sputnik made satellite research a high NACA priority; the two groups disagreed sharply, however, over whether the new priority of satellites should be placed higher on the NACA research agenda than the hypersonic glider. The majority of Ames people felt that satellites deserved higher priority. They said, in effect, that since the known science and technology of very low L/D seemed to suffice for satellite reentry, the NACA should decide to work on satellites rather than on more complicated and unknown HYWARDS-type winged configurations. The majority from Langley—some of whom had argued long and hard to convince their counterparts at Ames that high L/D was *not* needed for HYWARDS—felt that the winged glider continued to deserve higher priority.

Ira Abbott of NACA headquarters, a longtime Langley employee, mediated this new Langley-Ames dispute. At the close of the Round III meeting he voiced the majority opinion that the NACA should immediately begin to study the satellite reentry problem for nonlifting or slightly lifting vehicles. It should be "in *addition* to continuing R&D on the boost-glide system, however, not its *alternate*."[62] There was good reason for the NACA to think that its work on the boost-glide system was still, in spite of the growing reaction to Sputnik, more immediate and urgent from a military point of view than was work on satellites: after all, the air force had only two months earlier proposed Project Dyna-Soar to follow the X-15 project.

On 3 November 1957 the Soviet Union launched a second Sputnik carrying a 500-kilogram payload many times heavier than the small Vanguard satellite then being contemplated for launching by the United States, which weighed less than two kilograms. This new Russian feat intensified the Cold War anxieties of many Americans, because the weight-lifting capability confirmed the Soviet claim of an ICBM which could reach American cities. A genuinely concerned but politically shrewd Lyndon B. Johnson responded by convening a round of sensational hearings in the U.S. Senate during which the nation's apparently lagging and confused satellite and missile programs were thoroughly scrutinized. Facing a growing public demand for his administration to respond in some significant way to the challenge of the Sputniks, President Eisenhower was forced to insist that a test flight of Vanguard TV-3 scheduled for early December be billed as a fully developed national attempt to orbit a satellite. This insistence backfired horribly: on the sixth of December, with hundreds of reporters from all over the world watching, the Vanguard rocket rose a mere four feet off its pad at Cape Canaveral, toppled over, and erupted into a sea of flames. The international press dubbed the failed American satellite "Kaputnik" and "Stayputnik." Cynical and embarrassed Americans drank the Sputnik cocktail: two parts vodka, one part sour grapes. At the United Nations, a Soviet delegate even asked if the U.S. was interested in receiving aid to underdeveloped countries.[63]

A revolution in public mentality was unfolding. Until the last ninety days of 1957, *space* had been a dirty word in American political arenas. Ira Abbott recalls that the NACA stood "as much chance of injecting itself into space activities in any real way [in the pre-Sputnik period] as an icicle had in a rocket combustion chamber." When he mentioned the possibilities of space flight to a House subcommittee in the early 1950s, Abbott was accused by one congressman of talking "science fiction."[64] *Space* had also had negative connotations in certain NACA quarters. The NACA had taken formal notice of space flight as early as 1952, but only as a natural extension of aerodynamic flight through the atmosphere into space and return. The predominant attitude of the Committee and leaders of its research organization during the period 1952 to 1958 was to avoid "Buck Rogers stuff." John Stack's support of the X-15, HYWARDS, and Dyna-Soar projects, for example, was lukewarm in comparison with his ardent enthusiasm for supersonic transport and advanced military aircraft.* But now, in the wake of Sputnik, *space* was no longer a dirty word: rather, it represented a new field of battle in the Cold War. If the U.S. lost this battle in space, many in America and Europe began to believe, the entire world was perhaps doomed to communist hegemony.

NACA leaders and researchers alike saw the development of the necessary space technology not as a revolution requiring crash programs, but as an evolution fully within the capacity of the established aeronautical research agency. So, in late November 1957, the NACA did "as it had been wont to do in any crisis throughout its 42 years"; it created a committee—

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* Stack resisted the space technology revolution long after the Sputnik crisis, probably because it threatened to drain away precious resources from aeronautical programs. In the early 1960s he told his colleagues that he did not buy the "to-the-moon-by-noon" stuff. After noting the enormous sizes of the Apollo rocket boosters ("like the Washington monument"), Stack (who in November 1961 was appointed director of aeronautical research in the Office of Advanced Research and Technology at NASA headquarters) tried to persuade NASA to find a viable air-breathing, aircraft-like launch system. In June 1962 he left his high-level NASA post to become vice-president of Republic Aviation Corporation, where he could continue to work almost purely on aeronautical projects.

the Special Committee on Space Technology, which was chaired by H. Guyford Stever, an associate dean of engineering at MIT, and included James A. Van Allen, the University of Iowa physicist who had developed satellite instrumentation for Project Vanguard, and Wernher von Braun, head of the Development Operations Division of the Army Ballistic Missile Agency (ABMA) at Redstone Arsenal in Huntsville, Alabama.[65] A month later, on the day after Christmas, Hugh Dryden sent a letter to Henry Reid, requesting Reid to appoint a Langley committee of senior staff members for the purpose of "taking a critical look at the whole subject of aeronautical research as it was affected by space flight problems." This Langley committee, which was chaired by Robert Gilruth, reported back to Reid on 31 January 1958. The principal finding was that Langley was already in the midst of "an extensive shift in emphasis towards the fields of hypersonic and space flight."[66]

What made the NACA so confident of its ability to assume the new and expanded roles in space research brought on by Sputnik was in large measure the promising and ambitious work and bold outlook of its X-15 and HYWARDS study groups. And on no occasion was the confidence of these two groups more in evidence than at Ames in March 1958 during the opening session of the last NACA Conference on High-Speed Aerodynamics.

The Last NACA Conference on High-Speed Aerodynamics

The primary purpose of the NACA's periodic conferences on high-speed aerodynamics, begun in 1946, was to communicate the results of recent research in supersonic aircraft and guided missiles and to stimulate discussion of those results. Through the 1950s attendance ranged approximately from 200 to 500 people, about 90 percent of them from the NACA, the military, and industry, the remaining 10 percent representing other government agencies, universities, and private research and consulting firms.

As originally planned, the agenda of the last NACA Conference on High-Speed Aerodynamics would not explicitly include reentry vehicle concepts. This plan followed the longtime official NACA policy of leaving the design and development of specific aircraft to industry. A week after the December 1957 agenda-setting meeting, however, a contractor responding to the air force's interest in a manned minimum-orbit satellite (its "Man-in-Space-Soonest," or "MISS," project) visited Langley to discuss his company's candidate vehicle, a winged glider not altogether unlike the earlier HYWARDS configuration of the Becker group. The man's lack of understanding of how a long-range hypersonic glider should be drastically reconfigured as a satellite reentry vehicle convinced Becker—who, after Round III, had turned to apply the surprising results of his and Korycinski's coolant study to the design of a one-man satellite vehicle *with wings*—that an NACA paper on the subject was needed at the forthcoming conference.[67]

When the lab reopened after the Christmas holiday, Becker called on Robert Gilruth, who was coordinating Langley's conference papers, with a proposal for a paper on a winged satellite configuration. Noting that Ames researchers were quickly abandoning their winged reentry vehicle concept for new work on lifting-body satellites, Becker suggested that it was now up to Langley to provide the scientific, technological, and promotional support for winged vehicles. Gilruth agreed that all technical views needed airing and added that a study of a simple nonlifting satellite vehicle (which was to follow a ballistic path in reentering the atmosphere) by Max Faget, head of the Performance Aerodynamics Branch of PARD, also deserved presentation in a separate paper instead of being buried, as it was, in a general discussion of operational problems. Gilruth asked NACA headquarters if these two papers could be added to the conference agenda.

Headquarters replied that the papers by Becker and Faget could be added to the agenda, and it notified Harvey Allen at Ames, who was to chair the relevant technical session, of the addition. Not wanting to be outdone, a team of Ames engineers led by Thomas J. Wong (under the conceptual direction of Al Eggers) now proposed to add a paper on their own best concept of a manned satellite—a blunt, lifting "half-cone." The organizers of the conference agreed to this third additional paper and scheduled all three for presentation early in the first session.

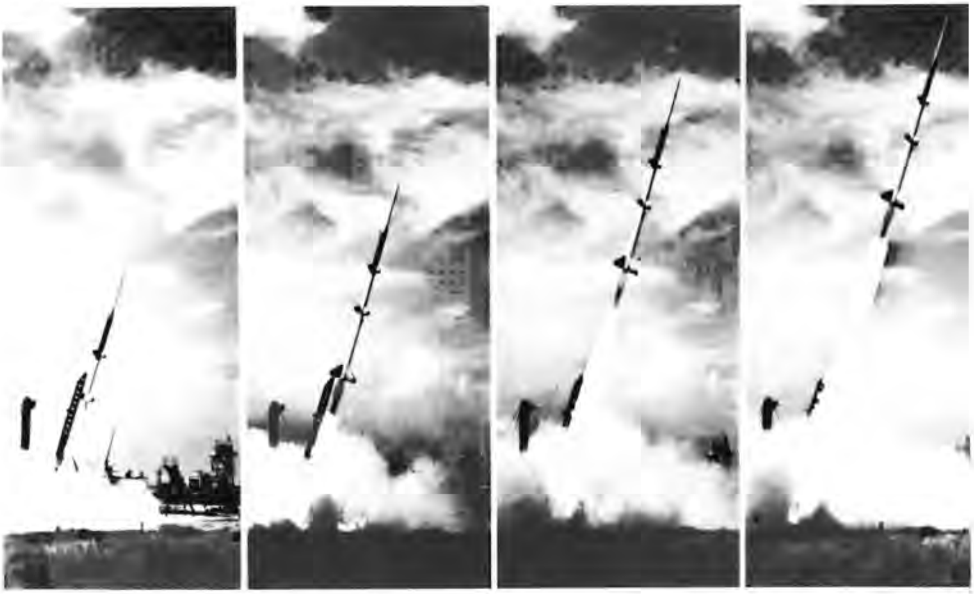
The opening paper of the last full-dress conference under the NACA banner was a general "Study of Motion and Heating for Entry into Planetary Atmospheres" by Ames's Dean R. Chapman, an aeronautical engineering Ph.D. from Caltech. In his paper Chapman considered the special problems of entry into the atmospheres of Venus, Mars, Jupiter, as well as of Earth, and he introduced some exact and versatile mathematical tools for dealing with trajectory and heating problems.[68] The three preliminary studies of manned satellites added to the agenda in early 1958 followed Chapman's presentation. Faget read his paper (coauthored by Langley's Benjamin J. Garland and James J. Buglia) first. He highlighted several advantages of the simple nonlifting ballistic vehicle, a pet concept:

> Since it follows a ballistic path there is a minimum requirement for autopilot, guidance, or control equipment. This condition not only results in a weight saving but also eliminates the hazard of malfunction. In order to return from orbit, the ballistic reentry vehicle must properly perform only one maneuver.

Maxime A. Faget was born in British Honduras in 1921, the son of an honored physician of the U.S. Public Health Service. In 1943 he earned a B.S. in mechanical engineering from Louisiana State University. After service as a navy submarine officer, he joined the Langley staff in 1946 as a member of the Pilotless Aircraft Research Division. His early work for PARD involved the invention of choking inlets for ramjets and a flight Mach meter.

> This maneuver is the initiation of reentry by firing the retrograde rocket. Once this maneuver is completed (and from a safety standpoint alone it need not be done with a great deal of precision), the vehicle will enter the earth's atmosphere. The success of the reentry is then dependent only upon the inherent stability and structural integrity of the vehicle.

Faget concluded that the state of the art in ballistics was "sufficiently advanced so that it is possible to proceed confidently with a manned satellite project" of the type he was proposing. He recommended specifically the design of a nearly flat-faced cone configuration 11 feet long, 7 feet in diameter, and weighing 2000 pounds.[69] Thomas Wong, a talented theoretician on Eggers's staff, followed with a paper (coauthored by Charles A. Hermach, John O. Reller, and Bruce E. Tinling) expressing the Ames position



Takeoff of a five-stage missile research rocket from Wallops Island in 1957. The first two stages propelled the model to about 100,000 feet; the last three stages were fired on a descending path to simulate the reentry conditions of ballistic missiles.

on manned satellites. This was that lifting bodies—like the blunt half-cone conceived by Eggers after Round III—would prove superior to nonlifting bodies for use as manned satellites. Though his paper challenged many of Faget's claims, Wong did not push his group's high-lift, high-drag configuration. As Eggers later explained, Ames people were not as enthusiastic as some Langley people were to participate heavily in a program to develop "hardware" and launch spacecraft of any kind, manned or unmanned.[70]

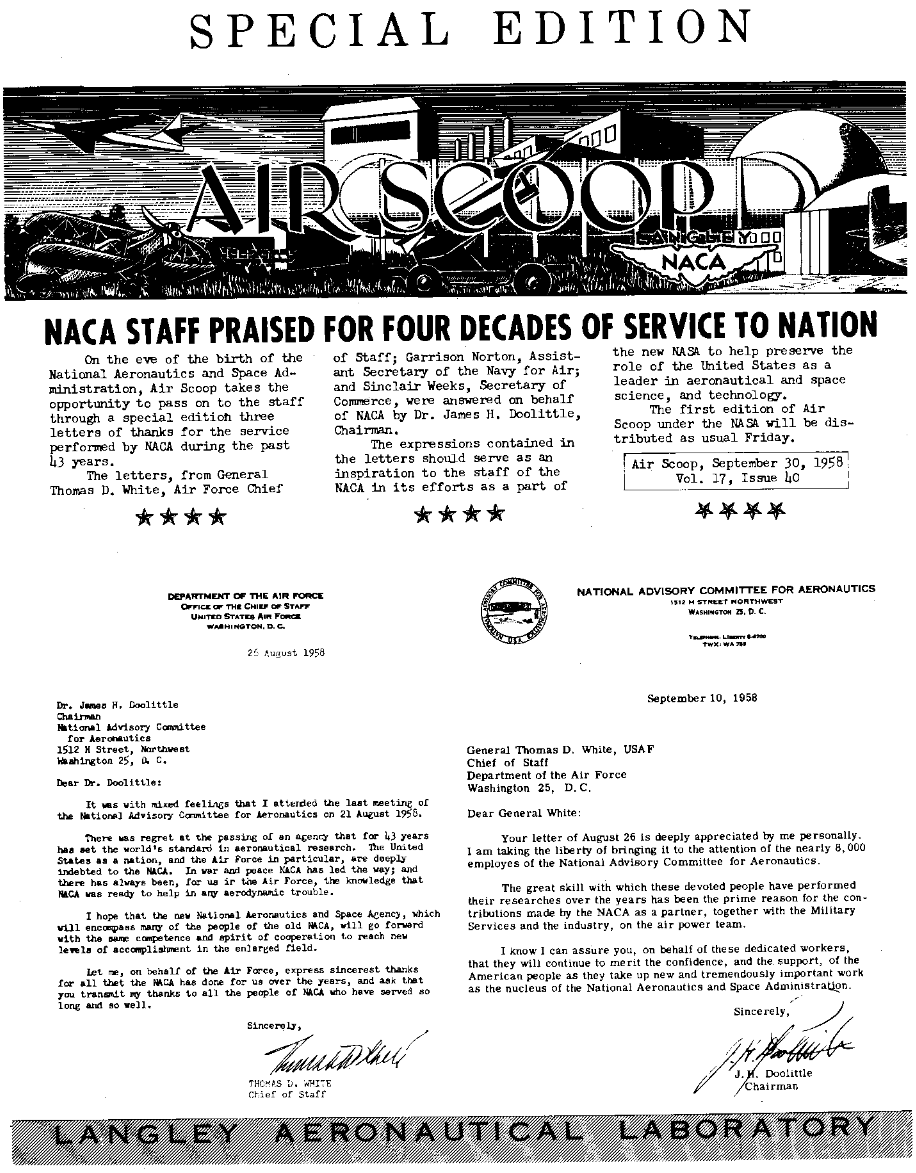
Becker read his paper last. He opened it with a brief discussion of "the general unsuitability" of high-L/D gliders as reentry vehicles, a diplomatic restatement of Langley's previous critique of Ames's earlier point of view. Becker then compared the relative heating effects of lifting and nonlifting reentry in order to emphasize the large reduction in heating rates and loads made possible by the low-L/D, high-lift operation of winged vehicles. The paper concluded with analysis of a small, winged satellite configuration embodying all of the desirable features identified by Langley during its previous X-15 and HYWARDS studies: low L/D for range control, hypersonic maneuvering, and the capability for conventional glide-landing; radiative solution of the heating problem by operation near maximum wing lift; use of a flat-bottomed wing—with a large leading-edge radius—and a fuselage crossing the protected lee area atop the wing. The weight of the winged satellite was only 3060 pounds, Becker emphasized, merely 1000 pounds more than a small ballistic capsule. He argued that a launching system similar to the booster system described earlier by Faget for wingless, nonlifting satellites could thus also do the job for his winged vehicle.[71]

According to Becker, this paper, which dissented from the consensus within the NACA favoring a ballistic projectile, created more industry reaction—"almost all of it favorable"—than any other he had written. What ruled out acceptance of his proposal, however, was the fact that the Atlas, the only ICBM anywhere near ready for use in 1958, did not have sufficient lift capability. Analysis showed that any weight beyond that of Faget's small and simple ballistic capsule would surely require an extra stage to the Atlas—and even the stages it already had were testing out unreliably—or it would require some other yet-undeveloped rocket. If not for these facts of systems technology, Becker today believes, "the first U.S. manned satellite might well have been a [one-man] landable winged vehicle," a miniature (3000-pound) version of the later (180,000-pound) space shuttle.[72]

* * *

The Langley engineers flying back to Hampton after the last NACA Conference on High-Speed Aerodynamics ended in March 1958 knew that some basic, quick, and dependable vehicle like the one Faget recommended would most probably carry the first man into space. Once home, they got researchers from PARD and other divisions busy brainstorming the problems associated with manned satellites. Through the spring and summer of 1958 these researchers performed tests and acted as consultants for the Man-in-Space-Soonest effort of the air force and the Advanced Research Projects Agency. Structures and materials experts—many of whom in the last five years had gone through a major transformation from "cold" to "hot"—worked to come up with satisfactory heat shield techniques and materials. Becker and his associates attacked the aerodynamic heating and hypersonic stability problems of variously shaped experimental space capsules in the 11-inch tunnel, while at the same time making the most of their opportunities to influence the X-15 and Dyna-Soar projects, thus sustaining the idea of winged hypersonic and reentry vehicles.[73]

If they had known that in less than four months, on 16 July, Congress would pass the National Aeronautics and Space Act, dissolving the NACA and establishing NASA, the Langley engineers flying home from Ames might have thought back with satisfaction on the quality of the 46 papers they had just heard at the NACA conference. These papers had dealt with such important new subjects as hypersonics, satellites, reentry trajectories, retrorockets, boosters, and interplanetary flight. Taken as examples of the NACA's ability to fulfill its mandated advisory and research functions, the papers suggested the ability of engineers and scientists trained in aeronautics to push their research talents into the new disciplines of aerospace and astronautics. There was no need for the returning engineers to worry about their careers being cut short. Because the NACA would serve as the nucleus for NASA, their work would change but continue.



SPECIAL EDITION

AIR SCOOP
LANGLEY
NACA

NACA STAFF PRAISED FOR FOUR DECADES OF SERVICE TO NATION

On the eve of the birth of the National Aeronautics and Space Administration, Air Scoop takes the opportunity to pass on to the staff through a special edition three letters of thanks for the service performed by NACA during the past 43 years.

The letters, from General Thomas D. White, Air Force Chief of Staff; Garrison Norton, Assistant Secretary of the Navy for Air; and Sinclair Weeks, Secretary of Commerce, were answered on behalf of NACA by Dr. James H. Doolittle, Chairman.

The expressions contained in the letters should serve as an inspiration to the staff of the NACA in its efforts as a part of the new NASA to help preserve the role of the United States as a leader in aeronautical and space science, and technology.

The first edition of Air Scoop under the NASA will be distributed as usual Friday.

Air Scoop, September 30, 1958
Vol. 17, Issue 40

★★★★ ★★★★ ★★★★

DEPARTMENT OF THE AIR FORCE
OFFICE OF THE CHIEF OF STAFF
UNITED STATES AIR FORCE
WASHINGTON, D. C.

26 August 1958

Dr. James H. Doolittle
Chairman
National Advisory Committee
for Aeronautics
1512 H Street, Northwest
Washington 25, D. C.

Dear Dr. Doolittle:

It was with mixed feelings that I attended the last meeting of the National Advisory Committee for Aeronautics on 21 August 1958.

There was regret at the passing of an agency that for 43 years has set the world's standard in aeronautical research. The United States as a nation, and the Air Force in particular, are deeply indebted to the NACA. In war and peace NACA has led the way; and there has always been, for us ir the Air Force, the knowledge that NACA was ready to help in any aerodynamic trouble.

I hope that the new National Aeronautics and Space Agency, which will encompass many of the people of the old NACA, will go forward with the same competence and spirit of cooperation to reach new levels of accomplishment in the enlarged field.

Let me, on behalf of the Air Force, express sincerest thanks for all that the NACA has done for us over the years, and ask that you transmit my thanks to all the people of NACA who have served so long and so well.

Sincerely,

THOMAS D. WHITE
Chief of Staff

NATIONAL ADVISORY COMMITTEE FOR AERONAUTICS
1512 H STREET NORTHWEST
WASHINGTON 25, D. C.

September 10, 1958

General Thomas D. White, USAF
Chief of Staff
Department of the Air Force
Washington 25, D.C.

Dear General White:

Your letter of August 26 is deeply appreciated by me personally. I am taking the liberty of bringing it to the attention of the nearly 8,000 employes of the National Advisory Committee for Aeronautics.

The great skill with which these devoted people have performed their researches over the years has been the prime reason for the contributions made by the NACA as a partner, together with the Military Services and the industry, on the air power team.

I know I can assure you, on behalf of these dedicated workers, that they will continue to merit the confidence, and the support, of the American people as they take up new and tremendously important work as the nucleus of the National Aeronautics and Space Administration.

Sincerely,

J. H. Doolittle
Chairman

LANGLEY AERONAUTICAL LABORATORY

Special edition of the Langley Air Scoop, *30 September 1958, with letters praising the NACA staff for 40 years of national service.*

Epilogue

In the hot early summer of 1958, before the creation of NASA, Hugh Dryden brought engineer Robert R. Gilruth from Langley to Washington to plan a man-in-space program "which would be acceptable not only to the NACA, but also to the Advanced Research Projects Agency, (or ARPA, which had been established by President Eisenhower in January 1958 to gather all antimissile and satellite activities in the Defense Department) and, of course, to the President's scientific advisors."[1] There, working less than ninety days in one large room on the sixth floor of the NACA building, a small task group of less than ten men, assembled by Gilruth over the telephone from the staffs of Langley and Lewis laboratories, came up with all of the basic principles of what would become Project Mercury.* The group's plan, which Gilruth outlined before the Senate Committee on Astronautics and Space Exploration on 1 August 1958, was to use an existing ICBM booster—the air force's Atlas rocket—to launch a small manned capsule into orbit. (The army's Redstone rocket, developed by von Braun's group in Huntsville, was to be used for early suborbital test flights of the Mercury capsule.) After a few passes around the earth, a retrorocket would be fired to slow the satellite down and thus initiate its descent from orbit. Following reentry into the atmosphere, which would be accomplished safely thanks primarily to the capsule's blunt heat shield, a large parachute would deploy, carrying the capsule and its human passenger on their final approach and landing into the open sea, where they would be recovered by helicopter and brought home aboard a naval vessel.[2] In essence, the plan for Mercury repeated what Langley engineer Max Faget, a member of Gilruth's task force, had proposed at the last NACA conference on high-speed aerodynamics in March 1958.

In the fall of the same year, after the establishment of NASA and ARPA's acceptance of the NACA's simple yet elegant plan for Project Mercury, Gilruth returned to Langley and immediately began to put together

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* Gilruth's original task group included Max Faget, Paul Purser, Chuck Mathews, and Charles Zimmerman from Langley, Andre Meyer, Scott Simpkinson, and Merritt Preston from Lewis, as well as a few part-timers who were brought in on an "as needed" basis. Later in the summer, under pressure to finalize a plan, Gilruth added Lewis's George Low and Warren North and Langley's Charles Donlan to his full-time staff.

In the mid-1950s Robert R. Gilruth, more than anyone else at Langley, began to push the idea that manned spaceflight was the next great challenge for aeronautical engineers. As head of NASA's Space Task Group, he was responsible for planning and carrying out Project Mercury, the country's first manned spaceflight program.

a larger and more formal group whose task was to rush implementation of the manned satellite project. Though to be located at Langley, this Space Task Group, or STG, was to report not to Langley management, but, in accordance with the instructions of NASA Administrator T. Keith Glennan, to Abe Silverstein in Washington, a veteran NACA engineer who, in the new NASA organization, had been made head of all space projects at headquarters.

This novel situation of a kingdom within a kingdom troubled Langley managers, who had good reason to fear the loss of many of their best people from the traditionally strong general research programs, but of course the feeling did not stop them from cooperating with the crash effort. In fact Floyd Thompson, the center's associate director, made things easy for Gilruth: "When I asked him how I could get men transferred from the research center at Langley Field to my new Space Task Group, [Thompson] suggested that a simple memorandum to him, stating that I had been authorized by the Administrator to draft people from Langley, would allow me to name those whom I wanted."[3] On 3 November 1958 Gilruth asked Thompson in writing for the transfer of 36 Langley personnel to STG, 14 of whom belonged to the Pilotless Aircraft Research Division (PARD) at

Wallops Island; the next day Thompson okayed all but one of the transfers—the sole exception being a man the Instrument Research Division wanted to keep, and for whom Thompson found a replacement. He even found a way to use the staffing of STG to the center's advantage. Thompson told Gilruth, "Bob, I don't mind letting you have as many good people from Langley as you need, but from now on I am going to insist that for each man you want to take, you must also take one that I want you to take." In this way, the associate director was able to serve both the interest of those employees who felt unfulfilled in their current positions, and were thus eager to transfer, and the interest of the center as a whole, by getting rid of employees who were causing some problem or who were disliked where they were.[4]

Although Thompson handled it well, the problem of staffing the Space Task Group signaled the start of a very intense and agitated era in Langley's history, that of the space technology revolution. The swift and enormous shift in emphasis from performing general aerodynamic research to planning and managing space hardware development and flight operations, a shift that began in association with Project Mercury, was a traumatic experience for Langley.

In part, this trauma resulted from a new and unusually heavy reliance on outsiders. "Contracting out" to private industry for certain necessary goods and services ran counter to the lab's tradition of the engineer in charge, the treasured independence (even from headquarters) and self-sufficiency made possible only by a broad range of in-house capabilities. But Project Mercury was "of an entirely different dimension than anything the NACA had ever done before," Gilruth remembers. "We had to cover many fronts, not only in the manufacturing area and the launch vehicle area, but also in the operations area." This coverage included procurement of the Atlas launch rockets from the air force and of the Redstone launch vehicles from the army, plus arrangement of launch services, as well as development of a worldwide satellite tracking network, coordination of recovery operations with the navy and air force, and cooperation between the various NASA centers involved in preflight testing. Specifications had to be prepared for industry, project guidelines had to be established, bidders had to be briefed, proposals from contractors had to be evaluated, contracts had to be placed, and work under contract (particularly at McDonnell Aircraft of St. Louis, which, in January 1959, was named prime contractor for the Mercury spacecraft) had to be supervised.[5] And all of this had to be done in a hurry if the United States was going to put a man in space before the Soviet Union did.

Besides adjusting to this new need to rely on outsiders, and besides absorbing the loss of talented personnel to the Space Task Group—which exploded in size from the original nucleus of 35 people in November 1958 to about 350 people in July 1959, over half of whom came from Langley—the center itself took on much of the direct responsibility for getting Mercury off the ground. Beginning in late November 1958, Langley provided extensive research and technical support for the development of the "Little Joe" launch vehicle, a new combination of four Sergeant solid rockets clustered in a single airframe, which had been conceived, even before STG was organized, by Langley engineers Faget and Purser as a means of testing the Mercury capsule configuration at Wallops Island before proceeding to the more expensive and difficult phases of testing at Cape Canaveral.[6] Then came the job of constructing part of "Big Joe," a full-scale instrumented mockup of the proposed Mercury spacecraft, that was to be launched from Cape Canaveral on the top of an Atlas D booster in September 1959 to prove the design of the Mercury capsule and its ablative heat shield. (Langley designed and fabricated the capsule's afterbody; Lewis constructed the forward, pressurized sections; General Electric built the heat shield.)[7] In February 1959, NASA headquarters gave complete responsibility for planning and contracting for the Mercury's worldwide tracking network to Langley.[8] During the same month, a number of the center's high-speed wind tunnel specialists accompanied STG members on a visit to the air force's Arnold Engineering Development Center in Tullahoma, Tennessee, to ascertain whether AEDC's facilities were equipped to test scale models of the Mercury spacecraft and, if the facilities were found equipped, to arrange a testing schedule.[9] At midyear the center estimated that, not counting the dozens of people it had already transferred to STG, 119 of its 1150 professional employees were spending 100 percent of their time working in support of Project Mercury. Many others were exploring hypersonic aerodynamics, reentry physics, and the Mercury escape tower configuration either in various tunnels at the center or with rocket models at Wallops. From the spring of 1959 on, Langley provided NASA headquarters with weekly progress reports on its extensive support of Project Mercury.[10] Only once before in Langley history, during World War II, had so many parts of the laboratory's organization been driven by the need, and the will, to perform with such singleness of purpose. And, unlike the wartime requirement, Project Mercury involved Langley in everything from in-house basic research, to out-of-house hardware development, to planning and management of actual flight operations.

The shift toward space technology development was also traumatic for Langley because it meant at least in part a shift away from aeronautics,

Besides losing many talented personnel to the Space Task Group, Langley itself assumed much of the direct responsibility for getting Project Mercury off the ground. Above, the Mercury space capsule was tested in the center's Full-Scale Tunnel in January 1959; left, the Little Joe launch vehicle, being prepared here for a test launch from Wallops Island in January 1960, was conceived by Langley engineers Max Faget and Paul Purser even before STG was organized.

the field which Langley engineers had been cultivating for over forty years. Veteran aeronautical engineer Raymond L. Bisplinghoff, who directed NASA's Office of Advanced Research and Technology from 1962 to 1966, put it mildly when he stated in a 1983 memoir that

> the formation of NASA ... had a dramatic, and at first deleterious, influence on the on-going program of aeronautical research [at the old NACA laboratories].
>
> The new space tasks were often under scientists who worked on a space problem for one week and then switched back to aeronautics the next week. The work was done while the entire NACA staff was occupied with the problems of reorganization under NASA, with the pressure of expanding staff and facilities, and with the problems of contracting for and monitoring or managing programs with outside industrial contractors.
>
> The massive priority which the country, from the president on down, placed on eclipsing the Russian lead in space flight had a profound influence on the NACA aeronautical research staff as they assumed positions in the new agency. Many took advantage of opportunities to move to higher grades and levels of responsibility in space activities. As a result, many moved from aeronautical research tasks to space program management tasks.[11]

Others, like John Stack, were so sure that the first *A* in NASA was being erased forever that they decided to leave the *space* agency entirely. At the time, especially after NASA's annual R&D budget for aeronautics fell below a million dollars in 1962, these disillusioned aviation enthusiasts could not have known how extensively, or how successfully, NASA would rebuild its aeronautics program following its major buildup for space.*

NASA's primary emphasis on building competence in space technology and on funding manned space flight caused some severe dislocations at Langley in the 1960s, to be sure. Moreover, it caused a major change in the way the public perceived the research center. Under the NACA, Langley was, relatively speaking, a low-key, mind-its-own-business type of organization whose activities were invisible to the average American.

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* During the 1970s NASA scientists and engineers would make significant contributions to aeronautical technology, including the development of the variable-sweep wing and of vertical takeoff and landing (VTOL) capabilities, the design of the supercritical airfoil, and the refinement of energy-efficient engines and fuels. Much of the work behind these contributions was done at Langley. Today, there is renewed interest at the center in the development of an American SST, a 250-passenger supersonic transport capable of cruising speeds in excess of Mach 2.5. See Richard H. Petersen and Cornelius Driver, "Readying Technology for a Super SST," *Aerospace America* 23 (July 1985): 56–59. Furthermore, Langley is also now spearheading the national effort to develop new technologies leading to a hypersonic transport, or HST, one proposed version of which is known as the "Orient Express." This vehicle would be capable of traveling twenty-five times faster than sound, going into orbit, or flying from Washington to Tokyo in two hours.

When residents of the Hampton area thought about Langley scientists and engineers, which was very rarely, they considered them as NACA nuts. But now, especially after the seven Mercury astronauts began their nationally publicized spaceflight training under STG direction at the center in April 1959, the area's residents perceived Langley's staff members more as wizards—technological magicians who could not only explain to them the meaning of the foreign objects orbiting ominously overhead, but who could also answer whatever challenges to the nation's security those objects implied.* (Conjure the scene from *The Wizard of Oz:* the wicked witch flies over the Emerald City spelling out "Surrender Dorothy," and all the terrified citizens rush to the wizard to find out what it means. In an exaggerated way, this gives some idea of how the Sputnik crisis and the resulting American space program triggered the local public's feelings of wonder about, and admiration for, Langley.) When Mercury proved successful, and ultimately grew into Project Apollo, respect for the center grew even greater—especially among the young people, as was indicated by the dramatic increase in mail received from students seeking information about NASA and its space programs. But adults were also caught up in the wave of enthusiasm. Hamptonians were so pleased with the attention that the space programs were bringing to their city that they voted to change the name of "Military Highway" to "Mercury Boulevard" and to dedicate the town's bridges in honor of the astronauts.

But despite the traumas in staffing and in reliance on outsiders, despite the professional dislocations for engineers and researchers, and despite the transformation in public perception, the space technology revolution of the 1960s did not destroy the legacy of the engineer in charge. There was a great deal about the place under NASA that remained virtually the same as it had been under the NACA. Those who had performed key research and supervisory jobs at the end of the NACA years played similar roles in the early NASA. Employees followed nearly all of the same procedures to initiate, monitor, and terminate work as had been followed in the last years of the NACA. Partial autonomy from headquarters and resistance to central controls continued to flourish. This remained true at least through the time that Floyd

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* In *The Good Old Days in Hampton and Newport News* (Richmond, Va.: Dietz Press, 1986), local historian and newspaper columnist Parke Rouse, Jr., remarked: "We locals at first regarded the bearded NACA [Nuts] as weirdos, up to no good. They dressed and acted like kooks, and they worked at mysterious jobs. But years later, when that research produced trips to the moon, we had to take it all back" (p. 69). Rouse's reference to *bearded* NACA Nuts undoubtedly testifies to the impact of Eastman Jacobs on the local public.

The spaceflight training of the Mercury astronauts (front row from left, Virgil "Gus" Grissom, Scott Carpenter, Donald "Deke" Slayton, Gordon Cooper; back row, Alan Shepard, Walter Schirra, and John Glenn) at Langley caused a major change in local residents' perception of the research center. Instead of considering the laboratory as the home of NACA nuts, they now saw it as the home of NASA wizards.

Thompson, engineer and NACA veteran of forty years, acted as director of the center (1960–1968).

In May 1968 Edgar M. Cortright, age 45, succeeded Thompson, age 69, as Langley's director. In certain respects the coming of Cortright was in keeping with Langley tradition. Like Thompson and Henry Reid, he was an engineer whose first professional employment was with the NACA. After graduating with a bachelor's degree in aeronautical engineering from Rensselaer Polytechnic Institute in 1947, Cortright had gone right to work at the NACA's Lewis laboratory, where he had specialized in the propulsion aerodynamics of supersonic aircraft and guided missiles. While in Cleveland, he had grown very close to Abe Silverstein, Lewis's dynamic associate director. Because Silverstein had worked at Langley from 1929 to 1943, Cortright, his protégé, was familiar with many of the traditions of the NACA's first laboratory.

But the coming of Cortright also meant dramatic change. Unlike his two predecessors as engineer-in-charge, he had never worked at Langley. Instead of making his way to a high position through leadership in the laboratory's general research program, Cortright had earned the directorship

Floyd L. Thompson (left), former center director retired since 1968, and Edgar M. Cortright (middle), outgoing director, welcome Donald P. Hearth as the new director (and fifth engineer-in-charge) of Langley Research Center in August 1975.

through his project management work at NASA headquarters*—where by the mid-1960s, a few key Langley veterans believe, there were some strong feelings at the top that Langley had gone its own way too often under Thompson's NACA style of management and needed to be brought under tighter central control.

A complete historical analysis of Cortright's appointment, and of the style and substance of his subsequent administration (May 1968 through August 1975), belongs not in this book, however, but in its sequel. Here one need state only that by the end of the second year of his tenure Cortright had directed the most sweeping reorganization in the center's history.[12]

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* When Silverstein came to Washington in the summer of 1958 to help prepare the transition to NASA, he brought Cortright with him. For most of his years in Washington, Cortright was associated with the unmanned space program (including the Mariner, Ranger, and Surveyor projects), where his immediate boss was Homer E. Newell, a former chief scientist at the Naval Research Laboratory. In 1963 Cortright became NASA's deputy associate administrator for space science and applications. Just before coming to Langley, he became deputy associate director of the Office of Manned Space Flight (OMSF).

Thompson reflects back in time to the design of this DeHavilland DH-4, the only aircraft built (under license) in the United States to serve in combat during World War I. The National Air and Space Museum loaned this historic aircraft to Langley in the fall of 1967 on the occasion of the laboratory's fiftieth anniversary.

Floyd Thompson retired from government service in November 1968, after serving for six months as administrator James Webb's special assistant and as chairman of a special group at headquarters whose task was to evaluate future manned spaceflight projects in the wake of Apollo.[13] He died on 10 July 1976, ten days before the first Viking lander touched down on the surface of Mars.

In retirement, after a life devoted to the advancement of flight, Thompson had looked back on the progress of American aeronautics in his time and had wondered: How was it that we were able to go from Kitty Hawk to the moon in the course of one man's lifetime? During World War I his high school science teacher had not been able to teach him anything about the principles of aeronautical engineering, because the teacher could not have known about them. His professors at the University of Michigan had informed him in the early 1920s that these principles had yet to be fully discovered, which meant that professional researchers still had to investigate the difficulties of the past, collect facts, and then, after finding out the meaning of the facts, determine the principles of flight. That investigation was part of the mission Thompson had assumed when he accepted a job with the NACA in 1926.

Four months after reporting to work at Langley Field, Thompson had witnessed the Schneider Cup Race over Hampton Roads; a U.S. Navy pilot took second place in the race flying an R3C-2 at the "fantastic" average speed of 231 miles per hour. The following year, as Thompson was helping to conduct flight research on seaplanes and rigid airships, Lindbergh had crossed the Atlantic. American aviation had boomed.

By the mid-1930s, Thompson's colleagues at Langley were beginning to explore the possibility of flight at more than 500 miles per hour. "And just as we got to the transonic field," Thompson exclaimed in a 1972 interview, "then all of a sudden we opened up with the supersonic field and find out that we're flying—militarily anyway—we're flying at speeds of [Mach] 2 and 3. And you just about get that pretty well understood and, Holy Smoke, here we are going to the moon and things like that."[14]

Somehow, in less than seventy years, aviation had moved from the Jenny to the X-15, from the drone of propellers to the roar of jets and rockets, from wind tunnels generating a maximum airflow speed of 90 miles per hour to tunnels generating Mach 8, from flight a few hundred feet above the ground to flight in space. For Thompson, the most incredible fact of this altogether incredible history was this: it was work that he and other individuals like him had done that was largely responsible for it.

How had these people been able to advance the technological front so far so fast? No doubt part of the answer rests—as Laurence K. Loftin, Jr., Thompson's longtime associate at Langley, points out in his 1985 book *Quest for Performance: The Evolution of Modern Aircraft*—in the unique nature of the airplane itself: "In no other type of machine, with the possible exception of space vehicles, do the often conflicting requirements of performance, safety, reliability, and economic viability place such a high premium on detailed design optimization."[15] But before this inherent motive for innovation could become a potent force driving the work of professional engineers, the airplane had to achieve a mission—and one recognized as important by a modern industrial society. This achievement was realized, of course, during World War I, as "the demands of combat aviation," together with the international struggle for air superiority, transformed the airplane from a "useless freak" into a highly practical and versatile vehicle whose every detail had to be designed rigorously if the total configuration was to prove successful.[16]

From that time on, as more and different missions for aircraft were conceived, aircraft design criteria changed radically and almost without interruption. And no single organization of aeronautical engineers felt the pressures and exhilarations of this flux any more than did those who worked at Langley. Faced with the challenge of constantly looking ahead, probing for problems, and establishing the feasibility of what the country's leaders wanted to be doing in relation to flight, the best NACA researchers made change into a habit, and the expectation of surprise into a rule of thumb. In many respects, this was a humbling experience, to learn over and over again that what they had *not* known at a given moment played as important a role in the evolution of an aircraft as what they had known, and to admit, both to themselves and to outsiders, that aerodynamic effects about which they had known absolutely nothing just a few years ago (such as the effects of boundary layer, in the late 1920s; of wind tunnel turbulence and of wing surface roughness, in the 1930s; and of the total cross-sectional area of wings, fuselage, and tail on transonic drag rise, in the late 1940s) were now among the most critical items on their research agenda.[17]

But the humbling experience prepared them well for what was to come. As the golden age of atmospheric flight reached full maturity in the 1950s—with only a few major things (like a supersonic transport) left undone—many of these same researchers moved successfully from their mature aeronautical specialties into the new ones of spaceflight and reentry. This cross-flow came largely from the young field of hypersonics.

The role of this cross-flow in the indisputable success of the overall American aerospace effort from the 1950s to the present should hold some meaning for those who are today educating and employing engineers. As Arthur L. Donovan, an historian of technology, wrote in a 1985 report for the National Research Council's Committee on Education and Utilization of the Engineer:

> The realization that the engineering manpower system possesses a high degree of resilience has important implications for engineering education. Because we are incapable of predicting with a useful degree of accuracy future shifts in the demand for engineers, and because the response times of universities are so slow in comparison with those of the marketplace for engineering labor, attempts to tie the content of engineering education closely to the needs of industry have been of little use in anticipating or responding to short-term stresses in the engineering manpower system. Indeed, attempts to forge a tight link between engineering curricula and specific employment opportunities have probably done more harm than good from the point of view of individual flexibility and the resilience of the system, for they have emphasized specialization at an early stage of education and have thereby reduced the breadth of understanding that in fact facilitates movement between specialties.[18]

One wonders if the young engineers who are undergoing early, highly specialized training today will be able to show the same conceptual and technical versatility that enabled Langley's engineers of the NACA period to move so fruitfully among old and new disciplines.

Appendixes

A Special Note on Appendixes

History books have *users* as well as *readers*. Though my dearest wish is for this book to attract cover-to-cover readership, I also wish its contents to be useful to those who may wish to take it off the shelf in search of special information or a certain fact about the history of NACA Langley laboratory. The appendixes permit the dissemination of a wealth of useful information which cannot be included gracefully in the text. The appendix user should note that similar material is to be found in the appendixes of Alex Roland's *Model Research*.

The following abbreviations have been used in the appendixes:

ACR	Advance Confidential Report
AERL	Aircraft Engine Research Laboratory (Cleveland, Ohio)
AR	*Annual Report* of the NACA
BuAer	U.S. Navy Bureau of Aeronautics
HSFS	High-Speed Flight Station (Muroc, California)
LaRC	Langley Research Center
TM, TN, TR	Technical Memorandum, Technical Note, Technical Report
USAAC	U.S. Army Air Corps
WR	Wartime Report

Appendix A

Law Establishing the NACA

(Public Law 271, 63d Congress, approved 3 March 1915)

An Advisory Committee for Aeronautics is hereby established, and the President is authorized to appoint not to exceed twelve members, to consist of two members from the War Department, from the office in charge of military aeronautics; two members from the Navy Department, from the office in charge of naval aeronautics; a representative each of the Smithsonian Institution, of the United States Weather Bureau, and of the United States Bureau of Standards; together with not more than five additional persons who shall be acquainted with the needs of aeronautical science, either civil or military, or skilled in aeronautical engineering or its allied sciences: Provided, That the members of the Advisory Committee for Aeronautics, as such, shall serve without compensation: Provided further, That it shall be the duty of the Advisory Committee for Aeronautics to supervise and direct the scientific study of the problems of flight, with a view to their practical solution, and to determine the problems which should be experimentally attacked, and to discuss their solution and their application to practical questions. In the event of a laboratory or laboratories, either in whole or in part, being placed under the direction of the committee, the committee may direct and conduct research and experiment in aeronautics in such laboratory or laboratories: And provided further, That rules and regulations for the conduct of the work of the committee shall be formulated by the committee and approved by the President.

That the sum of $5,000 a year, or so much thereof as may be necessary, for five years is hereby appropriated, out of any money in the Treasury not otherwise appropriated, to be immediately available, for experimental work and investigations undertaken by the committee, clerical expenses and supplies, and necessary expenses of members of the committee in going to, returning from, and while attending, meetings of the committee: Provided, That an annual report to the Congress shall be submitted through the President, including an itemized statement of expenditures.

Appendix B

Personnel

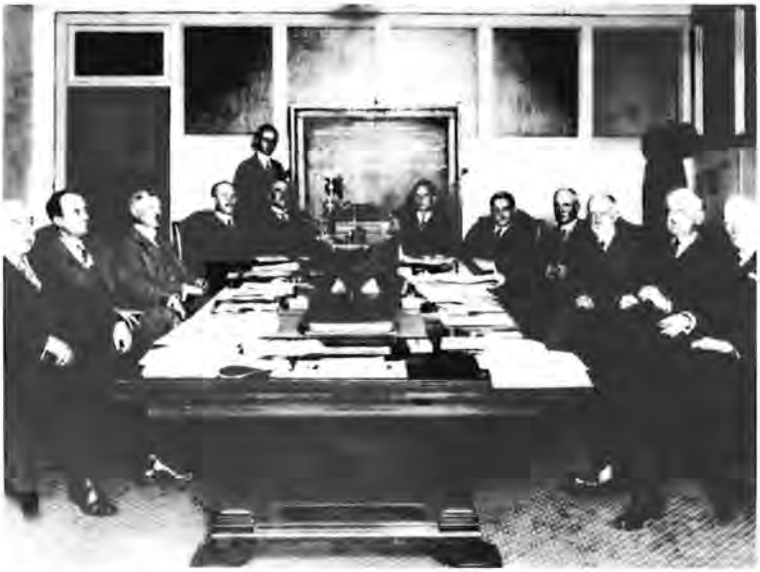
This appendix provides information about the chairmen of the NACA, members of the NACA Main Committee, executive officers at NACA headquarters, growth of the Langley staff 1919–1958, Langley researchers during the NACA years, and Langley officers at the end of the NACA years.

1. Chairmen of the NACA

One of the founding principles and greatest virtues of the NACA was the idea that the Committee should be composed of individuals of such high character and distinction as to insulate it as much as possible from political and economic influence. The Main Committee had only eight chairmen:

George Percival Scriven Brig. Gen., USA; Chief Signal Officer	1915–1916
William Frederick Durand Professor and Head of Department of Mechanical Engineering, Stanford University	1916–1918
John Ripley Freeman Consulting Engineer, Providence, R.I.	1918–1919
Charles Doolittle Walcott Secretary, Smithsonian Institution	1919–1927
Joseph Sweetman Ames Professor of Physics and President, Johns Hopkins University	1927–1939
Vannevar Bush President, Carnegie Institution	1939–1941
Jerome Clarke Hunsaker Chairman, Department of Aeronautical Engineering, MIT	1941–1956
James Harold Doolittle (Lt. Gen., USAF, Ret.) Vice President, Shell Oil Company of New York	1956–1958

The average period in office of an NACA chairman was 6½ years. The three longest tenures were those of Walcott (8 years), Ames (12 years), and Hunsaker (15 years). The chairmen's lives spanned from 1850 to the present, from wooden ships to spacecraft. All eight were born before 1900, the youngest (Doolittle) in 1896, seven years prior to the Wrights' landmark flight at Kitty Hawk. The first five chairmen, in fact, were born before the end of the Civil War. Four (Durand, Bush, Hunsaker, and Doolittle) lived to see the creation of NASA in 1958. In 1986, Doolittle was still living. The average age of the



Meeting of the Main Committee in the NACA conference room, Washington, D.C., 1920. At the far right are three men who would serve as chairman of the NACA: (from right to left) Charles D. Walcott, Joseph S. Ames, and William F. Durand. Fourth from the left is Orville Wright. Standing by the chalkboard is John F. Victory, the NACA secretary.

NACA chairmen at time of appointment was approximately 60, equivalent to the average age of new judges on the U.S. Supreme Court. Walcott chaired the Committee to the age of 77, Ames until 75. Bush was the youngest man to head the body; he was 49 at the time of his appointment.

Half of the group were born and raised in New England, including Durand (a small farm near Beaver Falls, Conn.), Freeman (West Bridgton, Maine), Ames (Manchester, Vt.), and Bush (Everett, Mass.). Only Hunsaker (Creston, Iowa; raised in Detroit and Saginaw, Mich.) and Doolittle (Alameda, Calif.) came from outside the Northeast. All seem to have come from solid middle-class families. Bush was the son of a Protestant minister (he received the name "Vannevar" in honor of the clergyman who married his parents). Hunsaker's father was a newspaper editor and publisher.

After Scriven resigned his chairmanship in 1916, no military man on active duty was chairman. However, Durand and Hunsaker had both graduated with high honors from the U.S. Naval Academy, the latter first in his class. Durand then served for seven years in the navy's new engineering corps, and Hunsaker spent a year at sea before being selected by the Construction Corps to study naval architecture at MIT. Doolittle rose to flag rank in his career as a military aviator. In the 1920s and 1930s he gained extensive experience in research and development in aeronautical research instruments and techniques. His most famous exploit, of course, was leading the bombing raid on Tokyo in 1942.

Some chairmen had experience in industry. Freeman acted as a consulting engineer on water power and mill construction for various large manufacturing corporations in the United States and Canada. In the early 1900s he studied the water supply of Greater New York City and tested gun carriages for the War Department. He resigned within

NACA chairman Jerome C. Hunsaker (left) chats with executive secretary John F. Victory during the NACA inspection held at Langley in October 1956.

James H. Doolittle, the NACA's last chairman, visited Langley in February 1928 in his Curtiss Racer, the plane in which he won the 1925 Schneider Trophy Race.

a year to travel to the Orient, where he consulted with the Chinese government on the improvement of its canal system. Bush worked in 1913 with General Electric. Hunsaker was employed in the late 1920s with Bell Telephone Laboratories to develop airway wire, radio, and weather services, and with Goodyear-Zeppelin to help build the *Akron* and *Macon* and to promote airships as transoceanic transportation. Doolittle worked for Shell Oil Company for many years, first as a manager of its aviation department and then as an executive officer.

Several spent most of their professional lives in teaching and research. Durand's 37 years of teaching began in mechanical engineering at Michigan State University in 1887. Four years later, he moved to Cornell to take charge of its graduate school of marine engineering and naval architecture. He accepted the chair of mechanical engineering at Stanford in 1904, staying in it until the age of mandatory retirement (65) in 1924. Ames taught physics at Johns Hopkins University, his alma mater, from 1890 to 1926, when he became the school's provost; later he became the school's president. Four chairmen of the NACA had strong ties to MIT. In 1914 Hunsaker returned to MIT, his alma mater, as head of the departments of mechanical and aeronautical engineering, expanding those programs to include studies in supersonics, aeroelasticity, vibration, instrumentation, automatic control, and jet propulsion. Bush taught at MIT from 1919 to 1939, advancing from professor of electric power transmission to vice-president and dean of the school of engineering. Bush, Freeman, and Doolittle earned engineering degrees at MIT.

The achievements, titles, and honors awarded this group, too numerous to list here, include some of the highest academic, civilian, and military awards presented in this country.

Sources: Series of biographical profiles entitled "Meet the Committee," appearing in Langley *Air Scoop*, 1944–1958; *Who's Who in Engineering: A Biographical Dictionary of the Engineering Profession*, 6th ed., New York, 1945; *American Men and Women of Science*, 10th ed., New York & London, 1960; *McGraw-Hill Modern Scientists and Engineers*, 3 vols., New York, 1980; Alex Roland, *Model Research*, NASA SP-4103, 1985.

2. Members of the NACA Main Committee

Public Law 271, 63d Cong., 1st sess., (see appendix A) set the number of NACA members originally at 12. Public Law 549, 80th Cong., 2d sess., raised the number to 15, and Public Law 908, 70th Cong., 1st sess., raised it to 17.

To ensure the primacy of government interests in the new agency, the enabling act established a ratio of seven government members to five from the private sector. The government majority was preserved in the subsequent increases in Committee size. The purpose of enlarging the NACA from 12 to 15 in 1929 was to make room for at least one representative of the new aeronautics section of the Department of Commerce, which had been created by the Air Commerce Act of 1926. The Civil Aeronautics Act of 1938 specified that at least two representatives of the Department of Commerce always sit with the Committee, guaranteeing government's predominance by a ratio of 9 to 6. Congress approved the 1948 increase in NACA membership from 15 to 17 in order to add a representative of the new Department of Defense, setting the government-to-private-interest ratio at 10 to 7.

From 1915 to the creation of NASA in 1958, a total of 120 men (and no women) served on the NACA:

> Most Committee appointments from government service were ex officio: i.e., the incumbent of a post like head of the air force or secretary of the Smithsonian Institution was automatically appointed to the NACA. Length of service ... depended on tenure in the [primary] government post, and this varied from agency to agency. Until 1938, appointments from private life were until the incumbent resigned; after 1938, they were for five years, though often renewed. (Roland, *Model Research*, app. B, p. 423.)

Approximately half of the 120 members (58) were on active military duty; at least another 5 had served as officers in the armed forces. Forty represented the civilian side of the federal government. There were 22 private citizens, 9 of whom were employed by large corporations at the time of their membership.

Though the army and navy each had two seats, the frequent transfer of military members tended to weaken their influence. The average length of NACA service by active military men was approximately three years. The NACA members averaging the longest tenures came from the Weather Bureau (14.5 years), the Smithsonian Institution (11 years), and the Bureau of Standards and the private sector (9 years each). Representatives from other government agencies averaged only three years in office. Like the military services, however, the Department of Commerce was compensated for its high turnover rate by always having two men on the Committee.

The following list of Main Committee members is arranged alphabetically.

Abbot, Dr. Charles G. Secretary, Smithsonian Institution	1928–1945
Adams, Joseph P. Civil Aeronautics Board	1952–1956
Alison, John R. Assistant Secretary of Commerce	1947–1949
Ames, Dr. Joseph S. Johns Hopkins University	1915–1939
Arnold, Henry H. General of the Air Force	1938–1946
Astin, Dr. Allen V. Director, Bureau of Standards	1952–1958
Bane, Col. Thurman H. USA	1919–1922
Bassett, Preston R. Sperry Gyroscope Co., Inc.	1953–1958

NACA meeting, 19 October 1939. Left to right: Brig. Gen. George H. Brett; Clinton M. Hester; Rear Adm. John H. Towers; Lyman J. Briggs; Charles A. Lindbergh; Orville Wright; Jerome C. Hunsaker; George W. Lewis; Vannevar Bush, chairman; George J. Mead, vice-chairman; John F. Victory, secretary; Charles G. Abbot; Edward P. Warner; Maj. Gen. Henry H. Arnold; Robert H. Hinckley; Capt. Sydney M. Kraus; Francis W. Reichelderfer.

Brett, Lt. Gen. George H. USAAC	1939–1942
Briggs, Dr. Lyman J. Director, Bureau of Standards	1933–1945
Bristol, Capt. Mark L. USN, Director, Naval Aeronautics	1915–1916
Bronk, Dr. Detlev W. Rockefeller Foundation for Medical Research	1945–1958
Burden, Dr. William A. M. Assistant Secretary of Commerce	1942–1947
Burgess, George K. Director, Bureau of Standards	1923–1932
Bush, Dr. Vannevar President, Carnegie Institution	1938–1948
Carmichael, Dr. Leonard Secretary, Smithsonian Institution	1953–1958

Cassady, Vice Adm. John H. USN, Deputy Chief of Naval Operations (Air)	1950–1952
Clark, Col. Virginius USA	1917–1918
Combs, Vice Adm. Thomas S. USN, Deputy Chief of Naval Operations (Air)	1952–1953; 1955–1956
Compton, Dr. Karl T. Research and Development Board	1948–1949
Condon, Dr. Edward U. Director, Bureau of Standards	1945–1951
Connoly, Donald H. (Maj. Gen., USA, Ret.) Administrator of Civil Aeronautics	1940–1942
Cook, Rear Adm. Arthur B. USN, Chief, Bureau of Aeronautics	1931–1934; 1936–1939
Craigie, Lt. Gen. Lawrence C. USAF	1951–1954
Craven, Capt. Thomas USN, Director of Naval Aviation	1919–1921
Crawford, Dr. Frederick C. Thompson Products, Inc.	1954–1958
Curry, Maj. Gen. John F. USAAC	1924–1926
Damon, Ralph S. Trans World Airlines, Inc.	1953–1956
Davis, Thomas W. S. Assistant Secretary of Commerce	1950–1953
Davis, Vice Adm. William V., Jr. USN, Deputy Chief of Naval Operations (Air)	1956–1958
Doherty, Robert E. Carnegie Institute of Technology	1940–1941
Doolittle, James H. (Lt. Gen., USAF, Ret.) Shell Oil Co.	1948–1958
Duncan, Vice Adm. Donald B. USN, Deputy Chief of Naval Operations (Air)	1947–1948
Durand, Dr. William F. Stanford University	1915–1933; 1941–1945
Echols, Maj. Gen. Oliver P. USAF	1942–1945

Fagg, Dr. Fred D., Jr.	Director, Bureau of Air Commerce	1937–1938
Fechet, Maj. Gen. James E.	USA, Chief of Air Service	1928–1931
Fitch, Vice Adm. Aubrey W.	USN, Deputy Chief of Naval Operations (Air)	1944–1945
Foote, Paul D.	Assistant Secretary of Defense (Research and Engineering)	1957–1958
Foulois, Maj. Gen. Benjamin D.	USA, Chief, USAAC	1929–1930; 1932–1936
Freeman, John R.	Consulting Engineer, Providence, R.I.	1918–1919
Furnas, Clifford C.	Assistant Secretary of Defense (Research and Engineering)	1956–1957
Gardner, Vice Adm. Matthias B.	USN, Deputy Chief of Naval Operations (Air)	1952–1953
Gilmore, Brig. Gen. William E.	USA	1926–1929
Gregg, Willis R.	Chief, Weather Bureau	1934–1938
Guggenheim, Harry F.	Long Island, N.Y.	1929–1938
Harrison, Rear Adm. Lloyd	USN, Deputy and Assistant Chief, BuAer	1953–1955
Hayford, Dr. John F.	Northwestern University	1915–1923
Hazen, Ronald M.	Allison Division, General Motors	1946–1954
Hester, Clinton M.	Administrator, Civil Aeronautics Authority	1938–1940
Hinckley, Robert H.	Assistant Secretary of Commerce	1939–1942
Hines, Rear Adm. Wellington T.	USN, Assistant Chief for Procurement, BuAer	1957–1958
Hunsaker, Dr. Jerome C.	Massachusetts Institute of Technology	1922–1923; 1938–1958
Kenly, Maj. Gen. William L.	USA, Director of Military Aeronautics	1918–1919

King, Rear Adm. Ernest J. USN, Chief, BuAer	1933–1936
Kinler, Brig. Gen. Walter G. USA	1939–1940
Kraus, Rear Adm. Sydney M. USN, BuAer	1936–1943
Land, Capt. Emory S. USN, BuAer	1923–1929
Lindbergh, Charles A. New York City	1931–1939
Littlewood, William American Airlines	1944–1953
Lonnquest, Rear Adm. Theodore C. USN, BuAer	1947–1952
McCain, Vice Adm. John S. USN, Deputy Chief of Naval Operations (Air)	1942–1944
McCarthy, Charles J. Chance Vought Aircraft, Inc.	1957–1958
MacCracken, William P., Jr. Assistant Secretary of Commerce	1929–1938
McIntosh, Col. Lawrence W. USA	1923–1924
Marvin, Charles F. Chief, Weather Bureau	1915–1934
Mead, George J. Hartford, Conn.	1939–1943
Menoher, Maj. Gen. Charles T. USA, Chief of Air Service	1919–1921
Mitscher, Vice Adm. Marc A. USN, Deputy Chief of Naval Operations (Air)	1945–1946
Moffett, Rear Adm. William A. USN, Chief, BuAer	1921–1933
Mulligan, Denis Director, Bureau of Air Commerce	1938
Murray, Robert B., Jr. Under Secretary of Commerce	1953–1954
Newton, Byron R. Assistant Secretary of the Treasury	1915–1918

Noble, Edward J. Chairman, Civil Aeronautics Authority	1938–1939
Nyrop, Donald W. Chairman, Civil Aeronautics Board	1951–1952
Ofstie, Vice Adm. Ralph A. USN, Deputy Chief of Naval Operations (Air)	1953–1954
Pace, Rear Adm. Ernest M., Jr. USN, BuAer	1943–1944
Patrick, Maj. Gen. Mason M. USA, Chief of Air Service	1921–1927
Pfingstag, Rear Adm. Carl J. USN, Chief for Field Activities, BuAer	1955–1957
Powers, Maj. Gen. Edward M. USAF	1945–1949
Pratt, Maj. Gen. Henry C. USA	1930–1935
Price, Vice Adm. John D. USN, Deputy Chief of Naval Operations (Air)	1948–1950
Pupin, Michael I. Columbia University	1915–1922
Putt, Lt. Gen. Donald L. USAF, Deputy Chief of Staff, Development	1949–1958
Pyle, James T. Administrator of Civil Aeronautics	1957–1958
Quarles, Donald A. Assistant Secretary of Defense	1954–1956
Radford, Vice Adm. Arthur W. USN, Deputy Chief of Naval Operations (Air)	1946–1947
Raymond, Dr. Arthur E. Douglas Aircraft Co., Inc.	1946–1956
Reber, Lt. Col. Samuel USA, OIC Aviation Section, Signal Corps	1915–1916
Reichelderfer, Dr. Francis W. Chief, U.S. Weather Bureau	1939–1958
Rentzel, Delos W. Administrator of Civil Aeronautics; Under Secretary of Commerce	1948–1951
Richardson, Capt. Holden C. USN, Naval Constructor	1915–1917

Richardson, Rear Adm. Lawrence B. USN, BuAer	1944–1946
Rickenbacker, Capt. Edward V. Eastern Air Lines, Inc.	1956–1958
Robins, Brig. Gen. Augustine W. USA	1935–1939
Rothschild, Louis S. Under Secretary of Commerce for Transportation	1955–1958
Ryan, Oswald Civil Aeronautics Board	1954
Sabine, Wallace C. Bureau of Aircraft Production	1918
Saville, Maj. Gen. Gordon P. USAF	1950–1951
Scriven, Brig. Gen. George P. USA, Chief Signal Officer	1915–1917
Spaatz, Gen. Carl USAF, Chief of Staff	1946–1948
Squier, Maj. Gen. George O. USA, Chief Signal Officer	1916–1918
Stevens, Rear Adm. Leslie C. USN, Assistant Chief, BuAer	1946–1947
Stratton, Samuel W. Director, Bureau of Standards	1915–1931
Taylor, Rear Adm. David W. USN, Chief Naval Constructor (civilian member from 1922)	1917–1938
Towers, Rear Adm. John H. USN, Assistant and Chief, BuAer	1917–1919; 1929–1931; 1939–1942
Twining, Gen. Nathan F. USAF, Chief of Staff	1954–1957
Vandenberg, Gen. Hoyt S. USAF, Chief of Staff	1948–1950
Vidal, Eugene L. Director, Bureau of Air Commerce	1933–1937
Walcott, Dr. Charles D. Secretary, Smithsonian Institution	1915–1927
Warner, Dr. Edward P. *Aviation* magazine; industry consultant; later, Civil Aeronautics Board	1929–1945

Webster, William Chairman, Research and Development Board	1950–1951
Westover, Maj. Gen. Oscar USA, Chief of Air Corps	1936–1938
Wetmore, Dr. Alexander Secretary, Smithsonian Institution	1945–1952
Weyerbacher, Cdr. Ralph D. USN, BuAer	1934–1936
White, Gen. Thomas D. USAF, Chief of Staff	1957–1958
Whitman, Walter G. Chairman, Research and Development Board	1951–1953
Wright, Orville Dayton, Ohio	1920–1948
Wright, Dr. Theodore P. Director of Aircraft Production; Administrator of Civil Aeronautics; Cornell University	1942–1953

Source: Eugene M. Emme, *Aeronautics and Astronautics: An American Chronology of Science and Technology in the Exploration of Space, 1915–1960* (Washington: NASA, 1961), pp. 202–205.

3. Executive Officers, NACA Headquarters

Director of Research	
George W. Lewis	1919–1947
Hugh L. Dryden	1947–1958
Secretary/Executive Secretary	
John F. Victory	1915–1958
Assistant Secretary and Executive Officer	
Edward H. Chamberlin	1918–1958

George W. Lewis

Hugh L. Dryden

4. Growth of Langley Staff, 1919–1958

Fiscal Year	Professional	Nonprofessional	Total
1919	4	7	11
1920	12	15	27
1921	12	32	44
1922	18	38	56
1923	23	52	75
1924	36	62	98
1925	39	72	111
1926	44	92	136
1927	45	104	149
1928	60	108	168
1929	79	110	189
1930	100	128	228
1931	102	155	257
1932	111	159	270
1933	110	150	260
1934	109	140	249
1935	111	164	275
1936	138	203	341
1937	149	253	402
1938	166	260	426
1939	204	320	524
1940	277	462	739
1941	369	571	940
1942	439	804	1243
1943	594	1624	2218
1944	937	2351	3288
1945	832	2388	3220
1946	692	2005	2697
1947	734	2051	2785
1948	899	2070	2969
1949	1082	2183	3265
1950	1158	2230	3388
1951	1235	2276	3511
1952	1255	2302	3557
1953	1189	2171	3360
1954	1138	2134	3272
1955	1072	2067	3139
1956	1034	2222	3256
1957	1093	2114	3207
1958	1151	2145	3296

Source: NASA chart, “Growth of Langley's Staff,” 16 September 1965, LaRC Historical Archives.

5. Langley Researchers

The following Langley researchers are mentioned in the text or notes of this book. In general, the list includes those researchers to whom more than mere passing reference is made. Some of the "researchers" included were in fact technical service employees who supported research.

Name & year of birth	College degree(s) & dates of Langley employment
Abbott, Ira H. (b. 1906)	MIT, B.S. '29, aero. eng.; 1929–1947 (detailed to NACA HQ)
Alford, William L. (b. 1921)	Wayne State, B.S. '49, aero. eng.; 1949–NASA
Allen, H. Julian (b. 1910)	Stanford, A.B. '32, M.A. '35, mech. eng.; 1936–1940 (transferred to Ames)
Ames, Milton B. (b. 1913)	Georgia Tech, B.S. '36, aero. eng.; 1936–1941 (detailed to NACA HQ)
Ayer, Bruce E. (b. 1904)	Univ. Illinois, B.S. '29, mech. eng.; 1929–1941 (transferred to AERL)
Baals, Donald D. (b. 1916)	Purdue, B.S. '38, mech. eng.; M.S. '39, aero. eng.; 1939–NASA
Babberger, Carl (b. 1909)	Stanford, B.S. '34, mech. eng.; 1936–1939
Bacon, David L. (b. 1895)	Yale, B.A. '16, physics; 1920–1924
Bailey, Frederick J., Jr. (b. 1911)	MIT, B.S. '34, aero. eng.; 1934–1955
Bamber, Millard J. (b. 1898)	Univ. Michigan, B.S. '26, aero. eng.; 1927–1944
Becker, John V. (b. 1913)	New York Univ., B.S. '35, mech. eng.; M.S. '36, aero. eng.; 1936–NASA
Beeler, De Elroy (b. 1915)	Kansas St. Univ., B.S. '41, mech. eng.; 1941–1954 (transferred to HSFS)
Biermann, Arnold E. (b. 1904)	Purdue, B.S. '28, mech. eng.; 1928–1942
Biermann, David J. (b. 1907)	Purdue, B.S. '29, mech. eng.; 1929–1943
Bioletti, Carlton (b. 1906)	Univ. California, B.S. '30, mech. eng.; 1930–1940 (transferred to Ames)
Bogdonoff, Seymour M. (b. 1921)	Rensselaer Poly. Inst., B.S. '42, aero. eng.; 1942–1947
Brevoort, Maurice J. (b. 1900)	Allegheny Coll., B.S. '22; Univ. Nebraska, M.A. '24, physics; 1930–1958
Brown, Clinton E. (b. 1920)	Purdue, B.S. '41, M.S. '42, mech. eng.; 1942–NASA
Buckley, Edmund C. (b. 1904)	Rensselaer Poly. Inst, B.S. '27, electr. eng.; 1930–NASA

The flight crew in front of a Fokker trimotor with experimental NACA cowlings, March 1929. Front row, left to right: John Spivey, John Haines, Robert Hunt, Charles Shobe, Melvin Gough, Samuel Eakin, Walter Quigley, Siegfried Hunsecker, and Frederick Hunsecker. Back row: "Mac" McConnaha, George Bulifant, William McAvoy, Thomas Carroll, Ernest Johnson, Charles Wolf, Raymond Braig, and John Houston.

Name & year of birth	College degree(s) & dates of Langley employment
Busemann, Adolf (b. 1901)	Tech. Hochschule Braunschweig, Ing. Dipl. '24, Dr. Ing. '25; 1947–NASA
Butler, T. Melvin (b. 1917)	Virginia Poly. Inst., B.S. '39, indus. eng.; 1939–NASA
Carroll, Thomas (b. 1890)	Georgetown, law '20; 1920–1929
Clay, William C. (b. 1903)	MIT, B.S. '28, aero. eng.; 1928–1939
Collier, Thomas M. (b. 1905)	No degree; 1926–1932
Conner, D. William (b. 1920)	Ohio Northern, B.S. '42, mech. eng.; 1942–NASA
Corson, Blake W., Jr. (b. 1908)	Univ. Richmond, B.S. '32, math; 1935–NASA
Crain, Percy J. (b. 1913)	Georgia Tech, B.S. '35, mech. eng.; 1939–NASA
Crigler, John L. (b. 1905)	William and Mary, B.S. '27, math and physics; 1928–NASA
Crowley, John W., Jr. (b. 1899)	MIT, B.S. '20, mech. eng.; 1921–1947 (transferred to NACA HQ)
Davidson, Milton (b. 1917)	Univ. Alabama, B.S. '38, aero. eng.; 1940–1946

Name & year of birth	College degree(s) & dates of Langley employment
Dawson, John R. (b. 1909)	Rice Inst., B.S. '29, civil eng.; 1930–NASA
Dearborn, Clinton H. (b. 1897)	Univ. Michigan, B.S. '22, mech. eng.; 1927–1950 (transferred to NACA HQ)
DeFrance, Smith J. (b. 1896)	Univ. Michigan, B.S. '22, aero. eng.; 1922–1940 (transferred to Ames)
Delano, James B. (b. 1911)	New York Univ., B.S. '35, mech. eng.; 1936–1953
Demming, Arthur (b. 1905)	Univ. California, B.S. '30, electr. eng.; 1930–1939
Donaldson, Coleman duPont (b. 1922)	Rensselaer Poly. Inst., B.S. '42, aero. eng.; 1943–1952
Donely, Philip (b. 1909)	MIT, B.S. '31, aero. eng.; 1931–NASA
Donlan, Charles J. (b. 1916)	MIT, B.S. '38, aero. eng.; 1938–NASA
Draley, Eugene C. (b. 1916)	Catholic Univ., B.S. '38, aero. eng.; 1938–NASA
Duberg, John E. (b. 1917)	Manhattan Coll., B.S. '38; Virginia Poly. Inst., M.S. '40; Univ. Illinois, Ph.D. '48, civil eng.; 1943–NASA
Ebert, John W., Jr. (b. 1913)	Johns Hopkins, B.S. '35, mech. eng.; 1935–1947 (detailed to NACA HQ)
Ellerbrock, Herman H. (b. 1906)	Johns Hopkins, B.S. '27, mech. eng.; 1930–1942 (transferred to AERL)
Ellis, Macon C., Jr. (b. 1918)	Alabama Poly. Inst. (Auburn), B.S. '39, aero. eng.; 1939–NASA
Faget, Maxime A. (b. 1921)	Louisiana St. Univ., B.S. '43, mech. eng.; 1946–NASA
Fairbanks, Andrew J. (b. 1901)	Cornell, B.S. '24, mech. eng.; 1924–1927
Fairbanks, Karl J. (b. 1902)	Univ. Michigan, B.S. '24, aero. eng.; 1924–1926
Fedziuk, Henry A. (b. 1916)	Univ. Michigan, B.S. '39, aero. eng.; 1939–NASA
Ferri, Antonio (b. 1912)	Univ. di Roma, Dr. Ing. '36; 1944–1951
Forrest, Marvin (b. 1906)	No degree; 1927–NASA
Gardiner, Arthur (b. 1898)	Swarthmore, A.B. '20, civil eng.; 1920–1927
Garrick, I. Edward (b. 1910)	Univ. Chicago, A.B. '30, math and physics; 1930–NASA
Gerrish, Harold C. (b. 1891)	Univ. Maine, B.S. '14, electr. eng.; 1926–1943 (transferred to Ames)
Gilruth, Robert R. (b. 1913)	Univ. Minnesota, B.S. '35, M.S. '36, aero. eng.; 1937–NASA

A jousting contest was one of the highlights of the NACA's annual picnic at Grandview Beach on Chesapeake Bay in July 1929. On the barrel to the left is Edward R. "Ray" Sharp, a future engineer-in-charge of the NACA's Aircraft Engine Research Laboratory in Cleveland, Ohio.

Name & year of birth	College degree(s) & dates of Langley employment
Glass, Clindon (b. 1899)	No degree; 1925–1940
Goett, Harry J. (b. 1910)	Holy Cross, B.S. '31, physics and math; New York Univ., B.S. '33, aero. eng.; 1936–1940
Gough, Melvin N. (b. 1906)	Johns Hopkins, B.S. '26, mech. eng.; 1926–NASA
Griffith, Leigh M. (b. 1882)	California Inst. of Technology, B.S. '02, mech. eng.; 1918–1924
Gustafson, Frederic B. (b. 1913)	Univ. Kansas, B.S. '36, M.S. '38, mech. eng.; 1938–NASA
Harris, Thomas A. (b. 1903)	William and Mary, B.S. '29, physics; 1927–NASA
Hartmann, Edwin P. (b. 1905)	Marquette, B.S. '29, mech. eng.; 1930–1940 (transferred to Ames)
Heldenfels, Richard R. (b. 1920)	MIT, B.S. '42, aero. eng.; 1947–NASA
Hemke, Paul E. (b. 1890)	Univ. Chicago, A.B., M.A.; Johns Hopkins, Ph.D. '24, physics; 1924–1927
Herrnstein, William H., Jr. (b. 1905)	Univ. Michigan, B.S. '27, aero. eng.; 1927–NASA
Higgins, George J. (b. 1897)	Univ. Michigan, B.S. '23, aero. eng.; 1923–1928
Hill, Paul R. (b. 1909)	Univ. California, B.S. '36, mech. eng.; 1939–NASA
Hooker, Ray W. (b. 1906)	Purdue, B.S. '29, mech. eng.; 1930–NASA

Name & year of birth	College degree(s) & dates of Langley employment
Hoover, Herbert H. (b. 1912)	Univ. Tennessee, B.S. '34, mech. eng.; 1940–1952 (killed in crash of B-45 during research flight)
House, Rufus O. (b. 1907)	William and Mary, B.S. '29, math; 1927–NASA
Howe, Edward A. (b. 1918)	Syracuse, B.S. '43, aero. eng.; 1943–NASA
Huckel, Vera (b. 1908)	Univ. Pennsylvania, B.S. '29, math; 1939–NASA
Hulcher, Charles A. (b. 1910)	No degree; 1930–1954
Jacobs, Eastman N. (b. 1902)	Univ. California, B.S. '24, mech. eng.; 1925–1944
Joachim, William F. (b. 1893)	Univ. Minnesota, B.S. '21, mech. eng.; 1922–1929
Johnson, Caldwell C. (b. 1919)	No degree (2 years at Univ. Virginia); 1937–NASA
Johnson, Harold I. (b. 1920)	Univ. Michigan, B.S. '41, aero. eng.; 1941–NASA
Johnson, W. Kemble (b. 1911)	Virginia Poly. Inst., B.S. '33, chem. eng.; 1934–NASA
Jones, Robert T. (b. 1910)	No degree (took classes at Univ. Missouri and Catholic Univ.); 1934–1946 (transferred to Ames)
Joyner, Upshur T. (b. 1908)	William and Mary, B.S. '31, physics and math; 1931–NASA
Kantrowitz, Arthur (b. 1911)	Columbia Univ., B.S. '34, M.A. '35, Ph.D. '45, physics; 1935–1946
Kaplan, Carl (b. 1904)	Johns Hopkins, B.S. '26, M.S. '28, chemistry; Ph.D. '30, physics; 1932–1956
Katzoff, Samuel (b. 1909)	Johns Hopkins, B.S. '29, Ph.D. '34, chemistry; 1936–NASA
Keffer, Percy R. (b. 1897)	No degree (Newport News Shipbuilding and Dry Dock Co. Apprentice School, 4 years); 1920–NASA
Kemper, Carlton (b. 1899)	Univ. Pennsylvania, B.S. '23, mech. eng.; 1924–1943 (transferred to AERL)
King, Paul B. (b. 1892)	No degree (took classes at Univ. Utah); 1922–1927
Kirschbaum, Howard W. (b. 1903)	Univ. Michigan, B.S. '27, aero. eng.; 1927–1941 (transferred to Ames)
Knight, Montgomery (b. 1901)	MIT, B.S. '22, electr. eng.; 1925–1930

Name & year of birth	College degree(s) & dates of Langley employment
Koppen, Otto (b. 1900)	MIT, B.S. '23, mech. eng.; 1924–1925
Korycinski, Peter F. (b. 1917)	Georgia Tech, B.S. '42, mech. eng.; 1942–NASA
Kotanchik, Joseph N. (b. 1903)	MIT, B.S. '38, mech. eng.; 1938–NASA
Kraft, Christopher C., Jr. (b. 1924)	Virginia Poly. Inst., B.S. '44, aero. eng.; 1945–NASA
Kuhn, Paul (b. 1903)	Univ. Michigan, B.S. '30, aero. eng.; 1931–NASA
Lindsey, Walter F. (b. 1910)	Virginia Military Inst., B.S. '30, electr. eng.; 1931–NASA
Loftin, Laurence K., Jr. (b. 1919)	Univ. Virginia, B.S. '43, mech. eng.; 1944–NASA
Lorenzen, Coby (b. 1905)	Univ. California, B.S. '29, mech. eng.; 1929–1931
Lundquist, Eugene E. (b. 1907)	Univ. Nebraska, B.S. '28, Ph.D. '44, civil eng.; 1941–NASA
McAvoy, William H. (b. 1896)	No degree; 1921–1940 (transferred to Ames)
McHugh, James G. (b. 1906)	North Dakota St. Coll., B.S. '29, mech. eng.; 1929–1956
McLellan, Charles H. (b. 1915)	Univ. Washington, B.S. '37, aero. eng.; 1938–NASA
Maher, Edward T. (b. 1920)	Pierce Business School '39; 1947–NASA
Mattson, Axel T. (b. 1916)	North Carolina St., B.S. '41, mech. eng.; 1941–NASA
Mayo, William B. (b. 1910)	No degree; 1939–NASA
Messick, J. Cabelle (b. 1909)	No degree; 1928–NASA
Miller, Elton W. (b. 1881)	George Washington Univ., B.S. '08, mech. eng.; 1922–1948
Mixson, Robert E. (b. 1895)	No degree; 1919–1957
Morgan, William C. (b. 1880)	Cornell, M.S. '06, mech. eng.; 1921–1950
Mueller, James N. (b. 1920)	Alabama Poly. Inst. (Auburn), B.S. '42, mech. eng.; B.S. '46, aero. eng.; 1946–NASA
Munk, Max M. (b. 1890)	Hanover Poly., Dipl. Ing. '14; Univ. Göttingen, Ph.D. '18, physics; 1921–1927
Myers, Edward A. (b. 1884)	No degree; 1927–1933
Neihouse, Anshal I. (b. 1908)	Virginia Poly. Inst., B.S. '30, M.S. '31, electr. eng.; 1935–NASA
Nelson, William J. (b. 1916)	MIT, B.S. '38, aero. eng.; 1938–NASA

LMAL accounting office, 1936, with pictures of the Wright brothers on the wall.

Name & year of birth	College degree(s) & dates of Langley employment
Nichols, Mark R. (b. 1916)	Alabama Poly. Inst. (Auburn), B.S. '38, aero. eng.; 1940–NASA
Norton, Frederick H. (b. 1897)	MIT, B.S. '18, physics; 1918–1923
Orlin, William J. (b. 1920)	Rensselaer Poly. Inst., B.S. '42, aero. eng.; 1938–NASA
O'Sullivan, William J., Jr. (b. 1915)	Notre Dame, B.S. '37, aero. eng.; 1938–NASA
Parkinson, John B. (b. 1907)	Webster Inst. of Nav. Arch., B.S. '29, nav. arch.; 1931–NASA
Parsons, John F. (b. 1908)	Stanford, B.S. '30, mech. eng.; 1931–1940 (transferred to Ames)
Pearson, Henry A. (b. 1906)	Worcester Poly. Inst., B.S. '30, mech. eng.; 1930–NASA
Phillips, William H. (b. 1918)	MIT, B.S. '39, M.S. '40, aero. eng.; 1940–NASA
Pinkel, Benjamin (b. 1909)	Univ. Pennsylvania, B.S. '30, electr. eng.; 1931–1942 (transferred to AERL)
Pinkerton, Robert M. (b. 1905)	Bradley Univ., B.S. '28, physics; 1929–1931, 1932–1941, 1949–1952
Platt, Robert C. (b. 1910)	MIT, B.S. '31, aero. eng.; 1931–1940

Name & year of birth	College degree(s) & dates of Langley employment
Polhamus, Edward C. (b. 1921)	Univ. Maryland, B.S. '44, mech. eng.; 1944–NASA
Purser, Paul E. (b. 1918)	Louisiana St. Univ., B.S. '39, aero. eng.; 1939–NASA
Recant, Isadore G. (b. 1913)	New York Univ., B.S. '33, mech. eng.; B.S. '34, aero. eng.; 1937–NASA
Reeder, John P. (b. 1916)	Univ. Michigan, B.S. '38, aero. eng.; 1938–NASA
Regier, Arthur A. (b. 1909)	Kansas St. Univ., B.S. '33, electr. eng.; 1938–NASA
Reid, Elliott G. (b. 1900)	Univ. Michigan, B.S. '22, M.S. '23, aero. eng.; 1922–1927
Reid, Henry J. E. (b. 1895)	Worcester Poly. Inst., B.S. '19, electr. eng.; 1921–NASA
Reiser, Walter H. (b. 1893)	No degree; 1923–1951
Rhode, Richard V. (b. 1904)	Univ. Wisconsin, B.S. '25, mech. eng.; 1925–1950 (detailed to NACA HQ)
Ribner, Herbert S. (b. 1913)	California Inst. of Technology, B.S. '35; Washington Univ., M.S. '37, Ph.D. '39, physics; 1940–1949 (transferred to AERL)
Ritter, W. K. (b. 1906)	Univ. Oklahoma, B.S. '26, mech. eng.; 1929–1943 (transferred to AERL)
Rizzo, Frank S. (b. 1886)	MIT, B.S. '17, mech. eng.; M.S. '22, aero. eng.; 1922–1932
Robinson, Russell G. (b. 1907)	Stanford, B.S. '28, M.S. '30, aero. eng.; 1930–1939 (detailed to NACA HQ)
Rodert, Lewis A. (b. 1906)	Univ. Minnesota, B.S. '30, aero. eng.; 1936–1940 (transferred to Ames)
Rogallo, Francis M. (b. 1912)	Stanford, A.B. '32, M.S. '35, aero. eng.; 1936–NASA
Rollin, Vernon G. (b. 1906)	Univ. Minnesota, B.S. '29, mech. eng.; 1929–1943 (transferred to AERL)
Rothrock, Addison (b. 1903)	Penn St. Univ., B.S. '25, physics; 1926–1942 (transferred to AERL)
Rubert, Kennedy F. (b. 1906)	Cornell, B.S. '27, M.S. '33, Ph.D. '35, mech. eng.; New York Univ., B.S. '28, aero. eng.; 1941–NASA
Runckel, Jack F. (b. 1916)	Univ. Wisconsin, B.S. '39, mech. eng.; 1940–NASA

Name & year of birth	College degree(s) & dates of Langley employment
Schey, Oscar W. (b. 1897)	Univ. Minnesota, B.S. '23, mech. eng.; 1923–1943 (transferred to AERL)
Schultz, Fred W. (b. 1903)	Kansas St. Univ., B.S. '26, agriculture; 1929–1930
Seidman, Oscar (b. 1909)	Rutgers, B.S. '29, M.S. '31, math and chemistry; 1931–1945
Sherman, Albert E. (b. 1910)	New York Univ., B.S. '31, aero. eng.; 1931–1943
Shoemaker, James M. (b. 1902)	Purdue, B.S. '25, mech. eng.; MIT, M.S. '28, aero. eng.; 1925–1927, 1931–1935
Shortal, Joseph A. (b. 1908)	Texas A&M, B.S. '29, mech. eng.; 1929–NASA
Silverstein, Abraham (b. 1908)	Rose Poly. Inst., B.S. '29, mech. eng.; 1929–1943 (transferred to AERL)
Soulé, Hartley A. (b. 1905)	New York Univ., B.S. '27, aero. eng.; 1927–NASA
Stack, John (b. 1906)	MIT, B.S. '28, aero. eng.; 1928–NASA
Stickle, George W. (b. 1906)	Purdue, B.S. '29, mech. eng.; 1929–1951
Stone, David G. (b. 1919)	Univ. Washington, B.S. '41, aero. eng.; 1941–NASA
Strailman, Gilbert T. (b. 1900)	No degree; 1927–1954
Theodorsen, Theodore (b. 1897)	Univ. Trondheim, Dr. Ing. '22; Johns Hopkins, Ph.D. '29, physics; 1929–1946
Thompson, Floyd L. (b. 1898)	Univ. Michigan, B.S. '26, aero. eng.; 1926–NASA
Toll, Thomas A. (b. 1914)	Univ. California, B.S. '41, mech. eng.; 1941–NASA
Tozier, Robert E. (b. 1908)	Linfield College (Ore.), B.S. '30, physics; 1930–1943 (transferred to AERL)
Truscott, Starr (b. 1886)	Univ. Michigan, B.S. '09, mech. eng.; 1926–1946
Truszynski, Gerald M. (b. 1921)	Rutgers, B.S. '44, electr. eng.; 1944–1954 (transferred to HSFS)
Tucker, Virginia (b. 1909)	Univ. North Carolina, A.B. '35, math; 1935–1947
Turner, Harold R., Sr. (b. 1894)	No degree; 1922–NASA
Turner, Harold R., Jr. (b. 1919)	No degree (attended Univ. Virginia night school extension); 1938–1953
Turner, Lindsey I., Jr. (b. 1912)	Georgia Tech, B.S. '34, aero. eng.; 1937–NASA

Test pilots James B. Whitten (left) and John P. "Jack" Reeder prepare to investigate the handling qualities of a tandem helicopter in 1951.

Name & year of birth	College degree(s) & dates of Langley employment
Underwood, William J. (b. 1916)	Georgia Tech, B.S. '40, aero. eng.; 1940–1948
von Doenhoff, Albert E. (b. 1910)	Columbia Univ., B.S. '30, electr. eng.; 1931–NASA
Waldron, Clyde D. (b. 1907)	Univ. Arkansas, B.S. '29, mech. eng.; 1929–1942 (transferred to AERL)
Ward, Kenneth E. (b. 1903)	Univ. California, B.S. '29, mech. eng.; 1929–1940
Ward, Vernon G. (b. 1919)	Syracuse Univ., B.S. 43, mech. eng.; 1943–1951
Ware, Marsden (b. 1896)	Rensselaer Poly. Inst., B.S. '18, mech. eng.; 1918–1927

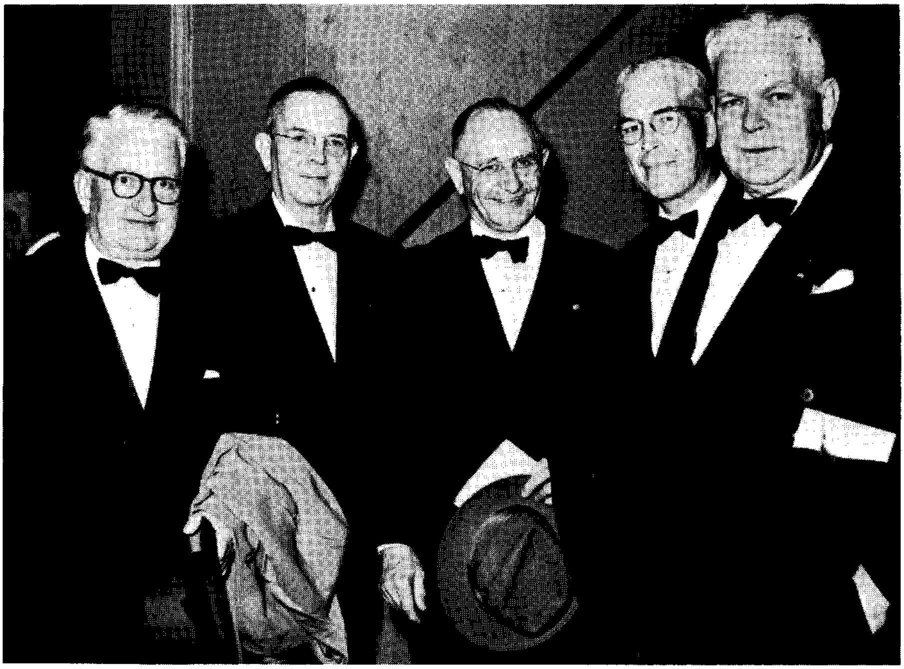
Name & year of birth	College degree(s) & dates of Langley employment
Weick, Fred E. (b. 1899)	Univ. Illinois, B.S. '22, mech. eng.; 1925–1929, 1930–1936
Wenzinger, Carl (b. 1903)	Swarthmore, A.B. '25, M.S. '29, mech. eng.; 1927–1946
Wetmore, Joseph W. (b. 1909)	MIT, B.S. '31, aero. eng.; 1931–NASA
Wheatley, John B. (b. 1908)	Stanford, B.S. '30, mech. eng.; 1930–1937
Whitcomb, Richard T. (b. 1921)	Worcester Poly. Inst., B.S. '43, mech. eng.; 1943–NASA
Williams, Walter C. (b. 1919)	Louisiana St. Univ., B.S. '39, aero. eng.; 1940–1954 (transferred to HSFS)
Wilson, Herbert A., Jr. (b. 1914)	Georgia Tech, B.S. '34, aero. eng.; 1937–NASA
Windler, Raymond (b. 1902)	Univ. Missouri, B.S. '24, mech. eng.; 1928–NASA
Wood, Donald H. (b. 1898)	Rensselaer Poly. Inst., B.S. '20, mech. eng.; 1924–1941 (transferred to Ames)
Woods, Robert J. (b. 1904)	Univ. Michigan, B.S. '28, aero. eng.; 1928–1929
Wright, Ray H. (b. 1907)	Univ. Kentucky, M.S. '33, physics; 1935–NASA
Young, Pearl I. (b. 1895)	Univ. North Dakota, B.A. '19, physics; 1922–1943 (transferred to AERL)
Zimmerman, Charles H. (b. 1907)	Univ. Kansas, B.S. '28, mech. eng.; 1929–1937, 1946–NASA

Source: NACA/NASA biographical files, LaRC Historical Archives.

6. Langley Officers at the End of the NACA Years

Director: Henry J. E. Reid
Associate Director: Floyd L. Thompson
Executive Assistant and Budget Officer: Rufus O. House
Chief, Research Reports Division: Henry A. Fedziuk
Assistant Director: John Stack
Chief, Compressibility Research Division: John V. Becker
Chief, Full-Scale Research Division: Eugene C. Draley
Chief, Theoretical Mechanics Division: Clinton E. Brown
Chief, Unitary Plan Wind Tunnel Division: Herbert A. Wilson, Jr.
Assistant Director: Robert R. Gilruth
Chief, Dynamic Loads Division: I. Edward Garrick
Chief, Pilotless Aircraft Research Division: Joseph A. Shortal
Chief, Structures Research Division: Richard R. Heldenfels
Assistant Director of Research: Hartley A. Soulé

Chief, Flight Research Division: Melvin N. Gough
Chief, Hydrodynamics Division: John B. Parkinson
Chief, Stability Research Division: Thomas A. Harris
Chief, Instrument Research Division: Edmund C. Buckley
Chief of Technical Services: Percy J. Crain
Chief, Engineering Service Division: John C. Messick
Chief, Mechanical Service Division: William B. Mayo
Chief, Maintenance Division: Marvin Forrest
Chief, Electrical Services Division: Joseph Getsug
Chief, Administrative Services: Elton W. Miller
Fiscal Officer: Edward A. Howe
Chief, Office Services Division: Edward T. Maher
Personnel Officer: T. Melvin Butler
Chief, Photographic Division: Harry H. Hamilton
Procurement and Supply Officer: Sherwood L. Butler



From left to right, John W. "Gus" Crowley, Edward H. Chamberlin, Smith J. DeFrance, Henry J. E. Reid, and Edward R. "Ray" Sharp at the NACA's 40th anniversary party at the Smithsonian Institution, April 1955. At the time of this anniversary, the five men had a total of nearly 170 years NACA experience and three were laboratory directors: DeFrance (Ames), Reid (Langley), and Sharp (Lewis).

Appendix C

Budget

In its 45-year lifetime, the NACA received just over $1 billion in federal funds, an average of almost $25 million a year. But the first time its annual budget actually reached $25 million was 1943—the middle of World War II. It was more money in one year than the NACA had received in its first 25 years combined. Clearly, World War II separated the NACA's budgetary history into two very unequal parts.

Table C-1, besides reporting the amounts appropriated to the NACA for each of its 45 years, demonstrates this two-part history. The left side of the table shows annual amounts for 1915 to 1940 in *thousands* of dollars; the right side shows annual amounts for 1941 to 1959 in *millions* of dollars. The left side indicates the fiscal modesty and slow growth of the NACA's early history, when the Committee almost never asked for any more money than it was sure it could get. Notice that appropriations reached the million-dollar level only in 1930. Not until the hard Depression years of 1932 and 1933 did Congress authorize less than the NACA had requested. Only about 3 percent of the billion-dollar NACA lifetime total came to the NACA in the 26-year period preceding American involvement in World War II (the period covered in the left side of table C-1). The right side of table C-1, covering 18 years, suggests the tremendous inflation of the NACA mission during World War II and in the Cold War that followed it, as the agency contributed to major national defense programs. The explanation for the peak in 1950 ($128 million) is the Deficiency Appropriation Act of 1950 (approved 29 June 1950). This act provided $75 million for the construction of wind tunnels authorized in the Unitary Wind Tunnel Plan Act of 1949 (Public Law 415, 81st Cong., approved 27 Oct. 1949). Nearly $15 million of this deficiency appropriation was allotted to Langley.

Langley's budgetary history is also divided clearly by World War II. Figure C-1, which traces the laboratory's annual expenditures throughout NACA history, confirms the periodization: the first phase is represented by a virtually horizontal line showing comparatively modest annual amounts, always less than $2 million; in the second phase, spending rises in a rapid 45-degree ascent to $34 million in 1958.

Table C-2 shows how expenditures were distributed among the major branches of the NACA between 1940 and 1958. Before 1940, there were only two branches, Langley and headquarters. One may estimate, from the known spending distribution during World War II and from analysis of Langley's prewar budget and accounting correspondence (LaRC Central Files, C77-1 and C83-1), that Langley spent about 90 percent of total NACA expenditures in the period 1917 to 1939, compared to 40 percent in the period 1940 to 1958.

Table C-1

NACA Appropriations

1915–1940		1941–1959	
Fiscal year	**Thousands $**	Fiscal year	**Millions $**
1915	5.0	1941	11.2
1916	5.0	1942	19.9
1917	87.5	1943	25.4
1918	112.0	1944	38.4
1919	205.0	1945	40.9
1920	175.0	1946	24.0
1921	200.0	1947	30.7
1922	200.0	1948	43.4
1923	225.6	1949	48.6
1924	307.0	1950	128.0
1925	470.0	1951	63.1
1926	534.0	1952	69.0
1927	513.0	1953	66.3
1928	550.0	1954	62.4
1929	836.7	1955	55.9
1930	1300.0	1956	72.7
1931	1321.0	1957	76.7
1932	1051.0	1958	117.3
1933	920.0	1959	101.1
1934	953.6	Subtotal	1095.0
1935	1255.7		
1936	2543.8		
1937	1630.5		
1938	1280.8		
1939	4063.9		
1940	4180.0		
Subtotal	24 926.1		
Total			$1.12 billion

Source: "NACA Budget Files, 1915–1958," LaRC Historical Archives.

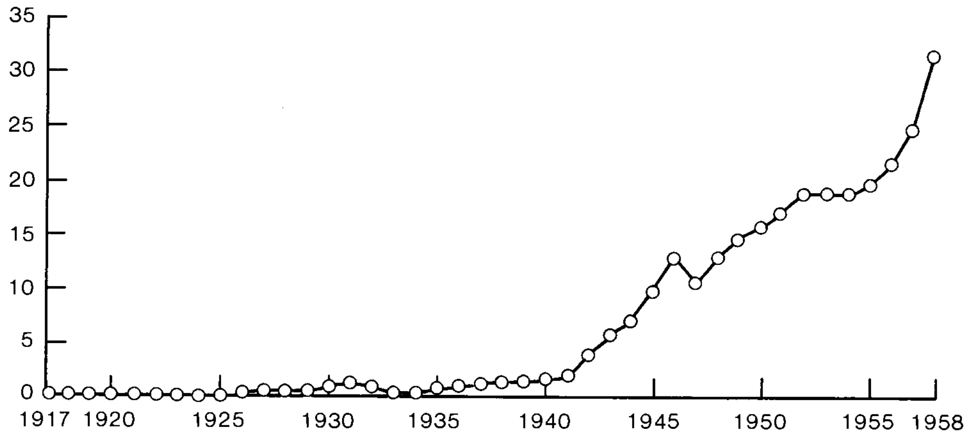


Figure C-1. Graph of Langley expenditures, 1917–1958, in millions of dollars.

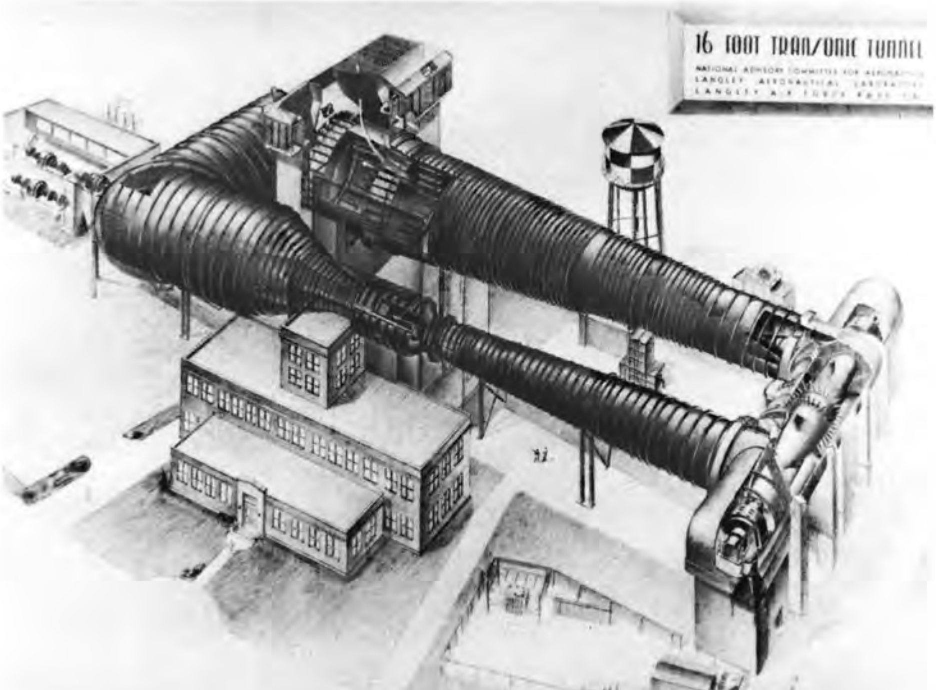
Table C-2

Expenditures by NACA Branches
1940–1958

Langley:	$282,759,760 = 40%
Lewis:	$258,759,760 = 37%
Ames:	$120,523,020 = 17%
NACA HQ:	$ 17,566,001 = 2.5%
HSFS:	$ 14,246,147 = 2.2% (1949–1958)
Wallops Island Flight Center:	$ 964,308 = 1.3% (1949–1958)

Source: Financial report sections of NACA annual reports, 1940–1958.

The NACA submitted its annual budget requests to Congress only after Langley and the other branches had prepared comprehensive budgetary estimates, supported by long and detailed explanations of current and projected expenses. As an example of the work involved in preparing budget requests, Document C-1 is the summary of operating expenses projected by Langley in August 1944 for FY 1946. The laboratory estimated that operating expenses would nearly double from FY 1944 to FY 1946. The field centers provided detailed justification for every piece of planned construction and new equipment. Document C-2 is Langley's estimate of construction and equipment costs for FY 1946. Document C-3 is the full text provided by Langley to justify over $2 million for construction of the most expensive single item requested for FY 1946, a new 35,000-horsepower drive system and housing unit for the 16-Foot High-Speed Wind Tunnel.



Phantom drawing of the 16-Foot Transonic Tunnel, 1950s. The new 35,000-horsepower drive system and its housing unit are at the right end of the facility.

3

LANGLEY MEMORIAL AERONAUTICAL LABORATORY
FISCAL YEAR 1946 ESTIMATES

SUMMARY - OPERATING EXPENSES

Page No.			F.Y. 1946 Estimated	F.Y. 1945 Estimated	F.Y. 1944 Expended
4	01	SERVICES			
		Positions	4,562	3,097	3,018
		Man-years	4,172.9	2,972.9	2,396.3
		Cost of regular services	9,110,240	6,133,010	4,846,673
		Cost of overtime services	1,822,048	1,226,602	1,012,464
		Cost of overtime services in excess of 48 hours	-	-	37,336
		Total cost, personal services	10,932,288	7,359,612	5,896,473
113	02	TRAVEL	34,000	19,000	26,229
115	03	TRANSPORTATION	45,000	12,000	29,685
117	04	COMMUNICATION	20,000	11,750	15,988
119	05	UTILITY SERVICES	303,140	152,000	126,656
130	07	(a) REPAIRS AND ALTERATIONS	550,090	109,038	141,824
156		(b) OTHER CONTRACTUAL SERVICES	30,735	15,575	22,345
176	08	SUPPLIES AND MATERIALS	1,476,892	753,552	743,598
189	09	EQUIPMENT	3,796,421	534,351	654,208
		TOTAL, OPERATING EXPENSES	17,188,566	8,966,878	7,657,006

Document C-1. (Source: "Langley Aeronautical Laboratory, F.Y. 1946 Estimates," 817 pages in two parts, 5 Aug. 1944, LaRC Central Files E12-5.)

LANGLEY MEMORIAL AERONAUTICAL LABORATORY

FISCAL YEAR 1946 ESTIMATES

SUMMARY, NEW ITEMS, CONSTRUCTION AND EQUIPMENT APPROPRIATION

		Estimated Cost
737	Addition and Alterations to Flight Research Laboratory	$ 278,000.00
744	Construction of West Area Cafeteria and Auditorium Building	817,000.00
747	Construction of Addition to Equipment Housing Building, Supersonic Wind Tunnel, West Area	205,250.00
749	Construction of High-Capacity Testing Machine and Housing, Structures Research Laboratory	1,012,000.00
754	Construction of Gasoline Distribution System, Propeller Research Tunnel	5,500.00
756	Construction of New 35,000 HP Drive and Equipment Housing, 16-Foot Wind Tunnel, West Area	2,039,470.00
763	Construction of Extension to the Existing Seawall and a New Concrete Road	80,500.00
765	Construction of Two-Story Addition to the Stability Tunnel Laboratory	55,522.00
768	Additions to Structures Research Laboratory	139,500.00
772	Construction of a Model Development Auxiliary, 16-Foot Wind Tunnel	695,000.00
776	Construction of a Concrete Pit for Airplane Scales, Aircraft Loads Building	45,370.00
779	Construction of Additions to 16-Foot Wind Tunnel Office Building	31,359.00
785	Construction of Office and Shop Addition to the Induction Aerodynamic Laboratory, West Area	50,000.00
790	Construction of Fluid and Gas Dynamics Analysis Laboratory Building and Apparatus	589,000.00
796	Additional Utilities, West Area	301,365.00
798	Construction of Maintenance Building, West Area	266,000.00
801	Construction of Additional Power Facilities for existing research facilities and equipment	607,050.00
806	Construction of Research Equipment Building, West Area	1,136,500.00
809	Construction of New Administration Building	840,000.00
814	Construction of Engineering Service Building	860,500.00
	TOTAL	$10,054,886.00

Document C-2. (Source: "Langley Aeronautical Laboratory, F.Y. 1946 Estimates," 817 pages in two parts, 5 Aug. 1944, LaRC Central Files E12-5.)

JUSTIFICATION OF NEED FOR CONSTRUCTION OF NEW 35,000 H.P. DRIVE AND EQUIPMENT HOUSING, 16-FOOT WIND TUNNEL, WEST AREA

1. DESCRIPTION:

This project consists of the installation of a 35,000 H.P. Drive in the 16-Foot Wind Tunnel to replace the existing 16,000 H.P. installation. The construction of motor and equipment housings and modifying the existing wind tunnel shell is also necessary. The attached Drawing, LD-28011 shows the location, elevations and general details of the proposed project.

The propellers and nacelles for the new drive will be located in a new tunnel section which will be constructed as an addition to the northwest section of the existing tunnel structure. A portion of the existing tunnel structure will be dismantled and removed, the new addition taking its place. The new tunnel section will be of steel and reinforced concrete construction with concrete foundations on piles.

The motors will be located in two housings at the east and west ends of the new tunnel section and each will be 24- by 24-feet in size and 49-feet high. The equipment housing will be 33-feet high and located adjacent to the east motor housing. The motor housings and equipment building will be of brick construction with concrete foundations on piles. The floors will be reinforced concrete and the roof of concrete construction with built up roofing on insulation. The equipment building will house the motor-generator sets, switch gear and controls for the new motors.

The new tunnel section, motor housing, nacelles and propellers will be constructed and installed before the existing tunnel shell is altered. The old motor, nacelles, propellers, etc. will be removed through the open end of the tunnel shell after the existing northwest section is removed and before the new section is joined to the present tunnel.

The new power installation will give a Mach number of .9 with the average model in place. The propeller arrangement will consist of two counter rotating propellers, eliminating the necessity for counter vanes and will make it possible to obtain a very symmetrical arrangement for the bearing support struts.

It is planned to encase the section of the tunnel in the region of the propellers with reinforced concrete, in order to give the necessary mass and rigidity to this section of the Tunnel.

The proposed design is considered the most practical from the standpoint of shaft installations, propeller weight and other similar considerations. A considerable increase in propeller efficiency over the existing installations will be made possible.

2. JUSTIFICATIONS:

The present 16,000-horsepower drive in the LMAL 16-Foot High-Speed Tunnel affords a maximum speed of approximately 500 mph and a Mach number of about 0.68. This speed seriously limits the study of compressibility effects which are increasingly serious in the operation of high-speed aircraft, since these effects become important at speeds slightly over the maximum now attainable in the tunnel. The existing tunnel structure and equipment are inherently suitable for operation at higher speeds, so that no major changes other than an increase in drive power are necessary to adapt the present tunnel to research at high air speeds.

A recent investigation of wind-tunnel performance has shown that for any tunnel of a given type, there is a limiting or "choking" Mach number that can be obtained, regardless of the power available. It, thus, becomes logical and appropriate, from both theoretical and experimental considerations, to provide any tunnel that purports to be a "high-speed" tunnel with sufficient power to reach this limiting condition. A study of the LMAL 16-Foot High-Speed Tunnel indicates that an increase in the drive power to 35,000-horsepower would permit the testing of models up to this maximum value of speed, thus fully realizing the capabilities of the equipment. Such an increase in performance is essential to the correct and effective solution of the problems of high-speed aircraft.

At the present time, there is but one wind tunnel in the country capable of reaching speeds comparable to dive speeds of modern pursuit aircraft at high values of Reynolds number. This tunnel is occupied full time with direct development testing of specific military aircraft. As the flow problems associated with these high speeds are entirely new types of phenomena involving compression shocks, little is known of the fundamental flow character and, hence, practically nothing is known of the aerodynamic forms suitable for such speeds. Equipment to accelerate aircraft model development testing is required as well as additional facilities to permit original research at these speeds. The existing LMAL 8-Foot High-Speed Tunnel will be capable of reaching the necessary high speeds when the installation of a new drive motor (now in progress) is complete, and the AAL 16-Foot High-Speed Tunnel has a high maximum speed, but these two tunnels, in addition to being already overburdened, do not constitute a full complement of high-speed large-scale research equipment any more than only two low-speed tunnels would be adequate in their field.

A large high-speed tunnel with a maximum attainable Mach number of the order of 0.9 is required for investigations of full-scale aircraft components at speeds now being attained in dives. In the immediate future, level flight speeds at Mach numbers approaching 0.8 appear to be possible. The principal full-scale airplane components requiring intensive research and development at these high speeds are as follows:

1.- Propellers.
2.- Control surfaces (both ailerons and tail controls).
3.- Wing sections.

Propellers.- The preliminary data available to date indicate that there are differences in the aerodynamic results obtained in high-speed wind tunnel tests of model propellers as compared with full-scale propellers. These differences become of increasing importance at high Mach numbers. Since the propeller problem becomes acute at Mach numbers above 0.7, it is of prime importance to carry the tests of full-scale propellers in the 16-foot high-speed tunnel at least to Mach numbers of 0.8 to adequately evaluate these high-speed effects.

Tests of a 10-foot diameter propeller recently completed in the 16-foot high-speed tunnel revealed severe flutter of the propeller blade at certain operating conditions. This condition caused a crack to develop in the blade. However, previous tests of a 4-foot diameter model of this propeller did not reveal the same flutter condition. The necessity for high-speed research using full-scale propellers is again indicated.

In investigating the propeller problem for a given pursuit or fighter type airplane, it is possible through full-scale tests in the 16-foot high-speed tunnel to determine the characteristics so obtained may then be used in analyzing flight test data obtained with the propeller installed in the airplane. It is obviously important that the wind-tunnel data extend over the same Mach number range as the flight tests.

In determining the effects of modifications to an existing propeller, such as the addition of an opening near the blade tip to allow hot air for de-icing purposes to flow through a hollow-blade, it is necessary to use a full-scale propeller. Modifications of this type are too small to permit accurate simulation on a model propeller. In the example cited involving the use of a hollow blade, it would also be impossible to construct a suitable model without very great difficulty.

Another aspect of the high-speed propeller problem that is requiring increasing study involves the determination of the vibration stresses set up in pusher propellers due to periodic passage of the propeller blades through the wake of the airplane wing. It is believed that such vibration stresses greatly increase when the wing wake enlarges at or above the critical compressibility speed of the wing which is reached at Mach numbers ranging from about 0.55 to 0.85. The use of full-scale propellers in vibration studies of this kind is required both because of the impossibility of installing adequate strain gage instrumentation on model propellers and because of the question as to whether the vibrations will be correctly simulated on small models.

Control Surfaces. - Actual control surfaces differ from the solid models tested in small high-speed wind tunnels in several important respects in addition to the scale differences. Structurally, the full-scale surfaces are much less stiff than the models, with the result that under high-speed flight loads the surfaces undergo large deflections. If the surfaces are fabric covered, the fabric either bulges or is depressed at high speed, thereby changing the shape of the control surfaces from that originally intended by the designer. Thus, while data from small solid models may serve as a guide, the determination of actual control characteristics for a high-speed airplane requires high-speed tests of the full-scale control surface. Such tests are made in the 16-foot high-speed tunnel of both ailerons and tail controls by mounting a section of the wing or tail containing the complete control surface from one wall of the tunnel. The inadequacy of the present speed range of the tunnel is emphasized by present tests for the Army Air Forces of the horizontal tail of the XP-83 airplane. The maximum Mach number attainable in the tests is 0.68, while the design level flight Mach number of the airplane is 0.76.

Wing sections. - Present research on the development of airfoils for high-speed airplanes is carried on (1) in a low-turbulence, low-speed pressure tunnel using full-size models, or (2) in small high-speed wind tunnels necessarily requiring small models. Recent measurements of relative airstream turbulence have indicated that the airstream of the 16-foot high-speed tunnel has a low turbulence level even at high airspeeds. Significant airfoil data may thus be obtained on large models in high-speed tests in which both the flight Mach number and the flight Reynolds number are simultaneously obtained.

The effects on drag of the surface condition of the airfoil can be accurately determined only by high-speed tests of a sample section of the actual production wing of a given airplane. Such effects become increasingly important as the airspeed is increased, both as a result of the thinning of the boundary layer and also as a result of reductions in the critical compressibility speed of the airfoil due to departures from the specified contour. As previously stated, airfoil critical speeds usually lie in the Mach number range from 0.55 to 0.85. It is essential, therefore, in tests of production airfoil samples that a maximum tunnel speed corresponding to M = 0.85 be attainable.

Various schemes have been proposed for reducing the rapid increase in drag that occurs when an airfoil is operated past its critical speed. The most promising of these plans involve the control of the airfoil boundary layer by suction through suitable slot arrangements immediately behind the shock wave. Tests of this type require the employment of large airfoil models so that the correct Reynolds number and boundary layer conditions will be obtained. The use of a large model is also required to permit accurate design of the suction slots. In tests of this kind, it will be desirable to reach a maximum effective Mach number of the order of 0.9.

Comparison of wind-tunnel and flight data. - The restriction to the airflow about a model imposed by the wind-tunnel walls requires that corrections of varying magnitude be applied to wind-tunnel data. These corrections become increasingly large as the Mach number is advanced. The ultimate determination of the corrections requires a comparison of flight and wind-tunnel data. In making such a correction, it is very highly desirable that the identical article, for example a propeller, be tested both in the wind tunnel and in flight. For this purpose, a large high-speed wind tunnel such as the 16-Foot High-Speed Tunnel is necessary. With the speed range extended to M = 0.9, the tunnel wall effects could be established over the complete range of usefulness of sub-sonic wind tunnels by direct comparison with flight.

3. COST ESTIMATE:

Building:		
181,600 cu. feet at .75	136,200.00	
Foundations for M.G. sets and motors	100,000.00	
Ventilating equipment for motors	20,000.00	
Ventilating equipment for M.G. sets	20,000.00	
Total		$ 276,200.00
Tunnel:		
Reinforcing existing test chamber	50,000.00	
Miscellaneous tunnel reinforcing	25,000.00	
Removal of existing motor; supports and tunnel sections	30,000.00	
New tunnel section, steel and concrete	314,000.00	
Mechanical equipment-drive shafts, bearings, etc.	135,000.00	
Electrical equipment-35,000 H.P. at $25.75	900,000.00	
Total		$ 1,454,000.00
Electrical equipment:		
23,000 volt bus structure	6,000.00	
23,000 volt switchgear	12,000.00	
23,000 volt cable	24,000.00	
Ducts and manholes for 23,000 volt cable	18,400.00	
Transformers, 2-12,000/16,000 kva 22,000:11,000 volts	64,000.00	
Transformer foundations and installations	7,000.00	
Ducts and manholes for 11,000 volt cable	3,000.00	
11,000 volt cable	4,000.00	
Control cable and telemetering	3,000.00	
1 15,000 kva, 11 kv to 22 kv transformer	40,000.00	
1 110 kv circuit breaker	12,000.00	
2 110 kv disconnect switches	4,020.00	
2 22 kv circuit breakers	6,000.00	
2 22 kv disconnect switches	850.00	
Installation of transformers and circuit breakers	5,000.00	
Total		$ 209,270.00

Additional electrical facilities to increase the guaranteed power supply to NACA by the Virginia Electric and Power Co. from 15,000 kw to 25,000 kw off peak.................... 100,000.00
(There will be a rebate of 10 percent of the gross electric bill each month made by the Virginia Electric and Power Company to pay for these extraordinary service connections).

Total.. 100,000.00

Total cost of project................................ $2,039,470.00

4. ESTIMATED CONSTRUCTION SCHEDULE:

Preparations of plans and specifications................ 3 months

Procurement of equipment)
Construction of equipment)............................ 10 months
Construction of tunnel)

Tunnel shut-down period................................ 30 days

Document C-8. Concluded. (Source: "Langley Aeronautical Laboratory, F.Y. 1946 Estimates," 817 pages in two parts, 5 Aug. 1944, LaRC Central Files E12-5.)

Appendix D

Facilities

Langley Memorial Aeronautical Laboratory (in July 1948, "Memorial" was dropped) was located at Langley Field in Elizabeth City County, Virginia, just north of the town of Hampton and some 100 miles south of Washington, D.C. By 1958, the lab occupied 773 acres divided into two areas by the runways of Langley Air Force Base. The original east area consisted of 23 acres, which the NACA used under War Department permit. The west area, developed in the early 1940s, consisted of 750 acres, 430 owned by the NACA and 320 by the army (later, by the air force). Runways, some utilities, and other facilities were used by the NACA and the military jointly.

Aerial view of Langley Field, 1950. The NACA's original east area is at the bottom of the picture, along the Back River. The west area, developed early in World War II, is at the top. The largest building in the west area, next to the woods, is the aircraft hangar.

The success enjoyed by NACA Langley as a research organization depended in large measure on the creative design and use of a variety of experimental equipment. This equipment included towing tanks (NACA Tank No. 1, operational 1931; Tank No. 2, operational 1942); a seaplane impact basin (1942); aircraft engine (1934), structures (1940), loads (1945), and instruments (1946) research laboratories; a helicopter apparatus (1944); and landing loads track (1955). By far the predominant type of facility, however, was the wind tunnel.

The following is a digest of the major research facilities at Langley in chronological order of their coming on line. Design features are summarized, as well as the purpose, cost, major modifications, and disposition of the equipment. Whenever possible, key members of the facility design teams are identified. Different NACA employees offer firsthand insights into the significance of the facilities.

The sources for this appendix are: Langley (Memorial) Aeronautical Laboratory building cost schedules, 1942–1958, (copies in LaRC Historical Archives); "Characteristics of Major Active Wind Tunnels at the Langley Research Center," NASA TM X-1130 (Washington, 1965); Alex Roland, *Model Research*, appendix E; Donald D. Baals and William R. Corliss, *Wind Tunnels of NASA*, NASA SP-440 (Washington, 1981); George W. Gray, *Frontiers of Flight: The Story of NACA Research* (New York, 1948), pp. 34–62; other sources as named and quoted below in specific cases.

5-Foot Atmospheric Wind Tunnel (AWT)

Purpose: To give NACA engineers some fast firsthand experience with a proven tunnel design and to inaugurate in-house aerodynamic testing of scale models for comparison with full-scale, free-flight results.

Initial cost: $38,000

Circuit and pressure: Nonreturn, atmospheric

Test section: 5' diameter, closed throat

Drive system: Fan; 200-HP electric motor

Maximum speed: 89 MPH

Special equipment: None

Key members of design team: Edward P. Warner and Frederick H. Norton

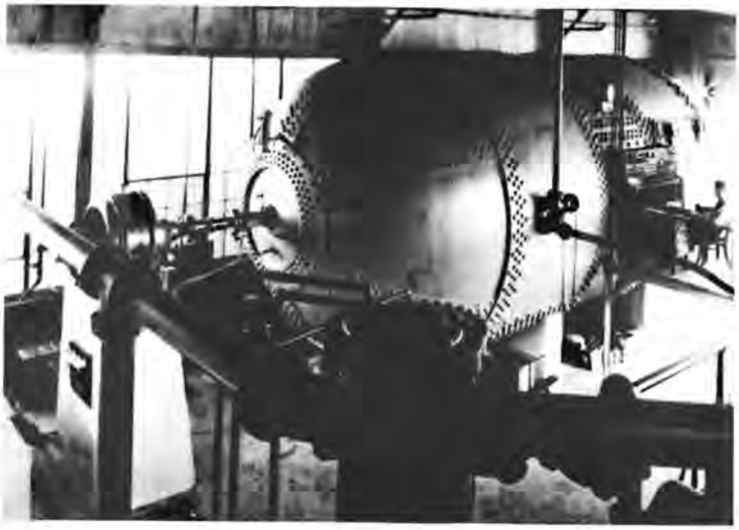
Authorized: 6 October 1917

Operational: 11 June 1920

Significance: "With relatively minor changes, the first Langley wind tunnel was patterned after one located at the British National Physical Laboratory. [The tunnel] was therefore obsolete when it was built .... From the standpoint of research, tunnel no. 1 was relatively unproductive." Baals and Corliss, *Wind Tunnels of NASA*, pp. 14–15.

Disposition: Dismantled in 1930 after a series of minor revisions. Replaced in December 1929 and June 1930, respectively, by the 5-Foot Vertical Tunnel and the 7 × 10-Foot Atmospheric Wind Tunnel.

References: F. H. Norton, "National Advisory Committee's 5-Foot Wind Tunnel," *Journal of the Society of Automotive Engineers* (21 May 1921): 1–7; TR 195.



Variable-Density Tunnel, 1923.

Variable-Density Tunnel (VDT)

Purpose: To conduct aerodynamic investigations at high Reynolds numbers.
Initial cost: $262,000
Circuit and pressure: Continuous, annular return; 20 atmospheres
Test section: 5' diameter, closed throat
Drive system: Fan; 250-HP electric motor
Maximum speed: 51 MPH
Special features: 85-ton pressure shell with walls made from steel plate lapped and riveted according to a practice standard in steam-boiler construction. Built at Newport News (Va.) Shipbuilding and Dry Dock Co.
Key members of design team: Max M. Munk (proposed design); Frederick H. Norton, David L. Bacon, Smith J. DeFrance
Authorized: 4 March 1921
Operational: 19 October 1922
Major modifications: Converted to open throat in April 1928 after serious fire damage in August 1927. Because the new open throat did not work properly, returned to closed throat in December 1930.
Significance: "This tunnel represented the first bold step by the NACA to provide its research personnel with the novel, often complicated, and usually expensive equipment necessary to press forward the frontiers of aeronautical science." Jerome C. Hunsaker, *Forty Years of Aeronautical Research*, Smithsonian Institution Publ. No. 4247 (Washington, 1956), p. 256.
Disposition: Used from the early 1940s as a high-pressure air storage tank. Thoroughly inspected in 1954. Closed for recertification in 1981. In 1983 the LaRC Pressure Systems Committee recommended that the vessel no longer be used due to its age and riveted construction.
References: TRs 185, 277, 391, 416; TN 60

Propeller Research Tunnel (PRT)

Purpose: To make possible accurate full-scale tests on aircraft propellers, fuselages, landing gear, tail surfaces, and other aircraft parts, as well as on model wings of large size.

Initial cost: $291,000

Circuit and pressure: Double return, atmospheric

Test section: 20' diameter, open throat

Drive system: Fan; two 1000-HP diesel engines (from naval submarine)

Maximum speed: 110 MPH

Special features: 8-bladed fan, 27' long

Key members of design team: Max M. Munk (proposed design); Fred E. Weick, Elton W. Miller, Donald H. Wood

Authorized: April 1925

Operational: July 1927

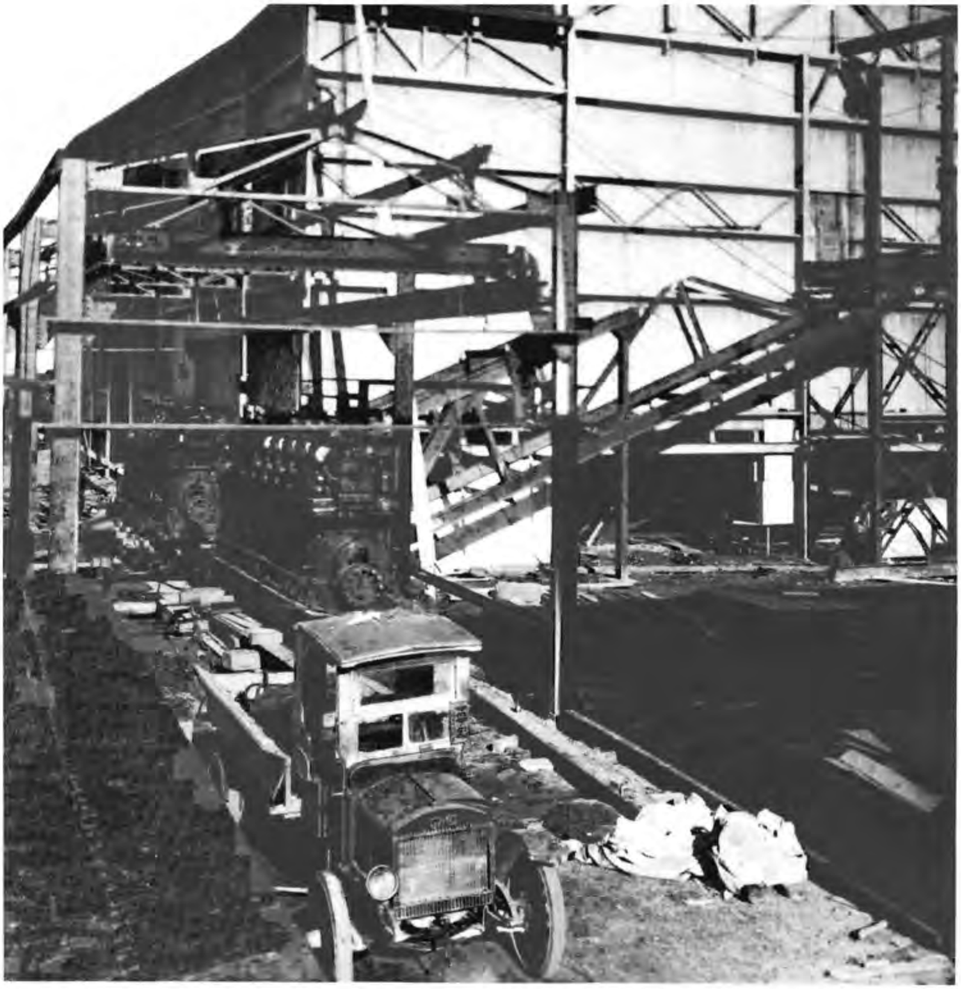
Major modifications: Changed to electric drive in 1933.

Significance: "The demonstrated inadequacies of theory, plus the failure to obtain a consistent correlation between model and full-scale results, made it clear that propeller data must ultimately come from tests at full scale. It was equally clear [that] full-scale testing in flight was slow and expensive. The only alternative was to build a wind tunnel large enough to take a full-sized propeller." Walter G. Vincenti, "The Air-Propeller Tests of W. F. Durand and E. P. Lesley: A Case Study in Technological Methodology," *Technology and Culture* 20 (1979): 739.

Disposition: Torn down in 1950 to make way for the 8-Foot Transonic Pressure Tunnel.

Reference: TR 300

PRT engineer Donald H. Wood ponders the unlikely transfer of a submarine engine from railcar to NACA truck, May 1926. Two such diesel engines powered the PRT.



Neither the Hampton nor the Newport News generating plant was powerful enough to supply the electricity needed to drive the Propeller Research Tunnel, so the NACA arranged to use two 1000-horsepower diesel engines salvaged from a navy T-2 submarine. Langley installed the two engines end-to-end, with their crankshafts connected to a large sheave, or pulley, between them. This sheave assembly was connected by belts to another sheave, 55 feet away, which turned the shaft of the propeller fan and drove the air through the tunnel. Here the engines are being readied for installation in the PRT building.

11-Inch High-Speed Tunnel (11-Inch HST)

Purpose: To begin the investigation of dynamic phenomena occurring as the flow of air approached Mach 1 over small models.
Initial cost: Under $10,000
Circuit and pressure: Nonreturn, atmospheric
Test section: 11" diameter, closed throat
Drive system: Induction; compressed air from Variable-Density Tunnel injected at annular port immediately downstream from test section.
Maximum speed: Mach 1 (with no model in throat)
Key members of design team: Eastman N. Jacobs (open-throat design, 1928); John Stack and W. F. Lindsey (closed-throat design, 1932)
Authorized: ca. 1927
Operational: Open throat, 1928; closed throat, 1932
Major modifications: Closed-throat test section, 1932
Significance: "The injection scheme permitted runs of only about one minute before the pressure plummeted to useless values. These short runs, however, were sufficient to demonstrate the sharp rise in drag, the loss of lift, and the changes in pitching moments that occur near Mach 1." Baals and Corliss, *Wind Tunnels of NASA*, pp. 23–24.
Disposition: Replaced by 4 × 18-Inch High-Speed Tunnel in August 1940 and by 4 × 19-Inch High-Speed Tunnel in August 1949.
Reference: TR 463

5-Foot Vertical Wind Tunnel

Purpose: To investigate the spinning characteristics of aircraft.
Initial cost: $8000
Circuit and pressure: Single-return, atmospheric
Test section: 5' diameter, open throat
Drive system: Fan; 50-HP electric motor
Maximum speed: 80 MPH
Special features: A vertical-axis balance
Key member of design team: Charles H. Zimmerman
Authorized: ca. 1928
Operational: December 1929
Major modifications: Changed to 4 × 6-foot, closed throat in 1938.
Significance: "By creating conditions that caused models to spin in the tunnel, spin-recovery procedures could be worked out on the ground without danger to pilots and planes." Baals and Corliss, *Wind Tunnels of NASA*, p. 19.
Disposition: Deactivated
References: TR 387, TN 734

7 × 10-Foot Atmospheric Wind Tunnel

Purpose: To study general aerodynamic effects with particular reference to stability and control problems and the improvement of the lifting power of wings.

Initial cost: $64,000

Circuit and pressure: Single-return, atmospheric

Test section: 7' × 10', closed throat

Drive system: Fan; 200-HP electric motor

Maximum speed: 80 MPH

Special features: Six-component, floating-frame balance that could measure forces and moments exerted along and about spatial axes of the tunnel airstream.

Authorized: 1928

Operational: 8 July 1930

Significance: "[This tunnel] was a simple, plain workhorse. [It] went on, for some 15 years, to handle an ever increasing workload of essential low-speed testing—including noteworthy systematic studies of stability, control, and high-lift devices. The 7-by-10 proportions were known from theory to minimize certain tunnel-wall correction factors .... The flat 10-foot wide 'floor' and the 7-foot high 'ceiling' allowed researchers and technicians easy access for model adjustments. [These dimensions] were retained in the early 1940s when four new atmospheric tunnels having increased speeds were procured for Ames and Langley." John V. Becker to author.

Disposition: Deactivated ca. 1946.

References: TRs 412, 664

Full-Scale Tunnel (FST)

Purpose: To make possible wind tunnel research into areas that could be explored best with full-scale models or with actual aircraft.

Initial cost: $900,000

Circuit and pressure: Double-return, atmospheric

Test section: 30' × 60', open throat

Drive system: Two fans; two 4000-HP electric motors

Maximum speed: 118 MPH

Key members of design team: Smith J. DeFrance, Abraham Silverstein, Clinton H. Dearborn

Authorized: February 1929

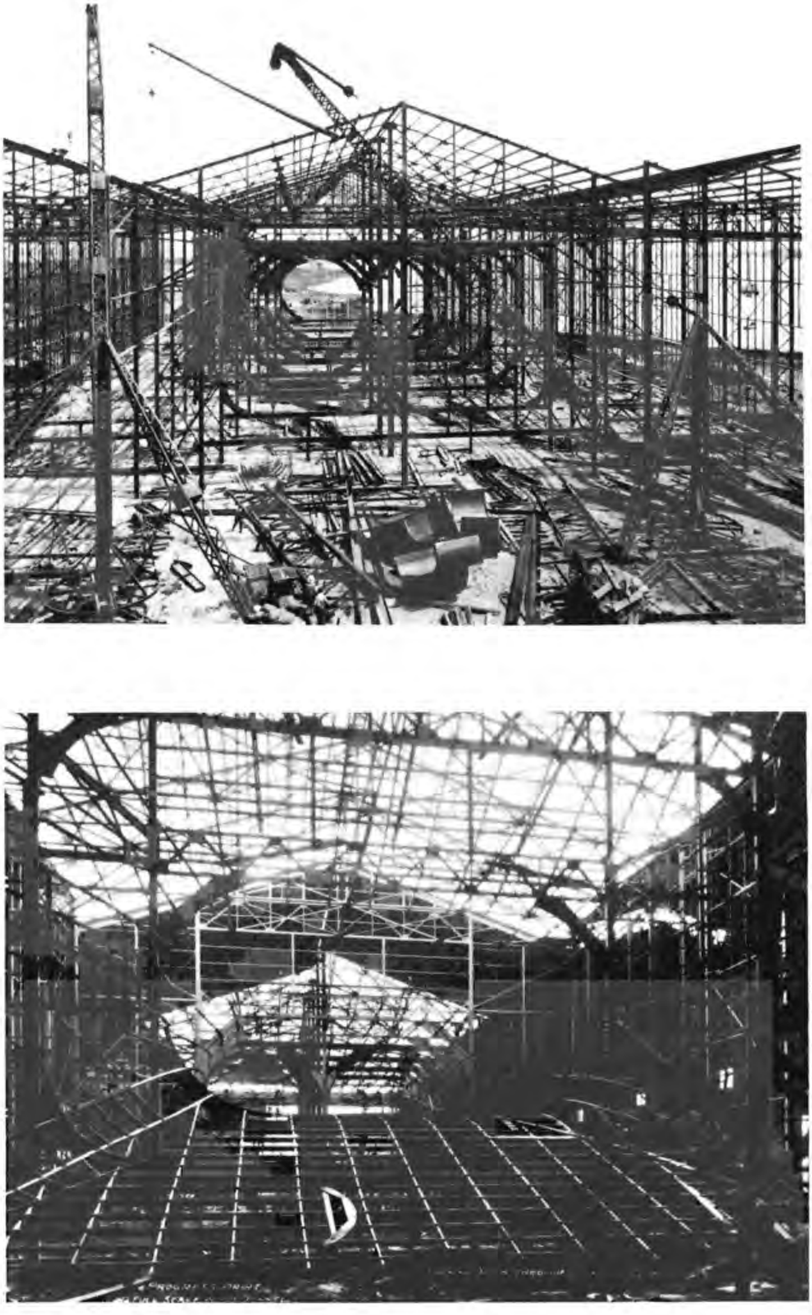
Operational: 27 May 1931 (formally dedicated during the 6th Annual Aircraft Engineering Conference)

Major modifications: Equipped for free-flight dynamic model studies in 1960s. Underwent major rehabilitation in 1977.

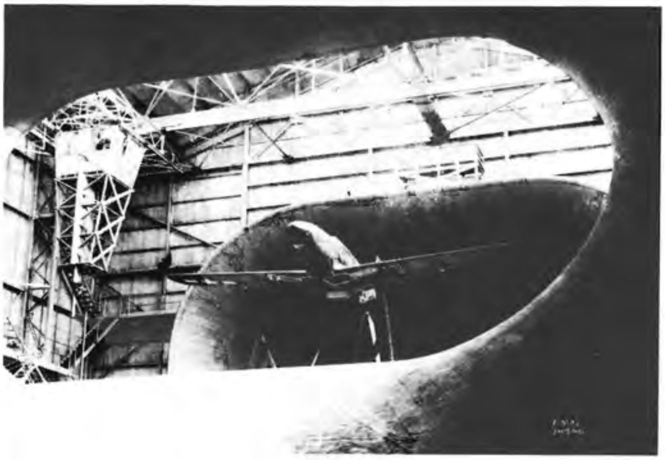
Significance: "The FST is perhaps the best example of a major NACA facility that found a multitude of additional uses not visualized in the beginning. In 1962, for example, it had an extended study of the handling problems of hypersonic aircraft and space reentry vehicles like the shuttle, using large free-flying models." John V. Becker to author.

Disposition: Operational

Reference: TR 459



Construction of the Full-Scale Tunnel, 1930.



The P-51 Mustang is tested in the Full-Scale Tunnel, 1943.

In 1950 Langley tested the drag characteristics of what was then the world's fastest submarine, the Albacore, *in the FST.*

NACA Tank (No. 1)

Purpose: To study the hydrodynamic resistance and other performance features of water-based aircraft.
Initial cost: $649,000
Dimensions: 2060' × 28' (avg. width) × 26' (avg. height)
Special features: Wave suppressors
Key member of design team: Starr Truscott
Authorized: March 1929
Operational: 27 May 1931
Major modifications: New higher-speed (80-MPH) carriage installed, 1936–1937. Tank extended by 900 feet to 2960 feet in 1936.
Significance: "The original research program at Langley made no provision for airplane hydrodynamics, and during its first decade the efforts of the staff were concentrated almost entirely on problems of the landplane. Many of the studies in wind tunnels were applicable to seaplanes, and they in common with landplanes benefited from improvements in wings, propellers, engine cowlings, and other developments of the 1920s. But it was recognized that the airplane on the water has problems that are not shared by the airplane in the air or on the landing strip, and in 1929 the Committee in Washington decided to enlarge the organization and equipment at Langley to provide for research in hydrodynamics." George W. Gray, *Frontiers of Flight* (New York, 1948), p. 65.
Disposition: Turned over to U.S. Navy in 1959.
References: TR 470, TN 513, TM 918

Aircraft Engine Research Laboratory

Purpose: To study methods (like supercharging) by which to increase the power and efficiency of aircraft engines and to improve ignition, cooling, and fuel economy.
Initial cost: $352,000
Description: Building with three dynamometer rooms, two fuel-spray research rooms, and a two-stroke-cycle test bed.
Key members of design team: Carlton Kemper, Addison Rothrock, Oscar W. Schey
Authorized: August 1933
Operational: September 1934
Significance: "The small but expert power plant staff made some important contributions, in addition to their cooperation with the wind-tunnel people in developing the remarkable NACA cowling for air-cooled engines. One recalls improved finning for air-cooled engine cylinders, methods to decrease the octane requirements of high-compression engines, and work on such fundamental matters as the behavior of fuels—how they ignite, how they burn, and how this burning corrodes critical parts of the engine." Jerome C. Hunsaker, *Forty Years of Aeronautical Research,* p. 264.
Disposition: Converted to office space after engine research moved to AERL in Cleveland in 1942.
References: TN 634; TRs 634, 644

The seaplane towing channel under construction in 1930 (top and middle) and during a test of a flying boat model in 1945 (bottom).

24-Inch High-Speed Tunnel.

24-Inch High-Speed Tunnel (24-Inch HST)

Purpose: To investigate phenomena occurring as air near Mach 1 flowed over airfoils and fuselages at twice the Reynolds numbers of the 11-Inch HST.

Initial cost: $12,600

Circuit and pressure: Nonreturn, atmospheric

Test section: 24" diameter, closed throat

Drive system: Blowdown of compressed air from the VDT through annular injection nozzle

Maximum speed: Mach 1 (with no model in throat)

Special features: Langley's first schlieren photographic system to show compressibility burbles and shock waves in air at high speeds; an improved manometer.

Key members of design team: Eastman N. Jacobs, John Stack, Ira H. Abbott, W. F. Lindsey, Kenneth Ward

Authorized: ca. 1933

Operational: 3 October 1934

Major modification: Enclosure in August 1949 to reduce problem of water-vapor condensation.

Significance: "The complex phenomena of the compressibility burble were seen for the first time with the new schlieren system and correlated with the pressure distributions for various wing sections. This new understanding led quickly to the development of improved high-speed airfoils." John V. Becker to author.

Disposition: Replaced by 20-Inch Transonic Tunnel in 1953.

References: TR 646, ACR L4LO7A

A technician mounts a model on a balance for testing in the Free-Spinning Tunnel, August 1935.

15-Foot Spin Tunnel
(Free-Spinning Tunnel)

Purpose: To conduct research on the general problems of spinning and stability and to test models of aircraft for which the spinning and stability characteristics were either unknown or known to be unsatisfactory.

Initial cost: $64,000

Circuit and pressure: Vertical nonreturn, atmospheric

Test section: 15' diameter, open throat; 12-sided polygon, closed throat

Drive system: Fan; 150-HP electric motor

Maximum speed: 40 MPH, variable to rate of falling aircraft model

Special features: Clockwork contained within the model automatically set the controls for recovery from a spin. Motion picture camera recorded the effects.

Key member of design team: Charles H. Zimmerman

Authorized: 8 June 1933

Operational: 3 April 1935

Significance: "During World War II, every fighter, light bomber, attack plane, and trainer—over 300 designs—had to be tested in Langley's spin tunnels. Subsequently, over half of these aircraft were modified in some way to ensure that their controls would be able to pull them out of a spin." Baals and Corliss, *Wind Tunnels of NASA*, p. 42.

References: TR 557; *Aero Digest* (June 1935): 20–22.

8-Foot High-Speed Tunnel (8-Foot HST)

Purpose: To test complete models of aircraft and aircraft components in a high-speed airstream approaching the speed of sound. Visualized as a full-speed companion to the low-speed Full-Scale Tunnel.

Initial cost: $266,000

Circuit and pressure: Single-return, atmospheric

Test section: 8' diameter, closed throat

Drive system: Fan; 8000-HP electric motor

Maximum speed: 575 MPH (Mach 0.75)

Special features: Because of the Bernoulli effect, the test chamber had to withstand powerful, inwardly directed pressure. An igloo structure with concrete walls a foot thick housed the test section. Operating personnel located inside the igloo were subjected to pressures equivalent to 10,000-foot altitude and had to wear oxygen masks and enter through airlocks. A heat exchanger removed the large quantities of heat generated by the big fan.

Key members of design team: Russell G. Robinson and Manley J. Hood; idea for tunnel first suggested by Eastman N. Jacobs.

Authorized: 17 July 1933; approval of funds by Federal Administrator of the Public Works Administration, under the authority of the National Industrial Recovery Act of 16 June 1933.

Operational: 28 March 1936

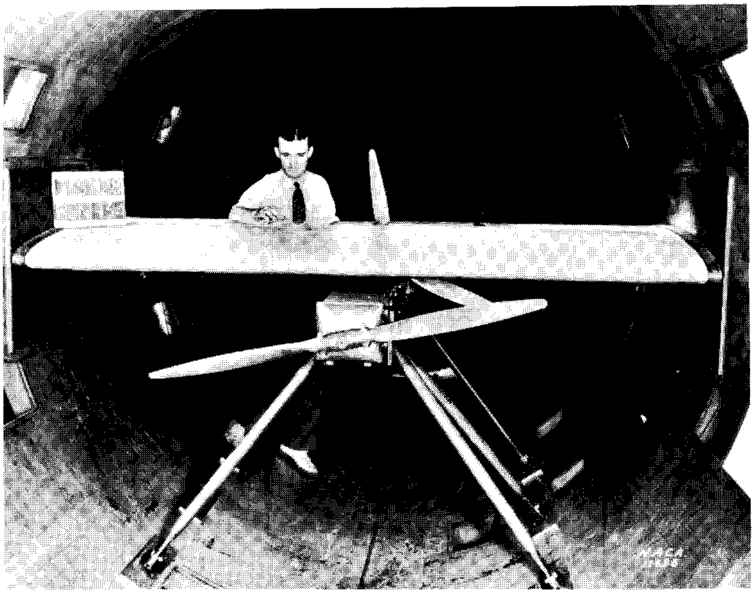
Major modifications: Repowered to 16,000 HP (Mach 1 capability), Feb. 1945. Mach 1.2 contoured nozzle installed, Dec. 1947. Slotted-throat test section installed, 1950.

Significance: "Perhaps the most important contribution of this trailblazing tunnel came from its investigations of complete aircraft models (made possible for the first time by its large size) in which the causes and cures for the severe adverse stability and control problems encountered in high-speed dives were first delineated. This tunnel also produced the high-speed cowling shapes used in World War II aircraft, and the new family of efficient air inlets used in jet aircraft. The first 500-MPH investigations of propellers were made here early in the war. After repowering, with new support systems, the 8-Foot Tunnel produced precise transonic data up to Mach numbers as high as 0.92 for such aircraft as the X-1, D-558, and others. Its final achievement was the development and use in routine operations of the first transonic slotted throat. The investigations of wing-body shapes in this tunnel led to Whitcomb's discovery of the transonic area rule. Surely, this is one of the most impressive records in the NACA or anywhere else of what can be accomplished in the hands of imaginative, competent researchers." John V. Becker to author, 18 Oct. 1984.

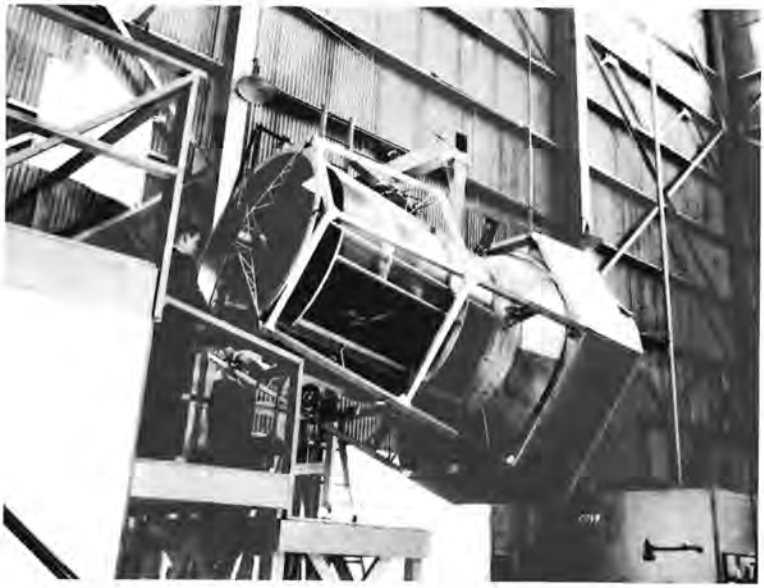
Disposition: Deactivated in 1956.

Reference: *AR* 1936

The concrete walls of the igloo-like structure around the test section of the 8-Foot High-Speed Tunnel were one foot thick.



The windmill power of an experimental propeller is tested in the 8-Foot HST in May 1939.



In the tiltable 5-Foot Free-Flight Tunnel, unpowered models flew stationary in a rising airstream, much like a hawk or buzzard hovers in natural air currents. The man piloting the model in this May 1937 photo is Charles H. Zimmerman, the tunnel's designer.

5-Foot Free-Flight Tunnel

Purpose: To study spinning and stability characteristics in free flight without the expense of building, testing, and modifying full-scale aircraft.

Initial cost: $120,000

Circuit and pressure: Nonreturn, atmospheric

Test section: 5' diameter

Drive system: Fan; 5-HP electric motor

Maximum speed: 25 feet per second

Special features: With one hand an engineer adjusted the speed of the tunnel and with the other hand he tilted the tunnel up, down, or around to follow the motion of the model. The basic idea was to get the model to remain stationary and horizontal in the rising airstream of the tilted tunnel, constantly adjusted to match the aircraft's glide angle. The engineer piloted the model's control surfaces via electrical signals sent through thin wires trailing behind the model.

Authorized: 1936

Operational: 20 April 1937

Significance: "Wind tunnel research has developed problems which required entirely new instrumental applications. One such problem originating at Langley came from the group which [operated] the free-flight tunnel. In this tunnel, instead of installing the airplane model on balances, the engineers actually fly it in the windstream while records are made of the effectiveness of the controls .... In early studies with [this tunnel] the model all too often would swerve to one side and crash into the tunnel wall. The Instrument Research Division was asked to correct this erratic performance ... and shortly produced an automatic control device which responded to a light placed at the end of the tunnel. By this means the model is caused to seek the light, and through its use the smash-ups have been reduced to a low figure." George W. Gray, *Frontiers of Flight*, pp. 55-56.

Disposition: Replaced by 12-Foot Free-Flight Tunnel in 1939.

Two-Dimensional Low-Turbulence Tunnel

Purpose: Ostensibly (that is, as stated for funding purposes) to study ice formation on airplane models and parts, but actually to assess the performance of airfoils in an airstream of very low turbulence level, approaching that of the smooth air of free flight.

Initial cost: $103,000

Circuit and pressure: Single-return, 1–10 atmospheres

Test section: 3' × 7.5', closed throat. Contraction ratio 19.6 to 1. Screening to reduce turbulence.

Drive system: Fan; 200-HP electric motor

Maximum speed: 155 MPH

Special features: To fulfill the announced purpose, icing research, Langley insulated the walls of this tunnel with a thick wrapping of crude insulation (kapok from life preservers) and added makeshift refrigeration equipment (consisting of an open tank of ethylene glycol cooled by blocks of ice). A pump circulated the cold mixture through coils that cooled air drawn from the tunnel.

Key members of design team: Eastman N. Jacobs and Ira H. Abbott

Authorized: 28 May 1937

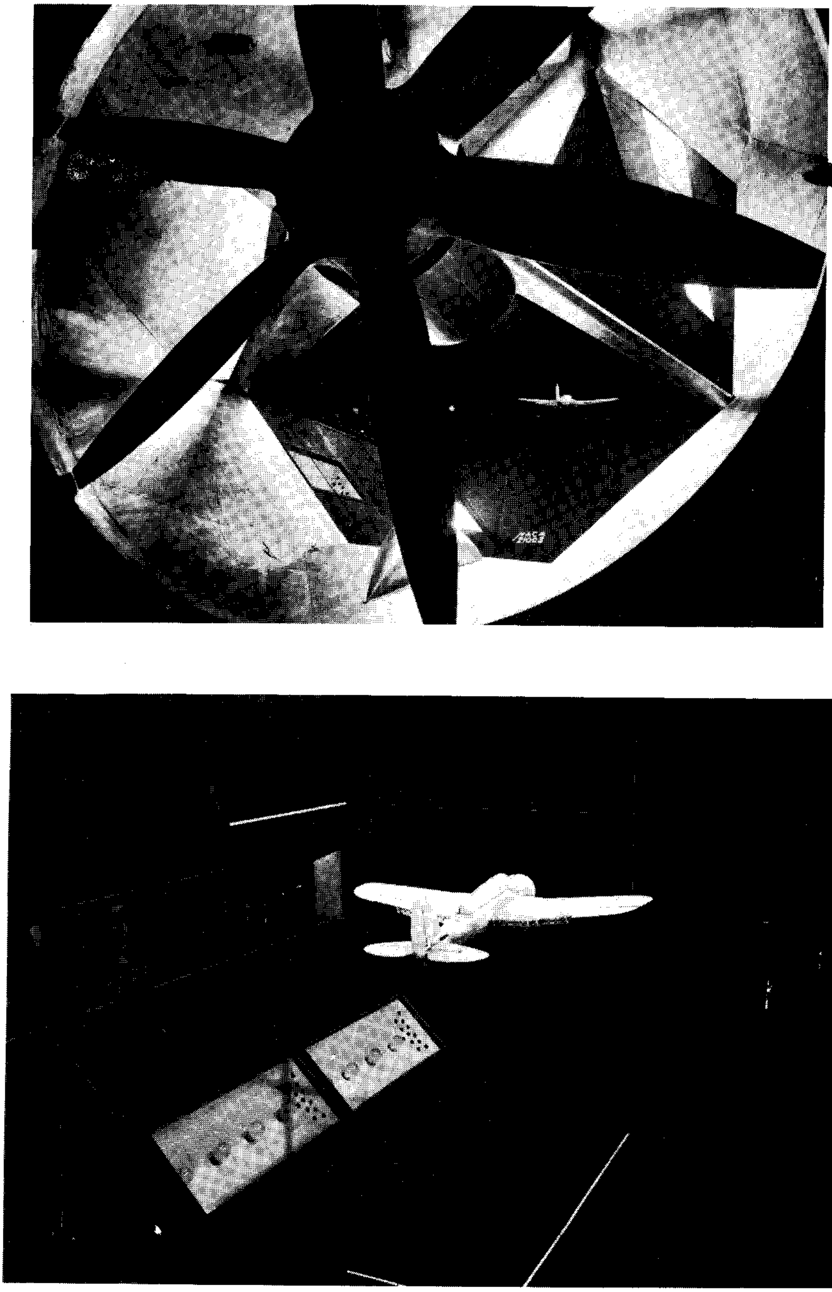
Operational: 15 June 1938

Major modifications: After the completion of a perfunctory series of icing tests, the refrigeration equipment was removed and an array of honeycombs and screens was installed upstream of the test section to homogenize the airflow. Testing of the low-drag potential of various airfoils then began.

Significance: "[This tunnel] was built as an experimental model to try out the idea of radical contraction and screening, to see if the combination would really lower the turbulence. It did, and the researchers began to plan a larger and still more radical tunnel [the Low-Turbulence Pressure Tunnel]." George W. Gray, *Frontiers of Flight*, p. 48.

Disposition: Dismantled, 1947–1948

Reference: TN 1283



Testing a 1/12th-scale model in the 12-Foot Free-Flight Tunnel, 1940.

12-Foot Free-Flight Tunnel

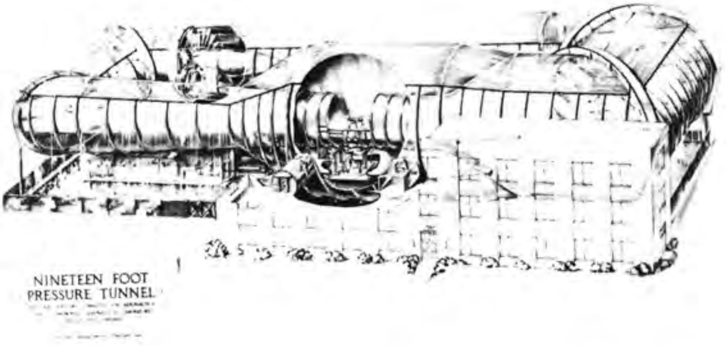
Purpose: To study spinning and stability characteristics of aircraft models in free flight, improve airplane safety, and test radically new aircraft design types.
Initial cost: approx. $250,000
Circuit and pressure: Annular return, atmospheric
Test section: 12' diameter, 12-[illegible] polygon
Drive system: Fan; 600-HP electric motor (5-minute rating)
Maximum speed: 50 MPH
Special features: As the model rose from the tunnel floor, climbed, dived, and banked, a camera recorded a motion picture of its responses to remote controls.
Key member of design team: Charles H. Zimmerman
Authorized: 1937
Operational: 1939
Significance: "[This tunnel was] useful with radically new aircraft where no reservoir of flight experience was available, namely, tailless aircraft, planes with delta and skewed wings, and vertical takeoff and landing/short takeoff and landing (VTOL/STOL) vehicles." Baals and Corliss, *Wind Tunnels of NASA*, p. 28.
Disposition: Used into the early 1950s; supplanted by powered models flown in the Full-Scale Tunnel.
Reference: TN 810

19-Foot Pressure Tunnel

Purpose: The high Reynolds number study of propellers and three-dimensional wings, as well as the stability and control characteristics of models of complete aircraft. Built in response to continued concern over the problem of scale effects.
Initial cost: $1,100,000
Circuit and pressure: Single-return, 2.7 atmospheres (0–40 psia)
Test section: 19', closed throat
Drive system: 34'6" fan; 8000-HP electric motor
Maximum speed: 330 MPH
Special features: Researchers had to enter their working quarters through a decompression chamber.
Key members of design team: Smith J. DeFrance, John F. Parsons, Arthur A. Regier, James G. McHugh, John C. Messick
Authorized: 22 June 1936
Operational: 20 June 1939
Major modifications: Converted to Transonic Dynamics Tunnel, 1955–1959, to study aeroelasticity, flutter, buffeting, vortex shedding, gust loads, and other dynamic characteristics. The TDT incorporated a slotted test section, new mounts, a quick-stop drive system, a gust-maker or airflow oscillator, and a Freon-12 test medium system.
Significance: "[The 19-Foot Pressure Tunnel] was the first attempt anywhere to combine large size and high pressure in a single facility." Baals and Corliss, *Wind Tunnels of NASA*, p. 29. "This tunnel was originally visualized as a 'super PRT,' but it became apparent before it was finished that its speed was too low for high-speed propeller research; this part of its intended usage was transferred to the new Langley 16-Foot High-Speed (500-MPH) Tunnel. The 19-Foot Pressure Tunnel conducted a great

Construction of the 19-Foot Pressure Tunnel, September 1937.

The original test chamber of the 19-Foot Pressure Tunnel, 1939.



Phantom drawing of the 19-Foot Pressure Tunnel, February 1945.

Aerial view of the Transonic Dynamics Tunnel (TDT), a greatly modified version of the original 19-Foot Pressure Tunnel. Between the TDT and the Back River stands the Full-Scale Tunnel.

deal of useful testing of complete aircraft models at higher Reynolds numbers than other low-speed Langley tunnels, but did not produce any landmark results like the original PRT." John V. Becker to author.

Disposition: Operational as Transonic Dynamics Tunnel.

Reference: NASA TN D-1616, March 1963.

Structures Research Laboratory

Purpose: To provide an experimental capability for investigating the problems encountered in the design of advanced aircraft structures.

Initial cost: $803,000

Special equipment: NACA Combined Loading (six components) Testing Machine; many tension and compression testing machines; optical and electrical strain measuring equipment; large, strong testing floor and vertical backstop; carbon-rod and quartz-tube radiators for simulating aerodynamic heating.

Key members of design team: Joseph N. Kotanchik and Norris F. Dow

Authorized: 23 August 1939

Operational: 18 October 1940

Major modifications: Addition of power supply and control system for radiant heating of structures, 1958.

Significance: "This general-purpose structures research laboratory was designed to be adaptable to a variety of testing requirements. Initial experimental and theoretical research concentrated on the strength of structures in compression and the stress distribution in redundant structures. Fatigue testing began in 1943, long before metal fatigue became a major factor in airplane design. A major transition in research began about 1950 to the structural problems of supersonic aircraft and missiles. This laboratory led the development of radiant heating devices used worldwide for the laboratory simulation of aerodynamic heating of structures. Support of ballistic missile programs led to the early development and research use of electric-arc-powered jets for testing thermal protection materials for reentry vehicles." Richard R. Heldenfels to author.

Disposition: Operational

References: R. W. Peters, "The NACA Combined Loading Testing Machine," *Proceedings of the Society for Experimental Stress Analysis*, 13, 1955; Richard R. Heldenfels, "High Temperature Testing of Aircraft Structures," *NATO AGARD Report 205*, Oct. 1958; NASA TM X-1129, July 1965.

20-Foot Spin Tunnel

Purpose: To investigate spinning characteristics of aircraft, especially those with high wing loading factors.

Initial cost: $100,000

Circuit and pressure: Vertical with annular return, atmospheric

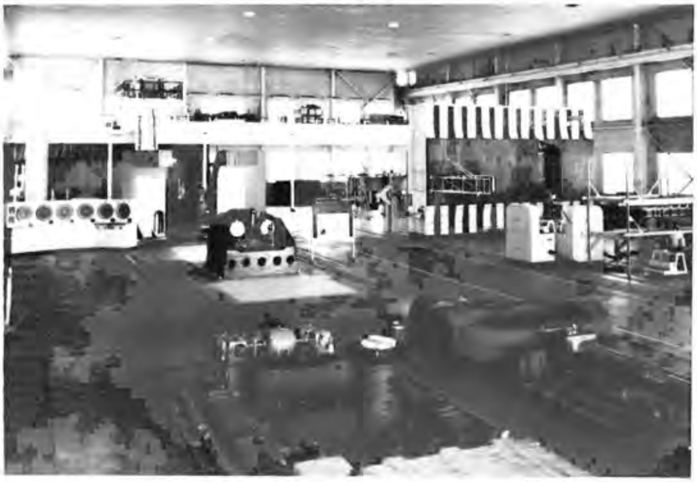
Test section: 20' diameter 12-sided polygon, closed throat

Drive system: Fan; 400-HP electric motor (1300-HP overload)

Maximum speed: 66 MPH

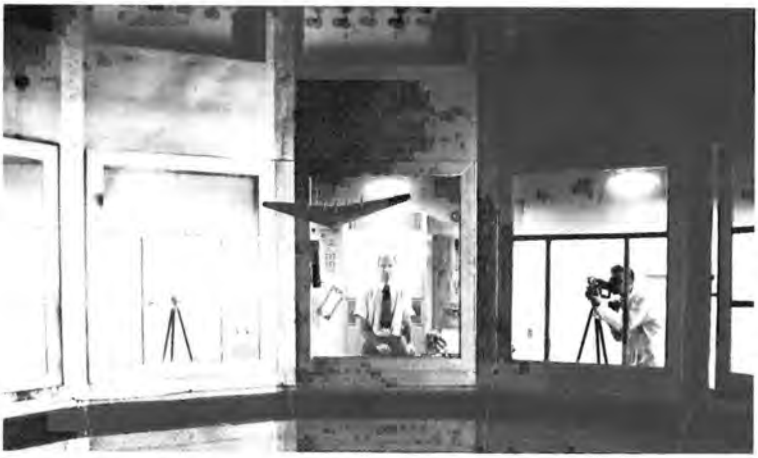
Special features: Tiny electric servo-actuators drive the model's control surfaces, activated electromagnetically to initiate recovery from a spin.

Key member of design team: Oscar Seidman



Interior of the Structures Research Laboratory, 1947.

Authorized: March 1939
Operational: 5 March 1941
Major modifications: Minor changes to accommodate study of capsules and recovery devices in vertical descent, late 1950s.
Significance: "Out of [investigations in this and Langley's other spin tunnels came] three methods of modifying an airplane to make it controllable in a spin; first, enlarge the vertical tail by extending it back, thus providing more surface to act against the air; second, lift the horizontal tail and set it at a higher level on the body of the airplane; third, extend the fin forward on the underside of the tail, that is, put in a ventral fin." George W. Gray, *Frontiers of Flight*, p. 156.
Disposition: Operational
Reference: NACA L-86258



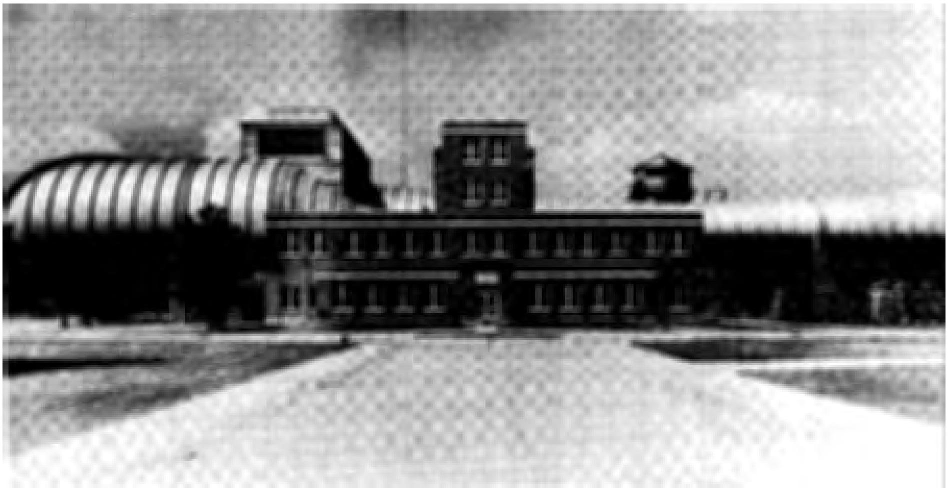
A model of the Northrop XB-35 flying wing was tested in the 20-Foot Spin Tunnel in 1943.

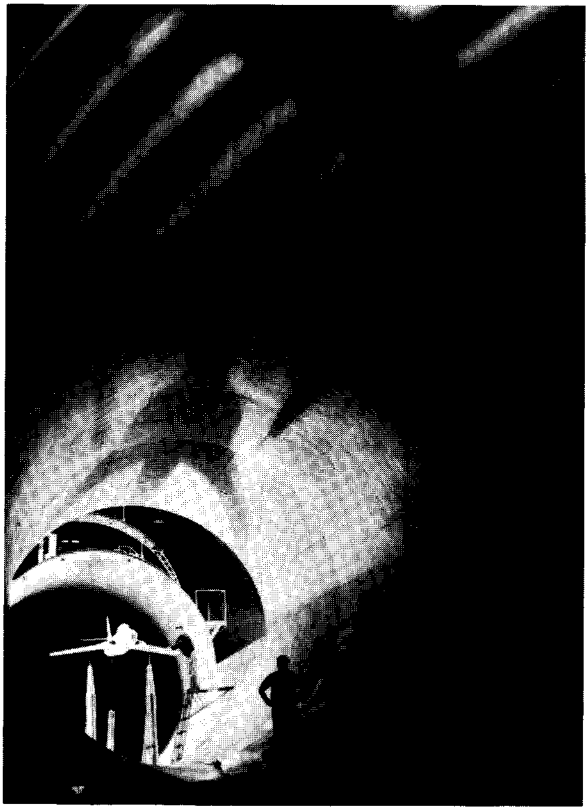
Two-Dimensional Low-Turbulence Pressure Tunnel (LTPT)

Purpose: To provide reliable airfoil data at high Reynolds numbers and, more specifically, to develop low-drag airfoils.
Initial cost: $611,000
Circuit and pressure: Single-return, 1–10 atmospheres (1–150 psia)
Test section: 7'6" × 3', closed throat
Drive system: Fan; 2000-HP electric motor
Maximum speed: 300 MPH (1 atm.), 220 MPH (4 atm.), 160 MPH (10 atm.)
Special features: Eleven screening elements to reduce turbulence levels; high-contraction-ratio entrance cone; unusual method of measuring lift and drag.
Key members of design team: Eastman N. Jacobs, Ira H. Abbott, Albert E. von Doenhoff
Authorized: 1938
Operational: Spring 1941
Major modifications: Converted for use with Freon, 1947–1948; converted to slotted throat in 1953. (Neither of these modifications was very successful, however.)
Significance: "When the LTPT commenced operation in the spring of 1941, it began war work on a crash basis. With its unique low-turbulence-flow characteristics, it was an ideal tool with which to explore the capabilities of a revolutionary type of wing—the newly conceived laminar-flow airfoil." Baals and Corliss, *Wind Tunnels of NASA*, p. 40.
Disposition: Served on a standby basis as a pressure vessel for the 26-Inch Transonic Blowdown Tunnel after 1955; reactivated in early 1970s because of interest in low-speed characteristics of new types of supercritical airfoil.
Reference: TN 1283

16-Foot High-Speed Tunnel (16-Foot HST)

Purpose: To investigate various aerodynamic problems of airplanes, including cowling and cooling of full-size engines and propellers, at high speeds.
Initial cost: $1,422,000
Circuit and pressure: Single-return, atmospheric
Test section: 16' diameter, closed throat
Drive system: Fan; 16,000-HP electric motor
Maximum speed: Mach 0.7
Special features: 2000-HP and 6000-HP dynamometers for full-scale propeller testing
Key members of design team: David J. Biermann and Lindsey I. Turner, Jr.
Authorized: 1939
Operational: 5 December 1941
Major modifications: Changed to 14-foot slotted-throat test section with octagonal throat and repowered to 60,000-HP drive, 1950; 35,000-HP plenum suction blower, 1969.
Significance: "Tests of full-scale aircraft nacelles with operating engines and propellers during the first two years of operation encountered many difficulties: choking of the tunnel at Mach numbers of about 0.6, with the associated distortions in the flow; inadequate angle-of-attack ranges, etc. This type of testing was terminated in 1943. But this tunnel proved ideal for investigating full-scale propellers with the high-powered electric dynamometers at airspeeds up to low supersonic speeds. With its relatively large-size test models it has been possible in more recent years to





The 16-Foot High-Speed Tunnel at the time of its dedication in 1941. Left, a scale model of a jet airplane being tested in the tunnel during the 1950s.

simulate effectively jet engine exhaust on aircraft models using hydrogen peroxide or compressed air." John V. Becker to author.
Disposition: Operational as 16-Foot Transonic Tunnel.
Reference: RM L52E01, July 1952.

Stability Tunnel

Purpose: To study the individual aerodynamic components of rotational motion in flight.
Initial cost: $295,000
Circuit and pressure: Single-return, atmospheric
Test section: Dual (interchangeable): 75" diameter or 6' × 6'; closed throats
Drive system: Fan; 600-HP electric motor
Maximum speed: 250 MPH
Special features: Rotating paddles started air swirling in test section; sides of 6 × 6-foot section were adjustable to different radii of curvature so that models could be tested in curved flow.
Operational: June 1942
Significance: "For many years the stability tunnel provided data for predicting the maneuvering performance of aircraft and missiles. Its eventual demise was hastened by the perfection of oscillating model techniques." Baals and Corliss, *Wind Tunnels of NASA*, p. 44.
Disposition: Deactivated and transferred to Virginia Polytechnic Institute in 1958 for use as an educational tool.
Reference: TN 2483

Tank No. 2

Purpose: To test models of floats for seaplanes and hulls for flying boats by dragging them through seawater.
Initial cost: $429,000
Description: Basin 1800' long, 18' wide, 6' deep; 60-MPH carriage
Design team: Starr Truscott, John B. Parkinson, John R. Dawson
Authorized: 2 May 1939
Operational: 18 December 1942
Significance: "[In this tank, researchers] experimented with radical departures from accepted hull design, trying to find the specifications for a seaplane body that would combine freedom from porpoising and skipping, low water resistance, and superior performance in the air. Out of these experiments [came] a novel design known as the hull with a planing tail." George W. Gray, *Frontiers of Flight*, p. 80.
Disposition: Deactivated in 1960s; carriage used in wake-vortex studies in 1970s.
References: TR 753; WRs L-584, L-687

Seaplane Impact Basin

Purpose: To measure impact loads on seaplane hulls.
Initial cost: $311,000
Description: Basin 360' long, 26'8" wide, 8' deep; made waves up to 3'.
Authorized: 2 May 1939

Operational: 10 November 1942

Significance: This facility led to the development of various aircraft ditching aids, such as hydroflaps and hydrofoils.

Disposition: Building used from 1950s as LaRC photographic laboratory.

Reference: TR 795

9-Inch Supersonic Tunnel

Purpose: To explore supersonic flight problems and fundamental supersonic flow phenomena.

Initial cost: Indeterminate, but probably less than $100,000.

Circuit and pressure: Atmospheric intake, nonreturn; stream pressures from 1/5 to 1/3 atmosphere

Test section: 9" × 9"

Drive system: Axial-flow compressor; variable-speed 1000-HP electric motor

Maximum speed: Mach 2.5

Key members of design team: Eastman N. Jacobs, Macon C. Ellis, Clinton E. Brown, Arthur Kantrowitz

Authorized: 1939

Operational: July 1942

Major modifications: Changed to closed-circuit dry air operation in mid-1940s; repowered to 3500-HP drive ca. 1946.

Significance: "This pioneering little supersonic tunnel provided timely education and experience for the NACA in the 1941–1945 period immediately preceding the major drive for large supersonic tunnels." John V. Becker to author.

Disposition: Dismantled

300-MPH 7 × 10-Foot Tunnel

Purpose: To reduce the backlog of work in Langley's 7 × 10-Foot Atmospheric Wind Tunnel (operational 1930) caused by World War II.

Circuit and pressure: Single-return, atmospheric

Test section: 7' × 10', closed throat

Drive system: Fan; 1600-HP electric motor

Maximum speed: 300 MPH

Special features: Remote-control survey apparatus permitting rapid exploration of airflow behind models; six-component balance system.

Key members of design team: Thomas A. Harris and Charles J. Donlan

Authorized: 1942

Operational: February 1945

Major modifications: 17' × 17' test section installed in settling chamber upstream of original test section, 1956.

Significance: The modified tunnel provided conditions appropriate for testing of aircraft in transition from hovering to cruising flight.

Disposition: Dismantled 1970

The V/STOL (Vertical/Short Take Off and Landing) Tunnel, shown here in 1949, was a modified version of the 300-MPH 7 × 10-Foot Tunnel of 1945.

Gust Tunnel

Purpose: To investigate aircraft loads produced by atmospheric turbulence and other unsteady flow phenomena and to develop gust alleviation devices.

Initial cost: Indeterminate

Circuit and pressure: Reversible single-return, atmospheric

Test section: 8' × 14', open throat, adjustable angle (jet type)

Drive system: 75-HP electric motor

Maximum speed: 100 MPH

Special features: Apparatus consisted of a catapult for launching dynamically scaled models into steady flight, a jet of air for simulating a gust, curtains for catching the model after it traversed the gust, and instruments for recording the model's responses.

Key members of design team: Philip Donely and Mike Goldberg

Authorized: June 1943

Operational: August 1945

Significance: "Often the gust [revealed] values that were not found by the best known methods of calculation. In one instance, for example, the gust tunnel tests showed that it would be safe to design the airplane for load increments 17 to 22 percent less than the previously accepted values." George W. Gray, *Frontiers of Flight*, p. 174. "The gust tunnel was one of a breed of facilities (like the spin tunnel, free-flight tunnel, and impact basin) that provided information to verify basic theories and concepts. The gust tunnel became obsolescent because its Reynolds number and Mach number capabilities were low." Philip Donely to author.

Disposition: Dismantled in 1965 after being used as a low-velocity instrument laboratory and noise research facility.

A speed-flash photograph of a scale model of a seaplane flying in the Gust Tunnel, April 1946.

Flutter Tunnel

Purpose: To study the serious and poorly understood problem of aeroelasticity and the effects of flutter on aircraft.
Circuit and pressure: Single-return, 0 to 1.8 atmospheres (air or Freon-12 as test medium)
Test section: 4'6" diameter, closed throat. Dual (interchangeable): four-component hydraulic balance section; flutter test section with 17 viewing portals.
Drive system: 1000-HP electric motor
Maximum speed: Mach 1
Authorized: 1944
Operational: September 1945
Significance: "Transonic aerodynamics further complicated an already complex aeroelastic problem. ... Aircraft designers needed definitive wind-tunnel tests to assure them that their thin-winged aircraft would not experience flutter under any anticipated flight conditions." Baals and Corliss, *Wind Tunnels of NASA*, p. 79
Disposition: Operational

High-Speed 7 × 10-Foot Tunnel

Purpose: To investigate general aerodynamic effects at high speed, especially stability and control problems into and through the critical speed range.
Initial cost: $2,052,000
Circuit and pressure: Single-return, atmospheric
Test section: 7' × 10', closed throat, adjustable
Drive system: Fan; 14,000-HP electric motor
Maximum speed: 675 MPH (approx. Mach 0.9)
Key members of design team: Thomas A. Harris and Charles J. Donlan
Authorized: 1943

The portable test section of the Flutter Tunnel being inspected in the Physical Research Laboratory. 1945.

Operational: November 1945

Major modifications: Transonic bump installed, ca. 1946; slotted test section, ca. 1953, increased maximum speed to Mach 1; connected to 35,000-HP compressor of 16-Foot HST in mid-1950s, increasing speed to Mach 1.2.

Significance: "Whereas the test area of the 300-MPH tunnel was expanded for low-speed work, the test section of its high-speed twin was constricted by a carefully designed 'bump.' Air flowing over the bump was accelerated to the transonic range even though the main airflow remained subsonic. This modification, though crude, led to a qualitative exploration of the transonic range that was just opening up after [World War II]." Baals and Corliss, *Wind Tunnels of NASA*, p. 37.

Disposition: Operational under direction of Full-Scale Research Division, but no longer has Mach 1 capability.

References: TN 3469, NASA TM X-1130

Induction Aerodynamics Laboratory

Purpose: To conduct research on the aerodynamics of subsonic and supersonic internal flows, such as the optimum methods of inducing air and supplying it to conventional and jet engines.

Special equipment: Air supply provided initially by three 1000-HP blowers.

Key members of design team: Kennedy F. Rubert and J. R. Henry

Authorized: 1944

Operational: March 1946

Major modifications: Underwent major upgrading in 1950s involving use of the replaced 16,000-HP motors from the repowered 16-Foot HST to drive larger blowers.

Significance: "Like the original concept of the NACA Lewis Flight Propulsion Laboratory in Cleveland, this small laboratory was originally aimed at piston engine problems and had to be reoriented towards jet engines and ramjet combustion research. Eventually it was connected to the central air supply of the Gas Dynamics Laboratory at Langley and applied to hypersonic ramjet research." John V. Becker to author.

Disposition: Operational under NASA, in part as a high-intensity noise research facility. Test cells now devoted to hypersonic ramjet propulsion research under High-Speed Aerodynamics Division.

Helicopter Apparatus

Purpose: To investigate fundamental factors affecting the performance, stability and control, and vibration characteristics of helicopters.

Description: Worked on the old principle of the whirling arm. Apparatus consisted of a cone-shaped steel tower 40 feet high with a drive shaft in its center for mounting a helicopter rotor. Strain gauge measured the torque and thrust on the shaft. Cameras recorded action of whirling rotor.

Key member of design team: Frederic B. Gustafson

Authorized: 1944

Operational: March 1946

Disposition: Deactivated in 1960s, but reactivated ca. 1970. Dismantled 1976 when NASA shifted helicopter work to Ames Research Center.

11-Inch Hypersonic Tunnel

Purpose: To explore the potential of flight at high Mach numbers. Inspired by Allies' discoveries concerning Nazi Germany's V-2 ballistic missile program at Peenemünde. Served as pilot model for large Continuous-Flow Hypersonic Tunnel.

Initial cost: approx. $200,000

Circuit and pressure: Nonreturn, 36 atmospheres (540 psia)

Test section: 11" × 11"

Drive system: Blowdown. To reach high pressure ratios, air from a 50-atmosphere pressure tank was blown through the test section into an evacuated tank. With high pressure on one side and very low pressure on the other, generated pressure ratios could be maintained for about 100 seconds.

Maximum speed: Mach 7

Special features: Electric resistance heater raised temperatures in settling chamber to 900°F.

Key members of design team: John V. Becker and Charles H. McLellan

Authorized: 1945

Operational: 1947

Significance: "Small pilot tunnels of the NACA were often used for other purposes, but the 11-Inch Hypersonic Tunnel went far beyond this to become a star in its own right .... Our experience with this remarkable tunnel and other 'blowdown' or 'intermittent' hypersonic tunnels suggested clearly that they are preferable to the continuous-flow type—which is extremely costly in drive compressor equipment. Long runs are not essential with modern instrumentation." John V. Becker to author.

Disposition: Operational under direction of Aero-Physics Division until 1973 when it was dismantled. Later given to Virginia Polytechnic Institute.

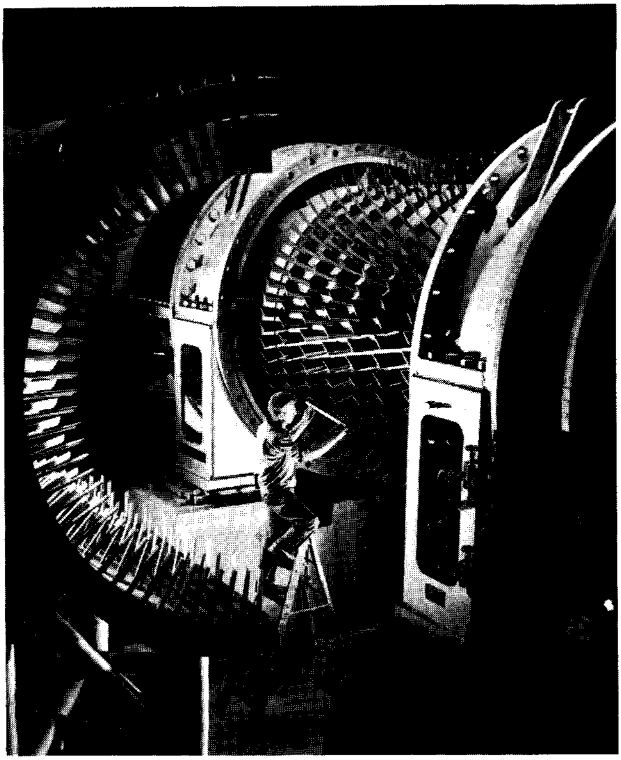
Reference: John V. Becker, "Results of Recent Hypersonic and Unsteady Flow Research at the Langley Aeronautical Laboratory," *Journal of Applied Physics* 21 (July 1950): 619–628.

4 × 4-Foot Supersonic Pressure Tunnel

Purpose: To investigate supersonic aerodynamics problems on models large enough to permit installation of extensive instrumentation, and to provide detailed information on viscous and interference effects unobtainable in smaller supersonic tunnels.
Initial cost: $909,000
Circuit and pressure: Single-return, 1/4 atmosphere
Test section: 4'6" × 4'6"
Drive system: 6000-HP electric motor driving a seven-stage axial compressor capable of handling 860,000 cubic feet of air per minute at a compression ratio of 2. The compressor was the key to the whole design.
Maximum speed: Mach 2.2
Special features: Flexible walls in test section; adjustable contour nozzle; drying and cooling equipment to reduce moisture content of tunnel air.
Key members of design team: Donald D. Baals and Kent Horton. The compressor was designed by M. F. Miller and J. R. Runckel of the 16-Foot HST staff.
Authorized: 1945
Operational: 1948 (construction halted for nearly two years by strike of the industrial contractor assigned the mechanical design and actual fabrication)
Major modifications: Repowered in 1950 to 45,000 HP (continuous) and 60,000 HP (for 30 minutes).
Significance: "Finally on the line, the 4 × 4-foot supersonic tunnel made up for lost time. Many well-known military aircraft and space vehicles were tested through the years: the famous Century Series fighters (F-102, F-105, etc.), the B-58 supersonic bomber, the X-2 research aircraft, and so on." Baals and Corliss, *Wind Tunnels of NASA*, p. 51.
Disposition: Dismantled in 1977, but drive motors, cooling tower, and some support facilities incorporated into new National Transonic Facility (built on the same site and operational in 1983).
Reference: NASA TM X-1130

26-Inch Transonic Blowdown Tunnel (26-Inch TBT)

Purpose: To study aerodynamic effects in the troublesome transonic speed range and to investigate flutter characteristics.
Initial cost: $135,000
Circuit and pressure: Nonreturn, 7 atmospheres
Test section: 26" octagonal, slotted top and bottom walls
Drive system: Induction; compressed air from LTPT (150 psia)
Maximum speed: Mach 1.45
Key members of design team: Albert E. von Doenhoff, and Laurence K. Loftin, Jr.
Authorized: ca. 1948
Operational: 1950



The key to the design of the 4 × 4-Foot Supersonic Tunnel was the powerful multi-stage axial compressor.

Significance: "The validity of flutter data obtained in the 26" TBT was substantiated in 1951 by comparative tests. Studies were made in this tunnel with models, appropriately scaled, similar to those for which flutter data had been obtained beyond Mach 1 by the free-fall drop-model technique. Agreement between results obtained from the two different test techniques was gratifyingly close. Following these comparative investigations, systematic studies of the effect of such wing planform variables as aspect ratio and sweepback angle on flutter in the Mach number range between 0.8 and 1.4 were undertaken in the TBT. The results of these studies began to appear in 1953. Shortly thereafter, the TBT was in great demand for investigations of the flutter characteristics of various aircraft and missile configurations." Laurence K. Loftin, Jr., "Notes on Flutter Investigation of Republic F-105 Tail Surfaces in the NACA 26-Inch Transonic Blowdown Tunnel," 12 January 1983, copy in LaRC Historical Archives.

Disposition: Deactivated in 1970s.

Gas Dynamics Laboratory

Purpose: To research basic aerodynamic, heating, and fluid mechanical problems in the speed ranges upwards from the conventional supersonic tunnels to hypersonic and space-reentry conditions.

Initial cost: $5.5 million

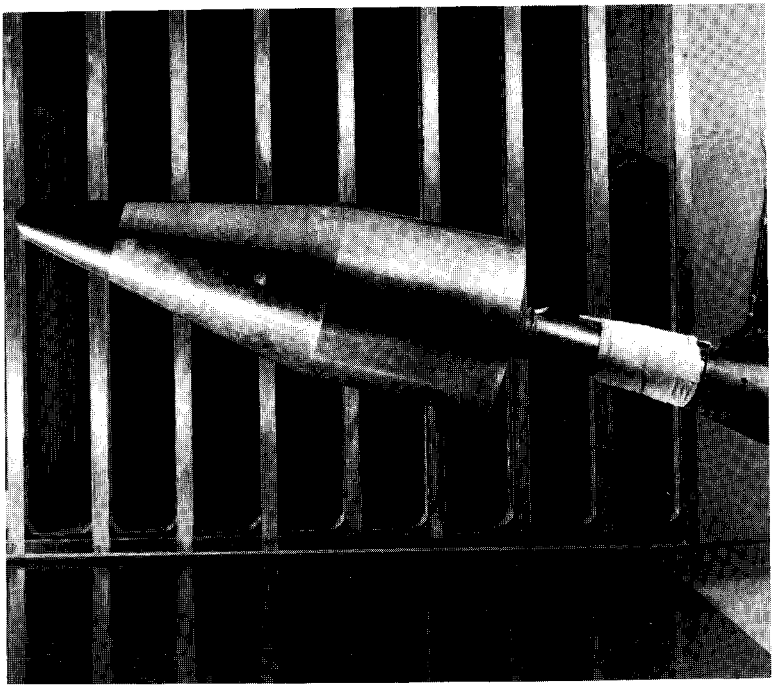
Circuit and pressure: Nonreturn, 200 atmospheres
Test section: Typically 20"
Drive system: Induction. Central 3000-psi tank farm provided heated air to several small cells.
Maximum speed: Mach 8
Special features: Huge steam and electric resistance heaters warm air to 680° and 1040°F, respectively, and prevent liquefaction in the test cells.
Key members of design team: Antonio Ferri, Macon C. Ellis, Clinton E. Brown
Authorized: ca. 1949
Operational: 1951
Major modifications: Under NASA, when models of various spacecraft had to be tested at reentry Mach numbers, pure nitrogen and helium were used as test medium instead of heated air.
Disposition: Operational

8-Foot Transonic Pressure Tunnel (8-Foot TPT)

Purpose: To further the study of transonic aerodynamics at high Reynolds numbers, and in particular to investigate flutter and buffeting in the transonic regime.
Initial cost: $5,495,000
Circuit and pressure: Single-return, 0.1 to 2.0 atmospheres
Test section: 7'1" × 7'1", slotted throat
Drive system: Fan; 25,000-HP electric motor
Maximum speed: Mach 1.2
Special features: Fine grid water-cooled coil in airstream removed excess heat but added no moisture to the circulating air.
Key members of design team: John Stack, Eugene C. Draley, Ray H. Wright, Axel T. Mattson
Authorized: ca. 1951
Operational: 1953
Major modifications: Plenum suction added, 1958, increasing speed to Mach 1.3.
Significance: Langley engineers designed this tunnel from its inception using the new concept of the slotted wall.
Disposition: Operational

Unitary Plan Supersonic Tunnel

Purpose: To contribute force, moment, pressure-distribution, and heat-transfer studies of high-speed airflow.
Initial cost: $15,427,000
Circuit and pressure: Single-return, 150 psia. (Normal operating temperature approx. 150°F with heat bursts of 300–400°F for heat-transfer studies.)
Test section: Dual. Both 4' × 4'; one capable of Mach 1.5 to 2.9 and the other capable of Mach 2.3 to 4.6.
Drive system: Family of electric motors rated at 100,000 HP, plus four compressor units
Maximum speed: Mach 4.6
Special features: Asymmetric supersonic nozzle developed at Ames Research Center
Key member of design team: Herbert A. Wilson
Authorized: 27 October 1949 (Unitary Plan Act)



In 1957 the reentry flight path of this nose cone model of a Jupiter intermediate range ballistic missile (IRBM) was tested in the Unitary Plan Wind Tunnel.

Operational: 1955

Significance: "A long series of missiles passed through the 4 × 4-Foot Unitary Tunnel, where they were tested for high-speed performance, stability and control, maneuverability, jet-exhaust effects, and other performance factors .... Despite the original dedication of this tunnel to missile development, it had been in operation scarcely a year before the now-famous McDonnell F-4 Phantom was being tested in model form. Later, the X-15, the F-111, and various supersonic transport configurations, as well as models of space vehicles, could be found mounted in the test section." Baals and Corliss, *Wind Tunnels of NASA*, pp. 68–69.

Disposition: Operational

Reference: *Manual for Users for the Unitary Plan Wind Tunnel Facilities of the NACA*, 1956.

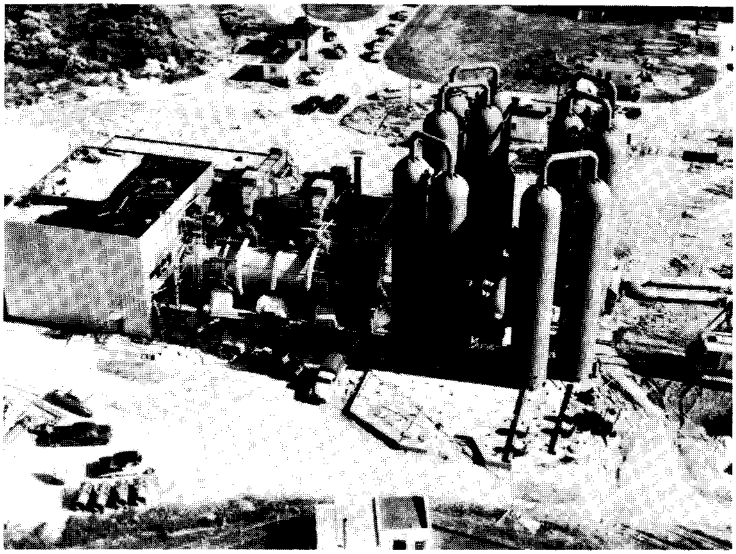
9 × 6-Foot Thermal Structures Tunnel

Purpose: To provide a capability to test aircraft and missile structural components under the combined effects of aerodynamic heating and loading.

Initial cost: $3,723,000

Circuit and pressure: Nonreturn, 3.4 to 13.6 atmospheres (stagnation pressure to 200 psia), 300° to 660°F

Test section: 8'9" × 6', solid walls with numerous viewing ports



In the early 1960s the 9 × 6-Foot Thermal Structures Tunnel (top, in 1956) tested the effects of reentry heating on various space capsule materials as part of Project Fire.

Hot-air jets employing ceramic heat exchangers played an important role at Langley in the study of materials for ballistic missile nose cones and reentry vehicles. Here a model is being tested in one of these jets at 4000 degrees Fahrenheit in 1957.

Drive system: Induction; 600-psia air stored in a tank farm filled by a high-capacity compressor located in an adjacent facility; exhausted to the atmosphere.

Maximum speed: Mach 3

Special features: 9 × 6-Inch Model Tunnel (1960)

Key members of design team: Richard R. Heldenfels and E. Barton Geer

Authorized: FY 1953. (In response to recommendations of its advisory committees, NACA management had decided in 1951 that a large, high-temperature structures research laboratory should be constructed at Langley to conduct experiments on structures for supersonic aircraft and missiles. A heated test chamber equipped with loading devices was proposed in June 1951, but further study and some spectacular test results in 1952 revealed that a ground facility that more nearly duplicated the flight environment was needed.)

Operational: September 1957. (Construction was delayed by a federal budget reduction action and by studies required to solve a few design problems.)

Major modifications: Additional air storage, 1957; high-speed digital data system, 1959; subsonic diffuser, 1960; Topping compressor, 1961; boost heater system (2000°F hot core provided by propane burners in the settling chamber), 1963.

Significance: "Research use of this facility was primarily concerned with the effects of aerodynamic heating and loading in combination on the structural integrity of vehicle components with emphasis on panel flutter. The many specific components tested included the vertical tail of the X-15 research airplane, the heat shields of the Centaur launch vehicle and the Project Fire entry vehicle, and elements of the Hawk, Falcon, Nike, Sam-D, and Minuteman missiles. The high-intensity noise field (162 db) at the tunnel exit was used occasionally to test the response of humans, equipment, and structures that included the Project Mercury capsule." Richard R. Heldenfels to author.

Disposition: Deactivated on 30 September 1971 as a result of metal fatigue in the air storage field which caused an accident that destroyed part of the facility and damaged other property. The failure resulted from wind-induced oscillations of a manifold loop between bottles. In the mid-1970s, all tunnel equipment was removed and the buildings converted to other uses.

References: NASA TNs D-517, D-907, D-921, D-1358; NASA TM X-1130

20-Inch Hypersonic Tunnel

Purpose: To investigate heat transfer, pressure, and forces acting on inlets and complete models in the hypersonic regime.

Initial cost: $1,409,000

Circuit and pressure: Nonreturn, 220–550 psia; running time over 15 minutes

Test section: 20" diameter

Drive system: Induction

Maximum speed: Mach 6

Special feature: Electrical resistance heater

Key members of design team: John V. Becker and Eugene S. Love

Authorized: 1957

Operational: 1958

Disposition: Operational under direction of High-Speed Aerodynamics Division.

Reference: NASA TN D-6280

Appendix E

Aircraft

Langley laboratory conducted both wind tunnel and free-flight testing. Sometimes the NACA researchers studied a problem first in flight and then moved on to explore it in a tunnel under a more extreme aerodynamic condition, such as higher speed. Other times the researchers made the initial investigation in a tunnel and then checked the accuracy of the simulation in flight. Such confirmation was essential to aeronautical progress because there were reasons to suspect the accuracy of tunnel data. No one knew, for instance, the exact effect of Reynolds number on test results. Walls, struts, supports, and other tunnel structures affected the aerodynamic performance of a test model in ways that could not be compensated for exactly.

The following is a catalog of the aircraft assigned to NACA Langley in order of receipt. Some of the aircraft were subjected to extensive research probes, others served as test beds for the study of specific innovations, some were given merely cursory flight evaluation, and still others were only visitors at the lab or military base at Langley Field.

NACA Langley's aircraft numbering system may be confusing because it does not conform sequentially to the order of receipt. Some aircraft had multiple NACA designations depending upon the type of arrangement by which the aircraft had been assigned to Langley (e.g., ownership, indefinite loan, temporary loan). An example is the P-80A-5, serial 44-85352, acquired on 5 November 1946, which was listed in early records as "NACA 281" but continued to carry the USAAF designation "PN-352" until transfer of ownership to the NACA on 1 May 1950, when it was designated "NACA 112." Over a period of years, in fact, more than one aircraft could have the same number. (The current NASA numbering system begins with 1 for the transports, but the research and support aircraft carry 3-digit numbers, the first digit of which indicates the center to which the aircraft is assigned. For instance, "NASA 1" is a transport operated by Langley for the NASA administrator in Washington, whereas Langley's other aircraft have numbers beginning with 5, such as "NASA 515," a research Boeing 737.) During the years of the NACA, many military aircraft were temporarily assigned to Langley lab for specific programs requested by the services. Rather than paint NACA numbers on most of these, Langley used the last three digits of the military number for the identification. If the aircraft was held over an appreciable time for research, or transferred in ownership to the NACA, Langley then applied NACA numbers. Aircraft purchased by the NACA carried NACA numbers from the start.

Order of receipt at Langley	Aircraft type and serial no.	Arrived	Departed	NACA no.
1	Curtiss JN4H (N6249)	1919	Nov. 1923	

Top, the original NACA hangars, 1931. The aircraft parked to the right is the Fairchild owned by the NACA. Just outside the hangar door is a modified Ford Model A that was used to start aircraft propellers.

Order of receipt at Langley	Aircraft type and serial no.	Arrived	Departed	NACA no.
2	Curtiss JN4H (AS 44946)	1919	Nov. 1923	
3	Curtiss JN4H (AS 38131)	1919	Apr. 1923	
4	Vought VE-7 (A 5669)	Apr. 1921	July 1929	4
5	British Royal Aircraft Factory (RAF) SE-5A (AS 8049)	Sept. 1922	Sept. 1926	
6	DeHavilland 4 (AS 22830)	Sept. 1922	July 1927	

What interested the NACA most about the famous Fokker D-VII was the German fighter plane's thick, internally braced cantilever wings.

Order of receipt at Langley	Aircraft type and serial no.	Arrived	Departed	NACA no.
7	Fokker D-VII (6328)		1923	7
8	Martin MO-1 (AS 63335)	Sept. 1922	Nov. 1923	
9	DeHavilland 9A (AS 31839)	Sept. 1922	Dec. 1927	
10	SPAD VII (AS 7142)	Sept. 1922	Feb. 25	
No number	Nieuport 23	1922		
No number	Thomas-Morse MB-3	Jan. 1923	1924	
11	Douglas DT-2 (A 6425)	May 1923	June 1925	
12	Curtiss JN6H (AS 44946)	Sept. 1923	Aug. 1924	
13	Consolidated CBS		1929	
14	Vought VE-7 (A 5950)		1929	14
15	Curtiss TS-1 (A 6249)	Nov. 1923	Dec. 1928	
16	Sperry M-1 Messenger (AS 68473)	Jan. 1924	1927	
17	Supermarine Vickers Viking IV (A6073)	1923	1924	
18	Curtiss F4C-1 (6689)			

Order of receipt at Langley	Aircraft type and serial no.	Arrived	Departed	NACA no.
19	Curtiss JNS-1 (AS 24232)	Nov. 1924	June 1926	
No number	Martin MB-2	1924		
20	Sperry M-1 Messenger (AS 64226)	Jan. 1925		
21	Vought UO-1			
22	Boeing PW-9	Jan. 1927		
23	Curtiss F4C-1 (6690)			
24	Douglas M3 Mailplane	1927	1931	
25	DeHavilland 4-B (AS 31839)	Dec. 1927	Jan. 1930	25
26	Fairchild FC-2W2	1928		1
27	Curtiss XF7C-1 Seahawk (A 7653)			
28	Atlantic Fokker C-2A (AC 28-123)			
29	Douglas O-2H (AC 29-168)	Jan. 1929	1935	
30	Curtiss H-16	Feb. 1929	July 1929	
31	Stearman C3B (NS 7550)			
32	Curtiss P-1A Marine Hawk (AC 25-411)		July 1929	
33	Consolidated NY-1			
34	Doyle O-2 Oriole			34
35	Vought O2U-1			
36	Consolidated/Fleet XN2Y-1 (A 8019)	1929		
37	Boeing F3B-1			
38	Boeing F2B-1			
39	Boeing XF4B-1 (A 8128)			
40	Consolidated NY-2			
41	Verville Sportsman			
42	McDonnell Doodlebug			
43	Vought O3U-1			
44	Pitcairn PCA-2 Autogiro	July 1931	Sept. 1933	44
45	Curtiss O2C-1			
46	Loening XSL-1 (BuNo. 8696)			
47	Fairchild C-7A			47
48	Martin XBM-1 (BuNo. 9212)			

Fokker trimotor with experimental NACA cowlings in flight over Langley, 1929.

Curtiss Bleeker helicopter in front of the NACA hangar, July 1930

The NACA's new hangar in the east area, 1932.

Martin XBM-1 equipped with experimental heated wing for icing research, 1939.

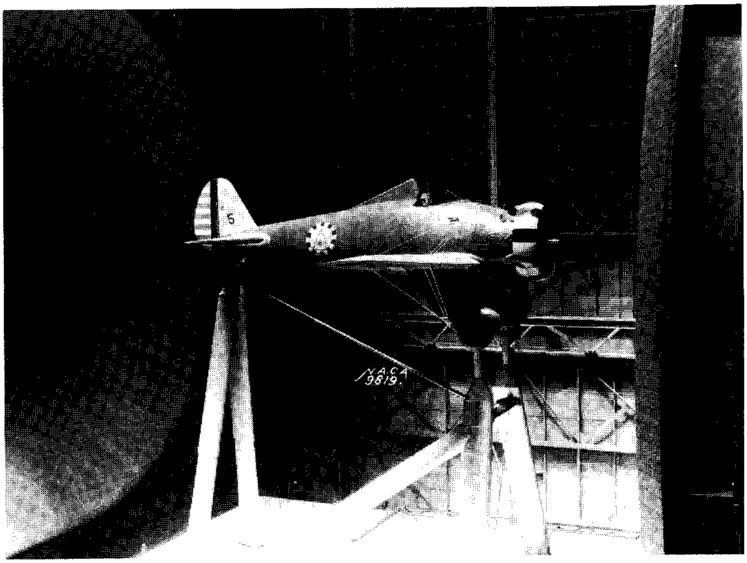
Order of receipt at Langley	Aircraft type and serial no.	Arrived	Departed	NACA no.
49	Douglas YO-31A			
50	Fleet N2Y-1			
51	Detroit/Lockheed XRO-1 (BuNo. 9054)			
52	Curtiss F6C-4			
53	Curtiss O2C-1 (BuNo. 8455)	Aug. 1932	Feb. 1933	

The NACA's Fairchild 22 with an experimental flap and spoiler installation, October 1940.

Order of receipt at Langley	Aircraft type and serial no.	Arrived	Departed	NACA no.
54	Boeing F4B-2 (BuNo. 8628)	July 1932	June 1934	
55	Boeing P-12C			
56	Curtiss P-6E			
57	Fleet N2Y-1	Sept. 1932		
58	Vought O2U-1			
59	Pitcairn PAA-1 Autogiro			
60	Fairchild 22	May 1933		60
61	Fairchild C-7A	June 1936		47
62	Boeing F3B-1			
63	Consolidated NY-2			
64	Vought			
65	Vought XO4U-2 (BuNo. 8641)	Apr. 1933	May 1933	
66	Boeing F4B-1			
67	Boeing P-12C			
68	Curtiss P-6E			
69	Vought XF3U-1 (BuNo. 9222)			
70	Weick W-1A (NS-67)	Feb. 1934	Apr. 1935	
71	Boeing P-26A (AC 33-56)	June 1934		
72	Grumman JF-1	June 1934	Aug. 1934	
73	Northrop XFT-1 (BuNo. 9400)	June 1934	July 1934	
74	Martin T4M-1			
75	Vought SU-2			
76	Boeing F4B-4 (BuNo. 8912)			

Fred Weick's homebuilt W-1A of 1934, one of the first aircraft to employ tricycle landing gear. Weick and a group of nine other Langley engineers built this small experimental airplane in their spare time to study the special needs of the private flyer. The plane was eventually purchased by the Department of Commerce. After leaving the NACA (for a second and final time) in 1936, Weick incorporated many elements of the W-1 into his design of the famous Ercoupe, a small, simple-to-fly airplane built first by the Engineering Research and Development Corporation (ERCO) of suburban Washington, D.C.

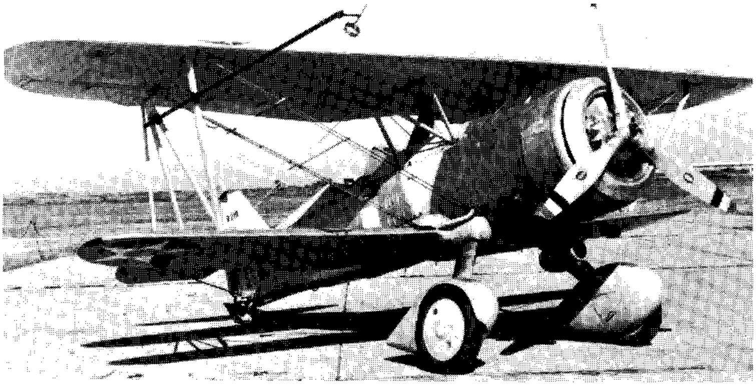
Order of receipt at Langley	Aircraft type and serial no.	Arrived	Departed	NACA no.
77	Boeing XBFB-1 (BuNo. 8975)	Aug. 1934		
78	Curtiss N2C-2	Nov. 1934		
79	Vought O2U-2			
No number	Consolidated XB2Y-1 (BuNo. 9221)	1934		
80	Boeing YP-29A (AC 34-24)	Mar. 1935	July 1940	
81	Kellet KD-1	Apr. 1935	July 1940	
No number	Bellanca Pacemaker	Apr. 1935	Apr. 1935	
82	Fairchild XR2K-1 (BuNo. 9998)	Sept. 1935	1946	82
83	Curtiss BF2C-1 (BuNo. 9586)	Oct. 1935	Dec. 1935	
84	Great Lakes XTBG-1 (BuNo. 9723)	Nov. 1935	Nov. 1935	
85	Grumman XSF-2 Scout (BuNo. 9493)	Dec. 1935	Jan. 1936	



Boeing P-26A fighter mounted in the Full-Scale Tunnel, 1934. This aircraft, known as the Peashooter, was the first army fighter to be constructed entirely of metal and to employ the low-wing monoplane configuration. The wings, though, were externally braced, and the landing gear was fixed.

Order of receipt at Langley	Aircraft type and serial no.	Arrived	Departed	NACA no.
86	Taylor E-2 Cub (NC13Y)	Dec. 1935		
87	Taylor E-2 Cub (NC12117)	Dec. 1935		
88	Kellett YG-2 Autogiro (35-279)	Dec. 1935	Mar. 1936	
89	Kellett YG-1 Autogiro (35-278)	Jan. 1936	May 1936	
90	Curtiss XBFC-1 (BuNo. 9219)	Jan. 1936		
91	Aeronca E113A (NR13089)	Jan. 1936		
92	Franklin PS-2 Glider (BuNo. 9615)	Apr. 1936		
93	Franklin PS-2 Glider (BuNo. 9614)	Apr. 1936		
94	Stinson Reliant SR-8E	July 1936	1951	94
95	Aeronca C-2N			95
96	Ryan ST	Aug. 1938	1947	96

Kellett autogiro, April 1936. This type of aircraft could make extremely short takeoffs and landings, but it was not capable of the helicopter's hovering or vertical flight. In theory the autogiro's main advantage over the conventional airplane was that, in the event of engine failure, its autorotation permitted safe descent.



The NACA equipped one of the navy's Curtiss XBFC-1 fighter-bombers with a nose slot cowling in October 1937.

Order of receipt at Langley	Aircraft type and serial no.	Arrived	Departed	NACA no.
97	Curtiss XSBC-3 (BuNo. 9225)			
98	Cunningham-Hall X14324	1936		
99	Grumman XSBF-1 (BuNo. 9998)			
100	Pitcairn XOP-2			

Stinson Reliant model SR-7 on floats moored in the Back River, 1936.

Order of receipt at Langley	Aircraft type and serial no.	Arrived	Departed	NACA no.
101	Vought O2U-4 (BuNo. 8104)	Mar. 1937		
No number	Consolidated PB2A	May 1937		
102	Curtiss SOC-1			
103	Boeing P-26A			
104	Curtiss XF13C-3 (BuNo. 9343)			
105	North American BT-9A			
106	Martin B-10B			
107	Vought SB2U-1			
108	Douglas DC-3 (NC16070, United Air Lines)			
109	Douglas XB-7 (AC30-228)			
110	Seversky P-35 (X1254 and X1390)			
111	Grumman F3F-2			
112	Fairchild F-46			
113	Douglas B-18 Bolo			
114	Kellett YG-1B Autogiro			
No number	Brewster XF2A-1			
115	Stearman-Hammond Y (NS73)			
116	Wilford XOZ-1 Autogiro	Aug. 1937	1941	
117	Grumman XF4F-2 (BuNo. 0383)			
118	Northrop A-17A (AC 85)			
119	Grumman F3F-2 (BuNo. 0967)			

This twin-engine Douglas Dolphin amphibian was unusual in that it employed a nose wheel instead of a tail wheel, January 1937.

Stearman-Hammond Model Y, January 1939, winner of the $700 safe airplane competition sponsored by the Department of Commerce in the mid-1930s. Only a few of these aircraft were produced, and at a cost far in excess of $700.

Order of receipt at Langley	Aircraft type and serial no.	Arrived	Departed	NACA no.
120	Fairchild XC-31 (AC 34-26)			
121	Vought SB2U-1 (BuNo. 0726)			
122	Douglas OA-4A (AC 32-404)	June 1938		
123	Curtiss AT-5 (AC 28-66)			

Langley tried a center fin to improve the directional stability of its Lockheed Model 12 executive transport, June 1940.

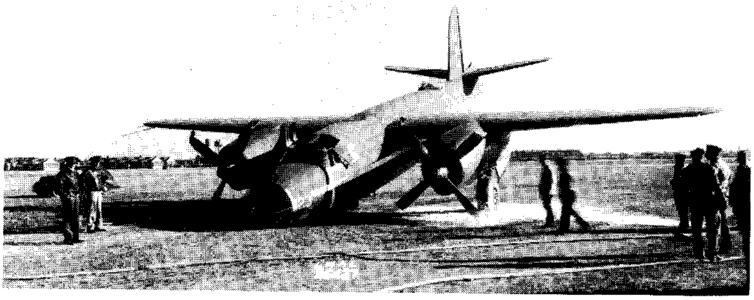
Order of receipt at Langley	Aircraft type and serial no.	Arrived	Departed	NACA no.
124	North American BT-9B (AC 37-227)	Aug. 1938	Feb. 1941	
125	Ryan			
126	North American BC-1	Aug. 1938	Sept. 1938	
127	Vought XSB3U-1 (BuNo. 9834)	Aug. 1938	Jan. 1939	
128	Vought SB2U-2 (BuNo. 1326)	Nov. 1938	Mar. 1939	
129	Vought SB2U-1 (BuNo. 0770)	Nov. 1938	Dec. 1938	
No number	Sikorsky XPBS-1 (BuNo. 9995)	1938		
130	Douglas XBT-2			
131	Lockheed 12 (transferred to Ames lab) (NC17396)	Jan. 1939	Oct. 1940	97
132	Northrop A-17A (AC 36-184)	Feb. 1939	June 1939	
133	Brewster XSBA-1 (BuNo. 9726)	Feb. 1939		
134	Lockheed XR4O Model 14 (BuNo. 1441)	Feb. 1939		
135	Curtiss P-36A	Mar. 1939	Nov. 1939	

Order of receipt at Langley	Aircraft type and serial no.	Arrived	Departed	NACA no.
136	Curtiss XP-40 (AC 38-10)	Mar. 1939	Mar. 1944	
137	Seversky P-35	Apr. 1939		
138	Seversky XP-41 (AC 36-430)	May 1939	Apr. 1942	
139	Fairchild (NX18689)	May 1939	Apr. 1942	
140	Bell XP-39 (AC 38-326)	June 1939		
141	Grumman XF4F-3 (BuNo. 0383)	Aug. 1939		
142	Aeronca 65-C	Aug. 1939	Sept. 1939	
143	Douglas B-18	Sept. 1939	Nov. 1941	
144	Kellett YG-1B Autogiro (AC 37-635)	Sept. 1939	Jan. 1940	
145	Piper Cub (NC26899)	Sept. 1939	Oct. 1947	98
146	Piper Cub #2	Sept. 1939	Oct. 1939	
147	Taylorcraft	Oct. 1939		
148	Douglas R2D	Dec. 1939	1941	
149	Stinson 105	Jan. 1940	Mar. 1940	
150	Beechcraft (NC20780)	June 1940	June 1940	
151	Beechcraft (NC19494)	June 1940	June 1940	
152	Curtiss P-40	June 1940	July 1940	
153	Bellanca Crusair (NC15690)	June 1940	July 1940	
154	Bellanca Crusair (NC25303)	July 1940	July 1940	
155	Douglas C-39	July 1940	July 1940	
156	Curtiss P-40	June 1940	July 1940	
157	Curtiss XP-42 (AC 38-4)	Sept. 1940	Nov. 1942	
158	Curtiss XSO3C-1 (BuNo. 1385)	Sept. 1940	Nov. 1940	
159	St. Louis PT-LM-4 (NX25500)	Sept. 1940	Oct. 1940	
160	Lockheed XC-35 (AC 36-353)	Oct. 1940	Feb. 1943	
161	Brewster XF2A-2 (BuNo. 0451)	Oct. 1940	June 1941	

Curtiss XP-42 fighter with various test modifications in flight over Langley, 1945.

The NACA used this Lockheed XC-35 in 1943 to study cabin pressurization at high altitudes.

Order of receipt at Langley	Aircraft type and serial no.	Arrived	Departed	NACA no.
162	Grumman F4F-3 (BuNo. 1845)	Nov. 1940	May 1942	
163	Curtiss P-36C	Dec. 1940	June 1941	

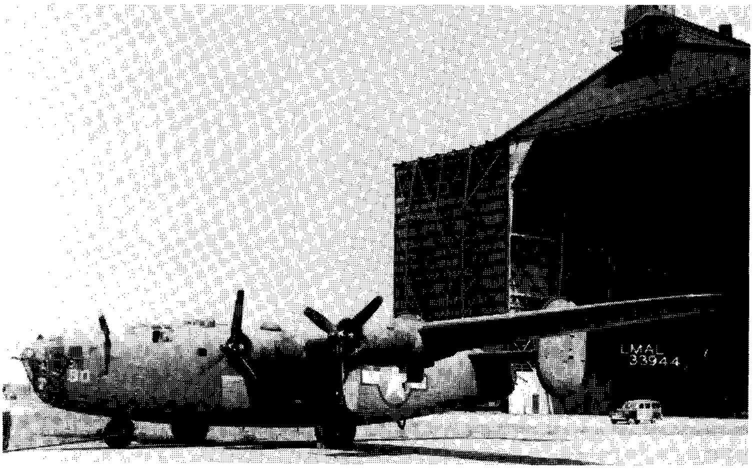


This Martin B-26 crashed at the end of an NACA test flight in December 1941 when its nose wheel failed to extend.

Order of receipt at Langley	Aircraft type and serial no.	Arrived	Departed	NACA no.
164	Republic XP-41 (AC 36-430)	Dec. 1940	Apr. 1942	
165	Douglas A-20A	Jan. 1941	Aug. 1941	
166	Brewster XSBA-1 (BuNo. 9726)	Jan. 1941	Sept. 1945	
167	Republic YP-43	Feb. 1941	Feb. 1941	
168	Bell YP-39	Feb. 1941	July 1944	
169	Curtiss YP-37 (AC 38-474)	Feb. 1941	Jan. 1942	
170	Grumman F4F-3 (BuNo. 2538)	Apr. 1941	May 1942	
171	Lockheed 12A	May 1941	Mar. 1960	99
172	Vought XF4U-1 (BuNo. 1443)	June 1941	Mar. 1943	
173	Fleetwing 33	Oct. 1941	Dec. 1941	
174	Martin B-26	Nov. 1941	Dec. 1941	
175	Hawker Hurricane (25017)	Nov. 1941	Dec. 1941	
176	Spitfire R7347 (35497)	Nov. 1941	Jan. 1943	
177	Lockheed YP-38 #2 (AC 39-690)	Nov. 1941	Feb. 1942	
178	Spitfire W3119 (37147)	Dec. 1941	Feb. 1942	
179	North American XP-51 (AC 41-38)	Dec. 1941	Dec. 1942	
180	Grumman F4F-3 (BuNo. 3990)	Feb. 1942	May 1943	

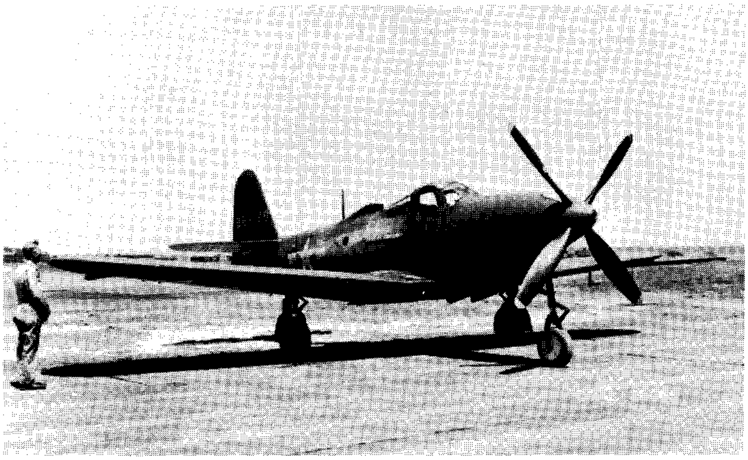
Order of receipt at Langley	Aircraft type and serial no.	Arrived	Departed	NACA no.
181	Curtiss P-40E (AC 41-5534)	Mar. 1942	July 1942	
182	Republic P-47B (AC 41-5897)	Mar. 1942	Oct. 1942	
183	Curtiss SNC-1 (BuNo. 6295)	Apr. 1942	May 1942	
184	Grumman XTBF-1 (BuNo. 2540)	May 1942	June 1943	
185	Curtiss P-40K-1	July 1942	Oct. 1944	
186	Republic P-47B #2 (AC 41-5901)	July 1942	Oct. 1942	
187	Curtiss P-40E (AC 42-45801)	July 1942	Sept. 1942	
188	Brewster F2A-2 (BuNo. 1426)	July 1942	June 1944	
189	Curtiss P-40F (AC 41-13600)	July 1942	Sept. 1942	
190	Fairchild 24 (transferred to Ames)	Sept. 1942	Oct. 1942	100
191	Republic P-47C	Oct. 1942	Dec. 1942	
192	Republic P-47C (AC 41-6102)	Oct. 1942	Dec. 1942	
193	Vought F4U-1	Oct. 1942	Mar. 1943	
194	Republic P-47C-1 (AC 41-6130)	Oct. 1942	Aug. 1944	
195	Curtiss SB2C-1 (BuNo. 00014)	Dec. 1942	May 1943	
196	North American SNJ-3C (BuNo. 01847)	Dec. 1942	July 1945	
197	Bell P-39D-1 (AC 41-28378)	Jan. 1943	Apr. 1943	
198	Grumman F6F-3 (BuNo. 04776)	Feb. 1943	June 1946	
199	Curtiss SB2C-1 (BuNo. 00056)	Feb. 1943	July 1943	
200	Republic XP-47F (AC 41-5938)	Feb. 1943	Oct. 1943	
201	Japanese Zero (4593)	Mar. 1943	Mar. 1943	
202	North American XP-51 (AC 41-39)	Mar. 1943	Jan. 1944	
203	Curtiss SB2C-1 (BuNo. 00140)	May 1943	Nov. 1943	

A captured Japanese Zero was evaluated (with new markings)at Langley in March 1943.



A Consolidated B-24D Liberator rests outside the Langley airship hangar in July 1943. The air force dismantled this hangar in 1947 and 1948.

Order of receipt at Langley	Aircraft type and serial no.	Arrived	Departed	NACA no.
204	Martin RA-30 (AC 41-27687)	June 1943	Mar. 1944	
205	Consolidated B-24D (AC 42-40223)	June 1943	Apr. 1944	
206	Vought F4U-1 (BuNo. 02161)	July 1943	Sept. 1943	
207	Bell P-63 (AC 42-68861)	July 1943	Nov. 1943	



The army's Bell P-63 Kingcobra fighter employed one of the NACA's laminar-flow airfoil sections, May 1944.

Order of receipt at Langley	Aircraft type and serial no.	Arrived	Departed	NACA no.
208	Curtiss P-40F (AC 41-14119)	July 1943	Feb. 1945	
209	General Motors TBM-1 (BuNo. 24820)	Aug. 1943	Nov. 1943	
210	North American P-51B (AC 43-12105)	Aug. 1943	Jan. 1951	
211	North American P-51B-1	Sept. 1943	Oct. 1943	104
212	Republic P-47D-3-RE (AC 42-8207)	Oct. 1943	Mar. 1944	
213	North American XP-51 (AC 41-38)	Jan. 1944	July 1945	
214	Sikorsky YR-4B	Jan. 1944	Oct. 1948	
215	Bell P-63A-1BE (AC 42-68889)	Feb. 1944	June 1944	
216	Douglas A-26B-2-DL (AC 41-39120)	Mar. 1944	June 1944	
217	Grumman XF6F-4 (BuNo. 02981)	Apr. 1944	Mar. 1945	
218	Sikorsky HNS-1 (BuNo. 39034)	Apr. 1944		
219	Curtiss SB2C-1C (BuNo. 18294)	Apr. 1944	June 1945	
220	Bell P-63A-1-BE (AC 42-68881)	May 1944	1946	

Langley test pilots, 1945. To the far left is chief test pilot Melvin N. Gough.

Order of receipt at Langley	Aircraft type and serial no.	Arrived	Departed	NACA no.
221	Cessna UC-78-18-1-CE (AC 43-31957)	June 1944	Aug. 1945	
222	Republic P-47D-15-RA	July 1944	July 1945	
223	Republic P-47D-28-RE (AC 42-28541)	July 1944	Apr. 1948	
224	Grumman TBM-3 (BuNo. 22857)	Aug. 1944	Aug. 1944	
225	Brewster F3A-1 (BuNo. 11213)	Aug. 1944	Sept. 1945	
226	Spitfire (EN-474)	Aug. 1944	Nov. 1944	
227	DeHavilland F-8 (AC 43-34928)	Aug. 1944	Jan. 1945	
228	DeHavilland F-8 (AC 43-34960)	Aug. 1944	Jan. 1945	
229	Douglas A-26B-10-DT (AC 43-22280)	Sept. 1944	Oct. 1945	
230	Douglas SBD-5 (BuNo. 28373)	Sept. 1944	Oct. 1945	101
231	Curtiss SC-1 (BuNo. 35324)	Oct. 1944	Feb. 1945	

Sikorsky's HNS-1 helicopter was test flown at Langley in March 1945.

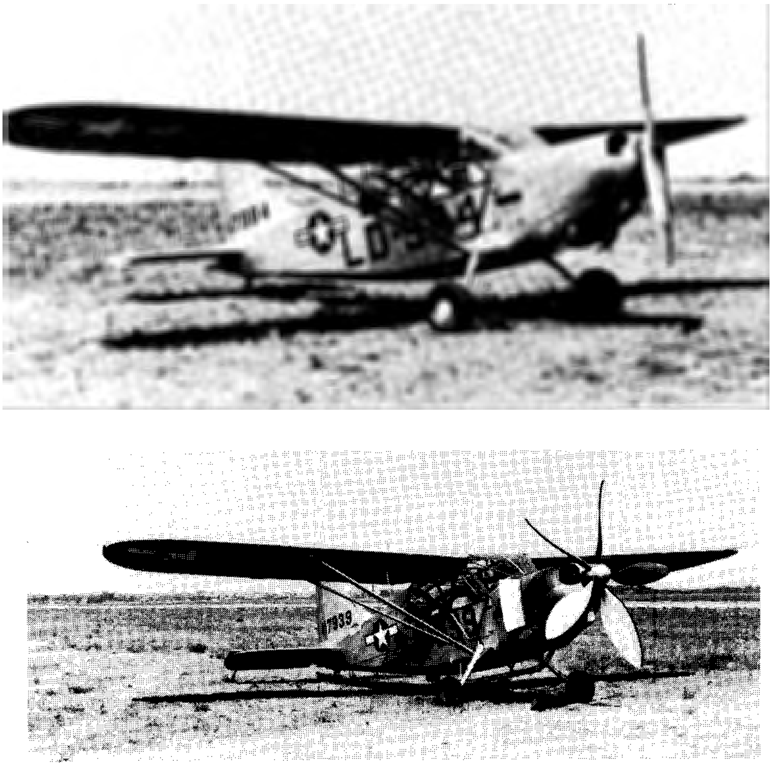
The NACA used this Grumman JRF-5 Goose amphibian to shuttle workers back and forth between Langley and Wallops Island.

Order of receipt at Langley	Aircraft type and serial no.	Arrived	Departed	NACA no.
232	Grumman XF8F-1 (BuNo. 90460)	Dec. 1944	Feb. 1945	
233	North American P-51D-5 (AF 44-13257)	Dec. 1944	May 1957	108
234	Curtiss SB2C-3 (BuNo. 19332)	Jan. 1945	June 1945	
235	General Motors FM-2 (BuNo. 74507)	Jan. 1945	Apr. 1945	
236	Culver PQ-14B (AF 44-21896)	Jan. 1945	Apr. 1949	
237	North American P-51D-5 (AF 44-14017)	Jan. 1945	June 1952	102
238	Republic P-47N-1-RE (AF 44-87790)	Jan. 1945	July 1945	
239	Republic P-47D-30-RA (AF 44-33441)	Jan. 1945	Oct. 1948	
240	Vought F4U-1 (BuNo. 82716)	Feb. 1945	Mar. 1945	
241	Beechcraft UC-45F (AF 44-47264)	Mar. 1945	1951	
242	Sikorsky JRS-1 (BuNo. 1063)	Apr. 1945	Nov. 1946	
243	Boeing B-29B (AF 44-83927)	May 1945	Sept. 1946	
244	Grumman XF8F-1 (BuNo. 90461)	May 1945	Apr. 1946	
245	North American P-51B (AF 43-12114)	June 1945	July 1945	
246	North American P-51B (AF 43-12491)	June 1945	Oct. 1945	
247	North American P-51H (AF 44-64164)	June 1945	Sept. 1946	
248	Grumman JRF-5 (BuNo. 34094)	July 1945	Apr. 1946	103
249	North American SNJ-3 (BuNo. 05475)	July 1945	Oct. 1948	
250	Boeing TB-29 (AF 44-69700)	July 1945	Nov. 1950	
251	Vought F4U-1D (BuNo. 50378)	Aug. 1945	Aug. 1945	
252	Consolidated PBY-5A (BuNo. 2473)	Aug. 1945	Sept. 1945	

Order of receipt at Langley	Aircraft type and serial no.	Arrived	Departed	NACA no.
253	Republic P-47N-25-RE (AF 44-89303)	Aug. 1945	Dec. 1950	
254	North American TP-51D (AF 44-63826)	Aug. 1945	Apr. 1946	
255	Douglas C-47-DL (AF 41-18392)	Aug. 1945	Oct. 1945	
256	North American P-51D-25 (AF 44-84864)	Aug. 1945	July 1957	126
257	Douglas BTD-1 (BuNo. 09060)	Aug. 1945	Apr. 1946	
258	Curtiss SC-1 (BuNo. 93334)	Aug. 1945	Apr. 1946	
259	North American P-51D-25 (AF 44-84900)	Sept. 1945	June 1952	127
260	North American P-51D-25 (AF 44-84944)	Sept. 1945	June 1952	128
261	North American P-51D-25 (AF 44-84953)	Sept. 1945	June 1952	129
262	North American P-51D-25 (transferred to Muroc HSFS) (AF 44-84958)	Sept. 1945	Aug. 1950	148
263	Douglas C-47A-25-DK (AF 42-93791)	Oct. 1945	Apr. 1946	
264	Douglas BTD-1 (BuNo. 09058)	Oct. 1945	Nov. 1945	
265	Curtiss SB2C-4E (BuNo. 82877)	Nov. 1945	Oct. 1946	
266	Boeing B-29-96-BW (AF 45-21808)	Nov. 1945	Dec. 1955	124
267	Grumman F8F-1 (BuNo. 90448)	Jan. 1946	Feb. 1946	
268	Beechcraft UC-45 (transferred to Muroc HSFS) (AF 44-47110)	Jan. 1946	May 1947	105
269	Douglas C-47B-51-DK (transferred to Lewis Research Center) (AF 43-49526)	Jan. 1946	Oct. 1971	106
270	Northrop P-61C (AF 43-8327)	Feb. 1946	Feb. 1946	
271	Grumman JRF-5 (BuNo. 34088)	Apr. 1946	June 1948	103

The L-39 was a purely experimental version of the Bell P-63 Kingcobra fighter designed to study the low-speed handling characteristics of swept wings, December 1946.

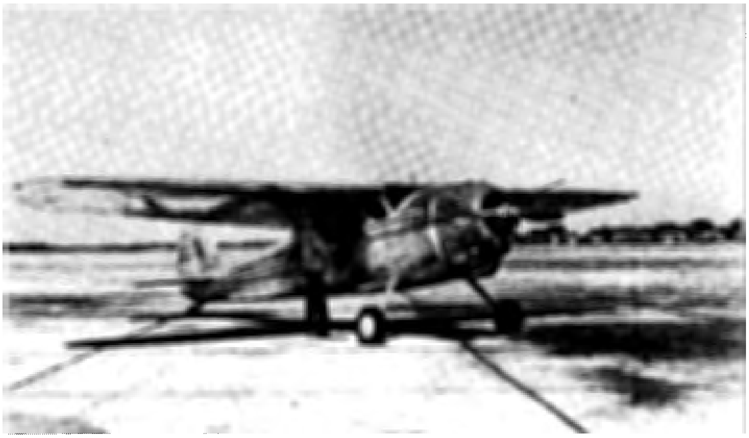
The east area hangar, 1947, with a Douglas C-54 and two B-29 Superfortresses and, in the background, the 19-Foot Pressure Tunnel and the Full-Scale Tunnel.



Langley modified a Stinson L-5 to show that a quiet airplane could be developed. During the lunchtime of the NACA's annual inspection in 1946, the modified (lower photograph) and the standard aircraft were flown separately over the conference building. Those who witnessed the demonstration were astonished by the relative quiet of the modified L-5.

Order of receipt at Langley	Aircraft type and serial no.	Arrived	Departed	NACA no.
272	Grumman F8F-1 (BuNo. 94812)	May 1946	Apr. 1947	
273	Fairchild PT-19A (AF 42-83595)	May 1946	1950	
274	Grumman F8F-1 (BuNo. 94873)	June 1946	1951	
275	North American AT-6 (AF 44-81682)	July 1946	Jan. 1959	117
276	Douglas R4D-6 (transferred to Lewis) (BuNo. 50795)	Aug. 1946	Oct. 1946	

Order of receipt at Langley	Aircraft type and serial no.	Arrived	Departed	NACA no.
277	Douglas R4D-6 (BuNo. 50826)	Aug. 1946	Dec. 1948	
278	Douglas R4D-6 (transferred to Ames) (BuNo. 50812)	Aug. 1946	Oct. 1946	
279	Bell L-39-1 (transferred to Lewis) (BuNo. 90060)	Aug. 1946	Dec. 1949	
280	Douglas C-54D (AF 42-72713)	Sept. 1946	Aug. 1947	
281	Lockheed P-80A-5	Nov. 1946	July 1954	112
282	Bell L-39-2 (BuNo. 90061)	Dec. 1946	Dec. 1949	
283	Vultee L-5E-1VW (AF 44-17984)	Mar. 1947	Oct. 1947	
284	Vultee L-5E-1VW (AF 44-17939)	Mar. 1947	Nov. 1947	
285	Grumman J4F-2 (BuNo. 32972)	Aug. 1947	June 1951	
286	Sikorsky HO3S-1 (BuNo. 122520)	Feb. 1948	Sept. 1961	201
287	Grumman J4F-2 (BuNo. 32976)	May 1948	1951	
288	North American XP-82 (AF 44-83886)	June 1948	Oct. 1955	114
289	Grumman JRF-5 (BuNo. 37778)	June 1948	July 1958	103
290	Cessna 190 (N3477V)	Aug. 1948	June 1958	104
291	North American B-45 (AF 47-21)	Oct. 1948	Aug. 1952	121
292	North American SNJ-5 (BuNo. 84839)	Oct. 1948	June 1957	
293	Douglas R4D-6 (BuNo. 50831)	Nov. 1948	July 1952	
294	Bell H-13B (AF 48-839)	May 1949	Apr. 1950	
295	Lockheed TO-1 (BuNo. 33870)	May 1949	Oct. 1949	
296	Beechcraft C-45F (AF 43-35906)	Aug. 1949	Mar. 1959	125



In 1948 this Cessna 190 was used by the NACA to test new ways of controlling the boundary layer at the wing's leading edge.

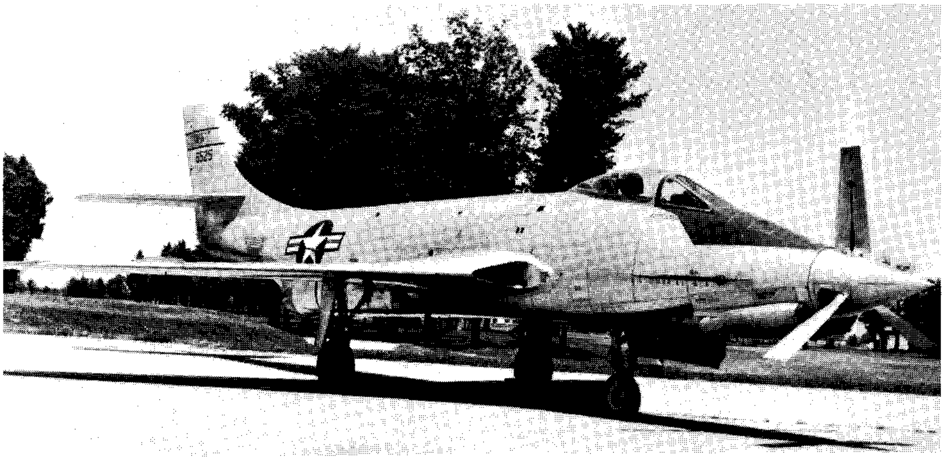
Order of receipt at Langley	Aircraft type and serial no.	Arrived	Departed	NACA no.
297	Republic YF-84 (transferred to Muroc HSFS) (AF 45-59490)	Aug. 1949	Nov. 1949	134
298	Vought F4U-4B (BuNo. 97392)	Mar. 1950	1951	
299	Vultee L-5 (AF 45-34927)	May 1950	Nov. 1950	
300	Beechcraft Bonanza 35 (N5094C)	Aug. 1950	Nov. 1950	
301	McDonnell F2H (BuNo. 122540)	Oct. 1950	June 1951	
302	Grumman F9F-2 (BuNo. 122560)	Jan. 1951	July 1960	215
303	Lockheed TO-2/TV-2 (BuNo. 124933)	Jan. 1951	Feb. 1960	
304	Piasecki HRP-1 (BuNo. 111813)	Feb. 1951	1951	
305	North American F-86A-1 (AF 47-620)	Feb. 1951	July 1958	136
306	McDonnell F2H-1 (BuNo. 122530)	Aug. 1951	Sept. 1959	214
307	Boeing B-47A (AF 49-1900)	July 1952	Mar. 1953	
308	Sikorsky HRS-1 (BuNo. 127783)	Mar. 1953	Aug. 1964	211

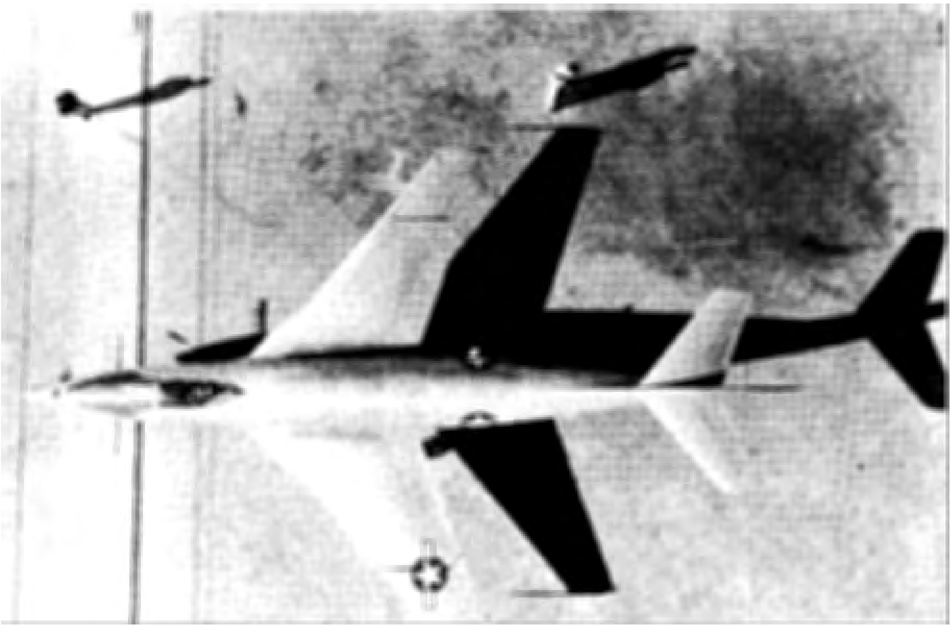
Congestion in the west area hangar, 1952.

Order of receipt at Langley	Aircraft type and serial no.	Arrived	Departed	NACA no.
309	McDonnell XF-88B (AF 46-525)	July 1953	Sept. 1958	
310	Hiller HTE-1 (BuNo. 128646)	July 1953	Feb. 1956	
311	Bell H-13G (AF 52-7834)	Sept. 1953	July 1967	
312	Lockheed F-80B (AF 45-8683A)	Dec. 1953	Oct. 1958	152
313	Grumman F9F-7 (BuNo. 130864)	Jan. 1954	Nov. 1959	
314	Vertol H-25A/HUP-1 (51-16574G)	Feb. 1954	Jan. 1960	
315	McDonnell F2H-3 (BuNo. 126300)	July 1954	Sept. 1959	210
316	Grumman JRF-5 (BuNo. 37816)	Aug. 1954	Nov. 1954	103
317	Vertol H-25 (51-16637)	Sept. 1954	Feb. 1955	

The navy's Vought XF8U-3 supersonic fighter was an entirely new design as compared to the earlier F8U Crusader series. This jet plane lost in competition with the McDonnell F4H, however, and was never put into production. Langley used the XF8U-3 in some of the first flight measurements of sonic boom intensity.

Order of receipt at Langley	Aircraft type and serial no.	Arrived	Departed	NACA no.
318	Grumman JRF-5 (replaced #103, which was lost in crash at Wallops Island, 3 Nov. 1954) (BuNo. 87748)	Feb. 1955	Mar. 1960	202/103
319	McDonnell XF-88A (AF 46-526)	Feb. 1955	July 1958	
320	Beechcraft C-45F (AF 44-47106)	Mar. 1955	Mar. 1959	
321	North American EJF-86D (AF 50-459)	Feb. 1956	May 1960	204
322	North American F-86D-31 (AF 51-5959A)	Mar. 1956	Sept. 1958	
323	North American JF-86D-5 (AF 50-509)	Apr. 1956	July 1960	205
324	Sikorsky HSS-1 (BuNo. 137855)	June 1956	Feb. 1957	
325	McDonnell F-101A (AF 53-2434)	Aug. 1956	Mar. 1960	219
326	Hiller YH-32 (55-4968)	Nov. 1956	Nov. 1958	
327	Hiller YH-32 (55-4970)	Nov. 1956	Nov. 1958	





Two views of the McDonnell XF-88B experimental jet fighter. Langley used this aircraft in the mid-1950s to explore the potential of a supersonic propeller. In the overhead view, note the survey rake mounted just behind the prop.

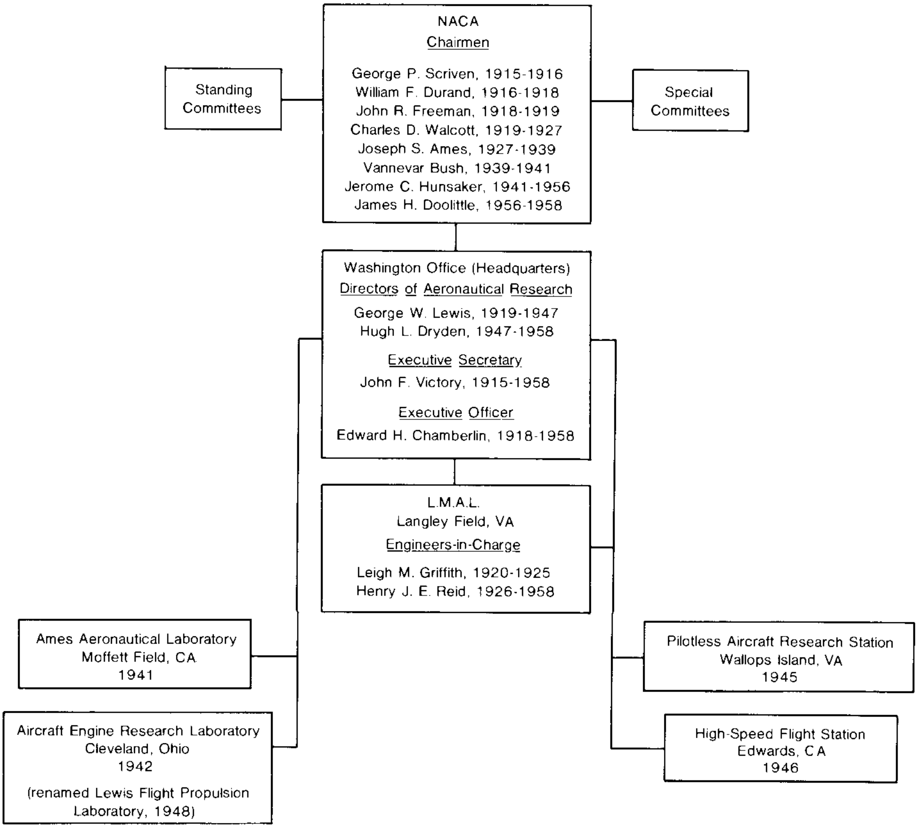
Order of receipt at Langley	Aircraft type and serial no.	Arrived	Departed	NACA no.
328	Vought F8U-1 (BuNo. 141354d)	Dec. 1956	Feb. 1959	
329	Grumman F11F-1 (BuNo. 138623)	Jan. 1957	Aug. 1961	
330	Grumman SA-16A (AF 49-088A)	May 1957	Mar. 1958	
331	North American T-28A (AF 50-279A)	June 1957	Mar. 1959	
332	North American F-100C-25 (AF 54-2024A)	Sept. 1957	Oct. 1959	
333	Lockheed T-33A (AF 49-939A)	Nov. 1957	Feb. 1958	
334	McDonnell F-101A (AF 54-1442A)	Apr. 1958	Dec. 1958	220

Sources: Log books, Aircraft Operations Branch, Bldg. 1244, LaRC; Airplane and Engine Records, 1939–1958, LaRC Historical Archives; Robert L. Burns, "Aircraft Assigned to Langley Aeronautical Laboratory," unpublished typescript with handwritten notes, Aug. 1986, copy in LaRC Archives.

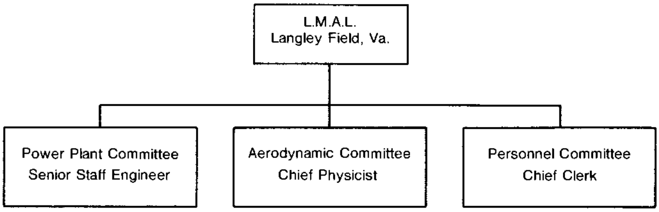
Appendix F

Organization

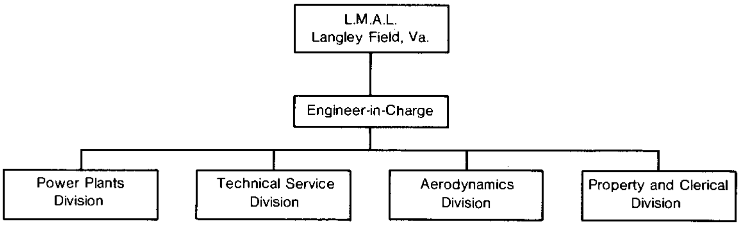
This appendix contains seven organization charts arranged chronologically. They are based on the actual organization charts, but shortened to include only the higher levels, and rearranged to facilitate the readers comparison from chart to chart. The first shows the relation of NACA Langley management to the rest of the NACA. The remaining six trace the evolution of Langley's internal organization.



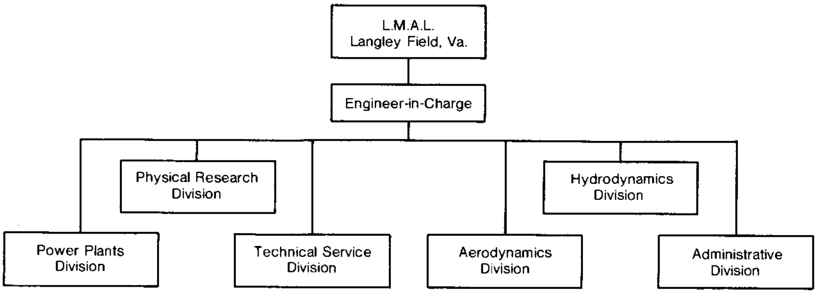
Organization of the NACA, 1915–1958.



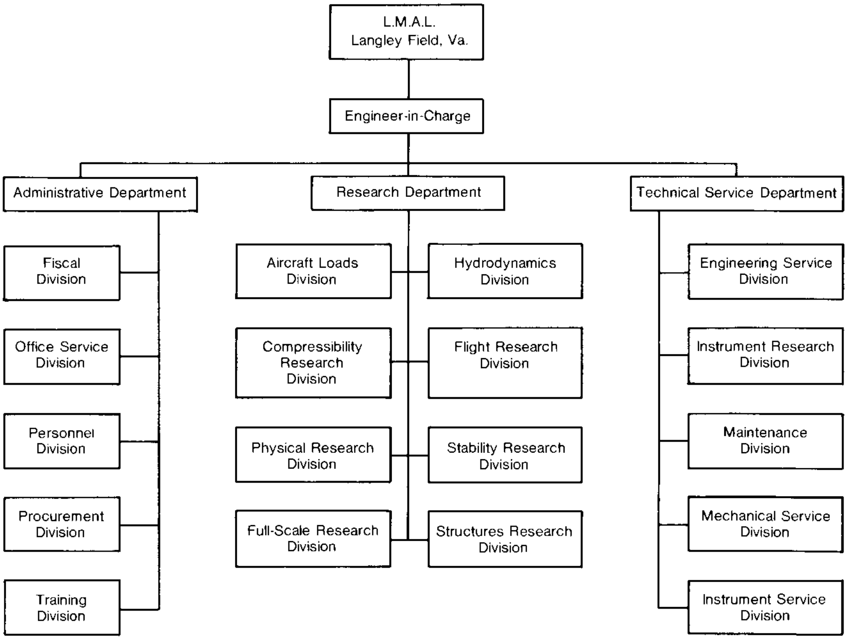
Langley organization chart at the time of the lab's dedication, June 1920.



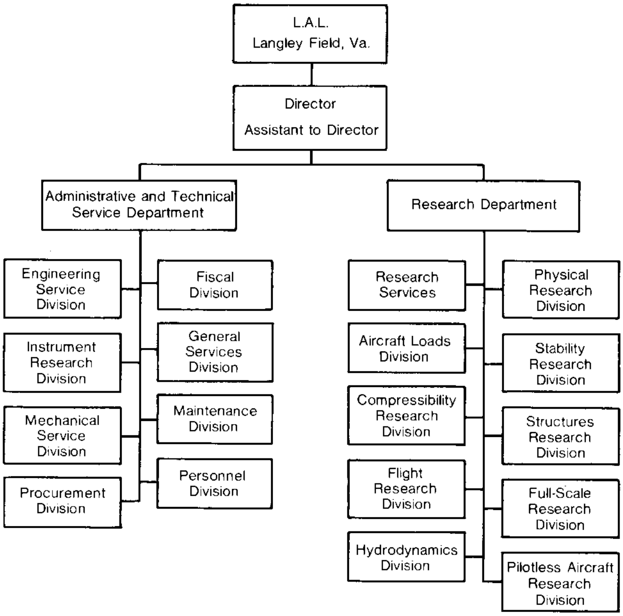
Langley organization chart at the time of Lindbergh's flight, May 1927.



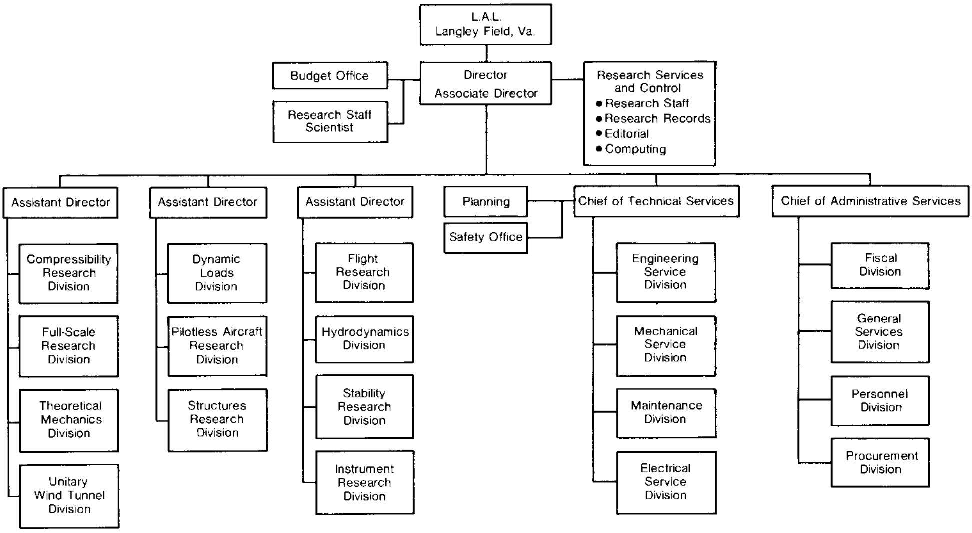
Langley organization chart at the time of the Volta Congress on High-Speed Aeronautics, October 1935.



Langley organization chart at the time of the Allied invasion of Normandy, June 1944.



Langley organization at the time of the first supersonic flight, October 1947.



Langley organization chart at the time of the NACA's 40th anniversary, June 1955.

Notes

Introduction

1. 63 Stat. 410. For the full text of this law, see Alex Roland, *Model Research: The National Advisory Committee for Aeronautics, 1915–1958*, NASA SP-4103 (Washington, 1985), 2:399–400.
2. Bush's remark was recalled by Ralph E. "Mike" Cushman, Special Programs Coordinator, Office of the Comptroller, NASA HQ, during my telephone conversation with him 16 May 1984. Mr. Cushman worked as procurement and supply officer at NACA headquarters from 1947 to 1958.
3. Michener, *Space* (New York: Random House, 1982), p. 173.
4. NACA Tech. Rpt. (TR) 411, printed in *Eighteenth Annual Report of the National Advisory Committee for Aeronautics, 1932* (Washington, 1933), p. 29. (Hereafter NACA annual reports will be cited in the form *AR 1932*.)
5. At various times the Committee attempted to determine the proportion of its work devoted to basic research. Only by using the most liberal interpretation of the word did the NACA conclude that 15 to 20 percent of its research activities were "basic." One NACA veteran believes in retrospect that a more realistic figure would have been about 5 percent. See Ira H. Abbott, "A Review and Commentary of a Thesis by Arthur L. Levine, Entitled 'A Study of the Major Policy Decisions of the National Advisory Committee for Aeronautics,' Dated 1963," NASA HQ History Office Archive (HQA) HHN-35, Apr. 1964, p. 157.
6. Constant, *The Origins of the Turbojet Revolution* (Baltimore: The Johns Hopkins University Press, 1980), p. 156.
7. Preface to *Applied Wing Theory* (New York: McGraw-Hill, 1932), p. vii.
8. I. E. Garrick interview with author, 24 Sept. 1981.
9. Arnold Pacey, *The Maze of Ingenuity: Ideas and Idealism in the Development of Technology* (Cambridge, Mass., and London, England: MIT Press, 1976), p. 15.
10. Ibid.
11. For a close examination of the history of U.S. government laboratories, see Hans Mark and Arnold Levine, *The Management of Research Institutions: A Look at Government Laboratories,* NASA SP-481 (Washington, 1985).

Chapter 1

Foundations

1. Announcement of the "First Annual Banquet of the Aeronautical Society," 1911, in file entitled "Materials Collected by Roland for NACA History," NASA HQ History Office Archive (HQA), Washington. (These materials, along with those collected by Walter Bonney between 1971 and 1975 during his unfinished work on an NACA history, were to be retired to the Washington National Records Center in a single

accession.) The events of this April 1911 meeting of the American Aeronautical Society are described by Alex Roland in *Model Research: The National Advisory Committee for Aeronautics, 1915-1958,* NASA SP-4103 (Washington, 1985), pp. 4-5.

2. Chief, Bureau of Construction and Repair, to Secretary of the Navy, "Relative to Proposed Establishment of an Aeronautical Laboratory in Washington," 20 Apr. 1911, in Roland's "Materials," HQA; Richard C. Maclaurin, "The Sore Need of Aviation," *Aero Club of America Bulletin,* Aug. 1912, p. 7. See also Lee M. Pearson, "The Role of the U.S. Navy in Establishing a National Aeronautical Research Agency," paper read before the History of Science Society, New York City, 28 Dec. 1956, copy in Roland's "Materials." For contemporary insight into the applications of hydrodynamics to the calculation of the forces acting on airplane wings and airship bodies, see Ludwig Prandtl, "Application of Modern Hydrodynamics to Aeronautics," NACA Tech. Rpt. (TR) 116, 1921. (This paper, translated by the staff of the NACA, was prepared by Prandtl at the NACA's special request for a detailed treatise on the hydrodynamic-aerodynamic relationship.)
3. Albert F. Zahm, *Aeronautical Papers* (Notre Dame, Ind.: University of Notre Dame Press, 1950), 1:245.
4. Quoted from Roland, *Model Research,* p. 2; Rep. Gilbert Hitchcock (R., Neb.), *Brooklyn Eagle,* 13 Mar. 1904.
5. See J. Laurence Pritchard, "The Dawn of Aerodynamics," *Journal of the Royal Aeronautical Society* 61 (Mar. 1957): 176, and N. H. Randers Pherson, "Pioneer Wind Tunnels," *Smithsonian Miscellaneous Collections* 93 (19 Jan. 1935).
6. *Model Research,* p. 5.
7. Charles D. Walcott, "Minutes of First Meeting of the Advisory Committee of the Langley Aerodynamical Laboratory, May 23, 1913"; reprinted in *Model Research,* app. H, no. 3, pp. 585-591. George E. Downey, Comptroller of the Treasury, to Walcott, 17 Mar. 1914, in "Secretary's File, 1909-1924," in Roland's "Materials."
8. H.R. 20975, U.S. Congress, *Congressional Record,* 63/3, 1915, pp. 4600-26, 4694-716, 5209-16. The full text of the law establishing the NACA is reprinted as app. A of this book.
9. The rider on the naval appropriation bill was based closely on House J.R. 413, 63/3, 1 Feb. 1915.
10. Wing-warping was the technique by which a pilot imparted helical twist to the wings by manually raising or lowering either wing. The technique derived from a method for the aerodynamic control of large kites developed from an understanding of the flight of birds. See Tom D. Crouch, *A Dream of Wings* (New York, 1981), p. 230.
11. David Noble provided a New Left analysis of this development in his 1977 book, *America by Design: Science, Technology, and the Rise of Corporate Capitalism* (New York: Knopf). Noble concluded that the American patent system "gradually fostered the corporate control of the process of invention itself and thus facilitated the commercially expedient retardation, as well as promotion, of invention" (p. 85). For an alternative appraisal, see Floyd L. Vaughn, *The United States Patent System: Legal and Economic Conflicts in American Patent History* (Norman, Okla., 1956).
12. Alice Quinlan, "World War I Aeronautical Research: A Comparison of the National Advisory Committee for Aeronautics and the National Research Council," NASA HHN-135, 1974, HQA.
13. *AR 1917,* pp. 31-32.
14. Griffith to Executive Committee, 4 Apr. 1918, in Roland's "Materials."

15. David Kite Allison, *New Eye for the Navy: The Origin of Radar at the Naval Research Laboratory,* NRL report 8466 (Washington, 1981), p. 14.
16. House Naval Affairs Committee, *Hearings on Estimates Submitted by the Secretary of the Navy, 1916* (64/1, 1916), p. 1811.
17. P. 20. Scriven apparently expected the NACA always to serve military interests and to endorse all military requests for congressional appropriations. See Brig. Gen. George P. Scriven to Advisory Committee for Aeronautics, 16 Apr. 1915, in Roland's "Materials."
18. Josephus Daniels to the president, 30 Nov. 1915; reprinted in *Model Research,* app. H, no. 8, p. 602.
19. The NACA's longtime executive secretary, John F. Victory, too often told the story that navy men disguised themselves as fishermen and army officers as hunters, when inspecting the Hampton site, in order to keep land prices down. Alex Roland dismisses the story as apocryphal: "It is true that the notion of a joint site was discussed with navy representatives present," Roland noted in *Model Research* (p. 340 n. 20), "but [the navy] quickly squelched any expectations the NACA may have had about [its] participation."
20. "Report of the Subcommittee on a Site for Experimental Work and Proving Grounds for Aeronautics, 23 November 1916," in Milton Ames Collection, Box 1, NASA Langley Research Center Historical Archive (LHA). Board members included Lt. Col. G. O. Squier, Capt. T. D. Milling, Capt. V. E. Clark, and Capt. R. C. Marshall, Jr. See also *AR 1917,* p. 20.
21. Squier to Chairman, NACA Executive Committee, 10 Nov. 1916, in Roland's "Materials." See also Squier to Adj. Gen. of the Army, "Acquisition of Land for Aviation Purposes, 4 December 1916." (Supporting document 2 to "History of Langley Field, Inception to 1 March 1935," unpublished and undated, USAF Tactical Air Command HQ History Office, Langley Field, Va.)
22. See A. H. Glennan, Acting Surgeon General, to naval constructor Holden C. Richardson, USN, NACA, 18 Nov. 1916, copy in Milton Ames Collection, Box 1, LHA; Charles D. Walcott to G. O. Squier, 23 Nov. 1916, in Roland's "Materials."
23. *AR 1916,* p. 19.
24. "Uncle Sam's Eagles Saved Hampton: Advent of State's Dry Era Threatened Historic City until Trio of Local Patriots Sold Old Plantations to Army for Flying Field and Stopped Real Estate Toboggan," Richmond *Times-Dispatch,* Sunday magazine, 13 Jan. 1935.
25. Word of the government's search for land may have spread to Hampton via newspaper stories, political or military contacts, or perhaps even through a direct inquiry. Walcott had written to a Mr. C. W. Baker, of Aberdeen, Md., on 16 Oct. 1916 about a possible site. Harry Holt, Jr., told the author 10 Sept. 1981 he believes his father may have heard about the proposed airfield through Gen. Billy Mitchell or Virginia's U.S. Senator Thomas S. Martin. The Hampton *Monitor* headlined "Big Aviation Plant May Locate Here" on 27 Oct. 1916, but a meeting the same day between the committee of Hamptonians and Capt. Richard C. Marshall, Signal Corps, strongly suggests earlier knowledge of the government's interest. The exact price paid by Holt and Groome for their options to sell the land is unknown to this author; however, Holt, Jr., told me in a private conversation that he remembers his father coming home one day and informing the family "that if this deal with the government fell through, he would be broke."

26. 17 Dec. 1916. Community enthusiasm for the location of the airfield near Hampton can be followed in a series of articles in the Hampton *Monitor* and the Newport News *Daily Press*: "U.S. Aviators May Come to Back River, Greatest Confidence in This Entertained by All, Decide by December 1, Biggest Thing for Hampton That Has Occurred since Location of Newport News Shipyards," *Monitor*, 24 Nov. 1916; "Mammoth Army Aviation School and Experimental Station Will Be Located on Back River near Hampton," *Daily Press*, 17 Dec. 1916; "Progress at Langley Field Made Each Day, Headquarters Opened by Officer in Charge of Hampton, Work Plant to be Rushed, Railway Organized and Plans and Specifications for Buildings Soon to be Out," *Monitor*, 2 Feb. 1917; editorial, *Monitor*, 29 Mar. 1917; "Facing a Crisis," *Monitor*, 27 July 1917. In July 1918 the *Monitor* published a special "Aviation Edition" supposedly featuring the construction of Langley Field but actually highlighting area businesses and industries.
27. "J. T. Sloan Will Supervise Building at Langley Field," Hampton *Monitor*, 8 Feb. 1917.
28. John Werth, Supt. of Construction, to Maj. C. T. Waring, Construction Div., Signal Corps, Langley Field, "Conditions on Langley Field, 6 December 1917"; John L. Boardman, Construction Supt., J. G. White Engineering Corp., to OIC Construction, Langley Field, 6 Sept. 1918; E. O. Bennett to J. F. Victory, NACA, Washington, 20 Aug. 1918; S. W. Stratton, Secretary of NACA, to Dir. of Military Aeronautics, War Dept., Washington, "Construction of Wind Tunnel Building at Langley Field." Copies of all the above documents are in the Milton Ames Collection.
29. On the history of the DH aircraft, see Aubrey J. Jackson, *DeHavilland Aircraft Since 1909* (London, 1978), pp. 58–66. On the history of the Liberty engine, see Robert Schlaifer, *The Development of Aircraft Engines* (Harvard University, Division of Research, Graduate School of Business Administration, 1950) and Herschel Smith, *Aircraft Piston Engines: From the Manly Baltzer to the Continental Tiara* (New York: McGraw-Hill, 1981).
30. Cited in Robert I. Curtiss, John Mitchell, and Martin Copp, *Langley Field: The Early Years, 1916–1946* (Office of History, 4500th Air Base Wing, Langley AFB, Va., 1977), p. 13. In 1984 and 1985, the Langley AFB *Flyer* published a series of well-researched historical essays on the early days of Langley Field authored by Col. Charles L. Weidinger. In "What Happened to the Proving Ground?" (16 March 1984, p. 19), Colonel Weidinger examines the role of the Liberty engine development in changing the army's plans for Langley.
31. Thomas Wolfe, *Look Homeward, Angel* (New York, 1929), p. 516.
32. Newport News, Va., *Daily Press*, 12 Oct. 1918.
33. Opinion expressed by Lieutenant Colonel Squier, cited in *Langley Field: The Early Years*, p. 196 n. 45.
34. *Model Research*, p. 81. See also Weidinger, "What Happened to the Proving Ground?" Langley *Flyer*, 16 March 1984.
35. The NACA appeals for formal assignment of property were made by Samuel W. Stratton (to Secretary of War, 12 Dec. 1916), William F. Durand (to Lt. Col. George O. Squier, 29 Aug. 1917), and Charles D. Walcott (to Secretary of War, 17 Dec. 1918). Colonel Bane expressed his opposition to any NACA control at Langley Field in a letter to Maj. Gen. William L. Kenly, 15 Jan. 1915. Copies of all of the above letters are in the Milton Ames Collection.

36. C. T. Menoher to Acting Secretary of War, with notation of approval, 22 Apr. 1919, Milton Ames Collection.
37. *AR 1918*, p. 24.
38. Col. Oscar Westover to the NACA, 16 Sept. 1919, copy in Milton Ames Collection.
39. J. H. DeKlyn to Dr. Joseph Ames, with memoranda by DeKlyn and Edward P. Warner, Langley chief physicist, 9 July 1919; J. F. Victory, "Memorandum Regarding Use of Langley Field by NACA," 27 Sept. 1919, in Roland's "Materials"; *AR 1920*, p. 14.
40. *AR 1917*, p. 16; *AR 1918*, p. 24; *AR 1919*, pp. 14–15.
41. NACA research authorization no. 10, approved by the NACA Executive Committee, 20 June 1919. (Hereafter NACA research authorizations will be cited as RAs.) All of Langley's RAs and RA files are in the LHA.
42. NACA Executive Committee minutes, 10 Jan. 1921, in Roland's "Materials."
43. Speech of Rear Adm. D. W. Taylor, USN, Langley Field, 11 June 1920, copy in Milton Ames Collection.
44. Ames to Col. W. N. Hensley, USA, CO, Langley Field, 21 June 1920, cited in Michael D. Keller, "A History of the NACA Langley Laboratory, 1917–1948" (University of Arizona Ph.D. thesis, 1968), p. 73 n. 77.
45. Leigh M. Griffith to George W. Lewis, 17 June 1920, C153-2, Langley Central Files (LCF).

Chapter 2

Langley Personality, Formative Years

1. B. R. Luczak confronted the issue of routine executive control for NACA Ames laboratory in his unpublished paper "A Management and Procedural Analysis of the NACA," submitted to the Stanford University Graduate School of Business Administration in 1950. Copy in the NASA HQ History Office Archive (HQA). Nancy Jane Petrovic also examined the executive management of the NACA and its successor agency, the National Aeronautics and Space Administration, in her doctoral thesis "Design for Decline: Executive Management and the Eclipse of NASA" (University of Maryland, 1982).
2. Speech by Victory, 1952 meeting of the Air Research and Development Command, Baltimore; copy in Langley Research Center Historical Archive (LHA).
3. Jerome C. Hunsaker, "George William Lewis (1882–1948)," reprint from *Year Book of the American Philosophical Society,* 1948, pp. 269–278, copy in LHA.
4. I. E. Garrick interview with Walter Bonney, 27 Mar. 1973, copy of transcript, p. 15. (The LHA preserves copies of all 32 of Bonney's interviews.)
5. T. Melvin Butler interview with Bonney, 29 Mar. 1973.
6. Lewis to Porter Adams, 6 Aug. 1936, in 60 A 635 (11), 1-36, Adams, Porter, Record Group 255, National Archives, Washington, copy in Roland's "Materials."
7. John V. Becker, *The High-Speed Frontier: Case Histories of Four NACA Programs, 1920-1950,* NASA SP-445 (Washington, 1980), p. 22.
8. Victory to John DeKlyn, 7 May 1919, copy in Milton Ames Collection. See also Roland, *Model Research,* p. 84.
9. Early reports by Warner include "Preliminary Report on Free Flight Tests," NACA TR 70; "Slipstream Corrections in Performance Computation," TR 71; "Wind Tunnel Balances," TR 72; "Statical Longitudinal Stability of Airplanes," TR 96;

"Notes on the Theory of the Accelerometer," NACA Tech. Note (TN) 3; and "Problem of the Helicopter," TN 4 (all 1920). For some time after his resignation from Langley, Warner remained the Committee's most prolific author of technical reports and notes. The LHA has a catalog of my three-by-five cards referencing Warner's papers.

10. Telephone interview, Frederick Norton, Dogtown, Mass., with author, 1 Oct. 1981, transcript in LHA. There is also a three-by-five card file in the LHA for papers by Norton.
11. Victory told doctoral candidate Michael D. Keller during an interview on 22 June 1967 that Samuel Stratton, NACA secretary, decided on the title engineer-in-charge. Copy of transcript in LHA.
12. I reached this conclusion after surveying routine correspondence between Griffith and NACA headquarters, in particular letters from 1924 and 1925 in B10-6 ("Washington Office"), Langley Central Files (LCF).
13. "Miscellaneous Correspondence. Local NACA Hqts.-Langley Color," folder in Milton Ames Collection marked "Note: Strict Discipline Required." The employee who recalled Griffith's departure was Smith J. DeFrance. DeFrance also believed that another complaint the Washington office had against Griffith was that his staff was producing an insufficient number of technical reports. DeFrance interview with Michael D. Keller, Hampton, Va., 16 June 1967, notes in LHA.
14. Engineer-in-Charge to Miss Dillon, 27 May 1926, C54-6, LCF.
15. The fact that Reid's signature was on so many Langley letters can be misinterpreted: most of the letters were actually prepared by others.
16. John V. Becker to author, written comments on early draft of this chapter, 10 Mar. 1983.
17. Ibid.
18. See this book's Guide to NACA Historical Sources at Langley for a discussion of the nuances of the research authorization (RA) files in the LHA.
19. Telephone interview, Eastman N. Jacobs, Malibu, Calif., with author, 27 Aug. 1983; Kantrowitz interview with Walter Bonney, Everett, Mass., 1 Nov. 1971.
20. Kantrowitz with Bonney, pp. 2–3. There is a fine description of the Kantrowitz-Jacobs fusion experiments in T. A. Heppenheimer, *The Man-Made Sun: The Quest for Fusion Power* (Boston & Toronto: Little, Brown, and Co., 1983), pp. 286–292.
21. S. Paul Johnston interview with Bonney, Bozman, Md., 19 Oct. 1971. The Committee hired Johnston without Lewis's wholehearted agreement. The two men became polite but intense rivals. Lewis prevailed, and Johnston left after less than two years' employment. Considering their antipathy, Johnston may not be the best source to quote for illustrating Lewis's insistence on soundness and accuracy. Robinson's response to Johnston's comment came to me in personal communication, 12 Feb. 1985.
22. Leigh M. Griffith to George Lewis, 6 Dec. 1927, E26-3, LCF.
23. Joseph Ames, minutes of the NACA Executive Committee meeting, 18 Mar. 1927.
24. Telephone interview, Norton with author, 1 Oct. 1981.
25. DeFrance interview with Bonney, 23 Sept. 1974, p. 1.
26. Thompson interview with Bonney, Hampton, Va., 27 Mar. 1973, pp. 1–3.

27. Elliott G. Reid, *Applied Wing Theory* (New York, 1932), p. vii. On the state of aeronautical education in America during the 1920s and 1930s, see Richard P. Hallion, *Legacy of Flight: The Guggenheim Contribution to American Aviation* (Seattle, 1977), p. 46ff.
28. Arthur Gardiner to Engineer-in-Charge, "Visit to Swarthmore College," 1 May 1924, E32-12A, LCF.
29. Lewis to Klemin, 24 Mar. 1926, 55 A 312 (6), 110.1, Klemin, Alexander (2), Record Group 255, National Archives. On the other hand, Langley engineer Robert Littell told Michael D. Keller on 21 Dec. 1966 (copy of transcript, p.2, LHA) that Lewis had attempted to block some employees from leaving the lab for industry. Lewis apparently did this by calling company presidents. Littell also claimed that Langley employees were aware of this and thus kept their negotiations for new jobs secret.
30. Leigh Griffith to E. P. Lesley, 28 Jan. 1925, in RA file 98.
31. Figures from *Automotive Industries* (23 Feb. 1935), p. 295. The best introduction to these events is still *Climb to Greatness: The American Aircraft Industry, 1920–60* (Cambridge, Mass., 1968), by John B. Rae.
32. Fred Weick has tape-recorded tens of hours of "Historical Reminiscences," and has kindly given transcripts of these recordings to the author. Weick has also given copies of these transcripts to the Smithsonian and to the history of aviation collection at the University of Texas at Dallas. I used these transcripts as the basis of my NASA Langley colloquium lecture, "The Life and Times of Fred Weick, Aeronautical Pioneer," given at Langley on 19 August 1985 in recognition of the sixtieth anniversary of Weick's employment with the NACA. A videotape of the colloquium, which was highlighted by the awarding of a plaque to Weick, who was present as NASA's guest, is available through the office of the film librarian at the Floyd Thompson Technical Library, Langley Research Center, Hampton, Va. Also available from the LaRC film librarian is a videotape of an October 1981 presentation by Zimmerman, "Remembering Langley in the 1930s," in the LHA.
33. Rhode, "NACA: Historical Comments" in response to questions posed to him in a letter from Michael Keller, typescript, 23 Jan. 1967, p. 9, LHA.
34. Hartley A. Soulé, "Synopsis of the History of Langley Research Center, 1915–1939," NASA HHN-40, 1966, ch. 2, p. 2, copies in HQA and LHA.
35. John V. Becker with Bonney, Hampton, Va., 27 March 1973, p. 9.
36. For examples of NACA Langley's LTA flight studies in the 1920s and 1930s, see John W. Crowley, Jr., and Smith J. DeFrance, "Pressure Distribution on the C-7 Airship," TR 223, 1926; DeFrance and C. P. Burgess, "Speed and Deceleration Trials of U.S.S. *Los Angeles*," TR 318, 1928; Floyd L. Thompson, "Full-Scale Turning Characteristics of the U.S.S. *Los Angeles*," TR 333, 1929. For detailed information on the administration of this research, see correspondence in the following research authorization files: RA 76, "Pressure Distribution on a 'C' Class Airship," approved by the Executive Committee on 23 May 1923; RA 102, "Investigation of Aerodynamic Loads on the U.S.S. *Shenandoah*," 12 June 1924; RA 282, "Study of the Forces on an Airship Entering a Hangar," 22 Mar. 1929 (modified to cover "Wind Tunnel Tests of U.S.S. *Akron* at Large Angles of Yaw," 21 Apr. 1932); RA 311, "Study of Deceleration on Metalclad Airship ZNC-2," 24 Oct. 1929; and RA 354, "Investigation of Aerodynamic Loads on U.S.S. *Akron*," 23 June 1931. An illustrated history of the army's airship squadron at Langley is presented in *Langley Field: The Early Years*, pp. 50–51 and 83–94.

37. See Richard K. Smith, *The Airships Akron and Macon: Flying Aircraft Carriers of the U.S. Navy* (Annapolis: U.S. Naval Institute, 1965), and Edward Horton, *The Age of the Airship* (Chicago: Regnery, 1973).
38. These tests were based on the earlier boundary-layer control research of Hugh B. Freeman, an engineer working in Eastman Jacobs's Variable-Density Tunnel section; see Freeman's "Measurements of Flow in the Boundary Layer of a 1/40th-Scale Model of the U.S. Airship 'Akron'," TR 432, 1933. This research was done under the cover of RA 201, "Investigation of Various Methods of Improving Wing Characteristics by Control of the Boundary Layer," which had been authorized in Jan. 1927. Roland has analyzed the history of this RA in app. F of *Model Research*, pp. 529–550.
39. Munk, "Aerodynamic Forces on Airship Hulls," TR 184, 1924. For expert contemporary insight into the special nature of airship aerodynamics, see Munk, "Aerodynamics of Airships" and Karl Arnstein and Werner Klemperer, "The Performance of Airships" in William F. Durand, ed., *Aerodynamic Theory*, 6 vols. (Berlin: Julius Springer, 1934–1936) 6:32–48 and 49–133. See also Edward P. Warner, *Aerostatics* (New York: 1926) and Charles P. Burgess, *Airship Design* (New York: 1927).
40. John V. Becker, *High-Speed Frontier*, p. 211.
41. Story told by Pearl I. Young to Michael Keller, 10 Jan. 1966, Hampton, Va., copy of transcript, pp. 3–4, LHA.
42. See relevant correspondence in C88-10 ("Housing"), LCF.
43. Rhode, "Historical Comments," pp. 6–8, LHA.
44. See "Green Cow Has Fascinating History," Langley Memorial Aeronautical Laboratory *Bulletin*, 5 June 1943. (All references to Langley in-house newspaper articles can be found in the LHA.)
45. See Michael D. Keller, "A History of the NACA Langley Laboratory, 1917–1947" (University of Arizona Ph.D. thesis, 1968), pp. 165–67.
46. Charles Zimmerman interview with Bonney, 30 Mar. 1973, pp. 5–6.
47. See *High-Speed Frontier*, p. 29.

Chapter 3

The Variable-Density Wind Tunnel

1. I am following Donald D. Baals and William R. Corliss, *The Wind Tunnels of NASA*, NASA SP-440 (Washington, 1981), pp. 2–3. On the 1871 tunnel, see also J. Laurence Pritchard, "The Dawn of Aerodynamics," *Journal of the Royal Aeronautical Society* 61 (Mar. 1957): 159–60, and Kenneth Goin, "The History, Evolution and Use of Wind Tunnels," *AIAA Student Journal*, Feb. 1971, p. 4.
2. Theodore von Kármán, *Aerodynamics: Selected Topics in the Light of Their Historical Development* (Ithaca, New York, 1954), pp. 9–10.
3. Tom D. Crouch, *A Dream of Wings*, pp. 246–47.
4. N. H. Randers Pherson, *Pioneer Wind Tunnels*, Smithsonian Publication 3294 (Washington, 1935). See also Pritchard, "Dawn of Aerodynamics," p. 176.
5. Albert F. Zahm detailed Eiffel's tunnels in his "Eiffel's Aerodynamic Laboratory and Studies," *Aero Club of America Bulletin*, Aug. 1912 (reprinted in Zahm's *Aeronautical Papers*, 1:239–44).
6. See L. A. Gilgore, "Wind Tunnel Drives," *Washington Engineer* 2 (Mar. 1954): 79–86.
7. Theodore von Kármán, *Aerodynamics*, pp. 73–82.

8. Laurence K. Loftin, Jr., interview with author, Hampton, Va., 12 June 1984, and telephone conversation, John V. Becker, Newport News, Va., with author, 18 Oct. 1984.
9. Interviews with Max M. Munk, Ocean City, Md., 1–4 Apr. 1982. (Munk's permanent residence was in Rehoboth Beach, Del.; he was staying at the time in a mobile home next door to his closest relatives in America, recuperating from eye surgery.) Munk would not permit me to tape-record our conversations, but I wrote detailed notes following each day's interview. On 20 Aug. 1985, I visited Munk again, this time at his home in Reboboth Beach, bringing with me Dr. Feri Farassat, a NASA Langley engineer, and Prof. Mark Levinson of the University of Maine. At this time, Munk graciously donated nearly his entire collection of technical books to the LHA.

   Also, Frederick Norton to George W. Lewis, 30 Apr. 1921, AV400-1 ("Sections, Langley Low-Turbulence Tunnel, General Correspondence"), LCF. Note that the correspondence of the VDT section was eventually mixed with the correspondence of its successor, the Two-Dimensional Low Turbulence Tunnel section.
10. Max M. Munk, "My Early Aerodynamic Research—Thoughts and Memories," *Annual Review of Fluid Mechanics* 13 (1981): 1–2. The NACA translated Munk's dissertation in 1921 as TR 121, "The Minimum Induced Drag of Aerofoils." Munk described the tunnel ideas he had while at Zeppelin in "On a New Type of Wind Tunnel," NACA TN 60, June 1921.
11. Quoted from Paul Hanle's *Bringing Aerodynamics to America,* (Cambridge, Mass., 1981), p. 155. A copy of this letter, dated 2 July 1924, is in AV400-1, LCF.
12. Hunsaker, "Europe's Facilities for Aeronautical Research," *Flying* (May 1914), p. 108. This was a popularized digest of the original which appeared in the *Transactions of the American Society of Mechanical Engineers* in 1914. On the Göttingen group under Prandtl, see Hanle's *Bringing Aerodynamics to America.*
13. This uncle was Adolph Lewisohn, who came to America from Hamburg ca. 1868. Lewisohn became president of the Tennessee Corp., Miami Copper Co., and South American Gold and Platinum Co., as well as president of the Hebrew Shelter Guardian Society and director of the Mt. Sinai Hospital in New York City.
14. Munk interview with author, 2 Apr. 1982. This version of the NACA's decision to hire Munk seems to be uncorroborated by written records.
15. See James McGovern, *Crossbow and Overcast* (New York, 1964), and Clarence G. Lasby, "German Scientists in America: Their Importation, Exploitation, and Assimilation, 1945–1952," unpublished Ph.D. dissertation, University of California at Los Angeles, 1962.
16. "Proceedings, Twentieth Wilbur Wright Memorial Lecture and Conversazione," *The Royal Aeronautical Society* 36 (Dec. 1932): 995–96. In this lecture, Wimperis cited a speech made in 1931 by Ernest F. Relf, superintendent of the NPL's aerodynamics department, to the British Association for the Advancement of Science, in which Relf attributed the idea of using a compressed fluid to Margoulis. Walter Diehl commented on the British claim in a letter to George Lewis, 25 June 1932, NACA research authorization file 70, "Standardization of wind tunnels." Diehl had read a preprint of Wimperis's Wilbur Wright lecture.

    In 1933 the French government would authorize the construction of Margoulis's design for a "reduced density" supersonic (Mach 2.7) tunnel at the University of Paris. See *Journées scientifiques et techniques de mécanique des fluides tenues à Lille en 1934* (Paris: Chiron, 1934).

17. The quotations are from John D. Anderson, *Introduction to Flight: Its Engineering and History* (New York, 1978), pp. 6–31 and 196–98.
18. Walter G. Vincenti, "The Air-Propeller Tests of W. F. Durand and E. P. Lesley: A Case Study in Technological Methodology," *Technology and Culture* 20 (1979): 743–44.
19. In building a 160-KPH-plus, all metal, smooth-skinned, midwing monoplane with internally braced cantilever wings in 1915, Hugo Junkers had set off an international chain reaction of innovation in aircraft structures. Boldly challenging the myth of the thin section, the design simplified wing construction and assembly, eliminated the resistance of the interplane bracing, and raised the maximum lift. Then, Anthony Fokker had built the era's most highly advanced fighters with thick, cantilever wings—the "Dr. I" (Dr. for "Dreidecker," or triplane) and the D-VII (the only aircraft specifically cited by the Treaty of Versailles as war materiel to be handed over to the Allies by the defeated Germans). Though the internally braced thick section dated back to an Antoinette monoplane flown in France about 1910, American recognition of the need to explore this type of wing came only after the tremendous success of the German warplanes. See Frederick H. Norton, "The Aerodynamic Properties of Thick Aerofoils Suitable for Internal Bracing," TR 75, 1919. For further discussion and analysis of the Junkers and Fokker designs, see A. R. Weyl, *Fokker: The Creative Years* (London, 1965); John H. Morrow, Jr., *German Airpower in World War I* (Lincoln, Nebr., 1982); and Laurence K. Loftin, Jr., *Quest for Performance: The Evolution of Modern Aircraft,* NASA SP-468 (Washington, 1985), pp. 22, 38, 43, and 47.
20. Lt. Col. Edgar S. Gorrell and Maj. H. S. Martin (Signal Corps, USA), "Aerofoils and Aerofoil Structural Combination," TR 18, 1919. The experimental series derived from "Durand 13," an airfoil section designed by William F. Durand at Stanford University.
21. *AR 1919*, p. 15. On the early years of the NACA's Paris office, see correspondence in 57 A 415, Box 66, National Archives.
22. "Aerodynamic Characteristics of Airfoils," TR 93, 1919.
23. For expert technical discussion of these developments in airfoil theory, see von Kármán, *Aerodynamics*, pp. 44–46 and 50–54, as well as relevant sections in Ira H. Abbott and Albert E. von Doenhoff, *Wing Section Theory* (New York, 1959).
24. See Grover Loening, *Our Wings Grow Faster* (New York, 1935).
25. Mark Levinson, professor of mechanical engineering at the University of Maine, claims that from the time of the appearance of Munk's 1922 report (NACA TR 142) "we may speak of the modern era in the history of airfoil profiles," whereas "all prior work, whether theoretical, experimental, or merely cut-and-try, may be considered as belonging to the pioneer period of that history." Munk's thin-wing theory is to airfoil design, Levinson explains, what "the Euler-Bernoulli beam theory is to any of the modern, sophisticated theories of elastic rods or what lumped-parameter electric-circuit theory is to the full equations of electromagnetic field theory"—it is a theory of the "first order." Such theories are "quite adequate for the purposes of engineering design; the good engineer understands the limitations of such approximate theories and knows when *not* [Levinson's emphasis] to use them." Unpublished manuscript, "Airfoil Profiles: Eyeballing, Design, and Selection, 1880–1922" (March 1985), pp. 28–29.

26. TR 142. See R. T. Jones, "Recollections from an Earlier Period in American Aeronautics," *Annual Review of Fluid Mechanics* 9 (1977): 3–6.
27. "Model Tests with a Systematic Series of 27 Wing Sections at Full Reynolds Number," TR 221, 1925, by Munk and Elton W. Miller.
28. *AR 1925,* p. 16. See also George J. Higgins, "The Comparison of Well-Known and New Wing Sections Tested in the Variable-Density Wind Tunnel," TN 219, May 1925, and "The Characteristics of the NACA M-12 Airfoil Section," TN 243, June 1926.
29. Langley staff memo order 123, 11 Jan. 1926, E27-6, LCF.
30. Griffith to Lewis, 9 Nov. 1923, AV400-1, LCF.
31. Norton to Lewis, 9 Aug. and 6 Oct. 1921, AV400-1, LCF.
32. See, for example, Langley staff memo orders 102, 9 Sept. 1924, and 115, 15 May 1925, E27-6, LCF.
33. Staff memo order 102, 9 Sept. 1924, E27-6; Western Union telegram, 23 Sept. 1924, AV400-1, LCF.
34. See Bacon interview with Michael D. Keller, 3 Oct. 1967, copy of transcript, pp. 10–11, LHA.
35. Weick tells the story of the PRT balance design in his tape-recorded "Historical Reminiscences," tape 3, sides 1 and 2. (A copy of the transcript is in the LHA.)
36. Floyd L. Thompson interview with Walter Bonney, 27 Mar. 1973, p. 4. The author confirmed the details of this story in an interview with Weick during a reunion of former NACA employees in Williamsburg, Va., 14 Nov. 1982.
37. In *Model Research,* Roland puts forward substantial evidence (pp. 95–98) that it was a confrontation with George Lewis that prompted Munk's resignation and that there was as much or more philosophy as personality involved. Knowing Roland's interpretation, I asked Munk—during the visit of Farassat, Levinson, and myself with him on 20 August 1985—if he had disagreed significantly with Lewis over research philosophy; Munk answered very strongly that he had not, and that his trouble with Lewis was purely personal. My version of the revolt against Munk is meant to shed light on the personality differences related to Munk's resignation, not to diminish Roland's ideas about philosophy—which, notwithstanding Munk's objections, seem to have been part of the explanation.
38. Norton to George Lewis, 9 Aug. 1921, AV400-1, LCF.
39. Lewis to Joseph Ames, 2 July 1924, AV400-1, LCF.
40. Griffith to Lewis, 9 Nov. 1923, AV400-1, LCF.
41. Ames to Lewis, 19 Aug. 1926, in RA file 102, LHA.
42. Diehl to Lewis, 18 Aug. 1926, in RA 102.
43. H. J. E. Reid to George Lewis, "Comments on the article in the Dec. 1930 issue of *Aero Digest,* entitled 'Why the NACA?'," 2 Jan. 1931, AV400-1, LCF. (The *Aero Digest* article had criticized the NACA for alleged mismanagement in having lost many good researchers.) On Hemke's problems with Munk, see Weick's "Historical Reminiscences," tape 3, side 2, copy of transcript, p. 19, LHA.
44. Dorothy and Fred Weick interview with author, Williamsburg, Va., 14 Nov. 1982.
45. Darwin H. Stapleton, "Benjamin Henry Latrobe and the Transfer of Technology," in Carroll W. Pursell, Jr., *Technology in America: A History of Individuals and Ideas* (MIT Press, 1981), pp. 38–41.
46. David McCullough, *The Great Bridge* (New York, 1972), pp. 48–50.

Chapter 4

With a View to Practical Solutions

1. For a concise description of the various NACA systems for coding airfoil information, see "Summary of Airfoil Data," TR 824, 1945, printed in *AR 1945,* pp. 262–65.
2. The predominance of graphic description of airfoil characteristics in NACA reports seems to reflect the power of nonverbal thought in the engineering mind. Unlike scientists who tend to think in mathematical or verbal terms, engineers work principally from learned mechanical alphabets, models, and curves: "If the efficiency of an engine is at issue, for example, an engineer's thoughts turn instinctively to a typical performance curve, that of efficiency versus load; a structural engineer carries a family of stress versus strain curves in his head; even the abstractedly thinking electronics engineer is likely to visualize curves of wave forms." Eugene S. Ferguson, "The Mind's Eye: Nonverbal Thought in Technology," *Science,* 197 (26 Aug. 1977): 831.
3. *Model Research,* pp. 539–540.
4. Quoted by George W. Gray, *Frontiers of Flight: The Story of NACA Research* (New York: Alfred A. Knopf, 1948), p. 16.
5. Norton to Lewis, 30 Apr. 1921, AV400-1, LCF.
6. For a "Report of fire in the VDT," see H. J. E. Reid to NACA, 8 Aug. 1927, AV400-1; Jacobs to Engineer-in-Charge, 9 Sept. 1932, AV400-1.
7. Correspondence on the design and construction of the FST in AS286-1, LCF; for a brief account of the FST, see Baals and Corliss, *Wind Tunnels of NASA,* pp. 22–24.
8. Smith J. DeFrance to Elton W. Miller, "Effect of Turbulence on C_L Max," 25 Nov. 1932, AV400-1.
9. Abe Silverstein, "Scale Effects on Clark Y Airfoil Characteristics from NACA Full-Scale Wind Tunnel Tests," TR 502, 1934. See also Ira H. Abbott, "Airfoils: Significance and Early Development," in *The Evolution of Aircraft Wing Design: Symposium* (American Institute of Aeronautics and Astronautics, 1980), p. 23.
10. Eastman N. Jacobs and Ira H. Abbott, "The NACA Variable-Density Wind Tunnel," TR 416, 1932; LMAL staff memorandum report, "Specifications for Airplanes and Airplane Models for Testing in the Full-Scale Wind Tunnel," 10 Feb. 1939, AS286-1, LCF.
11. John Stack, "Tests in the Variable-Density Wind Tunnel to Investigate the Effects of Scale and Turbulence on Airfoil Characteristics," TN 364, Feb. 1931; Eastman N. Jacobs and Albert Sherman, "Wing Characteristics as Affected by Protuberances of Short Span," TR 449, 1933; Albert E. von Doenhoff, "A Preliminary Investigation of Boundary-Layer Transition along a Flat Plate with Adverse Pressure Gradient," TN 639, Mar. 1938.
12. From TR 530, "Characteristics of the N.A.C.A. 23012 Airfoil from Tests in the Full-Scale and Variable-Density Tunnels," by Jacobs and William C. Clay, 1935. Jacobs first introduced the concept of effective Reynolds number in a paper he read to the national meeting of the American Society of Mechanical Engineers (ASME) at the University of California-Berkeley—Jacobs's alma mater—in June 1934.
13. See Jacobs and Sherman, "Airfoil Section Characteristics as Affected by Variations of the Reynolds Number," TR 586, 1937; and Robert M. Pinkerton, "The Variation with Reynolds Number of Pressure Distribution over an Airfoil Section," TR 613, 1938.

14. Jacobs to Engineer-in-Charge, "New Variable-Density Tunnel," 26 Apr. 1935, A206-1, LCF.
15. DeFrance to Chief, Aerodynamics Div., "Mr. Jacobs' Memorandum on Proposed New Variable-Density Tunnel," 4 May 1935, A206-1, LCF.
16. Theodorsen to Engineer-in-Charge, "Comments on Mr. Jacobs' Memorandum Regarding New Variable-Density Wind Tunnel," 4 May 1935, A206-1, LCF.
17. On the theory of oscillating airfoils, see I. E. Garrick and W. H. Reed III, "Historical Development of Aircraft Flutter," *Journal of Aircraft* 18 (Nov. 1981): 897–912; Theodorsen's "Theory of Wing Sections of Arbitrary Shape" appeared as TR 411.
18. TR 411, printed in *AR 1932*, p. 29. See also Theodorsen to engineer-in-charge, "Request from the New York Shipbuilding Company for Information on Airfoil Sections for Use as Blade Sections of a Marine Screw Propeller," 23 Sept. 1932, R1600-1, LCF. Theodorsen believed that the American system of engineering education did not put enough emphasis on mathematical training, and that this caused a cleavage between the country's engineers and mathematicians. From the mid-1930s, he served as an adviser to a Brown University program studying the transfer of elements of the European system of engineering education, including its system of more rigorous theoretical training, to American schools. I. E. Garrick interview with author, Hampton, Va., 26 Sept. 1981.
19. See, for example, Robert R. Gilruth interview with Michael D. Keller, Hampton, Va., 26 June 1967, copy of transcript, pp. 17–21, LHA. One of Jacobs's greatest adventures involved his own Pitcairn biplane. In 1933, as he tells it, he had his small airplane in Norfolk when a severe storm passed along the Virginia coast. He tied the plane down as best as he could against a grove of trees and waited a few hours until the wind had died down. Then he flew back to Hampton. After he landed, the wind became so strong again that it blew the roof off his hangar. Jacobs had flown back across Hampton Roads in the eye of a hurricane, the biggest one to hit the area yet in this century. Telephone interview, Eastman Jacobs with author, 27 Aug. 1983.
20. Jacobs to Engineer-in-Charge, 19 Nov. 1930, in RA 88.
21. Jacobs, memorandum for LMAL files, "Notes on the History of the Development of the Laminar-Flow Airfoils and on the Range of Shapes Included," 27 Dec. 1938, A173-1, LCF.
22. H. J. E. Reid to George Lewis, "Proposed New Variable-Density Tunnel," 7 May 1935, A206-1, LCF.
23. See Baals and Corliss, *Wind Tunnels of NASA,* p. 39.
24. Jacobs to Engineer-in-Charge, "Trip to Europe," 11 Nov. 1935, E32-12, LCF.
25. Ibid.; see also Geoffrey I. Taylor, "Statistical Theory Of Turbulence," *Proceedings of the Royal Society of London,* series A, 151 (1935): 421–78, also 156 (1936): 307–17.
26. Jacobs to Engineer-in-Charge, "Technical Description of the Equipment and Work Observed in European Laboratories," 11 Nov. 1935, E32-12, LCF. B. Melville Jones later announced his conclusion in his lecture before the Institute of the Aeronautical Sciences (IAS) in Washington, D.C., in Dec. 1939, and in his paper "Flight Experiments in the Boundary-Layer," *Journal of Aeronautical Sciences* (Jan. 1938): 81–94.
27. "Improvement of Airfoil Sections and Wing-Fuselage Combinations (Interference)," part III of notes to afternoon session #2, "Aerodynamic Efficiency and Interference."

Langley transmitted a record of the substance of Jacobs's presentation at the May 1936 manufacturers' conference in its memorandum, "Notes on Simultaneous Afternoon Conferences, May 22, 1936," E6-1, LCF.

28. Jacobs to Chief, Aerodynamics Div., 20 July 1936, AV400-1, LCF.
29. This paper by Jacobs was published in the *Journal of the Society of Automotive Engineers* (SAE), Mar. 1937.
30. The first research authorization to involve Langley in icing research was RA 247, "Ice Formation on Aircraft," requested by Rear Adm. William A. Moffett, chief, BuAer, in 1928. To study this problem, the NACA constructed a 6-inch tunnel having special refrigeration equipment; it was, according to *AR 1928* (p. 6), the "first icing research tunnel" in the world. Throughout the 1930s and early 1940s, under the guidance of Theodorsen, William C. Clay, and finally Lewis A. Rodert, the NACA explored various ways to prevent and remove ice formation on aircraft. In 1946 Rodert won the Collier Trophy for developing a thermal ice-prevention system. See Robert McLarren, "NACA Research Ends Ice Hazard," *Aviation Week* 47 (22 Dec. 1947): 24–27, and George W. Gray, *Frontiers of Flight,* chap. 14, "Heat Against Ice."
31. In 1941 the NACA transferred Rodert's team and the entire icing research program to its new Ames Aeronautical Laboratory at Moffett Field, Calif., south of San Francisco. The main reason for the transfer was that the interaction of mountain ranges and ocean air along the northern California coast provided icing weather much more certainly than did the skies of Tidewater Virginia. The continued progress of the test program at Ames throughout World War II demonstrated that properly heated wings could keep ice off while keeping the internal structure intact. According to Rodert, "This was the beginning which gave us assurance that eventually airlines would fly from Omaha to Chicago to New York and any other place, irrespective of ice clouds." See Gray, *Frontiers of Flight,* pp. 311–312 and 325.
32. Jacobs admitted during our telephone conversation of 27 Aug. 1983 that he "got an idea" from Theodorsen's paper. Yet in his "Notes on the History of the Development of the Laminar-Flow Airfoils," Jacobs did not allude in any way to the importance of his rereading of Theodorsen's paper.
33. John Stack and Albert E. von Doenhoff, "Tests of 16 Related Airfoils at High Speeds," TR 492, 1934.
34. Eastman N. Jacobs, Robert M. Pinkerton, and Harry Greenberg, "Tests of Related Forward-Camber Airfoils in the Variable-Density Wind Tunnel," TR 610, 1937; Ira H. Abbott, "Airfoils," in *The Evolution of Aircraft Wing Design*, p. 24.
35. For Jones's favorable impression of Munk's contributions to aerodynamics, consult the transcript of his interview with Walter Bonney, Moffett Field, Calif., 24 Sept. 1974, pp. 1–3. In 1979 NASA published a book by Jones (RP-1050) in which Jones (then senior staff scientist at NASA Ames Research Center) collected, under the title *Classical Aerodynamic Theory,* fourteen papers. Four of the papers were by Munk; two by A. F. Zahm; two by Theodore Theodorsen (one of which was coauthored by I. E. Garrick); and one each by H. Bateman, A. Betz, Otto Blumenthal, Theodore von Kármán (coauthor, with H. Rubach), Ludwig Prandtl, and E. Trefftz.
36. R. T. Jones, "Recollections," *Annual Review of Fluid Mechanics* 9 (1977): 10–11.
37. See H. Julian Allen, "A Simplified Method for the Calculation of Airfoil Pressure Distribution," TN 708, 1939.
38. Abbott, "Airfoils," pp. 23–24.

39. Robert Pinkerton responded to Theodorsen's challenge in "The Variation with Reynolds Number of Pressure Distribution over an Airfoil Section," TR 613, 1938.
40. Abbott, "Airfoils," p. 24; for Theodorsen's contribution, see NACA Adv. Restr. Rpt. L4G05, "Airfoil Contour Modifications," 1944.
41. Telephone interview, Jacobs with author, 27 Aug. 1983.
42. Jacobs, "Notes on the History of the Development of the Laminar-Flow Airfoils," 27 Dec. 1938, A173-1, LCF.
43. Jacobs, "Preliminary Report on Laminar-Flow Airfoils and New Methods Adopted for Airfoil and Boundary-Layer Investigations," Adv. Conf. Rpt., June 1939 (later published as Wartime Rpt. L-345).
44. After the Two-Dimensional Low-Turbulence Pressure Tunnel was put into operation in the spring of 1941, Langley researchers undertook a systematic study of the 63-, 64-, 65-, and 66-series sections. Working 48 hours a week each in three daily shifts, the men of Jacobs's section ran these tests at Reynolds numbers of 3, 6, and 9 million, with smooth surfaces and with a standard carborundum roughness on the leading edge. Though results made it clear that ideal laminar-flow airfoils were practically impossible to achieve, Jacobs would not let this information be published. Only after Jacobs resigned from Langley in 1944 did the NACA finally publish a report stating this conclusion: Laurence K. Loftin, Jr., "Effects of Specific Types of Surface Roughness on Boundary-Layer Transition," Adv. Conf. Rpt. L5J29a, 1946.
45. *Frontiers of Flight,* p. 107. On the effect of surface conditions on airfoil drag characteristics, see Ira H. Abbott and Albert E. von Doenhoff, *Wing Section Theory, Including a Summary of Airfoil Data* (New York, 1959), pp. 142–49.
46. *Model Research,* p. 132.
47. Ibid.
48. The title of the Munk article rejected by the NACA was "Influence of Obstacles on the Lift of Airfoils." Munk made his grandiose claims in letters to the Gibbs and Cox Co., 11 Apr. 1930, and to Sen. Hiram Bingham, a man very influential in aviation politics, 7 Apr. 1930; both in 57 A 415 (Box 73), National Archives. Roland quotes more of the letter to Bingham in *Model Research,* p. 132.
49. The following editorials critical of the NACA appeared in *Aero Digest* between 1930 and 1933: "Why the NACA?" (Dec. 1930), 47ff.; "Take Politics Out of Research," (Mar. 1932), 33ff.; "Perhaps Farewell, Lewis and Victory," (Jan. 1933), 25ff. All of these articles appeared in the regular column "Air—Hot and Otherwise," by editor Frank Tichenor, an outspoken and emotional advocate of a separate air force. George Lewis believed that the information concerning Langley in these articles by Tichenor had to have been prepared by Munk: "From our records, Mr. Tichenor has never visited the Committee's laboratories at Langley Field, ... so that I know he is not personally well acquainted with our activities." Letter to William H. Miller, 12 Dec. 1930, 57 A 415 (Box 14), National Archives. See *Model Research,* pp. 130–133.
50. In his letter to Ames, Munk wrote: "Let not unsacred shadows of the past interfere with your deliberations, which shadows, when really reduced to their origin, fade away and vanish into nothing. Mistakes and wrongs are unavoidable by reason of the inherent frailty of human nature. Even crimes have their statutes of limitation." Letter to Ames, 5 July 1939, 62 A 174 (Box 9), National Archives. All the references to correspondence that follow in the chapter belong to this accession.
51. Hunsaker to Munk, 12 July 1939; Lewis to Munk, 28 July 1939.

52. Munk closed his letter with a heavy-handed attempt at reconciliation with Lewis:

    > When I reflect on how much suffering in blood and life that tunnel immediately and in the future will save to the Country, how much benefit and blessing will flow from it, not to mention even the credit it will bestow on the agency that builds it, then all notions standing perhaps in the way fade for me into insignificance.
    >
    > It becomes then increasingly clear to me that you are or should be the proper and desirable sponsor of that laudable project, being best suited to carry it through without splitting the ranks of aeronautical science .... The Country needs a NACA with the largest tunnel. You want it yourself. Cooperate with me, and become my sponsor with respect to this matter. You can rest sure that I will do what I can to make you happier than you were before.

    Munk to Lewis, 2 and 3 Aug. 1939.
53. Lewis sent Munk's proposal to Langley after Edward P. Warner, influential member of the NACA Aerodynamics Committee and distinguished adviser to the Civil Aeronautics Authority, got news of the new tunnel concept from Munk and sent a letter to Lewis asking: "Would it not be desirable for someone to talk further with Dr. Munk and see if he could secure from him either more information or a definite proposal which could be considered at Langley Field?" Warner to Lewis, 5 Jan. 1940.
54. DeFrance to Lewis, 16 Jan. 1940.
55. Munk proposed to deliver three items under a contract valued at $20,000: first, a report elaborating the proposed tunnel's novel aerodynamic principles; second, a report containing complete descriptions, specifications, and drawings of the experimental setup, together with a list of tests proposed; and third, a complete set of plans and specifications for the final design. Munk, who had trained himself in patent law, then closed his proposal with three carefully composed clauses involving proprietary rights; they were meant to prevent the NACA from canceling the final and best-paying ($10,000) item after having learned his tunnel's major design features. Munk to NACA, 20 Apr. 1940.

    In a second critique of Munk's proposal, LMAL engineer DeFrance wrote to Lewis: "I will not say that we know all of the aerodynamic principles involved in wind-tunnel design, but it is believed that the experience ... at Langley Field in the design and operation of wind tunnels is greater than that ... in any other place in the world." DeFrance recommended that the Committee not contract with Munk "on the basis of information supplied so far," and then he especially advised it not to obtain any additional information from Munk because of the patent rights referred to in his proposal, to avoid possible litigation. DeFrance to engineer-in-charge, 2 May 1940.

    It should be noted that there usually was heated debate inside Langley over what new type of wind tunnel was most needed at a specific moment in time, and that DeFrance had strongly opposed Jacobs's idea for a new VDT in 1935.
56. Bush to Munk, 1 July 1940.

57. *Congruence Surds and Fermat's Last Theorem* (New York: Vantage Press, 1977).

One of the most memorable works left behind by the French lawyer Pierre de Fermat (1608–1665) was his copy of Claude Bachet de Meziriac's 1621 translation of Diophantus of Alexandria's *Arithmetica*. In the margin of one page of this ancient book, which dealt with algebra, Fermat wrote:

> It is impossible to write a cube as the sum of two cubes, a fourth power as the sum of two fourth powers and in general any power beyond the second power as the sum of two similar powers. For this I have discovered a truly marvelous proof, but the margin is too small to contain it.

From the end of the seventeenth century, this mysterious handwritten notation has been known as Fermat's Last Theorem.

More than 200 years later, in the early twentieth century, a learned citizen of the German town of Darmstadt bequeathed a small fortune (100,000 gold marks) to anyone who might solve Fermat's problem. So long as a correct solution did not come to light, the trustees of the bequest were entitled to devote the interest on the fund to any object they chose: the trustees chose to use the money to hold annual guest lectures at Göttingen. Among the prominent mathematicians and physicists who came to speak at the university on these occasions were Henri Poincaré, Max Planck, and Niels Bohr. As a sideshow, really—since Fermat's Last Theorem had no practical application—each tried his hand at solving the problem. Although many of them were elegant, all the attempts failed. (This delighted the trustees at Göttingen, because failure meant funding for the lecture series would continue.)

While at Göttingen as a doctoral student, Munk attended these lectures and made his own private attempts at solving the perennial problem. Sixty years later, Prof. Gabriel Bohler, Munk's former student at Catholic University, sent Munk an article from *Science* (vol. 178, 6 Oct. 1972) entitled "Fermat's Mathematics: Proofs and Conjectures," by Michael S. Mahoney, a historian of science at Princeton University. Mahoney concluded that Fermat's "proof" was probably no proof at all, "because Fermat could not be bothered with detailed demonstrations of theorems his superb mathematical intuition told him were true" (p. 35). Munk apparently misread Mahoney's conclusion to mean that the Last Theorem was fundamentally incapable of proof (which it may be). This misreading upset Munk so much that he began writing, as a rejoinder to Mahoney's article, *Congruence Surds and Fermat's Last Theorem.*

After visiting Munk with me on 20 Aug. 1985, Dr. Feri Farassat, a NASA Langley aeroacoustics specialist who is familiar with number theory, looked very carefully at the alleged proof published by Munk. What Mahoney says about Fermat, Farassat repeats about Munk: he states that Munk's proof "is no proof at all," since all Munk has done is to replace Fermat's conjecture with some vague and imprecise conjectures of his own. At most, then, what Munk believes intuitively to be the "proof" is very close to the same mysterious thing that Fermat thought he had grasped through an intuitive process over 320 years ago.

Chapter 5
The Cowling Story: Experimental Impasse and Beyond

1. Fred E. Weick, "Historical Reminiscences," copy of transcript, tape 3, side 2, p. 23, in LHA; see also Herschel Smith, *Aircraft Piston Engines* (New York, 1981), pp. 97–113.
2. 61 A 195 (Box 24), National Archives.
3. Research authorization (RA) 172, "Effect of Various Forms of Cowling on Performance and Engine Operation of Air-Cooled Pursuit Airplane," approved by the Executive Committee, 30 June 1926; RA 215, "Effect of Cooling and Fuselage Shape on the Resistance and Cooling Characteristics of Air-Cooled Engines," approved 22 June 1927.

   The navy lent the Apache aircraft to NACA Langley in the summer of 1926, but soon recalled it. Though the recall forced the laboratory to suspend cowling work on the Apache and its Whirlwind engine, RA 172 was kept open until 1932. Langley carried out most of its later cowling tests under RA 215, however.

   For the design details of the Propeller Research Tunnel, see Fred E. Weick and Donald H. Wood, "The Twenty-Foot Propeller Research Tunnel of the National Advisory Committee for Aeronautics," TR 300, 1928. For their history, see Weick's "Historical Reminiscences," tape 3, side 1, pp. 13–22, and side 2, pp. 25 and 27–28.
4. The National Aeronautic Association had awarded the Robert J. Collier Trophy annually "for the greatest achievement in aviation in America, the value of which has been thoroughly demonstrated by actual use during the preceding year," since 1911. In that year Collier, a wealthy sportsman (and son of the successful publisher and editor, P. F. Collier, who founded and edited *Collier's Weekly)*, became the president of the Aero Club of America. The first winner of the Collier Trophy was Glenn H. Curtiss for developing the "hydroaeroplane." Other Collier winners before 1929 included: Orville Wright, for developing the automatic stabilizer (1913); Elmer A. Sperry, for gyroscopic control (1914) and the drift indicator (1916); Grover Loening, for the aerial yacht (1921); personnel of the U.S. Air Mail Service, for night flying (1923); the U.S. Army, for its round-the-world flight (1924); S. Albert Reed, for developing the metal propeller (1925); and Charles W. Lawrance, for his radial air-cooled engine (1928). By the late 1920s the Collier Trophy was recognized as the most prized of all aeronautical honors to be accorded in the United States; the winner received his award from the president of the United States. See Frederick J. Neely, "The Robert J. Collier Trophy: Its Origin and Purpose," *Pegasus* (Dec. 1950): 1–16.
5. Walter G. Vincenti, "The Air-Propeller Tests of W. F. Durand and E. P. Lesley: A Case Study in Technological Methodology," *Technology and Culture* 20 (1979): 743–44.
6. Barton C. Hacker, "Greek Catapults and Catapult Technology: Science, Technology, and War in the Ancient World," *Technology and Culture* 9 (1968): 34–50.
7. For references, see Vincenti, "Air-Propeller Tests," pp. 714–15.
8. See M. W. McFarland, ed., *The Papers of Wilbur and Orville Wright* (New York, 1953), 1:547–93.
9. "Even when an adequate theory of some sort is available, experimental parameter variation may still be employed," Vincenti elaborated in his 1979 article (n. 5, above), "because of lack of numerical data on the physical properties of the substances

involved or insurmountable difficulties of one kind or another in carrying out the theoretical calculations." (Vincenti noted that high-speed electronic computers have mitigated this problem in recent years.) "Whether theory is or is not available, the case for the use of experimental parameter variation often boils down in the end to the very basic one that it provides usable results in an acceptable time, whereas waiting for theoretical understanding or guidance may involve indefinite delay." "Air-Propeller Tests," p. 745.

10. Ibid., p. 740.
11. TN 235, "Propeller Design: Practical Application of the Blade Element Theory," and TN 236, "Extension of Test Data on a Family of Model Propellers by Means of the Modified Blade Element Theory," both May 1926. Weick quoted in Vincenti, "Air-Propeller Tests," pp. 738–39 n. 82. See also Weick's "Historical Reminiscences," copy of transcript, tape 2, side 2, pp. 3–6, and tape 3, side 2, pp. 23–25.
12. Weick's "Historical Reminiscences," tape 4, side 2, pp. 20–25. Much of my analysis of the four stages of Langley's cowling work that follows in this chapter is based on Weick's autobiographical account. See also Weick, "The N.A.C.A. Cowling," *Aviation* 25 (17 Nov. 1928): 1556–57 and 1586–90, and William H. McAvoy, "Notes on the Design of the N.A.C.A. Cowling," *Aviation* 27 (21 Sept. 1929): 636–38.
13. "Historical Reminiscences," tape 4, side 2, p. 22.
14. See also Weick, "Drag and Cooling with Various Forms of Cowling for a 'Whirlwind' Radial Air-Cooled Engine, I," TR 313, 1929, and "II," TR 314, 1929.
15. Regarding the NACA's public announcement of the cowling, see George W. Lewis, "Cowling and Cooling of Radial Air-Cooled Engines," transcript of speech before the Society of Automotive Engineers, Detroit, 10 Apr. 1929, 61 A 195 (Box 25), National Archives.
16. Thomas Carroll, "Flight Tests of No. 10 Cowling," in E. P. Warner and S. Paul Johnston, *Aviation Handbook* (New York, 1931), p. 145; also Weick, "Historical Reminiscences," tape 4, side 2, p. 24.
17. 6 Feb. 1929, A176-11, LCF.
18. *AR 1930,* pp. 2–3. After reading an earlier draft of my cowling story, Richard K. Smith, aviation historian and the Verville Fellow at the National Air and Space Museum for 1984–85, commented: "I don't know if they [the NACA researchers] selected the Lockheed airplane for the publicity test—or if they simply lucked out. But they chose the right airplane! If they had selected a Bellanca or Stinson of 1929 vintage they would *not* have obtained the same happy results." Smith's point is very important—the effectiveness of the NACA cowl did depend significantly upon the *shape* of the airplane behind it.
19. William H. McAvoy, Oscar W. Schey, and Alfred W. Young, "The Effect on Airplane Performance of the Factors That Must Be Considered in Applying Low-Drag Cowling to Radial Engines," TR 414, 1932.
20. Weick, "Historical Reminiscences," tape 5, side 1, p. 3.
21. Donald H. Wood, "Tests of Nacelle-Propeller Combinations in Various Positions with References to Wings, I—Thick Wing—NACA Cowled Nacelle—Tractor Propeller," TR 436, 1932.
22. Elton W. Miller to Engineer-in-Charge, 19 Dec. 1930, A176-11, LCF.
23. As quoted in Roland, *Model Research,* p. 105.
24. Frank Tichenor, "Air—Hot and Otherwise," *Aero Digest*, Feb. 1931, p. 24. In the beginning neither the NPL nor the NACA was aware of the other's cowling work.

The NPL published the results of its ring research just before the NACA's cowling reports appeared. To impress American manufacturers with the value of its cowling, the NACA placed its design into competition with the Townend ring. George Lewis told Glenn L. Martin, for example, that Martin's B-10 bomber would not only fly significantly faster than its present maximum speed of 195 miles per hour, but would also land slower and more safely, if the engine's Townend ring were replaced by the NACA no. 10 cowl. Pratt and Whitney, the builder of the engine for the airplane, was contractually committed to using the ring. Martin eventually adopted the NACA cowling for the B-10, increasing the airplane's maximum speed by 30 MPH to 225 and also reducing its landing speed significantly. In 1933 and 1934, the army purchased more than 100 B-10s, rescuing Martin from the worst of the Depression. What the cowling did for the B-10's performance may well have been why Martin won the production contract and why Boeing's B-9, in competition with the Martin aircraft, lost. The B-9 used the Townend ring. See Lloyd S. Jones, *U.S. Bombers, 1928 to 1980s*, 3d ed. (Falbrook, Calif., 1981), pp. 30–32. The overall competitive situation fed the fire of the transatlantic dispute and resulted in a long series of patent suits. For a full discussion of the NACA cowling–Townend ring dispute and an interpretation of its effect on NACA history, see *Model Research*, pp. 116–117. For Langley's reaction to and role in the patent dispute, see Elton W. Miller to Engineer-in-Charge, "Criticism of Committee's Attitude with Reference to Townend Ring Cowling," 3 Mar. 1931, A176-11, LCF; George W. Lewis to LMAL, "NACA Cowling and Claim of Townend Patent," 12 Aug. 1931, ibid.; "Report of Meeting between Representatives of NACA and of the Army and Navy to Discuss the Cowling Patent Situation," 21 June 1932, ibid.

25. Becker, *High-Speed Frontier*, pp. 140–41.
26. *AR 1933*, p. 10; Arnold E. Biermann and Benjamin Pinkel, "Heat Transfer from Finned Metal Cylinders in an Air Stream," TR 488, 1934; Donald H. Wood, "Tests of Nacelle-Propeller Combinations in Various Positions with Reference to Wings, II—Thick Wing—Various Radial-Engine Cowlings—Tractor Propeller," TR 436, 1932; ibid., "III—Clark Y Wing—Various Radial-Engine Cowlings—Tractor Propeller," TR 462, 1933; James G. McHugh, ibid., "IV—Thick Wings—Various Radial-Engine Cowlings—Tandem Propellers," TR 505, 1934; E. Floyd Valentine, ibid., "V—Clark Y Biplane Cellule—NACA Cowled Nacelle—Tractor Propeller," TR 506, 1934; Donald H. Wood and Carlton Bioletti, ibid., "VI—Wings and Nacelles with Pusher Propeller," TR 507, 1934.
27. To direct cooling air around the hot engine cylinders, LMAL engineers had tried a number of different deflectors. One of the conceptually more refined ones tried in the early 1930s was the loosely fitting "shell" baffle. (At about this same time Pratt and Whitney and Vought were finding this type of baffle inferior to the tightly fitting pressure baffle.) Though LMAL tested the double-row R-1830 engine installation with a pressure baffle system in the Full-Scale Tunnel in 1934 (TR 550, "Cooling Characteristics of a 2-Row Radial Engine," 1935, by Oscar W. Schey and Vernon G. Rollin), thorough investigation came only in 1936 (TN 630, "Energy Loss, Velocity Distribution, and Temperature Distribution for a Baffled Cylinder Model," 1937, by Maurice J. Brevoort). For analysis of the comparative value of the two types of baffles, see *High-Speed Frontier*, pp. 141–43.
28. Rex Beisel, "The Cowling and Cooling of Radial Air-Cooled Engines," *SAE Journal* 34 (May 1934): 159.

29. Theodorsen, "Theory of Wing Sections," TR 411, printed in *AR 1932,* p. 29.
30. Theodorsen to Engineer-in-Charge, 28 June 1935, R1600-1, LCF; telephone interview, James G. McHugh, Hampton, Va., with author, 13 June 1983.
31. Theodorsen, Maurice J. Brevoort, George Stickle, and Melvin Gough, "Full-Scale Tests of a New Type NACA Nose-Slot Cowling," TR 595, 1937; Theodorsen, Brevoort, and Stickle, "Full-Scale Tests of NACA Cowlings," TR 592, 1937, and TR 662, 1939.
32. *High-Speed Frontier,* pp. 142–43.

Chapter 6

The Challenge of Teamwork

1. *Model Research,* pp. 47–49.
2. U.S. House, Independent Offices Subcommittee, *Hearings,* 67/2, 1921, pp. 359–62.
3. *Model Research,* p. 468.
4. Ibid., p. 56.
5. In April 1921, the Subcommittee on Federal Regulation of Air Navigation recommended that the NACA coordinate "in an advisory capacity" all aeronautical activities of the government. *AR 1921,* pp. 13–15.
6. *Congressional Record,* 67/1, 1921, p. 2687.
7. For an analysis of the details of the fight over this legislation, see *Model Research,* pp. 54–64.
8. Mitchell to T. B. Mott, 22 Apr. 1920, Correspondence 1920, Box 8, William Mitchell Papers, Manuscript Div., Library of Congress, cited in Michael D. Keller, "A History of the NACA Langley Laboratory, 1917–1947," (University of Arizona Ph.D. thesis, 1968), p. 74 n. 82.
9. NACA Executive Committee minutes, 27 Jan. 1921, in "Materials Collected by Roland for NACA History," HQA.
10. U.S. House, Select Committee of Inquiry into Operations of the U.S. Air Service, *Hearings,* 1925, p. 1890. This committee was popularly known as the Lampert Committee, after its chairman, Florian Lampert (Rep., Wisc.).
11. The report of the Lampert Committee is H.R. 1652, 68/2, 14 Dec. 1925.
12. These editorials appeared as part of Tichenor's regular column, "Air—Hot and Otherwise," Dec. 1930, p. 47ff., and Mar. 1932, p. 86.
13. Executive Order 5960, 9 Dec. 1932. For correspondence giving the NACA's reaction to the proposed transfer to the Dept. of Commerce in 1925, see "Efforts to Transfer NACA from Independent Agency to other Agencies," Milton Ames Collection, Box 2, LHA.
14. "Perhaps Farewell, Lewis and Victory," *Aero Digest* 22 (Jan. 1933): 18.
15. After the appearance of "Why the NACA?" in Dec. 1930, George Lewis had "every reason to believe that the article was prepared wholly by Dr. Max Munk and was published by Mr. Tichenor." See note 49, chapter 4.
16. "Report of Special Committee on the Proposed Consolidation of the National Advisory Committee for Aeronautics with the Bureau of Standards," 15 Dec. 1932, E1-2, LCF.
17. "President's Plan for Reorganizing Bureaus Deferred," *The United States Daily,* 20 Jan. 1933; Cy Caldwell, "The Man on the Flying Trapeze," *Aero Digest* 27 (Oct. 1935): 20.

18. For an explanatory chart of the NACA publications series, see *Model Research,* pp. 553–554.
19. Roosevelt to L. P. Padgett, 12 Feb. 1915, in House Committee on Naval Affairs, H.R. 1423 to accompany H.J. Res. 413, 63/3, 19 Feb. 1915, pp. 2–3.
20. Indirect industry access to Committee positions became more frequent after 1929 when the NACA enlarged its main body from 12 to 15 members. See *Model Research,* p. 423 and pp. 427–430.
21. At the third meeting of the NACA's Subcommittee on the Federal Regulation of Air Navigation in Apr. 1921, for example, the president of the Aircraft Manufacturers' Assoc. strongly recommended that the NACA appoint a representative to the Main Committee. "Minutes," 7 Apr. 1921, pp. 7–8. See also Edward P. Warner to Joseph Ames, 2 June 1927, in "Langley Inspections," Milton Ames Collection, Box 2. Roland offers a more thorough analysis of this challenge in *Model Research,* pp. 58–60.
22. Warner to Joseph Ames, 12 Apr. 1936, A197-1, LCF; *AR 1936*, pp. 28–29.
23. *AR 1925,* p. 57. After the war, the conference, then called the "Inspection," rotated among Langley and the NACA's two new laboratories, the Ames Aeronautical Laboratory in Sunnyvale, Calif., and the Flight Propulsion (later Lewis) Laboratory in Cleveland, Ohio.
24. *High-Speed Frontier,* p. 76.
25. "Eleventh Annual Aircraft Research Conference," *Aero Digest* 28 (June 1936): 48.
26. Robert Osborne, "Sideslips," *Aviation* 38 (June 1939): 17.
27. Victory's concern for detail rubbed off on the LMAL staff. After one conference, Langley's chief clerk requested suggestions by the staff for improving the event. Nearly everyone responded, remarking on everything from automobile parking problems to the proximity of toilets to meeting rooms. One of the stenographers complained that the desk at which she recorded meetings in shorthand did not fit her, compelling her to sit "with my shoulders hunched while writing, thus retarding speed." The chief clerk noted that next year he should "have a higher chair or lower table for Miss Wheeler." Catherine Wheeler to Mr. Edward R. Sharp, 24 Apr. 1932, E6-1, LCF.
28. George Lewis to the Executive Committee, "Aerodynamic Problems Suggested in Connection with Annual Research Conference, May 22," 3 June 1935, A197-1, LCF.
29. Pearl I. Young interview with Michael D. Keller, 10 Jan. 1966, Hampton, Va., p. 32, transcript in LHA.
30. Elton W. Miller to Engineer-in-Charge, 6 Mar. 1937, E6-1, LCF.
31. Handwritten note from Jacobs to the chief clerk, 5 May 1939, ibid.
32. Lewis to LMAL, 24 Apr. 1931, A197-1, LCF.
33. Elton W. Miller to Engineer-in-Charge, "Visit of Mr. Rex Beisel to the Laboratory on June 16 and 18, 1934," 22 June 1934, E37-3, LCF.
34. Lewis to LMAL, 18 Nov. 1931, E30-12, LCF; "General Information for Laboratory Guides," 15 Aug. 1938, E37-3, LCF. Though visits of airplane designers to Langley increased after the inauguration of the annual conference in 1926, the numbers do not seem unusual. The engineer-in-charge reported in 1936, for example, that only 96 (or 3 percent) of the 3082 visitors to the lab in the previous 12 months represented American industry. This compared to 81 from foreign countries, 407 from various educational institutions, 805 casual visitors or sightseers, and 1693 from the army, navy, and other government services and departments. H. J. E. Reid to NACA, 10 Sept. 1936, ibid.

35. S. Paul Johnston interview with Walter Bonney, 19 Oct. 1971, Bozman, Md., p. 12.
36. Ira H. Abbott, "A Review and Commentary of a Thesis by Arthur L. Levine," HQA, HHN-35, 1964, p. 161.
37. *The Origins of the Turbojet Revolution,* pp. 10–11, 16, 19–20, 22.
38. George Lewis to LMAL, "Col. Clark's Comments on the Fowler Wing," 9 May 1933, A197-1, LCF. After the 1933 conference, for example, the NACA justified its rejection of a suggestion from a representative of the International Aircraft Corporation for an investigation of the directional stability and spinning tendencies of its tailless airplane by stating that the proposed research involved "a very large amount of work on a type of aircraft" that was not being developed rapidly enough to merit it. Two years later, "in view of the promise of the tailless airplane," the NACA approved a test program very similar to the one asked for by International in 1933. R. F. Anderson to chief of Aerodynamics Div., 17 Aug. 1935, ibid.
39. John D. Anderson, *Introduction to Flight: Its Engineering and History* (New York, 1978), p. 256; Michael D. Keller, "A History of the NACA Langley Laboratory," p. 195. See "Report of Proceedings of Second General Conference between Representatives of Aircraft Manufacturers and Operators and the National Advisory Committee for Aeronautics," May 1927, E6-1, LCF.
40. Frederick J. Bailey to Chief Clerk, 5 June 1939, ibid.
41. In *Model Research,* Roland suggests that the conference favored larger companies and that industry exploited the meetings by drawing Langley "further into short-term, practical research and away from the long-range fundamental research to which it was philosophically committed" (p. 113). It is true that the Committee sent out few invitations in the early years, mostly to such established concerns as Wright Aeronautical Corp., Ford Motor Co., Pitcairn Aviation Co., Goodyear-Zeppelin Co., Fairchild Aviation Corp., and Curtiss Aeroplane and Motor Co., Inc. In later years, however, the expanded size of the meeting permitted the attendance of many more firms. Moreover, a number of aviation writers attended and reported fully on the laboratory presentations in newspapers and journals. No party interested in recent aeronautical research and development could plead ignorance of current NACA work. Lewis and Victory may have treated certain individuals as special—the so-called Gold Group—but everyone who came to Langley for the meetings seems to have received the same technical information.
42. Edward R. Sharp to George Lewis, "Communications Incorrectly Addressed," 8 Dec. 1925, C54-6, LCF.
43. Engineer-in-Charge to Chief, Aero. Div., "Compressors and Exhaust of the Variable-Density Tunnel," 9 Apr. 1931, AV400-1, LCF.
44. Quoted in *AR 1922,* p. 49.
45. See Sec. of the Navy to Hon. Frederick Hale, Chairman, Committee on Appropriations, U.S. Senate, 30 Dec. 1933, and Douglas MacArthur, Acting Sec. of War, to Hale, 31 Dec. 1933, E1-2, LCF.
46. *Model Research,* pp. 138–139. In commenting on Roland's description of Victory's use of this "bouquet file," retired NACA-NASA engineer John V. Becker wrote:

> NACA fought for its life by the accepted American principle of unrestrained "advocacy"—the technique universally used by advertisers,

> lawyers, bureaucrats, and congressmen—in which there is no nicely balanced weighing of the arguments on both sides or careful screening for purity. The case is made by harassed human beings with an ax to grind, not by saints.

"Comments on Roland's 'Research by Committee'," following p. 211 of manuscript (comment edition, Apr. 1980).

47. Quoted by Charles H. Helms, the NACA's asst. dir. for research (1941–52), in a brief statement on NACA-military relations, dated 3 Aug. 1948, copy in LHA.
48. See RA file 138B, 1925, and the reports it produced, including: Richard V. Rhode, "Pressure Distribution on the Tail Surfaces of a PW-9 Pursuit Airplane in Flight," TN 337, Apr. 1930, and Richard V. Rhode and Eugene E. Lundquist, "Pressure Distribution over the Fuselage of a PW-9 Pursuit Airplane in Flight," TR 380, 1931.
49. U.S. House, Independent Offices Subcommittee, *Hearings,* 68/2, 1925, p. 396.
50. Two of these military pilots were Lt. H. M. Cronk and Lt. Edmund T. "Eddie" Allen. Allen later became chief test pilot and head of aeronautical research for the Boeing Company. See E. P. Warner and Frederick H. Norton, "Preliminary Report on Free Flight Tests," TR 70, 1919.
51. Langley's first civilian test pilot was Thomas Carroll, a World War I fighter pilot and flight instructor. For examples of this routine liaison, see: H. J. E. Reid to C.O., Army Air Corps, Langley Field, Va., 6 Apr. and 9 June 1927, regarding conditions of the flying field in the vicinity of the NACA hangars, and Lt. Col. C. C. Culver to Reid, Engineer-in-Charge, LMAL, 7 June 1927; also, LMAL to NACA, "Proposed Runways to be Installed by Army," 22 Apr. 1939, AF250-1, LCF.
52. See Diehl interview with Michael D. Keller, Hampton, Va., 12 Sept. 1967, pp. 3–5; and Floyd L. Thompson with Walter Bonney, 27 Mar. 1973, pp. 10–11.
53. McAvoy to Engineer-in-Charge, 2 July 1929, A173-5, LCF. See also Clinton H. Dearborn and Howard W. Kirschbaum, "Maneuverability Investigation of the F6C-3 Airplane with Special Flight Instruments," TR 369, 1930.
54. Thomas Carroll to Engineer-in-Charge, "Policy in Regard to Borrowed Airplanes," 8 Apr. 1929, AF250-1, LCF.
55. See Maj. Leslie MacDill, Chief Engineer, Eng. Div., McCook Field, to George W. Lewis, with army's log of Sperry Messenger airplane attached, 5 Nov. 1923, RA file 83, LHA.
56. See Leigh M. Griffith to NACA, "Tests of Messenger Airplane," 16 Mar. 1923, RA 83, with reference to earlier order from Lewis.
57. Frederick H. Norton to Griffith, "Tests of Messenger Airplane," 15 Mar. 1923, RA 83.
58. Griffith to NACA, 16 Mar. 1923, ibid.
59. David L. Bacon to Griffith, "Outline of Tests of Messenger Airplane," 24 Oct. 1923, ibid.
60. In a letter to Samuel Stratton, dir., Bureau of Standards, 18 May 1918, Joseph Ames expressed the opinion that de Bothezat knew more about the design of wind tunnels and propellers "than any other man in the world" (Record Group 255, Box 5, National Archives). By the end of the year, Ames persuaded the NACA and the Army Air Service to engage him as technical adviser to the NACA in the articulation of its research agenda and as consultant to McCook Field in designing a propeller suitable for the Liberty engine. Soon both organizations regretted the engagement. At McCook de Bothezat "never came to grips with the problems he had boasted

of being able to solve. At the same time, he gave overblown public lectures on the possibilities of using jet propulsion for interplanetary travel" (Roland, *Model Research,* pp. 90–91). In 1920 the army severed all working ties with the man. The NACA also released him from its service, hiring Max Munk as his replacement. George Lewis to Frank W. Caldwell, 28 May 1923, RG 255, Box 3, Natl. Archives.

61. F. W. Caldwell and E. N. Fales, "Wind Tunnel Studies in Aerodynamic Phenomena at High Speeds," TR 83, 1920. See *High-Speed Frontier,* pp. 4–9.
62. C. N. Monteith to George W. Lewis, 16 Aug. 1923, RA 83.
63. Lewis to LMAL, "Investigation of Six Different Sets of Wings on Sperry Messenger Airplane," 28 Aug. 1923, ibid.
64. Leigh M. Griffith to Maj. Leslie MacDill, Chief Engineer, Eng. Div., McCook Field, 5 Jan. 1924, ibid.
65. David L. Bacon to Griffith, "McCook Field Report 'Determination of Airplane Drag Characteristics in Free Flight,' ser. 2197," 12 Nov. 1923, ibid.
66. Leigh M. Griffith to NACA Exec. Off., "Sperry Messenger Glide Tests," 13 Nov. 1923, ibid.
67. George W. Lewis to LMAL, "Sperry Messenger Glide Tests," 20 Nov. 1923, ibid.; the note from Diehl is attached.
68. Ibid.
69. David L. Bacon, "Memorandum on Visit to Dayton," 8 Feb. 1924, E32-12, LCF.
70. George W. Lewis to LMAL, 5 Mar. 1925; job order 607, requested by George J. Higgins, 26 Sept. 1925, RA 83.
71. Leigh M. Griffith to NACA, 29 Apr. 1924, and Lewis to LMAL, 2 May 1924, ibid.
72. *Model Research,* p. 551.
73. Max M. Munk and Walter S. Diehl, "The Air Forces on a Model of the Sperry Messenger Airplane without Propeller," TR 225, also in *AR 1926,* p. 381ff.
74. Due either to administrative oversight or a desire to keep the RA open for reporting of later charges, the Washington office still listed RA 83 as "active" until Feb. 1929, when headquarters noticed the "error" and asked for it to be rectified. See George W. Lewis to LMAL, "Status of Research Authorizations," 8 Oct. 1929, and Edward R. Sharp to NACA, "Status of Research Authorizations," 23 Oct. 1929, RA 83.
75. *AR 1926,* pp. 387–88.
76. J. W. Crowley, Jr., and M. W. Green, "An Investigation of the Aerodynamic Characteristics of an Airplane Equipped with Several Different Sets of Wings," TR 304, also in *AR 1928,* p. 493.
77. H. H. Arnold, Maj. Gen., Chief of the Air Corps, to Mr. G. W. Lewis, NACA, 13 Mar. 1939, B10-2, LCF. Lewis then informed the NACA that

> no definite arrangements have been made with Major Greene or with the Air Corps as to how information obtained by Major Greene will be transmitted to the Air Corps. Major Greene has stated that he would like to forward from Langley Field preliminary confidential reports that are usually forwarded from this [the NACA's Washington] office. I see no objection to this procedure, but the subject matter of each report to be forwarded should be discussed with the Director of Aeronautical Research·[Lewis himself] so that a copy can be forwarded to the Bureau of Aeronautics at the same time.

Lewis to LMAL, "Detail of Maj. Carl F. Greene, of the Materiel Division, to Langley Field," 17 Mar. 1939, ibid. However, Walter Bonney, who dug deeply into NACA documents during the 1960s and early 1970s in preparing an NACA history, informed LMAL engineer Hartley Soulé during a 28 Mar. 1973 interview in Hampton, Va., that he had found clear evidence that Lewis "didn't want him [Greene] to be assigned down here" (p. 19 of transcript, in LHA). The evolving relationship between Langley and the Greene House can be followed by surveying the correspondence in B10-2, as well as by reading "The Autobiography of Mr. Jean Roché," undated manuscript (ca. 1968), a copy of which is preserved in the LHA.

78. Examination of the McCook internal correspondence might answer important questions: Did the same self-censorship (i.e., the withholding of critical opinions about the other organization) prevail in Ohio? If so, would it indicate how important the military then believed the NACA to be, for military purposes?
79. This opinion was expressed by a number of people attending the second NACA reunion at Williamsburg, Va., 14 Nov. 1982.
80. Diehl interview with Keller, 12 Sept. 1967, pp. 22–23.
81. Charles H. Zimmerman, "Conference on Stability Research," 20 Nov. 1936, RA file 204.
82. Hartley A. Soulé, "Synopsis of the History of the Langley Research Center, 1915–1939," HQA HHN-40, 1966, part 2, pp. 21–22; Robert R. Gilruth, "Requirements for Satisfactory Flying Qualities of Airplanes," Adv. Conf. Rept., Apr. 1941, superseded by TR 755, 1943.
83. Soulé, "Synopsis," part 2, p. 22; see also Gray, *Frontiers of Flight,* pp. 132–33.
84. U.S. Air Force, *Research and Development Contributions to Aviation Progress (RADCAP): Executive Summary,* Aug. 1972, available as NASA CR-129574.
85. *High-Speed Frontier,* p. 31, and Abbott, "Review and Commentary of a Thesis by Arthur L. Levine," p. 135.

Chapter 7

The Priorities of World War II

1. John Jay Ide to NACA, "Large French Wind-Tunnel at Chalais-Meudon," 29 Nov. 1935; "Inauguration of the Citta Guidonia," 21 May 1935; "Report on Visit to Germany," 23 Oct. 1936, A1000, Floyd L. Thompson Technical Library, Langley Research Center, Hampton, Va. For critical analysis of German research policies and capabilities before and during World War II, see Horst H. Boog, *Die deutsche Luftwaffenführung, 1935–1945: Fuehrungsprobleme, Spitzengliderung, Generalstabbildung* (Stuttgart, 1982), pp. 68–76. The journal *Aerospace Historian* published Boog's English summary of this book in its Sept. 1983 issue, pp. 200–206.
2. George W. Lewis, "Report on Trip to Germany and Russia, September–October 1936," E32-12, LCF. Lewis addressed the LMAL staff soon after he returned from Europe. The "principal impressions" communicated by the director of research on this occasion, as reported in Becker's *High-Speed Frontier,* p. 30,

> were of major expansions, especially in Germany. Several large new centers for aeronautical research were under construction, and Lewis was even more impressed with the huge new staff, many times larger than NACA

> and populated by a larger proportion of advanced degree holders. He had little or nothing to say, however, about new aerodynamic or propulsion concepts or any new research results.

Lewis also repeated an anecdote told to him by his escort in Germany, Dr. Adolf Bauemker, chief of the aviation research division of the Luftwaffe. When Bauemker first met Hermann Goering, Lewis related, he took with him as a conversation piece a photograph of Langley's Full-Scale Tunnel. On the spot, Goering ordered construction of the same expensive facility for Germany.

But Bauemker was misleading Lewis with this story. It was not as easy to get money for aeronautical research facilities and new experiments in dictatorial Germany as he was telling Lewis. See Adolf Bauemker, "A History of German Aeronautical Research Facilities," trans. by F. W. Pick, Royal Aircraft Establishment, *RAE Trans. No. 87,* Jan. 1946, typescript, as well as Horst Boog's book (cited in note 1 above).

3. "Report on Trip to Germany and Russia," E32-12, LCF.
4. "NACA Gets Full-Speed Wind Tunnel," NACA press release for morning paper, Thursday, 17 July 1934, AH324-1, LCF. Jacobs first suggested the idea for what became the 8-Foot High-Speed Tunnel in 1933. In a memo to the engineer-in-charge, Jacobs argued that

> for several years this laboratory has felt the need of a very fine atmospheric wind tunnel. Our only moderate-size atmospheric tunnel has had more work than it can handle and, for many proposed investigations that could be carried out most economically in this tunnel, the 7 by 10 foot tunnel is not considered altogether satisfactory, either because the air-flow turbulence and flow conditions are not satisfactory or because the speed is too low. With the object of filling this need, some preliminary designs have been studied for a so-called full-speed tunnel, a moderate-size atmospheric tunnel capable of reaching the full speed of the fastest airplanes.

Jacobs to Engineer-in-Charge, 7 Nov. 1933, AH321-1, LCF. A penciled note at the bottom of this memo, initialed "GWL" (George W. Lewis) indicates that Henry Reid took the original to NACA headquarters on the same day the memo was prepared.

5. DeFrance to Chief of Aerodynamics, "20-by-14-Foot Two-Atmosphere Wind Tunnel," AU321-70, LCF.
6. *Model Research,* p. 149.
7. Westover Committee to NACA Chairman, "Relation of the National Advisory Committee for Aeronautics to National Defense in Time of War," 19 Aug. 1938, copy in Milton Ames Collection, Box 4, LHA.
8. *AR 1938,* p. 4.
9. Lindbergh to Dr. Joseph S. Ames, 28 Nov. 1938, copy in Milton Ames Collection, Box 4.
10. NACA Executive Committee minutes, 24 Oct. 1938, in Roland's "Materials," HQA.
11. The availability of electric power dictates whether wind tunnels operate or not. Lack of power can thus be a major inhibitor of experimental progress. The problem of insufficient electricity plagued the LMAL from its first days to at least the end of World War II. In 1919 the NACA cited lack of power as one of the principal reasons

for its recommendation that Congress authorize relocation of the lab from Hampton to Bolling Field. In the 1930s some of Langley's larger wind tunnels functioned at no more than 50 percent of their maximum horsepower for want of adequate electricity. The local power situation perenially restricted the operation of many tunnels to off-peak-demand hours late at night and early in the morning; that is why military personnel living nearby complained so much about tunnel noise disrupting sleep. For correspondence on Langley's ongoing quest for improved electrical services, see C-162 series (1 through 3A) in the LCF. Unfortunately, nearly all the files on electrical procurement dating before 1940 were destroyed in a Langley housecleaning of the 1960s.

12. George Lewis at first wanted to see the laboratory located within five flying hours of Washington, which in 1938 meant east of the Mississippi River. Lewis to staff, 8 Nov. 1938, Accession 47 A 415, Box 33, National Archives.
13. U.S. House, Deficiency Appropriations Subcommittee, *Hearings,* 76th Cong., 1st sess., 1939, p. 42.
14. *Model Research,* p. 159.
15. See *AR 1939,* p. 2; also NACA minutes, 19 Oct. 1939.
16. This point is made by Elizabeth A. Muenger in *Searching the Horizon: A History of Ames Research Center, 1940–1976,* NASA SP-4304 (Washington, 1985), pp. 11–12.
17. RA 603, "Investigation in Full-Scale Wind-Tunnel of Drag Reduction on XF2A-1 Airplane."
18. Clinton H. Dearborn, "Full-Scale Wind-Tunnel Tests of Navy XF2A-1 Fighter Airplane," Conf. Memo. Rpt., 17 May 1938.
19. The 230-series airfoils were highly efficient because of their high maximum lift and low minimum drag. The break at the stall did tend to be rather sharp, however, causing an abrupt "nosing up" and loss of lift from which it was difficult for a pilot to recover. John P. Reeder interview with author, Hampton, Va., 25 May 1982.
20. See Paul L. Coe, Jr., "Review of Drag Clean-Up Tests in Langley Full-Scale Tunnel (From 1935 to 1945) Applicable to Current General Aviation Airplanes," NASA TN D-8206 (Washington, 1976) and Laurence K. Loftin, Jr., *Subsonic Aircraft: Evolution and the Matching of Size to Performance,* NASA RP-1060 (Washington, 1980), pp. 265–268.
21. Arnold to NACA, "Full-Scale Wind-Tunnel Tests of XP-39," 9 June 1939, RA file 674.
22. Smith J. DeFrance to Chief, Aerodyn. Div., "Estimated High Speed of the XP-39 Airplane," 25 Aug. 1939; Abe Silverstein and F. R. Nickle, "Tests of the XP-39 in the Full-Scale Tunnel," 27 Sept. 1939. Both in RA file 674.
23. "Comments of Representatives of Bureau of Aeronautics on Report of Drag Reduction on XP-39 Airplane," 22 Nov. 1939, ibid.
24. Larry Bell, President, Bell Aircraft Corp., to George W. Lewis, 17 Jan. 1940, ibid.
25. For diverging retrospective appraisals of P-39 performance, see "The Contentious Cobra," *Air International* 22 (Jan. 1982): 31ff., and Peter Bowers, "Airborne Cobra: The Story Behind World War II's Most Misunderstood and Unjustly Maligned Fighter—The Bell P-39 Airacobra," *Airpower* 6 (Nov. 1978): 20ff.
26. Maj. A. J. Lyon, Office of the Chief of the Air Corps, Washington, D.C., to NACA, 6 Feb. 1940; Henry J. E. Reid, memo for file, "Telephone Conversation with Dr. George W. Lewis regarding XP-39 Airplane," 28 Feb. 1940. Both in RA file 674.
27. Elton W. Miller, Acting Engineer-in-Charge, to NACA, "Investigation of XP-39 Airplane," 4 Mar. 1940, ibid.

28. John P. Reeder and L. J. Nelson, "Tests of the XP-39 in the NACA Full-Scale Tunnel," Conf. Memo. Rpt., 16 Mar. 1940, ibid.
29. Lewis to LMAL, "State of RA 674," 3 Apr. 1940, ibid.
30. Abe Silverstein to Engineer-in-Charge, "Trip to Toledo Scale Co. and Bell Aircraft Co., 22–24 April 1940," 29 Apr. 1940, ibid.
31. John W. Crowley, Jr., to Engineer-in-Charge, "Visit of Mr. A. C. Fornoff of the Bell Aircraft Corp. to Discuss the P-39 Investigation," 6 Feb. 1941, ibid.
32. Benjamin Pinkel to Chief, Power Plants Div., "Request from Full-Scale Tunnel Section Regarding Increase in Speed of XP-39 When Nozzles Are Attached to the Exhaust Stacks," 5 Apr. 1940, ibid.
33. *Frontiers of Flight,* p. 123.
34. Westover Committee Report, "Relation of NACA to National Defense in Time of War," 19 Aug. 1938. The complete files of this committee are in Accession 57 A 415, Box 18, National Archives.
35. Folders labeled "Solicitation of Employees," Nov. 1940–Dec. 1942 and Jan.–Dec. 1943, C154-50, LCF.
36. John F. Victory interview with Michael D. Keller, Hampton, Va., 22 June 1967, LHA. For the reaction of a Langley division chief to the idea of the NACA's personnel problems during wartime, see Starr Truscott to Engineer-in-Charge, "The Laboratory In Time of War: Recommendation Regarding Preparation For," 12 Nov. 1936, E32-7, LCF.
37. W. Kemble Johnson interview with Keller, Hampton, Va., 27 June 1967, LHA. (Johnson was the LMAL officer who did most of the negotiating with the state Selective Service director.) See C137-2, LCF, for documents on the militarization of LMAL personnel, especially the folder Jan. 1944–July 1944.
38. Paul E. Purser interview with Keller, Hampton, Va., 26 June 1967, LHA. The negative reaction of some Hampton–Newport News citizens to NACA deferments and the army-navy-NACA plan of Feb. 1944 was described for me by Langley oldtimers at the second NACA reunion, Williamsburg, Va., 14 Nov. 1982. One Langley oldtimer recalled that his children were ostracized by schoolmates because their father was believed to be a draft dodger.
39. "Apprentice School Graduates First Class," *LMAL Bulletin,* 17 Feb. 1943; "Thirty-Six Langley Apprentices Receive Graduation Certificates," *Air Scoop,* 16 Feb. 1945.
40. "NACA Employment for Girl Modelers," *Model Aviation* 6 (Nov. 1942): 15; "Part-Time Workers Employed by LMAL," *LMAL Bulletin,* 19 Mar. 1943; "LMAL Seeks 200 Women Shop Workers," *LMAL Bulletin,* 14–21 Aug. 1943.
41. R. H. Cramer (Curtiss representative to LMAL) to R. A. Darby (Curtiss personnel manager), "Computing Group Organization and Practices at NACA," 24 Apr. 1942, E26-3, LCF. For analysis of the campaign encouraging American women to take jobs that had traditionally been filled by men, see Maureen Honey, *Creating Rosie the Riveter: Class, Gender, and Propaganda* (Univ. of Massachusetts, 1983). Honey argues convincingly that the image of the capable female did not survive the war.
42. Interview with Marie-Bird Allen Bircher, Vera Huckel, Helen Johnson, Kitty O'Brien Joyner, Jane Moore, Betty Toll, Catherine Turner, and Helen Willey, 26 July 1983. All but Ms. Joyner worked during the war as computers. Joyner, who was the first woman engineering graduate of the University of Virginia, 1939, worked in systems engineering; she later became the first woman branch head at Langley.
43. "Keep Up Recruiting of Army Personnel," *Air Scoop,* 16 Feb. 1945.

44. *High-Speed Frontier,* p. 33.
45. John V. Becker's written remarks on the Aug. 1984 comment edition of my manuscript, dated 24 Oct. 1984, p. 15; Axel T. Mattson interview with author, Hampton, Va., 23 June 1983.
46. John Stack to Engineer-in-Charge, "Organization within Compressibility Research Division," 23 Feb. 1944, E26-3, LCF.
47. Roland states that the NACA placed Mead in this leadership position because it felt dependent on his expertise to launch its new engine research laboratory in Cleveland. *Model Research,* pp. 163–164.
48. Abbott, "A Review and Commentary of a Thesis by Arthur L. Levine," p. 161.
49. Henry J. E. Reid to NACA, 10 Sept. 1936, E37-3, LCF.
50. See John Victory to LMAL, "Instructions for the Handling of Restricted, Confidential, and Secret Material by NACA Personnel," 1 Feb. 1944, C157-15, LCF.
51. Newport News *Daily Press,* 9 June and 21 July 1937.
52. Memo for staff, 11 Jan. 1942, C152-15, LCF.
53. J. Edgar Hoover, Director, FBI, to Director, Office of Naval Intelligence, Navy Department, 11 Apr. 1942, ibid.; Reid to NACA, "Security of Information," 21 May 1942, ibid. In his letter to NACA headquarters, Reid reported that

    > there is a good possibility of the informant being in error. Many of our young boys belong to the local model club and are making models of modern pursuit planes (as was recently requested by the Sec. of the Navy). Furthermore, the boys are continually making models of new and interesting airplanes, many of which are of original design and which they test in their own wind tunnels and fly. Much of the work which these boys do in connection with the model club activity is highly scientific and it is quite possible that such discussions have been overheard, with detriment to the Committee.

54. In-house NACA pamphlet, "Don't Talk," 1943, C152-15, LCF.
55. See, for example, Leslie C. Merrill to Engineer-in-Charge, "Security Report," 6 Oct. 1944, C152-15, LCF.
56. Cambridge: Belknap Press of Harvard University Press, 1957, p. 373.
57. Lewis to Abbott, 1947, cited in Abbott's "Review and Commentary of a Thesis by Arthur L. Levine," p. 155.

Chapter 8
Exploring Unknown Technology: The Case of Jet Propulsion

1. Oscar Seidman and Charles J. Donlan, "Spin Tests of a 1/20-Scale Model of the XF4U-1 Airplane with Modified Tail Arrangement and Antispin Device Installed," NACA Conf. Memo. Rpt., 1 Sept. 1939; Anshal I. Neihouse, Jacob A. Lichtenstein, and Philip W. Pepoon, "Tail Design Requirements of Satisfactory Spin Recovery," TN 1045, Apr. 1945. See also Baals and Corliss, *Wind Tunnels of NASA,* NASA SP-440 (Washington, 1981), pp. 41–43.

2. Eugene C. Draley, "Wind-Tunnel Tests of 1/6-Scale P-38 Model in the 8-Foot High-Speed Tunnel," NACA Restr. Memo. Rpt., 14 May 1942. The concept for the flap was Langley's, but its specific configuration was developed for Lockheed at Ames.
3. See Margaret Steiner, "Ditching Behavior of Military Airplanes as Affected by Ditching Aids," NACA Memo. Rpt. L5A16, Jan. 1945, also published as Wartime Rpt. L-647. In cooperation with the army, the LMAL conducted the only experimental full-scale ditching of an aircraft (a modified Consolidated B-24D) during the war. See "Bomber Ditched in [James] River as Part of Laboratory Test," *LMAL Bulletin,* 23–29 Sept. 1944, as well as Wartime Rpts. L-617 and 648. Though officially this test was said to be necessary for confirmation of experimental data acquired from model tests in the NACA towing tanks, Langley managers agreed to the ditching reluctantly. Col. Carl Greene, the head of AAF Materiel Command Liaison Office at Langley, had been pushing the idea of the ditching test as a way to save lives of aircrew members forced down at sea. In fact, Greene and copilot Maj. Julian A. Harvey flew the B-24 into the river. When the two army officers came bubbling out of the water, responsible NACA personnel felt relieved. Despite extensive reinforcement, the airplane was damaged severely. It did not split in half, however, as B-24s had been prone to do during ditchings. See Floyd L. Thompson interview with Walter Bonney, 27 Mar. 1973, p. 11.
4. See NACA testimony before U.S. House, Independent Offices Subcommittee, *Hearings,* 79th cong., 2d sess., p. 547.
5. Theodore von Kármán, *Aerodynamics* (New York, 1954), pp. 130–132; and Laurence K. Loftin, Jr., *Quest for Performance: The Evolution of Modern Aircraft,* NASA SP-468 (Washington, 1985), pp. 112–117. On the early history of the compressibility crisis, see Richard Hallion, *Supersonic Flight: The Story of the Bell X-1 and Douglas D-558* (New York & London, 1972), pp. 14–15.
6. Telephone interview, Eastman Jacobs, Malibu, Calif., with author, 27 Aug. 1983.
7. John T. Sinette, Jr., Oscar W. Schey, and J. Austin King, "Performance of NACA Eight-Stage Axial-Flow Compressor Designed on the Basis of Airfoil Theory," TR 758, 1943; see Becker, *High-Speed Frontier,* pp. 31–32. On Langley's axial-flow compressor work, see Gray, *Frontiers of Flight,* pp. 283–286, and Schlaifer, *The Development of Aircraft Engines* (Harvard University, 1950), p. 460n.
8. On Italian experiments with jet propulsion and the Caproni-Campini aircraft, see G. Geoffrey Smith, *Gas Turbines and Jet Propulsion for Aircraft* (New York, 1944), pp. 47–52, and Johnathon W. Thompson, *Italian Civil and Military Aircraft, 1930–1945* (Los Angeles, 1963), pp. 95–96. For thoughtful treatment of the historical development of piston engine and jet engine technology in general, rely on Schlaifer, *Development of Aircraft Engines.*
9. Staff of the Airflow Research Div., "NACA Investigation of a Jet-Propulsion System Applicable to Flight," Adv. Conf. Rpt., 17 Sept. 1943, copy in RA file 1021.
10. Telephone interviews, Eastman Jacobs, 27 Aug. 1983, and Macon C. Ellis, Yorktown, Va., 29 Aug. 1983, with author. See Schlaifer, *Development of Aircraft Engines,* p. 461.
11. Edgar Buckingham, "Jet Propulsion for Airplanes," TR 159, in *AR 1923,* pp. 75–90.
12. *High-Speed Frontier,* p. 158.
13. See Edward Constant II, *The Origins of the Turbojet Revolution* (Baltimore: The Johns Hopkins University Press, 1980).

14. Eastman N. Jacobs and James M. Shoemaker, "Tests on Thrust Augmentors for Jet Propulsion," draft report, 23 July 1927, LCF, AA248-2. See also NACA HQ memorandum, "Discussion of LMAL Report, 'Tests on Thrust Augmentors for Jet Propulsion,' with Mr. Nicholas W. Akimoff" (an obscure propeller manufacturer from Philadelphia who had recently brought his own scheme of jet propulsion to the attention of George Lewis), 31 Aug. 1927, and Jacobs to Engineer-in-Charge, "Mr. Akimoff's comments on the report, 'Thrust Augmentors for Jet Propulsion,' by Eastman N. Jacobs and James M. Shoemaker," 10 Oct. 1927, in RA file 70.
15. Lt. S. P. Vaughn to NACA, "Jet Propulsion and Method of Generating Power," 31 Aug. 1927; Eastman Jacobs to Engineer-in-Charge, "Paper on 'Jet Propulsion and Method of Generating Power,' by Lt. S. P. Vaughn," 11 Oct. 1927; Vaughn to Edward P. Warner, Asst. Sec. of the Navy for Aeronautics, 20 Jan. 1928; Carlton Kemper to Engineer-in-Charge, "Discussion of Papers Submitted on 'Methods of Generating Power, Jet Propulsion and Gas Turbines,' by Lt. S. P. Vaughn, U.S.N.," 2 Mar. 1928; and William F. Joachim to Engineer-in-Charge, "Lt. Vaughn's Proposal, Jet Propulsion," 2 Mar. 1928. All of this correspondence is in E23-7A, LCF. The details of Vaughn's proposal for a gas turbine and of Langley's evaluation of them would make an illuminating American comparison with pioneering turbine research and development in Great Britain and Germany.

    There is even more to make one curious about Sidney Parahm Vaughn. In the period 1926–1935, he wrote no fewer than ten letters to the NACA proposing new aeronautical inventions and other ideas for research. In a letter to the NACA in 1934, for example, Vaughn proposed the design of a helicopter. In response George Lewis pointed out to him "that there is no demand at the present time for a vertical-lift machine and ... as far as I know it has no military advantages." For the reaction of Langley engineers to Vaughn's helicopter proposals, see especially John B. Wheatley to Engineer-in-Charge, "Rotating wing system proposed by S. P. Vaughn," 28 June 1935, E23-7A, LCF.

    Unfortunately I have not been able to find out much about Vaughn. He was born in Mississippi on 17 Nov. 1886. He was not an Academy graduate, but came into the navy as an enlisted man in 1905. After serving as paymaster's clerk on various vessels, he entered the officer corps during World War I. He stayed in the supply corps from 1919 until November 1946 when he retired from the navy with the rank of captain. In 1934, when he sent the helicopter proposal to the NACA, Lt. Comdr. Vaughn was serving on the battleship U.S.S. *Arizona.* When World War II started, he was supply officer on the repair ship U.S.S. *Medusa.* His duty stations during the war are not known. Nor have I been able to locate a death date for him. There is no mention of Vaughn either in the papers of Rear Adm. William A. Moffett, which are in the manuscript collection of the Nimitz Library at the U.S. Naval Academy in Annapolis, Md., or in any of Moffett's papers in the Naval History Division's *U.S. Naval History Sources in the United States* (Washington, 1979).
16. Henry J. E. Reid to NACA, "Lt. Vaughn's Proposal for Jet Propulsion for Airplanes," 17 Mar. 1928, E23-7A, LCF.
17. George Schubauer, "Jet Propulsion with Special Reference to Thrust Augmentors," TN 442, Jan. 1933.
18. Lewis to LMAL, "Visit of Mr. E. B. Myers with jet propulsion apparatus," 30 Dec. 1938, E23-7A, LCF. (Myers had mounted a primitive pulsejet in a Ford station wagon.) The Bureau of Aeronautics had requested RA 673, "Investigation of

Recovery of Energy from Waste Potential Energy of Engine Exhaust Gases," in a letter to NACA HQ dated 5 July 1938.

19. Sherman to Engineer-in-Charge, "Conference on proposed test of jet propulsion," 11 Apr. 1940, RA 351.
20. The October 1939 report of Lindbergh's Special Survey Committee on Research Facilities—which led in June 1940 to the establishment of the NACA's Aircraft Engine Research Laboratory in Cleveland—said nothing about gas turbines. The report identified "the superiority of foreign liquid-cooled engines as the reason for European leadership in certain important types of military aircraft," a conclusion that was true (*AR 1939,* pp. 2–3). Moreover, a special committee formed by the National Academy of Sciences and including such leaders of American aerodynamics as von Kármán and Robert Millikan of Caltech also reported, in June 1940, that "the gas turbine could hardly be considered a feasible application to airplanes mainly because of the difficulties in complying with the stringent weight requirements posed by aeronautics." U.S. Navy Dept., Bur. of Ships, Tech. Bulletin no. 2, "An Investigation of the Possibilities of the Gas Turbine for Marine Propulsion," report submitted to the sec. of navy by the Committee on Gas Turbines appointed by the Nat. Academy of Sciences, 10 June 1940 (Washington, 1941), p. 37, copy in RA file 1021.
21. Frank W. Meredith, "Note on the Cooling of Aircraft Engines with Special Reference to Radiators Enclosed in Ducts," British Adv. Comm. for Aeronautics, *R & M no. 1683,* 1936. (This concept was first applied to the radiator of the Supermarine Spitfire.) By 1939 American propulsion engineers knew about the Meredith effect independently through their analysis of the effects of heat in internal flow systems. They understood the effect in principle, but a practical demonstration under controlled conditions was still desirable. See *High-Speed Frontier,* p. 162.
22. Sherman, "The NACA Jet-Propulsion System," Memo. Rpt., 2 May 1941, AV434-1, LCF. Copy also filed in RA 1021, LHA.
23. Lewis to LMAL, "Jet Propulsion: E. B. Myers' Device," 24 Sept. 1940, AV434-1, LCF; Pinkel to Chief of Power Plants Div., "Jet Propulsion," 18 Oct. 1940, E23-7A, LCF.
24. It would be more than two years later, however, before Lewis asked the Executive Committee to issue an RA formally covering the work. This eventual request came after the creation of the Durand Special Committee on Jet Propulsion in Mar. 1941 and resulted in RAs 1020, "Investigation of the Aerodynamic and Thermodynamic Characteristics Necessary for an Efficient Jet-Propelled Airplane," and 1021, "Investigation of Combustion Systems for Jet Propulsion," both of which were signed by the NACA Executive Committee on 10 Sept. 1942. Most of the work done by Langley on jet propulsion in 1940 and 1941 the NACA assigned to these two RAs retroactively.
25. "NACA Investigation of a Jet-Propulsion Scheme Applicable to Flight," Adv. Conf. Rpt., 17 Sept. 1943, p. 2, RA 1021.
26. Arnold to Bush, 25 Feb. 1941, National Archives Record Group 255. I wish to thank Virginia Dawson, contract historian at NASA Lewis Research Center, Cleveland, Ohio, not only for bringing this and other important documents from the National Archives to my attention but also for kindly allowing me to reference them in my book.

27. Bush to Arnold, 10 March 1941, Record Group 255, National Archives (Dawson research).
28. Minutes of the NACA semiannual meeting, 24 Apr. 1941, p. 5. All of the reports of this Special Committee on Jet Propulsion are on file in the LHA.
29. Hugh L. Dryden, "The Contributions of William Frederick Durand to Aeronautics," paper delivered at the Durand Centennial Conference, Stanford University, 5 Aug. 1959.
30. Jacobs thought back on his opinion of Sherman's 1940 report in a memo to the engineer-in-charge, 16 May 1942; see also Henry J. E. Reid to George Lewis, 16 May 1942. Both in RA file 1020.
31. Durand to Lewis, 11 Dec. 1942, RA 1021; Bush to Durand, 18 March 1941, RG 255, Natl. Archives (Dawson research).
32. Durand to NACA Executive Committee, 23 Apr. 1941, ibid.; NACA minutes, 24 Apr. 1941, ibid.
33. Telephone interview, Jacobs with author, 27 Aug. 1983; Carlton Kemper to Engineer-in-Charge, "Meeting of Dr. Durand and Personnel Interested in Jet Propulsion Project," 26 Nov. 1941, RA 1020.
34. *Model Research,* p. 192.
35. Hallion, *Supersonic Flight,* p. 17.
36. Macon C. Ellis, Jr., interview with author, Hampton, Va., 12 Sept. 1983.
37. Sherman to Dr. Durand, "Auxiliary jet propulsion for existing military airplanes to greatly increase thrust for emergency high speed," 20 Apr. 1942, RA 1021.
38. Lewis to LMAL, "Research authorizations for jet-propulsion projects at Langley," 4 June 1942, RA 1020.
39. Sherman to Engineer-in-Charge, "Application of the NACA jet-propulsion system," 1 May 1942, ibid.
40. Sherman to Dr. Durand, "Auxiliary jet propulsion for existing military airplanes," 20 Apr. 1942, ibid.
41. Telephone interview, Eastman Jacobs with author, 27 Aug. 1983.
42. Ibid.
43. "NACA Investigation of a Jet-Propulsion System Applicable to Flight," 17 Sept. 1943, p. 5, RA 1020.
44. Quoted in Hallion, *Supersonic Flight,* p. 17.
45. Lewis to LMAL, "Action of Jet Propulsion Committee at its meeting held at Langley Field, July 31, 1942," 4 Aug. 1942, RA 1021; "Report of Conference of Laboratory Personnel, Jet-Propulsion Airplane, 29 Aug. 1942," 2 Sept. 1942, RA 1020.
46. Durand to Lewis, 29 Sept. 1942, Record Group 255, Natl. Archives (Dawson research).
47. *Supersonic Flight,* p. 17.
48. In a contemporary discussion with Ira H. Abbott, an important member of Jacobs's airflow research team, about the design of an inlet for the airplane's high-speed cowling, John V. Becker learned that Abbott was "dubious" about the Campini system. *High-Speed Frontier,* p. 148.
49. Sherman to Chief, Aerodyn. Div., "Program of tests of Lorin Booster unit," 24 Oct. 1942, RA 1021; "Principles of the NACA speed-booster, present status of its development and proposed program for its further development," 25 Feb. 1943; "The NACA Thin-Plate Burner for Jet-Propulsion Engines," Memo. Rpt., 4 Mar. 1943, ibid.

50. See weekly progress reports on jet propulsion work from Langley to the NACA, late Dec. 1942 through Feb. 1943, in RA 1020.
51. K. K. Nahigyan to Ray Sharp, "Combustion tunnel for burner test at AERL," 18 Dec. 1942; Nahigyan to Sharp, "Progress report on study of jet propulsion burners," 2 Jan. 1943. Both in RA 1021.
52. Benjamin Pinkel to Virginia Dawson, 26 Oct. 1984.
53. Henry J. E. Reid, memo for files, "Visit to Wright Field to discuss NACA jet-propulsion airplane design," 20 Jan. 1943, RA 1020; Clinton H. Dearborn, "Minutes of the Langley Jet-Propulsion Committee, January 30, 1943," p. 2, E5-25, LCF.
54. Maj. Gen. O. P. Echols, Commanding Gen., HQ, Materiel Command, USAAF, to Dr. W. F. Durand, Chairman, Special Comm. on Jet Propulsion, NACA, 16 Mar. 1943, RA 1020; Bastian Hello, "Final Report of Development, Procurement, Performance, and Acceptance, XP-80 Airplane," USAAF Tech. Rpt. 5235, 28 June 1945, copy in 1001 Lockheed P-80/1, Floyd L. Thompson Technical Library, LaRC.
55. Sherman's staff service card, LaRC Personnel Office; Eastman Jacobs to Engineer-in-Charge, "Consideration of Army Air Forces and Bureau of Aeronautics of NACA jet-propulsion project," 25 Mar. 1943, RA 1020.
56. *Model Research,* pp. 191–192.
57. Quoted in *Model Research,* p. 191.
58. Durand to Echols, 27 Feb. 1942, Record Group 255, Natl. Archives (Dawson research).
59. Durand to Keirn, 29 Oct. 1942, ibid.
60. Jacobs to George Lewis, London, 25 June 1943, RA 1020; telephone interview, Jacobs with author, 27 Aug. 1983.
61. "Jet Propulsion in England: Report of a Conference Attended by Officers of the Army and Navy and Representatives of American Industrial Organizations Visiting England during the Summer and Fall of 1943 for the Purpose of Study of the Above Subject," held in NACA offices, Washington, 18 Dec. 1943. Jacobs's remarks are recorded on pp. 2 and 14. Copy in RA file 1020.
62. Lewis to Jacobs, 12 July 1943, RA 1020.
63. Dawson presented her preliminary findings on "The Turbojet Revolution at Lewis Research Center" at the annual meeting of the Society for the History of Technology (SHOT), 4 Nov. 1984.
64. John Sloop, *Liquid Hydrogen as a Propulsion Fuel, 1945–1959,* NASA SP-4404 (Washington, 1978), p. 74.
65. Bennie W. Cocke, Jr., and Jerome Pasamanick, "Clean-Up Tests of the Bell YP-59 Airplane in the Langley Full-Scale Tunnel," Conf. Memo. Rpt. L5E14, 1 June 1945.

Chapter 9

The Transonic Problem

1. Clarence L. Johnson, "Investigation of tail buffeting conditions on Lockheed P-38 airplane," Lockheed Report No. 2414, 23 Sept. 1941, copy in 1105/Lockheed, LaRC Technical Library. See also Martin Caiden, *Fork-Tailed Devil: The P-38* (New York, 1971), pp. 48–50, and Gray, *Frontiers of Flight,* pp. 150–51.
2. See Arthur Kantrowitz to Engineer-in-Charge, "Methods for Propelling Supersonic Wind Tunnels," 3 Oct. 1938, A206-1, LCF, and Kantrowitz interview with Walter

Bonney, Everett, Mass., 1 Nov. 1971, pp. 1–4, LHA. Also, Henry J. E. Reid, Engineer-in-Charge, L.M.A.L., to NACA, "Necessity of Providing a Wind Tunnel of Supersonic Velocities," 11 May 1940, A206-1, LCF; "Design of Supersonic Wind Tunnel," 8 Oct. 1940, ibid.; "Supersonic Wind Tunnel Design," 31 Oct. 1940, ibid. For a technical description of the condensation problems in the supersonic tunnel, see H. J. E. Reid to NACA, "Additional Information on the Air-Drying Equipment for the Supersonic Tunnel as Proposed in the 1946 Budget," 11 Sept. 1944, ibid.

3. RA 928, "Investigation of High-Speed Buffeting and Compressibility on Lockheed P-38 Airplane," approved by Executive Committee, 15 Jan. 1942.
4. See Henry J. E. Reid to AAF Materiel Division–NACA Liaison Office, "Full-Scale Wind-Tunnel Investigation of Diving and Buffeting Difficulties of the P-38 Airplane," 16 Dec. 1941, in RA file 928, LHA. Also, in the same RA file, Harold I. Johnson, "Summary of P-38 Flying Qualities," NACA Memo. Rpt. to AAF, 8 July 1943.
5. John Stack, memo. for AAF Materiel Division, "Progress Report on the Tests of the P-38 Airplane in 8-Foot High-Speed Wind Tunnel," 3 March 1942, pp. 3–4, RA 928. See also Laurence K. Loftin, Jr., *Quest For Performance: The Evolution of Modern Aircraft,* NASA SP-468 (Washington, 1985), pp. 112–117.
6. Albert E. Erickson, "Investigation of Diving Moments of the Lockheed P-38 Airplane in the 16-Foot Wind Tunnel at Ames Aeronautical Laboratory," WR A-65, Oct. 1942. See also Edwin P. Hartmann, *Adventures in Research: A History of Ames Research Center , 1940–1965* (Washington, 1970), pp. 97–99.
7. Becker, *High-Speed Frontier,* pp. 47–50.
8. See John Stack, "The Compressibility Burble," TN 543, Oct. 1935, and Stack, W. F. Lindsey, and R. E. Littell, "The Compressibility Burble and the Effect of Compressibility on the Pressures and Forces Acting on an Airfoil," TR 646, 1938.
9. W. F. Hilton, "British Aeronautical Research Facilities," *Journal of the Royal Aeronautical Society* 70, Centenary Issue (1966): 103–04.
10. "Methods Employed in America for the Experimental Investigation of Aerodynamic Phenomena at High Speed," NACA Misc. Paper No. 42, Mar. 1936.
11. John Stack, "Effects of Compressibility on High-Speed Flight," *Journal of the Aeronautical Sciences* no. 1 (Jan. 1934): 40–43. (Copy in Stack Collection, LHA.)
12. George H. Gibson, "A Pioneer in High Pressure Steam: Carl Gustav Patrick de Laval," *Power* 68 (1928): 762–64.
13. L. J. Briggs and H. L. Dryden, "Aerodynamic Characteristics of 24 Airfoils at High Speeds," TR 319, printed in *AR 1929,* p. 328.
14. W. J. Orlin, "Application of the Analogy between Water Flow with a Free Surface and Two-Dimensional Compressible Gas Flow," TR 875, printed in *AR 1947,* p. 311.
15. See Hartley A. Soulé interview with Bonney, Hampton, Va., 28 Mar. 1973, p. 17, and *High-Speed Frontier*, p. 90.
16. On Kotcher's earlier suggestion of a high-speed research airplane, see Hallion, *Supersonic Flight,* pp. 12–13. Hallion's book is the principal secondary source for much of the narrative that follows in this chapter.
17. Frank J. Molina, "Take-off and Flight Performance of an A-20A Airplane as Affected by Auxiliary Propulsion Supplied by Liquid Propellant Jet Units," GALCIT Project No. 1, rpt. 12, 30 June 1942.
18. See Otto Acker, "Horizontal and Vertical Tail Analysis of the P-39 Airplane," Bell Aircraft Corp. rpt. 12-441-005, 5 Apr. 1941; I. G. Recant and R. B. Liddell, "Wind-Tunnel Tests of 1/8-Scale Model of Curtiss SB2C-1 airplane," NACA Conf. Memo.

Rpt. to U.S. Navy, 21 Jan. 1943; and R. F. Goranson, "Flight Studies of the Horizontal Tail Loads Experienced by a P-47 Airplane in Abrupt Maneuvers," NACA Memo Rept. to AAF, 27 Mar. 1943.

19. Cited in Hallion, *Supersonic Flight,* pp. 22–23.
20. *Air Force Supersonic Research Airplane XS-1 Report No. 1,* 9 Jan. 1948, p. 5, copy in XS-1 file, LCF.
21. *High-Speed Frontier,* p. 91.
22. For a description and analysis of the drop-body test technique, see John Stack, "Methods for Investigation of Flows at Transonic Speeds," paper for presentation at Naval Ordnance Laboratory Aero-Ballistics Research Facilities Dedication Symposium, 27 June–1 July 1949, pp. 3–7. Copy in Stack Collection, LHA. Also, Floyd L. Thompson, "Flight Research at Transonic and Supersonic Speed with Free-Falling and Rocket-Propelled Models," Proceedings of 2nd International Aeronautical Conference, New York City, May 1949, copy in Floyd Thompson Collection, LHA.
23. W. S. Farren, "Research for Aeronautics: Its Planning and Application," *Journal of the Aeronautical Sciences* 11, no. 2 (Apr. 1944): 95–109.
24. J. R. Thompson and C. W. Matthews, "Drag Measurements at Transonic Speeds on a Freely Falling Body," NACA Adv. Conf. Rpt. L5E03, May 1945.
25. *High-Speed Frontier,* p. 86. Becker referred to Langley's drop-body test data at the first design review of the XS-1 at Wright Field on 15 Mar. 1945.
26. R. R. Gilruth, "Résumé and Analysis of NACA Wing-Flow Tests," paper presented at Anglo-American Aeronautical Conference, Sept. 1947, A312-1, LCF.
27. Floyd L. Thompson interview with Bonney, 27 Mar. 1973, pp. 22–23.
28. Robert R. Gilruth interview with Keller, 26 June 1967, pp. 7–8.
29. *High-Speed Frontier,* p. 84.
30. R. R. Gilruth and J. W. Wetmore, "Preliminary Tests of Several Airfoil Models in the Transonic Speed Range," Adv. Conf. Rpt. L5E08, May 1945.
31. On p. 11 of Joseph A. Shortal's *A New Dimension: Wallops Flight Test Range, The First Fifteen Years,* NASA RP-1028 (Washington, 1978), there is a table documenting Langley's support of guided missile projects from June 1941 to May 1945.
32. The members of this team were Floyd L. Thompson, Robert R. Gilruth, Robert T. Jones, Frederick J. Bailey, Edmund C. Buckley, Macon C. Ellis, Jr., John Stack, Hartley A. Soulé, Joseph A. Shortal, and Kennedy F. Rubert. Crowley acted as chairman.
33. For a thorough, firsthand account of the ideas and events leading up to the establishment of the Wallops flight station, see Shortal, *A New Dimension,* pp. 1–26.
34. *High-Speed Frontier,* pp. 86–87.
35. Joseph A. Shortal interview with author, Hampton, Va., 22 Sept. 1981.

Chapter 10

Defining the Research Airplane

1. See Robert L. Perry, *The Antecedents of the X-1,* Rand Corp. Paper No. P-3154, July 1965, copy in Stack Collection, LHA.
2. No author, "Calendar of Events for the XS-1 Airplane," undated (ca. Nov. 1947), in XS-1, LCF.
3. See Stack's handwritten notes entitled, "Review of Transonic Airplane Designs Submitted by Army and Replies to Army Criticisms of NACA Design, Prepared

for Conference with Army Personnel, Dec. 13, 1944, Reviewed by Messrs. Davidson, Turner, Stack, and Draley," in Stack Collection, LHA.

4. *High-Speed Frontier*, pp. 91–92.
5. Hallion points out in *Supersonic Flight* (p. 52 n. 21) that "MX-524" seems to have designated army high-speed flight projects in general. By the late spring of 1945, the XS-1 project had been given its own specific designation, MX-653.
6. Milton Davidson to Chief of Research, "Visit of Mr. Milton Davidson of the Langley Laboratory to the Airplane Design Group, Bureau of Aeronautics, Navy Department, Washington, D.C., Feb. 2, 1945 through Feb. 19, 1945, to make a Cooperative Preliminary Design Study for an NACA Transonic Research Airplane," 3 Mar. 1945, in Stack Collection, LHA.
7. Milton Davidson to Chief of Research, "Conference at Langley Laboratory with Representatives of the Douglas Aircraft Company, Navy Department Bureau of Aeronautics, and NACA on Proposals for a Transonic Research Airplane," 27 Feb. 1945, Stack Collection, LHA.
8. L.M.A.L. to NACA Headquarters, "Estimate of Instrument Requirements for Experimental Airplane," 27 Dec. 1944, A184-8, LCF.
9. Apparently, Bell and the Army Air Forces arrived at the load factor of 18g independently. *Supersonic Flight*, pp. 36 and 52 n. 20.
10. Antonio Ferri, "Completed Tabulation of Tests in the United States of 24 Airfoils at High Mach Numbers," NACA Wartime Rpt. L-143, June 1945. This paper was edited by Langley in Jan. 1945.
11. Handwritten note attached to Milton Davidson's memo to chief of research, "Status of development and effect on performance of rocket power plant for Bell Aircraft Corporation Transonic Research Airplane Project MX-653," 28 Apr. 1945, XS-1, LCF.
12. Gilruth interview with Keller, pp. 9–10.
13. See Albert E. von Doenhoff to Chief of Research, "Discussion with Mr. Vladimir Morkovin of Bell Aircraft Co. on March 21, 1945, Regarding Wing-Selection Problems for Army Project MX-653," 22 Mar. 1945, XS-1, LCF, and Engineer-in-Charge, L.M.A.L., to Air Service Technical Command Liaison Office at NACA Langley Field, Va., "Project MX-653: Recommendations for a Second Wing," 20 June 1945, RA 1347.
14. See series of relevant correspondence in RA 1347, Jan.–Sept. 1945. Also, *Supersonic Flight*, pp. 42–43.
15. This biographical sketch is derived essentially from Jones's "Recollections from an Earlier Period in American Aeronautics," *Annual Review of Fluid Mechanics* 9 (1977): 1–11.
16. There are several memorandum reports by Jones covering his missile work during this period in RA files 1316 and 1328.
17. Richard Hallion, "Lippisch, Gluhareff, and Jones: The Emergence of the Delta Planform and the Origins of the Sweptwing in the United States," *Aerospace Historian* (Mar. 1979): 5.
18. R. T. Jones interview with Walter Bonney, Moffett Field, Calif., 24 Sept. 1974, p. 5.
19. Max M. Munk, "Note on the Relative Effect of the Dihedral and the Sweep Back of Airplane Wings," TN 177, 1924, p. 2.
20. Adolf Busemann, "Aerodynamischer Auftrieb bei Überschallgeschwindigkeit," *Convegno di Scienze Fisiche, Matematiche e Naturali; Tema: Le Alte Velocita in avi-*

azione; Roma 1935 (Rome, 1936): 315–47. Busemann discusses the fate and historical significance of his Volta presentation in "Compressible Flow in the Thirties," *Annual Review of Fluid Mechanics* 3 (1971): 6–7, and in "An Interview with Adolf Busemann, Pioneer in Shock Waves, Supersonic Flight, and Fusion Power," *Fusion* (Oct./Nov. 1981): 36–37.

21. Telephone interview, Eastman N. Jacobs with author, 27 Aug. 1983. Also, see von Kármán, *Aerodynamics: Selected Topics in the Light of Their Historical Development* (Ithaca, N.Y., 1954), pp. 133–134.
22. R. T. Jones to Chief of Research, "Tests of Experimental Wing Shapes Designed to Minimize Compressibility Effects," 5 Mar. 1945, A174-1, LCF.
23. Jones, "Wing Plan Forms for High Speed Flight," 23 Apr. 1945. Editorial copy given to LHA by Macon C. Ellis, Jr., Nov. 1984.
24. I. E. Garrick interview with author, 24 Sept. 1981. Garrick was a member of the editorial committee, as well as a member of Theodorsen's Physical Research Division.
25. Jones interview with Bonney, p. 5.
26. Thompson interview with Bonney, pp. 7–8.
27. C. W. Matthews and J. R. Thompson, "Comparative Drag Measurements at Transonic Speeds of Straight and Sweptback NACA 65-009 Airfoils Mounted on a Freely-Falling Body," NACA Memo. Rpt. L5G23a, July 1945. Released in 1949 as TR 988.
28. Though the section of wire extended into the airstream at a swept *forward* instead of a swept back angle, Jones recognized that the principle was the same. Macon C. Ellis, Jr., interview with author, 23 Mar. 1984.
29. Engineer-in-Charge to NACA Headquarters, "R. T. Jones' Report, 'Wing Plan Forms for High-Speed Flight'," 7 June 1945, A184-9, LCF.
30. Busemann's paper was translated into English by W. J. Stern of the British Aeronautical Research Committee. NACA headquarters received a copy of Stern's translation in May 1942; however, the date of receipt stamped by the Langley library on its copy of the translation is 25 June 1945. This copy is available in the LaRC Technical Library under code 1107 46 A.
31. In March 1946, the NACA published Jones's report as TN 1033. A final version of the same report appeared in *AR 1947* as TR 863.
32. John V. Becker to Chief of Research, "Conference at Wright Field on March 15, 1945, to Discuss Transonic Airplane Proposed by Bell Aircraft Corporation," 21 Mar. 1945, in XS-1, LCF.
33. *High-Speed Frontier*, pp. 92–93.
34. Bell engineers Robert J. Woods and Robert Stanley disagreed over whether to design the XS-1 for air or ground launching. Woods, a former employee of the NACA at Langley laboratory, wanted to design the plane with retractable landing gear for conventional ground takeoff and landing. He felt, as the NACA did, that the more conventional the design, the more relevant to the development of an operational rocket-propelled manned interceptor experience with the new plane would be. Stanley, on the other hand, originally wanted to design the plane with skids for landing after air launching. He believed that the air launch mode was the only way for the XS-1 to achieve the supersonic performance the army wanted. Lawrence D. Bell, president of the Bell Aircraft Corporation, eventually resolved the dispute between his two engineers with a compromise; he decided that the XS-1 would be air launched but that it would also have retractable landing gear. For a

more detailed narrative and analysis of this design dispute inside Bell, see *Supersonic Flight*, pp. 44–45.

35. Laurence K. Loftin, Jr., *Subsonic Aircraft: Evolution and the Matching of Size to Performance,* NASA RP-1060 (Washington, 1980), pp. 370–73.
36. Engineer-in-Charge Henry J. E. Reid to NACA Headquarters, "MX-653 Airplane—Participation of Langley Laboratory in Flight Tests of the MX-653," 29 Dec. 1945, XS-1, LCF. Hartley Soulé, head of the Stability Research Division at Langley, remembers writing a memo at about this time pointing out "the undesirability of air launching the [XS-1] because we wanted to run the tests at Langley." "It turned out to be a very bad idea," Soulé recalled in 1973, "but we wanted to keep it at home. You like control of these things." Soulé interview with Bonney, 28 Mar. 1973, p. 17.
37. Bell's point of view is represented by Langley engineer Milton Davidson in his memorandum to Gus Crowley, Langley's chief of research, "Status of development and effect on performance of rocket power plant for Bell Aircraft Corporation Transonic Airplane Project MX-653," 28 Apr. 1945, XS-1, LCF.
38. John Stack, memorandum for Chief of Research, 1 May 1947, in RA 1347.
39. Edward C. Polhamus interview with author, Hampton, Va., 20 Apr. 1984.
40. See "Report of an Informal Discussion between Dr. Clark Millikan and officers of the Bureau of Aeronautics on 7 July 1945," copy in LaRC Technical Library, Code 6600 Germany/22. Millikan, acting director of the Guggenheim Aeronautical Laboratory at Caltech, carried the microfilmed German swept-wing reports from Europe to BuAer in Washington and to Douglas in California.
41. H. Multhopp, "The Sweptback Wing at High Velocity," Air Materiel Command, Wright Field, Tech. Intelligence Trans. F-TS-411-RE, 8 July 1946.
42. Floyd L. Thompson, memorandum for engineer-in-charge, 31 Oct. 1945, in RA file 1333.
43. *Supersonic Flight,* p. 61.
44. Milton Davidson to Chief of Research, "Conference with Representatives of the Douglas Aircraft Corp., Navy Department Bureau of Aeronautics, and NACA at the Navy Department Regarding High-Speed Research Airplanes, February 23, 1945," 26 Feb. 1945, D-558, LCF.
45. Davidson to Chief of Research, 3 Mar. 1945, ibid.
46. Davidson refers to Diehl's opinion in his memo of 26 Feb. 1945, ibid. He quotes Conlon in the memo he wrote to the chief of research the next day, ibid.
47. *Supersonic Flight,* p. 62.
48. Davidson, "Telephone Conversation between Comdr. E. W. Conlon, Bureau of Aeronautics, U.S. Navy Department, and Messrs. John Stack and Milton Davidson, Langley Laboratory, on Revised Douglas Transonic Airplane Proposal, April 11, 1945," memo for files, 16 Apr. 1945. See also John Stack to Chief of Research, "Douglas Model 558 Airplane," 12 June 1945. Copies of these two memos are in both the Stack Collection, LHA, and in D-558, LCF.
49. John Stack to Chief of Research, "Visit to Douglas Aircraft Company, El Segundo, California, with U.S. Navy Mock-Up Board to Review Transonic Research Airplane D-558," 20 July 1945. See also Floyd L. Thompson to Engineer-in-Charge, "Visit to Douglas Aircraft Company, El Segundo, California, as Official Observer on Mock-Up Board of Airplane D-558: July 2, 3, and 4, 1945," 7 Aug. 1945. Both memos in D-558, LCF.

50. *Supersonic Flight*, p. 65. Hallion based his account of the NACA's evaluation of the second mock-up on his interview with Milton Ames, the NACA's representative at the August inspection.
51. Milton Davidson to Chief of Research, "D-558 Airplane with Sweptback Wings and High-Lift Device," 20 July 1945, D-558, LCF.
52. See special delivery letter from E. H. Heinemann, Chief Engineer, Douglas Aircraft Company, Inc., to Bureau of Aeronautics, "General Arrangement of Model D-558 Airplane," 21 July 1945. Copy sent to Stack, D-558, LCF.
53. *High-Speed Frontier*, p. 92.
54. George Lewis to L.M.A.L., "Change in Status of Mr. E. N. Jacobs," 29 Jan. 1944, E26-3, LCF.
55. Macon C. Ellis, Jr., interviews with author, Hampton, Va., 12 Sept. 1983 and 20 Mar. 1984.
56. Macon C. Ellis, Jr., and Clinton E. Brown, "Analysis of Supersonic Ram-Jet Performance and Wind-Tunnel Tests of a Possible Supersonic Ram-Jet Airplane Model," NACA Adv. Conf. Rept. L5L12, Dec. 1945.
57. *High-Speed Frontier*, p. 98; Macon C. Ellis, Jr., interview with author, 12 Sept. 1983.
58. Engineer-in-Charge Henry J. E. Reid to NACA, "MX-653 Airplane—Participation of Langley Laboratory in Flight Tests of the MX-653," 29 Dec. 1945, XS-1, LCF. On the relation of the NACA to the preliminary flight testing of the XS-1, see also letter from Charles Helms, Assistant Director of Aeronautical Research, to Director, AAF Materiel Command, Wright Field, Dayton, Ohio, 8 Jan. 1946, and Hartley Soulé to Chief of Research, "XS-1 and XS-2 Airplanes—Discussion at Bell Aircraft Corporation Plant on January 3, 4, and 5, 1946," 25 Jan. 1946. Both pieces of correspondence in RA file 1347.
59. For letters and telegrams from NACA personnel at Pinecastle to research managers at Langley laboratory, see D-558 folder, 1 Dec. 1945–June 1946, LCF.
60. *Supersonic Flight*, p. 90.
61. See prologue to Hallion's *On The Frontier: Flight Research at Dryden, 1946–1981*, NASA SP-4303 (Washington, 1984).
62. Herbert H. Hoover, Head of Flight Operations, L.M.A.L., to Engineer-in-Charge, "Transportation of Personnel and Equipment for the XS-1 Project at Muroc," 23 Sept. 1946, in RA file 1347.
63. Engineer-in-Charge Henry J. E. Reid to Commanding Officer, Muroc Army Air Base, Muroc, Calif., 20 Sept. 1946, ibid.
64. Engineer-in-Charge Reid to NACA, "XS-1 Flight Test Program," 26 Sept. 1946, XS-1, LCF.
65. *Supersonic Flight*, pp. 92–97.
66. Hartley A. Soulé to Chief of Research, "Army Proposal for Accelerated Tests of the XS-1 to a Mach Number of 1.1," 21 July 1947, XS-1, LCF.
67. Walter Williams to Melvin N. Gough, Chief of the L.M.A.L. Flight Research Division, 29 May 1947, ibid.
68. Walter Williams to Hartley Soulé, 22 Sept. 1947, ibid. See also Williams to Chief of Research, "XS-1 Project, Progress Report for the Week Ending September 19, 1947," 22 Sept. 1947, ibid.
69. *Supersonic Flight*, pp. 107–08.
70. Chuck Yeager and Leo Janos, *Yeager: An Autobiography* (Toronto & New York, 1985), p. 122 and pp. 127–129.

71. Williams to Chief of Research, "XS-1 Progress Report for 2-Week Period Ending October 18, 1947," 20 Oct. 1947, XS-1, LCF.
72. Herbert H. Hoover to Chief of Research, "Collapse of Nose Wheel on XS-1 Airplane No. 2 on Flight of October 21, 1947," 22 Oct. 1947, ibid. The accident reinforced Yeager's impression that the XS-1 "demanded an experienced fighter pilot at the controls," and that the NACA's test pilots "just weren't qualified to fly it." *Yeager: An Autobiography,* p. 180.
73. For a chronology of NACA flights with the research airplanes, see *Supersonic Flight,* app. 2.
74. National Aeronautic Association news release, "Highest Aviation Award Made for Supersonic Flight," 17 Dec. 1948. Copy in John Stack biographical folder, LHA.
75. Frederick R. Neely, "The Collier Trophy For Flight Beyond the Speed of Sound," *Collier's* (25 Dec. 1948): 30–31.
76. *High-Speed Frontier,* p. 92.
77. Stack, "Methods for Investigation of Flows at Transonic Speeds," paper for presentation at Naval Ordnance Laboratory Aero-Ballistics Branch Research Facilities Dedication Symposium, 27 June–1 July 1949, copy in Stack Collection, LHA.
78. See, for example, Stack's 1951 paper to the 3d International Aero Conference, London, 7–11 Sept. 1951. This paper was an updated version of the one he had presented at the NavOrd Aero-Ballistics Research Facilities Dedication Symposium (see previous note).
79. *High-Speed Frontier,* pp. 95–96.
80. Ibid., p. 96.
81. Alex Roland offers a more negative interpretation of the research airplane program: It "was a success, but more clearly as a psychological breakthrough and a public-relations coup than as a research enterprise .... Even the defenders of the program are hard pressed to justify it in terms of cost effectiveness." *Model Research,* p. 250.

Chapter 11

The Slotted Tunnel and the Area Rule

1. Eugene S. Ferguson, "The Mind's Eye: Nonverbal Thought in Technology," *Science* 197 (26 Aug. 1977): 827–836.
2. On the sources of technological creativity, I recommend Robert Pirsig, *Zen and the Art of Motorcycle Maintenance* (1974); Arnold Pacey, *The Maze of Ingenuity: Ideas and Idealism in the Development of Technology* (1975); and three short books (published by Farrar, Strauss, and Giroux) by John McPhee: *The Deltoid Pumpkin Seed* (1973), *The Survival of the Bark Canoe* (1975), and *The Curve of Binding Energy: A Journey into the Awesome and Alarming World of Theodore B. Taylor* (1976).
3. Edward Constant II has pointed out that experimental technologies like wind tunnels can become "powerful traditions" in their own right, "at once enhancing and also channeling and inhibiting research." (See his review of Becker's *High-Speed Frontier* in *Isis* 73 [1982]: 609–10.) There may have been an element of this "tunnel vision" in Stack's attack on the transonic problem.
4. R. W. Byrne, "Experimental Constriction Effects in High-Speed Wind Tunnels," NACA Adv. Conf. Rpt. L4L07a, May 1944. Also published by the NACA as Wartime Rpt. L-74.

5. See Richard T. Whitcomb, "An Investigation of a Typical High-Speed Bomber Wing in the Langley 8-Foot High-Speed Tunnel. I. Basic Wing Characteristics," NACA Research Memo. L5F09, June 1945, and Becker, *High-Speed Frontier,* pp. 74–75.
6. Ibid., pp. 70–75.
7. L. W. Habel, "The Langley Annular Transonic Tunnel and Preliminary Tests of an NACA 66-006 Airfoil," NACA Research Memo. L8-A28, June 1948. See also Stack's paper for NavOrd Aero-Ballistics Research Facilities Dedication Symposium, pp. 13–17.
8. Edward Polhamus, "Summary of Results Obtained by Transonic Bump Method on Effect of Planform and Thickness on the Characteristics of Wings at Transonic Speeds," TN 3469, 1955. See also Baals and Corliss, *Wind Tunnels of NASA,* pp. 37–38.
9. *High-Speed Frontier,* pp. 99–100. NACA researchers at both Langley and Ames laboratories were studying subsonic interference effects (including the blockage problem mentioned earlier) and the transonic choking problem during this period. For Ames's work on interference effects, see Walter G. Vincenti and D. J. Graham, "The Effect of Wall Interference upon the Aerodynamic Characteristics of an Airfoil Spanning a Closed-Throat Circular Wind Tunnel," NACA Adv. Conf. Rpt. 5D21, May 1945.
10. Russell G. Robinson and Ray H. Wright, "Estimation of Critical Speeds of Airfoils and Streamline Bodies," NACA Adv. Conf. Rpt. No. L-781, 1940; John Stack, "Tests of Airfoils Designed to Delay the Compressibility Burble," TRs 763 (1943) and 976 (1944).
11. *Wind Tunnels of NASA,* pp. 60–61.
12. Ray H. Wright to Chief, Full-Scale Division, "Theoretical consideration of the use of axial slots to minimize wind-tunnel blockage," 24 May 1948, A206-1D, LCF.
13. C. Wieselberger, "On the Influence of the Wind Tunnel Boundary on the Drag, Particularly in the Region of Compressible Flow," NACA translation (1946) of report in *Luftfahrt Forschung,* 19, 1942.
14. *High-Speed Frontier,* p. 37.
15. Antonio Ferri, "Completed Tabulation in the U.S. of Tests of 24 Airfoils at High Mach Numbers," NACA Wartime Rpt. No. L-143, 1945.
16. *High-Speed Frontier,* p. 38.
17. Ibid., p. 100.
18. Mark R. Nichols interview with Walter Bonney, Hampton, Va., 29 Mar. 1973.
19. Richard T. Whitcomb interview with Walter Bonney, Hampton, Va., p. 11.
20. *High-Speed Frontier,* p. 103.
21. On Busemann, see Richard Hallion, *Designers and Test Pilots* (Alexandria, Va.: Time-Life Books, 1938), pp. 133–145. There exists in the Langley archives a rare file of U.S. government documents related to Busemann: E38-5 ("Policy and Procedure on Paperclip Specialists [BUSEMANN ONLY]").
22. *High-Speed Frontier,* p. 103.
23. Ibid., p. 104.
24. In the early 1950s, two Langley engineers successfully applied the method Busemann was here recommending; see D. D. Davis and D. Moore, "Analytical Study of Blockage and Lift-Interference Corrections for Slotted Tunnels Obtained by Substitution of an Equivalent Homogeneous Boundary for the Discrete Slots," NACA Research Memo. L53E07b, 1953.

25. *High-Speed Frontier,* p. 38.
26. Becker interview with Bonney, Hampton, Va., 27 Mar. 1973, p. 8.
27. Blake W. Corson, Jr., to Chief, 16-Foot Tunnel, "Description and Justification for Slotted Test Section, Material Prepared for FY 1949 Budget," 10 Jan. 1948, AC360-1, LCF.
28. *High-Speed Frontier,* p. 109.
29. Axel T. Mattson interview with author, Hampton, Va., 19 Oct. 1981.
30. *Wind Tunnels of NASA,* p. 63.
31. *High-Speed Frontier,* pp. 110 and 113.
32. *Wind Tunnels of NASA,* pp. 64–65.
33. "NACA Transonic Wind-Tunnel Test Sections," NACA Research Memo. L8J06, Sept. 1948. According to written instructions dated 12 Oct. 1948, the reproduction and distribution of this classified publication had "highest priority." A copy of these instructions exists in RA file 70.
34. Eugene C. Draley to Chief of Research, "Visit of representatives from Headquarters, Air Materiel Command, Wright-Patterson Air Force Base, wind-tunnel group," 21 Dec. 1948, ibid.
35. Hugh L. Dryden to recipients of NACA publication no. L8J06, 15 Mar. 1954, ibid.
36. Eugene C. Draley to Chief of Research, "University of Southern California slotted test section investigations," 18 May 1950, ibid.
37. The declassified publications—all Research Memorandums—included: Wright and Ward, "NACA Transonic Wind-Tunnel Test Sections," L8J06, 1948; Nelson and Bloetscher, "Preliminary Investigation of a Variable Mach Number Two-Dimensional Supersonic Tunnel of Fixed Geometry," L9D29a, 1949; Bates, "Preliminary Investigation of 3-Inch Slotted Transonic Wind-Tunnel Test Sections," L9D18, 1949; Nelson and Klevatt, "Preliminary Investigation of Constant-Geometry, Variable Mach Number, Supersonic Tunnel with Porous Walls," L50B01, 1950; Nelson and Bloetscher, "Preliminary Investigation of Porous Walls as a Means of Reducing Tunnel Boundary Effects at Low-Supersonic Mach Numbers," L50D27, 1950; Davis and Wood, "Preliminary Investigation of Reflections of Oblique Waves from a Porous Wall," L50G19a, 1950; Sleeman, Klevatt, and Linsley, "Comparison of Transonic Characteristics of Lifting Wings from Experiments in a Small Slotted Tunnel and the Langley High-Speed 7- by 10-Foot Tunnel," L51F14, 1951; Nelson and Bloetscher, "An Experimental Investigation of the Zero-Lift Pressure Distribution over a Wedge Airfoil in Closed, Slotted, and Open-Throat Tunnels at Transonic Mach Numbers," L52C18, 1952; Wood, "Reflection of Shock Waves from Slotted Walls at Mach Number 1.62," L52E27, 1952; Matthews, "Theoretical Study of the Tunnel-Boundary Lift Interference Due to Slotted Walls in the Presence of the Trailing-Vortex System of a Lifting Model," L53A26, 1953; Davis and Moore, "Analytical Study of Blockage- and Lift-Interference Corrections for Slotted Tunnels Obtained by the Substitution of an Equivalent Homogeneous Boundary for the Discrete Slots," L53E07b, 1953; Baldwin, Turner, and (from Ames laboratory) Knechtel, "Wall Interference in Wind Tunnels with Slotted and Porous Boundaries at Subsonic Speeds," A53E29, 1953; and Stokes, Davis, and Sellers, "An Experimental Study of Porosity Characteristics of Perforated Materials in Normal and Parallel Flow," L53H07, 1953.
38. Richard T. Whitcomb, "A Proposal for a Swept Wing Fuselage Combination With Small Shock Losses at Transonic Speeds," July 1948, AH321-1, LCF. For contemporary recognition of the lack of understanding about the large and highly variable

drag interference associated with wing-body combinations, see the following NACA Research Memorandums: Charles J. Donlan, Boyd C. Myers, and Axel T. Mattson, "A Comparison of the Aerodynamic Characteristics at Transonic Speeds of Four Wing-Fuselage Combinations as Determined from Different Test Techniques," L50H02, 1950; Langley Pilotless Aircraft Research Division (PARD), "Some Recent Data from Flight Tests of Rocket-Powered Model," L50K24, 1951; and William B. Pepper, Jr., and Sherwood Hoffmann, "Comparison of Zero-Lift Drags Determined by Flight Tests at Transonic Speeds of Symmetrically Mounted Nacelles in Various Spanwise Positions on a 45° Sweptback Wing and Body Combination," L51D06, 1951.

39. "Chronological Summary of the Transonic Drag Problem and Development of the Area Rule Concept," 19 July 1955, A329-3, LCF.
40. Whitcomb interview with Walter Bonney, pp. 1–2; for discussion of preliminary work in the 8-foot slotted tunnel, see Virgil S. Ritchie and Albin O. Pearson, "Calibration of the Slotted Test Section of the Langley 8-Foot Transonic Tunnel and Preliminary Experimental Investigation of Boundary-Reflected Disturbances," NACA Research Memo. L51K14, 1952.
41. Hallion, *Designers and Test Pilots,* p. 143.
42. Richard T. Whitcomb and Thomas C. Kelly, "A Study of the Flow over a 45° Sweptback Wing-Fuselage Combination at Transonic Mach Numbers," NACA Research Memo. L52D01, 1952. When the area rule was declassified in 1955, a revised version of this report, complete with all the accompanying charts and tables, appeared in *Aviation Week* (12 Sept. 1955), pp. 28–48.
43. Whitcomb interview with Bonney, p. 4.
44. Ibid., pp. 3–4. In 1952 the NACA published a study by Busemann on variations of streamtube area with changes in velocity and their effects on shock formation: "Application of Transonic Similarity," TN 2687.
45. Whitcomb interview with Bonney, p. 4.
46. Axel T. Mattson interview with Bonney, 29 March 1973, p. 10.
47. Hallion, *Designers and Test Pilots,* p. 143.
48. "Chronological Summary of the Transonic Drag Problem and Development of the Area Rule Concept," 19 July 1955, p. 7, A329-3, LCF.
49. For example, see Axel T. Mattson to Ira Abbott, Associate Director for Research, "Application of area rule to existing designs," 16 Apr. 1953, ibid.
50. "Chronological Summary on Application of Area Rule Concept to Convair F102," 19 July 1955, ibid. Hallion's *Designers and Test Pilots* presents an excellent brief history of the early applications of the area rule (pp. 144–145), as does Baals and Corliss's *Wind Tunnels of NASA*, pp. 62–63. The day-to-day details of Langley's role in the applications may be followed in the correspondence in A329-3 ("Interference of Airplane Bodies and Parts"), AH321-1 ("Langley 8-Foot Transonic Wind Tunnel Section"), B10-2 ("United States Air Force" and "Wright Patterson Air Force Base"), B10-5 ("Navy Department"), and E37-4 ("Visitors")—all of which are in the LCF.
51. These are the words of Convair test pilot Pete Everest, quoted in Hallion, *Designers and Test Pilots,* p. 144.
52. *Wind Tunnels of NASA,* pp. 62–63.
53. David A. Anderton, "NACA Formula Eases Supersonic Flight," *Aviation Week* (12 Sept. 1955), p. 15.

54. Commentary by Robert Hotz, "The Area Rule Breakthrough," *Aviation Week* (12 Sept. 1955), p. 152.
55. Whitcomb interview with Bonney, pp. 6–8.
56. *Model Research,* pp. 280–281.
57. Wallace D. Hayes, "Linearized Supersonic Flow," (California Institute of Technology Ph.D. thesis, June 1947).

Chapter 12

Hypersonics and the Transition to Space

1. Walter Dornberger, *V-2—Shot Into the Universe: The History of a Great Invention,* trans. by James Cleugh, with intro. by Willy Ley (New York: Viking Press, 1958).
2. John V. Becker to Chief of Research, "Proposal for new type supersonic wind tunnel for Mach number 7.0," 3 Aug. 1945, pp. 1–3, copy in Becker's personal files, Newport News, Va.
3. Ibid., p. 4.
4. Telephone conversation, Becker, Newport News, Va., with author, 16 Feb. 1983. Edwin P. Hartmann mentioned the reluctance of some NACA personnel to build hypersonic research facilities in his *Adventures in Research: A History of Ames Research Center, 1940–1965,* NASA SP-4302 (Washington, 1970), p. 139.
5. Becker, "Results of Recent Hypersonic and Unsteady Flow Research at the Langley Aeronautical Laboratory," *Journal of Applied Physics* 21 (July 1950): 619–28. The journal *Aviation Week* mentioned Becker's article in its 31 July 1950 issue, pp. 22 and 24. (Becker had presented a version of this paper on 27 Dec. 1949 before the American Physical Society at the University of Virginia.)
6. Alfred J. Eggers of Ames laboratory in California began the design of a 10 × 14-inch (continuous-flow) hypersonic tunnel in 1946; the resulting facility became operational in 1950. The first hypersonic tunnel of the Naval Ordnance Laboratory, which received parts of the unfinished Mach 10 tunnel that the Nazis had been building at Peenemünde, came on line at about the same time as the Ames facility.
7. Baals and Corliss, *Wind Tunnels of NASA,* pp. 94–95.
8. Macon C. Ellis, Jr., interview with author, Hampton, Va., 12 Sept. 1983.
9. *Wind Tunnels of NASA,* pp. 59–61.
10. Hartmann, *Adventures in Research,* pp. 215 and 221.
11. Ibid., pp. 216–18.
12. H. J. Allen and A. J. Eggers, Jr., "A Study of the Motion and Aerodynamic Heating of Ballistic Missiles Entering the Earth's Atmosphere at High Supersonic Speeds," NACA Conf. Research Memo. A53D28, Aug. 1953. The NACA published updated versions of this same report as TN 4047 and TR 1381 (1958).
13. *Adventures in Research,* p. 218.
14. Shortal, *A New Dimension,* p. 238.
15. Conceptual development of the "winged V-2" can be traced back at least to Sänger's "Über einen Raketenantrieb für Fernbomber," which, though produced in 1940, contains handwritten notes which postdate 1940. In the LaRC Technical Library is an English translation of this paper, "Work on Rocket Drive for Long-Range Bomber and Skip Re-Entry," done later by the Central Intelligence Agency. See Eugen Sänger, *Rocket Flight Engineering,* NASA TT F-223 (Washington, 1965), as well as Walter Dornberger's "Hinter den Kulissen der V-2" ("Back-Stage Facts about the

V-2"), German text and English summary, Proceedings of the 3d International Astronautical Congress, Stuttgart, 1–6 Sept. 1952. Copy of Dornberger's presentation in LaRC Technical Library.

16. Robert J. Woods to NACA Committee on Aerodynamics, "Establishment of a Study Group on Space Flight and Associated Problems," 8 Jan. 1952, in X-15 file, LCF. Attached to this letter is a 2½-page memorandum from Walter Dornberger to Woods. In the same file, see also Milton B. Ames, Acting Assistant Director for Research, to Langley, "Research on space flight and associated problems," 10 July 1952.
17. PARD to Chief of Research, "Proposal for Hypersonic Research," 5 June 1953, B10-6 (Wallops), LCF. See John V. Becker, "The Development of Winged Reentry Vehicles, 1952–1963," unpublished, dated 23 May 1983, copy in LHA. The understanding gained from this 57-page MS as well as from a long interview (on 7 Dec. 1983) about it proved to serve as the basis of much of what follows in this chapter.
18. C. E. Brown, William J. O'Sullivan, and C. H. Zimmerman, "A Study of the Problems Relating to High-Speed, High-Altitude Flight," 25 June 1953. Copy in LaRC Technical Library under code CN-141, 504.
19. See, for example, E. P. Williams et al., "A Comparison of Long-Range Surface-to-Surface Rocket and Ram-Jet Missiles (Project Rand)," Rand Corp. rpt. 174, May 1950.
20. Becker, "Development of Winged Reentry Vehicles," p. 30.
21. Shortal, *A New Dimension,* p. 208.
22. "Development of Winged Reentry Vehicles," p. 5.
23. Becker interview with author, 7 Dec. 1983.
24. Hubert M. Drake and L. Robert Carman, "A Suggestion of Means for Flight Research at Hypersonic Velocities and High Altitudes," May 1952, copy in X-15, LCF. On the cover sheet of Langley's copy of this once secret report there is a note in pencil, "HOLD UNTIL PROJECT IS COLD THEN DESTROY."
25. D. G. Stone to Chief of Research, "Preliminary study of the proposal for the flight of manned vehicles into space," 21 May 1952, ibid.
26. See J. W. Crowley to Langley, "Request for comments on possible new research airplane," 9 March 1954, E30-6, LCF.
27. "Development of Winged Reentry Vehicles," p. 2.
28. Ibid., pp. 7–8.
29. See John Duberg's "Remarks on the Charts Presenting the Structural Aspect of the Proposed Research Airplane" (given at NACA headquarters, 9 July 1954), X-15, LCF.
30. See relevant correspondence in Langley "special file" on X-15 dated Jan. 1952 through Sept. 1954, X-15, LCF.
31. "Development of Winged Reentry Vehicles," p. 9.
32. See "Summary of Results" in "Research Airplane Study," April 1954, pp. 20–24; copy of report in X-15, LCF.
33. In the original "Research Airplane Study," there was no definite recommendation for air launching or any other specific method of flight takeoff (see pp. 7–8 of study); there was only a comparative estimate of the performance of the assumed airplane with various possible methods of launching. In the conclusion of the study, the authors did state that "for initial operations, launching from the B-52 would permit Mach numbers up to about 6.3 to be determined" (p. 23).

34. The influence of Thompson's suggestion is obvious in "Results of Research Airplane Study," presented by Becker and Duberg at NACA headquarters on 9 July 1954. An abstract of these "Results" was incorporated into the document "NACA Views Concerning a New Research Airplane," Aug. 1954, a copy of which is in X-15 file, LCF.
35. "Development of Winged Reentry Vehicles," p. 10.
36. "NACA Views Concerning a New Research Airplane," p. 5.
37. Becker, "The X-15 Project, Part I: Origins and Research Background," *Astronautics and Aeronautics* (Feb. 1964): 56.
38. Arthur Henderson, Jr., "Wind-Tunnel Investigation of the Static Longitudinal and Lateral Stability of the Bell X-1A at Supersonic Speeds," NACA Research Memo. L55I23, Oct. 1955; for flight research results at Edwards, see Hubert M. Drake and Wendell H. Stillwell, "Behavior of the X-1A Research Airplane during Exploratory Flights at Mach Numbers near 2.0 and at Extreme Altitudes," NACA Research Memo. H55G26, Oct. 1955.
39. "The X-15 Project, Part I," p. 56.
40. The NACA published McLellan's detailed description of his wedge-tail scheme in August 1954 as Conf. Research Memo. L544F21, "A Method for Increasing the Effectiveness of Stabilizing Surfaces at High Supersonic Mach Numbers."
41. "The X-15 Project, Part I," pp. 56–57.
42. Navy representatives disclosed at this July 1954 meeting that the Bureau of Aeronautics had just contracted with the Douglas Aircraft Company for a feasibility study of a manned aircraft capable of flying to an altitude of one million feet. The representatives also reported that the early results of this study indicated that, from a standpoint of reentry deceleration, it appeared possible to fly to at least 700,000 feet. Becker and Duberg responded to this news by saying that Langley had analyzed the structural heating problem and that its results indicated a peak altitude of only about 400,000 feet. See "The X-15 Project, Part I," p. 57.
43. "Minutes of Meeting, Committee on Aerodynamics, October 4–5, 1954," copy in X-15 file, LCF.
44. Johnson was making a point in 1954 about the whole research airplane program which Becker made in *High-Speed Frontier* (p. 96) only in regard to the D-558-1.
45. "Minutes of Meeting, Committee on Aerodynamics, October 4–5, 1954," p. 16.
46. Ibid., pp. 16–17.
47. Clarence L. Johnson to Milton B. Ames, Secretary, Committee on Aerodynamics, "Minority Opinion of Extremely High Altitude Research Airplane," 21 Oct. 1954; attached to "Minutes of Meeting," X-15 file, LCF.
48. See Wendell H. Stillwell, *X-15 Research Results, With a Selected Bibliography,* NASA SP-60 (Washington, 1965).
49. "Development of Winged Reentry Vehicles," p. 9.
50. Though the military services and the NACA both believed that the HYWARDS program should be advanced immediately, they also felt that it should be done with discretion so as not to jeopardize the X-15 project, which was still having funding problems. "The Development of Winged Reentry Vehicles," p. 20.
51. Ibid., p. 15.
52. Becker to Floyd L. Thompson, Associate Director, "Hypersonic Research Airplane Study," 17 Jan. 1957, copy in John Stack Collection, LHA.
53. Ibid. See also "Development of Winged Reentry Vehicles," fig. 1.

54. Peter Korycinski to F. John Bailey, referring to contents of the first formal presentation by the Becker group of its HYWARDS study, 25 Jan. 1957; copy of letter appended to "Development of Winged Reentry Vehicles."
55. *Adventures in Research,* pp. 266–270.
56. Peter Korycinski to Hartley A. Soulé, Research Airplanes Panel Leader (RAPL), "Report on Interlaboratory Round III discussions," 1 Nov. 1957, A200-1A, LCF.
57. "Development of Winged Reentry Vehicles," pp. 18–19.
58. Ibid., p. 25.
59. Ibid., p. 22.
60. On the American reaction to Sputnik, see Walter A. McDougall, ... *The Heavens and the Earth: A Political History of the Space Age* (Johns Hopkins University Press, 1985), especially pp. 141–157. The author previewed his larger study in "Technocracy and Statecraft in the Space Age: Toward the History of Saltation," *American Historical Review* 87 (Oct. 1982): 1010–1040. For a shorter analysis of the Sputnik crisis and its specific impact on the NACA, see Roland, *Model Research,* pp. 290–291.
61. See A. J. Eggers, H. J. Allen, and S. Neice, "A Comparative Analysis of the Performance of Long-Range Hypervelocity Vehicles," NACA Research Memo. A54L10, 10 Dec. 1954.
62. "Development of Winged Reentry Vehicles," p. 23.
63. McDougall, *The Heavens and the Earth,* p. 154.
64. Ira H. Abbott, "A Review and Commentary of a Thesis by Arthur L. Levine," p. 195.
65. *Model Research,* p. 292.
66. Robert R. Gilruth, J. V. Becker, C. J. Donlan, and E. C. Draley to LAL Director, "Review of Langley Research Effort," 31 January 1958, A200-1A, LCF.
67. "Development of Winged Reentry Vehicles," p. 31–32.
68. Chapman's paper was published along with all 42 of the other papers presented in *NACA Conference on High-Speed Aerodynamics, Ames Aeronautical Laboratory, Moffett Field, Calif., March 18, 19, and 20, 1958: A Compilation of the Papers Presented,* (NACA: Washington, 1958), pp. 1–18.
69. Ibid., p. 19.
70. Eggers's explanation was cited in Lyod S. Swenson, Jr., James M. Grimwood, and Charles C. Alexander, *This New Ocean: A History of Project Mercury,* NASA SP-4201 (Washington, 1966), p. 89.
71. "Development of Winged Reentry Vehicles," pp. 33–34.
72. Ibid., p. 34. The best essay on this technological history to date is Richard P. Hallion, *The Path to the Space Shuttle: The History of Lifting Reentry Technology* (Washington, D.C.: U.S. Air Force, 1983).
73. *This New Ocean,* pp. 93–97.

Epilogue

1. Robert R. Gilruth, "Memoir: From Wallops Island to Mercury, 1945–1958," unpublished manuscript presented at the Sixth International History Symposium, Vienna, Austria, 13 Oct. 1972, p. 35, copy in LHA.
2. Ibid., p. 37. For Gilruth's testimony to Congress, see *Hearing Before the Select Committee on Astronautics and Space Exploration, Eighty-Fifth Congress, Second Session, on H.R. 13619* (Washington, 1958), pp. 17–21.

3. Gilruth, "Memoir," pp. 46–47.
4. Ibid.
5. Ibid., pp. 42–43 and 47–48. For information on the competition for the Mercury space capsule contract, see documents in "Special File, Mercury Project, 1958 through Feb. 1959," in LCF. Of particular note in this special file is the agenda for the "Briefing for Prospective Bidders for Manned Satellite Capsule," 7 Nov. 1958, and Henry Reid's letter to fifteen different addressees, "Information for bidders of manned satellite capsule," 19 Nov. 1958, to which numerous data plots and blueprint designs are attached.
6. Gilruth, "Memoir," pp. 42–43; see also John W. Crowley, Director of Aeronautical and Space Research, NASA Headquarters, to Langley, "Langley support of manned space vehicle development," 11 Feb. 1959, in "Special File, Mercury Project," LCF. Nearly every piece of correspondence in this special file documents some aspect of Langley's support of Project Mercury.
7. See Loyd S. Swenson, Jr., James M. Grimwood, and Charles C. Alexander, *This New Ocean: A History of Project Mercury,* NASA SP-4201 (Washington, 1966), pp. 125–126.
8. See RA A8-L8, "Planning and Contracting, Instrumentation and Control Center Facilities for the NASA Manned Satellite," issued 4 March 1959, copy in "Mercury Project, Jan. 1959–March 1959," LCF.
9. Albin O. Pearson to Langley's associate director, "Visit of NASA personnel to AEDC, Tullahoma, Tennessee, for the purpose of discussing the testing of models of the McDonnell (Project Mercury) capsule in the AEDC facilities," 5 Mar. 1959, in "Mercury Project, Jan. 1959–Mar. 1959," LCF.
10. NASA headquarters also received regular reports from the Langley Space Task Group: see "Project Mercury, Status Report No. 1 for Period Ending January 31, 1959," confidential, copy in "Mercury Project, April 1959," LCF. (This Jan. 1959 status report is in the Apr. 1959 correspondence file because it is attached to a 15 Apr. 1959 cover letter from Gilruth to NASA headquarters.)
11. Raymond L. Bisplinghoff, "Twenty-Five Years of NASA Aeronautical Research: Reflections and Projections," in *Space Applications at the Crossroads: 21st Goddard Memorial Symposium,* eds. John H. McElroy and E. Larry Heacock (San Diego, 1983), pp. 30–31.
12. For signs of the changes made by Cortright, one need only examine the organization charts in Langley's telephone directories from 1968 through 1970. (A nearly complete collection of the center's telephone books, dating from the late 1940s, is in the LHA.)
13. See Arnold S. Levine, *Managing NASA in the Apollo Era* (Washington, D.C.: NASA, 1982), pp. 256–258.
14. Thompson interview with Walter Bonney, Hampton, Va., 27 Mar. 1973, p. 25.
15. NASA SP-468, p. ix.
16. Ibid., p. 3.
17. For an enlightening study in the uncertainties of engineering knowledge, look for Walter Vincenti's essay "The Davis Wing and the Problem of Airfoil Design," to be published in *Technology and Culture* in 1986.
18. "Engineering in an Increasingly Complex Society: Historical Perspectives on Education, Practice, and Adaptation in American Engineering," p. 123, in *Engineering in Society: Engineering Education and Practice in the United States,* a report sponsored by the National Research Council (Washington, D.C.: National Academy Press,

1985). One of the eight recommendations of the NRC's Committee on the Education and Utilization of the Engineer states that "in order to retain the responsiveness of engineers and of the overall system, engineering schools should not introduce greater specialization into their curricula. Instead, they should continue to emphasize basic skills and interdisciplinary study" (p. 73). Langley's history certainly supports this recommendation.

Guide to NACA Historical Sources at Langley

Since this book is amply footnoted by chapter, and since so little is generally known about the recently constituted archives that provided most of my source material, I have chosen to provide readers with the following general-purpose guide to the major sources for research into NACA history at NASA Langley Research Center instead of with the traditional bibliographic essay. Those seeking guidance about historiography relevant to the NACA or about other archival collections with significant NACA materials in them should consult Alex Roland's *Guide to Research in NASA History,* 4th ed. (Washington: NASA History Office, 1984), or the extended bibliographic essay in Roland's *Model Research: The National Advisory Committee for Aeronautics, 1915–1958,* NASA SP-4103 (Washington, 1985), 1:300–305, both of which are richly informative and analytical.

Langley Research Center, the oldest laboratory of the NACA and NASA, possesses an historical collection that in combination with its technical library rivals any archives for aerospace history in this country. In Langley's archives (see diagram) are collections of rare books and photographs, technical reports, office memoranda, flight and wind tunnel logs, programs and minutes of major technical conferences, personal papers, transcripts of interviews with key personnel, as well as scale models of aircraft and spacecraft and other illuminating artifacts. Besides storing Langley's own historical records, the archives also include important files of the Wallops Island (Va.) rocket test range, created in 1945 as an auxiliary base of Langley laboratory and managed by Langley as part of the Pilotless Aircraft Research Division (PARD) until 1958, when Wallops achieved independent status as a NASA center.

The four most important collections in the Langley archives are (1) the NACA correspondence files, (2) the NACA research authorization files, (3) the Milton Ames Collection, and (4) the personal papers of Floyd L. Thompson and John Stack. Since they are so important, these four collections will be described fully below. Key aspects of Langley's Floyd L. Thompson Technical Library are also discussed.

Correspondence Files

Anyone who plans to do research in an organization's archives should first ask some questions about the correspondence policy in force for that organization's employees: Could they send letters directly to outside addresses? Did all letters have to go through a central office? What management official, if any, had to "sign off" the letters? Knowing the peculiarities of the correspondence policies will make it much easier to evaluate and utilize the records.

In Langley's case, a superb historical archive was created as the by-product of a tight-to-the-vest correspondence policy and a highly centralized filing system. Largely as a result of the early and continuing control by NACA executive secretary John F. Victory over the lab's bureaucratic affairs (see chapter 2), all of its outgoing correspondence was reviewed and revised up through the division level until sanctioned in its final form by the office of the chief of research; then it was signed by the engineer-in-charge, the top

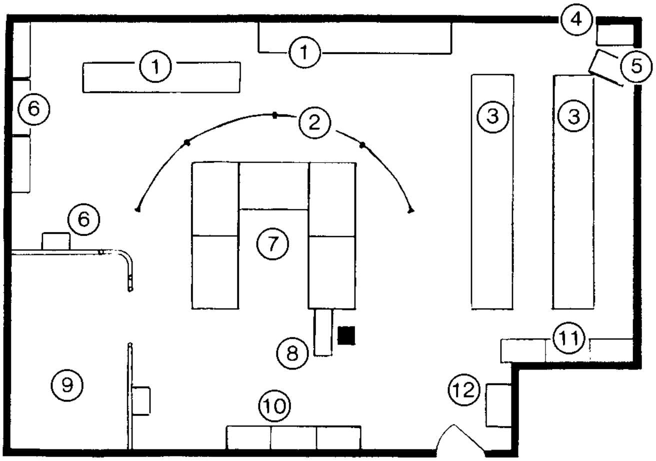


Diagram of the Langley Historical Archive (on the second floor of the Floyd L. Thompson Technical Library at NASA's Langley Research Center in Hampton, Virginia): (1) NACA research authorization (RA) files. (2) Panels depicting early aviation, painted at Langley in the 1940s and formerly displayed in the rotunda of the NACA Langley headquarters building. (3) NACA and NASA correspondence files from 1917 through early 1960s; special photographic collections; NACA and NASA visitor registers; index and cross-reference card files for NACA research authorizations. (4) Special file with index cards of technical reports produced by NACA Flight Research section. (5) Aircraft flight logs. (6) Special model and artifact displays. (7) Special model and document displays. (8) Complete collection of bound NACA annual reports. (9) Historian's office; five-foot shelf of NACA and NASA history books. (10) Milton Ames Collection: very early NACA photographs (many unique); complete collection of Langley Air Scoop; *special NACA files on various matters including personnel, facilities, and congressional activities. (11) Telephone directories; special aircraft files; special collection on hydrodynamic research; special collection on Viking Project; 50th anniversary photographic and administrative file; annual inspection files from 1926 on. (12) Floyd L. Thompson's personal papers; John Stack's personal papers; Max M. Munk's book collection.*

man in the laboratory organization. Incoming letters to individuals were routed directly to them, but only after being opened by the mail clerks. Copies of all letters, incoming and outgoing, were made for central files. Each letter was placed into one or more subject files, which were organized according to an alphanumeric code unique to Langley. Within each subject, papers were then arranged by date.

There are two catalogs to the correspondence file codes in the Langley archives, one that is alphabetical by subject and the other that follows the alphanumeric code; both are the products of the lab's mail filing operation. To illustrate the nature of these catalogs, the contents of their respective first pages are reproduced below.

Subject Guide, Alphabetical

Subject	Code Number
Aberdeen Proving Grounds	B10-3
Accelerometers	A184-8A
Accident Investigation Board—Langley	E1-11
Accidents—Ames	E1-12
Accidents—Lewis	E1-17
Accidents—Edwards	AF252-2
Accidents—Langley	AF252-1
Acoustics	A313-1
Administrative Policy and Procedure	E30-12C
Advanced Research Projects Agency (ARPA)	E20-6
Advisory Group for Aeronautical R&D (AGARD)	E2-12B
Aerial Spraying	B10-1
Aeroelasticity	A178-2
Aero Medical Association	E34-17
Aeronautical Symbols	E1-13
Aerospace Industries Association	E6-7
Agriculture Department	B10-1
Air Force	B10-2
Aircraft Companies—General	A173-4
Alsos Mission	E2-12C
Altimeters	A184-8H
Aluminum and Aluminum Alloys	A311-2
Ames Research Center	B10-6
Angle of Attack—Instruments	A184-8D
Antennas (Radio)	A173-7
Apprentice Program	C48-25

Alphanumeric Guide

Code	Subject
A170-1	Aerodynamic Theory
A172-1	Aerodynamics Committee—Langley
A173-1	Airfoils
A173-1A	Wings—Swept (Back and Forward)
A173-2	Research Equipment Facilities (American Non-NACA)
A173-2A	Equipment (NACA) on Loan to Outside Sources
A173-5	Airplanes—General
A173-5A	Hypersonic Aircraft
A173-5B	Helicopters—General
A173-5C	Privately Owned, Personal or Light Aircraft
A173-5D	Atomic Energy Commission
A173-5E	Airplanes—Disposition
A173-5F	Windshields and Cockpit Visibility Problems
A173-5H	Coupled Airplanes
A173-5J	Convertiplanes (Vertically Rising Aircraft, Except Helicopters)
A173-5K	Quarterly Status Reports on Projects Relating to Research Airplanes
A173-5L	Ground Cushion Phenomena
A173-5M	Use of Center Airplanes
A173-5N	NASA Aircraft Utilization Reports
A173-5P	Air Traffic Control
A173-5R	Integrated Programs for Aerospace Vehicle Design (IPAD)
A173-6	Bombs

Langley's correspondence files are a largely untapped reservoir for aerospace history. Only a few weeks before this book went to press I found in this collection, for example, the following letter, dated the eighth of July 1920. It was addressed to Leigh Griffith, Langley's first engineer-in-charge, and was written by a young man in California who was just getting into the aircraft business: Donald W. Douglas, the soon-to-be-famous airplane designer.

> 2331 Fifth Street
> Ocean Park, Calif.
> July 8, 1920
>
> Dear Leigh:
>
> I suppose that you know that I am out here in your old town trying to make a go of the aircraft business here now. I left Martin the end of March, and spent some time trying to get something in a large way going here. I found this impossible at this time as the financial situation is rather uncertain. Luckily however I found a young chap with money here to back me in a small way. We are organizing under the name of the Davis-Douglas Co.
>
> I am at work now on the engineering of our first job, and expect to start construction of it in a shop that I have rented down town, before the end of the month. We have hopes that we will have it in the air in November. The ship that I am laying out is a commercial type around a single Liberty motor,

> and embodies several new schemes that on paper seem to add a great deal to the efficiency and cheapness of it. This first ship is going to be made special to do some record and stunt tricks with. I am carrying 600 gallons of gasoline and hope to be able to make the trip from here to New York non-stop, in about thirty hours. This will be quite an achievement and should give us the necessary advertising to help us to get further business.
>
> I have thought that if we were able to equip the plane with a supercharger and an adjustable pitch propeller, we might be able to pick up enough speed at about 18,000 feet to make the trip in considerably less time. I wrote the Army bunch in Dayton and they told me that they would not give the General Electric Co. permission to sell us or loan us one of the Moss superchargers at this time. Some weeks ago I went in to see your father [owner of a machine works from which Douglas's company was getting machined parts] and he told me that they had built a supercharger for the National Advisory committee. I am wondering if it is at all possible that the Advisory committee might regard our attempt seriously and take the opportunity of getting some long distance data on their supercharger, by lending it to us for the flight. There is no question but that we will make the attempt with or without it, and I really would imagine that we might be able to get some very valuable dope on the supercharger and its use in this sort of work, for the Advisory Committee. I wish that you would let me know if there is any possibility of them considering such an arrangement, and if they do, that you would send me the data on it, as soon as possible. I am at a stage now where I could allow room for its installation if I knew what it was like. Also tell me what you think of this particular supercharger, and what tests it has been put to.
>
> I certainly do enjoy being back here, and the kids and the wife are of course getting a lot out of it too. I hate to make you homesick, so I won't tell you how much the same as old times it seems back here. I miss you at the Vidamar noons when I get in there, but Tiny is back on the job again, and so it looks much the same as ever. Why don't you come back out to God's country again. I sure hope that I can make a living out here and get things humming again in an aeronautical way here.
>
> Well Leigh I would appreciate very much having some news about your personal welfare, as well as the dope on this supercharger stuff. Charlotte joins me in sending the best of regards to you and Mrs. Griffith.
>
> As ever, yours sincerely
> [Signed] Donald W. Douglas

Brief further research indicates that Griffith went out of his way to help Douglas find a suitable supercharger. He sent the young airplane designer an assembly drawing showing the NACA Root-type supercharger as applied to a Liberty engine (an application which had never actually been made), but he advised him against using this particular device on Douglas's proposed airplane because the device was still in a preliminary test phase of design.

The information in this letter very likely fits into the history of Douglas's round-the-world biplanes, the famous "World Cruisers" of 1924, which constituted his company's first big order. In any case, the letter exemplifies the valuable and yet-to-be-used historical information in the correspondence files of the Langley archives.

Research Authorization Files

Although the correspondence files are tremendously valuable, the single most important source for aeronautical history at Langley is the NACA research authorization files. These files permit the historian to recreate the entire NACA research procedure for a given project from the raw research idea through the final polished report.

What exactly was an NACA research authorization? Whenever a project for research at Langley was approved by NACA headquarters, a research authorization (or RA) was signed by the chairman of the executive committee and forwarded to the lab for execution. Technically Langley was supposed to have an RA for each of its investigations, and each RA was expected to lead to the publication of an NACA report. Each RA had a title and a number, and each included information on the how and why of the investigation. Sometimes this information was stated very briefly and rather vaguely; other times it was expressed at great length and in detail. From the time of the authorization on, a copy of any letter or document, incoming or outgoing, that in any way concerned the subject of the RA was filed chronologically in the specific RA folder (as well as in the appropriate correspondence files). Thus by studying the RA files one can get a pretty clear idea of how the NACA went about its business. The files shed light on such things as the respective roles of headquarters and the lab in selecting and conducting research projects, the publications policies of the Committee, and the relations of NACA staff members with clients and colleagues.

Since there are over 2000 research authorization files, this collection provides virtually virgin territory for historical research. I looked in detail only at two or three dozen RA files. In preparation for *Model Research*, Alex Roland examined, I presume, about the same number. Clearly scholars have so far only scratched the surface of this prime source for NACA history.

The RA files are maintained in sequence in the archives from RA No. 1, "Comparison of mathematical analysis and model tests of air propellers," issued 18 July 1918, through RA No. 1584, "Free-fall tests to determine stability derivatives of Dove guided missile," issued 24 November 1950. (RAs after 1950, at present stored elsewhere, will be moved into the archives.) In the archive there is also a card file to the RA collection that cross-references subjects and titles of technical reports with RA numbers and the file codes of correspondence.

Still, the RA files are not easy to work with. Because of the vague and rather indiscriminate nature of the majority of RA titles and the built-in flexibility of RA procedure (discussed in chapter 2), it can be very hard now for anyone, even the talented and experienced Langley file clerks and librarians, to match individual research projects to the specific RA, or RAs, that covered them administratively. The best example of this difficulty during research for this book was my attempt to find the RA covering the preparation of the "Theory of Wing Sections of Arbitrary Shape," an important paper written by Langley physicist Theodore Theodorsen in 1931 and published by the NACA as tech. rpt. (TR) 411. (The contents of this paper are analyzed in chapter 4.)

On the day I was working on this problem, veteran Langley engineer Axel T. Mattson visited my office, and I enlisted his help to solve it. The logical first step was to identify all

RAs originating before 1931 whose titles most closely matched the subject of Theodorsen's paper. With Mattson's help, I narrowed down the possibilities to nine RAs, all of which in one way or another concerned airfoils or wing sections:

No. 43—"Pressure distribution for thick airfoil sections"
No. 77—"Pressure distribution over tapered thick airfoils"
No. 203—"Study of characteristics of very thick airfoil sections"
No. 206—"Investigation of airfoils tapered in form and section"
No. 217—"Investigation of a series of wing models with flat lower surfaces and varying upper cambers"
No. 254—"Investigation of methods of developing airfoil shapes to obtain desired characteristics"
No. 290—"Investigation of effect of thickness and mean camber line shape on airfoil characteristics"
No. 350—"Determination of standard design characteristics for certain airfoils"
No. 351—"Investigation of compressibility effects on airfoils"

We deemed RA 254, "Investigation of methods of developing airfoil shapes to obtain desired characteristics," our best chance, as it matched the subject of Theodorsen's paper most closely, and then ranked the other eight RAs in order of our evaluation of their relevancy.

One by one we pulled dusty RA files down from their shelves, with no success. Having spent about an hour exhausting our list of nine RAs, Mattson and I went back to the complete list of RA titles I had prepared early in my research. We found ten more RAs that we thought might have covered Theodorsen's wing-section analysis, but none of their titles actually looked anywhere as promising as had those of our first nine. We examined the first RA on this second list—No. 236, "Investigation of wing flutter"—because we knew that its subject was one of Theodorsen's specialties. When RA 236 also proved a washout, we gave up the idea of proceeding further with our method.

I then asked Mattson which research section Theodorsen had worked in during 1929 and 1930; I knew that Theodorsen became head of the Physical Research Division when it was created in 1931, but was unsure where he had worked previously. Though Mattson did not come to work at Langley until just before World War II, he knew the older crowd and replied that he thought Theodorsen had been a member of the Atmospheric Wind Tunnel (AWT) section.

Having failed to find anything in the RA files, I now decided to look into the correspondence files for contemporary records of the AWT section. Entry into these records was easy, thanks to the alphabetized subject guide discussed above. Mattson and I looked through some letters from the late 1920s and early 1930s, and though we saw nothing dealing with Theodorsen's paper, we did see in the upper left-hand corners of many of the letters, in parentheses next to the Central File code, the numbers 88 and 237. We knew that these numbers were cross-references to RA numbers where other copies of the same document had been filed by Langley clerks. The numbers meant that the administration of much of the work being done in the AWT section during this period had been covered by RAs 88 and 237.

RA 88, "Investigation of scale effect on airfoils," and RA 237, "Investigation of lateral stability with particular reference to rotary stability at large angles of attack," did not seem to match the subject of Theodorsen's paper. Of the two RAs, however, No. 88 looked closer to it. So we got a few folders of 88 down from the shelf and started looking through them. We found a memo with a note penciled in at the bottom, "TR 411 changed to RA 237."

TR 411 was the published NACA technical report by Theodorsen which we were looking for, so Mattson and I knew that our hunt was over. Mattson was astonished to find where it had led, for RA 237, "Investigation of lateral stability with particular reference to rotary stability at large angles of attack," had *nothing whatever* to do with the subject of Theodorsen's paper. In fact, Mattson noted (and I agreed completely) that RA 237 would have been just about the last place we would have looked for the administrative records of Theodorsen's work. RA 237 covered TR 411 simply because it was an RA which was generally blanketing a number of diverse research projects then being conducted at Langley by the AWT section. Scholars wishing to use the NACA research authorization files may benefit by keeping our experience in mind.

Milton Ames Collection

A third important collection of historical documents in the Langley archives is the Milton Ames Collection. In the early 1970s Ames, an ex-Langley engineer who had served as chief of aerodynamics at NACA headquarters from 1949 to 1958, began research for what he hoped would be a complete and publishable history of the laboratory. Although he did not achieve his goal, Ames did pull together hundreds of significant documents. Organized into folders which he titled and deposited into seven oversize boxes, the Ames Collection is stored—according to the original box scheme and folder titles—in file cabinets in the LaRC archive.

The Ames Collection is especially enlightening because it was created by an old NACA hand, a product of the institutional culture under investigation. The documents he found significant enough to include for research tell us something about both Ames's identity as a member of the NACA "corporation" and his approach as an engineer to historical understanding. Furthermore, since Ames was one of the NACA's most talented and forward-looking aerodynamicists, his choice of key technical papers for historical examination is very helpful to the nonspecialist.

The entire collection, comprising seven boxes, is outlined below.

Contents of Box No. 1

WRIGHT BROTHERS

- Articles on early flights from:
  - *The Croatan Courier*, 17 Dec. 1936
  - *Journal of the American Historical Society,* Fall 1966
  - *Collier's,* 25 Dec. 1948
  - *Air Scoop* [Langley in-house newsletter], 22 June 1953
  - *Air Scoop,* 22 Dec. 1944
  - *LMAL Bulletin,* 18–24 Dec. 1943
- Reprint from *Above and Beyond,* 1968
- Folder, Kill Devil Hill National Memorial
- Original army contract with Wright brothers, 1907–1908
- Miscellaneous photographs, Wright *Flyer*

ESTABLISHMENT OF BRITISH ADVISORY COMMITTEE FOR AERONAUTICS

- Papers, notices, 1909 interim report, 1909–1911 report

NEED FOR AN AERONAUTICAL LABORATORY IN AMERICA

From Dr. Zahm's papers, nos. 33, 34, 40, and 54

SMITHSONIAN ADVISORY COMMITTEE ON THE LANGLEY AERODYNAMICAL LABORATORY

Dr. Zahm's paper no. 53

SURVEYS OF AERONAUTICAL LABORATORIES IN EUROPE, 1913–1920

Papers by:
  Dr. Zahm, nos. 42, 43, and 55
  Dr. Hunsaker, articles from *Flying,* 1914
  William Knight, NACA Technical Note 17, 1920

AERONAUTICAL RESEARCH IN CANADA

AGARD paper by J. H. Parkin, June 1955
"First Wind Tunnel Constructed and Placed in Operation," 1919

EARLY HISTORY OF AERONAUTICAL RESEARCH IN GERMANY

Schlichting letter listing references [Hermann Schlichting, Univ. of Göttingen]

MISCELLANEOUS PAPERS ON AVIATION UP TO ESTABLISHMENT OF NACA

Dr. Zahm's paper no. 49
Three memoranda from Lee Dickinson to Milton Ames
Washington newspaper clippings

LEGISLATION PERTAINING TO NACA, AND APRIL 1958 SUMMARY

ESTABLISHMENT OF NACA

Articles from *Flying,* April and July 1915
War Department letter calling for first meeting of NACA
Letter from acting chairman to comptroller of Treasury, 27 July 1915
Dr. Walcott letter to Lt. Richardson, 8 April 1915
Letter from first chairman to president, 23 April 1915
Letter from Department of Commerce to Lt. Richardson, 19 October 1915
Miscellaneous papers, 1941–1950

NACA MEMBERSHIP, CHAIRMEN, ETC.

Articles from *Flying,* 1915
Summary of NACA membership, 16 March 1960

FIRST MEETING OF NACA

Minutes and photographs of members, 1915

LANGLEY SITE SELECTION AND TRANSFER OF LAND TO NACA

NACA STATEMENT OF POLICY, OCTOBER 1917, AND EXECUTIVE ORDER DATED 20 MAY 1918

MEMORANDUM OF UNDERSTANDING WITH THE ARMY RE USE OF LANGLEY FIELD BY NACA, 1919

SUMMARY OF IMPORTANT EVENTS IN EARLY HISTORY OF NACA, 1915–1917 (SUMMARY PREPARED DECEMBER 1929)

NACA PARIS OFFICE (ESTABLISHED MAY 1919)

MISCELLANEOUS PAPERS ON AERONAUTICAL RESEARCH IN USA, 1921–1925

"Aeronautical Research in USA," Edward P. Warner, May 1921
"Making America Independent in the Air," *Mechanical Engineering,* September 1923
"Aeronautics in the Government—The National Advisory Committee," Charles D. Walcott, June 1925
"A Chronology of U.S. Aviation," *Aircraft Year Book,* 1949

EARLY REVIEWS AND SUMMARIES—NACA AND LANGLEY

NACA library reference listing regarding NACA, Nov. 1943
Early talks by E. R. Sharp regarding origins of NACA and Langley
LMAL chronology through 1933
British view of NACA, by Sir Roy Fedden
John F. Victory interview with representative from Langley AFB historical office, 1944
NACA press release regarding need for national aeronautical policy, 1922
Chapters from various versions of John F. Victory's NACA history:
  "History and Development of the NACA"
  "Some Direct Accomplishments of the NACA"
  "Accomplishments"

MISCELLANEOUS LANGLEY BACKGROUND INFORMATION

Murals in the Administration Building
House organization
Miscellaneous papers

LANGLEY FIELD, VIRGINIA—HISTORY AND CONSTRUCTION (AIR CORPS VIEWPOINT)

Aviation Edition, *Hampton Monitor*—July 1918
Commanding officers, Langley Field, 1917–1946
Excerpt from *Look Homeward, Angel* (reference to construction of Langley Field during World War I)
Photographs—Mitchell bombing results
"Langley Field 1917–1945," *Journal of the American Aviation Historical Society,* Spring 1965
Colonel Carl F. Greene, AF liaison officer
"Early Langley Field Aircraft," *Journal of the American Aviation Historical Society,* Fall 1966

LANGLEY LAND RECORDS AND DEEDS

Original land records (Elizabeth City County)
Early deeds regarding Langley
Langley plats—photographs of clerk of court's documents
George Wythe birthplace

EARLY CONSTRUCTION, LANGLEY RESEARCH STATION

Correspondence regarding White Engineering Company, 1918–1919
Atmospheric Wind Tunnel, papers about land allotment and construction

DEDICATION OF LANGLEY (11 JUNE 1920)

Correspondence regarding official opening of the wind tunnel and dedication of the laboratory
Invitation list
Speech of Rear Adm. David W. Taylor
Expenditures for entertainment
Official designation of field station as "Langley Memorial Aeronautical Laboratory"
Photographic copies of 12 July 1920 editions of Newport News (Va.) *Daily Press* and *Times Herald*

VARIABLE DENSITY WIND TUNNEL—CONSTRUCTION

Land allotment
1924 report on design and operation
Copy of VDT logbook entry, 1 Aug. 1927, regarding wind tunnel fire
1931 and 1935 papers regarding VDT modifications

Contents of Box No. 2

LANGLEY ORGANIZATION CHARTS

Correspondence on organization, and original organization chart
John Victory's draft chapter, "The Langley Laboratory" (discussing early organization and difficulties with the army)
Folders on papers regarding early organization
Files on Langley organization prior to 1952
Langley organization charts, in order from 1952
Miscellaneous selected files regarding Langley organization

LANGLEY PERSONNEL AND PERSONNEL ACTIVITIES

Folder on early personnel
General background material and personnel statistics (complement 1919–1965)
Statistics regarding personnel
Personnel training
NACA emblems
Bond drives, etc.
Cafeterias
Green Cow [early social club]
Community cooperation
Activities building and related material
Miscellaneous material on personnel
Recruiting brochures

ESTIMATES OF LANGLEY PLANT COSTS

1934 estimates
1936 estimates
1943 estimates

ECONOMIC VALUE OF NACA RESEARCH (SUMMARY, 1937)

PRELIMINARY (LANGLEY) DATA ON NACA BUDGET (1915–1952)

EFFORTS TO TRANSFER NACA FROM INDEPENDENT AGENCY TO OTHER AGENCIES

Proposed transfer to Bureau of Standards and War and Navy departments, 1921
Proposed transfer to Commerce Department, 1925
Proposed transfer to Commerce (Bureau of Standards), 1932–1933
McKeller Bill (S-5044), proposed abolishment of NACA and transfer of its laboratories to War Department, 1933

LANGLEY INSPECTIONS (ORIGINALLY CALLED MANUFACTURERS' CONFERENCES)

Joseph Ames's papers—First Manufacturers's Conference at Langley, 24 May 1926
Henry Reid's papers—conference books, 1926–1935
Edward P. Warner's proposal for additional industry representatives on NACA committees and subcommittees
Folder on post–World War II inspections and anniversaries

Contents of Box No. 3

PHOTOGRAPHIC FILES

Early pictures selected from *Air Scoop*
Miscellaneous photographs, 1919–1935
H. J. E. Reid photographic albums:
  1. Flight section
  2. Wind tunnel section
Selected correspondence on Langley photographs

LOGBOOKS OF EARLY EXHIBITS

LANGLEY VISITORS' REGISTER, 1926–1934

Contents of Box No. 4

WILBUR WRIGHT MEMORIAL LECTURES

List of lecturers from 1913 through 1935
1918, William F. Durand, "Some Outstanding Problems in Aeronautics"
1923, Joseph Ames, "Relation Between Aeronautics Research and Aircraft Design"
1932, H. E. Wimperis, "New Methods of Research in Aeronautics"
1939, George W. Lewis, "Some Modern Methods of Research in the Problems of Flight"

1949, Hugh L. Dryden, "The Aeronautical Research Scene: Goals, Methods, and Accomplishments"
1961, Abe Silverstein, "Researches in Space Flight Technology"

FOLDERS ON KEY INDIVIDUALS ASSOCIATED WITH LANGLEY HISTORY

Ames, Joseph S.
Dryden, Hugh L.
Durand, William F.
Hunsaker, Jerome C.
Langley, Samuel P.
Lewis, George W.
Lindbergh, Charles A.
Reid, Henry J. E.
Sharp, Edward R.
Stack, John
Thompson, Floyd L.
Victory, John F.
Walcott, Charles D.
Zahm, Albert F.
Biographical material—miscellaneous

CLIPPINGS (1925–1930)

1933 HURRICANE

SPECIAL PUBLICATIONS—ANNIVERSARIES, HISTORIES

CONFERENCES, CEREMONIES, INSPECTIONS, VISITORS

ECONOMIC STUDY OF 1933 AND "NOTES ON AVIATION PROGRESS THROUGH RESEARCH"

LANGLEY HISTORY (COLLECTION OF PAPERS AND TALKS ON LANGLEY HISTORY)

MISCELLANEOUS PRESS RELEASES ON LANGLEY RESEARCH ACTIVITIES

MISCELLANEOUS CORRESPONDENCE REGARDING EARLY HEADQUARTERS-LANGLEY RELATIONSHIP

LANGLEY TELEPHONE DIRECTORIES, JAN. 1963 TO MAY 1971

Contents of Box No. 5

EARLY ENGINE COMPETITION (1920)

FATAL AIRCRAFT ACCIDENT REPORT, JN-6 44946, 20 AUGUST 1924

FORD RELIABILITY TOUR, 1926

CRASH OF THE "AMERICAN LEGION" AT LANGLEY, 26 APRIL 1927

RESEARCH ACTIVITIES DURING 1920S

Early model airplane use
Early noise study
Complaints on early flight reporting

- Langley documents on early research programs
- Cowling research
  - Lockheed Air Express
  - Propeller Research Tunnel
  - "Little America" (telegram regarding NACA cowling and modified propellers)
- Background to boundary-layer-control research
- Eastman Jacobs's laminar-flow work
- Rotary-wing aircraft

NACA PREPARATIONS PRIOR TO WORLD WAR II

- Policy regarding laboratory in time of war (George Lewis's memorandum dated 1 Dec. 1936 for chairman of special committee on policy regarding membership of employees in military reserves)
- Westover Committee report to NACA chairman, 19 Aug. 1938; subject: Relation of the NACA to national defense in time of war
- Initial report of Air Corps–NACA committee making recommendations for priority of research projects in LMAL program, 22 Dec. 1939
- Authorization to the NACA's director of aeronautical research from the NACA executive committee to carry out investigations for the army and navy for the duration of the war, and to issue research authorizations as required
- John F. Victory letter to NACA laboratories regarding views of high government officials on the NACA, 17 Feb. 1943
- NACA library listing of references pertaining to NACA preparation for war and support of World War II

LANGLEY CONTRIBUTIONS TO AMES AND LEWIS LABORATORIES

- Ames Laboratory
- Lewis Laboratory

LANGLEY ACTIVITIES DURING WORLD WAR II ERA

- NACA studies for U.S. Army Air Forces of factors affecting performance of advanced military aircraft
- Langley contributions to controlled bombs
- Miscellaneous files, World War II era
- Research studies for Army Air Forces and navy on supersonic aircraft (by Macon C. Ellis, Jr., and Clinton E. Brown)
- Wallops Island and miscellaneous related material

MEAD COMMITTEE INVESTIGATION—ORIGINAL CORRESPONDENCE

- U.S. Senate special committee investigating the national defense program, 1946

NATIONAL AERONAUTICAL RESEARCH POLICY, 21 MARCH 1946

POST–WORLD WAR II RESEARCH ACTIVITIES

- National program of transonic and supersonic research
- Transonic (slotted throat) wind tunnels

Flight research
Research airplane program

GAO SURVEY OF NACA, 1953

25TH ANNIVERSARY OF LANGLEY TOWING TANK AND FULL-SCALE WIND TUNNEL, 1956

NATIONAL AWARDS TO LANGLEY

Collier trophies
Air Medal for Herbert Hoover [NACA test pilot] for research airplane flight testing

EXTRA COPIES OF "AIR SCOOP"

MISCELLANEOUS AIRSHIP PHOTOGRAPHS FROM MELVIN N. GOUGH

Contents of Box No. 6

AREA RULE AND RICHARD WHITCOMB

LANGLEY CONTRIBUTIONS TO B-58

V/STOL RESEARCH

Summary papers, various authors
Charles Zimmerman's V-173

HIGH-SPEED SUBMARINE "ALBACORE" RESEARCH FOR U.S. NAVY

RESEARCH ON FLEXIBLE WINGS

LANGLEY SPECIAL GROUP ON RESEARCH FOR GUIDED MISSILES

(Only copy of file in existence)

LANGLEY RESEARCH FACILITIES

Wind tunnels, other facilities, and research techniques
Miscellaneous files on facilities

"NACA RESEARCH INTO SPACE," 1957

"ECHO I" AND WILLIAM J. O'SULLIVAN

EARLY MANNED SPACE FLIGHT

1958 proposals
Early Project Mercury articles
Project Mercury tracking range
Mercury astronauts

PROJECT APOLLO

Langley contributions
Genesis of lunar orbit rendezvous (LOR) concept
Miscellaneous material on LOR concept, including letters from John C. Houbolt to Dr. Robert Seamons

Langley working paper, "Preliminary Geologic Evaluation and Apollo Landing Analysis of Areas Photographed by Lunar Orbiter II," March 1967

Contents of Box No. 7

PAPERS AND TALKS RELATING TO HISTORY OF LANGLEY

- Papers by Jerome C. Hunsaker, 1941–1942
- Talks by John F. Victory, Hugh L. Dryden, Floyd L. Thompson, Arthur Regier, John Stack, and Axel Mattson, 1945–1954
- Talks by Langley and NACA officials
- Talks by Floyd L. Thompson
- Talks—miscellaneous
- Langley's 50th Anniversary
  - Floyd L. Thompson's opening remarks
  - Miscellaneous
  - Photographs
  - Anniversary plaque
- Miscellaneous publicity information
- Outside publications
- "America's Race for the Moon," story of Apollo Project in *New York Times*
- Miscellaneous technical papers by Langley authors
- Langley flight projects reference manual

Personal Papers

The Floyd L. Thompson Collection. Actually this collection holds more for the space historian than it does for the aeronautical historian. Most of its contents postdate the NACA; they derive from Thompson's term as director of the NASA Langley Research Center, 1960–1968. Box C of this collection, though, contains some important documents on NACA research dating back to the 1930s. (Thompson began working for the NACA at Langley in July 1926.) The entire collection of papers was donated to the LaRC archive in 1980 by Thompson's widow, Mrs. Jean Thompson. The following reproduces Floyd Thompson's own inventory of the subjects of the collection.

Box A

MORL (Manned Orbital Research Laboratory)
LUNAR ORBITER (historical notes)
APOLLO
MERCURY
SCOUT
X-15
SST
PASSIVE COMMUNICATIONS SATELLITE
LARGE BOOSTERS
MISCELLANEOUS TECHNICAL PROPOSALS AND MEMOS

Box B

EARLY SPACE PROGRAM PLANNING: MEMOS AND ORGANIZATIONS

VISITS AND EVENTS

- Mercury
- University of Tennessee
- ICASE (Institute for Computer Applications in Science and Engineering)—Munich
- Martin Plant—Denver, Colorado
- University of Michigan
- Boeing anniversary
- AGARD (Advisory Group for Aeronautical Research and Development, NATO)—Athens, Greece
- British flying display
- West Coast visits (Boeing, Lockheed)
- Miscellaneous

NEWPORT NEWS CYCLOTRON AND VARC (Virginia Associated Research Campus)

SPECIAL ASSIGNMENTS

- Elliot Committee regarding industrial funding
- Consultations on aeronautical development
- President's Advisory Council on Management Improvement
- ARPA workshop, May 1970
- NRC panel on hydrodynamics of submerged bodies
- NRC panel on submarines
- Committee for disposition of NASA artifacts
- Test site for space shuttle engine

Box C

OLD LANGLEY FLIGHT RESEARCH PROGRAMS

HISTORICAL NOTES ON FLYING QUALITIES WORK

OLD CONFERENCE MEMOS AND HISTORICAL NOTES ON DYNAMIC LOADS AND STRUCTURES RESEARCH

TRANSONIC RESEARCH

NOTES, COMMENTS, STATEMENTS ON MANAGEMENT PHILOSOPHY

AERONAUTICS POLICY, 1970

LANGLEY'S 50TH ANNIVERSARY

ROTARY CLUB TALKS

LOCAL AFFAIRS

UNIVERSITY OF MICHIGAN HONORARY DOCTORATE

WILLIAM AND MARY HONORARY DOCTORATE

RETIREMENT PARTY, 17 OCT. 1968

PERSONAL MATTERS, INCLUDING CORRESPONDENCE REGARDING APPOINTMENT AS CENTER DIRECTOR

NOTES ON OTHER PERSONS

MISCELLANEOUS TECHNICAL REPORTS AND PAPERS

Box D

COPIES OF PUBLIC TALKS, PUBLICITY STATEMENTS, PHOTOS

LETTER TO NATIONAL ACADEMY OF ENGINEERING

NUMEROUS TECHNICAL ARTICLES AND PAPERS, MOSTLY PUBLISHED

The John Stack Collection. This collection is more valuable to the aeronautical historian than is the Thompson collection because it includes a greater number and wider chronological range of older business correspondence and research program files—many of which concern Stack's pioneering work in transonic and supersonic technology. The papers were donated to the Langley archives by Stack's son, Peter, who, like Mrs. Thompson, chose to keep several of the more private letters in the family's possession, at least for the time being. The papers, which are in folders labeled by John Stack, have been organized into sections of file drawers according to categories:

Section No. 1
Wind Tunnel Design, Operation, and Test Techniques

CROCCO CURVE

KOCHEL ULTRA-SUPERSONIC WIND TUNNEL DEVELOPMENT

NEW TYPES OF TUNNELS

USES OF GAS OTHER THAN AIR IN WIND TUNNELS

HODOGRAPH REPORT

8-FOOT HIGH-SPEED TUNNEL OPERATIONS

SUPERSONIC WIND TUNNEL AT WRIGHT FIELD

4-FOOT SUPERSONIC TUNNEL

MISCELLANEOUS WIND TUNNEL DATA

SPECIAL TYPE TUNNELS—SLOTTED TEST SECTIONS

REPOWERING 16-FOOT HIGH-SPEED TUNNEL

UNITARY PLAN WIND TUNNEL

REVISED UNITARY PROGRAM

GAS DYNAMICS LABORATORY

FLUTTER TUNNEL

SUPERSONIC COMPRESSOR

ABERDEEN SUPERSONIC WIND TUNNEL

MADELUNG HIGH-PRESSURE WATER TUNNEL

PROPOSED AIR ENGINEERING DEVELOPMENT CENTER

NATIONAL SUPERSONIC RESEARCH CENTER

ELECTRIC POWER SUPPLY

REFRIGERATION

SCHLIEREN PHOTOGRAPHS

MISCELLANEOUS OPTICAL SYSTEMS

SCHLIEREN PHOTOGRAPHS—BRITISH NATIONAL PHYSICAL LABORATORY

AFTERGLOW PHOTOGRAPHS

SPHERE PHOTOS OVER A RANGE OF MACH NUMBERS

Section No. 2
Research Problems

JET ANALYSIS, INDUCTED

INTERACTION OF SHOCK AND BOUNDARY LAYER

SHROUDED PROPELLERS

DATA ON VARIOUS NACA AIRFOIL SECTIONS

DRAFTS OF STACK'S WRIGHT BROTHERS LECTURE, "COMPRESSIBLE FLOWS IN AERONAUTICS," 17 DEC. 1944

MISCELLANEOUS TECHNICAL REPORTS

Section No. 3
Reports of Meetings, Conferences, and Study Groups

GAS TURBINE CONFERENCE AT GENERAL ELECTRIC, 1945

HIGH-SPEED AERODYNAMICS CONFERENCE, NACA-NAVY-ARMY, 13 JULY 1945

STACK'S REPORT ON ABERDEEN CONFERENCE, 17 JAN. 1946

AMERICAN PHYSICAL SOCIETY MEETING, 25 APRIL 1946

NACA CONFERENCE ON SUPERSONIC AERODYNAMICS, AMES LAB, 4 JUNE 1946

LANGLEY CONFERENCE ON HIGH-SPEED AERODYNAMIC THEORY, 3 FEB. 1947

LANGLEY CONFERENCE ON SUPERSONIC AERODYNAMICS, 19–20 JUNE 1947

AMES CONFERENCE ON SUPERSONIC AERODYNAMICS, 31 AUG. 1948

AMERICAN PHYSICAL SOCIETY MEETING, UNIVERSITY OF VIRGINIA, DEC. 1949

MISCELLANEOUS CONFERENCE REPORTS

CONFERENCES

MINUTES OF MEETINGS

SUBCOMMITTEE ON HIGH-SPEED AERODYNAMICS

COMMITTEE ON ADVANCED STUDY

AD HOC PANEL ON LONG-RANGE AIR-TO-AIR GUIDED MISSILES

DRAPER COMMITTEE

DOD TECHNICAL ADVISORY PANEL ON AERODYNAMICS, AD HOC GROUP ON WATER-BASED AIRCRAFT

Section No. 4
Memos and Correspondence

HENRY J. E. REID'S TRIP TO EUROPE, 1944

DEVELOPMENTS IN HIGH-SPEED AERONAUTICS DURING WORLD WAR II

RIPARBELLI REPORT

LETTERS FROM COLEMEN DUPONT DONALDSON ON GERMAN SCIENTISTS AT WRIGHT FIELD, 1946

BELL TELEPHONE LAB

PERSONAL CORRESPONDENCE

MEMOS FOR ASSOCIATE DIRECTOR

LETTERS BETWEEN PROF. CARLO FERRARI, UNIVERSITY OF TURIN, AND ANTONIO FERRI, NACA, 1947–1948

MEMOS ON AIRFOILS

MEMO FROM HARTLEY SOULÉ, 1948

MEMOS FOR FILES

MISCELLANEOUS CORRESPONDENCE

Section No. 5
Aircraft Development Projects

NORTH AMERICAN P-51

HIGH-SPEED BOMBER PROGRAM, 1945

SUPERSONIC AIRPLANE

PROJECT 506

WATER-BASED AIRCRAFT

REPUBLIC P-47B

B-35 ELEVON

PROPELLER FOR SPITFIRE 21

XP-69 HORIZONTAL TAIL

EAGLE

REPUBLIC AVIATION CORPORATION 5-YEAR PLAN

SUPERSONIC TRANSPORT (SST)

GROUND EFFECTS MACHINES

V/STOL

MUTUAL WEAPONS DEFENSE PROGRAM (MWDP)

- VTOL-FAA PROJECT "HUMMINGBIRD"
- NATO BRIEFING, 13 OCT. 1960
- SECRETARY OF DEFENSE BRIEFING, 28–31 OCT. 1960
- TRAINING CENTER FOR EXPERIMENTAL AERODYNAMICS, BRUSSELS
- STACK'S TRIPS TO EUROPE ON MWDP, JUNE–OCT. 1959
- MISCELLANEOUS DATA FROM 1959
- MISCELLANEOUS DATA FROM 1960
- CORRESPONDENCE FOR FISCAL YEAR 1961
- ITALY'S HYPERSONIC TUNNEL
- BRITISH HAWKER P-1127 AIRCRAFT
- BE-53 ENGINE DATA INCLUDING SWALLOW AND HAWKER P-1127

TFX DEVELOPMENT

- HISTORICAL NOTES
- SOURCE SELECTION BOARD
- CONGRESSIONAL HEARINGS
- UNITED KINGDOM TSR-2

Section No. 6
Miscellaneous

MISCELLANEOUS PHOTOGRAPHS

BLUEPRINT DRAWINGS

"STACK'S STUFF," MISCELLANEOUS

The Floyd Thompson Technical Library

What really makes Langley one of the very best places in the country for research in aeronautical history is the technical library in which the historical archives are located. Besides holding major collections (over 3.8 million volumes) in the physical sciences and engineering with emphasis in aerospace science and technology, aeronautics, structures, materials, acoustics, energy, electronics, and the environment, and besides holding supporting collections in physics, chemistry, mathematics, and management, the library also preserves the complete NACA publications series—over 16,000 reports in more than 1000 bound and nearly 2000 unbound volumes—including Technical Reports (TRs), Technical Notes (TNs), Technical Memorandums (TMs), Wartime Reports (WRs), Aircraft Circulars (ACs), Research Memorandums (RMs), Advance Confidential Reports (ACRs), Advance Restricted Reports (ARRs), Confidential Bulletins (CBs), Restricted Bulletins (RBs), and Memorandum Reports (MRs). (For an analysis of the NACA publications series, see Roland, *Model Research,* app. G, pp. 551–567.)

What gives the library its unparalleled strength as a place for historical research, though, is the fact that its staff maintains the same index to aeronautical literature that was begun by the NACA in the 1920s. Cards reference tens of thousands of aeronautical papers from all over the world by subject, author, title, and, in the case of NACA reports and research authorizations, by number. Many of these papers are unpublished or classified. This makes the NACA card file one of this country's most treasured guides to aeronautical information. Langley's file is all the more precious because it is the only set extant.

The library is accredited and open to the public.

Photographic Collection. Langley's collection of photographs (housed separately from the library) runs to roughly 100,000 negatives, all of which are logged by date and by subject. The motion picture film collection (part of which is housed in the library and part in the separate photographic laboratory in building 1155) is also extensive, but most of the films are not in any shape to be loaned out for exhibition. There is a subject card file to this motion picture collection.

* * *

Anyone interested in conducting historical research at Langley should contact Richard T. Layman, Historical Program Coordinator, Mail Stop 123 (telephone 804/865-3688), or Sue K. Seward, reference librarian, Mail Stop 185 (804/865-2634), Langley Research Center, Hampton, Virginia 23665.

Index

Rosholt, Robert L., *An Administrative History of NASA, 1958–1963* (NASA SP-4101, 1966).

Sloop, John L., *Liquid Hydrogen as a Propulsion Fuel, 1945–1959* (NASA SP-4404, 1978).

Swenson, Loyd S., Jr., James M. Grimwood, and Charles C. Alexander, *This New Ocean: A History of Project Mercury* (NASA SP-4201, 1966).

Reference Works

Aeronautics and Space Report of the President, annual volumes for 1975–1982.

The Apollo Spacecraft: A Chronology (NASA SP-4009, vol. 1, 1969; vol. 2, 1973; vol. 3, 1976; vol. 4, 1978).

Astronautics and Aeronautics: A Chronology of Science, Technology, and Policy, annual volumes 1961–1977, with an earlier summary volume, *Aeronautics and Astronautics, 1951–1960.*

Dickson, Katherine M., ed., *History of Aeronautics and Astronautics: A Preliminary Bibliography* (NASA HHR-29, 1968, multilith).

Hall, R. Cargill, ed., *Essays on the History of Rocketry and Astronautics: Proceedings of the Third through the Sixth History Symposia of the International Academy of Astronautics* (NASA CP-2014, 2 vols., 1977).

Hall, R. Cargill, *Project Ranger: A Chronology* (JPL/HR-2, 1971, multilith).

Looney, John J., ed., *Bibliography of Space Books and Articles from Non-Aerospace Journals, 1957–1977* (NASA HHR-51, 1979, multilith).

Roland, Alex F., *A Guide to Research in NASA History* (NASA HHR-50, 6th ed., 1982, available from NASA History Office).

Skylab: A Chronology (NASA SP-4011, 1977).

Van Nimmen, Jane, and Leonard C. Bruno, with Robert L. Rosholt, *NASA Historical Data Book, 1958–1968,* vol. 1, *NASA Resources* (NASA SP-4012, 1976).

Wells, Helen T., Susan H. Whiteley, and Carrie E. Karegeannes, *Origins of NASA Names* (NASA SP-4402, 1976).

Recent volumes are available from Superintendent of Documents, Government Printing Office, Washington, DC 20402; early volumes from National Technical Information Service, Springfield, VA 22161.