THE HIGH-SPEED FRONTIER

Case Histories of Four NACA Programs, 1920–1950

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Foreword

It is refreshing as well as unusual to find such an account as this of past technical programs that were so important to aeronautical progress. The author deals not only with the research in which he was intimately involved but also with the personalities of the participants and the doubts, false starts, and misconceptions that occurred before the final solutions were achieved.

In my view, the flavor imparted to these case histories by the very personal impressions of the impact of certain of the key players is a necessary ingredient in getting to the bottom line of how and why things worked the way they did in the prime years of the National Advisory Committee for Aeronautics (NACA).

Each of the four programs described grew from small beginnings in the third and fourth decades of this century to become substantial elements of the NACA contribution to the achievement of high-subsonic and transonic flight. All of the programs had been essentially completed by the time of the termination of NACA in 1958 and the transition to NASA.

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CHAPTER I

Introduction

Previous writings about NACA research achievements, for example G. W. Gray's *Frontiers of Flight,* contain generally excellent descriptions of the problems of aeronautics and the solutions developed. To anyone personally involved in these programs, however, there are serious omissions, particularly the absence of vital information on how the solutions actually evolved. More often than not the solutions seem to have emerged automatically—the inevitable result of wise management, inventive researchers, and unparalleled facilities. In the four programs considered here the previous treatments passed over so much of the important action I had seen as a participant that I was inspired to undertake this effort to complete the record.

To provide fundamental insights into NACA's technical accomplishments the record should include the doubts and misconceptions that existed in the beginning of a project, the unproductive approaches that were tried and abandoned, the stimulating peer discussions that provided new insights, and the gradual evolution of the final solution. This kind of information is hard to find. Only bits and pieces of it appear in the written records in NACA files. Most of it is stored in the minds of those who participated in the NACA programs. A participant-author can draw on obvious major assets in establishing this part of the record—his personal knowledge of the fertile areas to probe, the roles played by the others, and the profitable questions to ask. The true facts can be learned through the process of pooling and editing the recollections of all the principal participants. In the present study I drew heavily on the help of many former colleagues who are identified in the acknowledgments and elsewhere throughout the text.

Three other historical documents have recently been authored by former NACA engineers: E. P. Hartman's *Adventures in Research: A History of Ames Research Center, 1940–1965* (NASA SP–4302); J. A. Shortal's *A New Dimension. Wallops Island Flight Test Range: The First Fifteen Years* (NASA RP–1028); and J. L. Sloop's *Liquid Hydrogen as a Propulsion Fuel, 1945–1959* (NASA SP–4404). These works had different objectives than the present study and each covers vastly larger territory, dealing extensively with management and administrative operations in addition to research activities. Beyond any question each of these books contains innumerable important contributions to the record which would otherwise have been lost if these knowledgeable participant-authors had not taken up their pens.

From the large body of NACA's total contribution to high-speed technology the particular programs treated here were selected for two reasons: first, because of their quite inadequate coverage in previous writings, and second, because of my intimate personal involvement with each of them either as a researcher or as a supervisor. All of the programs fall in the category variously referred to as "general," "fundamental," or "basic" NACA research. They are typical of what was done in this category; only one, the slotted tunnel, became a celebrated NACA achievement. (Each of the programs involved a number of different research authorizations and none appears consistently in agency records under the titles I have used. The term "high-speed" is used here in the same sense that it was used during those programs to mean high-subsonic and transonic speeds up to about Mach 1.2.) Most of the work was completed by 1950 and all of it by 1958; an interesting renaissance of the airfoil program in the mid-sixties is also covered briefly.

In the prospectus for the study I proposed to attempt some hindsight analysis, which is rare in NASA literature but a potentially useful device for improving the R&D process (ref. 1). My experience in a previous study (ref. 2) suggested that, insofar as possible, hindsight observations should be separated from the historical narrative. Accordingly, I have located them under the heading "Commentary" at the ends of the appropriate sections.

CHAPTER II

The High-Speed Airfoil Program

BACKGROUND AND ORIGINS (1745–1927)

The first discovery of an aerodynamic anomaly near the speed of sound was made over 200 years ago by the brilliant British scientist Benjamin Robins, inventor of the ballistic pendulum. He observed what we now call the transonic drag rise by firing projectiles into this device and inferring the law of their air resistance as a function of velocity from the deflections of the pendulum (ref. 3). He states:

> . . . the velocity at which the body shifts its resistance [law from a V^2 to a V^3 relation] is nearly the same with which sound is propagated through air. Indeed if the [V^3 relation] is owing to a vacuum being left behind the body, it is not unreasonable to suppose that the celerity of sound is the very least degree of celerity with which a projectile . . . can in some way avoid the pressure of the atmosphere on its hinder parts . . . but the exact manner in which the greater and lesser resistances shift into each other must be the subject of further experimental inquiries.

By the end of the 19th century a considerable body of understanding of the differences between subsonic and supersonic flows for projectiles had been built up by the work of Lamb, Ernst Mach, Lord Rayleigh, and others, establishing the speed ratio V/a (later "Mach number") as the controlling nondimensional parameter, and clearly implying drastic changes in the flow in the vicinity of $V/a = 1$.

The flight speeds of the primitive aircraft of the first two decades of this century were so low that compressibility effects were nil as far as the airframe was concerned. However, by the end of World War I engine powers and propeller diameters had increased to the point where tip speeds as high as the speed of sound were being considered (ref. 4). This appears to have been a matter of particular concern to the British who, perhaps from firsthand acquaintance with Lord Rayleigh's classical studies (ref. 5), or perhaps from his direct personal advices as a member of the British Advisory Committee for Aeronautics, had become aware of a possible critical problem near the speed of sound. That the problem did indeed exist was first demonstrated by Lynam (ref. 4) in free-air zero-advance tests of a low-pitch propeller model at tip speeds up to 1180 ft/sec, the structural limit for the "thoroughly well-seasoned black walnut" test blades. The tests indicated loss of thrust and increase in blade drag, but provided no quantitative data or detailed insight into the phenomena. Wind-tunnel tests of a more representative model propeller at advance ratios in the range of flight operations were recommended.

Contemporary with this early British work, the first American tests pertaining to the propeller problem were undertaken at McCook Field in 1918 by Caldwell and Fales of the U.S. Army's Engineering Division. Almost as though complementary programs had been deliberately planned and coordinated, the Americans chose to make high-speed wind-tunnel tests of stationary propeller blade sections instead of propeller tests. The magnitude of the undertaking was by no means less than that of the British, however, because no high-speed wind tunnel had ever been built, and Caldwell and Fales had to develop the world's first such facility (ref. 6). Exploratory tests using an 8-inch diameter throat were made at the National Bureau of Standards where they were undoubtedly observed with interest by a brilliant young Ph. D. in physics, Hugh L. Dryden, who had recently joined the staff and who would shortly become a pioneer investigator of high-speed aerodynamic phenomena. After exploratory experimental work on all components, a final configuration of the Eiffel-type tunnel was decided upon and constructed at McCook Field.

The tunnel had a 14-inch diameter throat and was powered by a 200-hp motor which produced a maximum speed with test model in place of about 675 ft/sec (Mach .64). (This actual speed was never calculated correctly by Caldwell and Fales. Not knowing how to determine the true air density in the test section they used the ambient air density in the room to calculate an "indicated" airspeed from the measured pressure drop between intake and test section of the tunnel.)

Although well below maximum propeller tip speeds, 675 ft/sec was high enough to demonstrate large "compressibility" losses in lift coefficient and increases in drag for the thicker sections and high angles of attack. Caldwell and Fales called the speed at which these changes occurred the "critical speed" and the flow change the high-speed "burble"—terminology which was adopted by succeeding investigators. It is most interesting, however, that they made no mention of the velocity of sound or the speed ratio as a controlling parameter. At the same time, they were not surprised to find changes in the character of the flow as the speed increased. Orville Wright contributed to this outlook by telling them of a hysteresis effect he had seen in his early low-speed wind tunnel tests in which two regimes of flow occurred for certain airfoils at the same test conditions (ref. 6) (now believed to be a separated-flow condition with laminar boundary layer, alternating with an attached flow with turbulent boundary layer).

A most interesting feature of the Caldwell/Fales report is inclusion of the first recorded attempt to provide a specific theoretical explanation of the observed critical speed phenomena. Unfortunately the new hypothesis ignored the speed ratio parameter, and attempted to define a "limiting shear stress" in the flow at high speeds beyond which it would separate from the airfoil. The theory was put forward by George de Bothezat, a foreign aerodynamicist of some reputation, author of a textbook on aircraft stability, and a former lecturer at the Polytechnic Institute of Petrograd. De Bothezat had been hired by the newly created NACA and assigned temporarily to McCook Field, since NACA had not yet acquired facilities of its own (ref. 7). Between 1919 and 1921 he published no less than four comprehensive NACA papers (Reports No. 28, 29, and 97, and TN No. 2) which were creditable for their time. He went on to invent the Army helicopter which bore his name and which flew at McCook Field for 2¾ minutes at altitudes up to 15 feet in 1923. De Bothezat was almost certainly aware that dynamical similarity suggested the speed ratio as the controlling parameter at high speeds, but he evidently thought the assumptions of similarity were violated by flow separation.

During the same period as the Caldwell/Fales investigations, Sylvanus A. Reed was pursuing a remarkable and unaccountably often overlooked program of high-speed tests of thin-bladed metal propellers (ref. 8). Reed had invented a semi-rigid metal propeller formed from ⅝-inch-thick duralumin billets, tapering to ⅛-inch at the tips. The bending moments due to aerodynamic thrust for the outer portions of the blades were balanced largely by the centrifugal moments due to rotation and blade deflection. This design made it possible to employ extremely thin sections contrasting markedly with the very thick sections of the wooden propellers then in universal use. In the introduction of his paper, Reed made the following revealing observation: "There has been a tradition general among aeronautical engineers that a critical point exists for tip speeds at or near the velocity of sound indicating a physical limit . . . , something analogous to what is known in marine propellers as cavitation." Evidently the expectation of the sonic anomaly was so widely known as to be called a "tradition." Reed goes on to state, however, that the only supporting evidence for this "tradition" that he could find were the British propeller tests of Lynam (ref. 4). He notes that Lynam used blunt-edged, thick blades which, by inference from the poor performance of bullets fired blunt end forward, he postulated would have poor sonic and supersonic performance. He therefore conducted a series of high-speed tests of his thin-bladed metal propellers to investigate this postulate.

A series of metal model propellers of 17-inch, 22-inch, and 4-foot diameter were tested in still air at tip speeds up to nearly 1.5 times the speed of sound; and 9-foot diameter full-scale propellers were tested in flight on a Curtiss airplane at near sonic tip speeds with help from the Curtiss Aeroplane and Motor Company. On some of the test propellers the very thin (of the order of 4 percent thickness) outer sections had sharp leading edges. The data showed no significant changes in the thrust/torque coefficient relationships in the region of sonic speed, and only small deterioration at low supersonic tip speeds. The sound generation became very loud and "penetrating" but had none of the "confused and distressing violence" noted in the British tests. Reed concludes that the high-speed problems of the British propeller were due to "the use of [thick, blunt-edged] blades not adapted to high speeds." This remarkable investigation was made before any high-speed section data had been obtained, and it preceded by over 30 years tests of "supersonic" propellers by NACA. Reed appears to have been unaware of the Caldwell/Fales program or perhaps he considered their highest test speed, V/a = .64, too low to be applicable. In any case, Reed had proved that the deterioration of propeller performance at near-sonic tip speeds could be avoided by the use of thin sections. The general failure to accord proper recognition to Reed's work in the subsequent literature may be partly due to the cumbersome and misleading title of his report, and perhaps partly to the rather limited amount of data and analysis it contained.

Following Lynam's initial propeller tests in free air the British started immediately to develop a powerful turbine-driven propeller dynamometer suitable for testing 2-foot diameter propellers at high tip speeds in their 7-foot low-speed wind tunnel. Douglas and Wood's report of this investigation (ref. 9) is one of the classical documents of the early years of aeronautical research. The tip section of their wooden test propeller was 10 percent thick and compressibility losses started at about V/a = .78. At their highest tip speed of 1180 ft/sec, V/a = 1.08, the propeller efficiency had dropped from 0.67 to 0.36. The British displayed great ingenuity in their deductions of blade section data from the measured propeller data, aided by pitot surveys and optical measurements of blade twist. The latter measurements made it possible to derive section moment coefficients showing the rearward movement of the center of pressure at the highest speeds. The inclusion of all of the test data and the detailed analysis of results in the Douglas/Wood paper may account for the fact that it is widely referenced, while the Reed paper, which contained only minimal test data and analyses, has seldom been cited in the subsequent literature.

The Caldwell/Fales program had been accomplished under the general direction of Col. Thurman H. Bane, Commander of McCook Field and also the Army Air Service's member of NACA from 1919 to 1922. Bane is believed to have apprised the Committee of the results and arranged for their publication as a NACA report (ref. 6). Although the need for follow-on wind tunnel tests at higher speeds was quite obvious, none was attempted by the McCook group; presumably they moved on to more pressing problems. The seeds of interest had been sown, however, in both NACA and in the Bureau of Standards. It is likely also that NACA was aware of the continuing British effort on the high-speed problem. The personal relationship between Joseph S. Ames, Chairman of the Physics Department at Johns Hopkins and member of the Executive Committee of NACA and Hugh Dryden of the Bureau, one of Ames' most outstanding recent graduates at Johns Hopkins, was probably a factor in NACA's negotiation of a contract for the Bureau to extend the investigation of propeller sections to high speeds. Authorization for the work was signed in 1922 by George W. Lewis, the recently appointed Executive Director of NACA and also its "Budget Officer" (ref. 10).

Lyman J. Briggs, a senior official at the Bureau (soon to become its Director and a member of NACA), was in charge of the program. He personally designed the compact balance used in the tests and also participated in the testing. The curve plotting, analysis, and evidently the report writing was done mainly by Dryden, aided by G. F. Hull (ref. 11).

Primary emphasis was on extending the Caldwell/Fales data to near-sonic speeds. Rather than taking on the costly problem of designing a new wind tunnel or perhaps improving the one at McCook Field, the Bureau of Standards group located a large 5000-hp air compressor capable of continuously supplying air at 2-atmospheres pressure to a 12-inch diameter nozzle. This provided them in effect with a ready-made free-jet wind tunnel having about twice the test Reynolds number of McCook facility and a maximum speed of about Mach .95. A disadvantage was that the airfoil testing had to be done incidentally to developmental testing of the compressor at the General Electric plant at Lynn, Massachusetts. And thus it was that Briggs and Dryden found themselves on Christmas Day, 1923, subjected to the rigors of airfoil testing in an open jet. Shortly afterwards, as Dryden explained later, "We walked down the street in Lynn discussing the jet and noticed passers-by staring at us strangely and shaking their heads. It was some time before we discovered that we'd been shouting at each other at the top of our voices, both temporarily deaf as a result of working with our heads only a few inches from the large jet" (ref. 12).

The test models were 3-inch chord end-supported wings which extended through the jet boundaries. It was not possible to determine the boundary effects and thus quantitatively meaningful true section data could not be obtained. Qualitatively, however, the results were of great significance, confirming and extending the findings of Caldwell and Fales. The speed ratio, V/a, was used as the primary parameter, and for the first time a hypothesis as to what might be happening was put forward which has stood the test of time (ref. 11):

> We may suppose that the speed of sound represents an upper limit beyond which an additional loss of energy takes place. If at any point along the wing the velocity of sound is reached the drag will increase. From our knowledge of the flow around airfoils at ordinary speeds we know that the velocity near the surface is much higher than the general stream velocity . . . the increase being greater for the larger angles and thicker sections. This corresponds very well with the earlier flow breakdown for the thicker wings and all of the wings at high angles of attack.

This was the first statement of the relation between the critical speed and the known low-speed velocity distribution about the airfoil—one of the fundamental ideas in high-speed airfoil research which was resurrected and exploited in the thirties. Significantly, no mention was made of the apocryphal theory of de Bothezat.

To probe more deeply into the mysteries of the compressibility "burble" and to provide load distribution data, Briggs and Dryden undertook pressure distribution measurements on the same airfoils used in their force tests. The Lynn compressor was no longer available, and a small-capacity plant at Edgewood Arsenal had to be used, capable of supplying only a 2-inch diameter jet. It had the advantage, however, of sufficiently high pressure to achieve low supersonic velocities. Briggs and Dryden designed a converging-diverging (supersonic) nozzle which produced $M = 1.08$, and their program included the first known aerodynamic tests in this country at a supersonic speed. There were three important new findings from the pressure data (ref. 13):

- Sudden breakaway of the flow on the upper surface occurred at the burble point.
- Briggs and Dryden noticed that a sudden shift occurred in the pressure near the trailing edge—from lower-than to higher-than stream pressure—at the onset of the burble. (This phenomenon was noticed again some 35 years later by Britishers Gadd and Holder and proposed as an index of the onset of transonic buffeting, no mention being made of the earlier discovery (ref. 14).)
- The transonic drag coefficient was found to peak in the speed range

between Mach .95 and 1.08, following the same pattern as the drag of projectiles. And, for the first time in history for an airfoil, the bow shock wave was seen standing about ½-inch ahead of the leading edge at Mach 1.08.

There was also one major misinterpretation of the pressure data. The authors stated that the lowest observed upper-surface pressures corresponded approximately to the attainment of the local velocity of sound, and that lower pressures could occur only in "dead air" spaces. "This observation suggests that in an airstream obeying the law of Bernoulli the pressure cannot decrease indefinitely but reaches a limit . . . near the critical [sonic] value of 0.53." This is, of course, quite wrong. An examination of their pressure data actually shows quite clearly the existence of supersonic local velocities ahead of the probable locations of the upper surface shocks. Unfortunately, the orifice spacing of 0.25 chord in the aft region of the upper surface precludes any precise examination of the flow and this may explain the misinterpretation.

The pressure data underscored what was already evident from the earlier force data—that the burble phenomena were exceedingly complex, involving shock-boundary layer interactions quite beyond any possibility of theoretical treatment. Future researches would be almost exclusively experimental; not until the later forties, when it was learned that the shocks moved off the airfoil for Mach numbers greater than about 0.95, did valid theoretical solutions appear for Mach 1 and above.

In 1927 a conference of NACA and the military services recommended a final extension of the Briggs/Dryden program to provide force data for additional more recent sections of interest to propeller designers. Included was a typical 10-percent-thick airfoil used by Reed in his metal propellers which was one of the best tested for that thickness ratio (ref. 15). The last extension was a series of tests of circular-arc sections, recommended by the authors for the outer regions of propellers for very high tip speeds (ref. 16). Unaccountably, they made no reference to Reed's work of nearly a decade before suggesting a similar use of sharp-edged sections.

Although NACA continued to sponsor the Briggs/Dryden program until it ended in 1930, it had been decided in 1927 to develop a new high-speed tunnel at Langley and to embark on in-house NACA research at high speeds. The initial direct involvement of the staff with high-speed research was the Jacobs/Shoemaker investigation of thrust augmentors for jet propulsion (ref. 17) in 1926. Although the jet-propulsion connection was much ahead of its time, this study stirred in Jacobs the beginnings of a strong interest in high-speed aerodynamics. The thrust augmentor inspired in G. W. Lewis not only keen interest, but also a display of technical imagination and inventiveness seldom seen in administrators at his level. He saw in this device a possible economical means of powering a large high-speed tunnel, using waste high-pressure air from the frequent blow-downs of the Variable Density Tunnel (VDT) (ref. 18). Dr. Ames, now NACA Chairman, had also followed the high-speed testing of Jacobs, Briggs, and Dryden with interest. All were aware that a major deficiency existed in the Briggs/Dryden investigations, namely the unknown jet boundary effects. The in-house program was therefore launched with the immediate objective of obtaining accurate quantitative high-speed section data for propellers to supplement the comparative results of Briggs and Dryden (ref. 19).

Preliminary trials were made by Jacobs with a 1-inch diameter throat which indicated that the jet-augmentor principle could indeed be successfully applied to drive a high-speed tunnel. Sufficient pressure was available during VDT blow-down to induce supersonic flows, and sonic conditions could be maintained for long periods. Even with a 12-inch throat Jacobs' estimates showed several minutes test duration. The dimensions and configuration selected for the first tunnel coincided with those of the first Briggs/Dryden testing at Lynn: a 12-inch open throat with 3-inch chord wings. The proportions of the open throat and its diffuser inlet were similar to those employed in the NACA VDT and Propeller Research Tunnel (PRT) facilities. However, following Briggs' and Dryden's design, the test wing spanned the jet and was supported at the ends on a photo-recording balance designed by Jacobs and his group (ref. 19). It is unclear now what the rationale was for obtaining more accurate section data with this arrangement since it duplicated the Briggs/Dryden setup in all important respects except for the addition of a diffuser. Several of those interviewed indicated that this was a "real wind tunnel with good flow" while the former was "only an open jet" and this may reflect the early NACA attitudes. Or it may be that the open throat was intended to provide a direct comparison with the earlier test results, prior to the development of an improved closed throat configuration. But this could not be verified in the interviews. In any case, by mid-1928 NACA was ready to begin using its first high-speed wind tunnel (ref. 20).

COMMENTARY

The combination of the British tests of model propellers at high tip speeds, Reed's tests of thin metal propellers, and the American investigations of blade sections by Caldwell and Fales and by Briggs, Dryden, and Hull constitutes one of the first concerted efforts of the fledgling aeronautical community to solve what was feared to be a serious obstacle to progress. By any standards, the array of talent mustered was truly exceptional. Within the short time of about five years, the problem was accurately delineated and practical solutions had been found. The use of thin sections at low angles of attack in the tip region was the basic prescription, and this was readily practical for the new metal propeller designs that were beginning to appear. Beyond that, however, the use of gearing, and finally variable-pitch and constant-speed propellers eliminated the problem entirely for the airplane speeds foreseeable in 1925. Accordingly, most of the researchers initially involved moved on to more pressing problems in other areas. Briggs and Dryden had developed sufficient scientific and personal interest to carry on for a time under their own momentum, but they both became increasingly involved with other pursuits. The pressure for blade-section research was further diminished when NACA's new "PRT" was placed in operation in 1927.

Certainly there was little comprehension in 1927 that the airframe as well as the propeller would become subject to compressibility problems. Advanced pursuit planes reached speeds of only about 200 mph and it would be six or seven years later before serious speculations regarding the "500-mph airplane" would appear. A scan of the literature of the mid-twenties shows only rare suggestions of very high future speeds. (One overly sanguine prediction found in a NACA re-publication of a 1924 French document (ref. 21) envisioned aircraft flying at Mach 0.8 or more by 1930, including development of some wholly new but unspecified type of propulsion plus appropriate new high-speed wind tunnels to support these developments.)

The initiation of in-house NACA research in high-speed aerodynamics in 1927, coming in a period where industry pressures for such work were nonexistent (except for extending the Briggs/Dryden program to a logical conclusion), has been called an act of "great foresight" (ref. 20). More probably, the start at this particular time was a natural consequence of Jacobs' 1926 investigation of jet augmentors. This provided both the basis for Dr. Lewis' imaginative suggestion to use VDT blowdowns to actuate a "large" tunnel, and a sufficient level of interest in both men to take on such a project. Jacobs and Lewis also realized intuitively that there was a place in Langley's burgeoning stable of wind tunnels for one that could deal with high-speed problems, eliminating continued dependence on the Bureau of Standards and outside test facilities.

THE QUEST FOR UNDERSTANDING (1928–1935)

On July 16, 1928, the man who was to dominate Langley high-speed aerodynamics for the next 30 years reported for duty. John Stack was the son of Irish-born parents, a heritage which may have accounted for his personal charm, garrulousness, love of horses, and ability to absorb large quantities of whiskey. Educated at the Chauncey Hall School and Massachusetts Institute of Technology, his distinctive accent retained little to suggest an Irish background (it can be described as upper-class Bostonian with variations). Stack was at his best in the midst of conflict, crusading passionately for some cause such as a new wind tunnel against the forces of reaction and stupidity (which in his view was anyone and everyone who had any objection to the project).

He had applied for NACA employment during his senior year at MIT, where several of the faculty were involved in various ways with NACA activities. On his arrival there were fewer than 60 professionals at Langley, loosely organized in "sections" attached to the research facilities they operated. As was customary, Elton W. Miller, the fatherly, mild-mannered Chief of Aerodynamics, escorted Stack around the Laboratory introducing him to virtually the entire staff. After the tour, "Mr. Miller," as he was universally called, indulged himself with a final question that he invariably directed to new engineers with private enjoyment, "Where would you prefer to be assigned?" Believing he had a choice Stack said, "the VDT." "Very good, I had already decided to put you there," Miller replied. (More often than not, as in my own case, the new arrival's choice did not agree with Mr. Miller's and he was told, "Well I have decided to place you elsewhere. Let me know in a year or two how you like it.")

Stack was assigned immediately to the 12-inch high-speed tunnel project which was then under construction—the lone NACA researcher in this field. For the next decade his work would be closely followed by Eastman N. Jacobs, VDT section head, a man for whose technical sagacity Stack had enormous respect. Both men had the same kind of restless energy and pragmatic approach to research problems. Neither was a theoretician, although both of them frequently supported theoretical work by others and frequently made use of such work. Their own activity in this area was limited to applying the usual analytical tools of the engineer.

In his first years at Langley, Stack was quite modest about his knowledge of aerodynamics and was eager to learn. As W. F. Lindsey, who arrived in 1931 and was a major contributor throughout the high-speed airfoil program, puts it, "Practically all we knew about compressible flow theory at that time was what was written in five or six pages in Glauert's 1926 textbook." Among the five professionals in the VDT group in 1930, Stack was chosen to act as section head in Jacobs' absence. (In those days, there was no formal appointment to the assistant section head position.) Apparently Stack's general deportment as a junior engineer was exemplary; the tough assertive characteristics mentioned earlier began to show themselves slowly at first, not reaching full flower until after Jacobs departed Langley in the mid-forties (refs. 22, 23).

The first attempts to operate the 12-inch tunnel with its unique jet-augmentor induction drive produced such violent flow oscillations that it was soon decided to convert to a closed throat. Stanton's small supersonic tunnel in England, in which the test airfoil spanned the throat (ref. 24), may have suggested the configuration. This configuration eliminated the pulsations and the uncertain large boundary effects of the open-tunnel setup, but suffered large constriction effects which were not understood at that time. Pressure distributions on the 3-inch chord airfoils were found to be similar in character to the Briggs/Dryden results but different in detail. There was no way to tell whether either set of data was correct at the higher speeds. The renowned British theorist, G. I. Taylor, visited Langley in late 1929 and examined the data. Results of his recent studies of subcritical compressible flows by the electrical analogy method seemed, by inference and extrapolation, to cast doubts on the 12-inch tunnel data. Discouraged, Stack and Jacobs set the data aside and decided to go back to the open-throat configuration, with the first objective of achieving stable flow. (It is now believed that the closed-throat data were valid at speeds below the onset of tunnel choking. Unfortunately they were never published and were later disposed of.)

Another famous visitor, Amelia Earhart, came to view a test run in the high-speed tunnel at this same time period. She was clad in a raccoon fur coat. When the tunnel started she leaned forward to feel the flow of air into the entrance bell and her coat was instantly sucked into the bell, causing a large tear and terrifying its owner (ref. 22).

Stack has reviewed the laborious succession of design changes to the tunnel (ref. 20) that followed Taylor's visit: reversion back to the open throat modified by incorporation of a ½-inch annular enlargement at the entrance to the diffuser and a large reduction in length of the open section; rejection of the open throat, primarily because of windage effects on the balance and secondarily because of flow pulsations; a second reversion back to the closed throat—11 inches in diameter but virtually the same arrangement at the 12-inch tunnel except for the ½-inch step at the entrance of the diffuser and the use of 2-inch chord test models. By this time (1931) a high-tip-speed propeller test had been made in the PRT which afforded a basis for comparison and evaluation of the closed-throat wind tunnel data. Stack applied Goldstein's method to calculate the performance of the test propeller using the new high-speed section data from the 11-inch tunnel (ref. 25). His results agreed with the PRT tests except that the onset of performance deterioration in the calculation occurred at a somewhat lower Mach number. We now know this shift in speed was due to a combination of constriction effects in the tunnel, Reynolds number differences, and three-dimensional relief at the propeller tip. Still, the comparison was close enough to confirm that the tunnel was an effective tool, and it was used at once to try to define improved sections. Following the lead of Briggs and Dryden, airfoils with the maximum thickness shifted rearward were found to offer improved high-speed performance, a fact which further strengthened confidence in the 11-inch tunnel (ref. 26).

At this stage (1933) the Langley group, according to Stack, had "exhausted its intuition as regards methods for further improvement of aerodynamic shapes" (ref. 20). However, now that making the tunnel work was no longer the primary problem, interest finally shifted to the nature of the "burble" phenomena. E. N. Jacobs is believed to have first suggested to Stack that the schlieren optical system ought to be tried to make the phenomena visible. From his interest in amateur astronomy Jacobs was familiar with the Foucault test for mirrors, and the schlieren system, first described in 1889 by Mach, was optically a close relative. Unfortunately, in the limited Langley library of the early thirties nothing could be found except a schematic drawing in Wood's *Physical Optics.* This was used as a guide to construct the first crude schlieren (ref. 23). Reading-glass quality lenses about 3 inches in diameter were located together with a short-duration-spark light source. Celluloid inserts were used to support the test model at the tunnel walls. The first tests were made on a circular cylinder about ½-inch in diameter, and the results were spectacular in spite of the poor quality of the optics. Shock waves and attendant flow separations were seen for the first time starting at subsonic stream speeds of about 0.6 times the speed of sound. Visitors from all over the Laboratory, from Engineer-in-Charge H. J. E. Reid on down, came to view the phenomena. Langley's ranking theorist, Theodore Theodorsen, viewed the new results skeptically, proclaiming that since the stream flow was subsonic, what appeared to be shock waves was an "optical illusion," an error in judgment which he was never allowed to forget. At the annual dinner of the Langley staff in the fall of 1936, a skit was presented in which Stack played the role of Theodorsen, complete with Norwegian accent, making the "optical illusion" pronouncement.

Flow pictures for an airfoil at high speeds were obtained in short order. All of the implications were not immediately understood; however it was seen that a shock wave formed shortly after the speed of sound was reached locally and that flow separation was induced by effects of the shock. This emphasized the idea that shapes should be sought with the least possible induced velocities. Stack has described this concept as "the inspiration . . . which led immediately to a new approach to the problem of developing better shapes" (ref. 20).

Shortly after the first dramatic results of the schlieren tests had been obtained, Jacobs came back from a meeting with Reid and announced that $10 000 of Public Works Administration funds would be made available to build a 24-inch high-speed tunnel, provided that a design could be accomplished in a few weeks. Justification for the larger tunnel rested entirely on Jacobs' argument that it was the low Reynolds number of the 11-inch tunnel data which was responsible for the discrepancy with the PRT propeller data mentioned previously. Jacobs' idea was to build a 24-inch tunnel exactly similar in all respects except size and Reynolds number to the 11-inch tunnel, and this was the basic design specification. A number of improvements were included however: a new 5-inch schlieren system, an improved balance, and a photo-recording multiple-tube manometer.

The tunnel was erected outside the VDT building on a reinforced concrete base which also formed the entrance section and the test chamber surrounding the tunnel throat. Ira Abbott quickly became an expert in reinforced concrete. Dick Lindsey and Ken Ward were instructed by Jacobs to design the entrance section independently and bring their results to him for comparison. (They were sufficiently similar to merit Jacobs' quick approval.) Stack specialized in aerodynamic issues and coordinated the design project. The design was completed as scheduled and the tunnel was built approximately within the cost limitation in about one year's time. Figure 1 shows the two principal operators of the 24-inch tunnel involved with a survey rake installation in a scene typical of the mid-thirties.

The first test in the new tunnel involved a much more important issue than the Reynolds number-effect question for which the tunnel had been built. Jacobs had been invited to present a paper at the forthcoming Fifth Volta Congress on High Speeds in Aviation in Italy, and he realized that an elucidation of what was actually happening in the compressibility burble phenomena would be most appropriate and important, especially

FIGURE 1.—*John Stack and W. F. "Dick" Lindsey (standing inside the 24-Inch High-Speed Tunnel) in the thirties.*

in view of the possibility now of presenting flow photographs in addition to pressure distributions and forces. Accordingly, a 5-inch chord 4412 airfoil model built for the VDT with 56 small pressure holes was tested in the 24-inch tunnel and simultaneous pressures and flow photographs were obtained for the first time. After describing the new understanding of the burble phenomena achieved in the Langley program, Jacobs went on to derive for the first time the relation between the low-speed suction pressure peak on an airfoil and the speed ratio (Mach number) at which the local speed of sound would be reached. That is, the critical Mach number could now be related to or estimated from the low-speed pressure signature of the airfoil. Obviously this relation contained a powerful implication: the critical Mach number could be increased by shape changes which could be determined by simple incompressible theory or low-speed tests.

A NACA Technical Note covering some of the same ground as the Volta paper was written by Stack (ref. 27), and a more elaborate Technical Report (ref. 28) was issued later in which Stack credits Jacobs with the critical Mach number derivation. Together with Jacobs' paper these publications proclaimed the first major contribution of NACA in-house high-speed research—the fundamental understanding of the burble phenomena derived in large part from the revelations of the schlieren photographs.

COMMENTARY

Throughout the history of NACA newer types of test facilities were often placed into service somewhat prematurely in order to capitalize on their advanced capabilities. This frequently resulted in some unforeseen difficulties. In the case of the first NACA high-speed wind tunnel these difficulties were compounded by strong interactions between the tunnel flow and the test airfoil flows at high speeds. Furthermore, the high-speed airfoil problem was so new that no criteria existed for judging whether valid data were being obtained, a situation which had its roots in the lack of knowledge of what actually happened in airfoil flows when the compressibility burble occurred. It seems obvious now that the first goal in such circumstances should be to acquire at least a qualitative understanding of the basic flow phenomena, and that this should always precede any program to produce force data for use by designers. The closed-throat 12-inch tunnel of 1929 could have been used to provide the great enlightenment from combined pressure and schlieren pictures which did not come until some five years later in the program actually pursued. It was the eventual achievement of this fundamental understanding that now stands out as NACA's first major accomplishment in high-speed aerodynamics. It also formed the solid base on which the advances in critical speed discussed in the next section could be made. By comparison, perfection of the testing technique so as to acquire improved force data for designers, which was the goal of the early program (ref. 19), produced only relatively unimportant data prior to 1934.

A principal factor in the long delay in acceptance of the closed-throat data was the doubt engendered by G. I. Taylor in 1929. W. F. Lindsey points out that Taylor's real expertise extended only to the critical speed, and beyond that point his speculations should not have been taken as seriously as they were (ref. 23). E. N. Jacobs also feels that the cautious conservatism often displayed by so-called "experts" when they are asked to judge new phenomena beyond their previous experience has been a cause of undue delays (ref. 19). As another example he cited his 1926 investigation of thrust augmentors (ref. 17). Lewis turned the report of this work over to Dryden for review. Dryden expressed some doubts about it based on momentum considerations. As a result, publication was held up for several years, until 1931. Another obvious example was Theodorsen's off-hand "optical-illusion" pronouncement, but by that time Jacobs and Stack had acquired enough confidence and momentum to proceed on their own judgments. As a general rule, the speculations and doubts of experts in viewing new phenomena should not be overrated.

The essence of the idea that the critical speed could be related to the low-speed velocity profile of the airfoil was first stated by Briggs and Dryden in 1925 (ref. 11). However, the only use they made of it was to show that the trends in their observed critical speeds were qualitatively consistent with the concept. They never considered applying the idea as a tool to develop improved shapes. It remained for Stack and Jacobs to recognize the potential of this concept and to put it to quantitative use. They established the mathematical relationship between M_{cr} and the low-speed peak negative pressure coefficient, thereby making it possible for designers to estimate from low-speed theoretical or experimental data the critical speeds of their designs, and providing high-speed researchers with a practical theoretical tool for achieving improved forms. Stack clearly felt a sense of excitement and fresh "inspiration" from this accomplishment (ref. 20). In his view the "new" concept was one of the fruits of the combined pressure and schlieren study for the 4412 airfoil in 1934. Whether previous readings of Briggs and Dryden had planted the seeds of the idea matters little; the revelations of the 1934 research gave the concept real meaning and inspired its useful application.

It will be difficult for today's researchers to comprehend the procurement story of the 24-inch high-speed tunnel. That kind of quick action—design by the research staff in three or four weeks and construction for some $12 000 in less than a year—is rarely seen in the present complex organization. Facility procurements follow a complex process of reviews and approvals and many stages of design and construction involving several inhouse and outside agencies. Procurement of test models has followed a similar pattern. Of perhaps even greater concern than time and cost is the discouraging effect of these long and costly procurements on the interest and initiative of researchers.

Periodically throughout the history of NACA situations would arise, in the research programs as well as in facility procurements, where it was obvious that the normal agency procedures could not accomplish the job effectively within time or cost limits. 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. Almost invariably these special groups did an impressive job.

The use of this special-group technique, not only in emergencies but as a regular device in R&D and procurement programs for recapturing the benefits of the small organization, offers partial salvation to today's enormous bureaucracies, industrial as well as governmental.

INCREASING THE CRITICAL SPEED (1936–1944)

On the morning of August 31, 1936, I boarded a street car in Hampton, Virginia, and traveled to Langley Field to report for duty as a Junior Aeronautical Engineer at $2000 per annum. After the usual short indoctrination in his office, Mr. Miller escorted me to the 8-foot high-speed tunnel and introduced me to Russel G. Robinson who would be my boss. Robinson had been project engineer for this new facility since its conception in 1933 at about the time the 24-inch tunnel was started. Following the usual practice of that period, he had more or less automatically become head of the small group of researchers who would now operate the facility. The basic idea for this large tunnel is believed to have been first suggested by Jacobs. It was to be a "full-speed" companion to the "full-scale" tunnel, using the same drive power (8000 hp) to produce 500 mph-plus in an 8-foot throat as the full-scale tunnel used for its 100 mph-plus speed in a 30- x 60-foot throat. The name was later changed to the less vague "500-mph tunnel," and finally to the "8-foot high-speed tunnel." The very large power input in this closed-circuit tunnel had introduced an unprecedented heating problem which Robinson had solved by an ingenious air exchanger in which part of the hot air was continuously and efficiently replaced with cool outside air without the need for any auxiliary pumping or air cooling equipment.

We spent the rest of the morning examining the new tunnel and then walked down to the lunchroom on the second floor of the administration building. The entire professional staff and some of the support people, except for a few "brown-baggers," assembled here everyday for a simple but excellent plate lunch costing 25 or 30 cents (35 cents on steak days). Walter Reiser, in charge of "Maintenance," and also head of the employee's organization which operated the lunchroom, the Langley Exchange, personally marked down everyone's charges as they passed through the line and once each month collected payment. The lunch tables had white marble tops, a feature which was a great boon to technical discussions. One could draw curves, sketches, equations, etc., directly on the table, and easily erase it all with a hand or napkin. This great unintentional aid to communication was lost in later years when the lunchroom was replaced with a much larger modern cafeteria.

It was exciting and inspiring for a young new arrival to sit down in the crowded lunchroom and find himself surrounded by the well-known engineers who had authored the NACA papers he had been studying as a student. I well remember that first day at a table that included Starr Truscott, Ed Hartmann, and Abe Silverstein. There were no formal personnel development or training programs in those days, but I realize now that these daily lunchroom contacts provided not only an intimate view of a fascinating variety of live career models, but also an unsurpassed source of stimulation, advice, ideas, and amusement. An interesting consequence of these daily exchanges and discussions was that often no one originator of an important new research undertaking could be identified. The idea had gradually taken form from many discussions and in truth it was a product of the group. At the same time there were undoubtedly instances where perceptive individuals picked up new ideas from someone else's off-hand remarks and went on to develop them successfully, perhaps not remembering where the initial stimulation had come from.

Frequent references to these lunchroom contacts can be found. R. T. Jones tells of his first indoctrinations into the mysteries of supersonic flow by Jacobs and Arthur Kantrowitz in 1935 in "lunchroom conversations" (ref. 29).

After lunch that first day, Robinson took me on a tour of the various sections. I have a vivid memory of the 24-inch high-speed tunnel office. Stack and Lindsey were working up some test data which Stack discussed with characteristic intensity and impressive profanity.

The following morning Robinson outlined the NACA outlook at that time for high-speed aeronautics, what was expected of the 8-foot high-speed tunnel, and what part he wanted me to play. He said that it had been determined that about 550 mph was the probable upper limit of airplane speeds. Beyond this speed the occurrence of the compressibility burble would cause the drag to increase prohibitively "like throwing out an anchor." Our first job with the new tunnel would be to determine in detail what the high-speed aerodynamic characteristics for components and complete configurations actually were. Our next goal would be to develop improved shapes with higher critical speeds so that aircraft could approach as closely as possible to the ultimate limiting speed, perhaps even a bit higher than 550 mph. We would not invent advanced aircraft but would provide designers with accurate high-speed component data.

Our work in the 8-foot tunnel was necessarily mostly experimental because flow problems involving shocks held little possibility of theoretical solution. In effect the tunnel was used as a giant analog computer producing specific solutions to the complex flow problems posed by each test model. Many other Langley programs generated important theoretical advances, among them airfoil and wing theory, wing flutter, propeller noise, nose-wheel dynamics, stability, control, spinning, compressible flows, heat transfer and cooling, and others. Langley's principal theoreticians and analysts of the thirties included T. Theodorsen, I. E. Garrick, C. Kaplan, R. T. Jones, B. Pinkel, A. Kantrowitz, H. J. Allen, S. Katzoff, E. E. Lundquist, and P. Kuhn.

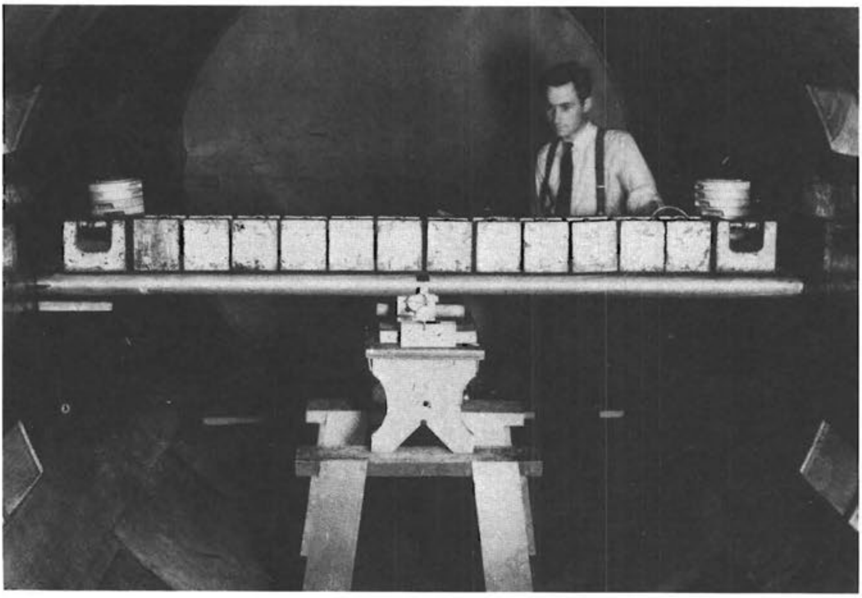

FIGURE 2.—*Typical airfoil in the original 8-Foot High-Speed Tunnel, weighted to determine deflection corrections. J. V. Becker in photo, 1937.*

At that time in 1936 "550 mph" seemed to all of us to present enormous challenges for distant future applications. True, 407 mph had been reached in 1931 by the last of the Schnieder Cup racers. And more recently Stack had calculated that an advanced racing-type airplane with increased power, retractable gear, and skin-type radiators could reach about 525 mph in spite of some 18 percent increase in drag plus a nominal loss in propeller efficiency due to incipient compressibility effects. But these extreme racing vehicles were so unlike any practically useful airplanes as seen in 1936 that they had little impact on our outlook.

There was an air of pregnant expectation about the splendid new 8-foot tunnel as I started work in September 1936. My previous experience had been limited to the venerable wooden tunnel at New York University which drew only 250 hp and had a speed of about 60 mph. The 8-foot tunnel, gleaming with polished metal and fresh paint, was still undergoing acceptance testing of its 8000-hp drive motor. The mechanical aspects of the operation were supervised meticulously by one Johnny Huston, a sharp-tongued veteran NACA shop mechanic who seemed to relish catching and correcting the not-infrequent mistakes of neophyte engineers in the mechanical operations of the tunnel. I wondered if my talents would prove worthy of this impressive and demanding facility.

The acceptance testing had to be done late at night when the Hampton power plant was able to provide us with the necessary 5500 kw. Airfoil force tests and test-section flow surveys were made concurrently with the motor tests (fig. 2). In those days the entire operation was conducted by one engineer and one mechanic in the igloo-shaped test chamber. (One other engineer involved in the electrical drive measurements was present only during the acceptance tests in the drive equipment room.) During a test, the engineer controlled the tunnel speed, changed angle of attack, pushed the "print" button for the scales at selected times, recorded visual data readings from the scales, made quick slide rule calculations of the coefficients, and plotted the results to insure that good data were being obtained. (A recent visit to a comparable NASA tunnel during a test run revealed a test crew of no less than two engineers and two engineering aides plus three mechanics, for a similar type of operation except that the preliminary coefficient plots were produced by an automatic computer and data plotter.)

One night during my second week on the job just before I closed the airlock doors at the entrance to the test chamber for a test run, an unusual-looking stranger dressed in hunting clothes came in and stood there watching my preparations. Robinson had advised me not to allow visitors in the test chamber during a high-speed run primarily because the pressure dropped quickly to about two-thirds of an atmosphere, the equivalent of about 12 000-foot altitude. Assuming that the visitor had come in from one of the numerous duck blinds along Back River, I said firmly, "I will have to ask you to leave now." Making no move he said, "I am Reid," in such ponderous and authoritative tones that I quickly realized it was Langley's Engineer-in-Charge whom I had not yet met. No one had told me that Reid, who lived only a couple of miles from Langley Field, often came out in the evening, especially when tests of electrical equipment were being made (he was an electrical engineer).

When I came to know Reid better, the memory of this incident softened into proper perspective.

About a year later at 3:06 a.m. on October 8, 1937, I was running the tunnel at full power and had just promised the operator at the Hampton generating plant that I would reduce power gradually when, without warning, there was a sickening break in the steady roar of the 550-mph wind (ref. 30). Acrid smoke filled the test chamber as I pushed the red emergency stop button, no doubt blowing the safety valves in Hampton. On entering the tunnel we found the huge multi-bladed drive fan twisted and broken. The cast aluminum alloy blades had failed in fatigue from vibrations induced by their passage through the wakes of the support struts. Operations were suspended until March 1938, and the staff was temporarily dispersed to other sections.

Demonstration runs of the 8-foot tunnel were made for the last of the NACA Annual Engineering Conferences, held in May 1937. Naturally, we wanted to dramatize the compressibility burble and to do so we mounted one of the worst (lowest-critical-speed) NACA cowling shapes in the tunnel with a static pressure orifice near the suction peak and a total-pressure tube on the surface of the cowl afterbody which provided a qualitative indication of drag. There was no way to actually see the shock wave on the cowl, but at Robinson's suggestion, we set up a large chart with a red light bulb directly behind a line of small slots at the part of the cowl drawing where the shock was located. During the demonstration the tunnel speed was advanced rapidly to the critical speed, about 400 mph. At that point the suction-pressure tube indicated local sonic conditions on the chart. At a slightly higher speed the total pressure tube showed a dramatic increase in drag and the red light was flashed on (manually by the tunnel operator) showing the presence of the shock. Runs were made for six groups of visitors on each of the three conference days and we received many compliments. Orville Wright and several other pioneers were among the visitors. I had time for a chat with Alexander Klemin, my college mentor, who perennially reported on these NACA affairs for *Aero Digest*.

The desire to dramatize compressibility effects in that period reached its peak with our high-speed testing of a model of the DC–3 configuration in 1938. Although that stolid vehicle cruised at only about 160 mph, we tested it up to 450 mph to show the speeds at which the various components, designed without regard for compressibility, became afflicted with shock wave problems. The tests showed the drag rise for the engine cowls started to develop at speeds as low as about 350 mph. For the first time we noticed the adverse effects of interference between components; the critical speeds of the cowls and of the wing were reduced about 20 mph by the presence of the fuselage (ref. 31).

The predicted critical speeds of a large number of existing airfoils and bodies were determined by Robinson and Ray H. Wright from their low-speed pressure distributions as a necessary prelude to the development of improved shapes (ref. 32). Stack led the effort in the period 1936–1940 to find airfoils with higher critical speeds, aided by Robinson, Lindsey, and others. It was a relatively simple matter to determine analytically from thin airfoil theory the uniform-load camber lines which would give the lowest possible induced local velocities for airfoils of zero thickness. There was no way then, however, to calculate the optimum thickness distribution, and a cut-and-try process had to be resorted to. A considerable number of systematically varied thickness distributions were analyzed by the Theodorsen method to obtain the theoretical incompressible pressure distribution, until one giving a nearly uniform distribution was found. Curiously, it was almost identical to one of the NACA family of airfoils previously defined, the 0009–45 (ref. 20). Combining this thickness distribution with the uniform-load mean camber line gave what was called the "16-series" family, the first of the high-critical-speed low-drag families (ref. 33). Selected members of the family were tested at high speeds and first reported in the general literature in 1943 (ref. 34). (An extended and improved series of tests was reported in 1948 (ref. 35), and in 1959 tests at transonic speeds up to Mach 1.25 were reported (ref. 36)).

The 16-series sections found immediate acceptance by propeller designers, not only because of their high critical speeds but also because of their relatively thick convex shape in the trailing edge region which was desirable from the structural standpoint. A remarkable testimony to these sections was heard at the NASA Airfoil Conference of March 1978, some 35 years afterward, when a spokesman for propeller manufacturers said that the 16-series sections, still used in modern propellers in thickness ratios from 2 to 10 percent, provided excellent performance.

Although it was originally thought that the 16-series sections would be desirable also for high-speed wing applications, it rather quickly was learned that they were not suitable. The problems included: a low maximum lift coefficient, a narrow operating range for high M_{cr}, a tendency for flow separation in the trailing edge region for the thicker sections, and laminar flow characteristics inferior to the 6-series sections which also had high critical Mach numbers. It was also found that the uniform-load camber line used in the 16-series family, while it obviously gave the highest possible critical speed for zero thickness, did not give the highest possible M_{cr} for finite thickness. Slightly higher M_{cr} could be obtained with a camber line which concentrated the lift loading toward the rear (ref. 37), but the small advantage is obtained at the expense of an undesirable rearward shift in center of pressure. An interesting later attempt to develop high-critical-speed sections with large leading-edge radii and good maximum lift characteristics was made by Loftin (ref. 38) with some success, but unfortunately this program was terminated in mid-course when NACA management decided to phase out the airfoil program in the early fifties.

In late 1939, we undertook an unusual project for Howard Hughes—the only privately-funded testing ever done in the Langley 8-foot high-speed tunnel. Hughes was represented by his aerodynamics consultant, Col. Virginius E. Clark, an old-timer in aeronautics and designer of the well known "Clark Y" airfoil. Carl Babberger, a former Langley engineer, was Hughes' Chief Aerodynamicist and he was also present for the tests. (Clark explained the simple philosophy behind the "Clark Y" section: it was simply the thickness distribution of a Goettingen airfoil deployed above a flat undersurface—the flat feature being highly desirable in the manufacture and operation of propellers as a reference surface for applying the protractor to measure or set blade angles. An unhappy problem in using the Clark Y was the interdependence of camber and thickness ratio.)

The most remarkable aspect of this Hughes program, however, was the fact that the test models were not actually representations of the configuration Hughes was designing. To preserve company secrecy, the test models had been designed to answer questions relative to nacelle placement, etc., without revealing the real configuration to NACA engineers.

The underlying theme for much of our work in the first few years of the 8-foot high-speed tunnel was "to provide accurate component data for designers." Often plans for a forthcoming test program would include sketching the anticipated data plots in advance, so that running the test seemed more a matter of nicely filling in the data points rather than a search for anything new. Our Chief of Aerodynamics, Mr. Miller, encouraged this conservative philosophy, telling the staff at one of the monthly department meetings, "Our aim is to produce good sound research data —nothing spectacular, just good sound data." I can provide this quote with confidence because, even in those days when there was little thought given to R&D philosophy, agency goals, etc., it provoked some negative reactions among the more lively members of the staff after the meeting.

Dr. Lewis had a broader outlook and a willingness to invest occasionally in speculative new ideas such as the thrust-augmentor work which led to the induction drive scheme for the first high-speed tunnels. A specific instance occurred during a 1938 visit of Lewis to our office at the 8-foot tunnel to review recent results and forthcoming test plans. He approved our plans but advised us to "take some shots-in-the-dark now and then."

The Langley of the thirties did not think of itself as a part of the federal bureaucracy. Broadly directed by a committee whose distinguished members served without compensation, and managed by a minuscule Washington office, the Langley operation was spiritually as well as physically separated from Washington. The youthful staff had been largely handpicked in one way or another to form an elite group unique in the federal system. It was possible for the entire staff of this small organization to become personally acquainted all the way up through Lewis, and this resulted in a beneficial sense of family. Whatever their personal foibles, the senior managers, all of whom held career appointments, were intensely loyal to the organization. They could be relied on for continuing interest in and understanding of our researches, and for continuing support and advocacy. These important intangibles are missing in large agencies whose top managers come and go at four-year intervals with changing presidential politics. The costly, crippling internal friction common in today's large agencies, in the form of voluminous paperwork, repetitious program reviews and justifications, lengthy procurements, unending staff meetings, etc., were virtually nonexistent in the Langley of the thirties. We were also blessed in those days with relatively simple research problems which yielded to straightforward pragmatic research methods. But this happy situation was soon to deteriorate in the enormous expansion and other changes wrought by World War II.

Crossing the Atlantic on the dirigible *Hindenburg* in the fall of 1936, Lewis visited Germany and Russia and saw many of their aeronautical research installations. On his return he spoke to the Langley research staff in the large room on the second floor of the Engine Lab building used for such convocations. His principal impressions 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 any new aerodynamic or propulsion concepts or any new research results (ref. 39). He made a second similar visit to Germany in June 1939 which further impressed him with Germany's preparations for war. But again he learned little of their advanced programs. (The Heinkel He 178, the world's first turbojet-powered airplane, was then being readied for its first flight which occurred on August 27, 1939.) These Lewis visits to Germany together with those of Lindbergh provided the justifications needed for major expansions of facilities and staff at Langley starting in 1938, and for the establishment of two major new NACA centers at Cleveland, Ohio, and Sunnyvale, California, well before December 7, 1941. Significantly, however, there was little effect of any of these visits on the nature of our research programs or the problems being tackled prior to the actual start of the war. We were increasingly conscious that a war was coming, but considered all of our existing programs apropos to the improvement of military aircraft.

Although there was considerable advocacy of "military preparedness" in the press at that time there was little pressure on us by NACA management to do anything different in character from what we had been doing. There was no real sense of emergency or war peril to motivate a search for radical new weapons or bold new concepts in aircraft. Aviation had been making rapid progress and the NACA contributions had been substantial. Although there was a minority group of vocal detractors, the majority opinion was clearly that the United States led the world in technical development. NACA believed that continued supremacy could be assured by expansion of its existing programs through increases in manpower and conventional test facilities. Most NACA veterans believe that it would have been quite impossible in the pre-war period to have obtained 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 and supersonic aircraft (ref. 40).

A noteworthy exception to the generally conservative pattern was E. N. Jacobs' investigation of a full-scale Campini system of jet propulsion in the 1939–1943 period. Initially, Jacobs was motivated more by his penchant for new ideas than by a sense of war emergency. A great deal of effort went into this project, but like many hybrid concepts it had major limitations, and it fell by the wayside in 1943, yielding to the pure turbojet. The Jacobs group harbored a misconception in this project which was shared by the American engine companies at that time; they believed the gas turbine (turbojet) engine would be impractical for aircraft because of prohibitive structural weight (refs. 41, 42).

Not really in the same category as the Campini effort but worthy of special note because of its important implications for turbojet development was the axial-flow compressor designed and tested in 1938 by E. W. Wasielewski and E. N. Jacobs. Intended for the piston-engine supercharger application, this machine, designed on the basis of airfoil theory, developed an efficiency of 87 percent at a pressure ratio of 3.4, a convincing early demonstration of the high performance potential of this type. This result is believed to have later influenced American turbojet designers to favor the axial over the centrifugal compressor (ref. 41). Interestingly, Jacobs himself 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. He believed this might be an inherent weakness preventing practical applications (ref. 42). It is significant that both this early misconception and the one relating to excessive turbojet weight involved structural considerations outside the field of expertise of the Jacobs group.

In 1939 R. G. Robinson became an assistant to Lewis in Washington and Stack replaced him as head of the 8-foot high-speed tunnel section. Stack's upbringing under Jacobs, plus his natural inclinations, relaxed and enlivened the atmosphere at 8-foot. There was a lessening of the emphasis on data-gathering and chore-doing for industry. There was also a pronounced increase in the level of talk, badinage, and practical-joke playing. Although his entire background had been in high-speed airfoils, Stack rather quickly became interested in the other areas of our work—high-speed cowlings, internal flow, interference effects, and aircraft configuration problems.

The war period at NACA has often been described as a time when "fundamental" or "general" research was largely forsaken and replaced by war work for specific aircraft. This is inaccurate. Virtually all of the general programs underway in 1939, together with their natural extensions and many new programs as well, were completed during the war years, subject to occasional delays due to the specific work. Much of the burden of specific configuration testing fell upon the horde of new employees; extended facility test periods were obtained by multiple shifts and the 48-hour week. The involvement in general research undoubtedly declined on a per capita basis, but in absolute terms my belief is that it increased.

The long exhausting hours which NACA employees generally are said to have put in during the war is another myth. Only a small minority at Langley worked more than the 48-hour week except for infrequent stints of additional overtime. Of course there were some notable exceptions, one of the more interesting occurring in E. N. Jacobs' section. About a year before U.S. entry into the war Jacobs unilaterally imposed a 48-hour week on his men, with no increase in pay, in order to expedite their growing programs. He also let it be known that leave requests were not likely to be approved unless the applicant had put in considerably more than the 48-hour minimum. Surprisingly, there were only a few protests. The fact that a strong section head could get away with a high-handed move of this kind implies both patriotic motivations in the staff and relaxed flexibility in Langley personnel operations at that time. Such a move would be unthinkable by any federal agency in today's world.

The influx of thousands of new employees during the war period caused irreversible changes. The selective standards which had provided the exceptional talent of the twenties and thirties 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 to describe inveterate tunnel operators.

A distinctly pleasant aspect of the large expansion of the 8-foot tunnel staff was the addition of a female computing group. They not only took over most of the slide-rule work and curve plotting formerly done by the engineers, but also added an interesting social dimension.

The staff relaxed through all of the usual sports and social events with little apparent effect of wartime pressures. Five of us had formed an informal golfing group consisting of Donald Baals, Henry Fedzuik, Carl Kaplan, Stack, and myself. Stack called it the "Greater Hampton Roads Improvement Society and Better Golfing Association." I well recall the first afternoon we played at the Yorktown course. Stack had never played before and had no clubs of his own, but we offered to loan him an old bag with a broken strap and some of our spare clubs. Fedzuik, who was the chief humorist of the group, had often been the butt of Stack's practical jokes and saw here a welcome chance to turn the tables. With enthusiastic help from some of the rest of us he lined the bottom of Stack's bag with some 10 pounds of sheet lead. We also made sure the bag had a full complement of clubs, and we told Stack that caddies were used only by the rich and decrepit. By the start of the back nine, with a score card showing well over a hundred in spite of considerable cheating, Stack was seen to start dragging the bag along behind him, his expletives becoming louder and more colorful, and a short time later he discovered what had been done. Understandably, he always examined his equipment very suspiciously at subsequent sessions.

In mid-1944, Stack was notified that he had been chosen to present the Wright Brothers Lecture of the Institute of Aeronautical Sciences for that year, the first of many honors he was to receive. He was now recognized not only as NACA's leading expert in high-speed aerodynamics but also as an unusually colorful character. This lecture (ref. 20) was in essence an updated broader version of Jacobs' Volta paper. The compressibility burble phenomena were illustrated and discussed in full detail, results of the systematic efforts to obtain improved components with high critical speeds were reviewed, and the stability and control problems of advanced aircraft in dives through shock stall were discussed for the first time in the open literature. Several of us, particularly W. F. Lindsey, participated in making new flow photographs and a schlieren movie of shock-stalled flows in the 4 x 18-inch tunnel which had been placed in operation in 1939, superseding the 11-inch high-speed tunnel. It had higher choking Mach numbers and the 4-inch width made it better suited for airfoil flow photography. The movie proved to be the highlight of the lecture. H. L. Dryden, commenting on the talk 20 years after his pioneering high-speed tests, said, "We did not understand these [high-speed flow-breakdown] results at the time [1925]. The lecturer and his associates have now given us a complete interpretation. . . . The direct shock loss is much smaller than the loss due to [shock-induced] separation (ref. 20)."

In the course of producing these pictures a mysterious oscillating shock structure was observed in the wake of a circular cylinder which engendered much discussion. Stack dubbed the apparition "Yehudi" but this appellation was edited out of the text. (Among other names he coined were "Reichenschmutz" for a ducted propulsion scheme, and "Rumble-gut-whiz" for an unsuccessful noise-making device considered by the Army during the war; it was to be attached to diving airplanes in the hope of terrifying the enemy.)

A figure was included in the lecture emphasizing the inadequacy of the critical Mach number as an index of force break—either the Mach numbers of force break or the severity of the subsequent changes. A long discussion of "supercritical flows" was included but unfortunately this covered only the speed range up to about 0.83, the highest speed at which reliable results could be obtained with the 4 x 18-inch tunnel. Interest in the entire transonic range up to low supersonic speeds was only starting to build up at this time, as a consequence of the exciting new propulsion possibilities opened up by the turbojet and rocket engines. Stack's Wright Brothers Lecture brought to an end the period in which the true nature of the shock stall had been exposed in detail and the concept of designing for the highest possible critical speeds to avoid shocks had been fully exploited. For the next decade or more, the emphasis would be on developing airfoils and wings capable of efficient performance through the entire transonic speed range. Only the threshold phenomena had been treated so far, and what happened beyond shock stall in the transonic zone from about Mach 0.8 to up to 1.2 was still unrevealed.

COMMENTARY

In the light of later events, NACA's 1936 vision of the "550-mph" propeller-driven piston engine airplane as the ultimate goal of high-speed aeronautical research was obviously too shortsighted and restrictive. Focusing the effort totally on the immediate problem of increasing the critical Mach number of conventional aircraft components denied consideration of the broader and far more important "barrier" problem areas of transonic flight, including new propulsion concepts, radical configurations, transonic facilities, etc. A small cadre of the more imaginative thinkers could have been separated from the main effort to provide high-critical-speed data for industry, and encouraged to look beyond the speed range of the existing high-speed tunnels at these "barrier" problems. Even in 1936, it was predictable with certainty that within a few years the approach of improving the critical speed would reach a point of zero return, leaving the barriers still to be reckoned with.

The 550-mph airplane was achieved in the early forties by the Germans in the form of the turbojet-propelled Me 262, which went into service about the time of Stack's Wright Brothers lecture in 1944. In January 1945 every airplane in a 12-plane American bomber squadron was destroyed by Me 262's. Only the German failure to produce them in large numbers made possible continued Allied bombing (ref. 41). The Germans were also applying variable-sweep (with outboard pivot locations) to more advanced aircraft as the war terminated (ref. 43). These shocking developments together with the German long-range rocket missiles produced in NACA a "large loss of prestige. Never had NACA relations with the industry, Congress, and the scientific community sunk so low" (ref. 40).

NACA's prestige was largely recovered during the next five years, not by the usual research services which were continued as necessary, but by several bold new ventures, the most noteworthy of which were the transonic research airplanes, the impressive rocket-model testing at Wallops Island, and the transonic wind tunnels. However, the unique esprit de corps and effectiveness of the NACA organization of the twenties and thirties was never fully regained.

SUPERCRITICAL AND TRANSONIC AERODYNAMICS (1945–1956)

The emergence of the transonic research airplanes in the mid-forties (Chapter III) greatly heightened our interest in the supercritical behavior of airfoils and in developing testing techniques for exploring the supercritical and transonic regions. But an even stronger motivation had developed concurrently with the advent of the turbojet engine. In 1944, the Army had sent an XP–59A, the first U.S. jet-powered airplane, for flight demonstrations at Langley. Standing beside the main runway and watching this airplane fly by at nearly 400 mph, we sensed for the first time that here was the key to transonic and supersonic flight, a practical new propulsion concept capable of the enormous power required to penetrate the transonic region. The so-called "sound barrier," which had been almost universally thought of as a set of adverse aerodynamic problems, in reality also involved a fundamental limitation of the piston engine due to its fixed "displacement," or capacity to inhale air for combustion. Since the displacement was independent of airspeed, no significant increase in peak power could occur as the flight speed increased. Thus, there had been no realistic hope that piston engines could be developed in the sizes that would have been needed for transonic flight; the transonic "barrier" was actually as much a piston engine barrier as an aerodynamic barrier. The jet engine on the other hand ingests a volume flow of air that increases as the flight speed increases, permitting a continuous increase in power in contrast to the fixed power of the piston engine. This power increase is significantly augmented at high speeds by the "ram" pressures of the air which provides supercharging and improves the cycle efficiency.

We understood the principles and enormous potential of the turbojet only vaguely at the time of the XP–59A demonstration. Very little data were available to us on the details and performance of the G.E. I–16 engine. Several of us spent the next few days in exciting speculations of possible jet-engine thermodynamic cycles, airflow characteristics, and crude performance estimates, which gave us a better understanding. K. F. Rubert, who had taught internal combustion engines at Cornell, undertook a more careful systematic analysis, published in 1945 in a paper which I reviewed as chairman of the Langley Editorial Committee (ref. 44). (Periodic editorial duties of this kind were of great value as a means of education and stimulation of all involved—in addition to their obvious direct benefit to the quality and accuracy of Langley reports.)

By now our limited goal of the 550-mph subcritical airplane of the mid-thirties had become meaningless and we could foresee the imminent achievement of supersonic flight. Few doubted that operational supersonic military aircraft would soon follow the research airplanes. The need to acquire accurate supercritical and transonic aerodynamic data had become acute, and Langley researchers responded to the challenge with considerable inventiveness. Eight innovative techniques were eventually devised and explored in various forms by NACA, ending with general acceptance of the semi-open tunnel for two-dimensioned airfoil testing up to Mach 1, and the slotted transonic tunnel for wing and aircraft configuration testing as the most satisfactory devices. (These developments are listed in the Appendix and discussed in detail in Chapter III.)

Unknown to us, the Italians had already succeeded in obtaining airfoil force data through the supercritical range up to about Mach 0.94, and the Germans to about 0.92. We first learned of this in 1944—before any of the new Langley schemes materialized—upon the arrival of Antonio Ferri, formerly of the Italian aeronautical laboratory at Guidonia, and recently an Italian Partisan in the war. Ferri brought with him extensive airfoil data from their tests in a semi-open high-speed tunnel in the early forties. He completed analysis of the data at Langley and we published the results in a NACA wartime report (ref. 45). His English at that time was negligible, and I wrote the final text after much heated consultation with Ferri and help from Lou Nucci who acted as interpreter. (Major confusions arose from Ferri's pronounciation of "subsonic" and "supersonic," both of which sounded to me like "soup-sonic.") The proportions of Ferri's tunnel (1.31 feet across the open top and bottom and 1.74 feet on the closed sides) corresponded to 43 percent of the perimeter being open. This closely approached the value of 46 percent suggested by a theoretical analysis of Wieselberger in Germany (ref. 46) as the correct proportion for zero "blockage" (zero axial-velocity correction) applicable to a three-dimensional test model. However, this large degree of "openness" had a serious drawback in the large pulsations which occurred at high speeds. None of us was quite sure of the validity of the semi-open tunnel technique at that time.

Despite the questions of technique, Ferri's data revealed a most important new finding: the loss in lift associated with the compressibility burble did not persist indefinitely. At about Mach 0.9 a marked recovery in lift occurred, suggesting that the separated ("shock-stalled") flow tended to disappear as Mach 1 was approached. Later that year support for this result was indicated in tests of small wings by means of the "wing-flow" technique (ref. 47). In 1946 we obtained German airfoil data from their large 2.7-meter closed-throat tunnel (ref. 48) which provided further verification at speeds up to Mach 0.92. And early in 1947 the first airfoil pressure distributions ever obtained at Mach 1 were successfully measured in our rotating-disc annular transonic tunnel (ref. 49). These showed conclusively that at Mach 1 the shocks had moved to the trailing edge and the flow was supersonic about the entire section except for a small region at the blunt leading edge. The German tests had included a systematic study of the effects of airfoil camber at high speeds which clearly showed that conventional positive camber was undesirable for Mach numbers greater than 0.75, and in fact best lift-drag (l/d) ratio was obtained with negative camber at supercritical speeds, a result with which Ferri's data agreed.

I had become involved in a study of all available transonic data in 1947 in connection with writing a chapter on "Transonic Aerodynamics" for a prospective aeronautical handbook (ref. 50). At Stack's suggestion

I discussed my airfoil material at a meeting of the Langley General Aerodynamics Committee on January 16, 1948. This was the first time that many of the members had seen the German results and the general agreement of all of the new data as regards flow phenomena and trends of airfoil performance at supercritical speeds approaching Mach 1 (ref. 51). Dick Whitcomb was an interested participant at this meeting. In commenting on the effects of camber at supercritical speeds Whitcomb suggested that upper surface curvature might be the important parameter and that the use of "proper" curvature might reduce the upper-surface shock strength and tendency of the flow to separate (ref. 51). Some 16 years later he would resurrect this idea and apply it successfully in the "supercritical" airfoil (see pp. 55ff.).

A few months after this meeting I presented the unclassified parts of my summary material at a NACA University Conference (ref. 52). Airfoil shock and separation patterns inferred from the available force and pressure data (refs. 47, 49) throughout the transonic zone were illustrated (fig. 3). The points brought out in the discussion included:

- The region of shock-stalled flows ("compressibility burble") was limited to speeds approximately between Mach 0.75 and 0.95.
- By Mach 0.95 in most cases the shock had moved off the rear of the airfoil, the flow field was entirely supersonic except for a small region near the leading edge, and no significant viscous separation effects were present.
- Beyond Mach 1 a smooth transition to purely supersonic airfoil characteristics could be expected.
- Camber was undesirable beyond about Mach 0.75. (Schlieren pictures were used in ref. 52 to contrast the shock effects on a thin uncambered airfoil of low surface curvature and the much larger adverse effects on a cambered airfoil of large surface curvature.)

Ferri's successful use of the semi-open tunnel, together with encouraging results of Langley studies of this configuration by Donaldson and Wright (ref. 53) and Lindsey and Bates (ref. 54) led to our decision in the fall of 1947 to convert the 4 x 18-inch high-speed tunnel to the semi-open arrangement with the object of systematic airfoil testing at Mach numbers up to 0.95, and higher if possible. The first results, obtained in 1948, showed that the tunnel (now 4 x 19 inches in size) could be operated with 4-inch chord models at a nominal Mach number of 1.0, but it was not immediately certain that the sonic results were valid. This tunnel was ideally proportioned for schlieren photography, and from the start impressive photographs were obtained which provided the first visual proof that our speculations about the flows at Mach 1 based on force and pressure data were correct. Figure 4, constructed from photographs taken in 1949, contains typical results from this program. The top row of photographs, for Mach 1, are of particular interest, showing that the shocks lie downstream behind the trailing edge. The flow on the airfoil is virtually separation-free and entirely supersonic in character except for a small subsonic region near the leading edge. Extensive systematic pictures of this kind for other sections were obtained in the 4 x 19-inch tunnel by B. N. Daley and R. S. Dick and published later (ref. 55). Similar flow pictures were also obtained

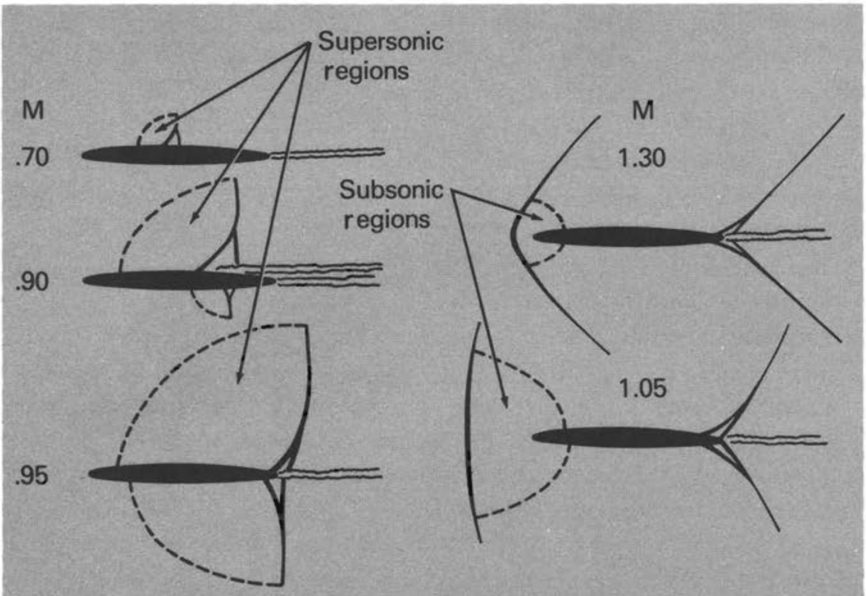

FIGURE 3.—*Airfoil flow patterns at transonic speeds discussed at NACA University Conference, 1948.*

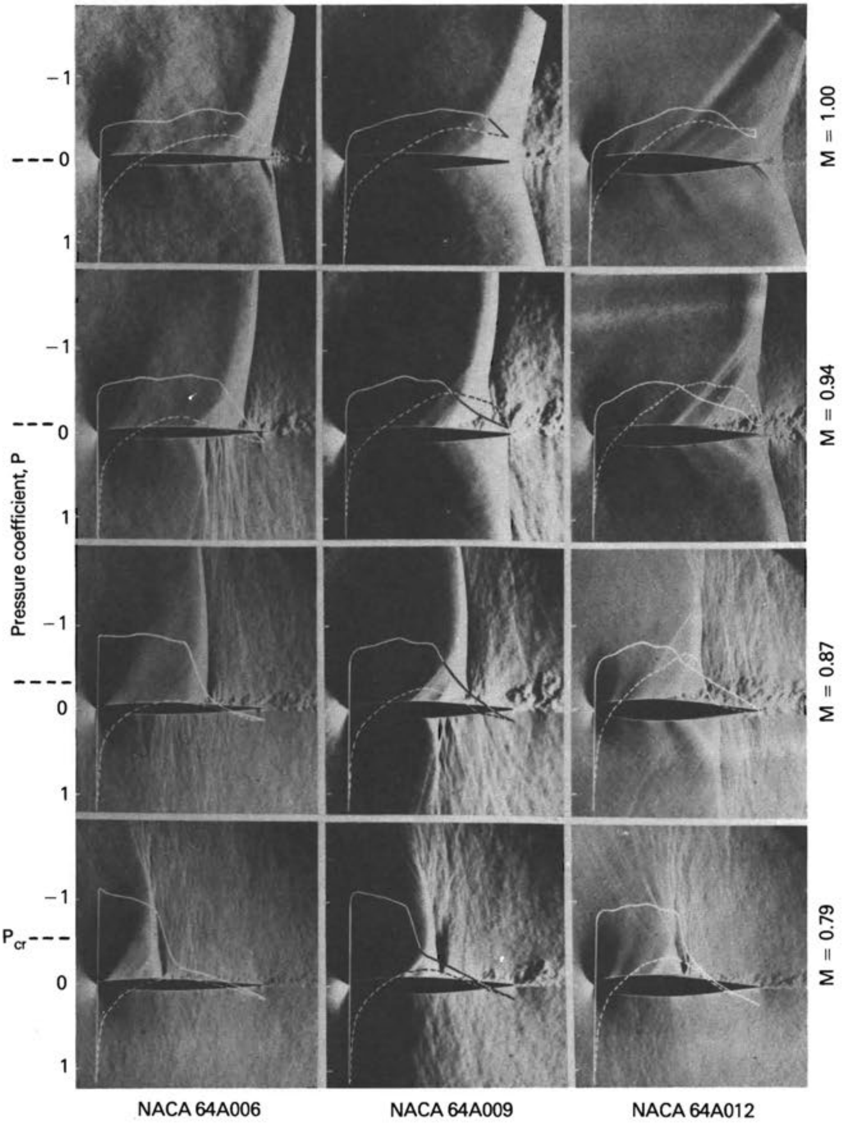

FIGURE 4.—*Transonic flows and pressure distributions, Mach 0.79 to 1.00. Angle of attack, 3.2 deg. From the 4 x 19-Inch Semi-Open High-Speed Tunnel, 1949.*

by the British some years after the first Langley work and were used by W. S. Farren in his Wilbur Wright Memorial Lecture of 1955 (ref. 14).

It is most important to note that our large burst of understanding about airfoil behavior beyond shock stall was acquired in the 1945–1947 time period, several years before any data on this problem were obtained from the research airplanes. My summary airfoil paper (ref. 52) was prepared in the spring of 1948, before the X–1 pressure data had been obtained. When I first saw the X–1 pressure data for the 10 percent thick wing about a year later the fact that it confirmed what was already known from the wind tunnels was satisfying but not at all surprising. Nevertheless the wing pressure distributions obtained in flight on the X–1 were of very great value because they provided the ultimate indisputable basis for judging the relative merits of the various ground facilities. The basic airfoil used on the number 2 airplane was the NACA 65–110, and both the Annular Transonic Tunnel and the 4 x 19-Inch Semi-Open High-Speed Tunnel programs had scheduled this section for their initial tests in anticipation of critically important comparisons. (A minor flaw in the plan was discovered after the tests had been made; in building the airplane the slight cusp in the basic 65–110 section had been removed for structural reasons, and this caused a minor change in the flight pressures just ahead of the trailing edge.) Figure 5 compares the X–1 flight data with the results from the two transonic facilities at Mach 1. Agreement with the 4 x 19-inch tunnel was considered excellent. The annular transonic tunnel data, although showing the generally correct shape, indicate pressures consistently too high. This same type of discrepancy was noted in subsequent tests of other sections and was never satisfactorily explained. Of the several transonic techniques only the 4 x 19-inch semi-open tunnel remained active in airfoil testing throughout the concluding years of the NACA program.

An early airing of our new knowledge of airfoil behavior near Mach 1 was made by Daley and Habel at the NACA Transonic Airplane Design Conference of September 1949 (ref. 56). During preparations for this meeting both the 4 x 19-inch tunnel data and the X–1 data were so new that Daley balked at presenting them without more time for analysis, but he finally yielded to management pressure. No conclusions were drawn, however, concerning the relative merits of the test techniques.

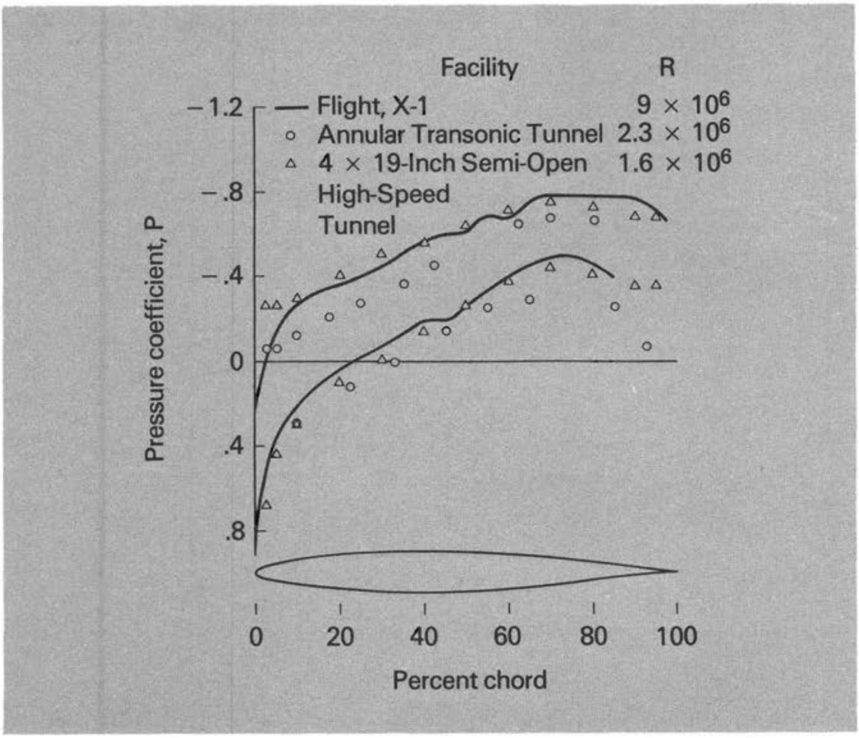

Figure 5.—*The X–1 pressure distributions compared with those of the Annular Transonic and the 4 x 19-Inch tunnels. Mach 1.0, NACA 65–110 airfoil,* $c_n = 0.41$.

The discovery that airfoil flows beyond about Mach 0.95 did not suffer from significant viscous separation effects lent new encouragement to the theorists. It had been previously believed almost universally that sonic flows in real gases would be characterized by large viscous separation effects, so that any theoretical treatment, based on the usual ideal gas assumptions, would have little realism. Thus the main theoretical concentration up to the mid-forties was on refinement of subcritical compressible-flow calculations. While this was consistent with the original belief that practical aircraft would not be able to operate much above the critical speed, in retrospect it is apparent that these efforts were not very profitable. The simple approximations developed early in this work were adequate for most engineering purposes, although minor refinements were laboriously attained later.

By 1947, however, an increasing number of theorists, encouraged by the new experimental findings, started to tackle the transonic problem in a variety of new ways. The development of the transonic similarity laws was a useful aid in data correlations, although these laws, of course, provided no solutions for any flow problems. Major progress came when the special case of the wedge airfoil at zero angle of attack at Mach 1 was solved by Guderley and Yoshihara in 1948 (ref. 57). I was privileged to see this accomplishment before its publication when Guderley visited Langley to discuss the work with A. Busemann, who had been assigned to the Compressibility Research Division after he had been brought to this country under the auspices of a Navy postwar program. For some years previous Busemann had consulted with Guderley on this problem and had contributed suggestions for its solution. The initial solution was for a cusped wedge shape, but this was followed shortly by similar results for a symmetrical double wedge (ref. 58). These results were very important to us; at long last we had theoretical sonic pressure distributions against which the experimental data from our new test techniques could be evaluated. These assessments would constitute a valuable supplement to the X-1 data as a means of insuring the validity of the experiments. I enthusiastically arranged for tests of the wedges in both the Annular Transonic Tunnel and the 4 x 19-Inch Semi-Open High-Speed Tunnel (ref. 59). The gratifying results (fig. 6) were presented for the first time at the September 1949 conference (ref. 56). A photograph of the flow about the wedge at Mach 1 confirmed the absence of any significant viscous separation effects except for a very small bubble just downstream of the sharp crest.

An important feature of the Guderley flow field was a region of smooth shockless deceleration of the local flow downstream of the crest of the wedge, caused by reflections of the expansion waves from the curved sonic line extending upward from the crest. The reflections from this free boundary were compression waves which decelerated the flow in smooth reversible fashion. Previously, for conventional airfoil shapes at low supercritical speeds, no such shockless compressions had been identified and it was thought that shocks were the inevitable device employed by nature to return the flow of stream velocity.

We now know that a considerable degree of smooth recompression prior to the terminal shock can occur for a wide variety of airfoil shapes near Mach 1. Actually this could be seen in the conventional airfoil pressure distributions obtained in the late forties at Mach 1 (see fig. 4, top row, for example). A more direct indication of the effect can be seen in fig. 7 which shows a Mach 1 pressure distribution obtained in 1948 in the Annular Transonic Tunnel. By comparison with supersonic expansion theory the measured pressures over the rearward portion of the airfoil were unaccountably high, and not understanding the possibility of the recompression effect we speculated that boundary layer growth might be the cause. The effect is actually primarily recompression due to reflections from the sonic line and secondarily the boundary layer contribution. Theoretical treatment revealing that all conventional

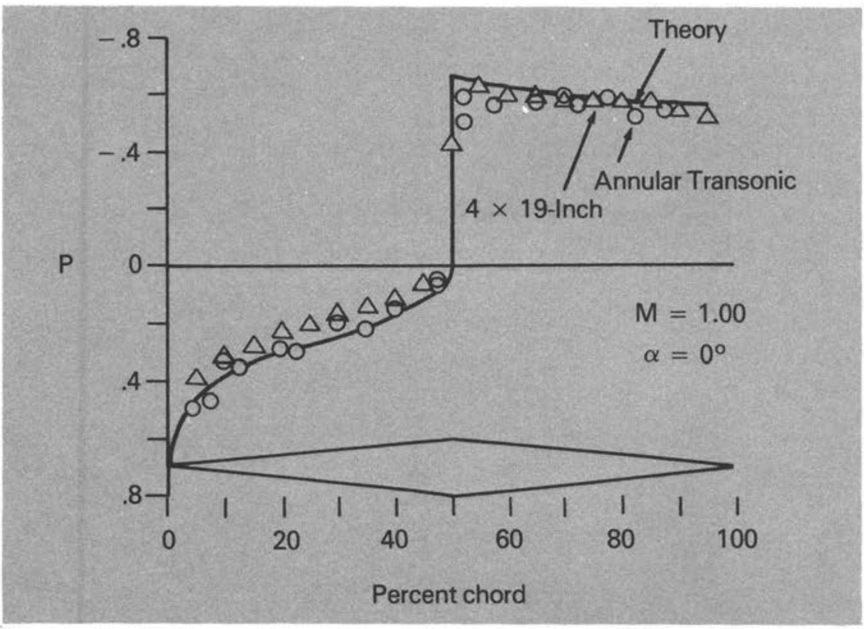

FIGURE 6.—*Guderley theory for Mach 1 compared with Langley transonic tunnel data.*

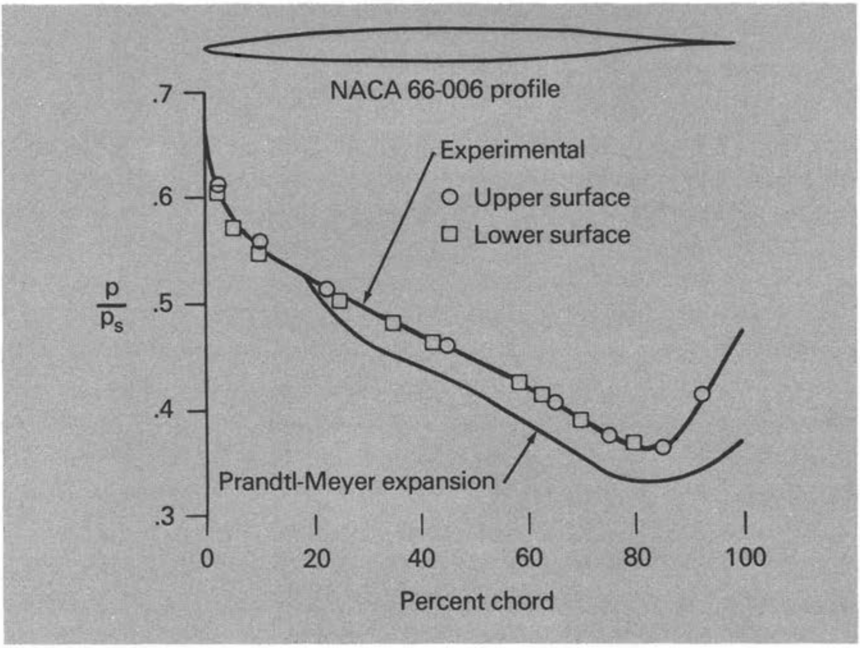

FIGURE 7.—*Pressure distribution obtained in the Langley Annular Transonic Tunnel at Mach 1. NACA 66–006 airfoil at zero angle of attack.*

sections experience this effect near Mach 1 came about 10 years later in 1959 (ref. 60). Of great interest here is the implication that smooth recompression can in some circumstances also play a major role at speeds well below Mach 1 in the achievement of improved supercritical airfoils, accomplishing the benefit suggested by von Karman in 1941 (ref. 61).

One of Busemann's first projects after his arrival was to summarize the theoretical possibilities for treating transonic flows, starting at Mach 1 and extending upward in speed through the detached shock region (ref. 62). Applying these methods, Vincenti and Wagoner extended the flow field calculations for the wedge to low supersonic speeds with detached bow waves, showing that the transition to pure supersonic flow with attached shock was a stable, orderly process (ref. 63). These results tended to support the conclusion we had already come to from the experimental work, that there was little need for systematic experimental airfoil research in the supersonic part of the transonic region. We believed that such wing investigations as would routinely be made in the course of configuration development in the slotted tunnels and supersonic tunnels would be sufficient, and later experience proved this assessment correct.

The development of airfoils with improved supercritical characteristics was a major thrust of the 1945–1955 decade. Nearly everyone working in this field naturally thought of the possibilities of achieving a "delayed compressibility burble." Stack in 1938 (ref. 28) and von Karman in 1941 (ref. 61) specifically discussed this possibility. The term "supercritical" in its broadest sense means any speed beyond the critical Mach number, but as used by most of us in that period it meant speeds greater than the force-break speeds and extending upward into the sonic or low supersonic region. In recent years Whitcomb has introduced a more restrictive meaning: his "supercritical" airfoil is designed to delay the drag rise and thus the term refers to airfoil operation in the speed region between critical Mach number and drag-rise Mach number.

In a sense, the "dive-recovery" flaps developed for the P-38 were the first attempt to obtain an airfoil with improved supercritical performance (ref. 20). Throughout the forties, the tendency of diving aircraft to lock into a severe nose-heavy condition from which recovery was often difficult remained the principal problem for supercritical research. The buffeting which accompanied the lift loss in shock-stalled flows was a parallel concern. It had become generally accepted by the mid-forties that high critical Mach number was no index of good supercritical performance. There is little correlation between critical Mach number and force-break Mach number for a wide variety of sections. It was generally agreed that new criteria would have to be found for the design of airfoils with good supercritical performance. H. J. Allen came up with a fresh idea for minimizing the lift loss and moment changes at shock stall, which was tested with some success (ref. 64). He reasoned that if both upper and lower surface flows reached local sonic velocity at the same flight speed, a more equal separation would occur on each surface, leaving the net lift relatively unchanged. He and D. Graham developed an airfoil having an "M-shaped" camber line which achieved a reasonable approximation of this type of flow. Unfortunately, it had high subcritical drag and was never used as far as I have been able to learn. Nevertheless, it was the first attempt to tailor a specific fixed-geometry airfoil for alleviation of the shock-stall lift loss; no mention was made of improvement in supercritical l/d.

In the early forties when the P-38 was in trouble, I recall a conversation with Allen and Stack in which we agreed that conventional cambered airfoils showed improved supercritical lift and moment performance if operated inverted in the negative-lift attitude (i.e., with negative camber in the positive lifting sense). Negative camber meant a less curved upper surface which had reduced separation losses at shock stall. Allen dismissed this approach as being rather unthinkable and remarked facetiously that pilots would hardly accept inverted flight as a technique for pulling out of supercritical dives. None of us gave much thought to the supercritical lift-drag ratio at that time; I was certainly unaware that negative camber in addition to the lift-loss benefit resulted in better supercritical l/d until I noticed that this was so in editing Ferri's airfoil report in 1945. I looked back at our own data and some 1945 Ames data (ref. 65) obtained in systematic airfoil tests in their 1 x 3.5-foot tunnel at speeds up to about Mach 0.85, and noted with interest that the supercritical l/d was significantly better for negative camber. Figure 8 taken from the Ames data shows this result.

My past upbringing to the effect that positive camber was inherently beneficial and essential to conventional lifting airfoils at normal speeds was so deeply ingrained that I dismissed these results as an impractical aerodynamic curiosity. Three years later, in 1948, I included in my summary NACA Conference paper on high-speed airfoils (ref. 52) a plot based on the German airfoil data (ref. 66) which showed in detail how the camber for best l/d quickly diminished to negative values as the Mach number advanced beyond about 0.75 (fig. 9). Actually the data clearly showed that *negative* camber (dashed lines on fig. 9) gave best l/d at the higher speeds. But still believing that negative camber was unthinkable for practical applications, I terminated the plots at zero camber and suggested as a major conclusion that zero camber (symmetrical) airfoils were the best compromise for transonic applications (ref. 52). This interpretation was shared by the other airfoil

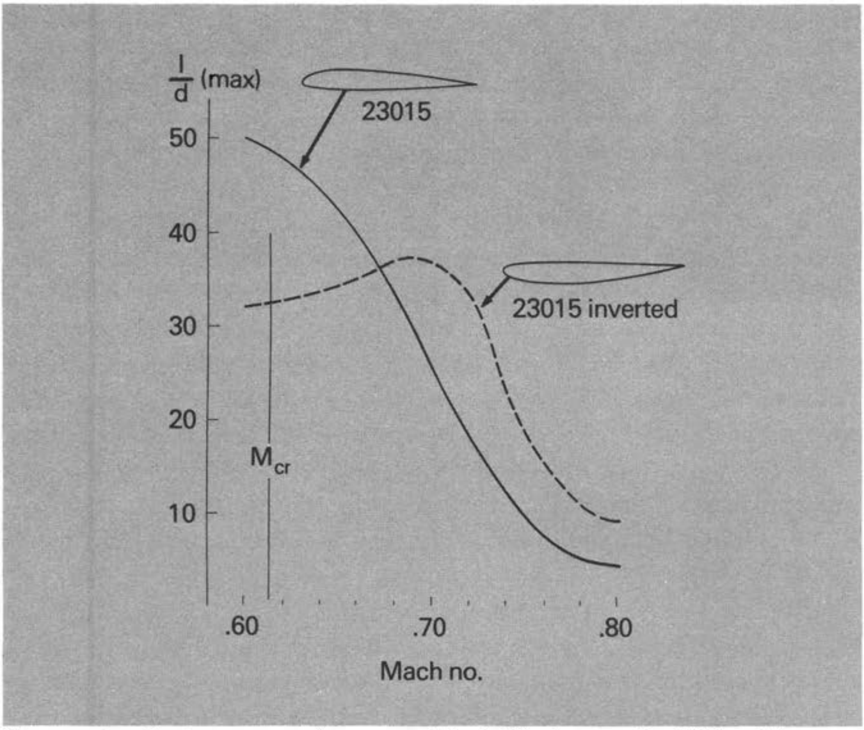

FIGURE 8.—*Improvement in supercritical lift-drag ratio (l/d) with negative camber. Ames data, 1945.*

specialists at Langley at that time and also by Allen and Graham (ref. 64).

The physical mechanism by which the improvements due to negative camber came about was thought at that time to be related to the location of the peak suction pressure and shock near the leading edge for the "peaky" distribution of pressure that occurred over the relatively flat upper surface with negative camber. For this forward shock position, the boundary layer was thin and not as prone to separation as it was for the positively-cambered case where the shock occurred far aft on the curved afterbody where it triggered separation. We did not realize then that an additional mechanism was at work for the "peaky" case, namely some degree of shockless recompression due to reflections from the sonic line. ("Peaky" is a term coined a decade later by Pearcey, who called attention to the recompression effect (ref. 67).)

The first attempt to derive "supercritical" airfoils in the restricted Whitcomb sense was made in 1951 by Woersching (ref. 68). He had studied all of the available negative camber data including the negative lift operation of the Allen/Graham "M" cambered NACA airfoil 847B–110 (ref. 64). He noticed that this airfoil in the inverted or negative lift attitude had a drag-rise Mach number of 0.81 at $c_l = .42$, while in the normal attitude the drag rise occurred at $M = 0.73$. He also examined the inverted airfoil in the region of shock stall and beyond and found it to be generally as good as or superior to the normal attitude. After further study, Woersching concluded, "Maximum drag rise Mach number is obtained with negative camber over the forward chordwise portion of the airfoil, and positive camber aft to the trailing edge—but at the expense of large negative moment coefficients." This, of course, is a qualitative description of the features of the Whitcomb "supercritical airfoil"—together with one of its special problems. Woersching goes on to advocate inclusion of the last arm of the "W" camber in order to relieve the pitching moment problem at some loss in drag-rise Mach number. He also visualized aircraft incorporating both sweep and the proposed sections, designed "for cruise near Mach 1." This work was undoubtedly the first serious attempt at delaying the drag rise—with a profile that would qualify as a "supercritical airfoil" in the present-day sense. By way of explanation of the action of negative camber Woersching pointed out that it results in a degree of flatness of the suction surface comparable to that of a much thinner symmetrical section. Unfortunately, he did not have the resources to continue development.

Probably inspired by the Woersching paper, Britisher W. F. Hilton published in 1953 a report (ref. 69) which he had written in 1947 on the advantages of negative camber. The original report had apparently been given only restricted circulation in Great Britain, perhaps for reasons of security. It is interesting to note that Hilton had been employed in the United States for several years following the war and had access to and personal interest in the available American, Italian, and German airfoil data. Hilton did not recommend any particular

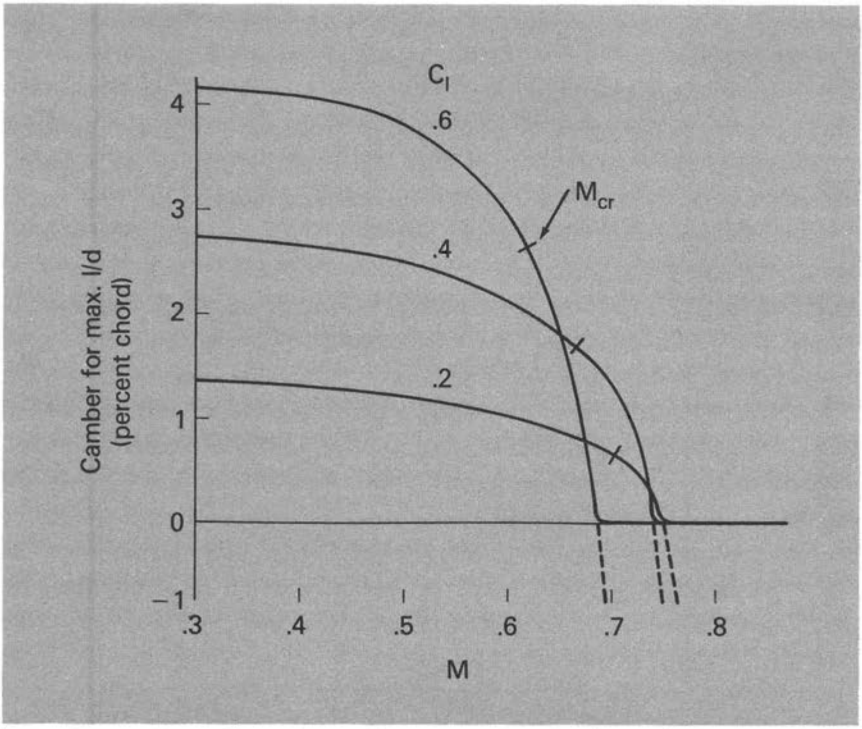

FIGURE 9.—*Effect of Mach number on camber for best lift-drag ratio. 12-percent-thick airfoils.*

distribution of negative camber. His aim was primarily to reduce the adverse lift and moment changes due to shock stall and secondarily to improve l/d beyond shock stall.

Without doubt the period from 1945 to 1951 was one of the most productive eras in the history of high-speed airfoil research. Several new transonic ground facilities and flight techniques were developed and applied successfully; reliable wind tunnel data at Mach numbers up to 1.0 were obtained, including airfoil flow photographs; new theoretical treatments of the flow were accomplished for wedge airfoils at Mach 1 and throughout the detached shock range; criteria were established for airfoils having delayed drag rise and an inverted NACA airfoil meeting these criteria was specified (the first "supercritical" airfoil in today's parlance). NACA program activities were at the core of this progress, although there were also important outside contributions, especially on the theoretical side. On June 2, 1950, I reviewed this satisfying progress in considerable detail for the NACA Executive Committee, concluding, "The principal details of two-dimensional transonic flow are now known as a consequence of recent progress both experimental and theoretical. . . . Many problems remain for the three-dimensional case of complete wings. . . . Our 8-foot high-speed tunnel with its new slotted throat provides a transonic facility of adequate size for . . . the needed work on complete wings."

John Stack's role in the high-speed airfoil developments of this period was quite different than his intimate personal participation during the first dozen years of his career. From about the time of his Wright Brothers lecture it had seemed likely that he would be moved into a management position in the Langley "front office." His special talents as a tough, persuasive technical salesman were badly needed and, furthermore, it was obvious that life would be much more pleasant for Langley management with Stack as a member of their office rather than as a combative division chief who increasingly was cast in an adversary relationship to higher management in regard to approvals and funding allocations for our projects. Thus in mid-1947 Stack became an assistant to Chief of Research F. L. Thompson, and I succeeded him as Chief of the Compressibility Division.

Although he remained invariably supportive of our projects, my relationship with Stack was inevitably changed. He was now one of "them" rather than a close colleague in research. His principal preoccupation became the promotion and development of major new transonic and supersonic tunnels, and he also became involved with other problems beyond our field of interest. He observed the airfoil developments with interest as they unfolded but had no direct part in them except through related facility developments—such as the Annular Transonic Tunnel, which might never have been successfully promoted without Stack's support. His early experiences with the open-throat tunnels made him rather suspicious of semi-open tunnels and this was reflected in his encouragement of studies of their transient disturbances by Lindsey and Bates (ref. 54). In contrast to Stack's many publications in his earlier airfoil research period, the paper covering his review of facility developments (ref. 54) was his principal publication in the 1945–1951 period.

In the final period of the NACA program from 1951 through 1956 a rapid dwindling of the effort took place. This was due partly to the large shift in research emphasis to swept and low-aspect-ratio wings for supersonic aircraft, and partly to the fact that a substantial high-speed airfoil technology base had been established. The demand for two-dimensional airfoil research diminished to low levels except for the special area of helicopter blading. Ames experimental work in the field had been terminated in 1951 when their 1 x 3.5-foot tunnel was phased out as a closed-throat facility. Finally, the abolishment of W. F. Lindsey's section at Langley in 1956 brought to a close the NACA high-speed airfoil program which had started 29 years before. Although several worthwhile projects were left unfinished, they could not compete in priority with the demands of supersonic aircraft and the burgeoning space program.

COMMENTARY

Curiously, the impressive progress in high-speed airfoil technology in the last decade of the program is often overlooked. At a recent NASA airfoil conference (ref. 70) several practitioners in the current program seemed to believe that the NACA program had terminated with the 16-series sections and Stack's Wright Brothers Lecture of 1944 which, so to speak, left high-speed airfoils in the depths of the shock stall. The most likely explanation is that the researchers of 1945–1956 did not produce any specific new airfoil families. They produced important new understanding of transonic flows and they extended the accurate data for existing airfoil families to Mach 1, but unfortunately, perhaps, there were no associated clever baptisms or new acronyms to help publicize the progress that was made. This solid but unspectacular airfoil progress was overshadowed by the more dramatic events of that period—the first supersonic flight, the slotted transonic tunnel, and the area rule.

The high-speed airfoil program provides an excellent example of NACA accomplishing its mission in an important problem area of aeronautics. For the first 20 years, from the early twenties to the early forties when the propeller was the primary application, the program provided both fundamental understanding of the flow phenomena and new airfoils to improve propeller performance at high speeds. These solutions were in hand well before they were needed by industry. Only for the brief period from about 1944 to 1947 was the program deficient in meeting the new needs for transonic data beyond Mach 0.85. During this interim, foreign data plus information from the "wing-flow" and "body-drop" techniques were used effectively. Early in 1947 the first airfoil/pressure data at Mach 1 were obtained (ref. 49) and by 1949 an effective semi-open wind tunnel was being used routinely for airfoil testing to Mach 1, the technique verified by X–1 flight data.

The program wound down rapidly in the mid-fifties, partly because there was no obvious need then to expand the technology beyond its already substantial proportions, but mainly because several of its talented researchers had been lured into more urgent and fascinating supersonic and space-related projects. Almost a decade would pass before the renaissance described in the next section would take place, based on the recurrence of an old need, but carried forward with fresh inspiration by a wholly new research team.

The report editing procedure mentioned in this chapter and elsewhere deserves comment. The primary technical editing was accomplished by an inter-divisional committee of the author's peers. This was followed by editing for grammar, availability of references, etc., by a female "English critic" in the editorial office. The generally superior reliability, clarity, and freedom from "governmentese" of the NACA reports produced by this system have been widely acclaimed. Unfortunately, however, most of them are rather dull from a literary point of view. The report-writing manual used to indoctrinate young NACA engineers emphasized accuracy, clarity, and adherence to the standard format, rather than any matters of style or technique to make the report interesting. Language which added humor or sparkle was frowned on and almost always deleted. Imaginative speculation was forbidden unless specifically identified as such. All of this was perhaps appropriate for simple reports intended to present reliable data in a readily usable form. But by the time NACA writers had progressed to more sophisticated subjects such as advanced concepts or state-of-the-art papers for a national audience, most of us were so crippled by habitual adherence to the system that these writings also tended to be stereotyped and less interesting than they might have been.

"SUPERCRITICAL" AIRFOILS (1957–1978)

In the late fifties, the first American jet transports became operational and the first concepts for supersonic transports began to appear. Langley aerodynamics researchers tended to regard the subsonic jet transport as a perfected accomplishment and devoted themselves to the problems of the supersonic concepts. Stack, and his successor after 1961, L. K. Loftin, Jr., strongly supported work on the first Mach 3 supersonic transport (SST) designs which emerged during this period. The British and French also became deeply involved at this time in the developments which led to the *Concorde*. Unlike NACA and NASA, however, they maintained a continuing program of high-speed airfoil research applicable to subsonic swept-wing aircraft (ref. 71). The cruising speeds of the more advanced subsonic jet transports were limited by the drag rise of the wing which started to occur in the vicinity of the critical Mach number, and Pearcey of the National Physical Laboratory undertook a study aimed at improving supercritical drag characteristics. He showed in 1962 (ref. 67) that the conditions for shock-free recompression previously suggested by Sinnott and others could be realized in airfoils whose curvature decreased abruptly downstream from the leading edge. For these airfoils a limited region of smooth isentropic recompression existed ahead of the terminal shock at supercritical speeds. Thus the shock which eventually occurred was weaker and the shock-induced drag rise was delayed by perhaps 0.03 in Mach number as compared to cases where there was no recompression. It was also apparent from experimental experience that this effect is present naturally to some degree for the thinner sections previously tested in which the critical Mach number could frequently be exceeded by as much as 0.2 without shock stall. Derivatives of the Pearcey sections were used on such second-generation jet aircraft as the 747, DC–10, and the A–300. Noting Pearcey's work, G. S. Schairer of Boeing suggested in his 1964 Wright Brothers Lecture that additional research to evolve optimum airfoil shapes "when shocks are present would be timely."

One of the principal Langley investigators caught up in the SST program was Richard T. Whitcomb. He had evolved a configuration which enjoyed a higher lift-drag ratio than other competing Langley concepts. But when industry evaluations of these designs became available in mid-1963 it was evident that Whitcomb's design had the highest structural weight and poorer range performance than the others. All the designs had such high fuel and operating costs relative to subsonic transports that Whitcomb became quite disillusioned and rather dramatically declared that he was quitting the SST program.

For some months he cast about for a new challenge (ref. 72). Quite by accident he was asked by Loftin, his boss in the Langley front office, to comment on some high-speed model test data for a vertical takeoff (VTO) design under study by the Ling-Temco-Vought Company. The design incorporated an upper surface blowing slot supplied with engine air as a part of its VTO system. When the slot was operating it appeared to produce a substantial increase in the force break Mach number. Whitcomb reasoned the slot blowing effect was delaying shock-induced separation and he began to wonder if this mechanism might not be a way to increase the cruise speed of subsonic transports which in some cases were limited by the drag-rise Mach number. He became sufficiently interested to start experimenting, although there had been little pressure for work in this area.

The first tests were made on a conventional NACA 6-series section with a self-actuated slot in which air flowed from the high-pressure region under the wing. (Power blowing was ruled out from the start as being too costly in weight and complexity.) Whitcomb used Lachman's book for guidance in design of this slotted model (ref. 73). The slot action did delay and reduce the shock-induced separation losses. But in so doing the normal shock moved further aft and became so strong that the direct shock losses nullified much of the gain due to reduced separation. Thus the next step was to try to modify the upper surface shape so as to weaken the shock.

Whitcomb at this time was aware in a general way of the previous work of Lindsey's group which had culminated with Daley's systematic studies of 6-series airfoils in the 4 x 9-inch tunnel extending to Mach 1 (ref. 55). He knew of the advantages of reduced camber for supercritical operation, but he was not aware of the special airfoil developed by Allen and recommended by Woersching to be used in the inverted attitude. He had, however, recently read Pearcey's paper (ref. 67) which utilized a flattening of the forward region of the upper surface. Whitcomb had, of course, realized for many years that reducing the curvature or flattening the upper surface would generally reduce the local Mach numbers and reduce the shock strength as he desired (see p. 39ff. and ref. 51). He therefore drafted a slotted airfoil with a flattened upper surface ahead of the slot, which naturally resulted in large negative camber. A large portion of the lift then had to be carried by a short, positively cambered portion aft of the slot. Tests of this arbitrary design showed a substantial increase in drag-rise Mach number (ref. 74). It was found a short time later that the slot could be eliminated for only a small penalty in the onset of drag rise and with considerable simplification in structure and ease of application in three-dimensional wings.

Continued development of these sections has taken place over the past decade. Flight demonstrations on the Navy's F–8 and T–2C airplanes and on an Air Force F–111 have verified the wind tunnel results (ref. 74).

In the course of developing the wings for these flight programs, it was learned that the supercritical airfoils had excellent high lift characteristics because of their large leading-edge radii. This important benefit tended to offset the fact that their subcritical profile drag is higher than for comparable 6-series sections.

Whitcomb's initial development of these supercritical sections was entirely experimental. By an Edisonian process of intelligent guesswork, intuitive reasoning, and cut-and-try testing—with the wind tunnel used in effect as a computer—successful profiles were achieved.

After the first work began to produce impressive results, Loftin suggested in 1965 that a simple baptism similar in character to the area rule be found. Whitcomb proposed "supercritical," a more fortunate choice than the "peaky" appellation used for Pearcey's airfoils.

Loftin also instigated in 1969 the first program to apply transonic theory to the supercritical airfoil problem, realizing that the Edisonian approach was hardly practical for producing the many different airfoils that would be needed to supply the increasing demands of designers for a variety of applications (ref. 75). Paul Garabedian of New York University was chosen to try to develop a practical theoretical program that could be used with large modern computers as a routine airfoil design tool. He describes the work of Murman and Cole (ref. 76) as the "breakthrough" which underlies the recent achievements of the transonic theory (ref. 70). The first theoretical results for Whitcomb's section did not account for the boundary layer displacement thickness and showed poor agreement with regard to shock location. Good agreement was obtained when the boundary layer was included (refs. 70, 74). The theory is now used routinely as a major tool in the program, saving an enormous amount of wind tunnel testing.

An important new development has also been contributed by the theoretical program: upper-surface shapes were found by Garabedian and Korn (ref. 77) which produced shockless supercritical flow for limited ranges of speed and angle of attack. The basic mechanism involved is the previously mentioned reflection of expansion waves back from the curved sonic line as compressions. The upper surface shapes which accomplish this recompression without a terminal shock are remarkably similar to those of some of the Whitcomb sections. In fact, Whitcomb had noticed a drag reduction in certain tests of his sections which he attributed to the existence of local conditions approximating the requirements for shockless or near-shockless flow.

In von Karman's summary of compressibility effects in 1941 (ref. 61) he included a brief but significant review of the theoretical possibilities of exceeding the critical Mach number without the occurrence of shock. He cited the work of Taylor, Gortler, and Tollmein which suggested that local velocities as high as 1.6 times the speed of sound could be achieved with smooth shockless recompression, and concluded, "The mere fact that air passes over the wing with supersonic velocity does not necessarily involve energy losses by shock waves . . . or the compressibility burble," and "careful theoretical and experimental research might be able to push the velocity of [efficient] flying closer to the velocity of sound than is possible now." Coming as they did at the threshold of the war, these wise words were lost, and a quarter century would pass before the theoretical supercritical airfoil program of the seventies would prove them correct. The general attitude of most airfoil researchers of the forties was that shockless flows were a curiosity of the theoretician not likely to exist in real viscous flows.

The extent to which the shockless designs will further improve supercritical airfoils is not clear at the early stage they are in at this writing. They were clearly of special interest at the recent airfoil conference (ref. 70), but it was too soon to expect a definitive perspective of their true potential. The Whitcomb airfoils have only weak shocks and thus complete elimination of the shocks would not be expected to make large improvements.

COMMENTARY

It is quite interesting that over the entire NACA history no attempt to develop superior high-speed airfoils by the Edisonian technique was ever made. A great deal of valuable experimentation was done to learn what was happening on particular airfoil shapes, and systematic testing of families such as the 16-series and 6-series was carried out from which the most effective members of these established families could be identified. But Whitcomb was the first to embark on a zealous crusade to develop an improved airfoil by intelligent cut-and-try procedures. This situation is even harder to explain when one notes that the Edisonian technique was often employed in other NACA programs—the cowling programs, for example. On problems of great complexity such as the supercritical airfoil this least sophisticated of all research techniques is likely to prove ineffective—unless the practitioner has truly unusual insights and intuitions.

Whitcomb's first successful supercritical sections contained the same type of camber distribution (negative camber over most of the forward portion followed by positive camber) recommended in 1951 by Woersching for maximum delay of the drag rise. Whitcomb, however, employed much more drastic profile changes leading to a radical new section. Woersching's airfoils (of which Whitcomb was not initially aware) looked more like slightly modified conventional sections.

In spite of its doubtful credibility in the mid-sixties, the transonic theory in combination with the modern computer was actually on the verge of achieving shockless supercritical airfoils. Undoubtedly the focus and stimulation provided by the Whitcomb developments hastened the derivation of the shockless airfoils, which are very similar in their upper surface configurations to Whitcomb's designs. However, this achievement would certainly have come along eventually without the prior Whitcomb developments. Thus, in the long-term perspective, the Whitcomb contribution by the sheer accident of coming when it did, produced the supercritical airfoil perhaps some 10–20 years sooner than it might otherwise have emerged from the theoretical approach.

A final point of considerable interest centers on the fact that several important applications have appeared where the supercritical airfoil principle is used to achieve thicker wings rather than higher drag-rise Mach numbers, the thicker wings being lighter structurally, thus providing larger payload fractions and improved economics. Alternatively, thicker wings permit the use of higher aspect ratio with associated performance and economic benefits. The interesting fact here is that the existence of the new airfoils illuminated important needs and applications which were not clearly seen in the beginning of the development. This underscores once again the old, but often still not accepted axiom that it is impossible in advance to identify all the real applications and justifications for a research undertaking.

Whitcomb's work has sparked a lively renaissance of high-speed airfoil R&D in which the new theoretical approaches, used in combination with experiments, are providing a degree of technical elegance that was lacking in the prior NACA programs. There can be little doubt that high-speed airfoil technology is now approaching its ultimate levels of sophistication and performance.

CHAPTER III

Transonic Wind Tunnel Development (1940-1950)

In 1940 the so-called "Transonic Barrier" was perceived primarily as a set of adverse and uncertain aerodynamic effects including an order-of-magnitude increase in drag coefficient, severe and perhaps catastrophic buffeting, and abrupt changes in the stability and control characteristics of the airplane. There was no realistic possibility that the piston engine-propeller system could ever be developed to produce the enormous powers required for transonic flight, and in reality this was a more substantial component of the "barrier" than the unknown aerodynamics. The third major element of the problem was the failure of the classical tools of aircraft development to function at transonic speeds; conventional wind tunnels appeared to be useless in the Mach number range from about 0.8 to 1.2, and flight testing of military aircraft beyond about 350 mph could be accomplished only by dives, which were extremely hazardous and in any case could not exceed about Mach 0.8, the terminal speed in vertical dives for typical 1940 drag-weight ratios.

We have already mentioned the advent of the jet engines pioneered by the British and Germans, which eliminated the propulsion barrier. The remaining aerodynamic and facility "barriers" were dispelled by NACA programs of the forties in one of the most effective team efforts in the annals of aeronautics. These NACA achievements were recognized twice by aviation's highest award, the Collier Trophy. The first award, for the achievement of supersonic flight by the X-1, was presented in 1948 jointly to John Stack for the NACA contributions, to Lawrence D. Bell the manufacturer, and to Charles E. Yeager the USAF pilot. The second award, for the slotted transonic tunnel development, was presented in 1952 to John Stack and Associates.

A few weeks before the second award was presented to him by President Harry S. Truman on December 17, 1952, Stack appeared unexpectedly in my office in a state of considerable agitation. He had just received notice of the award from J. F. Victory, chairman of the committee for the Collier Trophy. Stack said he was reluctant to accept the award as the sole recipient because so many others at Langley had contributed importantly. He wondered how the others would react. I believed they would feel as I did that he richly deserved this recognition. Without his aggressive leadership and promotional efforts there would have been no large transonic tunnels at Langley at that time. But Stack was insistent that the other principals should be included and we worked up a list of some 19 names. After negotiation, the Trophy Committee agreed to make the citation read, "to John Stack and Associates," but not to name the associates as Stack had desired. To a degree, however, he had the last word by issuing a press release at the time of the award which included the names of the others and a brief indication of their contributions (ref. 78). Stack also helped organize a recognition dinner sponsored by local businessmen on January 17, 1953, at which he introduced his associates.

Although the primary concern in this chapter will be the events leading to the achievements of the transonic wind tunnels, we will also necessarily be dealing with the development of new transonic flight techniques. In a sense, the flight approaches were also transonic research "facilities." Of particular interest are a number of strong interactions between the flight and ground developments which influenced the course of events.

THE CHOKING PROBLEM

More than 100 years before wind-tunnel choking became a prime problem of aeronautics, St. Venant and Wantzel derived the compressible flow relations revealing that the velocity in the throat of a channel (or empty test section of a wind tunnel) could not be increased indefinitely but rather was limited to sonic velocity, regardless of how high the driving pressure became (ref. 79). Speeds greater than Mach 1 could only be achieved by expanding the channel area downstream of the throat to accommodate the increased volume required by the supersonic flow. The first practical applications of these fundamental channel flow relations to achieve supersonic velocities were in the converging-diverging nozzles of the deLaval steam turbines (ref. 80).

These inherent features and limitations of high-speed gas flow in channels did not in themselves mean that any particular regime of speeds was unattainable. Clearly, the speed in a conventional subsonic tunnel could be increased to Mach 1 in an empty throat section, and any desired supersonic speed could be obtained by converging-diverging nozzle shapes. The basic unknown factor in the twenties and thirties was the "choking" effect on the achievable speeds due to the presence of a test model.

It is interesting that Briggs and Dryden in their pioneering experimental work discussed in Chapter II avoided the choking problem by use of a free jet rather than a closed test section. This was done, however, not from any understanding or consideration of choking, but simply for reasons of expediency. It was necessary that their model and balance hardware be completely independent of the compressor equipment and easily inserted or removed. In their subsequent 1926 work in the 2-inch jet at Edgewood Arsenal they employed the first converging-diverging supersonic nozzle on record in American research, to obtain Mach 1.08 (fig. 10). But it is not clear that they understood the basic supersonic channel flow requirements; the reason given for the nozzle area expansion (ref. 13) was "to avoid pressure pulsations," and their nozzle area ratio corresponded to a theoretical Mach number of 1.25 instead of 1.08. (A listing of transonic test facilities capable of Mach 0.9 or higher is given in the Appendix.)

It remained for Jacobs and Stack in the NACA in-house tests of 1929 to demonstrate that, with a test model present, higher speeds could be reached with the open throat than with the closed throat. In fact, the natural expansion of the free boundary permitted sonic and low supersonic speeds in the open throat, although the flow was pulsating and nonuniform, and it was doubted that the near-sonic data were valid. Because of its potential speed advantage the open throat was modified and improved to the final configuration shown in fig. 11. In spite of its speed advantage, however, the open arrangement was abandoned, primarily because of flow asymmetry, pulsations, and large and indeterminate aerodynamic end effects where the test airfoils passed through

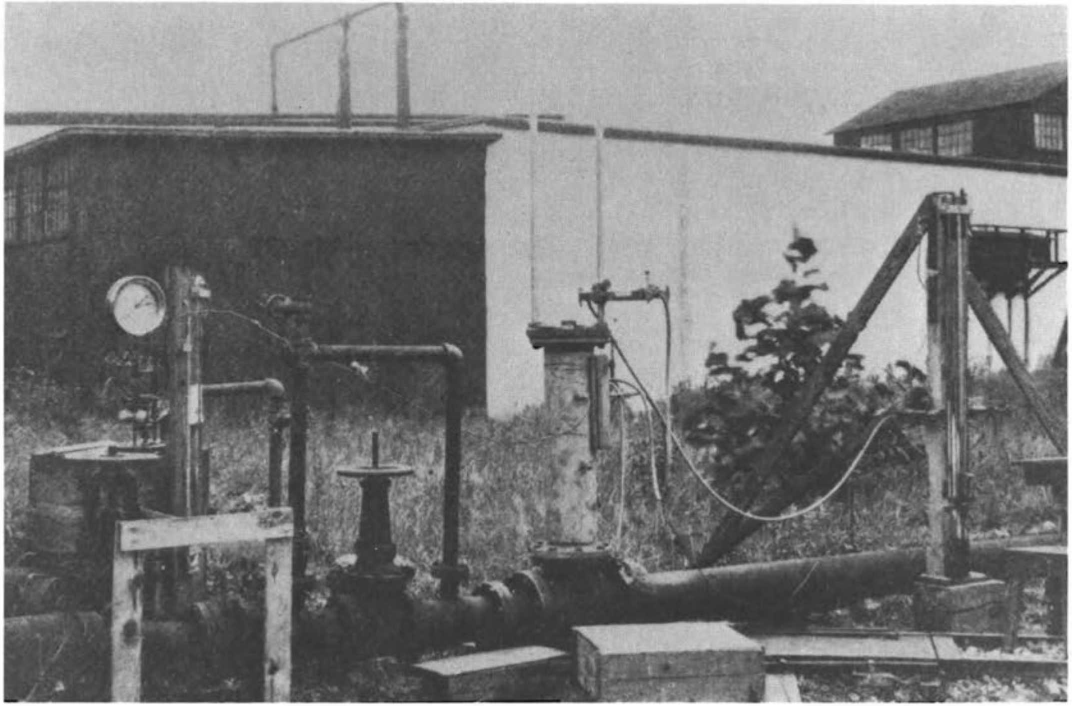

FIGURE 10.—*The first American facility capable of very high subsonic and low supersonic speeds, Mach 0.95 and 1.08. Briggs' and Dryden's 2-inch jet at the Edgewood Arsenal, 1926. Jet is at the top of the pipe in the center. Test airfoil is seen rotated out of the jet; single pressure tube from airfoil attaches to manometer at right.*

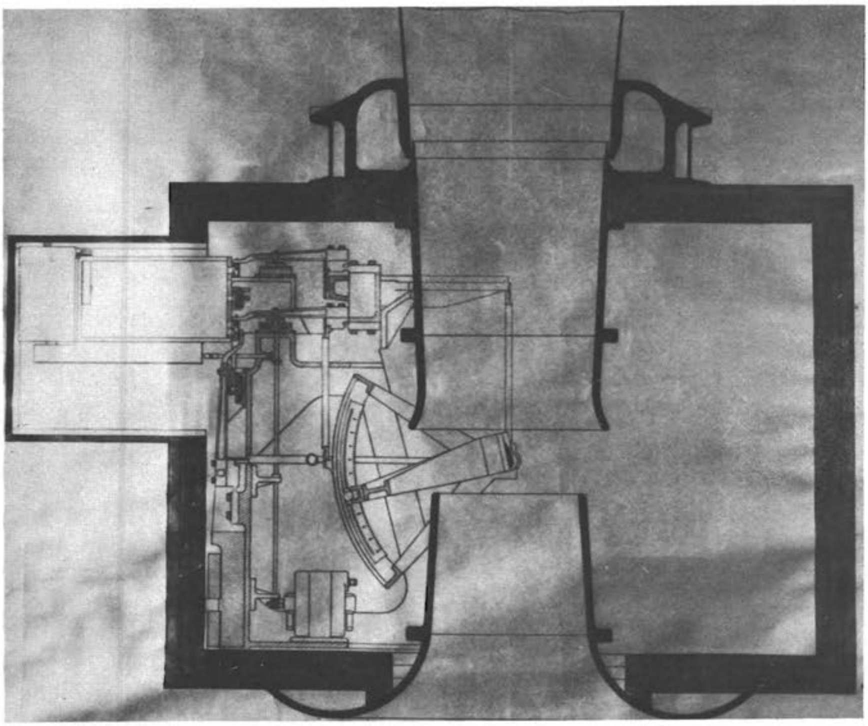

FIGURE 11.—*The Langley 11-Inch High-Speed Tunnel with the open throat developed in 1930.*

the boundaries of the airstream. Nevertheless, these investigations of the open throat were by no means wasted effort. They demonstrated an approach in which choking due to the presence of large models did not occur, and this experience more than any other single factor encouraged Stack and his cohorts 15 years later to embark on the further developments which produced the transonic slotted tunnels. Stack often referred to this early work as the genesis of transonic facility development and said it had been set aside in 1930 because there was no need for it at that time after the decision to go ahead with the closed throat (ref. 81).

The 11-inch and 24-inch high-speed tunnels had sufficient power to reach the choked condition for all types of test models, and this condition is evident in some of the published results (ref. 18) where the drag coefficient eventually rises vertically in plots against Mach number. Generally, however, such plots were arbitrarily terminated at Mach numbers .03 or so below choking because we knew that the choked data were not valid. Actually the term "choking" was seldom used then, and the phenomenon was not fully understood. Instead, we tended to think in terms of a large "constriction" or "blockage" effect by which the presence of the model increased the effective stream velocity above the values indicated by the tunnel calibration. Glauert and others had derived theoretical formulas applicable to low-speed tunnels for determining the blockage effect (ref. 82); however, the effect of compressibility was not known theoretically until the early forties (refs. 83, 84).

In the 8-foot high-speed tunnel an attempt was made in 1938 to determine the blockage corrections experimentally by comparing the pressure distributions on 0012 test airfoils of different chord with the low-speed distributions obtained from tests in the full-scale tunnel and from theory. The results were never published because of a number of uncertainties, but they were used to provide "corrected" data for some of the 8-foot tunnel investigations (ref. 85). These experimental blockage corrections tended to increase very rapidly at the higher speeds, and as choking was approached they became so large and doubtful that we arbitrarily terminated the data plots, omitting the points at the highest test speeds. The theoretical results that became available a few years later confirmed the rapid increase at the higher Mach numbers, and showed that there was no hope of "correcting" data taken in the choked condition.

In order to understand better the nature of the choking phenomena, a small water channel was set up at the 8-foot tunnel in 1940 (fig. 12). In this device the low-speed flow of a liquid such as water can be related to the high-speed compressible flow of a gas such as air. Developed carefully by W. J. Orlin, this little facility operating at about 3 feet per second, provided some interesting enlightenment on the process of choking, including flow visualization (ref. 86) which agreed well with schlieren pictures taken in air.

By this time many different models had been tested in the 11-inch and 24-inch tunnels at speeds up to choking. R. W. Byrne was assigned the task of correlating the choking data. He found that the choking

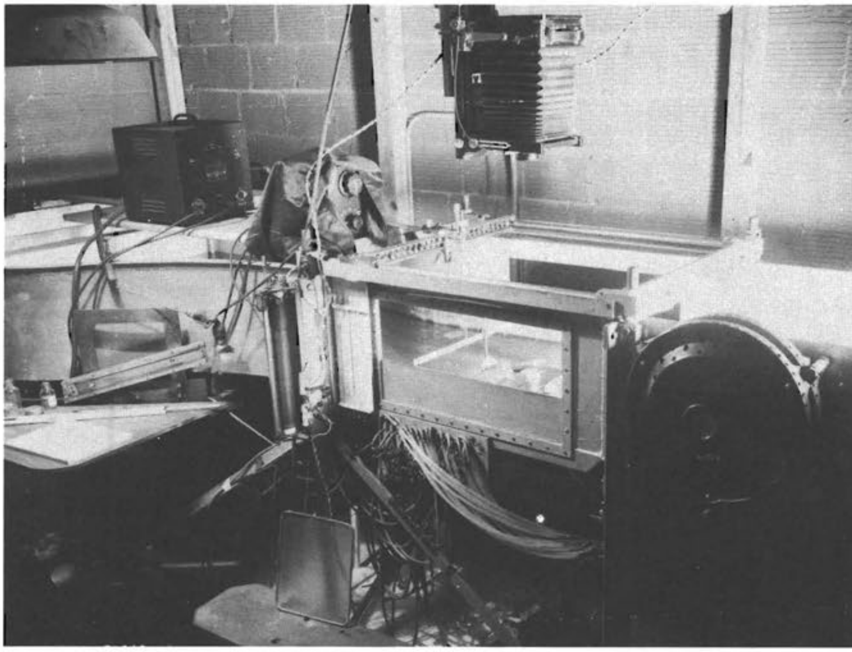

FIGURE 12.—***Water channel used by the 8-foot tunnel group to investigate tunnel choking phenomena by means of the hydraulic analogy.***

Mach number was a function primarily only of the maximum cross-sectional area of the test models; the shape of the models had only minor effects (ref. 87). Each test model in effect created a secondary throat whose area was less than that of the tunnel throat by the amount of the model's maximum cross-sectional area. Choking occurred when Mach 1 was reached in the secondary throat, and the choking Mach number in the tunnel throat could be calculated from simple one-dimensional flow relations for each size of test model. To attain a tunnel choking Mach number as high as 0.95, for example, required a test model cross-sectional area of only one-fifth of 1 percent of the tunnel throat area. This implied much smaller models than we had been using, but they were by no means out of the question for a tunnel of 8-foot throat size. For example, a typical wing of 4-inch mean chord and 36-inch span with 10-percent-thick sections, having the same Reynolds number as the airfoils used in the high-speed airfoil tunnels, would have a choking speed of about Mach 0.96 in the 8-foot tunnel. The possibilities and requirements for major reductions in the "choked-out" speed region of our high-speed wind tunnels were now accurately delineated.

Unknown to us at Langley, Allen and Vincenti at Ames had undertaken a study of compressibility corrections in high-speed tunnels (ref. 84) which included more elaborate theoretical discussion of choking than that given in Byrne's paper. The end result was identical to Byrne's, but the Ames paper contained no experimental verification of the choking relationships. A useful conclusion that could be drawn for the "small-model" situation was that correctable data could be expected up to Mach numbers just below the onset of choking—but there was no hope of correcting the data for the fully-choked condition.

THE REPOWERED 8-FOOT HIGH-SPEED TUNNEL; SMALL MODEL TECHNIQUES

The original 8000-hp drive of the 8-foot tunnel produced maximum test speeds with typical models of about Mach 0.75, and the bulk of the testing done in this tunnel was limited in speed not by choking but by lack of power. When Stack became section head in 1939 about three years after the 8-foot began operating, he almost immediately started talking about the need to increase the power. He had been accustomed to no power limitations in the 11-inch and 24-inch tunnels and tests of airfoil models in those facilities usually extended upward to the choking limit. At first we thought in terms of enough power to provide Mach 1 in the empty 8-foot tunnel plus a margin for installation of models, giving a total requirement of 12 000 hp. As time went on, however, the need for testing at low supersonic speeds became more apparent, and by 1942 when the first tentative Langley management approvals of a repowering plan were obtained, 18 000 hp had been decided on (ref. 54), a rather arbitrary increase of 50 percent over the original figure. (A later agency press release has it that this liberal level of power was in anticipation of the large requirements for a ventilated type transonic tunnel. Actually, it was based on the idea of achieving a supersonic operating capability for which the power requirements were uncertain at that time.) Little is to be found in the way of documentation relating to the promotions of the repowering up to 1942. For the most part, it was talk between Stack and his bosses, Miller and Reid. Occasionally, Stack dashed off handwritten notes to Miller which did not survive in the Langley files. There were no exhaustive reviews by any advisory groups as there would have to be today. Actually there was practically no substantive concept development or design study behind all the talk up to 1942; we never made any engineering designs of model support systems or test section modifications for supersonic testing during this period. It was simply taken for granted that all of this would be done later if plans for an ample power increase went through. The proposal went all the way to Lewis and finally gained his approval, supported mainly by informal discussions and general good intentions to work on the problems later.

The 16-foot high-speed tunnel which started operating in 1941 had been built with a 16 000-hp drive, the maximum power available at Langley at that time. More seriously underpowered than the 8-foot tunnel, it could reach a maximum speed of only about Mach 0.7 with the smallest model test setups. The principal use of this tunnel as originally conceived was to extend the kind of full-scale propeller and engine nacelle-propeller testing done in the old Propeller Research Tunnel to high speeds. After the tunnel was well along in construction, it became clear that full-scale engine nacelles would produce such enormous blockage effects that choking would occur typically at speeds as low as Mach 0.6, and that throughout the entire speed range major distortions would be present in the data. Only a few such setups were tested during the first years of operation, primarily to investigate and improve radial-engine cooling.

Early in 1943 Reid and Miller decided to create a new research division to incorporate all the ground-based high-speed aerodynamics activities including the following groups: 8-foot tunnel, 16-foot tunnel, 9-inch supersonic tunnel, and the group under A. Kantrowitz involved with fundamental gas dynamics research.

The new division was called "Compressibility Research," compressibility being a basic property of gases which becomes important in aerodynamics at high speeds. (Langley usually favored vague general organizational titles, believing they might help to discourage criticism of what was going on, and insure that researchers were not unduly hemmed in by nominal organizational boundaries.)

Stack's first problem as a new division chief in mid-1943 was to appoint a replacement for David Biermann who was vacating his position as head of the 16-foot tunnel section for industrial employment. He selected me for the job, but I was not happy about it. I was comfortable with the 8-foot group, and we were in the midst of promising plans for the repowering. By comparison, it seemed to me that the underpowered 16-foot was doomed to routine testing at subcritical speeds. Stack's answer to these misgivings was, "By God, we'll repower 16-foot!" He was elated at this first contemplation of an exciting new crusade, and I was sufficiently encouraged to move into my new assignment with some enthusiasm in July 1943. One of the first visitors to my new office was Mr. Miller. He emphasized the importance of the job and offered some typical advice, "Don't do anything without first checking with Stack or me."

In my last weeks at 8-foot, I had started work on the problem of how best to support test models in the repowered tunnel to provide testing as close as possible to Mach 1. Byrne's results (ref. 87) gave a firm indication of how small the test models would have to be, and it was obvious from the outset that conventional strut supports (fig. 13) could not be used because the struts themselves would contribute more blockage than the small test models. Upon moving over to 16-foot, I continued to study this problem as time permitted, partly because I knew that we would eventually be confronted with it when the 16-foot was repowered, but mainly because I had developed an interest in it.

For wing testing, I first considered half-span models mounted from the tunnel wall. This eliminated the struts, but the tunnel wall boundary layer, several inches thick, made the flow over the root section of the wing invalid—an especially serious deficiency for the small wings that would have to be used. Mounting the wings on a support plate which bypassed the wall boundary layer was considered next, but the asymmetry of this arrangement seemed clearly to be undesirable at near-choking speeds. And then a symmetrical solution suggested itself: locate the support plate in the center of the tunnel in the plane of

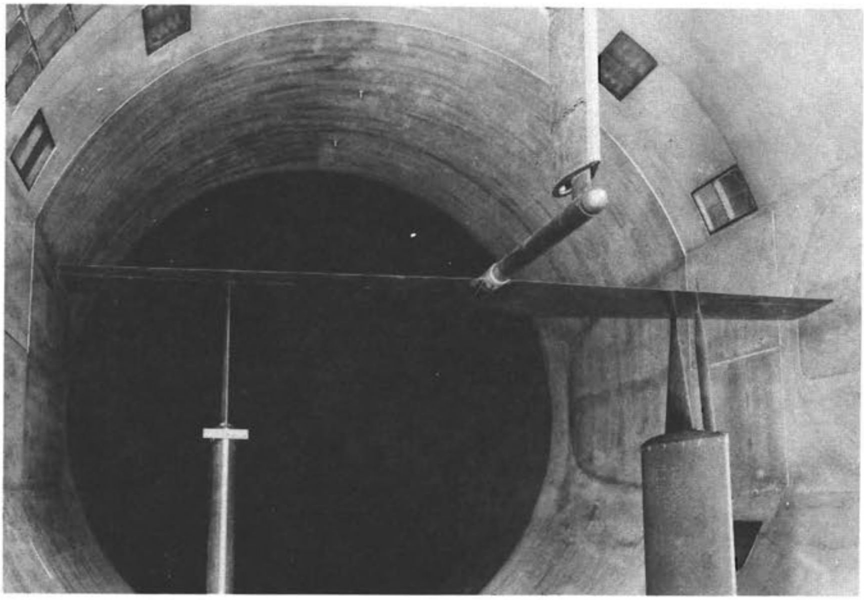

FIGURE 13.—*Typical strut support system used prior to 1944 in the 16-Foot High-Speed Tunnel. Choking speed was about Mach 0.8.*

symmetry of a complete wing model (fig. 14, top). The thickness of the plate would have no effect on choking because in effect a new strutless test section was established on each side of the plate. Being at the plane of symmetry of the wing, the plate would not affect the wing flow, and the plate boundary layer was negligibly thin.

I recall a sense of satisfaction as I described the center-plate support idea to Stack in mid-1944 during one of his frequent visits to my office in the 16-foot tunnel building. He proposed to start design work at once to implement the idea in the 8-foot tunnel, which was to shut down for repowering in a few months. Care was taken by the design group to shape the leading edge of the plate so as to avoid a local velocity peak. By the time 8-foot commenced operations with its new 18 000-hp drive in February 1945, the center plate was ready for installation. The first wings tested were part of a comprehensive general research program set up in November 1944 to support the Army Air Corps' first jet-powered high-speed bomber development. Wings of

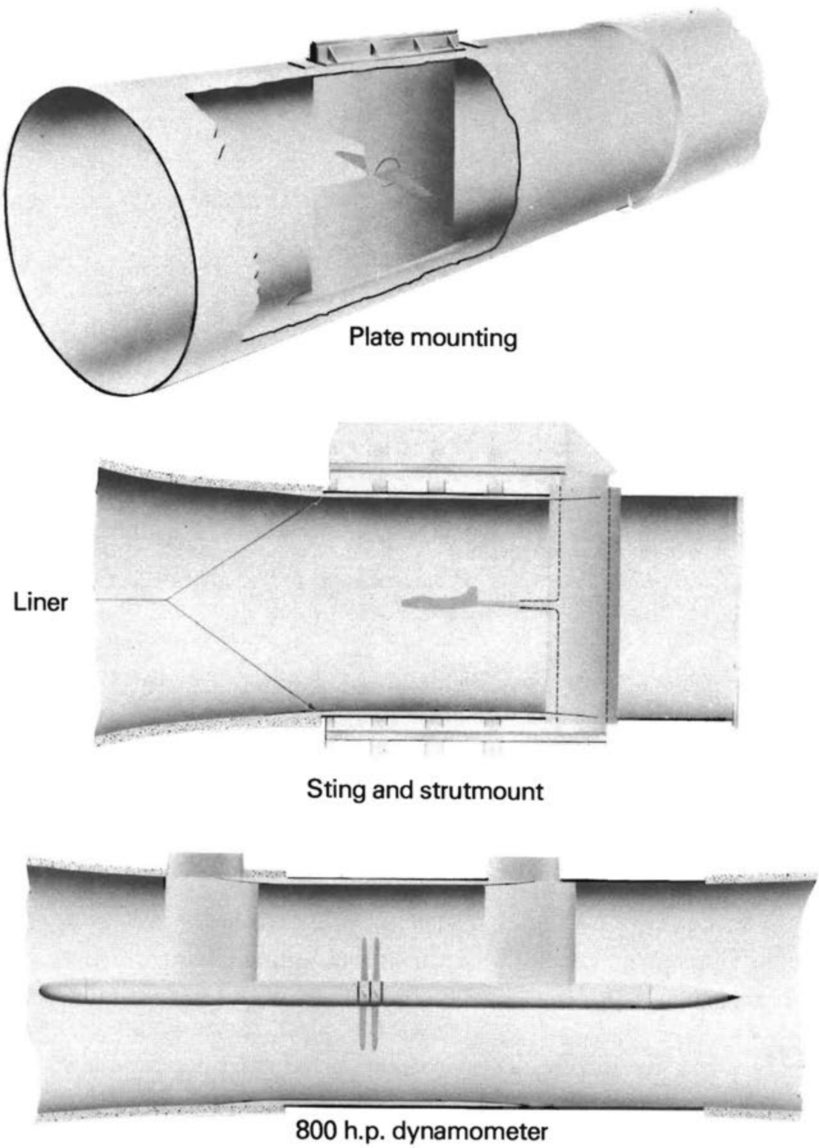

Figure 14.—*Support systems developed by Langley which do not cause a decrease in choking Mach number. From a 1946 Conference chart.*

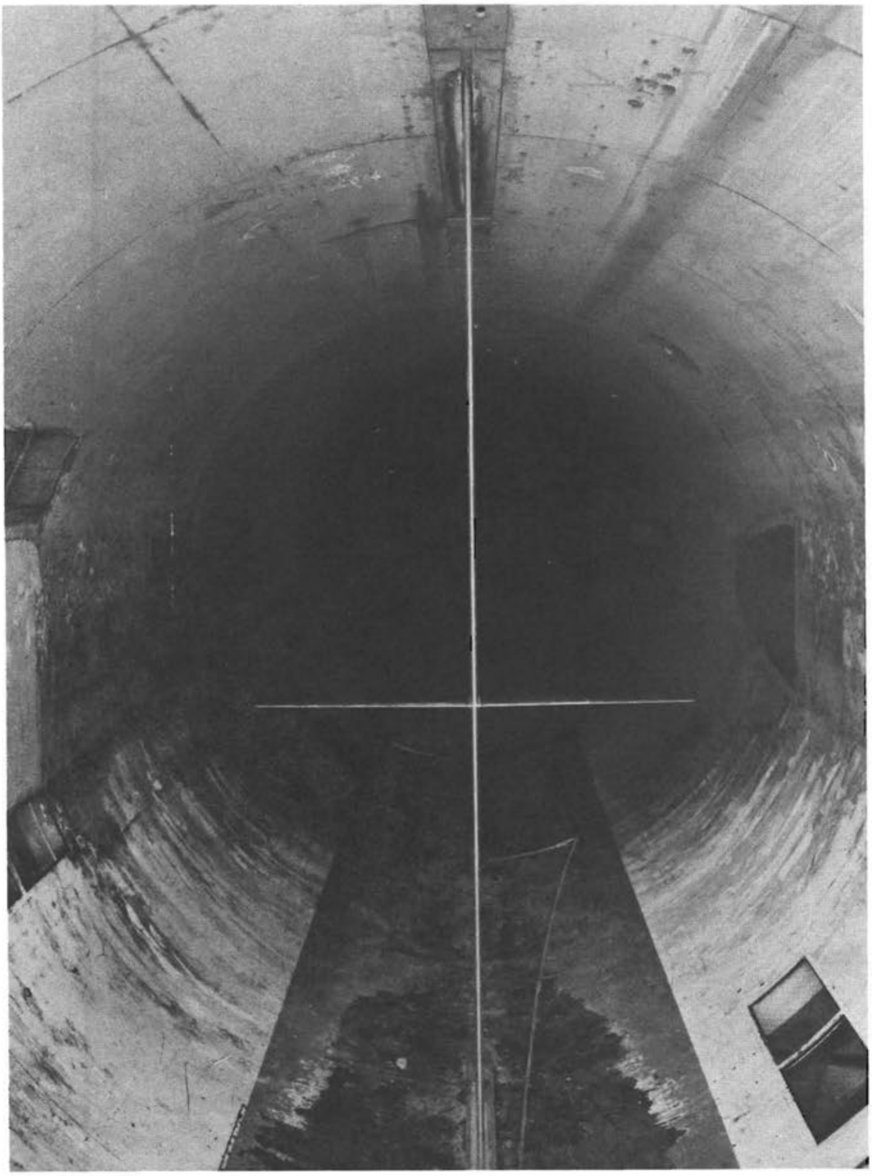

FIGURE 15.—*Center-plate support for wing testing installed in the repowered 8-Foot High-Speed Tunnel, 1945.*

6-inch root chord, 38-inch span, and 10 percent thickness ratios made the choking speed about Mach 0.95; reliable data were obtained up to about Mach 0.93 (see fig. 15). The first comprehensive pressure distributions and wake-survey drag measurements were transmitted to the Army in the summer of 1945 (refs. 88, 89) and other wing and tail configurations followed in short order (ref. 90).

I have described the center-plate development in some detail because it is a good example of innumerable creditable but unspectacular contributions made by NACA supervisors and seasoned researchers, routinely and usually anonymously. So much of this happened all the time that it would have been quite impractical for NACA authors to acknowledge all such contributed ideas in their papers. If an idea had been formalized by a memorandum to the Chief of Research, by a patent application, or by a publication, some acknowledgement would be expected; but in the absence of such documentation, the origin was likely to be quickly forgotten. In the case of the center plate, the test reports give only a description of the device, and Stack's later brief review of facilities (ref. 54) says only that it evolved from "intensive study."

As we progressed in transonic research, we learned that the prime problems lay not with isolated wings but with wing/bodies and complete configurations. The center plate was not well adapted for testing such configurations, and some type of sting support system was needed. In this case, the support-choking problem was not the sting itself but the large strut downstream of the test model which extended to the tunnel walls or to an external balance. The avoidance of strut choking in this setup had a more obvious solution: divergence of the tunnel walls to compensate for the strut area blockage. If the strut were located in the test section, this would have required a major mechanical operation on the tunnel structure, and it was easy to see that the same effect could be realized much more expediently simply by installing an insert or liner within the existing walls to create a new throat section for the test model ahead of the sting support strut (fig. 14, center). The same scheme could obviously also be used to avoid strut choking for the propeller dynamometer installations in the 8-foot and 16-foot tunnels (fig. 14, bottom). The principle of these liners seems to have evolved from informal group discussions in 1944. E. C. Draley, R. H. Wright,

E. Palazzo, and R. Moberg were among the implementers of the new support systems. Initially, the sting was attached to a balance outside the tunnel through a large strut housed within a fairing. This produced troublesome tare forces, and a much improved arrangement used small strain-gage balances contained within the test models. With this latter arrangement the support strut could be located farther downstream in the diverging diffuser section where it would not contribute to choking in the test section. Thus the effect of the liner (fig. 14, middle sketch) was achieved without any alteration to the tunnel contours.

The D–558–1 and the X–1 research airplanes were the first configurations tested with the new sting systems (fig. 16), providing extremely important data at speeds up to about Mach 0.92 prior to the first high-speed flights of these aircraft.

In December 1947, the 8-foot tunnel test section was equipped with a plaster throat insert contoured theoretically to produce uniform shockless flow at Mach 1.2 (ref. 93). The nozzle shape was perfected experimentally by tracing pressure disturbances measured near the tunnel center line back to their point of origin on the wall, and then

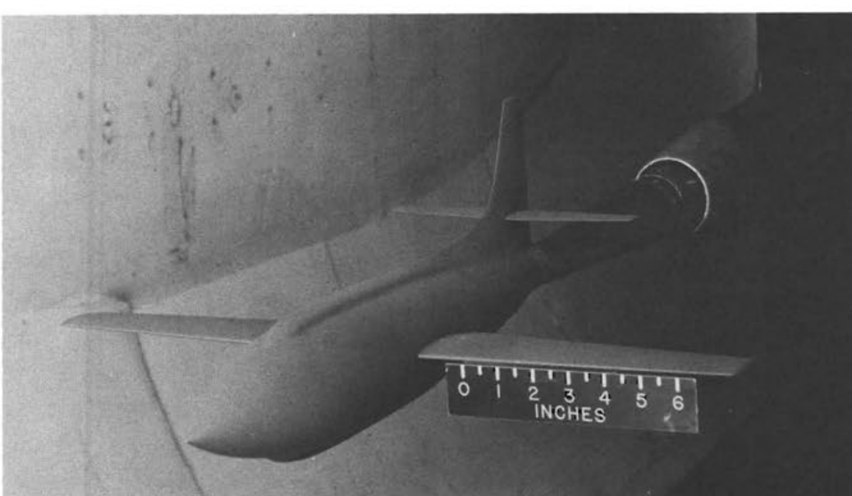

FIGURE 16.—*Sting-supported model of the X–1 in the repowered 8-Foot High-Speed Tunnel, 1946.*

making the minute changes in wall contour determined theoretically to be needed (ref. 54). Once perfected, the nozzle was used for the remaining two years of closed-throat operation, finally being removed in January 1950 to make way for the slotted test section.

It was concluded from the success of the Mach 1.2 closed-throat nozzle that the same techniques could be applied at lower supersonic speeds; Mach 1.1 was thought to be clearly feasible. The problem of reflection of the bow shock back on to the rear of the test model would probably determine the lower limit rather than any limitation of nozzle design.

Thus by seriously coming to grips with the choking problem, NACA work in the early forties reduced the unattainable speed range for closed tunnels to the narrow region between Mach 0.95 and about 1.1, approximately one-third its former proportions. The price that had to be paid for the small-model technique was, of course, a reduction in test Reynolds number. Even so, test Reynolds numbers of the order of one-fifth those of the small research airplanes could be obtained, close enough to permit very important valid comparisons.

One of our most important duties as NACA supervisors was to insure the prompt flow of the results of our research to industry and the military. NACA had learned by hard experience in the twenties that the issuance of technical documents, while of course essential, was not sufficient as the sole mode of communication. The top managers in industry and in the military seldom had time to read NACA technical reports, and—equally important from NACA's viewpoint—Congressmen had neither the time nor the qualifications to read the technical reports and judge whether the agency's output justified its appropriations. Starting in 1926, the so-called Engineering Conferences provided periodic opportunities to highlight recent research accomplishments, and at the same time to "blow the horn" for the agency in a most effective and unobnoxious way. Great care was taken to make these presentations simple enough for managers and Congressmen to understand without losing any important technical implications.

In 1946 it was especially important to reveal and advertise our progress in transonic and supersonic testing capabilities. We spent some time developing conventional charts and illustrations, but I was unhappy with the rather uninspiring results. Finally, we decided to replace the charts and pictures with live action. We built two small wind tunnels with 6-inch glass-sided throat sections revealing not only the tunnel contours but also schlieren images of the shocks formed on the test models and in the tunnel diffusers (fig. 17). Above the tunnels was a manometer board calibrated to show the velocities along the tunnel walls and over the test models. The lower tunnel was a conventional

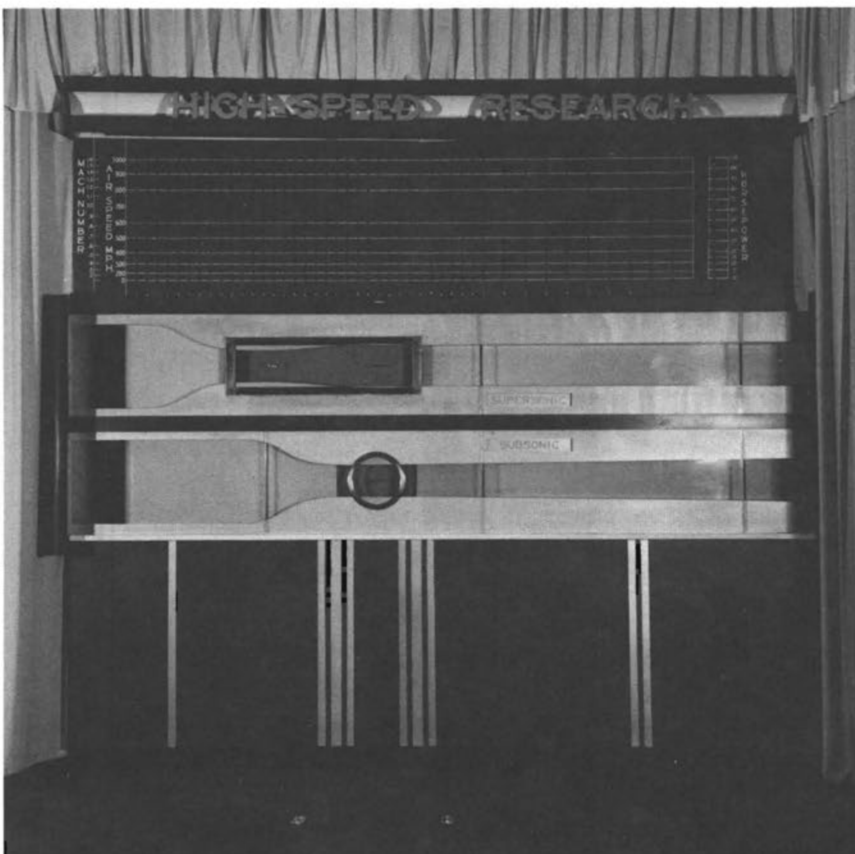

FIGURE 17.—*Small tunnels used to demonstrate choking, supersonic nozzles, power requirements, and transonic and supersonic airfoil flows, 1946.*

subsonic design which illustrated choking and the upper was a convergent-divergent supersonic tunnel illustrating the principles of NACA's three large new supersonic facilities then in design. Because of our very limited budget, we had to employ a unique drive system for the little tunnels: they were connected by a long diffuser to the low-pressure test chamber of the 16-foot high-speed tunnel. By running the 16-foot at something over 400 mph, enough suction was provided to choke the subsonic model tunnel and generate Mach 1.6 in the supersonic tunnel. Most of the visitors, hearing only a distant rumble, were not aware that the 16-foot was being used as an oversized pump to activate the little tunnels.

The first demonstrations were made in January 1946 for a conference of aviation writers, and their reactions provided ample evidence of the effectiveness of this show. After the little tunnels had served again in the 1946 Annual Inspection of the Langley Laboratory, we used the supersonic tunnel to investigate the flow phenomena and forces on a control flap at supersonic speeds (ref. 95), the first time this problem had been examined in a supersonic tunnel. Schlieren photographs taken in these demonstration tunnels also found their way into several books and periodicals.

TRANSONIC AIRFOIL FACILITIES

The Germans employed an interesting variant of the "small-model" technique to obtain two-dimensional airfoil data in their large (2.7 meter) high-speed tunnel. Test models of about 1-foot chord were mounted in the center of the tunnel between large, thin, wire-supported end plates (ref. 66). An impressive amount of systematic data was produced by this setup in 1943 and 1944 (see Chapter II).

Langley was slow to accept either the German or Italian semi-open techniques for airfoil testing. Recalling his early difficulties with the open-throat 11-inch tunnel, Stack was suspicious of the semi-open configuration and at the same time chagrined that it had not come from Langley. The best that he would say for it was, "a marked reduction in choked range" had been achieved (ref. 54).

On his arrival in 1945, Ferri had been surprised to learn that NACA had not tried to develop a semi-open tunnel. However, he had had so much difficulty with flow pulsations above Mach 0.95 that he was reluctant to recommend that we become involved with one. C. duP. Donaldson was stimulated by Ferri's work to undertake in 1945 a series of tests of a small airfoil in a 1 by 3-inch jet to evaluate constriction effects in both the closed and semi-open configuration (ref. 53). At about the same time, W. F. Lindsey wrote a memo to Stack suggesting a more thorough investigation at a more adequate scale, utilizing the 4 x 18-inch tunnel equipment. Stack rejected the proposal, telling Lindsey that Ames was planning to undertake a similar study (perhaps the work of Allen and Vincenti (ref. 84)). But about a year afterward, in 1946, Stack approved tests of a 9 x 9-inch open-throat configuration in connection with studies then in progress of various wind tunnel designs for the so-called "NACA Supersonic Center." Lindsey and Bates found

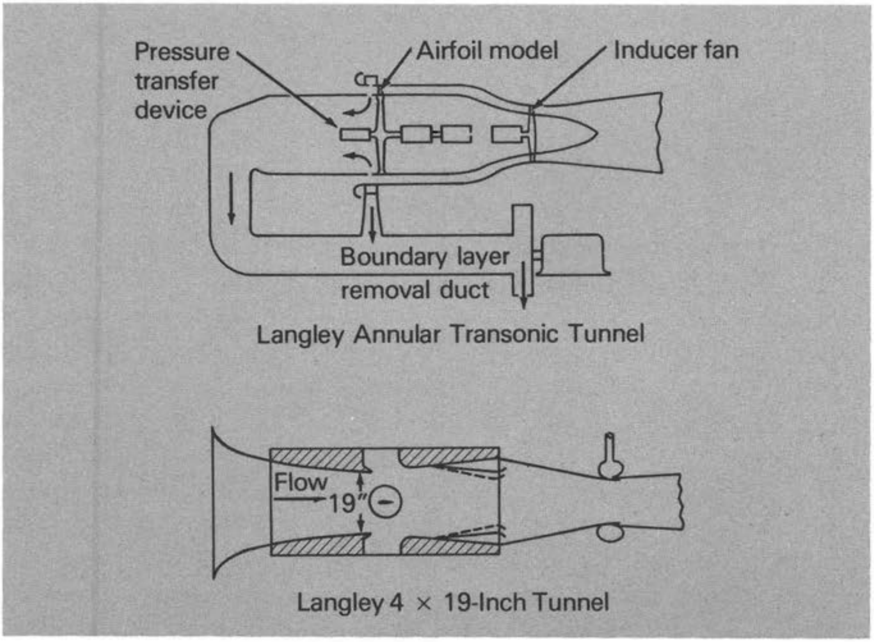

FIGURE 18.—*Schematic drawings of the Annular Transonic and the 4 x 19-Inch tunnels.*

that the flow pulsations near Mach 1 in the 9 x 9-inch throat could be reduced by improved design of the diffuser entrance, but satisfactory conditions for testing at low supersonic speeds could not be obtained (ref. 54).

A few weeks after becoming division chief in the summer of 1947, I called Lindsey and suggested that we convert the 4 x 18-inch airfoil tunnel into a semi-open facility to extend our airfoil testing to Mach 1. Recalling his earlier rejected proposal along these lines, Lindsey was naturally happy to proceed. In addition to applying the diffuser design criteria he had developed in the 9 x 9-inch throat work, Lindsey incorporated a very effective adjustable choking device located in the diffuser section to prevent downstream disturbances from affecting the test section (fig. 18). The rather small "open" sides of this 4 x 19-inch tunnel (17 percent open in contrast to Ferri's 43 percent) undoubtedly also contributed to the reduction of pulsations. Preliminary runs in 1948 revealed that testing up to Mach 1 was possible with negligible pulsations and transient disturbances (ref. 54). A comprehensive airfoil test program (reviewed in ref. 51) was initiated and the first results for Mach 1.0 (figs. 4, 5, 6, and refs. 54, 56) were published in 1949.

THE ANNULAR TRANSONIC TUNNEL

Not really a wind tunnel at all in the usual sense, the Annular Transonic Tunnel more properly falls in the "whirling-arm" category. It was also variously referred to as the "Rotating-Disc Transonic Research Equipment," "Special Transonic Research Equipment," "Annular-Throat Tunnel," and "Langley Transonic Tunnel." C. duP. Donaldson proposed the scheme in late 1944, thinking of it as a single-bladed axial fan rotating at transonic speeds. The single blade or test airfoil had very small tip clearance with the annular passage so that the flow could approach two-dimensionality. To avoid wake interference a low-speed axial flow was induced in the annular passage by a blower, and the boundary layers on both surfaces of the annulus were removed ahead of the rotor (fig. 18). In effect, the test airfoil would be flying in a channel of infinite depth and choking would not occur.

Of the several difficult problems of this scheme, the most formidable

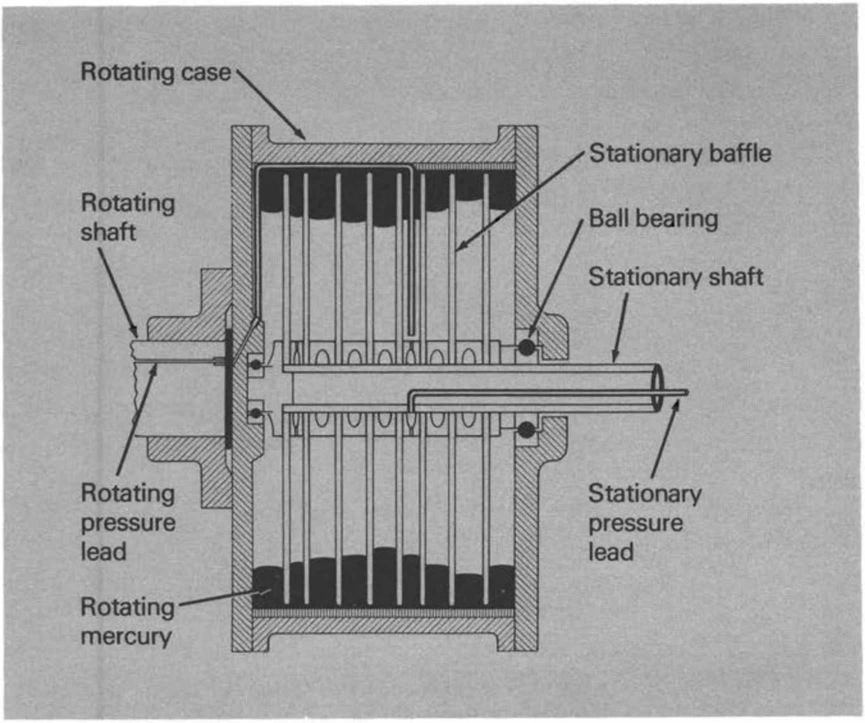

FIGURE 19.—*Section through axis of rotation of the pressure-transfer device of B. W. Corson, Jr.*

was the accurate determination of the forces on the whirling test airfoil. In all probability the annular tunnel would never have been attempted, and perhaps it would never have been conceived at all, had it not been for the invention, successful development, and prior use by our 16-foot tunnel group of a pressure-transfer device which made it possible to obtain accurate pressure distributions about the test airfoil.

The design of the multiple-pressure transfer device was proposed by Blake W. Corson, Jr., in 1943 (ref. 96) (see fig. 19). It was developed successfully by J. F. Runckel, R. S. Davey, and M. F. Miller, in consultation with Corson, substantially as proposed. It was used initially in a pressure distribution study on the rotating blades of a 42-inch diameter axial compressor (ref. 97). Davey later developed an improved transfer device using mechanical seals which was used in the annular transonic tunnel and in the high-speed propeller program (ref. 98). The availability of the original device was at the heart of the Annular Transonic Tunnel enterprise.

Because of its close relationships to our axial compressor research at the 16-foot tunnel, the Annular Transonic Tunnel concept was placed in our group for design, development, and exploitation. Much of our electrical equipment could be used directly. A suitable cinder-block building was erected on the 16-foot grounds to house the new tunnel. As one can see in the photograph (fig. 20) the annular tunnel was a substantial new facility, considerably exceeding the other small high-speed airfoil tunnels in cost. Early in 1947 the first successful runs were made (ref. 49), and the first pressure distributions ever measured on an airfoil at Mach 1 were obtained (ref. 99).

After a 5-year life, the Annular Tunnel was decommissioned in 1952. It had major limitations: only simple airfoils could be tested; the test process was cumbersome (only 5 airfoils were tested in 5 years); for structural reasons speeds above Mach 1 were never attempted; and when the X–1 data and semi-open-tunnel data became available, it was evident that the Annular Tunnel pressures were uniformly too high by a small but ever-present amount for which no explanation could ever be found (ref. 100). There was obviously no justification to continue the Annular Tunnel after the simple, more productive semi-open, and the more versatile slotted tunnels came into operation.

COMMENTARY

Public announcement of the Annular Transonic Tunnel was made at the opening exercises of the May 1949 Biennial Inspection of the Langley Laboratory. To heighten the emphasis, NACA called on John Stack to describe the accomplishment. What was actually happening here, beyond the revelation of an interesting new facility, was an unprecedented attempt by NACA to divert attention away from the slotted transonic tunnel developments. NACA subsequently admitted the dual intent of this announcement in a rather surprising statement found in the 40th Annual Report of 1954,

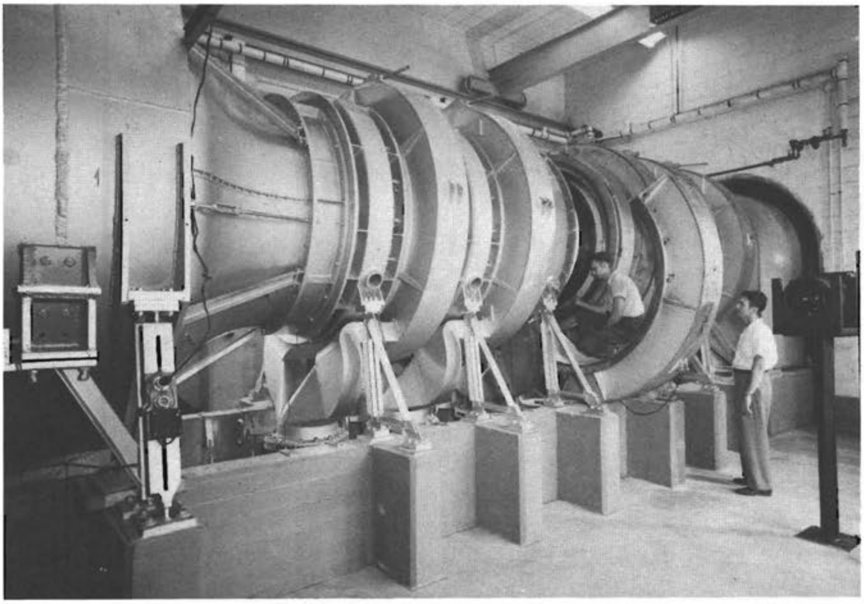

FIGURE 20.—*The Annular Transonic Tunnel, opened at the rotor section. R. Turner, left, and L. W. Habel.*

It is to be doubted whether the NACA would have given the [ATT] device the importance implied by the [1949] public announcement except that it served to explain away rumors that a successful transonic wind tunnel had been developed. Such was in fact the case. . . . At the same time that the [1949] announcement of the annular-throat wind tunnel was being made, construction was being rushed towards completion of the first of the large [slotted] transonic wind tunnels.

By the time of Stack's public announcement, it was already clear that the ATT had serious deficiencies and very limited prospects; the semi-open 4 x 19-inch facility had already proved itself and had taken up the entire burden of the transonic airfoil program.

The necessity for NACA in 1954 to reveal the dual nature of the 1949 ATT announcement is obvious. They were saying in effect, "This time we are going to tell you about the real transonic tunnel." It seems to have been taken for granted that tactics of this kind were justified in the interest of national security in the environment of the early fifties.

I doubt that the 1949 announcement did much to divert attention from the slotted tunnel; too many outsiders already knew about it.

WING-FLOW AND BUMP METHODS

The idea of using the local region of transonic and supersonic flow that develops on wings at high subsonic speeds as a medium in which useful testing of small aerodynamic models could be accomplished did not occur to high-speed wind tunnel researchers for a simple reason: their airfoil and wing models were generally so small that there was no practical possibility of such an approach. However, starting with the Brewster XF2A–2 airplane dive tests of 1940 (ref. 101) the Flight Division had seen these local transonic flow fields develop on wing sections 10 to 20 times larger than the small wind-tunnel airfoil models. Noting the absence of any constriction effects due to tunnel walls, R. R. Gilruth proposed in 1944 that these aircraft wing flow fields be utilized for transonic testing of small models (ref. 47).

The first reaction of our high-speed wind-tunnel group was quite negative. With our 15-year background of effort at generating uniform flows for valid testing we pointed to the many obvious problems of the wing-flow technique—the flow-field nonuniformities both chord-wise and normal-to-chord, the wing boundary layer, the problem of wing shock passage over the test model, interference due to clearance between model and wall, and the very low test Reynolds numbers which were well below those of our smallest wind tunnel models. Gilruth persisted, however, arguing that any transonic data would be preferable to none. When his results (ref. 47) became available, showing for the first time continuous plots of wing lift, drag, and moment data through Mach 1 and up to about 1.3 and trends which appeared to conform to expectations, we were impressed.

The rather obvious thought that the wing-flow scheme could be applied by mounting a large wing section in one of our high-speed tunnels, equally well or better than on a diving airplane, must have occurred to many in 1945. There was no immediate rush to exploit the idea, however. Many researchers, perhaps a majority, still found the scheme so fraught with problems and impurities as to be unworthy of adoption, and this view tended to prevail in our 16-foot-tunnel group. Nonetheless, early in 1946 we decided to make a quick preliminary check of what might be done by investigating a large-chord airfoil spanning the 16-foot tunnel test section. G. Heiser reported the results at the June 3, 1946, meeting of the Langley General Aerodynamics Committee. He had found that the large airfoil absorbed so much power that the maximum local Mach number reached at full tunnel power was only about 1.0, considerably less than we had hoped for. Heiser estimated that much better performance could be obtained by mounting a short section of the airfoil directly on the floor of the tunnel and fairing it into the wall. Obviously a tunnel boundary-layer removal system would also be required. This would have involved more cost and effort than the idea was worth, in our opinion, and Heiser told the committee we were planning no further work at 16-foot. This investigation is believed to have been the first NACA attempt to define and develop what later came to be called the "bump." Lockheed (ref. 102), Ames (ref. 103), and Langley (ref. 104), started subsequent successful developments of the bump in 1946. It was used extensively in the Ames 16-foot high-speed tunnel and the Langley high-speed 7 x 10-foot tunnel, largely replacing the aircraft wing-flow work in the period before the large slotted tunnels became fully operational. The bump programs naturally disappeared in the early fifties along with the other stop-gap transonic techniques (the wing-flow, the annular tunnel, and the body-drop programs). The final summary of the Langley bump tests of wings by Polhamus (ref. 104) contains the following modest obituary: "There are many shortcomings of the Transonic Bump technique. . . . The results are believed to give at least a qualitative indication of the type of effects encountered at transonic speeds, and fairly reliable indications of trends. . . ."

THE BODY-DROP AND ROCKET-MODEL TECHNIQUES

Early agency literature refers to these techniques as "Bomb-Drop" and "Missile-Test," revealing their wartime origins. It is not certain whether NACA supervisors Crowley and Thompson, who are said to have "considered" the body-drop approach in 1942 (ref. 54) had any knowledge of the prior German use of the technique in 1941 (ref. 105). It is apparent, in any case, that the American development was a great improvement over the German (ref. 106). The technique supplied primarily zero-lift drag data at speeds up to about Mach 1.3. A question regarding possible errors due to acceleration effects was raised early in the program by von Karman, but it was proved later, by drops of identical models of varying weight, that the effect was negligible. The reliable drag data from the body drops were used, for example, to estimate the drag and power requirements for the transonic research airplanes (ref. 107). As the rocket models came into use in the latter forties the body drops diminished but continued to be used occasionally for special purposes. They have provided important comparative data for evaluation of slotted tunnels (ref. 108), but otherwise there was little interaction of this technique with the transonic wind tunnel developments.

The rocket-model approach started as a missile test and development program, but it rather quickly started to change character. Reflecting both the interests of its NACA operators and the growing demand for transonic aerodynamic data, it evolved into a program of general aerodynamic tests covering the entire transonic region and beyond into the supersonic regime. The flight data became increasingly more accurate and more comprehensive as time went on as a result of the impressive ingenuity brought to bear on the many challenging aspects of this technique. Inevitably, the practitioners of the technique tended to become as much interested in making the rocket models do more things more accurately as they were in the research problems. To a large degree, therefore, one finds that the Pilotless Aircraft Research Division (PARD) reports tended to be data reports for specific test objects rather than general or analytical treatments of research problems.

One aspect of the technique caused major interference with the wind tunnel programs: each firing required the sacrifice of the test model, including in many cases complex and costly internal instrumentation. For example, in the years 1947, 1948, and 1949 no less than 386 models were expended (ref. 109). This is roughly equivalent to the requirements of perhaps 10 major wind tunnels such as the 16-foot. Furthermore, the wind tunnel models generally carry only pressure taps, or house a balance which can be used repeatedly in many models. There was a major slowdown in both wind-tunnel model and instrumentation production as a consequence of PARD's voracious appetite.

By mid-1946 transonic data reports had been published from all of the transonic methods then in use and a serious problem had arisen. Large discrepancies were apparent, and, understandably, queries had been received from industry users. The matter was discussed at the July 12, 1946, meeting of the Langley General Aerodynamics Committee, and a special ad hoc group was set up to study the problem. The group made its first report on September 17, 1946. W. H. Phillips showed that in one category, significant differences in the transonic drag of straight wings were believed explainable on the basis of large test Reynolds number differences. In the case of complete wing body configurations, very large and unexplainable differences existed; the gross trends, however, were similar. The group recommended that a specific wing and the X–2 aircraft configuration be tested by all of the techniques, including the 9-inch supersonic tunnel, for comparative study. Nine months later, on June 13, 1947, the group reported "no new conclusions." By that time the Langley bump was in full operation and the supply of discrepant data was growing rapidly. Cases were found where not only the data values disagreed, but also the trends were at variance. The low opinion of the bump data shared by a majority of Langley aerodynamicists found expression in a memorandum submitted to Langley's Chief of Research F. L. Thompson by E. C. Draley of the 8-foot tunnel, and discussed at the June 13, 1947, meeting of the Langley General Aerodynamics Committee. Draley was particularly concerned about the validity of stability and control data from the bump.

Thompson considered this problem of sufficient importance to take the unprecedented step of personally presenting an introductory paper on the subject at the NACA Conference on the Aerodynamic Problems of Transonic Airplane Design on September 27, 1949. He gave a brief objective assessment of all the different transonic techniques and focused special attention on the problems of the wing-flow and bump. He said in effect that the bump in most cases provided useful trends or comparisons, but bump data should not be used quantitatively.

HIGH-SPEED RESEARCH AIRPLANES

On a spring morning in 1940, Stack and I left the office and drove to the remote beach at the easternmost tip of the Virginia Peninsula to watch the first attempt to obtain supercritical aerodynamic data on an airplane in free flight. A Navy fighter, the Brewster XF2A–2, was to be dived vertically over Chesapeake Bay to its terminal velocity, about 575 mph, and then make a pullup at its design load factor. The Brewster had been instrumented to measure the pressure distribution at an inboard wing station by the Langley Flight Division. We were most apprehensive as we watched the dive through binoculars. This was before the possible consequences of compressibility effects on the buffeting and control of diving airplanes had been highlighted by the P–38 tragedy of 1941; nevertheless, our knowledge of shock-stalled flows in the wind tunnel left little doubt about the dangers of this dive. Happily, the flight was completed successfully without any undue difficulties for the Navy pilot, but we were both left with the strong feeling that a diving airplane operating close to its structural limits was not an acceptable way to acquire high-speed research information. This experience undoubtedly contributed to Stack's later advocacy of a special research airplane capable of supercritical speeds in level flight.

Tests of the NACA 230-series section used on the Brewster were made in the 4 x 18-inch high-speed tunnel and the results are compared with the flight data in fig. 21. The principal differences (in shock location) were due primarily to irregularities in the airplane wing, some of them distortions under air loads (ref. 101). In general, we were satisfied that the wind tunnel had been validated at least up to Mach 0.75, but we could see that future flight testing would be much more valuable if the surface distortions could be eliminated by use of thicker skins.

By 1942, it was apparent that the diving speeds of advanced fighters would penetrate more deeply into the supercritical region, equalling or exceeding the choking speeds of the wind tunnel test configurations then in use. We considered it unlikely at that time that the wind tunnel could ever be used at speeds beyond about Mach 0.8, and we therefore increasingly leaned toward the idea of a specially configured and instrumented test airplane capable of safe operation in this speed range. The

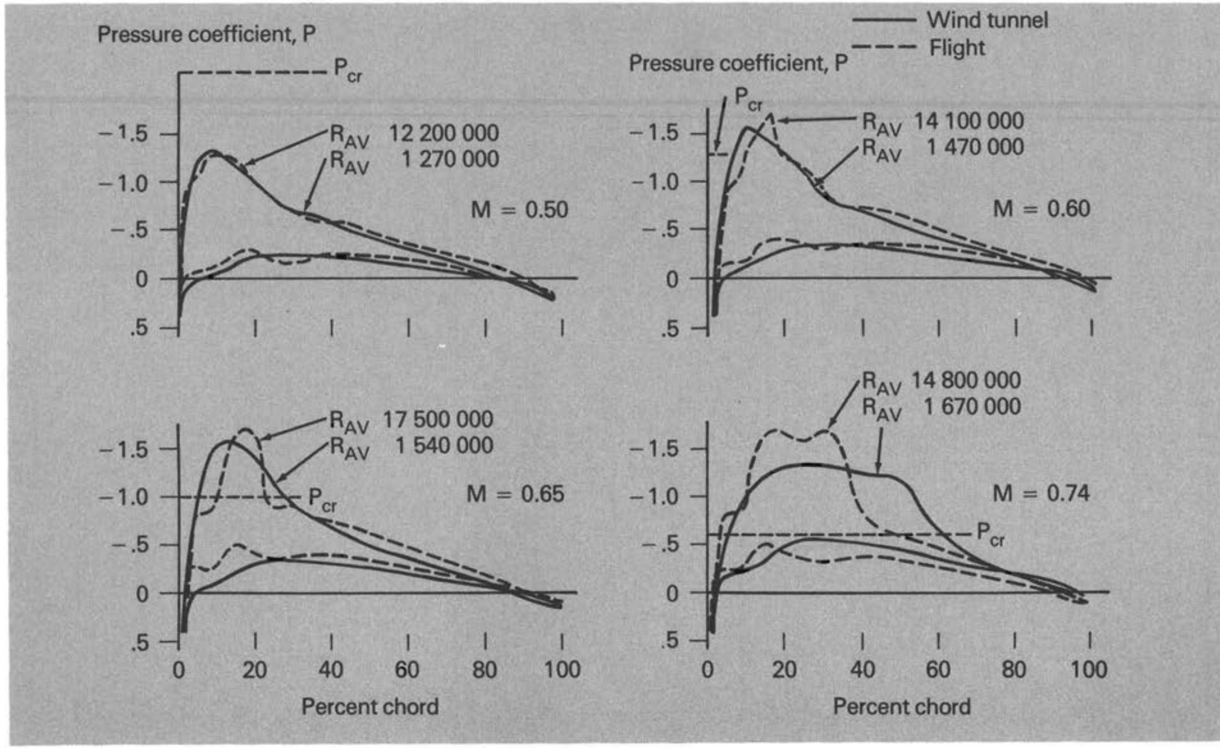

FIGURE 21.—*Comparison of flight and wind tunnel pressure-distribution measurements for $C_n = 0.4$; airplane wing, NACA 230-series section 14⅓ percent thick; wind tunnel model, NACA 23015 section.*

best recollection of those of us who were involved is that this idea did not appear full-blown as a visionary new proposal of any single individual. Rather it took form gradually, manipulated and developed in innumerable lunchroom conversations and other contacts. Stack was a central figure in these discussions, and became the chief Langley promoter of the idea, but he was vague in regard to a specific origin. In a talk given in 1965 at a history session of the AIAA devoted mostly to the X–1 research airplane (ref. 10), he said:

> After some deliberation, free flight with a manned instrumented airplane seemed the best and most direct way. Now, of all the people who contributed to this effort, it seems to me the two most noteworthy were General Arnold of the Air Force and Dr. Lewis of NACA. And as I noted, it was just about 23 years ago to the day [the summer of 1942] when word was given that we ought to go on something like this, with the caution that we couldn't spare many men because there was a war on.

This verbal authorization to "go" by no means implied general approval to design and procure a research airplane; it was simply permission for a limited preliminary study of the problems and desirable design features. Milton Davidson and Harold Turner, Jr., were logical choices to make preliminary layouts and performance estimates because they had done some work of this kind for Jacobs. Under Stack's direction, Davidson and Turner first concentrated on designs capable of high subsonic speeds up to about Mach 0.9. It is important to note here that Stack, in that period, did not consider or advocate pushing through Mach 1 to supersonic flight speeds. My firm recollection on this point is substantiated by those of several others including Soule (ref. 110). It is also supported by documentation (e.g., ref. 107) which gives Mach 0.8 to 1.0 as the range of NACA interest for a research airplane. The possible performance of prospective turbojets was uncertain at that time but it appeared likely that an engine would emerge which might marginally provide Mach 0.9 in a small airplane. The idea of rocket propulsion was quite beyond NACA thinking at that time; however, the Army with its background of JATO rocket development was willing to consider it. The first Army proposal for a high-speed research airplane by E. Kotcher in 1939 listed rocket propulsion as an alternative, and in the Army study of the "Mach 0.999" research airplane in early 1944 a principal objective was to compare performance of rocket and turbojet versions (ref. 111). It was obvious that the only hope at that time for pushing through Mach 1 in level flight lay in rocket propulsion.

Navy interest in a possible high-speed research airplane also began to stir in the 1942–44 period (ref. 112). However, no direct action toward procurement was taken by any of the interested parties prior to the March 15, 1944, seminar-type meeting at Langley of Army, Navy, and NACA personnel (ref. 113). Some significant differences of opinion relating to the design features and goals of a transonic research airplane surfaced at this meeting. NACA tended to think of the airplane as a device for collecting aerodynamic data unobtainable in the wind tunnel at high subsonic speeds. But the Army thought of it more as a major developmental step toward higher operating speeds extending upward through Mach 1. The Navy view inclined toward dispelling the myth of an impenetrable barrier and providing needed high-speed data. These differences could rather easily be accommodated in a single vehicle concept except for the Army's interest in demonstrating transonic and low-supersonic speeds which led to their advocacy of a rocket engine for propulsion, a feature which NACA considered too risky. Except for propulsion, the configurations of the airplanes being studied by NACA and the Army were similar; both agencies considered very simple conventional unswept designs.

Undoubtedly, one of the main values of this meeting was a stirring-up of the competitive natures of the participants to the point where actual procurement activities would soon be initiated. Further discussions took place with the Army on May 15 and 16, 1944, and on July 18, 1944, the final NACA turbojet-powered design produced by the small Langley group was transmitted to the Army personnel who by now had declared themselves expressly interested in funding a research airplane (ref. 113). A critique of the NACA design was presented by Army personnel at a Langley meeting on December 13 and 14, 1944, centering on the inadequate performance achievable with the turbojet. NACA emphasized the supposed safety aspects and relatively long-duration data-gathering flights possible with the conventional power plant. Furthermore, the turbojet would have obvious applicability to future military aircraft while the rocket propulsion system did not. This apparently unreconcilable difference was easily resolved; the Army was putting up the money and they decided to do it their way. In late December they started negotiations with Bell Aircraft to procure a rocket airplane.

When it became clear at the meetings in early 1944 with E. Kotcher and his cohorts that the Army was likely to be insistent on a rocket airplane, Stack renewed his efforts to interest the Navy in procuring the kind of airplane NACA wanted. Almost all of his contacts were by telephone, personal visits, or through M. Davidson who had been detailed to the Navy. Stack's view then was that the rocket approach was so risky that the Bell airplane would probably not survive many flights and in any event would not get enough air time to collect much data. The Navy, in the persons of E. Conlon, W. S. Diehl, and I. Driggs, was receptive. Nothing had been done in the Navy in the way of research airplane studies and they were ready to accept the NACA general guidelines. Belatedly, in September of 1944, they started to consider details of such a vehicle within the Bureau of Aeronautics, developing a philosophy not inconsistent with NACA's that the aircraft should be designed with some potential for militarily useful follow-on versions. Douglas was selected to build the airplane in early 1945. It was designated the D–558–1, and was almost exactly the airplane Stack desired (ref. 110). Throughout the development period, he displayed a strong preference for the Navy airplane and we extended ourselves in every way to assist in its development.

During Stack's absence on his first European trip, I was sent to Wright Field on March 15, 1945, to represent NACA at the first design review of the X–1 (then designated XS–1). Prior to leaving, I examined recent drop-body drag data in the vicinity of Mach 1, visited the Flight Division, and talked to Davidson to get their views on performance, operations, and instrumentation. According to my notes, Mel Gough, Langley's chief test pilot, condemned the rocket airplane. "No NACA pilot will ever be permitted to fly an airplane powered by a damned firecracker" was his ultimatum. (Ironically, it was the turbojet-powered D–558–1 which killed a NACA pilot due to engine failure while the X–1's had a good safety record at Edwards. The D–558–1 barely exceeded Mach 0.83 in level flight and was limited to Mach numbers below 1.0 in dives. With further irony, it was the transonic and supersonic flight achievements of and they decided to do it their way. In late December they started negotiations with Bell Aircraft to procure a rocket airplane.

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At Wright Field, I found Bell's design to be basically similar to the simple arrangements of the Army Mach 0.999 study and the NACA studies. In general, NACA recommendations other than power plant and speed range had been accepted (refs. 107, 114). Almost two-thirds of the takeoff gross weight was in rocket propellants—an unheard-of fuel fraction for military aircraft of that day. Were it not for the fact that a major part of the propellants were used up in takeoff and climb, the X–1 as then defined could have reached projected speeds far in excess of Mach 1.2, the "cruise" speed required by the Army. It was apparent that a cruise speed of Mach 1 could certainly be reached from ground takeoff even with more conservative drag estimates based on the body-drop data, and I pointed out that this made the airplane acceptable from the NACA viewpoint which suggested Mach 0.8 to 1.0 as the desired region for flight research (ref. 107).

Later Bell's considerations of safety and performance with a less energetic propulsion system led in May 1945 to a major change from ground takeoff to air launch. NACA strongly opposed air launch. Not only did it violate the NACA notion that a research airplane should operate as conventionally as possible, but it also meant that in all probability the airplane could never be operated out of Langley Field. Langley managers thus feared they would lose control of an air-launched X–1 flight program (ref. 110). The NACA protests were of no avail because air launch was now the only remaining option if low supersonic speeds were to be achieved as required by the Army.

Concurrently with, but unrelated to, the X–1 and D–558–1 research airplane activities of 1944 and 1945, M. C. Ellis and C. E. Brown of Langley's 9-inch supersonic tunnel section studied the feasibility of a small supersonic airplane powered by a hypothetical ramjet engine at Mach 1.4. As was appropriate in a rough preliminary assessment of this kind, their airplane was a primitive assemblage of basic elements—straight sharp-edged wings and tail, and simple propulsive-duct fuselage with the pilot sitting in a small enclosure in the middle of the duct (fig. 22). The results showed that a ramjet of practical proportions could

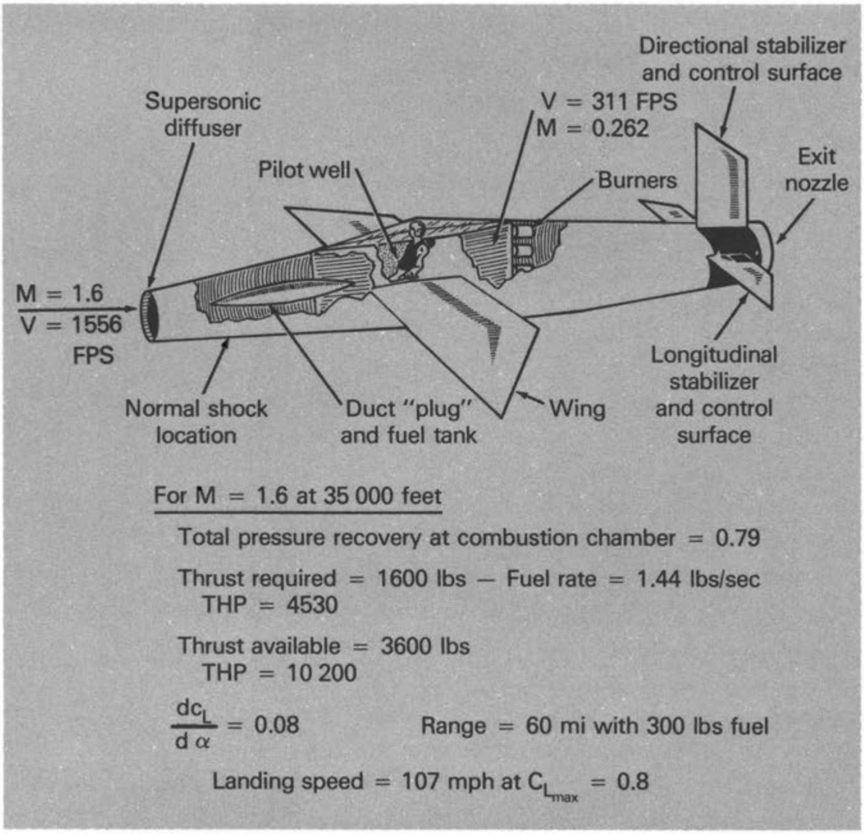


FIGURE 22.—*Ramjet-powered configuration analyzed by Ellis and Brown, 1944–1945. From a Langley Conference chart.*

indeed provide the necessary cruise propulsion for a 60-mile range at Mach 1.4; however, other means of acceleration through the transonic region (rockets) would be required, and airplane tow (later, air launch) was envisioned for takeoff (ref. 115). At Stack's instigation, there was brief local consideration of this vehicle as a possible addition to the X–1 and D–558–1 stable of research airplanes. However, because of the lack of any ramjet engine, the problems of acceleration, and more particularly the fact that transonic flight testing of the simpler X–1 and D–558–1 was still several years away, it was quite obvious that such a vehicle could not logically be undertaken at that time, and Stack wisely did not attempt to mount a real crusade for it.

COMMENTARY

It is unlikely that the many innovations and rapid progress in the transonic ground facilities would have happened as they did without the stimulus and focus provided by the X-1 and D-558. Clearly there was a most important two-way flow of benefits: stimulated by the problems of the research airplanes, new ground facilities and techniques were developed which, in turn, produced vitally needed new data in time for the design and safe operation of the aircraft.

The primary postulate justifying the transonic research airplanes was the supposed impossibility of useful wind tunnel operations in the speed range above Mach 0.8. And yet before the research airplanes were operated transonically the fallacy of this justification had been demonstrated; the wind tunnel choking problem had been circumvented in a variety of ways. Thus the early concept of research aircraft providing unique new data otherwise unobtainable became obsolete. Instead, a principal value of the transonic flights lay in evaluation and validation of the ground-based techniques. The fact that the first transonic flights showed no unexpected occurrences was also of great value. The most basic value, however, was the liberation of researchers and aircraft designers from their fears and inhibitions relative to the "sonic barrier." The awesome transonic zone had been reduced to ordinary proportions, and aeronautical engineers could now proceed with the design of supersonic aircraft with confidence.

During the course of this review of the first research airplanes, I turned up a number of apparent misconceptions and inaccuracies in the records which are worth noting. One should expect, of course, that the offhand and undocumented remarks recorded in interviews of NACA old-timers will contain inadvertent inaccuracies, distortions, and oversimplifications. I am concerned here with larger issues, in which questionable NACA party-line versions of what happened seem to have gained general acceptance, establishing a sort of agency mythology or folklore.

Myth: That NACA deliberately planned for two complementary vehicles, one (the X–1) to be a unique special design for pushing through Mach 1, and the other (the D–558–1) to be representative of advanced military service types with turbojet propulsion for studying flight problems in the Mach range up to about 0.95. This view is specifically stated in certain of the interviews conducted by Bonney in the early seventies, and both Keller and Hallion gained the same impression from their interviews (refs. 112, 116). Actually, as previously documented, NACA had argued strongly against a rocket vehicle like the X–1, and even after it was in procurement NACA stated that the subsonic speed range from 0.8 to 1.0 was the area they desired to explore (refs. 107, 114). The D–558–1 was the research airplane NACA wanted (ref. 110).

If the D–558–1 could have been promoted in the early forties, it would have been timely. But coming into the flight picture as it did in 1947, it was unnecessary. Contemporary service airplanes with equal or better performance became operational in the same period and they could have been instrumented and used for most of the work conducted by the D–558–1. For example, the F–86 Sabre began to exceed Mach 1 regularly in dives in the summer of 1948, some time before the D–558–1 inadvertently slightly exceeded Mach 1 for the only time on September 29, 1948. The world's speed record of 650 mph briefly held by the D–558–1 also fell to the F–86 on September 15, 1948, when 671 mph was recorded.

Nevertheless, it was the D–558–1's and not the advanced service aircraft that were used for extensive flight research at high subsonic speeds by NACA, complementing coverage of the higher transonic speeds by the X–1's. It is quite understandable how some NACA managers by hindsight can see a logic in the way these two vehicles were used that did not really exist when they were promoted in 1944 and 1945.

NACA always chose to emphasize the positive factors of the program as it finally evolved, passing over early controversies. An example is seen in Stack's 1951 paper (ref. 54) in which he said, "The research airplane program has been a cooperative venture from the start. . . . The extent of the 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."

Myth: That a lack of knowledge or misunderstanding of the effects of wing thickness ratio on transonic performance led to the major differences of opinion in NACA as to what thickness ratio should be used on the research airplanes. Actually, from the earliest works of Dryden and the NACA high-speed airfoil group, a major conclusion was that the severity of shock-stall effects could be minimized by using thin sections. Ferri's airfoil work (ref. 45), extending to Mach 0.94, edited in report form in January 1945 and published in June 1945, listed as a primary conclusion: "Airfoils of large thickness ratio should not be used at high Mach numbers because of radical adverse changes in their characteristics at supercritical speeds." Gilruth's secret wing-flow data of 1945 extended the test speed range beyond Mach 1 and it served to underscore the existing understanding of the problem. It did not provide pivotal new revelations of the advantages of thin wings as has been implied (ref. 112).

The real argument was over whether the research airplanes should be designed deliberately to encounter severe shock stalls well below Mach 1 for correlation with the wind tunnel data. Stack argued vociferously for a 12-percent-thick wing (an "average" rather than a "thick" wing according to 1945 practice) which would start to encounter flow changes at Mach numbers of about 0.75. This was one of the first major crusades into which he put the full force of his unusual talents. The main thrust of his argument was that there would be far less risk with this over-strength airplane with a 12-percent wing in level flight than Army test pilots had accepted repeatedly in pullups from high-speed dives. Gilruth, however, took the more conservative view that the first aircraft to penetrate deeply into the supercritical zone should have every known feature which would contribute to a safe operation—and a thin wing was indisputably one of the most important features for minimizing supercritical buffeting, lift loss, and control problems. Thompson sided with Gilruth. The first X–1 was flown with an 8-percent-thick wing of very low camber. However, the pressure distribution measurements, which were of prime importance for comparison with the wind tunnels, were made on a 10-percent-thick wing—not much thinner than Stack had wanted.

At it turned out, the most important region for comparison of flight and tunnels was from Mach 0.9 to 1.1, and the thinner wings served as well as a thicker one would have. The region of deep shock stall, Mach

0.75 to 0.9, which Stack advocated, proved relatively unimportant from the correlation standpoint. Twenty years later, accepting the teachings of history, Stack acknowledged the correctness of the thin-wing decision in remarks made at the AIAA history meeting of 1965 (ref. 10) where he said, "We knew it should have a thin wing."

Myth: That NACA made a substantial effort to promote a supersonic ramjet-powered research airplane in 1945. The unusual emphasis with which Stack recalled the exploratory study of Ellis and Brown in his 1965 history talk and interviews with Hallion and others (refs. 10, 112) seems to have created an exaggerated historical view of the importance of this concept in the research airplane picture of 1945. There was no ramjet engine then in existence to power such a vehicle; the X–1 and D–558 were still in the early stages of procurement; rather obviously, any proposal for such a vehicle was premature and had virtually no chance of support. Neither Ellis nor I have any record of the proposal. H. A. Soulé believes he recalls a Stack memorandum which was either lost or withdrawn (ref. 110). In any case Stack's effort was brief and in no way comparable to his vigorous and long-standing promotions of the transonic airplanes. There is no doubt that Stack had a strong personal interest in supersonic flight in 1945—in addition to his better-known interest in flight research at high subsonic speeds. Perhaps this is the point he wished to make in his talks with the historians.

THE SLOTTED TRANSONIC TUNNEL

The idea that the opposite effects of open and closed walls could be utilized in certain combinations to reduce or eliminate any net effect of the walls on wind-tunnel test results dates back to the classical Prandtl and Glauert work of the twenties. It was considered extensively by several other authors in the thirties. During the war, theoretical work on the problem was continued in England, Germany, Italy and Japan, and several investigators identified partly-open wall arrangements which theoretically eliminated the blockage effect on velocity at the tunnel axis (refs. 46, 94). Moreover, the general similarity rules showed that this result would continue to be valid at high subsonic speeds if the models were not too large (ref. 83). Reid mentioned the German activity to us when he returned from his War Department Alsos Mission assignment in 1945. No actual construction of a multi-slotted tunnel had been started, however, because of war circumstances. Technical reports covering the foreign theoretical work did not become available to Langley until after the war. Ferri's successful use of the rectangular semi-open tunnel for body and airfoil testing up to Mach numbers near 1 with no apparent jet-boundary effects was the first real demonstration that partly-open arrangements could be used effectively. As explained previously, Langley developed an improved version of this approach for high-speed airfoil testing in 1948. However, it was not practicable to employ this scheme in very large facilities such as the 8-foot tunnel because of its excessive power requirements relative to closed throats and other problems.

The first successful many-slotted transonic tunnel configuration was devised single-handedly by Ray H. Wright. Wright had been hired in 1936 as a scientific aide at $1200 per annum following unhappy employment as an inspector in a whiskey distillery where his M.S. in physics from the University of Kentucky was largely wasted. (The distillery job had been especially distasteful to Wright, a teetotaler, because he came home every afternoon reeking of whiskey.) Expecting to be told what to do at Langley under the close supervision of some senior physicist, Wright was surprised to find his boss at the 8-foot tunnel, R. G. Robinson, to be an engineer who sought theoretical answers and advice from him in an area where he had little knowledge and no experience. He had a natural aptitude for applied mathematics but his training in the subject had been rather limited. He received permission to acquire the needed additional skills by studying on the job as time permitted. As a result, in a group populated almost entirely by engineers he became an indispensable consultant on matters mathematical and theoretical.

No one told Wright that the time had come to define a slotted tunnel. His assignment was very broad—to study the wall interference problem with reference to operations in the repowered 8-foot tunnel. He was, of course, familiar with previous research and he had aided Donaldson in a small preliminary study relating to airfoil blockage in semi-open and closed tunnels (ref. 53). He was aware in a vague way that Stack along with many others intuitively anticipated that a partly-open configuration could be found eventually, but had received no specific directive to work on the problem. The goal that he chose to focus on was specifically related to the test section of the 8-foot tunnel—to determine a slot configuration for its circular test section which would produce zero axial velocity increment due to the blockage effect from a body of revolution. The semi-open rectangular tunnel solution of Weiselberger, which in effect was a slotted tunnel with two slots, was not applicable on three counts: it was not circular, it would have had a power requirement well in excess of what was available, and it was known from Ferri's application to have serious flow pulsations.

Wright attacked the problem analytically because, as a specialist in applied mathematics, that was his established method of research. Experimental work at 8-foot had almost always been done by the engineers. He agrees that a systematic experimental attack on the problem might have been equally effective (ref. 117). A specific 10-slot configuration was selected for analysis, the object of the calculations being to find the slot width or degree of openness that would result in zero blockage. All such calculations, because of their difficulty, necessarily assumed low-subsonic or incompressible flow. If, however, zero net axial blockage could be achieved, the general similarity rules suggested this result would continue to be valid at transonic Mach numbers (refs. 46, 48). Wright regards the tedious mathematics he used as "sloppy" because of the lack of definite convergence of his solution. The results suggested that the tunnel should have about 12 percent of the periphery open in contrast to Weiselberger's two-slot value of 46 percent (ref. 118). This was most encouraging because the excess power required by slots tends to be proportional to the open area, and would be much less in the 10-slot circular tunnel.

In the late summer of 1946, Wright discussed his tentative results with his section head, E. C. Draley, emphasizing the dual questions of convergence and whether the result would hold good at high Mach numbers. They decided to try to answer these questions by experiments with a 10-slotted model. Wright approached Lindsey to learn whether the 9 x 9-inch jet equipment might be utilized, but primarily because of its circular shape the slotted test section could not readily be adapted. He came next to my office with his problem. For some time we had been investigating blockage corrections at the 16-foot tunnel using the "parasite" technique previously described for our demonstration tunnels to power three circular test sections of varying size (fig. 23). Thus, it was quite easy for us to add a test program for Wright's circular 10-slotted arrangement. V. G. Ward, who had been working with C. H. McClellan on our blockage correction study, was assigned as project engineer for the experiments.

In the spring of 1947, Wright had an opportunity to discuss his work with Busemann who had recently been assigned to Langley as one of the foreign scientists acquired under the Navy's "Paperclip" program. Busemann suggested that a better theoretical approach would be to assume that the slot effect was uniformly distributed about the periphery rather than in discrete slots. He believed both lift interference and blockage effects could be treated from the standpoint of this homogeneous boundary. He also noted that the mathematics for the homogeneous wall promised to avoid the convergence problem. Unfortunately, much of

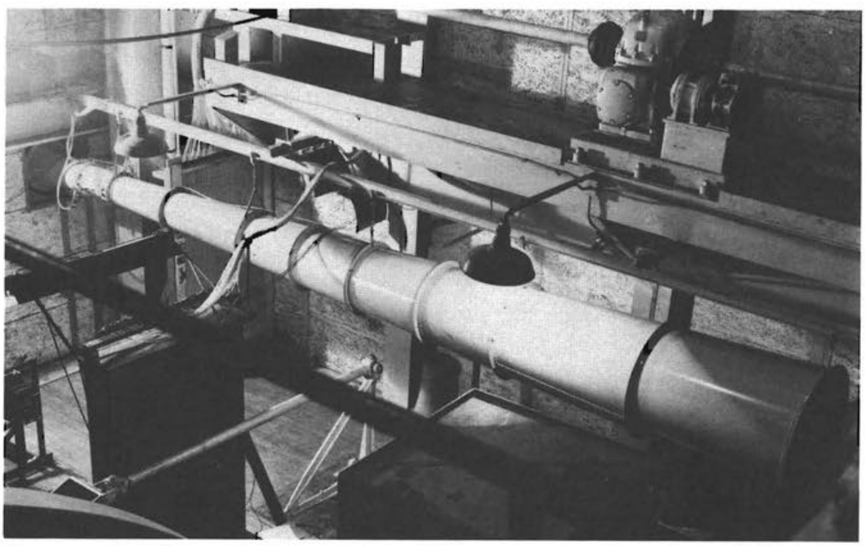


FIGURE 23.—*"Parasite" tunnel used to test the first successful slotted throat. The 12-inch diameter test section is at extreme left. Tunnel operates by suction of outside air into the test chamber of the 16-Foot High-Speed Tunnel.*

this advice was wasted, partly because Wright could not understand much of Busemann's English. Some four or five years later, Don Davis heard Busemann's arguments, presumably in improved English by that time, and applied the method successfully. His solution (ref. 119) is general in character but can be made to yield results comparable to any particular slot arrangement. When applied to Wright's particular case, satisfactory agreement was revealed between the two theories. Significant further improvements and extensions of the theory appeared later (ref. 94).

When Stack was told of Wright's theoretical results in the late summer of 1946, he sensed that the partly-open test section he had long anticipated had been found. He informed Dr. Lewis of the results and evidently mentioned their possible implications for the 8-foot and 16-foot tunnels (ref. 78). High priority in the Langley shops was provided for Ward's 12-inch diameter model slotted test section, and early in 1947 the experiments started. A key feature was tests of a body of revolution which would have caused choking in a 12-inch closed-throat tunnel at about Mach 0.70. In the first runs, the slotted tunnel speed could be increased to Mach 0.97 before choking occurred at the diffuser inlet, not in the test section—a problem which could be eliminated by relieving the diffuser contour. Unexpectedly, the Mach number without the model could be increased smoothly through Mach 1 up to about 1.15 as the diffuser pressure was reduced. Comparative pressure tests of the same small model in the 8-foot closed-throat tunnel showed good agreement with the small slotted tunnel up to the onset of choking at $M = 0.96$ in the 8-foot tunnel tests.

These early 1947 results were impressive, but there was no immediate acceptance of the slotted configuration and no immediate planning to incorporate it into either the 8-foot or 16-foot tunnels. Stack presented a summary of the situation as it existed in mid-summer of 1947 at a meeting of the General Aerodynamics Committee on July 25, 1947. He made no mention of any specific plans to install slotted throats in the large tunnels, although he did infer that the Wright/Ward work had important implications. According to the minutes, he told the group only that plans and funding had been approved to repower 16-foot with 60 000 hp (instead of the 40 000 hp originally requested and approved for the fiscal year 1947 budget) to produce Mach 1.3 in a closed-throat supersonic nozzle. Similar performance was believed obtainable in the 8-foot tunnel with its 18 000 hp (ref. 81).

Privately, however, Stack had begun telling his associates in mid-1947 that the 16-foot tunnel should consider using a slotted throat. He described this in his later press release as the period when a "definite commitment" to this idea was made (ref. 78). At first, it was really only a commitment in his own mind. He went on in the press release to say that some of his colleagues considered such a move premature. In his words, he was conscious "of a very strong undercurrent of disbelief." And, indeed, there was good reason for disbelief. The many major unanswered questions at that time included: the power requirements, the details of slot shaping, especially near the entrance and diffuser regions, 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. My own opinion was that an orderly continuation of the model tunnel program for as long as needed to provide answers should be pursued before any commitment was made to incorporate slots in the 16-foot or 8-foot tunnels.

A day or so after the July 25 meetings, Ferri knocked on my door and sat down to discuss a new concern relating to the slotted tunnel program. He 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. NACA's reputation would be blemished, he said, unless we could convince Stack to use some words of qualification when discussing slotted tunnels. Later discussions with Busemann revealed that he agreed with Ferri on this point. I suggested that the best way to make this important point clear to all concerned would be to air the subject at a meeting of the General Aerodynamics Committee, and I arranged with Sam Katzoff, Chairman of the Committee, to make the slotted tunnel problem a principal item on the agenda for the September 1947 meeting. Meanwhile, I told Stack of this special concern. He agreed to attend the meeting but was obviously irritated.

Ward and Wright presented their results in rather modest terms at the

September meeting. Stack 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." Ferri made his comment but the point was lost through a combination of poor English and extreme politeness, and the minutes of the meeting make no mention of it.

Actually, there was basic validity to Ferri's argument. In their report (ref. 118), Wright and Ward cautioned that the allowable model size for zero blockage "must decrease as the [subsonic] Mach number increases." This fact was strongly underscored in a much later very careful investigation (ref. 108) which found that, even with a model blockage ratio as small as 0.0003, significant interference effects in slotted tunnels occurred near Mach 1. Such a model would have a cross-section of only about nine square inches in the 16-foot tunnel, and this is more than an order of magnitude smaller than the previously considered "safe" size of about 144 square inches. For the larger size, the results appear interference-free up to about Mach 0.98, however, so that the extensive data obtained with large models through the fifties and sixties are suspect only in the range beyond about 0.98. (See fig. 24.)

There had been several ideas for possible closed-throat test section concepts for the repowered 16-foot tunnel which would have enabled it to cover the subsonic speed range up to $M = 0.95$ and the supersonic range from about Mach 1.1 to 1.3. On March 5, 1946, B. W. Corson, Jr., had suggested that trials be made of the use of air addition to form a "throat," or air removal to provide expansion similar to the diverging walls of a supersonic nozzle. (In later years, he successfully combined the removal idea with the slotted test section in the design now in use to vary the speed of the 16-foot tunnel between Mach 1 and 1.3.) The Langley engineering section had developed designs incorporating adjustable walls in a rectangular test section, and a "revolver" design using an assemblage of interchangeable fixed nozzles. At the time of Stack's decision to go with a slotted throat in 1947 the interchangeable nozzles were the favored scheme (ref. 120). Mechanically, this was a rather awesome arrangement of several 16-foot diameter nozzles carried on a rotating mechanism similar to the cylinder of a revolver.

Next to its elimination of choking the slotted tunnel was especially

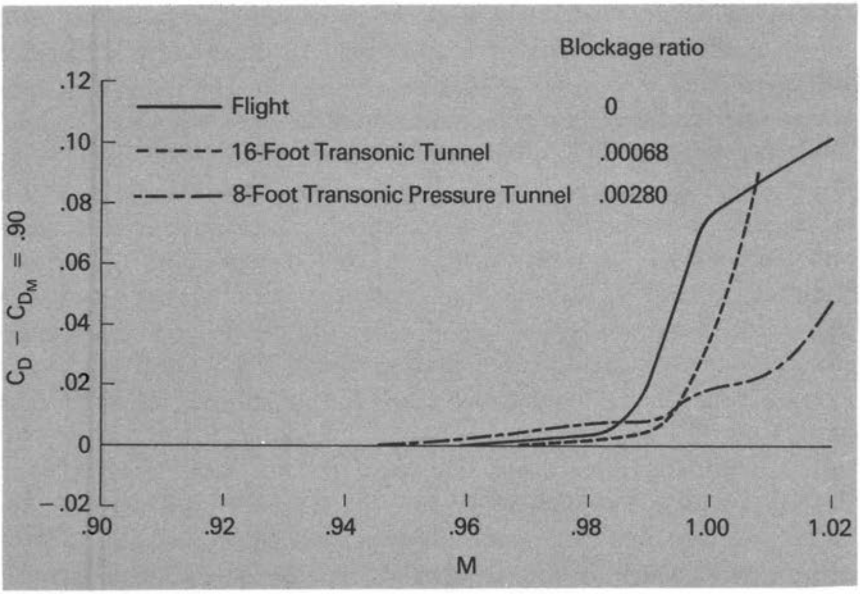


Figure 24.—*Comparison of flight (drop tests) and slotted-tunnel drag data showing discrepancies at Mach numbers above 0.98, 1973 data.*

attractive because it also eliminated the need for these complex and costly mechanically-variable test sections. I had given a good deal of thought to other possibilities for Mach number variation in fixed closed-throat test sections in the hope of finding a sound scheme that would be competitive with the slotted throat. In August 1947 I proposed that heat addition or removal be investigated as a means of Mach number variation (ref. 121). This idea grew out of an analysis I had made the year before of the possibility of using the ramjet principle to power high-speed wind tunnels, a suggestion offered by Vannevar Bush (ref. 122). Although sound in principle, the heat transfer schemes proved impractical for very large test sections.

The large postwar shift of research emphasis toward supersonic flight caused a major expansion of the Compressibility Division's facilities, including the addition of the 4-foot Supersonic Pressure Tunnel, the Gas Dynamics Laboratory, and the Induction Aerodynamics Laboratory (transferred from the Full-Scale Division). In order to achieve a better balance in regard to size and scope of management responsibility, the 8-foot and 16-foot tunnels were transferred to the Full-Scale Division which operated the 19-foot and full-scale tunnels. The individuals now responsible for the slotted throat developments were not likely to offer much resistance to Stack's inclination to rush ahead, a situation which he undoubtedly considered satisfying. My doubts about the path he was taking were so strong, however, that I ignored the organizational changes and continued to plague him with criticism and suggestions.

During the fall of 1947, as Stack's plans to install a slotted throat in the 16-foot continued to solidify, I spent some time analyzing a new scheme whereby variable Mach number could be obtained simply and at low cost in a fixed closed-throat nozzle for the 16-foot (ref. 123). The basis of my idea was a 50-foot-long Mach 1.3 nozzle (fig. 25) which had such a gradual area expansion that quasi-uniform flow existed at each station, providing a continuous gradual Mach number increase from 1.0 to 1.3. A sting-supported model mounted through a swept-back strut on an external track positioned the model at any desired location. Mach 0. to 0.95 would be covered in the throat location and the low supersonic range from about 1.10 to 1.30 would be covered by moving the model and its support downstream. (Because of its high power requirements, the slotted throat would be limited to a maximum Mach number of about 1.1 for the same 60 000-hp input.) Subsonically, a model of smaller size than for the slotted throat would have to be used and the choked speed range between about 0.95 and 1.10 could not be covered. The test models would also be operating in a small pressure gradient; however, this effect was quite small, amounting to less than 3 percent correction in drag for 5-foot-long models (ref. 123). The scheme appeared to be much simpler and cheaper than the "revolver" idea for alternate nozzles. A recent demonstration of the practicality of changing model location in a fixed nozzle had been made in the 8-foot Mach 1.2 nozzle where the model had undergone subsonic testing in the throat and Mach 1.2 testing in the downstream position (ref. 124).

I presented this scheme to Stack in late December 1947, hoping that there might still be time to encourage what I believed to be a more rational sequence of events for developing the slotted concept. I emphasized how a slotted throat could readily be incorporated later in the

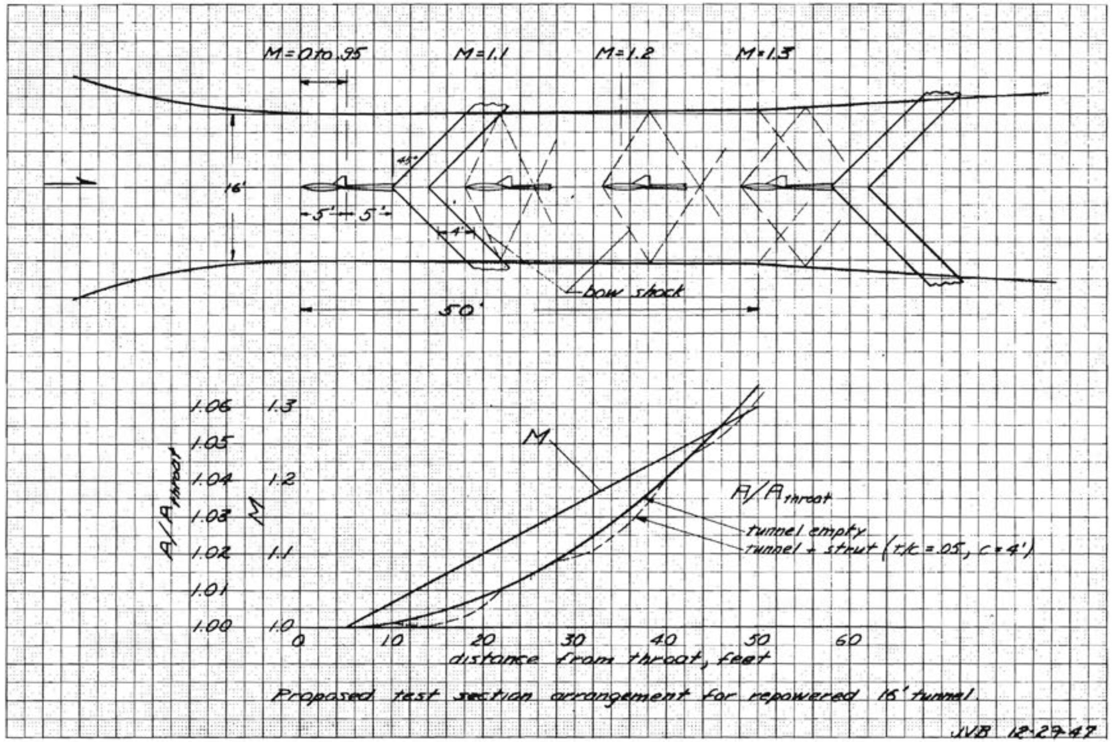


FIGURE 25.—*Fixed-geometry nozzle with variable model position for transonic testing up to Mach 0.95 and from 1.10 to 1.30. This was one of several schemes put forward in the 1946–1948 time period as alternatives to the slotted throat.*

long test section after the slot design had been properly developed. It was quite obvious, however, that by this time Stack had become so deeply and totally committed to the slotted throat that there was no turning back. It was apparent that he regarded my scheme more as a possible obstacle to gaining top-level final approvals for the slotted program than as an opportunity to pursue a more moderate course. He suggested that in case a major difficulty should be encountered in the slotted program the long test section should be considered as an alternative, and he requested that I record the idea in a memorandum (ref. 123).

The slotted throat for the 16-foot tunnel was handled as a part of the 60 000-hp repowering project. A formal description and justification was prepared by Corson, head of the 16-foot tunnel section, on January 10, 1948 (ref. 120). It is obvious now that the understanding of the slotted throat as evidenced in the Corson text was seriously deficient in at least two major areas, the power requirements and the problem of valid testing at low supersonic speeds. The fact that the slotted tunnel would not provide generally useful test capabilities at speeds for typical models in the range from about Mach 0.98 to 1.05 because of problems related to reflections of compression waves had not yet been learned (ref. 108).

Ward was under heavy pressure to come up with the additional data needed for the 16-foot design. The technique of operating the 16-foot tunnel itself in order to provide suction power for the 12-inch model slotted tunnel was proving too slow to meet the demand, and Stack asked me to consider using the blower equipment in our Induction Aerodynamics Laboratory to power Ward's tunnel. We assigned W. J. Nelson to work with Ward, and by early spring of 1948 a comprehensive program had been agreed upon (ref. 125) and tests were in progress. Nelson quickly developed a keen interest in the problem and initiated his own program of investigation of slotted and porous-wall configurations (ref. 126) using a small rectangular tunnel better suited to such work than Ward's scale-model of the 16-foot test section, which by now had become octagonal and 8-slotted. (This octagonal arrangement had been proposed by E. M. Gregory of the engineering group as a mechanically desirable approximation to Wright's original configuration.)

By early spring of 1948, Stack was providing his 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 test section, and dealing as always with funding and approval problems. He held frequent meetings of the key individuals involved at this time including E. Johnson, P. Crain, and E. Gregory of the engineering and shop groups, and E. Draley, B. Corson, A. Mattson, R. Wright, V. Ward, and W. Nelson of research. Stack was at his best in this kind of operation. He was adamant regarding schedules, at times ruthless in dealing with any interference, and always able to inspire, to make quick decisions, and to give effective orders.

One day, after the 16-foot tunnel project was well underway, he surprised everyone by announcing that the 8-foot tunnel should also be converted to a slotted throat. At that time, the plaster liner had just completed successful development and was starting to be used for research. The 8-foot group had given little thought to the next step beyond the plaster liner and protested that they would need time to study the possibilities. The plan to slot the 8-foot tunnel quickly took form under strong pressure from Stack. Since the necessary fabrication could be done in Langley's shops and the installation made by Langley labor, this relatively inexpensive alteration was not subject to the formal approval and procurement processes of a major new facility. Before long, it was apparent that it would precede the 16-foot project, becoming the first large slotted tunnel to be placed in operation. Stack's main motivation in adding the 8-foot tunnel slot development was probably concern over the low Reynolds numbers of the model throat tests, a concern which turned out to be well founded. He was also naturally very impatient at the prospect of two to three years of procurement time before the 16-foot tunnel would be operable.

It was a fairly straightforward matter to replace the old 8-foot test section with a 12-sided, 12-slotted version built in the Langley shops. Some use was made of Ward's model work with the 16-foot tunnel configuration, particularly for the diffuser entrance area requirements. Ward had also found that tapering of the slot width was desirable to prevent too rapid initial expansion at low supersonic speeds (ref. 127), but this feature was not used; the slots were rectangular and similar to those of Wright's analysis. The slots opened directly into the igloo-shaped test chamber and it was obvious that hazardous pressure, temperature, and noise levels would be encountered (fig. 26). Diving suits were, therefore, worn by operators whose presence was required in the test chamber during the initial tests with the slots (fig. 27).

As in Ward's model tests, the simple rectangular slots provided reasonably uniform flow for choke-free model testing at speeds up to Mach 1. At supersonic speeds, however, intolerable large deviations occurred (ref. 128). On the tunnel axis for a nominal Mach number of 1.09, the speed varied between extremes of Mach 1.0 and 1.16. Large power losses occurred due to inefficient features of the flow at the downstream end where it entered the diffuser. Several months were devoted to correcting these difficulties. Valuable guidance and design data were provided by the work of Ward and Nelson with the model slotted test sections. But it is quite evident from a study of the final reports (refs. 128, 129) that by working directly with the 8-foot throat itself, a degree of important detail and refinement were attained well beyond anything that could have been done with the small models. Wright's principal coworkers in this effort were V. Ritchie and R. Whitcomb. Excellent supersonic tunnel-empty flow distributions were eventually achieved. An efficient flapped scoop-type entrance section for the diffuser entrance was devised by Whitcomb to reduce the power requirements, the flap being left open for subsonic operation and closed for supersonic (fig. 28). Research usage of the tunnel commenced on October 6, 1950, some seven months after the start of slot developmental testing.

The slot technology improvements from the 8-foot program were passed on to the 16-foot, 8-slot design. According to NACA claims (ref. 129) this made it possible for 16-foot to become operational after only 30 hours of shakedown in December 1950. Actually, along with the research operations a continuing program of slot development was pursued in both tunnels. The presence of the tunnel boundary layer was found to have an important influence on slot behavior, neglected in all of the theoretical studies. Furthermore, the slot widths for elimination of lift interference were shown to be much smaller than those for zero drag interference (ref. 130). Perhaps the most important limitation discovered in the early usage of the big tunnels, however, was the inability of the slots to alleviate significantly the reflection of pressure disturbances from

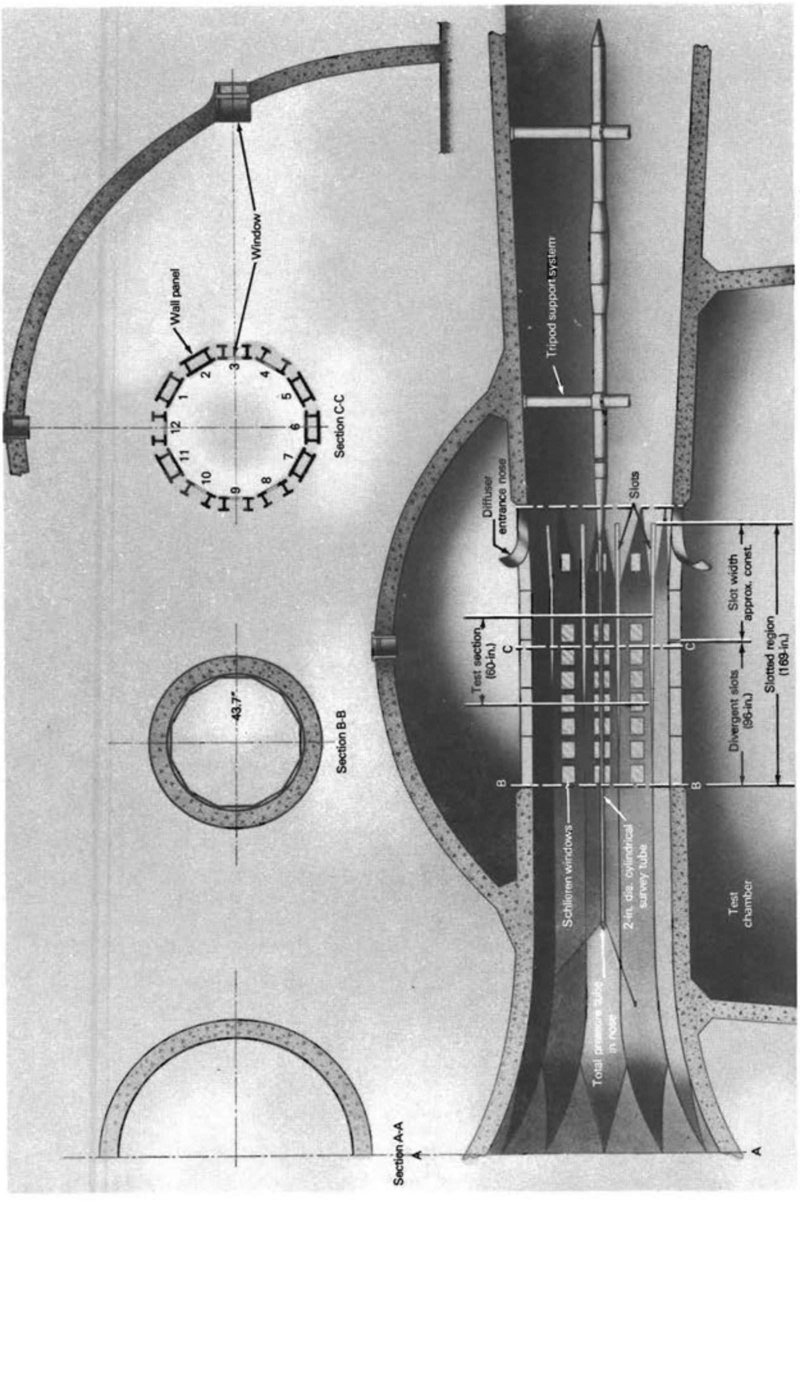


FIGURE 26.—*Slotted throat installation in the repowered 8-Foot High-Speed Tunnel, 1950.*

FIGURE 27.—*Ray H. Wright, designer of the slotted throat, dons a diving suit for protection against noise and heat in early runs in the test chamber of the 8-foot slotted tunnel.*

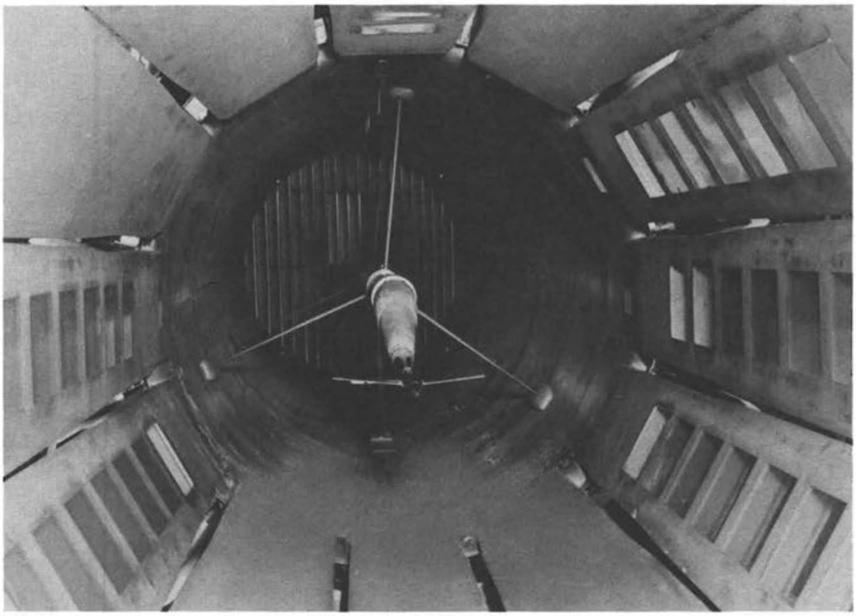


FIGURE 28.—*View of the 8-foot slotted throat showing diffuser-entrance flaps.*

the solid regions of the walls. Thus, although there was no choking and although the speed could be increased continuously in the low supersonic region above Mach 1, the test data often exhibited significant discrepancies when compared with free air. For the selected cases considered in ref. 129, the Mach range above about 1.02 showed such effects in the 8-foot tunnel; better agreement was shown for another selected model tested in 16-foot. The general experience in 16-foot, however, has revealed so many uncertainties in the range from about 0.98 to 1.05 that it is usually bypassed in setting up test programs (ref. 135). Similar low-supersonic data are also considered not valid in the present 8-foot tunnel operations (Whitcomb and Bielat interviews). The model sizes for valid operation in the range Mach 1.1 to 1.3 are no larger than for solid-wall tunnels.

Knowledge of the NACA programs of the 1946–1950 period was, of course, readily available to the military services and their contractors, and this stimulated many activities outside NACA. Some of these are listed in the Appendix. Publications covering work with slotted test sections at Brown University (ref. 131), and with porous test sections at Cornell Aero Lab (ref. 132) appeared in the early fifties along with others. In the 1951 Annual Report of the NACA, J. C. Hunsaker's letter of transmittal to the Congress announced to all the world,

> During the year the Committee completed the installation of a transonic ventilated throat in the 16-foot tunnel at the Langley Aeronautical Laboratory. This . . . is of exceptional importance because it permits model airplane tests at transonic airspeeds in wind tunnels, hitherto impossible because of choking. . . .

With this stimulus together with the advanced status of background technology on the subject, it was a foregone conclusion that transonic throats would quickly blossom throughout the world. By 1954, realizing that the technology already had become more or less universal, NACA dramatically removed the classified wraps from much of its work and announced in the annual report that through "intensive effort" starting prior to 1942 and a "calculated gamble of millions of dollars" NACA had won a "vital" two-year advantage for the United States in the "world race" to learn how best to fly at transonic speeds.

COMMENTARY

Looking back on the situation that existed in early 1945 when the 8-foot tunnel started operating with 18 000 hp, one sees a combination of favorable circumstances from which it was inevitable that some usable form of partly-open throat configuration would crystallize. Pertinent features of this environment included:

- A 15-year worldwide background of theory clearly suggesting the general potentialities and identifying certain specific tunnel configurations such as that of Weiselberger (ref. 46).
- Experimental success of the semi-open two-dimensional tunnel for airfoil tests at speeds approaching Mach 1.
- New demands for transonic design data starting with the design of the research airplanes.
- Concern about the low Reynolds numbers of the "small-model" and especially the "wing-flow" techniques.

- Two major high-speed wind tunnels with large power margins at Mach 1.
- Researchers with the necessary experience, skills, and freedom to explore.

Wright's personal decision in 1945 to get down to cases and try to define analytically a multi-slotted circular configuration was the act that set in motion the events that led in about five years to the successful operation of the first large transonic tunnels. Most of the developmental testing in this period also clearly bears the stamp of Wright's insights and personal integrity. It is equally clear that without the enormous contributions of a quite different kind made by Stack the achievement of the large slotted tunnels would not have happened in 1950. To begin with, Stack had promoted almost single-handedly the projects to repower 8-foot and 16-foot. And, although he did not specifically initiate the slotted-tunnel studies, he had created a research environment in which an idea like Wright's could be freely explored and allowed to grow. Stack's principal personal contribution, however, was in promoting and implementing the plans for immediate application of the slotted throat in the two major facilities. He persevered in this against the conservative advice of senior staff members. What drove him with such zeal is not entirely clear. We had only begun to exploit the "small-model" technique and could have continued for years supplying much of the transonic data needed by designers at speeds up to Mach 0.95 and at Mach 1.1 or 1.2, with the rocket models providing additional high Reynolds number transonic data. In part, Stack's zeal grew from his ill-founded belief that the slots would permit interference-free testing of large models throughout the transonic zone—in the low supersonic as well as in the high subsonic portions. Perhaps he particularly wished to make good on his ambitious projections to G. W. Lewis in 1946. Undoubtedly, he also sensed the dramatic impact that the first large tunnel operating through Mach 1 with substantial models would have.

It is also evident now from experience with large slotted tunnels that no amount of preliminary testing in small model tunnels can eliminate the need for refined developmental testing in the full-scale facility itself. Thus, by proceeding immediately (and to all appearances in 1948, prematurely) with the 8-foot installation, the NACA slotted tunnel developers came to grips at once with all of the real full-scale problems. The solutions found here had a convincing validity and value beyond anything that could have been done in the model tunnels.

It could hardly be expected that NACA's first public disclosures of the slotted tunnels would be modest and carefully qualified. The entire accomplishment dating back to the start of high-speed research in the early thirties was indicated to be exclusively NACA's (refs. 129, 133). Both of these documents emphasize that "large-scale aerodynamic research" can now be "conducted throughout the full transonic speed range." Ref. 133, the 1954 Annual Report of the NACA, gave the details later and mentioned some problems under the heading of "Fluid Mechanics."

Learning that NACA had declassified sufficient slotted-tunnel material to cover the 1954 disclosures, the Institute of the Aeronautical Sciences solicited a paper on the subject from Stack for its summer meeting in 1954. Stack relayed the preparation of this paper down to section head A. T. Mattson. With Stack breathing down his neck and the agency involved in glorification of a dramatic new accomplishment, Mattson was under great pressure to accent the positive aspects, and this explains the slanted quality of his paper. For reasons unknown, Stack told Mattson at the last minute that he would not attend the meeting, and Mattson had the unhappy task of presenting his glowing paper to an audience which included a number of outspoken eminent skeptics. Fortunately, the paper did admit the problem that had been found at low supersonic speeds, although it stated a bit too hopefully, "in most practical cases this is not a serious problem as even within this range the effects are not great and can be defined" (ref. 129). As we have seen, the problem still exists in the region from about Mach 0.98 to 1.05 in which valid testing is not ordinarily possible.

The 1954 NACA Annual Report heightened the drama by calling the entire enterprise "a calculated gamble" involving "millions of dollars and the future value of one of NACA's most valuable wind tunnels." Actually, the cost of the new throats was a minor part of the total costs of repowering. And, if the slots had failed to perform, they could have been simply covered over and both tunnels could have operated with the small-model technique.

The general claim by NACA that the slotted tunnel provided America with a two-year lead time in aircraft development over her adversaries (ref. 78) is evidently based on Whitcomb's initial use of the 8-foot tunnel for the wing-body testing which led to his enunciation of the area rule (refs. 72, 134). General unsupported statements of this kind are hard to accept, but even so, few would argue with it if the claim were based on the massive total contribution of NACA's many-pronged attack on the transonic problem in the forties.

COMMENT ON MANAGEMENT METHODS

In today's large federal research agencies any program of comparable scope and importance would be managed by a Program Director and his staff in agency headquarters. Several committees of outside "specialists" would be involved with senior agency managers to define a structured program. Program definition would be followed by promotions, approvals, negotiations for funds, and finally by the start of work at the agency's centers and its contractor establishments.

By contrast, the NACA wind tunnel development program described in Chapter III was almost entirely unstructured. Management assumed that research ideas would emerge from an alert staff at all levels, rather than from outside sources. On a problem of major proportions such as transonic facilities any scheme for research that survived peer discussions and gained section and division approvals was likely to be implemented. In almost every instance the individual who proposed the idea for the research was made personally responsible for its execution. Thus each project was carried out by the one most highly motivated to make it succeed. The interest and zeal of such researchers is seldom seen on the staffs of today's project offices which are likely to be assembled from individuals who happen to be available from recently completed previous assignments.

Large structured programs require frequent reviews, coordination with other agencies, and repeated justifications. These functions are major time consumers and generators of enormous volumes of paperwork. Very little of this was required in the simple NACA system. Occasional chats with his division chief or department head, or a brief verbal report at the monthly department meeting were about all that was required of the NACA project engineer. The paperwork burden was almost nil; in many cases the final technical report was the only significant paperwork—surely the ideal minimum.

The ambitious Rocket-Model Program at Wallops Island and the High-Speed Research Airplane Program were exceptions to the simple pattern of the smaller projects. They required interagency coordination which necessarily involved more formal management structuring. Even so, by comparison to current practice the management of these programs was delightfully simple, direct, unobtrusive, and inexpensive.

CHAPTER IV

The High-Speed Propeller Program

The extensive propeller testing at low airspeeds and high rotational speeds in the Propeller Research Tunnel consistently showed a marked loss in efficiency starting at tip Mach numbers of about 0.9. Clearly, this was only a part of the compressibility problem of propellers for 500-mph aircraft, for which high Mach numbers would exist over the entire blade. The fact that the PRT tests showed a considerable delay in the onset of compressibility effects as compared with wind-tunnel section data suggested that estimates of high-speed propeller performance based on strip theory and section data as understood at that time could not be relied upon. Tests of propellers at high forward speeds were needed to provide precise information on the attainable performance.

Unlike many other NACA programs which started with little understanding of the problem, the high-speed propeller program enjoyed a well-established basic understanding; a substantial body of high-speed section data and criteria for design of efficient advanced propellers had been built up over the previous 20 years. In these circumstances, it was obviously unnecessary to explore the problem by testing existing propellers which would clearly prove to be inefficient at high speeds. Instead, a family of advanced high-speed propellers embodying the features known to be needed to favor high-speed performance was defined at the outset. The main purpose of the test program was to determine accurately the attainable high-speed performance as affected by systematic changes in the principal design variables.

The useful but rather uninspiring nature of this test program, together with the tedious aspects of high-speed dynamometer development, made it unattractive to impatient imaginative researchers seeking higher levels of challenge and excitement. But, like many other instances which come to mind, the propeller program seemed to attract the type of talent appropriate to the job—competent, practical, conservative engineers who were willing to devote many years to the exacting tasks of perfecting the large dynamometers and obtaining precision data under difficult test conditions.

The first step taken by NACA toward higher-speed testing was the approval in mid-1936 of plans for the 19-foot, 250-mph Pressure Tunnel, in essence a super-PRT. A new propeller dynamometer powered by large electric motors was a major feature of the plan. By the time this new tunnel was dedicated in May 1939, however, the continued increase in speed of military aircraft plus the growing war threat made it apparent that 250 mph was inadequate for high-speed propeller testing, and a second new Langley tunnel was undertaken—the 500-mph 16-foot High-Speed Tunnel. This facility was intended to concentrate on full-scale propellers and engine cowling and cooling, while the 19-foot tunnel would become involved primarily with scale-model aircraft testing and dynamic-loads research. Accordingly, the new propeller dynamometer project was transferred to the 16-foot tunnel enterprise.

Shortly after Stack had taken up his duties as head of the 8-foot tunnel section in 1939, the outlook for high-speed propeller testing in the 16-foot tunnel was discouraging. Major delays had been encountered in design and procurement of the new electrical equipment for the dynamometer, and it appeared that three or four years, at least, would elapse before testing could be expected. Stack reacted with characteristic impatience. He was quite unhappy at the prospect of a long delay in testing propellers incorporating the new 16-series blade sections. The only apparent solution was to procure a dynamometer for the 8-foot tunnel and run the tests on 4-foot diameter propellers. This would have the advantage of smaller (200-hp) electrical equipment, some of which was already available, and we projected that the desired answers should be forthcoming within about two years. Stack had little difficulty in selling this plan, and it was called the "Emergency Propeller Program" to answer any question of duplication with the 16-foot tunnel plans.

After the repowering of the 8-foot tunnel in 1945, propeller testing was extended to higher speeds (Mach 0.93) with an 800-hp dynamometer, and this program continued until conversion of the repowered tunnel to a slotted throat in 1950.

The 16-foot tunnel program utilizing nominally full-scale (10-foot-diameter) propellers got underway in 1945 with the 2000-hp dynamometer that had been so long in procurement and development. As in the case of 8-foot, this was later replaced by an improved 6000-hp dynamometer when the 16-foot was repowered in 1950. Speeds up to Mach 1.04 were achieved in full-scale propeller testing in the 16-foot slotted throat.

The Ames Laboratory embarked on a limited program of propeller testing in their 12-foot high-speed tunnel in the early fifties when it appeared that the turboprop application required research at high subsonic speeds. Their propeller dynamometer used 4-foot-diameter blades and 1000-hp motors taken from the Langley program, and it incorporated several other features from the Langley installations. Forward speeds up to Mach 0.84 were covered in the one series of high-speed tests made at Ames (reported in NACA TR 1336).

Throughout the period of the high-speed wind-tunnel propeller programs (1938–1958), periodic propeller testing was also done in flight on advanced fighter aircraft. Starting with such piston-engine aircraft as the XP–42 and P–47, the flight work ended in the mid-fifties with testing of three propellers at speeds up to about Mach 1, using a special turboprop engine installation in the nose of an XF–88B jet fighter.

THE EMERGENCY PROPELLER PROGRAM

No one in the 8-foot tunnel group had had any experience in propeller research except Stack. He had been periodically involved with PRT programs through his high-speed airfoil work, and since 1938 had been consulting with E. Hartman and others on the design of the high-speed propellers to be used in the forthcoming high-speed wind tunnel program. He continued to be deeply involved in the design of the test propellers, along with L. Feldman of the 8-foot group and J. Delano who was the designated project engineer for the emergency program. The test propellers that were designed represented major improvements over the best propellers then in service (fig. 29). They were generally thinner,

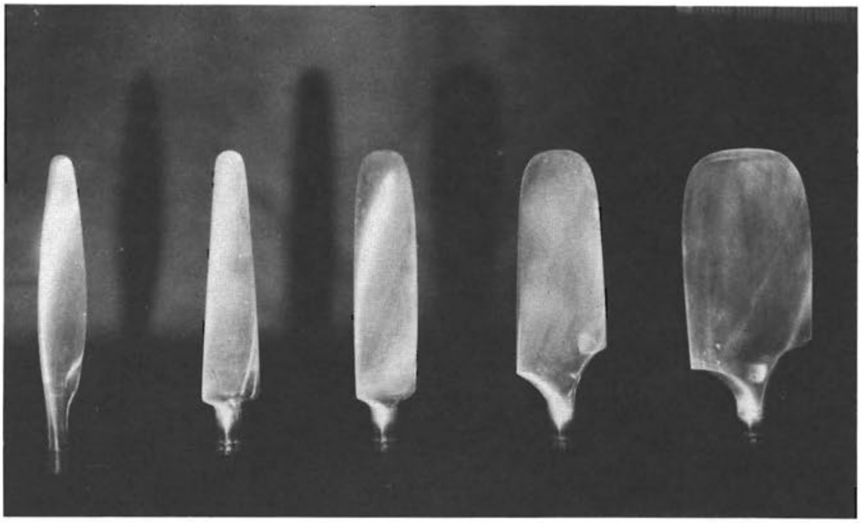


FIGURE 29.—*Blade shapes tested in the Emergency Propeller Program in the 8-Foot High-Speed Tunnel.*

tapering to about 4 percent thickness ratio near the tip from about 12.5 percent at the spinner. It had been obvious for some time that the thick root sections exposed on many then-operational propellers would suffer compressibility losses at high forward speeds, adding to the tip-region losses. These inefficient shank sections were completely covered in the NACA program by the large spinners employed on the dynamometers, a spinner diameter one-third the propeller diameter being used in the 8-foot tunnel tests. Blade widths of one and one-half, two, and three times the normal width were included because at a given power input the blade lift coefficients were correspondingly reduced and the critical speeds increased. Or, for a given lift coefficient and critical speed the power absorption could be correspondingly increased. All these important improvements were quite independent of the choice of blade section shape. The 16-series sections at that time were thought to offer improvements in critical speed of the order of 50 feet-per-second over some of the older sections, and they were used in nearly all the test propellers. Since 1938, Stack had been vigorously selling the 16-series to propeller designers and to NACA managers, and we were now under considerable pressure to confirm the advertised gains in an actual propeller test.

Following PRT practice, we selected a more-or-less representative nacelle for the 4-foot propeller tests. What is actually measured in a test of this kind is more properly termed "propulsive efficiency" of the propeller/nacelle unit, rather than "propeller efficiency." That is, the thrust determination includes effects of the slipstream on the body and support drag, and other secondary effects not present in tests of the forces on the propeller itself. The nacelle had one unusual feature which considerably complicated both its structural development and the problem of determining accurate tare forces, an open-nose spinner through which passed a flow of air representative of that required for cooling a large radial engine (fig. 30). The high-speed aerodynamics of this arrangement had been developed in an 8-foot tunnel program to have a critical

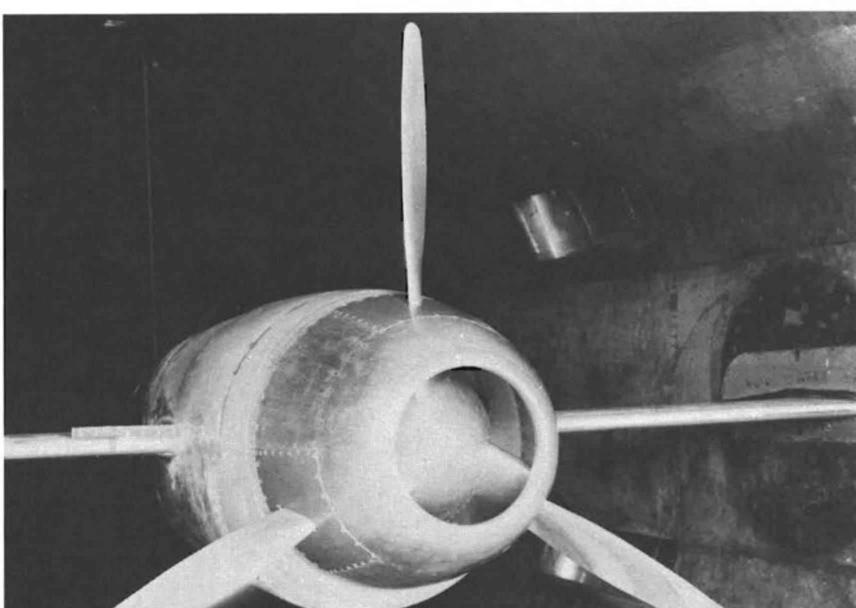


FIGURE 30.—*The 200-hp Emergency Propeller Dynamometer in the 8-Foot High-Speed Tunnel with 4-foot diameter standard blades.*

Mach number higher than the highest propeller test speed (see Chapter V), and this particular design had been the subject of a recent study of pursuit-airplane performance in the 19-foot tunnel (ref. 136).

The equipment needed for the 200-hp dynamometer was more readily obtainable than that for its larger counterpart at 16-foot. By December 1941 it was ready for the first tests of two-blade propellers. Reflecting our special interests, the first two test propellers were identical except for blade section shape, one having 16-series and the other having conventional Clark Y sections. To our dismay and disappointment, the 16-series propeller showed no advantage at high speeds; in fact the Clark Y appeared slightly better. Stack asserted emphatically that some systematic error must be present in the data and he assigned me the task of finding it. I had previously been only peripherally involved with the propeller program except for six weeks' work in the spring of 1941 on a theoretical analysis to determine the tunnel-wall corrections that would have to be applied. There were indeed several sources of significant error, particularly in the strain gage system used to measure torque and in the thrust and torque tares due to the blower-spinner. However, these were all either random in character or of about the same magnitude for both Clark Y and 16-series propellers. Regretfully, I concluded that any advantage of the 16-series was too small to be discernible within the existing rather poor limits of accuracy. The better part of the following year was devoted to improving the accuracy. Strain gages at that time were in an early stage for applications of this kind, but eventually acceptable accuracy was obtained through frequent calibrations. Satisfying confirmation of the overall accuracy including the tunnel-wall effect corrections was obtained in 1945 by running comparative tests of the 4-foot dynamometer in the 16-foot tunnel (ref. 137).

The probable explanation of the nearly equal high-speed performance of the Clark Y and 16-series propellers of equal thickness gradually became clear with additional two-dimensional testing and comparisons with other sections. Although the 16-series sections had higher critical speeds near their design lift coefficients, their force-break speeds were often not much higher than those of other good sections because the occurrence of shock at the rear of the 16-series profiles tended to produce separation shortly after the critical speed was reached (ref. 52). The sections for which the shocks occurred farther forward could in many cases significantly exceed the critical speed without encountering force break (see p. 36ff.). In spite of their failure to show any marked high-speed performance advantage over other good high-speed sections, the 16-series sections have been generally used by propeller designers for other reasons, particularly for the structural advantages of propeller blades which are relatively thick in the trailing edge region, compared, for example, to the cusped low-drag sections.

The results of the Clark Y propeller tests were never published and it was never tested again. Perhaps the relatively poor accuracy of these first tests justified withholding these data, but there was little real doubt in our minds that the two propellers had nearly equal performance.

On the positive side, these first high-speed wind tunnel tests of improved propellers showed that propulsive efficiencies in the range of 85 to 90 percent could be maintained to forward speeds of 500 mph, provided that high blade angles (of the order of 60°) were used to keep the rotational speeds low enough to avoid compressibility losses. Generally, performance started to deteriorate sharply if the tip Mach numbers exceeded about 0.91, a value about 0.05 to 0.10 higher than expected from section data, the discrepancy being explained by three-dimensional tip relief effects (ref. 136). The effects of increased solidity (ref. 138), shank shape (ref. 139), pitch distribution (ref. 140), and camber (ref. 141) were found to be consistent with expectations from the two-dimensional section data. In reviewing these results from the emergency program (ref. 142), E. C. Draley claimed that a 100-mph speed gain had been achieved over "typical previous propellers" by use of 16-series airfoils, thin sections, and ideal Betz distributions. However, he did not identify the previous propellers, but evidently assumed that they had thick shanks and thicker blade sections than these improved propellers.

FULL-SCALE PROPELLERS IN THE 16-FOOT HIGH-SPEED TUNNEL

The primary source of full-scale high-speed propeller data was the NACA 16-foot tunnel program on related 10-foot propellers conducted

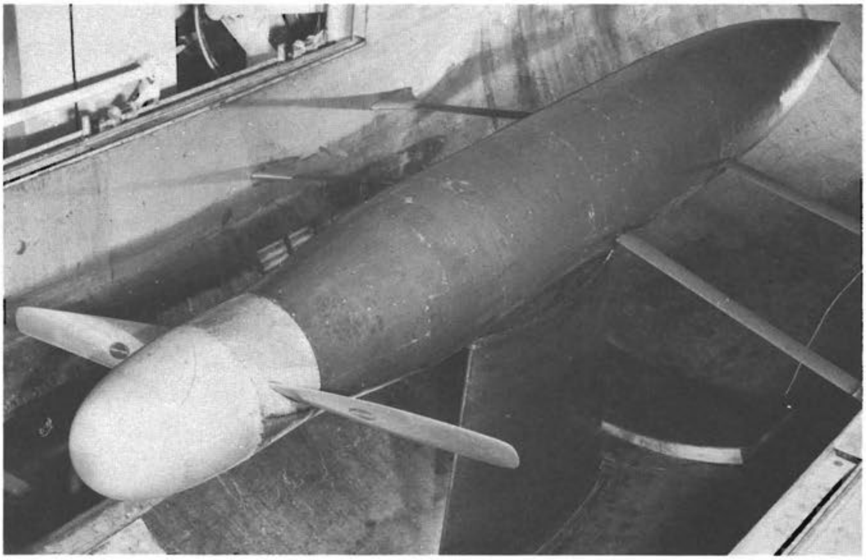


FIGURE 31.—*The 2000-hp Propeller Dynamometer in the 16-Foot High-Speed Tunnel.*

from 1945 to 1958. This was the program which had started to take shape in the thirties but had been long delayed pending the design and procurement of the 16-foot high-speed tunnel and the 2000-hp dynamometer. The staff of 16-foot comprised ex-PRT engineers almost exclusively, and most of them retained the conservative, practical attitudes toward propeller research which had characterized the PRT managements of Donald Woods and David Biermann. When I arrived at 16-foot in the summer of 1943, I soon learned that the staff regarded the emergency propeller program with its 4-foot "model" propellers in the 8-foot tunnel with considerable skepticism; the meaningful data would come later from the full-scale tests in 16-foot, conducted by men who understood propellers. None of us realized then that the 8-foot tunnel high-speed program would skim much of the cream, so to speak, leaving the 16-foot force-test programs of the forties to supply data which in most cases differed only in detail from the so-called "model" propeller tests.

The propeller program at 16-foot was supervised by B. W. Corson, Jr., a studious researcher who made many personal contributions, both analytical and inventive, in addition to his management activities. With only one or two exceptions, his colleagues were engineers and experimentalists. J. D. Maynard, a meticulous hard-working senior member of the staff, is credited with contributions to the dynamometer development in addition to the prolific production of precision propeller data evidenced by his publications.

Following shake-down testing and several important modifications and improvements, the 2000-hp dynamometer (fig. 31) began producing useful data in 1945 (ref. 143). It measured directly the thrust and torque of propeller plus spinner. Deducting the spinner forces yielded the characteristics of the propeller itself, free from the body-drag changes included in the "propulsive" characteristics determined in the 8-foot tunnel tests. The close agreement for most operating conditions between the 10-foot propeller data and much of the 4-foot "propulsive" data implies that both the scale effects and the propeller/nacelle interference effects were small. By mid-1948, the systematic force testing of the related 10-foot propellers on the 2000-hp dynamometer had been completed (refs. 144, 145, 146).

PROPELLER BLADE PRESSURE DISTRIBUTIONS AT HIGH SPEEDS

When we embarked on the project to measure the pressure distribution on the rotating blade of an axial-flow compressor in 1944 (refs. 96, 97), the ultimate application of the technique in the back of our minds was propeller pressure distributions at high speeds. If pressure data could somehow be obtained they could be analyzed to yield the blade section characteristics throughout the regions of the propeller over which the flows were supercritical and transonic. Not only were such airfoil data nonexistent in 1944, but also no method existed to apply airfoil data with confidence to the conditions existing over the outer region of the blade—conditions characterized by three-dimensional effects and a strong radial velocity gradient. The action of centrifugal force on the blade boundary layers was an additional uncertainty. Clearly the full-scale propeller program at 16-foot would be importantly enhanced if a technique for pressure measurement could be evolved.

Assuming the pressure transfer device could be made to work under high-speed conditions, we recognized that the next most difficult problem was how to install hundreds of pressure taps in the highly stressed blades without losing their structural integrity. I brought up this question at lunch with C. S. MacNeil, Chief Engineer of the Aeroproducts Division of General Motors, during his visit to Langley on September 1, 1944. Aeroproducts was producing hollow propellers fabricated from steel sheet and it had occurred to me that perhaps pressure tubes could be installed internally during fabrication. MacNeil thought they could and he promised to study the problem. About a week later he called to say the scheme was feasible and that he would like to build four test blades for us, each containing two sections with 24 pressure taps per section. I started the procurement with a memorandum to Mr. Miller describing our plan in detail (ref. 147). Some time after the work had been started at Aeroproducts, MacNeil, in his mid-thirties, suffered a fatal heart attack. The project continued but never recovered from the loss of MacNeil's zealous interest. When the test blades were delivered, many of the tubes were found to be blocked, and many others were leaking. None of the blades was ever used in research.

Corson took up the problem during the summer of 1946. By that time, the compressor-blade pressure measurements had been obtained successfully, and Corson's idea was to apply a similar method of tube installation in solid Duralumin propeller blades. In a sketch dated September 19, 1946, he suggested locating pressure tubes near the surface in radial slots on the test blade and covering them with a suitable filler. Langley shop supervisors improved on this scheme. They retained the tubing by peening the edges of the grooves and then filling them with a metal spray and refinishing the blade to its original contours. Holes were then drilled at the outermost station at the tip for the first tests. After completion of the test run, this row of holes was filled with a low-melting point alloy, and a second row of holes was then drilled at the adjacent inboard radial station. In this way, a total of 264 pressure taps were eventually installed in each blade, and only 24 radial tubes were needed. The first successful results with this technique were achieved in the fall of 1947 on the standard NACA test propeller, using the mercury-seal transfer device (ref. 148). By the end of 1949, five additional propellers had completed pressure-distribution testing (ref. 149) using the improved mechanically sealed transfer device developed by R. S. Davy (ref. 98). A total of 47 blade sections was investigated and 6554 individual pressure distributions were measured, in addition to wake surveys, force tests, and blade deflection measurements.

One of the first important uses of the high-speed pressure data was in the derivation of supercritical and transonic blade-section force coefficients for use in a general method for predicting propeller performance at high flight speeds involving transonic conditions on the propeller. Significant departures from two-dimensional airfoil data are evident in the outboard regions, chargeable to the combined effects of tip relief, Mach number gradients, radial flow of the boundary layers, and possibly to an induced-camber effect. The method successfully predicted the performance of the 4-foot propellers tested at airspeeds up to Mach 0.93 in the repowered 8-foot tunnel program (ref. 150).

ONE-BLADE PROPELLER TESTS

Both the 200-hp emergency propeller dynamometer and the 2000-hp dynamometer were underpowered for many of the desired high-speed testing conditions. It was for this reason that the bulk of the testing was carried out with two-blade propellers. It occurred to me in 1945 that we could double the power loading of our test blades if a one-blade propeller could be made to work; that is, we could obtain the same blade operating conditions as for a two-blade 10-foot propeller absorbing 4000 hp. As time permitted, I analyzed the balancing problems of articulated counter-balanced one-blade propellers in sufficient depth to convince myself that they were feasible and practical and they were added to our program. Tests with the one-blade propellers showed more vibration than the two-blade propellers because of their unbalanced aerodynamic loads but these were not excessive and did not affect the propeller data (ref. 151). The one-blade propellers (fig. 32) were used principally in the pressure-distribution programs (ref. 149), and in a few cases, additional force tests were made after the tubes had been removed and the grooves filled.

The measurement of blade deflections in these tests, and in general

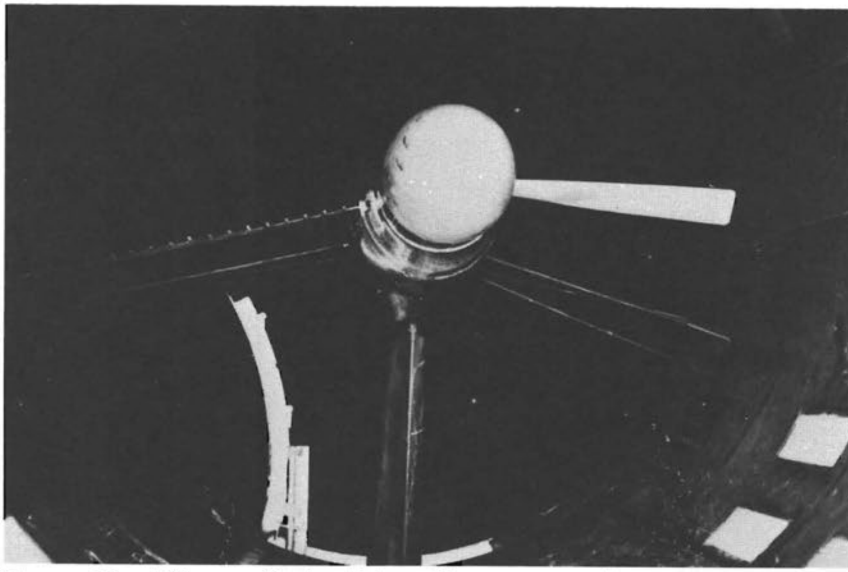


FIGURE 32.—*The one-blade, 10-foot-diameter propeller.*

the measurement of torsional deflections of thin-bladed propellers, is often required. An optical system for such measurements, developed by Corson in the PRT in 1940, was used in the 16-foot tunnel program (ref. 152). Torsional deflections were applied in the angle of attack determinations needed for the analysis of pressure distribution and other propeller test results.

SWEPTBACK PROPELLERS

Propeller researchers were in the forefront of the rush to apply the sweep principle in the mid-forties. The Alsos mission had reported that the Germans had tested a full-scale propeller with sweptback tips but no data on the results were available (ref. 153). A couple of years later, we obtained a translation of Quick's 1943 paper on the early German tests which indicated, rather inconclusively, that there was some advantage of sweep at high tip speeds (ref. 154).

It was clear from the outset that incorporating sweep in a propeller blade was a very complex matter, structurally as well as aerodynamically.

Any appreciable sweepback in the outer region of the blade had to be accompanied by sweepforward in the inner portion, the two portions being joined at an unswept "knee" (fig. 33). The first attempts to explore swept propellers, a brief flight program by the Curtiss-Wright Corporation and two propeller tests by the 16-foot tunnel group (ref. 154), involved only small amounts of sweep and showed small or negligible gains. They set the stage, however, for a better-planned effort involving more highly-swept blades and comparable unswept blades to provide meaningful evaluations of the sweep effects. The full-scale swept propeller tested in the 16-foot tunnel was designed for moderately high speed and power. A second propeller designed by Whitcomb to have the largest amount of sweep (45°) that could reasonably be incorporated within structural limitations was tested in the 8-foot tunnel at speeds up to Mach

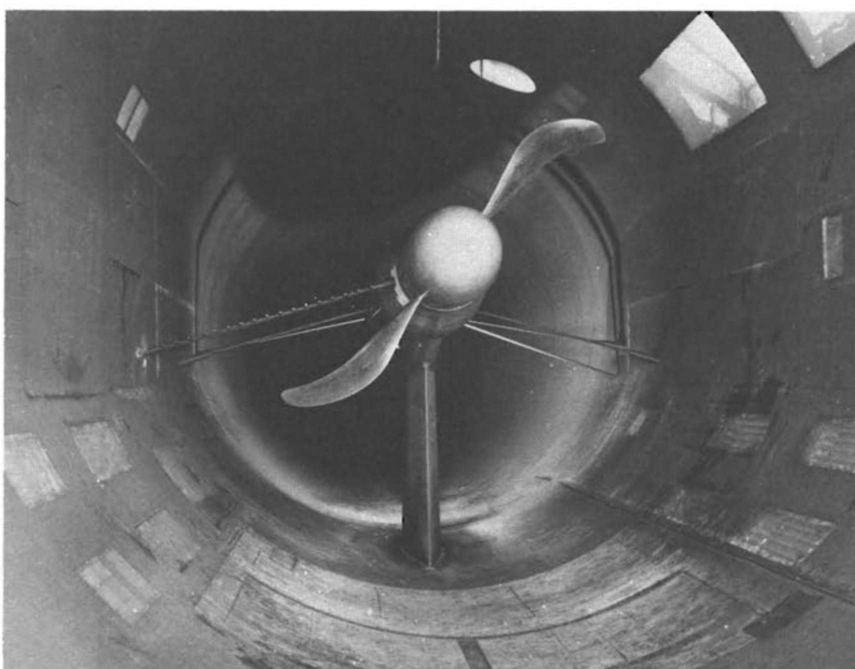


FIGURE 33.—*The swept-blade propeller tested on the 2000-hp dynamometer.*

0.93 on their 800-hp dynamometer, together with appropriate straight blades for comparison (ref. 155).

Both of the swept propellers showed a delay in the onset of compressibility losses to higher tip speeds than those of the straight blades of equal thickness. However, the delay was only about a quarter of what might be expected from the simple sweep theory. Offsetting the beneficial high-speed effect were generally lower levels of efficiency and other aerodynamic problems for the swept propellers. But the major conclusion brought out in the analysis stated that an unswept blade of slightly reduced thickness could always be found which would have equally good high-speed performance, better overall performance, significantly lower blade stresses, and freedom from the other structural complications of the swept propellers. This emphatic and disillusioning result put an end to any further attempts to exploit swept propellers.

TRANSONIC AND SUPERSONIC PROPELLERS

The 800-hp dynamometer placed in operation in the repowered 8-foot tunnel in 1946 (fig. 34) was used to extend the testing of the related NACA blade families to higher solidities and higher power loadings, including dual rotation. It was now possible to explore propeller performance at airspeeds up to Mach 0.93 where obviously the entire blade was operating supercritically. With large spinners supersonic helical Mach numbers could be obtained over the entire blade. Still deeper penetration into supersonic operation was achieved with the 6000-hp dynamometer used in the repowered and slotted 16-foot tunnel at airspeeds up to Mach 1.04 (fig. 35). Both dynamometers incorporated important improvements over their earlier counterparts (refs. 156, 157). The strut-choking effect for the 800-hp installation in 8-foot was largely avoided by locating the plane of the propellers in the subsonic throat region of the Mach 1.2 plaster nozzle, following the scheme sketched in fig. 14. The propeller program in 8-foot was completed before the slotted throat was installed in 1950, all propeller research at Langley thereafter being conducted in 16-foot or in actual flight.

After the war, with the growing reality of the turbo-propeller, the prospect of using propellers at speeds well beyond 500 mph, upward to

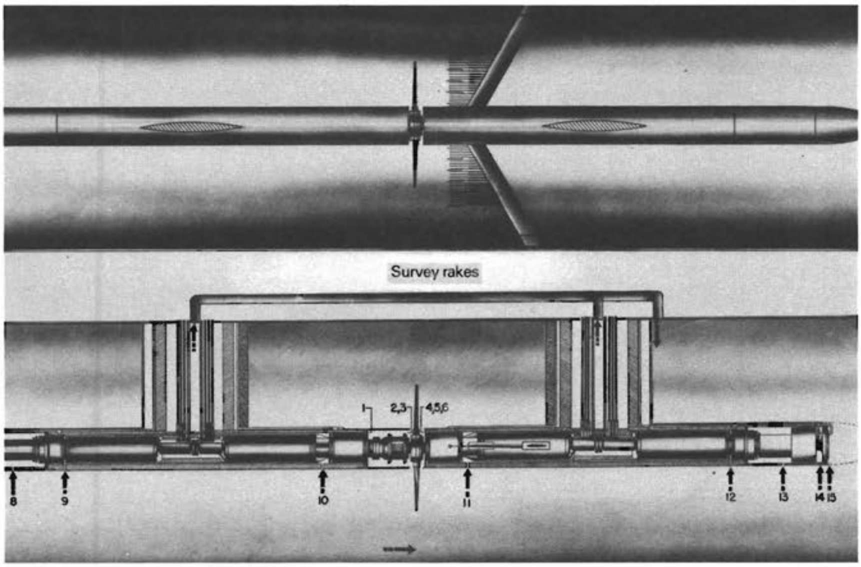


FIGURE 34.—*The 800-hp Propeller Dynamometer used in the repowered 8-Foot High-Speed Tunnel.*

transonic and even low supersonic flight speeds, seemed likely. High efficiencies had been maintained up to 500 mph by the devices of improved (primarily thinner) blade sections and reduced rotational speeds (high blade angles, or high advance ratio, V/nD). For normal subcritical operation best efficiency is obtained at reference blade angles of about 45° corresponding to a V/nD of about 2. At 500 mph the best blade angle is typically about 60°. Obviously, this device cannot be continued indefinitely as the speed increases because the blade angle ultimately becomes so high that efficiency starts to fall drastically due to the unfavorable inclination of the force vectors. One of the important contributions of the 8-foot tunnel program was to delineate the V/nD limits for best efficiency at speeds up to Mach 0.93. It was found that the increasing-blade-angle approach remained effective up to about Mach 0.85 but by Mach 0.9 and beyond a reversion to lower blade angles resulted in best efficiency.

A major additional asset in the use of high rotational speeds at

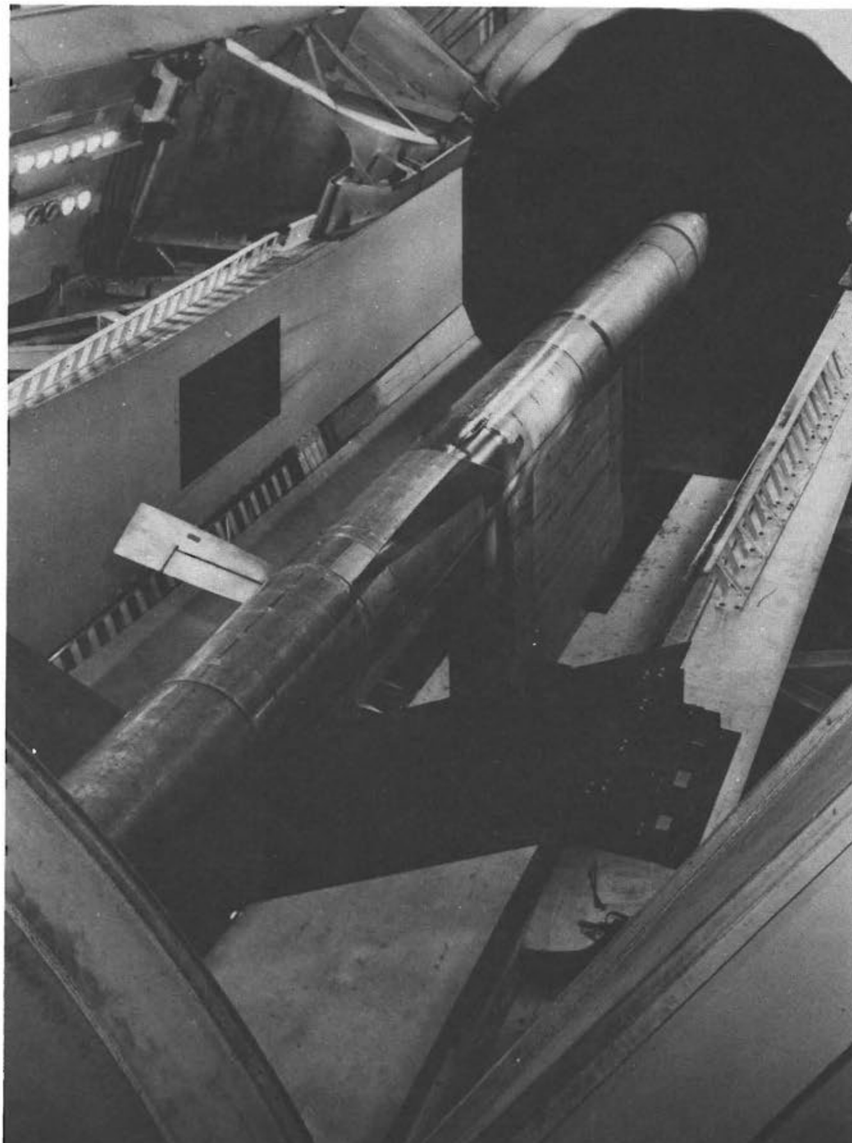


FIGURE 35.—*The 6000-hp Propeller Dynamometer installed in the re-powered slotted 16-Foot High-Speed Tunnel.*

transonic speeds is a large reduction in the physical size of the propeller, from 26 feet in diameter for $V/nD = 6$ to 12 feet for $V/nD = 2$, in an example given in ref. 155. A small propeller of this kind has supersonic conditions over virtually the entire exposed blade and is thus referred to as a "supersonic propeller" even though the design forward speed may be still subsonic.

The aerodynamic criteria for design of transonic or "supersonic" propellers with low profile losses were clear: use the thinnest possible blade sections, sharp or very small-radius leading edges, and little if any camber. The first two of these criteria had in fact been obvious for some 25 or 30 years—since the thin propeller tests of Reed, and the section tests of Briggs and Dryden. Propellers tapering from about 5-percent thickness ratio at the base to 2-percent at the tip with either zero or very small cambers yielded efficiencies of 75 to 80 percent at a forward Mach number of 0.9. At Mach 1, peak efficiencies as high as 0.75 were obtained (refs. 155, 158). The recovery in lift and l/d observed in airfoil section tests at high supercritical speeds where the separated flow disappears (fig. 4) is also seen in the propeller tests; curves of peak efficiency against flight speed level out and may rise slightly at speeds beyond about Mach 0.9 (ref. 155).

HIGH-SPEED FLIGHT TESTS OF PROPELLERS

Throughout the forties the Flight Research Division at Langley measured propeller performance on several piston-engine fighter aircraft at speeds in excess of 400 mph. The test propellers were generally typical of advanced service practice and they provided useful data on compressibility effects as they were encountered in actual operating conditions (refs. 160, 161). The results were invariably consistent with expectations based on the wind tunnel programs, but were rarely directly comparable because of differences in the test propellers. Principal figures in the flight work were T. Voglewede, A. Vogeley, and J. Hammack.

The successes of the high-speed research airplanes in the late forties had led to thinking at Langley about a possible "propeller research-airplane," and the flight division eventually succeeded in promoting such a project. Aimed primarily at potential long-range military applications, it was developed as a joint effort with the services; the Air Force provided the XF–88B airplane and the test propellers and associated equipment, and the Navy provided the turbojets and the T–38 turboprop engine which was installed in the nose of the XF–88B to power the test propellers. Unfortunately, this program did not start to produce results until the mid-fifties when interest in high-speed propellers had almost disappeared. Three propellers were eventually tested at flight speeds up to slightly above Mach 1 on the XF–88B (fig. 36). By the time the results were analyzed in 1957, the Subcommittee on Propellers for Aircraft had been disbanded, eliminating a main heading on this subject in the NACA Annual Report. Thus, we find in the 1958 (Final) NACA Annual Report only an obscure reference to these interesting data, the crowning achievement of a difficult and costly project, under the heading "Low-Speed Aerodynamics." Peak efficiency of 80 percent had been measured at Mach 0.95 on a thin "supersonic" propeller, generally confirming the levels indicated in the Langley high-speed wind tunnel programs (ref. 162).

COMMENTARY

Taken as a whole, the high-speed propeller program is clearly one of the more substantial NACA contributions. The magnitude of the undertaking was well beyond anything that might realistically have been expected from private industry, and this was another example of NACA fulfilling its proper governmental function.

There were occasional noteworthy flashes of inspiration. The impressive blade pressure distribution surveys afford perhaps the best example. Several innovative developments had to be brought together to make these measurements possible. These unique data still have been only partially analyzed and remain available to enterprising future researchers.

Like several other NACA programs, high-speed propellers has its mythology. Evidence of this can be found, for example, in the writings of G. W. Gray whose book (ref. 163) states, "Almost everything in the way of improving propeller efficiency for high-speed flight rests on the utilization of the 16-series airfoils." The principal source of his education in high-speed propellers and the NACA reviewer of this material was

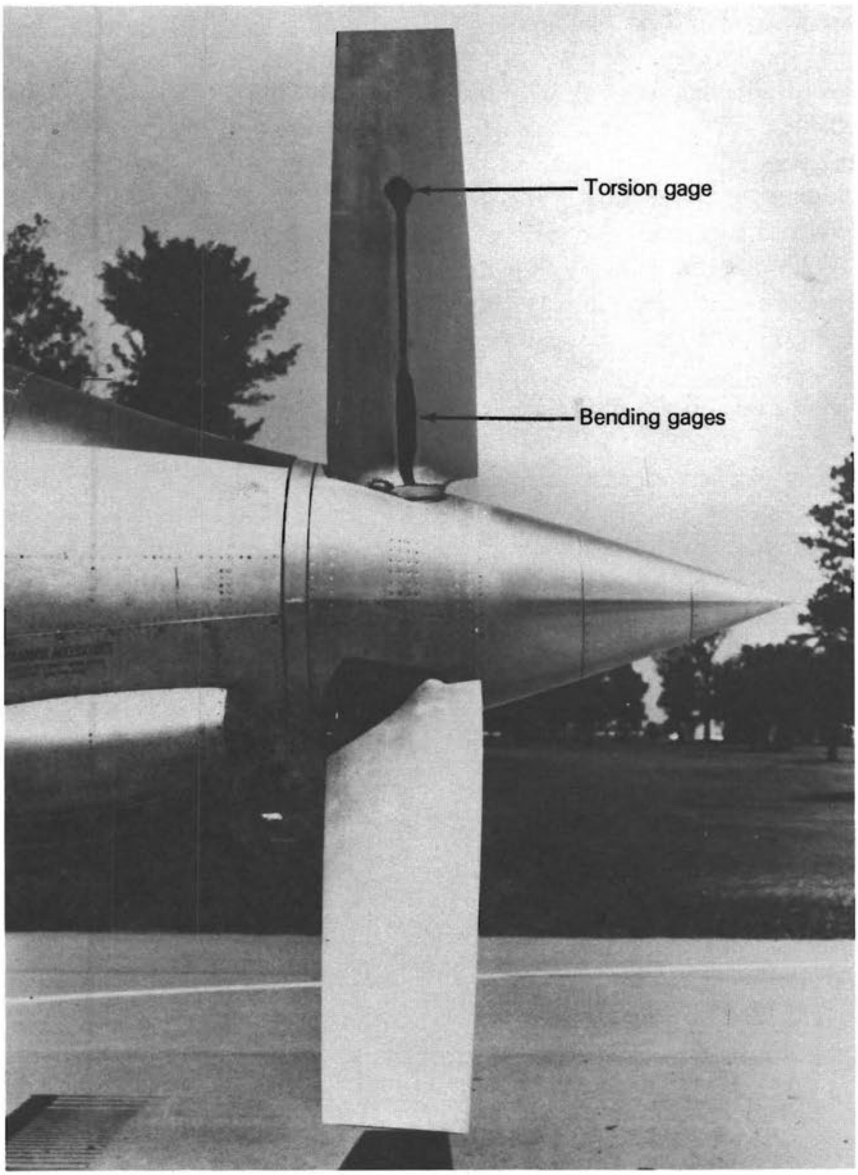


FIGURE 36.—*Supersonic propeller (feathered) driven by T–38 turboprop engine mounted on XF–88 propeller research airplane.*

Stack, a man who had outstanding talent for technical salesmanship. Any qualifications or words of caution which Stack probably included were undoubtedly lost in his effusive account of how NACA had created efficient 500-mph propellers. He often used the term "16-series" to encompass all the improvements embodied in the NACA propellers, including the all-important reduced thickness ratios, a usage which Gray evidently misunderstood.

The progress in high-speed propeller technology made in the NACA program took place in an environment of dwindling user interest. In 1949 T. B. Rhines of the Hamilton Standard (Propeller) Division of United Aircraft complained poignantly that ". . . various representatives of the aircraft industry imply that even if the [high-speed] propeller is good it is not wanted" (ref. 155). There was still hope that 500–600-mph transports might need transonic propellers, especially for long range, but with the advent of the Comet and the 707 this application also faded and the NACA high-speed propeller program ended with the transition to NASA.

The oil crisis is now forcing new considerations of high-speed propellers, and we may see a renaissance for both military and commercial applications.

CHAPTER V

High-Speed Cowlings, Air Inlets and Outlets, and Internal-Flow Systems

These high-speed programs were superimposed on the low-speed foundations built up in the 1926–1936 decade. An understanding of the earlier work is important in reading this section and, therefore, the pertinent background will be reviewed briefly.

The exposed radial engine had been in use only a short time before designers began to be concerned about its drag and cooling problems. The most rudimentary knowledge of aerodynamics suggested some kind of rounded fairing to cover the engine. The first "cowling" of this kind was designed in 1922 by Col. V. E. Clark of "Clark Y" airfoil fame, for the Dayton-Wright XPS–1 airplane which was powered by the first Lawrance radial engine. Cooling problems were encountered and the lack of understanding of how the cowl worked and how much drag it saved discouraged others from using it (ref. 43). A successful foreign application of a cowling which completely covered a 50-hp air-cooled engine was made by one Piero Magni in 1926 (ref. 164). This well-shaped cowling employed a "blower-spinner," which provided satisfactory cooling. The design must have had very low drag but no data were given. The "blower-spinner" was rather obviously too great a complication to be considered for the large radial engines of the twenties.

By 1927 the drag problem of radial engines was widely perceived as very serious. Several requests were made by attendees at the 1927 Engineering Conference to make a full-scale engine/cowling investigation the first work to be undertaken in the new PRT, which was then nearing completion. NACA fully concurred, having already in hand a request from the military for cowling work (ref. 165). Fred E. Weick, who had been hand-picked by G. W. Lewis to design and manage the PRT, laid out a tentative test program and diplomatically submitted it to industry for comment and suggestions. As finally agreed upon, the program reflected the great concern of the time that cowlings might seriously inhibit engine cooling. Five of the seven shapes to be tested on a representative cabin fuselage with a Wright J–5 engine did not completely cover the cylinders; only one complete exterior cowling was designed, and it was tested with two inner-body shapes (ref. 166).

Perhaps the most important single test was the first run with the fully exposed J–5 engine. The simple scales of the PRT registered 85 pounds increase in drag due to the engine at 100 mph. For typical J–5 powered airplanes of that day this meant that up to as much as 30 percent of the engine power and fuel was being expended simply to provide cooling. There was now the strongest possible motivation to find an effective cowling.

Only the complete exterior cowling produced a really dramatic reduction in drag. Weick's test procedure was to make cut-and-try changes until the engine temperatures approached those of the fully exposed engine. Only two important changes were made in the original complete cowl—the exit area was increased severalfold by cutting 3 inches from the skirt and the inlet diameter was increased from 24 to 28 inches. Although the cooling was finally judged adequate, the barrel temperatures were still some 60° F hotter than for the exposed engine. After these changes to favor cooling had been made the drag was measured carefully and found to be 60 percent lower than that of the uncowled installation (ref. 166).

In retrospect there was nothing very remarkable about the cowling itself—an arbitrary, rounded external fairing tailored by straightforward cut-and-try changes to favor engine cooling. The underlying achievement was NACA's creation of the PRT, which made it possible for the first time to work with a full-scale engine/propeller/cowling in a wind tunnel and measure precisely the drag and cooling data. NACA was not the first to lay out the cowling and did not "discover" it in the usual sense of that word. They did discover its enormous drag-saving potential. The basic internal flow processes and the cooling mechanisms of the radial engine still remained obscure at the conclusion of the J–5 installation testing in the PRT, in spite of the fact that tolerable cooling had been achieved by crude methods involving what was later recognized as a large excess of internal airflow and internal drag. Some of the first industrial applications of the cowling were less successful in solving the cooling problem, and employed very large exit openings to try to encourage cooling flow. In several cases the external drag advantage was apparently nullified by the huge internal drag of the large cooling flow. Lack of understanding of these internal drag effects caused much puzzlement. Rex B. Beisel of the Vought Company described the situation in his classic 1935 paper (ref. 167): "The first five years of industrial experience with the NACA cowling has brought forth a maze of contradictory data . . . leading to confusion and suspicion."

Added to the real problems was at least one largely illusory one, the belief that the cowling would seriously impair the pilot's vision. The surviving pilot of an Army midair collision had claimed that the cowling had blocked his view. A consequence was the Army's adoption of the Townend Ring for its P–12 and P–26 airplanes (ref. 41), and this in turn led to NACA's decision to flight-test several truncated cowlings on a Curtiss XF7C–1 airplane. It was clear in NACA's tests that the visibility issue had been overplayed and would not exist at all in many cases (ref. 168). Aside from this useful result these flight tests illustrated the primitive state of knowledge in 1929: the cowls were tested and compared with different exit areas and thus different internal drags. Furthermore, shutters were used in front of the engine to control part of the cooling flow, subsequently known to be one of the least efficient flow-control techniques.

The lack of a solid framework of understanding is evident also in other programs of the early thirties (ref. 169). The basic cooling problem was visualized as how to divert or deflect a part of the cooling-air flow toward hot parts of the engine. This was consistent, of course, with the excessive cooling flows that characterized the early installations. In this concept a great deal of effort was expended on "deflectors" of the type tried by Weick. The curved "shell" baffle emerged from this work; however, it was conceptually more a refined deflector than the ultimate tight-fitting baffle which actually contacted the fins and formed an outer wall for the finned heat transfer channel. As late as 1935 the loose-fitting baffles were still being tested by NACA (ref. 170) in spite of the fact that by that time the work of Vought and Pratt and Whitney had evolved the "pressure-baffle" concept. A NACA investigation of a tight-baffled cylinder was finally carried out in 1936 (ref. 171). Useful NACA work relating to fin design and fin spacing was also carried out in the thirties.

The cylinder cooling work was one part of a three-pronged program often described in agency literature in the 1931–1934 period; the second part was aimed at finding the best cowling shape, and the third was verification of the concepts developed in parts 1 and 2 by tests of an actual cowled engine in the PRT. An R–1340 Wasp engine was borrowed from the manufacturer in 1932 for part 3. This program ran into difficulties in each of the three areas and was never completed as planned. Contributing factors were the loose, inefficient, shell-type baffles employed in the engine which were obsolete before testing was completed, the fact that only the climb condition of engine operation could be simulated in the 100-mph PRT, and the use of a very short nacelle on which the flow was prone to separate from the afterbody so that much of the cowling drag data were useless. In the report of the engine tests finally issued in 1937 (ref. 172) the negligible differences in engine cooling in climb with the four different cowlings is shown, but nothing is concluded in regard to the relative drags of these cowlings, and no reference is made to the initial ambitious objectives of the program.

During the 1931–1934 period the Vought group under R. B. Beisel conducted a program of wind tunnel and flight investigations of cowling and cooling problems which provided definitive enlightenment on the key issues. They established the high cost in drag of the large excess of cooling air that flowed through unbaffled and loosely-baffled engines, and aided by Pratt and Whitney, they evolved the idea of "pressure baffling" in which all flow through the engine is blocked except through tight-fitting baffles in contact with the after-quarters of the cylinders. They invented cowl flaps to vary the size of the exit opening and so to regulate efficiently the airflows to the minimum value required for cooling in each condition of flight. The internal flow system of the NACA cowling was at last understood and criteria for efficient engineering design and operation were established (ref. 167).

Beisel's group had periodic contact with the Langley finned-cylinder and PRT work, but they credit British studies, particularly those of Pye (ref. 173), as a principal source of their inspiration. In 1934, Langley was called on to test, in the Full-Scale Tunnel, the first double-row engine installation with Vought's pressure-baffle system. The first NACA references to this system are found in the 1934 Annual Report. Vought was able to demonstrate that a properly baffled cowled engine had lower operating temperatures than the fully-exposed engine—an accomplishment believed to be physically impossible in the early years of cowling development.

In 1934, G. W. Stickle and M. J. Brevoort who had been working on the R–1340 cowled-engine/nacelle tests in the PRT were transferred to T. Theodorsen's group. In reviewing a prospective report covering their PRT work, Theodorsen had pointed to the blunt nacelle afterbody as the probable source of the drag anomalies that had been observed, a perceptive speculation that was later verified. Theodorsen's interest in the many problems of cowling and cooling was now aroused and he supervised plans for a comprehensive new investigation. NACA's unfortunate experiences with the R–1340 tests together with Vought's recent successes provided a framework for better planning. Following Vought's lead, a wind tunnel model employing a dummy engine was used. However, one cylinder was heated electrically so that cooling tests could be made, and a propeller with a 150-hp electric drive was included. Complete pressure distributions and smoke flow studies were very important special features of the program.

This solid full-scale investigation answered virtually all the remaining questions (ref. 174). The Vought findings were corroborated and extended importantly. It was found that large-scale turbulence, induced more or less automatically in the front of the cowling, was the mechanism that cooled the front of the cylinders. The internal flow was analyzed by evaluating the efficiency of the cowling considered as a pump. Unfortunately, the unbaffled engine showed the highest pump efficiency; however, it also had by far the highest pumping power requirement due to its large air flow. The tightly-baffled engine had the lowest pump efficiency but also the lowest power absorption because of its low flow. The cooling drag penalties were consistent with the Vought results. Regrettably, no reference is made to Vought's work in the NACA reports.

By the end of 1936 it was obvious from the Vought and Langley investigations that the radial-engine cowling had virtues that were unsuspected in the beginning when it was thought of only as a device for external drag reduction. With proper design the cowling would enhance cooling rather than inhibit it as originally believed. Through physical containment of the internal flow, the cowling made the enclosed baffled engine in essence equivalent to a ducted radiator as far as cooling was concerned. By application of basic heat transfer principles the overall drag-power cost of cooling the radial engine had now been reduced to low levels, comparable to those of the liquid-cooled engine.

After 1937 and continuing through World War II, there were many NACA contributions to cowling and to fin and baffle design. The so-called "cooling correlations" were evolved relating engine operating conditions, fin temperatures, and cooling flow requirements. With the framework of basic understanding now firmly established, these later contributions were for the most part sharply focused and valuable. We will be concerned in this chapter only with the high Mach number projects relating to the NACA cowling. Of greater long-term significance were the high-speed investigations of generalized inlet, outlet, and internal flow systems, with which this chapter is chiefly concerned.

HIGH-SPEED COWLINGS

The NACA cowling was an obvious subject for research in the new 8-foot high-speed tunnel in 1936. It was the center of much attention in the NACA program and the pressure distributions obtained in the PRT by Theodorsen's group revealed local velocities as high as twice the flight speed for the blunter shapes. Using Jacobs' criterion, we estimated that the critical speeds would be as low as 300 mph at altitude, well within the performance spectrum of pursuit aircraft of that period (ref. 175). R. G. Robinson planned a high-speed program which would start by testing five of the cowlings used in Theodorsen's program and then proceed to develop improved less-blunt shapes. I was fortunate to be project engineer on the cowling investigation, my first substantial project assignment.

During design of the cowl models I had noticed one of the then-new

DC–3's parked on the Army flight line. It had a very blunt cowl shape and a large fixed exit opening. I picked up some straightedges and clamps from our shop and walked over to the flight line where I found a sergeant servicing the DC–3. He located a ladder and helped me set up my equipment to obtain accurate profile ordinates at several stations on the DC–3 cowls. They turned out to be intermediate to two of the blunter Theodorsen cowls and were therefore not included in our test program. A couple of years later, however, we used them in our DC–3 test project (ref. 31). This is a small illustration of the direct informality with which things were done in those days.

Tests of the blunt cowlings confirmed our low critical-speed estimates, and showed prohibitive drag increases beyond the critical speeds (ref. 175 and fig. 37). They also provided the live demonstration for the 1937 Engineering Conference previously described (page 26).

The next phase of the cowling work provided my first experience at Edisonian tailoring of an aerodynamic body in an effort to obtain a particular pressure distribution. The basic difficulty in subsonic flow

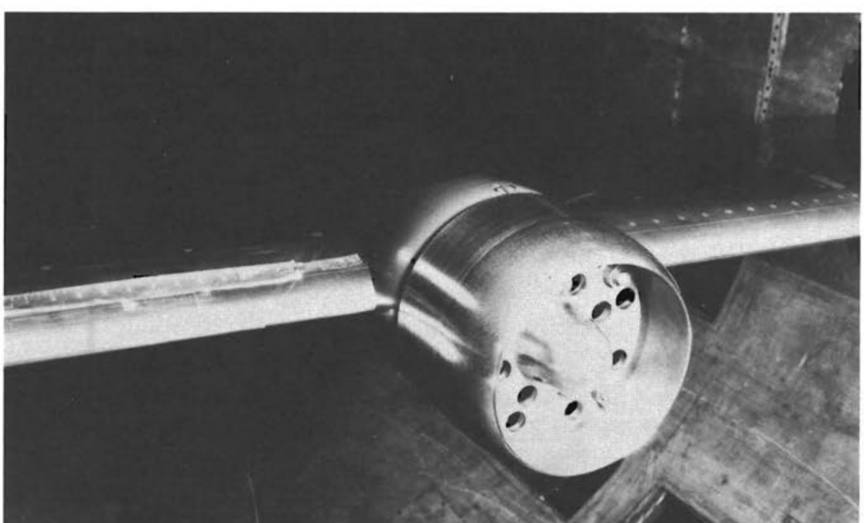


FIGURE 37.—*High-speed cowling test setup in the 8-Foot High-Speed Tunnel, 1937.*

is that a change in local shape and pressure also causes usually unwanted changes elsewhere on the body. After testing a given profile, I would lay out a speculative change, carry the model back to our machine shop, and often help the machinist in laying out a new nose profile template. When the new shape had been tested and the pressure distribution studied, the process was repeated until eventually we obtained the lowest suction pressure peak possible within the dimensional limits established for the cowling study. These limits were set by the specified diameter of the inlet opening and the distance between the cowl lip and the plane of the engine face which were typical of advanced engines. The resulting high-speed shape, designated cowling "C," had a critical Mach number of 0.64 or 480 mph at sea level, about 170 mph faster than the blunt cowls. The "C" cowling also had somewhat lower drag at subcritical speeds than the blunt shapes and it found use on a wide variety of piston-engine aircraft, including several where the high critical speed was not required.

HIGH-SPEED AIR INLETS AND OUTLETS

While the 8-foot high-speed tunnel was shut down from October 1937 to March 1938 after the drive-fan accident described in Chapter II, page 26, I was assigned temporarily to the Atmospheric Wind Tunnel (AWT). At first I feared this would be time lost but it turned out to be very fortunate.

The AWT was noted for its vast but rather uninspiring production of low-speed test data and it had accumulated a staff which included several older individuals who seemed content with this type of work. However, off in a corner of the building a bright-eyed young engineer was engaged in a special investigation of what were then called "scoops and vents" (later known by the more dignified name "auxiliary air intakes and exits"). F. M. Rogallo had acquired a better-than-average understanding of propulsion theory from the extensive propeller research carried on by Durand and Lesley, his teachers at Stanford. He was now making good use of this background in setting up a meaningful theoretical framework for the scoop-and-vent investigation (ref. 176). I was assigned to work with Rogallo on this interesting project. Of special interest to me was a part of the analysis which could be applied to the internal flow system of the NACA cowl. It showed that the drag power expended by the airplane to propel an internal flow system was always significantly greater than the "pumping power" required to force the internal flow through the cowling as considered in Theodorsen's cowling analysis (ref. 174), the difference being the power represented by the velocity of the wake. It was obvious to me almost at once that direct calculation of the internal drag of the cowling system from the pressure loss data would have been much more useful than the pumping power of the PRT cowling reports, in which the drag due to the coolant air flow could be found only indirectly by analysis of the total drag measurements. Rogallo had shown that a previous outside study of scoops and vents (ref. 177) also suffered from analytical flaws. Nearly all of these small openings were found to have high drag coefficients, especially if uncontrolled intake or exhaust of air was involved, and it was clear that aircraft of that day, many of which carried a multiplicity of these small openings, were paying a severe penalty.

In mid-December, I was asked to work with Abe Silverstein in the Full-Scale Tunnel on boundary-layer measurements for a family of full-scale wings. I learned a great deal about turbulence, transition, and hot-wire techniques which was used repeatedly on my return to 8-foot (ref. 85). Furthermore, collaboration with Silverstein was an interesting education in itself (ref. 178).

Eastman N. Jacobs paid an unexpected visit to our office in the 8-foot tunnel early in 1939, shortly after Stack had become Section Head. He was in the early stages of the Campini system investigation (Chapter III, p. 68ff.) and was concerned about how best to design an inlet at the fuselage nose to handle the large propulsive airflow. None of the performance numbers had been firmly fixed, but a flight speed on the order of Mach 0.8 or higher was contemplated. I described our cowling work leading to Cowl "C" with its critical speed of Mach 0.64. Industry engineers I had talked to previously were delighted with Mach 0.64, which in all cases had been well beyond their level-flight speeds. But here was a man who wanted Mach 0.8 plus! Jacobs was also hoping for substantial runs of low-drag laminar flow over the fuselage forebody, another requirement which seemed to me then to be impossible. We had never been able to avoid a suction pressure peak at the nose of the cowls and this, it seemed to me, would trigger transition. I mentioned the turbulent pulsations found in the cowlings in the PRT; these also would tend to prevent laminar flow. Jacobs fidgeted with characteristic impatience at these objections. He noted that the Campini system had no propeller and thus a basic requirement of the cowled engine—that the plane of the propeller be close to the face of the engine—did not apply. There was no limitation either on the size of the inlet opening as there had been in the cowl work. In principle, it should be possible to improve on Cowl "C." The same approach which improved the critical speed should also favor longer runs of laminar flow. As Jacobs departed, he said that his colleague Ira H. Abbott had developed a family of streamlined body shapes with falling pressures back to their maximum thickness stations. He would send Abbott to talk to me about using one of them in high-speed tests to develop an inlet for the Campini application.

Discussing the problem with Abbott I learned that he was dubious about the Campini system, but he argued that critical speeds beyond that of Cowl "C" would eventually be needed for more orthodox radial-engine installations. We chose a basic body shape from Abbott's family having a fineness ratio of 5—more representative of a radial-engine nacelle than a Campini fuselage. It was obvious that inlet velocity would have a large effect on inlet performance and critical speed and this suggested an inlet size substantially smaller than the cowling inlets; a diameter half that of the "C" cowl was selected.

By now it was quite clear to me that the dimensional restrictions of the current radial engines which we had arbitrarily imposed in developing Cowl "C" were artificial and undesirable. As this mental roadblock was dispelled, my imagination expanded and I began to think in larger terms. Suppose all restrictions were lifted and the question was phrased in the broadest possible terms, "What is the most drag-effective way to ingest or expel air into or out of a streamlined body at very high speeds?" To answer this question, the investigation would have to be greatly broadened. I felt a mounting enthusiasm at the prospect of contributing fundamental new knowledge. Inlet size was made a primary variable. Two types of outlet opening in various sizes were also selected (fig. 38). Both the cowling work and Rogallo's tests had indicated that interference effects existed when inlets and outlets operated in combination, which were usually indeterminable; to avoid this problem, I would test all the inlets and exits separately. This meant that the ingested air would have to be removed by a large blower, or, in the case of the outlets, supplied by a blower. The blower offered an additional feature of great importance. It would ensure that the very high velocity ratios we desired would actually be attained. The blower system was a large complication requiring a flexible seal for drag measurements and careful evaluation of airflow momentum changes in analysis of the results. My experience with Rogallo's setup was valuable in designing this equipment. Jacobs' inlet test, which was now only a detail of the investigation, would be delayed a couple of months to allow for design and procurement of the more elaborate equipment.

After a week of "shakedown" and learning how best to conduct the testing with the rather complicated blower system, we were ready to start testing Nose B, the intermediate inlet sized for Jacobs' application, in August 1939. The tailoring process proceeded more easily and quickly than in the cowling work, partly because we were using wooden models. The final optimized profile provided exciting performance. The suction pressure peak that existed at low inlet velocities disappeared completely at velocity ratios greater than about 0.2 (fig. 39), and the critical speed thus became that of the streamline body itself, Mach 0.84 for our particular rather fat body. At the inlet velocity ratio for disappearance of the pressure peak, the transition point jumped rearward to the same location observed for the basic body. And so in less than nine months since his initial visit, we had provided Jacobs with an inlet fulfilling all his ambitious requirements.

Analysis of the drag results revealed an unexpected dividend: the external drag with combinations of the optimized inlets and outlets did not exceed the drag of the basic streamline body, and in some cases was significantly less. All previous work with the NACA cowlings had shown substantial increases in drag; the summary recommendations from the PRT programs suggested a drag coefficient increment of 0.033 for good cowls (30 to 60 percent of typical streamline nacelle drags). Our largest inlet, which was of NACA cowl proportions, added only about one-fourth the PRT value.

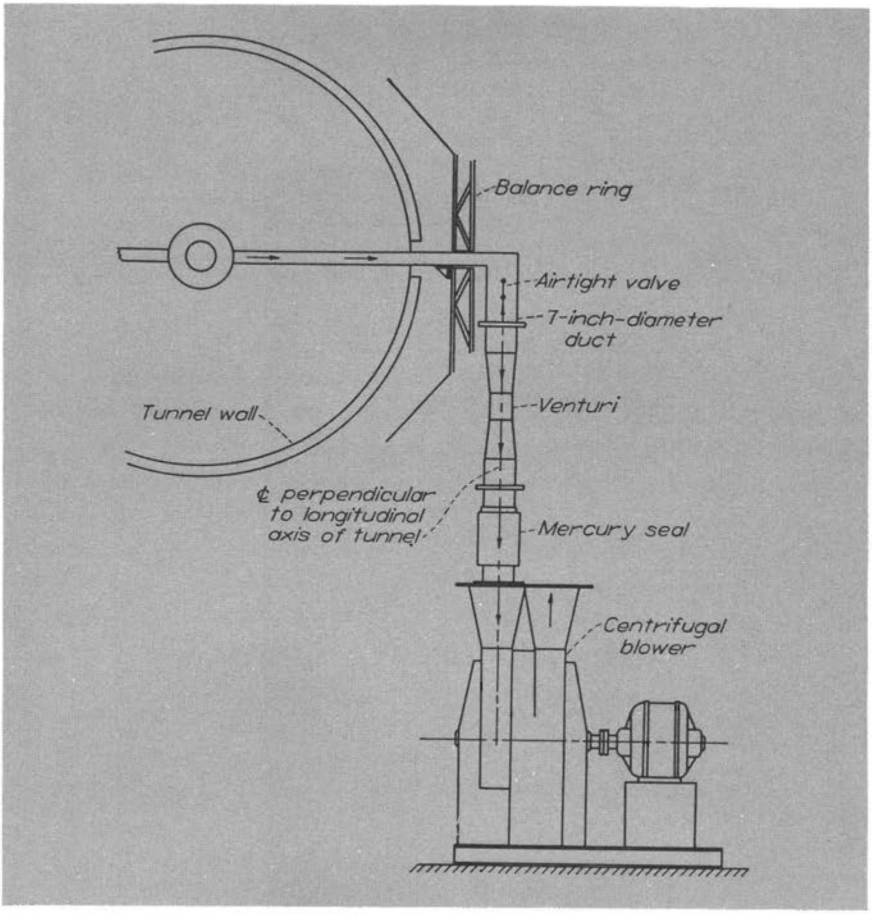
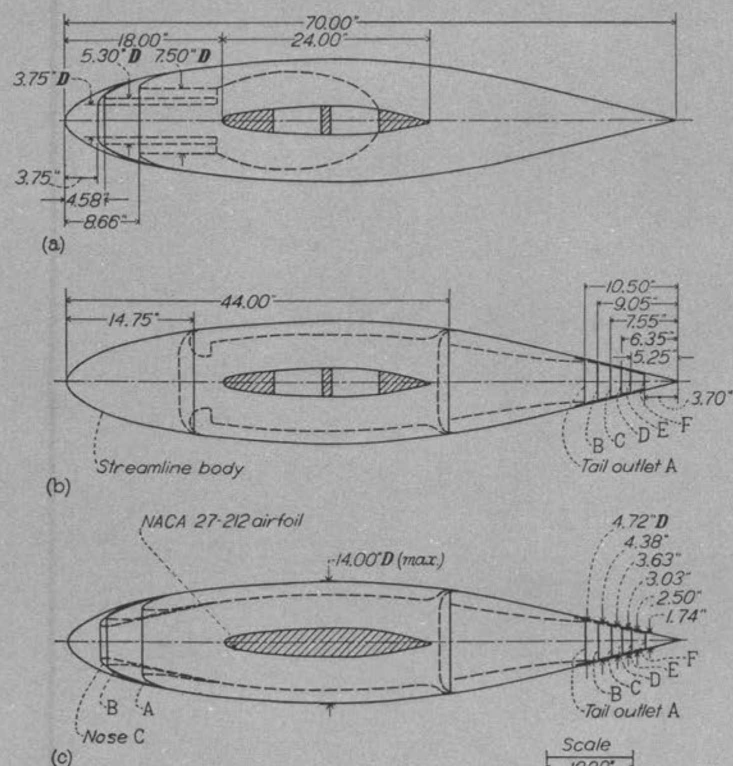


FIGURE 38.—*Blower installation in 8-Foot High-Speed Tunnel for investigation of high-speed air inlet and outlet openings, and the principal shapes tested.*



(a) Inlet openings.
(b) Outlet openings.
(c) Inlet-outlet combinations.

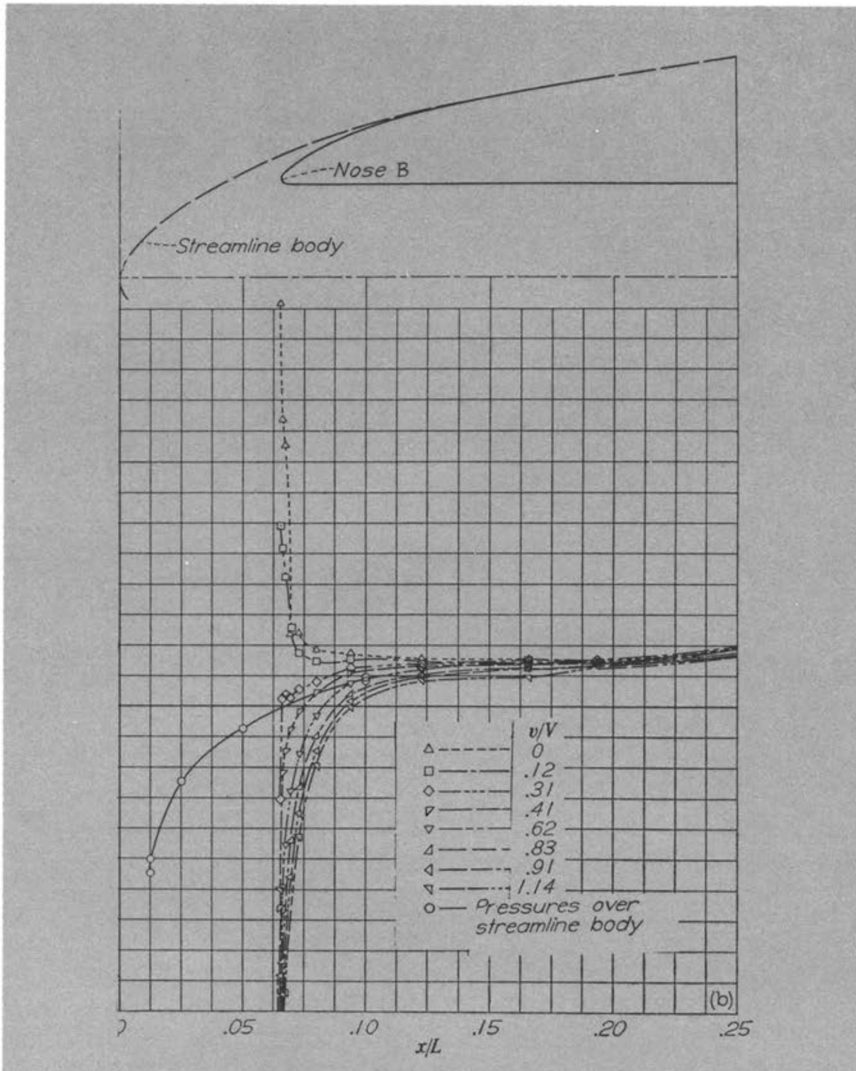
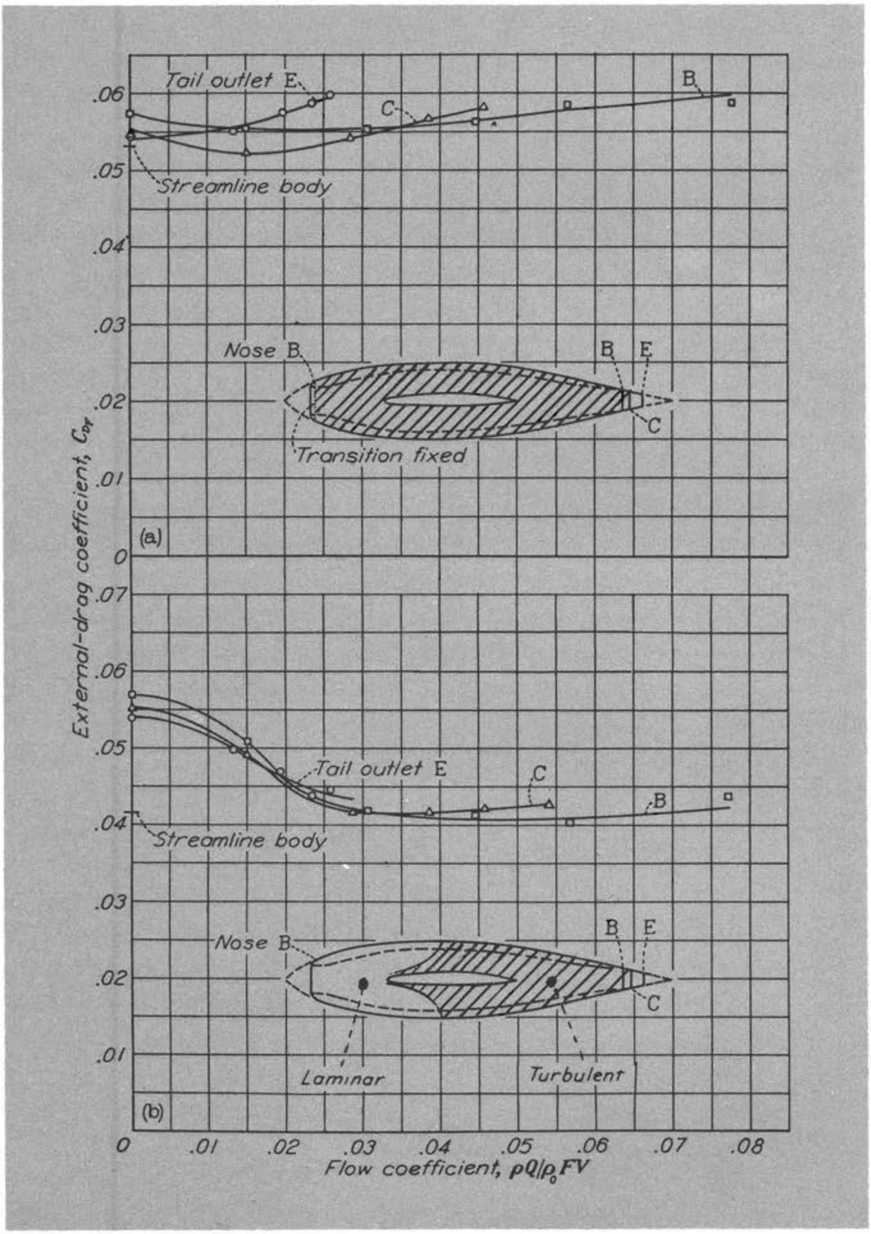


FIGURE 39.—*Typical drag, transition, and pressure data from inlet-outlet investigation.*


Tail outlet E
B
C
Streamline body
Nose B
B
E
C
Transition fixed
(a)
External-drag coefficient, C_{D_F}
.06
.05
.04
.03
.02
.01
0
.07
Tail outlet E
C
B
Streamline body
Nose B
B
E
C
Laminar
Turbulent
(b)
0
.01
.02
.03
.04
.05
.06
.07
.08
Flow coefficient, $\rho Q/\rho_0 FV$

Another notable discovery was made during analysis of the profiles of the optimized nose shapes. When they were "stretched" analytically to a common length and depth all three had nearly the same profile. The Nose C and B contours were identical within 1 percent of their average ordinates. This implied that an infinite family of optimal nose shapes could be derived from the contours established in these tests. Designers could select the correct shape for their dimensional requirements without the need for any additional testing and development (ref. 179).

My instincts as an aeronautical engineer urged immediate exploitation of these impressive inlets and outlets in aircraft design studies. The Campini system which had triggered the investigation was an obvious application, but at the time it seemed quite remote and doubtful except perhaps to the Jacobs group. To me, the most likely near-term application was a submerged radial engine driving a pusher propeller. In my original report (ref. 179), I had suggested that the nose inlet supply all of the air requirements for such an installation—carburetor, oil cooling, engine cooling, intercooling, and aircraft ventilation; there would be no drag-producing auxiliary inlets. Both Rogallo's work and the first "drag clean-up" studies of actual aircraft in the full-scale tunnel provided alarming evidence against the use of a multiplicity of small scoops and vents. We proceeded at once with layouts of hypothetical military aircraft employing our new openings.

I had acquired a new colleague in late 1939 in the person of D. D. Baals, freshly out of Purdue. In due course, we worked as a team on several inlet-outlet/internal-flow projects and I found the association to be both profitable and more enjoyable than working alone. One of Baals' first assignments was to design a fighter-type submerged-engine fuselage employing the new openings (fig. 40). This involved considerable stretching of the Nose B profile as recommended in my paper. Baals found that a reference length extending to the maximum diameter station was more convenient than the one first suggested, and this was adopted thereafter. We built and tested a model of the submerged-engine "air flow" fuselage, with gratifying results. All aspects of the new inlet and outlet technology were confirmed (ref. 180).

An important interface between NACA researchers and industry propulsion specialists and layout men was the Power Plant Installation

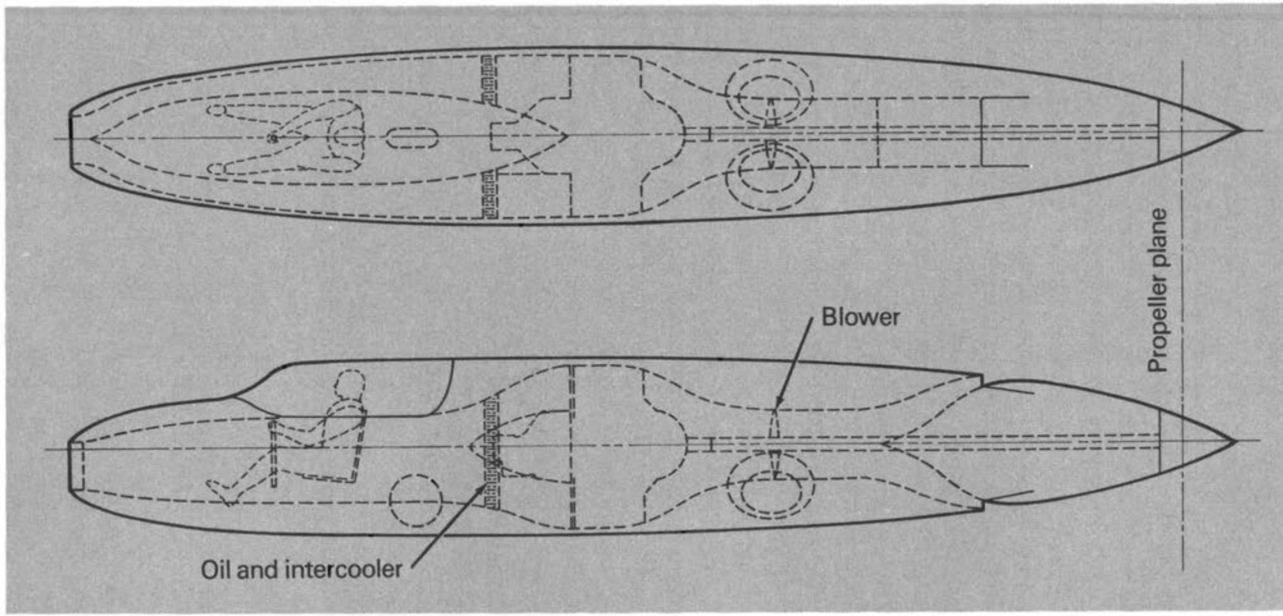


FIGURE 40.—*NACA concept of submerged radial-engine fighter employing Nose B high-speed inlet, 1939.*

(PPI) group set up at Langley about this time. Organized with the help of the Army Air Corps Liaison Office at Langley in 1940, industry engineers were temporarily assigned to Langley where they pursued advanced installation work, with NACA researchers giving advice on the use of the latest research findings. The group was headed by C. H. Dearborn who frequently called on Baals and me for data and consultation related to the high-speed aspects of cowling, airflow fuselage, and nacelle design. The first company-proposed tentative submerged radial engine installation to appear, after our work had been published, was a pusher-propeller design for the XP–59 incorporating an R–2800 engine in a 22-foot-long nacelle. We provided the design of a 20-inch diameter version of Nose B, and made both internal and external drag calculations, including an estimate of the effect of waste heat recovered as thrust. Among other aircraft installation studies for which we provided similar aid were the B–24D, XB–33, and XB–36.

The era of the submerged radial engine was short-lived, as interest shifted suddenly to jet-engine installations. Following our work on the XP–59 submerged R–2800 nacelle in the spring of 1941, there was a great silence from the Bell Company and the Army as to the progress of this project. Actually the XP–59 had been selected in mid–1941 to become the first U.S. jet-propelled airplane, but such absolute secrecy had been imposed by General H. H. "Hap" Arnold that NACA was not allowed to participate in this project until it was reclassified "Confidential" in 1943 (ref. 41). Our simple high-speed inlets and outlets were ideally adaptable to jet-engine installations, and the submerged-engine fuselage arrangement we had developed for the radial engine (ref. 180) became a popular arrangement for jet aircraft. Among the first were the Navy XFJ–1 and D–558–1 and the Air Force P–84 and F–86. The jet nacelle or "pod" also afforded an almost ideal application, a recent example being the C–5A. After the first decade of jet aircraft, as speeds moved upward into the supersonic region, both the inlet and jet exit problems developed new complexities involving variable geometry and integration with other features of the airframe which have been the subject of much additional research and development beyond the scope of this review.

There were two other ways of adapting our high-speed nose inlets to radial-engine/propeller installations. The so-called NACA "D" cowling (ref. 163) employed a very large spinner, in part to cover the inefficient hub sections of the propeller, and in part to permit high inlet velocities with their resultant benefit in high critical speed (ref. 163). The contours of both spinner and cowl sections could be derived from our stretched Nose B ordinates. Only a few piston engine installations of the "D" cowl were flown, but it found important later applications in turboprop aircraft.

The other alternative way to use our high-speed inlets in tractor propeller installations was the NACA "E" cowl (ref. 163). (These baptisms had been adopted by the PPI group in 1941.) In this design, the nose inlet lines were extended forward through the plane of the propeller, necessitating a large open-nose spinner. Our purpose was primarily to obtain low drag and high critical speed, but secondarily the hollow spinner offered the possibility of pumping cooling air if it were equipped with appropriate fan blades. This latter possibility had been the prime objective of an earlier test of a "blower spinner" in the PRT (ref. 181). Unfortunately, the PRT model was so crudely designed and constructed that the tests had little meaning. The blower efficiency was on the order of 50 percent and inspection of the external shape suggests a low critical speed. This PRT project is incorrectly said to be the origin of the blower spinner and the "E" cowl in ref. 163. Actually, the first blower spinner was developed in 1926 by Magni (ref. 164), and the "E" cowl originated from the 8-foot-tunnel program in 1940. The first investigation of a correctly designed "E" cowl was the work of McHugh in the 19-foot pressure tunnel in 1941 (ref. 182). This was the same design used later in our emergency propeller program in the 8-foot tunnel (Chapter IV, fig. 30). A number of taxing design problems were solved in developing the "E" cowl, one of them the problem of the spinner-body juncture. Several of our engineers favored some sort of sliding seal which was very difficult mechanically. We solved the problem by contouring an open juncture to serve as an efficient outlet for the leakage flow. It was necessary to test this design to convince several skeptics that it involved only negligible drag and pressure losses (ref. 183). The "E" cowl had generally excellent performance but it never found an aircraft application because of its mechanical complexity and vulnerability, and because the advent of the jet eliminated propellers for most high-speed aircraft.

After I left the 8-foot tunnel in July 1943, Baals continued to work the problem of applying our "universal" Nose B profile to a variety of design situations. He sensed the desirability of making it easier for industrial designers to arrive at optimal configurations. With assistance from N. F. Smith and J. B. Wright, he spelled out a system for deriving "NACA 1-series inlets" and produced an appropriate identification code. Some 15 illustrative inlets were laid out and selected inlets were tested to prove the validity of the "stretching" process. Design charts were prepared which made the selection process virtually foolproof (ref. 184). This work gave identity and visibility to the NACA high-speed inlets which would otherwise have been lacking. This system of design has been successfully applied not only to simple nose inlets, but also to scoops, wing inlets, circular inlets, and even to spinners in the "D" cowl. Their performance has also proved acceptable in some cases at supercritical speeds extending above Mach 1 (refs. 185, 186).

COMMENTARY

Like the results of the original NACA cowling tests, the advances achieved in this investigation were there waiting to be discovered and evaluated accurately. There was nothing remarkable in the testing and analyses, but a very important, very simple principle was involved in the initiation and planning of the project which deserves to be underscored. In the words of the first report (ref. 179), "The present investigation was designed . . . without any restrictions arising from engine dimensions, location, or air-flow requirements." There are many examples of research which could have been greatly enhanced if restrictions relating to current system concepts had not been imposed. A well-known example is the failure of the U.S. propulsion community to involve itself with jet propulsion in the years prior to 1942. Propulsion research was slaved so strongly to the piston engine because of its low fuel consumption that serious attention to jet propulsion was ruled out until the British and German achievements revealed the true potential.

The idea that a single universal inlet profile could be manipulated to fit all sorts of scoops, wing inlets, spinners, etc., and still provide optimum drag and critical-speed performance is, of course, not believable in the exact sense. What is implied in the apparent universality of the Nose B profile as applied in the NACA 1-series system is that "approximately optimum" shapes are adequate in most cases. If one were starting over today, the indicated approach would probably be theory plus the modern computer. It might prove practical by this means to derive the exact optimum profiles for each type of application.

INTERNAL FLOW SYSTEMS—EFFECTS OF HEAT AND COMPRESSIBILITY

During the course of my cowling and inlet work in the late thirties and early forties and in my first contacts with the Power Plant Installation group virtually all engineering calculations relating to internal flow and cooling were based on incompressible (low-speed) formulas. I first became involved in applying compressible flow relations in extending internal drag calculations to high speeds during my inlet-outlet opening project. It seemed obvious that before long there would be a widespread need for such refinement, and Baals and I therefore set out to develop engineering formulas likely to prove generally useful. It was obvious from the outset that the addition of heat in fins and radiators was a prime factor to be accounted for. The chief value of our engineering analysis probably was its illustration of the importance of density changes due to heat and compressibility in advanced systems then under development (ref. 187). For example, our calculations showed that the pressure drop for cooling an R–2800 engine in Mach 0.6 flight at 35 000 feet was almost 50 percent higher than predicted by simple methods then in use, which neglected the density change across the cylinders. A blower would be needed for cooling at higher altitudes, and at about 42 000 feet sonic velocity (choking) would occur at the baffle exits. These results implied a very difficult future for the piston engine, from which we were all spared by the advent of the jet engine. We felt somewhat uneasy over these predictions, because the actual flow in a baffled cylinder undoubtedly violated our basic flow assumption of one-dimensionality. Confirmation was provided about a year later in a completely independent

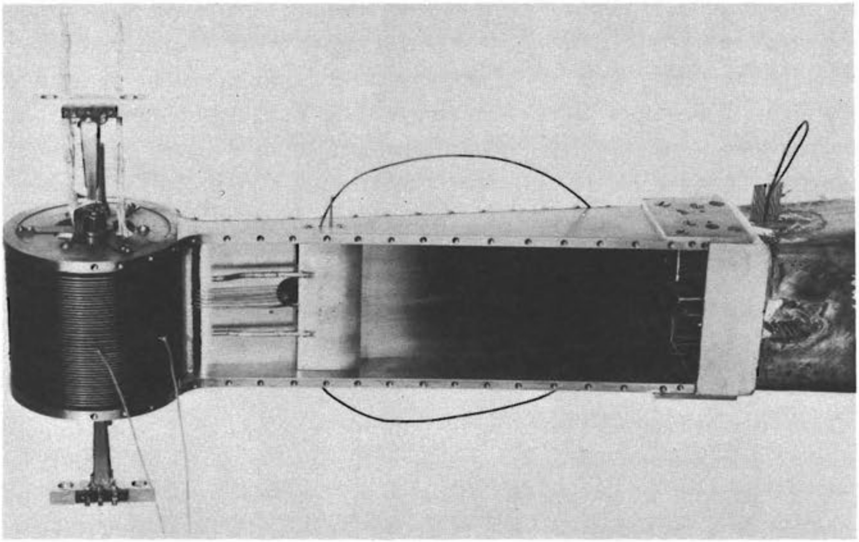


FIGURE 41.—*Electrically heated finned-cylinder model used by 8-foot tunnel group to investigate cooling-airflow relationships at high speeds.*

study by Brevoort (ref. 188), but so many simplifying assumptions had been made in both studies that we decided to undertake measurements of high-speed flows within the fins of a baffled electrically heated test cylinder (fig. 41). A few test runs were made in the spring of 1943, producing data which would require much study and analysis to interpret correctly. Under the press of more urgent business (the high-speed propeller problems previously discussed and my impending departure for the 16-foot tunnel) we set aside the heat-cylinder data, fully intending to take it up later, and not realizing that interest in piston-engine development would shortly disappear and with it our plans for future work with these data.

Closely related to the complex flow problem of the baffled cylinder was the more tractable case of high-speed heated flow in constant-area straight radiator tubes. Baals and I had applied our one-dimensional engineering procedure to this case and issued a paper outlining a simple approximate method for dealing with it (ref. 189). A few months after my arrival at 16-foot, I interested engineers Habel and Gallagher in investigating the flow in an electrically heated tube up to choking conditions. Their tests provided general confirmation of our prediction method and some added insights into the nature of these flows from the point of view of the designer of radiator installations (ref. 190). The boundary condition in this problem is the fixed assumed flow rate and entrance Mach number, and the problem is to relate the conditions at the exit of the tubes to the specified entrance conditions, the tube geometry and heat input being principal variables. Actually, we were dealing with a relatively simple special case of the much more complex general problem of heat addition in a constant-area duct, which assumed great importance in the later forties because of its relevance to ramjet combustor design. In the general case, heat addition in subsonic flow affects the upstream conditions and the resulting changes are, of course, different and more complex than those of the simple radiator. No less than 20 significant papers dealing with the general problem, several of them with conflicting and controversial conclusions, had appeared by 1950 (ref. 191).

THE RAMJET INVESTIGATION

In 1936, F. W. Meredith pointed out that the waste heat of a piston engine which is transferred to the cooling-air flow in a radiator is not all lost; it produces a small thrust provided the pressure at the exhaust of the radiator tubes is higher than the free static pressure of flight (ref. 192). This phenomenon became known as the "Meredith effect." Its mechanism was something of a mystery to many engineers of that period. A common fallacious notion was that the radial engine, because its fins were hotter than usual radiator temperatures of liquid-cooled engines, would enjoy greater benefits. (This mistaken notion still existed as late as 1949 and is stated by Schlaifer to constitute an "inherent advantage of the radial engine" (ref. 41).) The Meredith effect was so small at 1936 airspeeds that it could conveniently be neglected in performance estimates both by those who did not understand it and by those who doubted that such an effect really existed.

In our engineering analysis of the effects of heat in internal flow systems, the conversion of heat to thrust power was clearly the most intriguing aspect. Thinking in terms of flight speeds of 550 mph, we calculated ideal thermal efficiencies of as much as 10 percent, and by Mach 1.5 the heated duct would have a thermal efficiency comparable to an internal combustion engine. Clearly, the insignificant "Meredith effect" had the potential to become a primary jet-propulsion system. (The term "ramjet" was not then in general use, and we were unaware that there were several discussions of propulsive ducts in the literature starting with Lorin in 1913 and including later treatments by Carter, Leduc, Roy, and others.)

Excited at these prospects, I arranged a meeting with Langley's leading propulsion analyst at our Power Plant Division, Ben Pinkel. I also talked briefly with D. T. Williams, a young physicist whom Pinkel had recently assigned to analyze propulsive ducts at high subsonic speeds, including the effect of an engine-driven blower typical of the Campini system under study by Jacobs. Neither man showed any real hope for these systems, and Pinkel, reflecting the general attitude of most of the propulsion community at that time, patiently explained "the great weakness of all forms of jet propulsion—excessive fuel consumption compared to piston engines." When Williams' work was published about a year later (ref. 193), its primary conclusion emphasized the same point, showing an overall propulsive efficiency at Mach 0.8 on the order of one-sixth that of a piston-engine driving a propeller. Both men felt that tests of a propulsive duct in the 8-foot high-speed tunnel would be of little value. The duct and heater losses would, they speculated, largely nullify any possibility of net thrust at Mach 0.75.

In fairness to Pinkel and Williams it should be recalled that in 1940 the aircraft industry generally saw no possibility for supersonic aircraft. Mach 0.8 was regarded as a rather optimistic upper limit for the future. The potential of the turbojet for large improvements over the Campini cycle was not recognized either, and it is not mentioned in Williams' paper.

In spite of my disappointing session with Pinkel and Williams I resolved to proceed with the propulsive duct test. At the very least it would establish the Meredith effect as a major design factor at high speeds. Our 8-foot, high-speed tunnel afforded a unique tool for such an experiment. Stack solidly supported the idea. In promoting the project

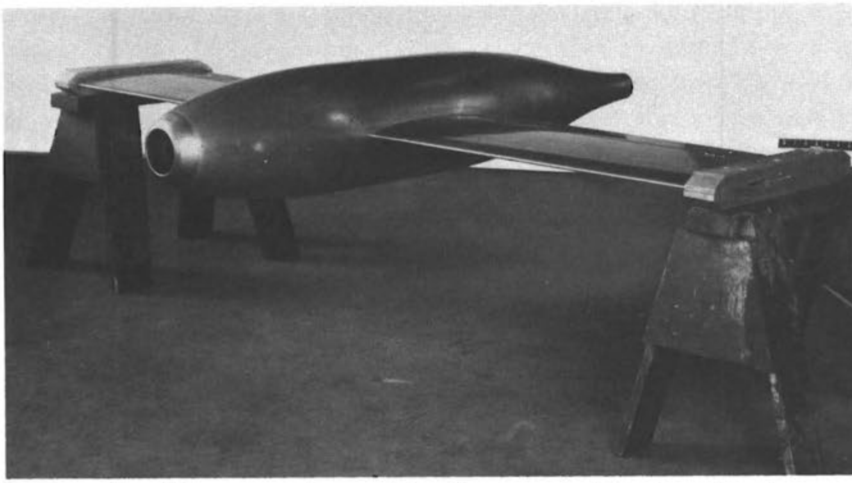


FIGURE 42.—*"Heat model" used in the first NACA investigation of a propulsive-duct (ramjet) system in the 8-Foot High-Speed Tunnel in February and March 1941. Model incorporated a 160-kw heater. Nose B and cusped outlet from ref. 179.*

we decided not to mention the jet propulsion implications in order to avoid the negative reactions of the propulsion people.

The nacelle model chosen for the tests embodied our universal Nose B shape together with our most effective cusped tail outlet (fig. 42). The all-metal nacelle was supported on a new thin metal wing selected to avoid the local area of flow separation that existed in the wing/body juncture of my inlet-outlet model. (In reviewing my original work at the request of Mr. Miller, A. M. Kuethe, who was employed briefly by NACA during the war, had endorsed my findings generally but had raised questions about possible drag interactions involving the separated flow. These would now be answered. By comparing the inlet results from the new model with the original data, we found no measurable effect of the separated flow.)

How to add heat at a high rate was our primary design problem. Combustion of fuel in the 8-foot tunnel was quite out of the question for many reasons. A search of the electrical heater catalogs with help from G. T. Strailman, Langley's principal electrical engineer, turned up no high-output heater capable of being fitted into our 11-inch diameter duct. Baals and I therefore became high-capacity heater designers and produced a 160-kw, three-phase, 1500° F heat exchanger with 32 square feet of surface area in the form of 1.5-inch-wide Nichrome ribbon woven on reinforced asbestos millboard supports. This heater produced air temperature rises of about 300° F at high speeds with very small frictional losses. The rates of heat input were larger than those due to piston-engine cooling, but still only a small fraction of the heat of combustion of kerosene.

Testing of the "heat model" started in February 1941, the first NACA wind tunnel investigation of a propulsive duct producing thrust. At a Mach number of about 0.5, the propulsive effect had become equal to the internal drag, and beyond this speed substantial net thrust was developed by the internal flow. At the highest test speed, Mach 0.75, the heated duct developed the respectable thermal efficiency of some 9.5 percent, close to the ideal theoretical value. As expected, the phenomena depended on the ratio of duct pressure to stream pressure, and was independent of heater surface temperatures per se. In all other respects, the careful measurements of these tests confirmed the calculations made by our engineering relations for analysis of this kind of internal flow system (ref. 187).

COMMENTARY

In 1941 during the period of our propulsive-duct investigations, Stewart Way, of Westinghouse, made an analysis of the subsonic propulsion possibilities of "open-duct jet propulsion," his name for what was later called the ramjet. He also apparently conducted some tests with an electrically-heated model at about the same time of our high-speed tests in February and March of 1941, although the experimental work was never published (ref. 194), and we knew nothing of Way's work until years later.

In the first version of our internal-flow-system report which was issued in September 1942 as a confidential document (ref. 189), the propulsive-duct data were included but there was no emphasis in the title or text that the first NACA tests of a potentially important jet-propulsion system had been made. Our "heat-model" tests rather definitely settled once and for all the doubts and arguments about the Meredith effect. Whether they had any impact on ramjet development is questionable. The revelation of the British and German turbojets shortly after our paper was issued had such an enormous impact that all the scattered U.S. activities in jet propulsion were in effect rendered insignificant. Almost overnight the propulsion community reversed its attitudes. By war's end, the ramjet was under vigorous development for missile applications. Both the Langley and the Lewis Laboratories of NACA had organized ramjet projects, concentrating on the prime problems of combustion and burner design which we had not been able to deal with in our 1941 project.

NACA was now being severely criticized for its prior general neglect of jet propulsion and it was clearly desirable to highlight whatever had been done. Accordingly, our report was reorganized to emphasize the tests of the ramjet system, and the words "Ram-jet System" were added to the title. The revised version is included in the 28th Annual Report of the NACA, dated 1943 but actually issued after the war.

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Appendix: List of Transonic Facilities

Transonic Aerodynamic Test Facilities (M>0.9), 1923–1950

Year	*Facility/Agency*	*Max. Useful Mach No.*
1923	*12-inch open jet*/Bureau of Standards and General Electric Co.	.92
1926	*2-inch open jet*/Bureau of Standards and Army Believed the first U.S. facility with a converging/diverging nozzle and supersonic capability.	.95, 1.08
1930	*11-inch open throat tunnel*/NACA Low supersonic speeds in some tunnel-empty tests. No published data.	1.0
1941	*High-Altitude drop tests*/German DVL	1.1
1942	*16 x 21-inch semi-open*/Guidonia First reliable airfoil data at M>0.9, using semi-open configuration to avoid choking.	.94
1944	*High-altitude drop tests*/NACA	1.3
1944	*Aircraft "Wing-Flow"*/NACA	1.3
1944	*2.7-meter (8.8-foot) tunnel*/German DVL First use of "small-model" technique to achieve M>0.9.	.92
1945	*Contoured-wall tunnel*/German, Ottobrun 7-foot tunnel with flexible wall to simulate flight streamlines at wall. Never used.	1.0

Year	Facility/Agency	Max. Useful Mach No.
1945	*8-foot closed-throat tunnel (repowered)*/ NACA Used "small-model" technique and zero-blockage support systems. First large contoured axisymmetric supersonic nozzle, M, 1.20 in 1948.	.95, 1.20
1945	*Rocket Model Technique*/NACA Higher M's achieved in later programs.	1.6
1946	*Wind tunnel bump*/Lockheed, NACA Eventually used primarily in Langley 7 x 10-foot and Ames 16-foot high-speed tunnels.	1.3
1946	*D–558–1 research airplanes*/Navy, NACA Max. M in level flight, 0.83	.98
1946	*Annular transonic tunnel*/NACA Single-blade airfoil mounted on rotor, annular channel.	1.0
1946	*X–1 research airplane*/USAF, NACA First manned supersonic flight, October 14, 1947.	1.45
1947	*12-inch model slotted tunnel*/NACA First successful slotted tunnel, used only for tunnel development.	1.26
1947	*9 x 9-inch closed or open tunnel*/NACA Used only for tunnel development.	1.1
1948	*4 x 19-inch semi-open tunnel*/NACA The facility in which Langley's systematic testing of airfoils was extended to Mach 1.	1.0
1948	*4.5 x 6.25-inch slotted tunnel*/NACA Used for slot-shaped and porous-wall testing.	1.3

Year	*Facility/Agency*	*Max. Useful Mach No.*
1949	*Twin-stream technique*/United Aircraft Corp. Inner transonic stream controlled by varying outer subsonic stream. Problems: mixing disturbances, uncertain wall corrections, poor velocity distributions.	1.3
1949	*10-foot high-speed tunnel at Wright Field*/USAF Throat air bleed and movable side-wall segments used to obtain and vary low supersonic flows.	1.2
1950	*8-foot slotted-throat tunnel*/NACA First large operational transonic tunnel.	1.15
1950	*16-foot slotted throat tunnel*/NACA Second large operational transonic tunnel.	1.08

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