Chapter 2

Building a Research Establishment

The American Way

> *The only people so far who have been able to get at something like accurate results from wind-tunnel experiments are the workers at the Experimental Station at Langley Field, which is run by the National Advisory Committee for Aeronautics of the United States of America. Thanks to the wealth of the United States and the high intelligence of those who are charged with the task of aeronautical experiments, workers in the American research establishments have acquired knowledge that is in many ways far ahead of anything we have in this country. And they have it very largely by what is called "ad hoc research,"—that is to say, going and looking for the solution of one particular problem, instead of experimenting around blindly in the hope that something may turn up, after the fashion which is known as "basic research."*

The above editorial comment by C. G. Grey, a prominent British aeronautical engineer and editor of the aviation journal *The Aeroplane*, appeared in the 6 February 1929 issue of that journal.

From the Wright Bicycle Shop to the Langley Full-Scale Tunnel

For those who followed the ingenious Wright brothers into the air after 1903, the maturation of the airplane depended on a growing and increasingly sophisticated understanding of aerodynamics, for there was still much about flight that was unknown. The route to greater aeronautical knowledge in the post-Wright era was not a straight highway. To the extent a map even existed, it offered a maze of twisting roads involving trial-and-error design of new flying machines; dogged pragmatic testing; deeper scientific inquiry; and, perhaps most importantly, a shrewd combination of the best that both theory and experiment had to offer.

Yet for the aeronautical scientists and engineers in pursuit of aviation progress in the early twentieth century, there was perhaps no surer course to progress than the one laid out by the Wright brothers themselves. Unfortunately, not everyone in the brave new world of aeronautics understood the Wrights' path to success. Many wrongly interpreted their invention of the airplane either as the heroic act of ingenious mechanical tinkerers or as a basic scientific discovery, rather than as a solid technological program of engineering research and development.

In fact, the Wrights' first successful flying machines, starting with their 1902 glider and the 1903 airplane that followed, not only represented key breakthroughs in their efforts to master heavier-than-air flight, but also bore testimony to the irreplaceable value of combining careful laboratory experiments with actual flight testing.

In 1915, twelve long years after the epochal Wright flight, the U.S. Congress established the National Advisory Committee for Aeronautics, the predecessor of present-day NASA, with the mission "to supervise and direct the scientific study of the problems of flight with a view to their practical solution." Eventually this agency recreated the Wrights' formula for success in a series of federal research laboratories.[1] The NACA accomplished this, originally, by building an extraordinary community of aeronautical engineers, scientists, technicians, and test pilots at the Langley Memorial Aeronautical Laboratory (LMAL) in Tidewater, Virginia—the NACA's first and, until 1941, only research facility.[2] At NACA Langley, the laboratory staff faced, and eventually resolved, many fundamental questions about how to plan and conduct institutional aerodynamic research. These questions dealt with issues of how research should be focused, what sort of facilities should be built, what type of experimental investigations should be carried out, and how experimentalists and theoreticians could work together fruitfully. The early NACA community also dealt with the issue of whether science or engineering should be in control of the research program, and especially whether the German academic laboratory model could be transferred successfully to America—or whether an American lab would have to find its own way. Through the resolution of these issues, in conjunction with a growing list of important achievements in aeronautics, a sustainable organizational identity for Langley and for the subsequent NACA laboratories began to evolve. The result was a dynamic and highly creative organization that, although its original facility was named after scientist Samuel P. Langley, actually reflected more the systematic engineering approach of Wilbur and Orville Wright, with an emphasis on a search for practical solutions. Throughout this sometimes torturous process, the NACA helped a fledgling American aircraft industry advance the airplane in a few short decades to an astoundingly high level of technological performance and corresponding importance in modern society.

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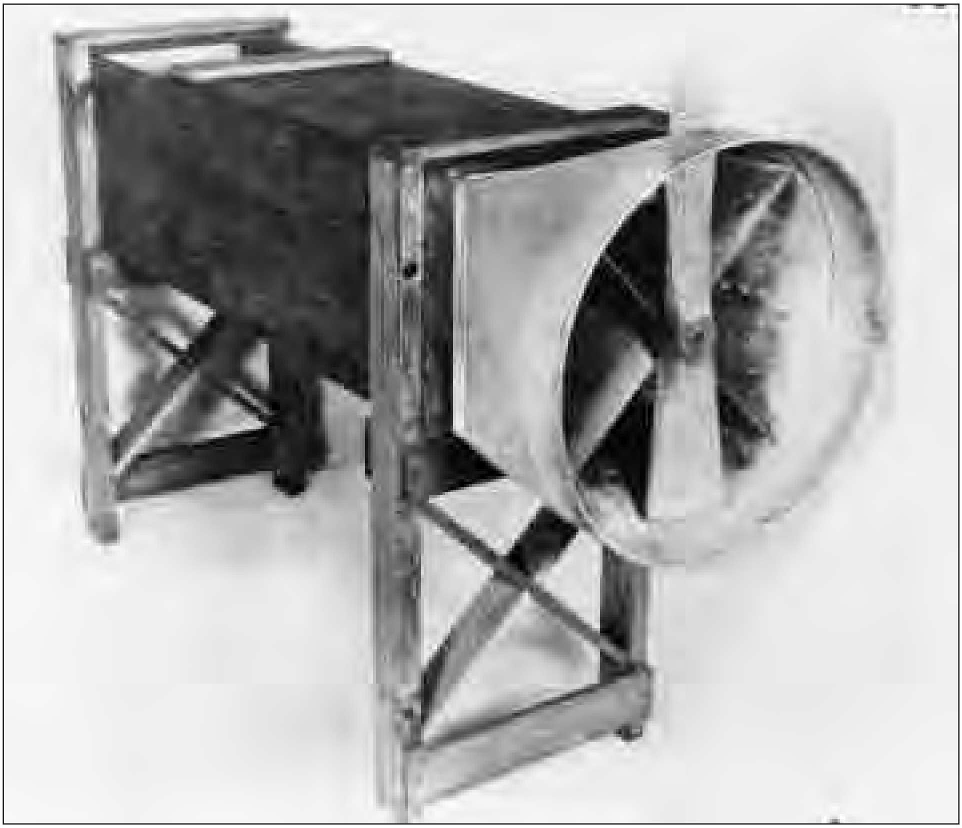
[1] On the history of the National Advisory Committee for Aeronautics, the place to start is Alex Roland's *Model Research: The National Advisory Committee for Aeronautics* (Washington, DC: NASA SP-4103, 1985), two volumes.

[2] NACA Langley's history is told in James R. Hansen's *Engineer in Charge: A History of the Langley Aeronautical Laboratory* (Washington, DC: NASA SP-4305, 1987).



In building a test device like the wind tunnel and then using it systematically to generate reliable data, the Wright brothers forged a solid link between aeronautical research and the design of successful aircraft. SI Negative No. A-2708-G

Center stage in NACA research, from the beginning, was a unique device designed for basic aerodynamic investigation: the "wind tunnel." Following the initial achievement of flight, wind tunnels quickly became the central research facilities at aeronautical laboratories all over the world. This, too, was a legacy of the Wright brothers' approach, for the aerodynamic shape of their landmark aircraft had evolved directly out of testing conducted in a simple little wind tunnel they built in their Dayton bicycle shop. From these wind tunnel experiments in 1901, they garnered the empirical insights needed to design the first truly effective flying machine, their breakthrough glider of 1902.

The Wright brothers did not invent the wind tunnel, and the basic concept behind it predated their work by roughly 400 years. During the Renaissance, the brilliant technological dreamer Leonardo da Vinci recognized that air blown past a stationary object produced the same effect as the object itself moving at the same relative speed through the air, the fundamental concept upon which wind tunnels operate. Da Vinci expressed this idea in his statement from the *Codex Atlanticus*, "As it is to move the object against the motionless air so it is to move the air against the motionless object." Sir Isaac Newton later recognized this same principle.

From these humble beginnings, the first person to apply the concept to practical aerodynamic research seems to have been Benjamin Robbins (1707–1751), a brilliant English mathematician who conducted a series of fundamental experiments on the ballistic properties of artillery projectiles. To evaluate the air resistance of different shapes, Robbins constructed a device known as a "whirling arm." This apparatus consisted of a four-foot horizontal arm attached to a vertical spindle that was rotated by the force of a falling weight. Robbins mounted test shapes with similar cross-section areas on the end of his whirling arm, and found that the speed of the arm's rotation varied considerably with objects of different profiles. From this he concluded that different shapes produced different amounts of air resistance, identifying the factor subsequently understood as aerodynamic "drag."[3]

Following Robbins came John Smeaton (1724–1792), an English civil engineer interested in sources of power for practical applications. Smeaton used a whirling arm device of his own making to investigate the function of windmill sails. In 1759, he described his experiments and the elaborate apparatus used in his tests in a seminal paper presented to the Royal Society of London. In this report, he outlined the need for such equipment by noting a basic problem in aerodynamic research: relying solely on natural wind. "In trying experiments on windmill sails, the wind itself is too uncertain to answer the purpose; we must therefore have recourse to an artificial wind," wrote Smeaton, adding that tests "may be done two ways: either by causing the air to move against the machine, or the machine to move against the air." This principle of relative motion, the same observed earlier by da Vinci, proved to be the key to the future development of the wind tunnel. However, at the time, and indeed for the next hundred years, relatively few of Smeaton's colleagues or successors either understood or accepted it.[4]

Nevertheless, there were some provident individuals intrigued with the investigation of flight in the nineteenth century who utilized Smeaton's whirling arm apparatus to understand aerodynamic principles. Sir George Cayley, for one, whose work outlined the basic shape of the airplane (see Chapter 1 of this volume), used a five-foot whirling arm that achieved tip speeds of up to twenty feet per second

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[3] John D. Anderson, Jr., provides a concise technical summary of the contributions of Leonardo da Vinci, Benjamin Robins, and other pioneers to the genesis of early aerodynamic concepts in *A History of Aerodynamics and Its Impact on Flying Machines* (Cambridge, England: Cambridge University Press, 1997), pp. 14–27 and 55–57.

[4] On John Smeaton, see Anderson, *A History of Aerodynamics*, pp. 58–61 and 76–79.

to measure the lift and drag properties of different wing shapes.[5] Later in the century, some of the most important whirling arm experiments of the pre-flight era were conducted by Professor Samuel P. Langley, who built a large testing apparatus with a thirty-foot arm on the roof of Pittsburgh's Allegheny Observatory in 1886. Powered by a ten-horsepower steam engine, this whirling arm produced tip speeds of nearly 150 feet per second, the equivalent of about 100 miles per hour. But while Langley's research produced much valuable information, his frustrations with the whirling arm also epitomized the limitations of this type of aerodynamic testing machine. Because its sixty-foot diameter made the device too large to be used indoors, Langley was forced to conduct his tests outside where the results were influenced by the vagaries of natural wind and atmospheric conditions. Even worse, the rotation of the arm itself created currents that disrupted the air and compromised the test results.[6] Clearly, researchers needed a superior apparatus if they were to obtain reliable results in a controlled laboratory setting.

A completely new type of device invented by Francis H. Wenham (1824–1908) offered the solution. In 1867 Wenham, a British marine engineer, submitted a proposal to the Aeronautical Society of Great Britain to build a novel machine that applied the principles garnered from earlier studies of hydrodynamic water channels to the new field of aerodynamic research. The Aeronautical Society sponsored Wenham's project, and in 1871 he completed an apparatus equipped with a steam-powered fan that blew air through "a trunk 12 feet long and 18 inches square, to direct the current horizontally, and in parallel course." It was, in essence, the world's first "wind tunnel"—though this term would not be coined until forty-two years later.[7]

From the very beginning, wind tunnels proved a boon to aerodynamic discovery, and the technical data they generated established a new and much more viable basis of knowledge about virtually all phenomena of flight. From Wenham's inaugural wind tunnel investigations came the realization that the aerodynamic lifting forces working on wing surfaces were much greater than Newtonian theory predicted. This early result bred real confidence that powered flight was possible, and it bequeathed much-needed credibility to the serious scientific study of aeronautics. Following Wenham's lead, more students of flight adopted the wind tunnel, start-

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[5] Sir George Cayley's significance in aerodynamic research is analyzed in Tom D. Crouch, *A Dream of Wings: Americans and the Airplane, 1875–1905* (Washington, DC: Smithsonian Institution Press, 1981, 1989), pp. 27–29, 33–35, 47–48, and 63–64.

[6] See Crouch, *A Dream of Wings*, pp. 48–52, for insights into Samuel P. Langley's research equipment.

[7] N. H. Randers-Pherson, "Pioneer Wind Tunnels," *Smithsonian Miscellaneous Collections* 93 (19 January 1935): 1–2. See also F. H. Wenham, "On Forms of Surfaces Impelled Through the Air and Their Effects on Sustaining Weights." On Wenham's contributions to aerodynamics, see Anderson, *A History of Aerodynamics*, pp. 116–117 and 119–126.

ing with fellow Englishman Horatio Phillips, who in 1884 used a wind tunnel of his own design to establish that cambered airfoils developed significantly more lift than flat planes. By 1896, when Hiram Maxim constructed a wind tunnel to advance his aerodynamic research (outlined in the Maxim document in Chapter 1), the use of wind tunnels had spread to France and other European nations. In that year, wind tunnel technology also crossed the Atlantic to the Massachusetts Institute of Technology (MIT), where an inventive graduate student in engineering by the name of A. J. Wells built the first American wind tunnel, a makeshift arrangement that diverted air from a building ventilation duct for simple aerodynamic tests of flat planes.[8]

By the dawn of the twentieth century, though wind tunnel development was still in its infancy, awareness of these valuable tools had spread through the small community of aviation advocates working to achieve mechanical flight. What was most significant for the subsequent history of aviation was that Wilbur and Orville Wright knew enough about wind tunnels to turn to one when they were confronted with anomalous data about the lifting power of wings. As outlined in the previous chapter, the Wrights reached this point after the frustrating 1901 season of flight tests, when they were forced to face the fact that their gliders, based on Otto Lilienthal's coefficients, simply did not achieve the predicted amount of lift. From this they reasoned that Lilienthal's tables might possibly contain errors, which led them to conduct tests of previous assumptions and then move on to develop their own experimental data.

Before building a wind tunnel, the Wrights utilized a clever apparatus of their own design, consisting of a freely rotating horizontal wheel mounted on the front of a bicycle. An airfoil model and a control shape were mounted on opposite sides of this wheel, and the forward movement of the bicycle was used to generate a flow of air past these test surfaces. Results obtained from tests with this device confirmed their suspicion that the Lilienthal tables were indeed faulty, but the method was neither precise nor consistent enough to develop data for a new set of tables. For this purpose, the Wrights required a wind tunnel. In their bicycle workshop they constructed a six-foot-long square trough sixteen inches wide, through which they channeled a twenty-seven-mile-per-hour airflow produced by a two-blade fan driven by an electrical shop motor. As part of their tunnel they also designed a set of balances to measure the relative lift and drag on test airfoil sections. With their "laboratory" thus equipped, sometime around 22 November

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[8] Albert J. Wells describes his wind tunnel device in his bachelor's thesis, "An Investigation of Wind Pressure upon Surfaces," (pp. 4–23), completed at MIT in 1896. For descriptions of the tunnels devised by Horatio Phillips and Hiram Maxim, see Randers-Pherson, "Pioneer Wind Tunnels," pp. 3–4.

Reproduction of the 1901 Wright drag balance. Orville actually built two balances, one for lift measurements and the other for drag. Model airfoils could be mounted on both balances and easily changed. Lift and drag forces calculated from these measurements clearly indicated that the previously accepted aerodynamic coefficients on which the design of airfoils were being based were grossly in error. SI Negative No. A-41899-B

1901 they began a short but intensive two-week sequence of tests, through which they systematically and precisely evaluated over 150 different airfoil shapes. By the conclusion of these tests the Wrights had amassed the greatest body of aerodynamic data in the world, and from this pinnacle of understanding they were armed with the knowledge to build a true working flying machine. Noted historian and Wright biographer Tom Crouch observed of this period of wind tunnel testing that "[b]oth brothers would look back on these few weeks in November and December 1901 as the psychological peak of their joint career in aeronautics."[9] In retrospect, this short stretch of time also proved to be a turning point in the

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[9] Tom D. Crouch, *The Bishop's Boys: A Life of Wilbur and Orville Wright* (New York, NY: W.W. Norton & Co., 1989), pp. 227–228. See also Crouch, A Dream of Wings, pp. 246–248.

entire history of technology, because it prepared the way, only two years later, for the successful flight of a powered airplane.

On 7 December 1901 the Wright's sister Katherine noted in a message to their father that "[t]he boys have finished their tables of the action of the wind on various surfaces, or rather they have finished their experiments." The important work of analyzing and applying the results lay ahead, and it is at this point that the documentary trail of Chapter 2 begins, with a series of letters written by Wilbur Wright to Octave Chanute and George Spratt, a Chanute disciple who, with his mentor, would subsequently join the Wrights at Kitty Hawk for the momentous 1902 flying season.

Like the airplane itself, the use of wind tunnels quickly advanced beyond the work of the Wright brothers. In unrelated work in 1901, Professor Albert F. Zahm (1862–1954) began operating an impressively sized forty-foot-long tunnel with a six-foot-square cross section at Catholic University of America in Washington, D.C. This large test apparatus was entirely enclosed in a building specifically designed for its operation, making it the world's first true wind tunnel laboratory. Zahm used this facility to conduct pioneering tests on skin friction, and went on to become an important figure in the institutional foundation of American aerodynamic research.[10]

Although the Wrights, Zahm, and other Americans continued to pursue aerodynamic discoveries, it was the Europeans who quickly came to dominate the field in the first quarter of the twentieth century. Between 1903 and the start of World War I in 1914, no less than ten wind tunnels began operation in Europe, as bellicose governments concerned with the military applications of aircraft invested in new aeronautical laboratories. This government interest produced such important facilities as the British National Physical Laboratory outside London and the influential German laboratory at the University of Göttingen. The latter facility featured an innovative closed-circuit wind tunnel designed by Professor Ludwig Prandtl, who was at the time perhaps the greatest scientific mind delving into aerodynamic phenomena. Its signature design feature was the incorporation of a return passage, which kept all the air moving through the tunnel instead of allowing the air to circulate uncontrolled through the building. Although Prandtl's tunnel was simple, with a constant two- by two-meter cross section throughout, his design was vastly more efficient and eventually spawned a whole new generation of wind tunnels. With a lower volume of air in motion, the closed tunnel required

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[10] A. F. Zahm, "New Methods of Experimentation in Aerodynamics" [paper presented at the meeting of the American Association for the Advancement of Science, Pittsburgh, PA, 20 June 1902], in *Aeronautical Papers of Albert F. Zahm, Ph.D., 1885–1945* (Notre Dame, IN: University of Notre Dame, 1950).

less power than comparably sized open tunnels; furthermore, the quality of the air and its flow could be more precisely controlled. The majority of modern wind tunnels utilize this closed-circuit concept, and their lineage can be traced to the original tunnels of Ludwig Prandtl.[11]

In neighboring France, renowned structures engineer Gustav Alexandre Eiffel (1832–1923) invested not only his legendary energy but also his considerable fortune in aeronautical research, establishing two wind tunnel facilities in and around Paris. In 1909, he built a 1.5 meter diameter tunnel at Champs de Mars, near the famous tower that bears his name. Two years later, after conducting over 4,000 tests in his first tunnel, Eiffel constructed a new and much larger wind tunnel laboratory at Auteuil. (Eiffel published a book entitled *La Resistance de l'Air* to report on his initial research, and a 1913 translation of this volume by American naval observer Jerome C. Hunsaker was the first to use the term "wind tunnel" to describe these facilities.) An important focus of Eiffel's tests involved pressure distribution on airfoil surfaces, and he used his wind tunnels to reveal that the reduction in surface pressure on the top surface of a wing was more significant in generating aerodynamic lift than the pressure increase on its lower surface. Eiffel also conducted important research on propeller aerodynamics, and he was the first to test models of complete airplanes in a wind tunnel. This work helped to establish a clearer understanding of the correlation between test results and the actual performance of full-size aircraft. Eiffel was also able to establish empirically the validity of the relative motion principle, providing scientific proof for the fundamental theory underlying wind tunnel simulation that began back with da Vinci's idea.[12]

While the epicenter of aerodynamic research and development shifted to Europe by the start of the Great War, small pockets of aeronautical enthusiasts existed in the United States, many of whom were not at all happy that the homeland of the Wright brothers had given away its early advantage to the French, Germans, and British. American research lacked a strong organizational backing, and a few farsighted individuals who appreciated the long-term importance of aviation to the United States lobbied for a governmental commitment to aeronautical

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[11] For the history of the aerodynamics research organized under Ludwig Prandtl's leadership at the University of Göttingen, see Paul A. Hanle, *Bringing Aerodynamics to America* (Cambridge, MA: MIT Press, 1982). Those who read German should consult Julius C. Rotta, *Die Aerodynamische Versuchsanstalt in Göttingen* (Göttingen, Germany: Vanderhoeck & Ruprecht, 1990), especially pp. 45–47. For a detailed analysis of Prandtl's contribution to aerodynamics, see Anderson, *A History of Aerodynamics*, pp. 251–260.

[12] For an English translation of Eiffel's own description of his wind tunnels, see G. Eiffel, *The Resistance of Air and Aviation: Experiments Conducted at the Champs-de-Mars Laboratory*, trans. Jerome C. Hunsaker (London: Constable & Co.; and Boston: Houghton Mifflin & Co., 1913). For an assessment of Eiffel's tunnels and their contributions to aerodynamics, see Anderson, *A History of Aerodynamics*, pp. 267–282.

research that could compete with the European establishments. Some of the most dedicated enthusiasts were naval officers Jerome C. Hunsaker, David W. Taylor, and Washington Irving Chambers. In 1912, Chambers produced a report on aviation for the U.S. Navy (part of which is included in this chapter), which included a detailed proposal for a national aerodynamic laboratory. Following this, David Taylor enlisted the technical assistance of Albert Zahm to design a closed-circuit wind tunnel with an eight-foot-square test section, completed in 1913 at the Washington Navy Yard.

On the academic side, MIT, where wind tunnel pioneer A. J. Wells held a position on the faculty, initiated efforts to develop a program in aeronautical research. MIT president Richard Maclaurin personally investigated Britain's National Physical Laboratory in 1910, and shortly thereafter he approached the U.S. Navy about a cooperative effort to establish a course of study in aeronautical engineering. When Jerome Hunsaker finished a master's degree at MIT in 1912, Maclaurin asked the Navy to allow him to remain for three years as an instructor. Before taking up this assignment, Hunsaker accompanied Zahm on part of the latter's six-month inspection of European aeronautical research facilities, sponsored by the Smithsonian Institution. An excerpt from Zahm's report of this trip is included in the following documents. After his return to the United States, Hunsaker used plans of the British National Physical Laboratory's (NPL) four- by four-foot open-circuit wind tunnel to construct a duplicate facility on the MIT campus.[13] The Smithsonian published papers by Hunsaker and five other MIT professors in 1916 as *Reports on Wind Tunnel Experiments in Aerodynamics*, one of the earliest comprehensive reports on American aeronautical research. Hunsaker's description of wind tunnel testing at MIT is excerpted from these papers and included in the document section of this chapter.

Finally the United States government took action and, with a two-paragraph rider attached to a 3 March 1915 naval appropriations bill, Congress established a new agency, the National Advisory Committee for Aeronautics (NACA). This charter—with its specific charge "to supervise and direct the scientific study of the problems of flight with a view to their practical solution"—guided the NACA for the next forty-three years, until on 1 October 1958 it became the nucleus of the new National Aeronautics and Space Administration. In the beginning, however, the NACA was just a single panel of advisers with a modest $5,000 annual budget to tackle the ambitious task of coordinating both civilian and military aeronautical research and development programs. Initially, the NACA sponsored research in

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[13] For an excellent treatment of Jerome C. Hunsaker's role in the progress of American aeronautical research, see William F. Trimble, *Jerome C. Hunsaker and the Rise of American Aeronautics* (Washington, DC: Smithsonian Institution Press, 2002).

No organization would ever do more to foster the development of wind tunnel technology than the National Advisory Committee for Aeronautics. This photograph shows a meeting of the NACA in January 1921. Around the table, from left to right, sat Professor Charles F. Marvin, chief of the U.S. Weather Bureau; Dr. John F. Hayford of Northwestern University; Orville Wright; Major Thurman H. Bane, chief of the engineering division of the U.S. Army; Paul Henderson, second assistant to the postmaster general; Rear Adm. William A. Moffett, chief of the Navy Bureau of Aeronautics; Dr. Michael I. Pupin of Columbia University; Rear Adm. D. W. Taylor, chief of the U.S. Navy's bureau of construction and repair; Dr. Charles D. Walcott, secretary of the Smithsonian Institution and chairman of the NACA; and Dr. Joseph S. Ames of The Johns Hopkins University, chairman of the NACA's executive committee. NASA Image #NACA-1921

the Washington Navy Yard tunnel and at Stanford University, where Dr. William F. Durand pursued a systematic delineation of the best propeller shapes.[14] It was not long, however, until the Committee realized that it needed a fully staffed laboratory of its own that could investigate all aspects of aeronautical research. With two special appropriations in 1916 and 1917 of nearly $190,000 to establish an aeronautical laboratory, the NACA sought a site where they could share a flying field with the military. After consideration they settled on a location near Hampton, Virginia, where the U.S. Army was setting up an air base christened Langley Field. Construction at Langley began in April 1917, just as the United States entered World War I.

America's entry into the war injected a sense of urgency into the nation's aviation research, but the timing could not have been worse for the concept of a joint

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[14] On the Washington Navy Yard tunnel and its aeronautical research, see J. Norman Fresh, "The Aerodynamics Laboratory—The First 50 Years," *Aero Report* 1070 (Washington, DC: Department of the Navy, 1964), pp. 7–14.

The U.S. Army's first-ever wind tunnel, built at McCook Field in Ohio in 1918, could reach extremely high speeds for its day. It had a twenty-four-blade fan that spanned five feet in diameter, which could push the airspeed (within its small fourteen-inch diameter closed-throat test area) to a little over 450 miles per hour. The tunnel is now on display in the Air Force Museum in Dayton. SI Negative No. A-1855

civilian-military laboratory. Both the army and the navy felt that the civilian laboratory at Langley Field could not be adequately equipped and staffed in time to solve wartime problems. Thus, using the rationale of wartime expediency, both services put their efforts into their own resources. The navy expanded its Washington Navy Yard laboratory, while the army established a facility of its own at McCook Field near Dayton.

To handle the increasing work load at the Washington Navy Yard, David Taylor persuaded Albert Zahm in 1917 to leave Catholic University and take a new position as head of the navy aeronautical laboratory. There at the Navy Yard in 1918, a second wind tunnel began operation. This was an unsophisticated copy of a 1912 British NPL open-circuit, forty-five-MPH design with a four- by four-foot test section. The navy used the new wind tunnel mainly for tests on airfoils and for instrument development and calibration, freeing the older but larger eight-foot tunnel for studies involving scale models of complete aircraft. Zahm supervised the navy's aeronautical laboratory until 1930, and during his thirteen-year career at the Washington Navy Yard he conducted important research concerning skin friction, component drag, instrumentation, and wind tunnel design, with results presented in more than thirty-five published papers.

On the army side, the wartime Air Production Board established by Congress authorized the establishment of a "temporary" aviation engineering and experimental facility at McCook Field in September 1917. To staff the facility, the army pulled its research people from the still-incomplete Langley Field. Initially, work at McCook Field focused on production matters, but the need for basic research rapidly became apparent, resulting in the construction of a small wind tunnel in 1918. The army's wartime research concentrated on the refinement of engines, propellers, and a variety of aircraft-operation issues, but also saw the beginnings of an aerodynamic research program utilizing both wind tunnels and in-flight testing. An excerpt of a July 1918 report from the Airplane Engineering Department at McCook, "Full Flight Performance Testing," is included in this chapter's documents.

By the end of the war, Langley Field was essentially out of the army's aviation research efforts, leaving the NACA alone in Virginia to develop its own independent program. Although the NACA began its own flight testing at Langley Field in 1919, it was not until the following year that its first wind tunnel was ready for operation. This "NACA Wind Tunnel No. 1," an open-circuit design with a five-foot test section, was nothing more than another American copy of an outdated British NPL tunnel. While limited, this first tunnel did allow NACA engineers to practice the application of airflow theory and the design of wind tunnel equipment.[15] This critical initial experience is reflected in early NACA technical reports on the "Design of Wind Tunnels" from 1919 and 1920. Other documents provided in this chapter also bear witness to the immature state of the American aeronautical engineering community around 1920 and signal how far that community had to advance to catch up with the Europeans.

The formative nature of the entire field of aerodynamic research in the 1920s is captured in an interesting sequence of documents beginning with a 23 August 1920 call from Dr. Joseph Ames, physics professor and then vice-chairman of the NACA Committee on Aerodynamics (Ames would later serve as the chairman of the NACA's Main Committee, from 1927 to 1939), for the "standardization of wind tunnels"—or as Ames described it, "information which would enable one to connect the data published" from all the various wind tunnels around the world. In answer to Ames's appeal for standardization came letters from many of the greatest names in aerodynamic research, including Dr. Prandtl at the University

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[15] The design of the NACA's first wind tunnel is examined in Donald D. Baals and William R. Corliss, *Wind Tunnels of NASA* (Washington, DC: NASA SP-440, 1981), pp. 2–3.

Two wind tunnel researchers pose near the entrance end of Langley's five-foot Atmospheric Wind Tunnel (AWT), where air was pulled into the test section through a honeycomb arrangement meant to smoothen the flow. NASA Image #L-1990-04342 (LaRC)

of Göttingen, Dr. Zahm at the Washington Navy Yard, Dr. Durand at Stanford, and many representatives of the nascent American aircraft industry. A selection of these responses to Ames are included in the documentary collection, and they provide a snapshot into what the leading aerodynamic researchers thought needed to be done in order to improve the reliability of wind tunnel data around the world and to build an international network that would allow aerodynamic experts to better learn from each other. The NACA eventually drew up specifications for comparative tests in wind tunnels followed by wind tunnel personnel at Langley. While the overall objective of worldwide standardization was never fully realized, this early attempt toward it brought the minds of international experts together on basic issues of aerodynamic research, and it raised fundamental questions about the problems of testing scale models in wind tunnels.

A comparable call for more exactitude in aerodynamic testing came a few months later in 1920 when Commander Jerome Hunsaker, at that time with the U.S. Navy Bureau of Construction and Repair, recommended that the NACA pursue a systematic program comparing wing characteristics as ascertained in wind tunnel tests with those learned in free flight. (Hunsaker became a committee member of the NACA in 1922 and later served as NACA chairman from 1941 to 1956.) The proposal, which the NACA embraced and carried out at Langley, led to the establishment of new research methods that produced important results. For the first flight tests the NACA obtained a Curtiss JN4H "Jenny" biplane, and Langley model makers built two models of this aircraft for wind tunnel testing. The engineers used one model for tests measuring lift, drag, and moments, while the other was outfitted for pressure-distribution tests. The wind tunnel data from the models was then compared to measurements obtained during actual flight tests of the full-size Jenny. Such comparative tests were first tried a decade earlier by Gustav Eiffel, but the Langley program sparked by Hunsaker's proposal proved far more extensive and was accomplished with considerably greater precision. Learning a great deal from research programs that combined wind tunnel experiments with free flight tests, the NACA began to stake out a crucial role for itself as a research establishment. Unlike the Army and Navy programs, which tended to be more development-oriented than research-oriented, the NACA focused on basic aerodynamic problems that affected civilian and military aviation alike.

In a roundabout way that was nevertheless typical of the serendipitous path of basic research, Langley's JN4H comparison studies also helped lead the NACA to an altogether unexpected destination, the design of a revolutionary new type of wind tunnel. The test program resulting from Hunsaker's proposal confirmed that wind tunnel testing was a valid predictor of aircraft performance and behavior, but it also pointed NACA researchers toward another well-known, but poorly understood, problem concerning scale effects. Prior to 1920, no wind tunnel ever built had been able to address scale effects with any success. Yet by that time people working in aeronautical research realized that the forces generated by a scale model were not, in fact, proportional to the model's scale. A novice to the field might assume that a ¹⁄₂₀-scale model of a airplane placed into an air stream moving ¹⁄₂₀ as fast as the actual airplane flew would generate roughly the same forces of lift and drag as a full-scale machine in flight, but that was far from the truth; the scale model actually generated considerably less. Early aerodynamicists had developed empirical coefficients to "scale-up" the data, but the NACA comparison tests showed just how unreliable these coefficients were. The air itself could not be "scaled-down" to model size, and air properties, such as density and temperature, manifested themselves the same way in the wind tunnel as they did around a full-size airplane. To gain the maximum value from wind tunnel testing, an answer to the scaling problem had to be found.

The conceptual tools needed were found in the hydrodynamic work of Osborne Reynolds (1842–1912). Experimenting with the characteristics of fluid flows at the

University of Manchester in England, in 1883 Reynolds established experimentally that the transition from laminar or smooth flow to turbulent flow always occurred when certain variables exceeded a critical value defined by a specified flow parameter. The dimensionless number that resulted came to be known as the "Reynolds number," and it became the key to understanding scale factors. This fundamental finding, acknowledged as "a stunning discovery," eventually enabled researchers to establish a direct quantitative link between experiments with scale models used in wind tunnels and the airflow patterns of full-scale designs. In essence, it meant that the testing of scale models in wind tunnels could lead not only to meaningful theoretical results, but also to practical design applications.[16] It took nearly forty years for Reynolds's breakthrough to make the transfer from scientific esoterica into a practical factor for aircraft engineers, but the tremendous expansion of wind tunnel work in the United States in the 1920s was in part driven by a growing appreciation of the significance of Reynolds number effects.

In demonstrating that a fluid flow suddenly changed from laminar to turbulent as speed increased, Reynolds showed that the forces a moving fluid exerted on a body depended on the fluid's velocity, density, and viscosity, and on key dimensions of the body itself, such as length or diameter. To achieve the "Reynolds number," he combined these parameters into a mathematical expression where all of the dimensions cancelled one another out. Because it was dimensionless, the Reynolds number could be used to compare fluid-flow forces around similarly shaped, but differently sized, objects. One could achieve what came to be called "dynamical similarity" by varying different parameters, such as decreasing the velocity or increasing the density, to produce the same Reynolds number for different tests.

In theory, then, it was possible for aerodynamicists to make an excellent correlation between model tests and aircraft performance, but this was easier said than done. When a 1⁄20-scale model was tested in the NACA's original atmospheric wind tunnel in 1920, the Reynolds number for the test represented no better than 1⁄10 that of the corresponding full-scale flight. In principle, larger models could have been used, but wingspans greater than about 3 1⁄2 feet could not be used in the five-foot-diameter test section of the first NACA wind tunnel due to aerodynamic interference from the tunnel walls. In addition this atmospheric wind tunnel also lacked the power to run at the high speed necessary to generate the required Reynolds number, as such a velocity was simply not practical in an open wind tunnel.

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[16] See Osborne Reynolds, "An Experimental Investigation of the Circumstances Which Determine Whether the Motion of Water in Parallel Channels Shall Be Direct or Sinuous, and the Law of Resistance in Parallel Channels," *Proceedings of the Royal Society* (London, 1883), pp. 84–89. On Reynolds's contribution to fluid mechanics, particularly the concept of the "Reynolds number," see Anderson, *A History of Aerodynamics*, pp. 109–114.

Dr. Joseph S. Ames at his desk at NACA headquarters in the early 1920s. Ames was a founding member of the NACA, appointed by President Woodrow Wilson in 1915. He served as chairman of the NACA's Main Committee from 1927 to 1939. NASA Image #LAL90-3738 (Ames)

Two possible solutions to the problem emerged almost simultaneously in the year 1920. Both originated in Europe, and both were offered for "sale" to the NACA in America. In each case, the technical solution was to increase the density/viscosity factors in the Reynolds number calculation. The first concept came from Wladimir Margoulis, a Russian-born aerodynamicist and protégé of Nikolai Joukowski and Gustav Eiffel, who had just started work for the NACA's Office of Aeronautical Intelligence in Paris as technical consultant and translator. Margoulis's proposal was to replace air with carbon dioxide and completely seal this atmosphere in a fully enclosed wind tunnel. Because carbon dioxide's density was over 1 ½ times greater than that of air, the Reynolds number of any test in such a chamber would be correspondingly higher.

The second concept (though it is impossible to resolve whose idea actually developed first) came from Dr. Max Munk, a brilliant star student of Ludwig Prandtl, whose laboratory at Göttingen was the leading aerodynamic research facility in the world. In 1916, Prandtl had designed a major new wind tunnel, the virtues of which American aeronautical observer and NACA consultant Edward P. Warner extolled in his 1920 "Report on German Wind Tunnels and Apparatus." Unlike his earlier 1908 tunnel, which had a constant cross section, Prandtl's second-generation apparatus merged the tapered diffuser of an open tunnel with a closed-circuit design, resulting in much-improved air management. Prandtl based the design on the principle that the potential energy (static pressure) and kinetic energy (velocity) of air could be interchanged in different parts of a wind tunnel to enhance performance. The open test section of the new Göttingen wind tunnel measured two meters in diameter, but by enlarging the return duct and thereby reducing the velocity of the returning air, friction and the associated power requirements were reduced—yet because slower-moving air exerted a higher static pressure than high-speed air, the air's momentum was retained. The return duct, cast in concrete (and in this particular tunnel uniquely placed in a vertical orientation underneath the floor) possessed guide vanes to turn the air flow around the corners. Two other particularly significant features of this design were the incorporation of a stilling chamber and a contraction cone ahead of the testing area that acted to reduce turbulence and then accelerate the air passing into the test section. The design was so efficient that a 300-horsepower motor rotating a four-bladed fan was sufficient to produce test section wind velocities of 170 feet per second. While historians sometimes call Prandtl's earlier 1908 design the first "modern" wind tunnel, it is actually his second Göttingen tunnel that truly

The young NACA first built its reputation as an outstanding aeronautical research institution on the strength of its Variable Density Tunnel (VDT). The tank for the VDT arrived at Langley by rail from its manufacturer, the Newport News (Va.) Shipbuilding & Dry Dock Company in February 1922. It was an eighty-five-ton pressure shell with walls made from steel plate lapped and riveted according to a practice standard in steam-boiler construction. NASA Image #L-1990-04352 (LaRC) deserves the accolade, for this revolutionary design established a new standard and became the model for subsonic wind tunnels around the world.

It was the environment and technical culture of Prandtl's laboratory that structured the thinking and nurtured the remarkable talent of Max Munk, who in 1917 earned not one but two doctorates at Göttingen (in both engineering and physics). With the exception of Prandtl, no one knew the Göttingen wind tunnels better than Munk—and as subsequent achievements in wind tunnel design proved, no one, not even Prandtl, better understood the principles and potential of wind tunnel technology.

In early 1920, Munk proposed the idea of a wind tunnel built inside a pressure vessel so that tests could be run under high pressure, thus increasing the density of the air as much as twentyfold. The thirty-year-old temperamental genius tempted the American research establishment with his concept in a personal letter sent from Germany to the navy's Hunsaker, who knew of Munk's work at Göttingen from his continuous review of German aeronautical activities. Hunsaker informed NACA Chairman Dr. Joseph Ames of Munk's idea, and Ames persuaded the rest of the Committee, which was hard-pressed for talented aerodynamicists, to offer Munk a position as a technical consultant. To employ the German aerodynamicist in the United States required two special orders from President Woodrow Wilson, one to allow a recent enemy into the country, and the other to authorize him to hold a government job. When these were secured, Munk arrived in Washington, D.C., in late 1920 and began seven turbulent years of NACA employment, first as a technical assistant in the NACA's Washington office and later as chief of aerodynamics at Langley.[17]

The acceptance of Munk's idea and the design of such a bold new type of wind tunnel turned the NACA from a second-rate player into a world leader in aerodynamic research. This tunnel, known as the Variable Density Tunnel, or VDT, went into service at Langley as "NACA Wind Tunnel No. 2" in 1922, and its results were vastly superior to those obtained with any previous tunnel design.

Two NACA technical papers by Munk excerpted as documents for this chapter provide insights into the revolutionary nature of the VDT's design. Externally, it appeared to be little more than a large cylindrical tank with hemispherical ends. But, in fact, a five-foot-diameter wind tunnel was mounted inside such that air flowed through a central test section, then past a fan that blew the air back around via an annular return passage. The entire tank could be pressurized to 300 pounds per square inch (20 atmospheres), sufficient to produce Reynolds numbers for tests of $\frac{1}{20}$-scale models that were equivalent to full-scale flight. An externally

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[17] On Munk's coming to the NACA, see Hansen, *Engineer in Charge*, pp. 72–78 and 84–95 and Roland, *Model Research*, pp. 87–98.

mounted 250-horsepower synchronous motor turned a seven-foot-diameter propeller to produce test speeds of seventy-five feet per second. Small windows in the tank permitted technicians to view the test section during operation, and a hatch at one end provided access for mounting test specimens and for tunnel maintenance.

Langley engineers quickly ran a wide variety of tests in the VDT, including studies of several model airplanes, to validate the high-pressure concept. But the most long-lasting and significant investigations involved airfoils. Through extensive use of the VDT, the NACA drew accurate performance curves for the commonly used airfoils of the era, and then extended the investigation to develop entire families of airfoils with similar characteristics. This iterative process eventually led to the development of the highly refined airfoils prominent in both the aircraft design revolution of the 1930s and the laminar flow developments of the 1940s, areas that will be addressed in subsequent chapters. Even now, it is difficult to overestimate the effect of these airfoil test programs on aeronautical development. The unique capabilities of the VDT and the valuable library of airfoil data it generated greatly advanced the state of aerodynamics not only in the United States but also around the world.

NACA Langley's chief of aerodynamics, Elton W. Miller, inspects his researcher's installation of a Sperry M-1 Messenger airplane into the lab's new Propeller Research Tunnel (PRT) in early January 1927. This was the first complete, full-scale airplane ever to be tested in a wind tunnel in the United States. NASA Image #L-01892 (LaRC)

As a civilian organization, the NACA was relatively unencumbered with military security requirements during this period, and it made the airfoil information readily available to airplane designers through a formal series of "Technical Reports." These "TRs" were carefully prepared publications that not only helped the American aircraft industry select its wing shapes but also served as basic textbook material for an entire generation of up-and-coming aeronautical engineers. A number of documents in this chapter and in the chapters that follow are excerpts from classic NACA Technical Reports.

The VDT provided for unprecedented airfoil research at high Reynolds numbers, but it was not well suited for certain other investigations, such as propeller testing. Model propellers were unsuitable because they did not deflect during operation the same way full-size propellers did, a source of such significant error as to invalidate model tests. Testing of full-size propellers, on the other hand, required an evaluation of actual performance on an airplane in expensive and often dangerous flight tests, and even then accurate measurements were difficult to obtain. Though the NACA sponsored propeller research in Stanford University's Eiffel-type tunnel started in 1917, until the mid-1920s no valid laboratory method of studying the entire propulsion system and its relationship with the body of the aircraft existed. The NACA's response to this situation is shown in documents starting with a series of memos from Dr. Munk and others at the NACA that trace the genesis of the world's first wind tunnel of considerable size, the Propeller Research Tunnel (PRT) of 1926–1927.[18] These contemporary documents are complemented by a retrospective account from the autobiography of Langley engineer Fred E. Weick, who built and first operated the NACA Propeller Research Tunnel (PRT) at the Langley laboratory.

The PRT featured a Prandtl-style tunnel with a huge twenty-foot open test section. The enormous power required for a tunnel of this size was provided by two navy surplus 1,000-horsepower submarine engines, which produced wind velocities of over 160 feet per second, or the equivalent of 110 miles per hour. The balance supported a full-size airplane body or a substitute "test fuselage" equipped with an onboard dynamometer to measure engine torque directly. Special methods were devised to measure blade deflection optically during tests. When it went into operation in 1927, the PRT quickly proved its worth for propeller testing in conditions that were close to those in actual flight, but because it was the first tunnel large enough to accommodate full-size airplane fuselages its use soon expanded to

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[18] On the design, construction, and early operation of the NACA's Propeller Research Tunnel, see Fred E. Weick and James R. Hansen, *From the Ground Up: The Autobiography of an Aeronautical Engineer* (Washington, DC: Smithsonian Institution Press, 1988), pp. 49–59, as well as Hansen, *Engineer in Charge*, pp. 87–90.

Four hundred thirty-four feet long, 222 feet wide, and ninety feet high, the building housing the thirty- by sixty-foot Full-Scale Tunnel dominated the Langley scene. Its location along the Little Back River, a tidal river off the nearby Chesapeake Bay, occasionally caused flooding problems for the tunnel during hurricanes and nor'easters. NASA Image #EL-1999-00405 (LaRC)

include drag studies of other aircraft components, such as landing gears, tail planes, and cooling systems. The latter tests in the PRT led to the development of the celebrated NACA cowling for radial aircraft engines and Langley's first Collier Trophy Award. (These events are detailed in the next chapter, found in the forthcoming Volume 2, which deals with the design revolution in aircraft aerodynamics.) These crucial results from the PRT, following closely on the heels of the contributions of the VDT, catapulted the NACA Langley research laboratory to a position of unparalleled importance in American aviation.[19]

The PRT also inspired the NACA to build an even larger wind tunnel. The close connection that existed between the engineering design of the PRT and the conception of Langley's next mammoth facility, the historic thirty- by sixty-foot Full-Scale Tunnel completed in 1931, has not been fully appreciated by historians. But thanks to the discovery of a series of neglected NACA memos from 1925,

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[19] For a historical analysis of the NACA cowling program, see James R. Hansen, "Engineering Science and the Development of the NACA Cowling," in *From Engineering Science to Big Science: The NACA and NASA Collier Trophy Research Project Winners*, ed. Pamela E. Mack (Washington, DC: NASA SP-4219, 1998), pp. 1–28. The latter is an expanded version of Hansen's chapter on the cowling in *Engineer in Charge*.

The first tests of an aircraft in the FST involved a Vought O3U-1 "Corsair." In the summer of 1931, the NACA used the navy airplane for some preliminary tests to check out the FST and as the subject of the first publicity photographs taken of FST operations. NASA Image #EL-1999-00425 (LaRC)

reproduced in the document section of this chapter, one can now see how NACA engineers actually anticipated the FST in the building of the PRT. On 7 April 1925, after reviewing a seven-page memo on a "Proposed Giant New Wind Tunnel" from an enthusiastic assistant aeronautical engineer named Elliott G. Reid, Langley engineer-in-charge Leigh M. Griffith asked the NACA Washington office, Why not, after finishing the PRT, plunge ahead with an even larger tunnel capable of testing a full-size aircraft? "If we could actually fly the same model that we test in the tunnel," Griffith wrote, "we would have the unquestioned means of investigating airplane performance and characteristics in the most direct, accurate, convenient, and conclusive manner."

In other words, the NACA would have a means of eliminating the scale effect factor altogether. The VDT showed that scale-model tests at high Reynolds numbers produced accurate data about airfoils, but was not effective for testing fuselages. The PRT, on the other hand, demonstrated the value of testing the synergistic component characteristics of full-size aircraft, but were not large enough to include

The cavernous test section of the FST also came in handy at NACA conferences. In this picture from May 1934, attendees at the NACA's annual aircraft engineering conference posed beneath a Boeing P-26A "Peashooter." Present in this photo, among other notables, were Orville Wright, Charles Lindbergh, and Howard Hughes. NASA Image #EL-1996-00157 (LaRC)

the full span of the wings. So while both tunnels produced valuable breakthroughs, neither was capable of totally reproducing the conditions experienced by an entire airplane in flight. It was the desire to cross this new threshold in aerodynamic research, prompted by the work of the VDT and PRT, that inspired NACA engineers to design and build a wind tunnel large enough for full-scale testing.

The NACA Full-Scale Tunnel (FST), completed in 1931, proved gigantic in every respect and loomed over every other structure at Langley. A closed-circuit wind tunnel with an unprecedented thirty- by sixty-foot open test section, the FST was large enough to handle airplanes or large-scale models with wingspans of up to forty-five feet. Two monstrous thirty-five-foot-diameter propellers in the dual return ducts, each driven by a 4,000 horsepower electric motor, circulated almost 160 tons of air through the 838-foot-long circuit and produced wind velocities of nearly 120 miles per hour. These speeds were sufficient to enable measurements that could be confidently extrapolated to cover the aircraft's entire speed range because scale factors were minimized or eliminated entirely. The remarkable FST and the aerodynamic research it enabled was a crowning achievement for the

In 1928, the NACA replaced its original Atmospheric Wind Tunnel ("Wind Tunnel No. 1") with two tunnels—a five-foot vertical tunnel and a seven- by ten-foot Atmospheric Wind Tunnel. An NACA engineer sets up a test in Langley's seven- by ten-foot AWT. Though an all-purpose facility, its main purpose was to study stability and control problems. NASA Image #EL-1999-00418 (LaRC)

NACA, and it marked how far American aeronautic research institutions had come since the end of World War I.[20]

The NACA also built other wind tunnels along with the highlighted VDT, PRT, and FST. The first pioneering high-speed research was begun in 1927 with a small eleven-inch tunnel that used the exhaust air released when the VDT was depressurized to produce brief flows approaching the speed of sound. The concept proved successful and led to the construction of two improved high-speed tunnels in the next decade. In 1929 Langley's obsolete "Wind Tunnel No. 1" was dismantled and replaced with two new tunnels, a seven- by ten-foot Atmospheric Wind Tunnel (AWT) and a five-foot Vertical Wind Tunnel. Completed in 1930, the new

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[20] On the design and history of the NACA's Full-Scale Tunnel, see Baals and Corliss, *Wind Tunnels of NASA*, pp. 22–23, plus Hansen, *Engineer in Charge*, pp. 101–105, 194–202, and 447–449.

Housed in the same building as the seven- by ten-foot AWT, Langley researchers used the five-foot Vertical Wind Tunnel mainly for spin tests. An engineer kneels on a platform next to the test chamber. In the foreground is the device's (closed) return passage. Like the AWT, the tunnel had an open throat. The vertical tunnel stood thirty-one feet tall and was twenty feet long and ten feet wide. It was not ready for operation until early 1931. NASA Image #EL-1999-00410 (LaRC)

Another major center of U.S. aerodynamic research—this one sponsored by the U.S. Army—flourished at McCook Field near Dayton, Ohio. During World War I, McCook engineers made significant contributions to airplane and engine development, but even greater contributions came in the decade after the war. Like NACA Langley, the aircraft engineering division at McCook combined systematic wind tunnel testing with a full program of flight research. But beyond that, the McCook operation also engaged more directly in the actual design, construction, and operational problems of aircraft, which were beyond the mandate of NACA research. In 1927, the army closed McCook and moved its flying field and associated units to a major new parcel of land dubbed Wright Field. Eventually the site became Wright-Patterson AFB. SI Negative No. A-1848

AWT was a marked improvement over the 1920 tunnel it replaced, and it remained a workhorse facility at Langley for many years. The novel Vertical Tunnel, with its upward flowing air stream, was an innovative concept that offered an unprecedented opportunity to investigate aircraft spins, a leading cause of disastrous crashes. An excellent example of adapting an existing technology to a new use, the Vertical Tunnel of 1930 was the prototype for additional spin tunnels at Langley, as well as for the concept of free-flight wind tunnel testing, which the NACA also pioneered.[21]

By the beginning of the 1930s, the NACA had assembled a collection of wind tunnels at Langley whose collective capabilities surpassed those of any other

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[21] All of the early NACA tunnels are discussed in Baals and Corliss, *Wind Tunnels of NASA*. Appendix D of Hansen's *Engineer in Charge* provides a comprehensive catalog of all the facilities that were developed at NACA Langley from 1917 to 1958.

Between 1926 and 1930, the Daniel Guggenheim Fund for the Promotion of Aeronautics disbursed over $3 million for the creation of aeronautical engineering programs at several American universities. In association with these programs, a number of new wind tunnel facilities came to life. Trustees of the Guggenheim fund included, left to right standing, J. W. Miller, secretary; F. Trubee Davison; Elihu Root, Jr.; Hutchinson Cone; Charles Lindbergh; Harry Guggenheim, the fund's president; Dr. Robert Millikan; and, left to right seated, John D. Ryan; Daniel Guggenheim, the fund's creator; Orville Wright; and Dr. William F. Durand. SI Negative No. A-3519

aeronautical laboratory in the world, an achievement highlighted in the C. G. Grey editorial reproduced at the beginning of this chapter. That a European authority acknowledged America's leading position in aerodynamic research was particularly significant, considering how far behind the United States had been just fifteen years earlier.

The NACA was not alone in the establishment of American aerodynamic research institutions during this period. The United States Army Air Service expanded its aerodynamic work at McCook Field after World War I and constructed a five-foot-diameter open wind tunnel there in 1922. An important area of aerodynamic research in this facility became the search for solutions to aircraft "flutter," the often catastrophic uncontrolled oscillations of wings and control surfaces in flight, a subject that NACA researchers also pursued vigorously beginning in the late 1920s. When McCook Field closed in 1927, the Army moved this tunnel to the new Wright Field (now Wright-Patterson Air Force Base) near Dayton, where the five-foot tunnel remained in service into the 1990s as an educational learning tool for the Air Force Institute of Technology (AFIT). And as already noted in the work of Albert Zahm, during this period the U.S. Navy also sponsored important research in the wind tunnels of the Washington Navy Yard.

Along with the government investment, there was also a growing private interest in advancing aerodynamic research in the 1920s. A leading figure in this was multimillionaire philanthropist Daniel Guggenheim, who devoted part of his mining fortune to foster the development of academic programs in aeronautics at American universities. In 1925, when only the MIT and the University of Michigan offered degrees in aeronautical engineering, Guggenheim donated half a million dollars to New York University to establish a School of Aeronautics. The next year he founded the Daniel Guggenheim Fund for the Promotion of Aeronautics with an endowment of $2.5 million. Between 1926 and 1930, the Guggenheim Fund awarded major grants to seven prestigious engineering schools, expanding the programs at MIT and the University of Michigan and starting new programs at Stanford, the California Institute of Technology (Caltech), the University of Washington, the Georgia Institute of Technology (Georgia Tech), and the University of Akron. A large portion of these grants went into construction of wind tunnel laboratories, but, significantly, some funds were earmarked for the hiring of exceptional professors and researchers. For example, a Guggenheim grant allowed Clark and Robert Millikan to recruit Theodore von Kármán, one of Europe's most outstanding theoretical aerodynamicists, to come to the United States as the new director of Caltech's Guggenheim Aeronautical Laboratory (GALCIT), where, for the next thirty years, he would play a dominant role in shaping the growth of theoretical aeronautics in the United States. By mid-century, over 90 percent of the nation's leading aeronautical engineers were graduates of Guggenheim-funded colleges.[22]

Another significant factor in the formation of the American aeronautical engineering community in the 1920s and 1930s was the diaspora of experienced research engineers from NACA Langley into the larger world of aeronautics. Dozens of early NACA employees moved on to accept important positions in the aircraft industry or at universities. For example, Montgomery Knight and Elliot G. Reid

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[22] On the Guggenheim connection with aviation, see Richard P. Hallion, *Legacy of Flight: The Guggenheim Contribution to American Aviation* (Seattle, WA: University of Washington Press, 1977). See also Hallion's chapter, "Daniel and Harry Guggenheim and the Philanthropy of Aviation," in *Aviation's Golden Age: Portraits from the 1920s and 1930s,* ed. William M. Leary (Iowa City: University of Iowa Press, 1989), pp. 18–34.

left Langley to help start the Guggenheim programs in aerodynamics at Georgia Tech and Stanford, respectively. These programs in turn produced a new crop of aeronautical engineers for the future expansion of the NACA and the civilian aircraft manufacturing industry. This process of networking and cross-fertilization with the broader American aeronautical community proved to be one of the NACA's most important contributions to aerodynamics and American aeronautics generally.

To be sure, it was people—primarily engineers and scientists—who perceived the needs of aeronautics, envisioned the flying machines and wind tunnels, brought them to life, and thought up and performed the aerodynamic test programs. With such a complex and exciting technology as aviation, it should not be surprising that the field attracted some of the finest minds available. But with brilliance frequently comes ego and a high degree of individualism, a combination that can make it difficult to establish and maintain a sense of community among professionals with different ideas and opposing modes of operation and cultural norms.

Over time, in America as elsewhere, an international aeronautical research community formed as practitioners in various countries began to realize that significant progress resulted at least as much from cooperation and the free exchange of ideas as from secrecy and cut-throat competition. Indeed, corporate and national competitions remained intense in the inter-war period. Americans for their part wanted to catch up with and surpass the Europeans in the field of aeronautics. Documents in this chapter involving the NACA's Office of Aeronautical Intelligence—primarily John Jay Ide's reports from Paris back to the NACA in Washington, D.C., on what was developing at Europe's many aeronautical centers in the early 1920s—certainly need to be evaluated with the American military and commercial goals of the early inter-war period in mind. For the United States to be on the cutting edge of aeronautical science and technology, its aeronautical specialists had to know what its European rivals were up to in their laboratories, aircraft industries, and military installations. Although complete histories of NACA and U.S. military intelligence in the field of aeronautics have not yet been written, it is clear from what is known, and from the progressive evolution of the American propeller-driven airplane into the World War II era, that the intelligence mission succeeded in major respects.

Unsavory manifestations of national loyalties surfaced from time to time just about everywhere. Without dwelling on them, this chapter offers one arresting insight into the chauvinism of an early NACA researcher, Frederick H. Norton, Langley's chief physicist from 1920 to 1923. In 1921 Norton wrote to NACA Headquarters complaining that Dr. Max Munk, only recently arrived to the NACA from Germany, had the audacity to propose the use of a German airfoil section for a small helicopter he was considering. An MIT graduate, who one might think possessed a less parochial view, Norton could not understand how NACA leadership would allow Munk to use any foreign airfoil, especially one from Germany, the wartime

Dr. Max M. Munk, the Variable Density Tunnel's creator, inspecting the machine not long after its installation. One peered into the machine through two small portals on the side. NASA Image #EL-1999-00258 (LaRC)

enemy, when there were plenty of good American airfoils from which to choose.

Munk himself continued to be embroiled in conflict; it was not easy for anyone to work with this temperamental genius, either during the early years at NACA Langley or in subsequent jobs. Whether the problems with Munk at Langley (which eventually resulted in Munk's dismissal from the NACA in 1927) represented simply a clash of personalities or something deeper like a culture clash has been a matter of some interesting historical analysis and interpretation.

One way to understand the Munk affair is to consider that two basic approaches to aerodynamic research exist: the empirical approach, where experimentation and practical assumptions seek solutions to problems; and the theoretical approach, where a mathematical analogy is created to foster an understanding that can be used to solve problems. Such a dichotomy oversimplifies the research process, unquestionably, but it serves to illustrate the mindsets of two emphases in research that definitely exist—and that sometimes even break into camps. In the era following the Wrights' first flight covered by this chapter, both empiricists and theorists contributed important elements to the overall picture, and what began in many places as a mutual lack of understanding and distrust of each other's methods slowly merged into a mature discipline with room, and a need,

Von Kármán (black coat and tie) sketches out a plan on the wing of an airplane as members of his Jet-Assisted Takeoff (JATO) engineering team looks on. Clark Millikan stands to von Kármán's far right with Martin Summerfield in between. To von Kármán's immediate right is Caltech rocket pioneer Frank J. Molina. The man in uniform is Capt. Homer A. Boushey, who later that day (23 August 1941) became the first American to pilot an airplane that used JATO solid propellant rockets. NASA Image #JATO-VONKARMAN (Ames)

for both. Yet the maturing did not take place without occasional trouble.

The roots of aeronautical empiricism penetrated deep, and dated at least as far back as Cayley. Generally speaking, all of the aeronautical pioneers from Cayley to the Wrights were experimenters who learned through careful observation of their successes and failures. By the 1920s, this long tradition had led not only to successful flight and a proliferation of practical flying machines, but also to the establishment of aeronautical laboratories dedicated to expanding the knowledge of flight through experimental means. The empirical investigations performed by several of these researchers, including Francis Wenham, Horatio Phillips, Gustav Eiffel, Albert Zahm, and the Wrights, created a firm base for later theoretical work. Faced with a dearth of knowledge about aerodynamics, they built machines with which reliable measurements could be made. In so doing, they not only learned how various objects interacted with an airstream, but also furnished themselves and others with the fundamental facts needed to construct theoretical models that could describe and predict those interactions with increasing accuracy.

In the United States during the first decades of the twentieth century, empirical methods and straightforward approaches to solving practical problems suited the mood of the engineering culture and the abilities of its practitioners well; this certainly proved to be the case at the NACA laboratories, where the engineer—not the scientist—gained the upper hand. Nonetheless, the contributions of science and, perhaps even more importantly, the public perception of science greatly influenced the course of NACA research. American scientists sought a fundamental understanding of natural phenomena, as European scientists did, but science and engineering were often seen as one and the same by the American public; typically, what engineers did—at least what they did successfully—was credited as "science." Engineering accomplishments, solutions to practical problems built out of concrete or steel rather than new knowledge for its own sake, were commonly celebrated as "scientific" achievements. In America, "science" solved problems and built things, and the field was largely open to anyone who could prove himself. Like the rest of American society, a sense of democracy permeated the enterprise. This is not to imply there was no management hierarchy in American technology—far from it—but ideas could originate from lower strata and receive consideration. Subordinates in many organizations, including the NACA, felt free to debate and test notions coming to them from the top down. Even more often, ideas percolated from the bottom up.

The European model, and especially the German, differed substantially. In the physical sciences, mathematics was seen as the route to true understanding, and the objective of an investigation was the development of a mathematical formula that described the phenomenon.[23] Although an admirable objective, this could lead to formulae that were extremely difficult, if not impossible, to solve when applied to practical engineering problems. At the very least, such a technical culture required its engineers and scientists to be good mathematicians and to think abstractly. The national technical cultures of most European nations remained hierarchical and, unless one had the benefits of aristocracy, scientists and engineers earned their positions in the upper echelons of a profession through many years of formal study and subservient work. In such cultures, the leader of an organization held a commanding position over the ideas and methods that the entire organization would use.

Ludwig Prandtl, Max Munk, and Theodore von Kármán were outstanding products of the German system and, not coincidentally, they proved to be among

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[23] See Paul A. Hanle, *Bringing Aerodynamics to America* (Cambridge, MA: MIT Press, 1982), for an insightful analysis of the "Prandtl School" and the German technological culture of aerodynamic research in the early 1900s.

the first and finest theoretical aerodynamicists. Prandtl led the way for his two brilliant students. In demonstrating his ability to tackle practical problems in innovative ways by building the first closed wind tunnels, he built, not successful airplanes, but an edifice of fundamental aerodynamic understanding that was unexcelled anywhere. Prandtl wanted not to observe and measure lift so much as he wanted a mathematical explanation of it; this ambition underlay most of his published papers, including his classic statement of 1921, "Applications of Hydrodynamics to Aeronautics," written expressly for NACA publication (excerpts of which are published in this chapter's documents). In this quest for theoretical enrichment, Prandtl was not alone. Other contemporary Europeans, notably Nikolai Joukowski in Russia and F. W. Lanchester in England, also devoted most of their careers to a search for satisfying theoretical explanations of this complex phenomenon. None of them completely solved the problem, but all of them contributed vital pieces to the puzzle. Munk and von Kármán acquired their mentor's appreciation of the value of theoretical aerodynamics, and they both brought it, along with their considerable intelligence and talent, to the United States in the 1920s. Both men were well known to prominent American aeronautical figures, notably to Hunsaker and Zahm, who recommended them highly. Once in America, however, their careers took decidedly different directions.

Von Kármán, who arrived later (in 1929), came to the U.S. after being actively recruited by Caltech's Robert Millikan. He moved into a university research environment that shared characteristics with his former school, the Technische Hochschule in Aachen, Germany. At Caltech, von Kármán directed GALCIT, but he also continued to work with graduate students and pursue his own research. He adapted well to academic life in America, and he quickly gained the respect of the American aeronautical community.[24]

Munk's American career started out perhaps even more meteorically but later came crashing down. Munk started his NACA employment in the early 1920s, working almost exclusively by himself on theoretical problems in the Washington, D.C., office. Besides refining and carrying out his idea for the VDT, he explored a number of critical aerodynamic problems and published an important series of reports. (The NACA would eventually publish over forty of Munk's papers.) But Munk wanted to be in charge of aeronautical research at Langley.

Only a few of the many documents that exist in the NACA archives related to problems with Munk need to be seen to appreciate the clash of ideas and attitudes that ultimately led to a revolt against Munk at Langley—and to his forced departure from the NACA. One of them published in this chapter involved Munk's

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[24] Theodore von Kármán's life is covered in fascinating detail in Michael H. Gorn's *The Universal Man: Theodore von Kármán's Life in Aeronautics* (Washington, DC: Smithsonian Institution Press, 1992).

supervision of the construction of the VDT. In a 6 October 1921 memo to Washington, Langley Chief Physicist Frederick Norton expressed resentment at Munk's overbearing manner and vague directions during construction of the tunnel, complaining that "Dr. Munk does not seem to have any clear idea as to what he wishes in the engineering design, excepting that he is sure that he does not want anything that [I or my men] suggest." The best story about the difficulties of working with Munk comes from Langley engineer Fred E. Weick's autobiography *From the Ground Up* (Washington, DC: Smithsonian Institution Press, 1999), an excerpt from which is included in this chapter. Moving from the Navy Bureau of Aeronautics, where he worked as a civilian engineer, to the NACA in 1925, Weick took on the responsibility of actually building the Propeller Research Tunnel, which was also a Munk concept. Unfortunately for Weick, Munk's incredibly demanding supervision of the PRT work nearly drove Weick crazy trying to find ways to please the man and still do the job correctly. To Weick's credit, he found a way of working with—and mostly around—Munk, and the facility was successfully built.

Other documents in this chapter reveal different perspectives on the Munk affair. A 16 November 1926 memo from Munk to George Lewis, the NACA's director for research in Washington, D.C., indicated that Langley was "at present pretty well filled up with problems; we are really overstocked." On the surface, Munk's memo simply expressed concern over the need to give "the fullest amount of thought and interest" to the research problems his laboratory staff was already busy exploring. Reading between the lines, however, one senses that Munk may actually have been more concerned about his own personal control of the research program at Langley and wanted to prevent new ideas from reaching NACA Headquarters that he had not evaluated first.

Relations with Munk were difficult from the start, even when he spent most of his time in Washington; the problems definitely intensified when George Lewis sent him to Langley for extended periods. The Langley engineers considered Munk an arrogant outsider, and Munk's efforts to fit in, to the extent he made them, failed miserably. Perhaps it was because he saw himself as intellectually and technically superior to those around him—and made that belief clear to others, insisting they conform to his own ideas and conclusions. While they respected Munk's abilities, the engineers at Langley, many of them capable people in their own right, balked at his autocratic rule. Resignations of key people, like Norton, began in 1923; the rate increased after Munk was assigned to Langley as chief of aerodynamics in 1926, culminating with the mass resignation of all the section heads less than a year later. Lewis still hoped to retain Munk in Washington, but Munk, not yet grasping the "culture-shock" nature of the problem, chose to leave the NACA. When he left, the section heads returned.

Even though Munk's personality doomed him at Langley, he succeeded in introducing theoretical aerodynamics to the laboratory, and he helped to usher in an important change in the way the LMAL worked. Munk, like Prandtl, saw the connection between empirical and theoretical aeronautics and how each could help advance the other. One of his seminal papers, “General Theory of Thin Wing Sections,” published by the NACA as Technical Report 142 in 1922, resulted from his conviction that theoretical aerodynamicists would not be able to generate improved airfoils from scratch using only the mathematical methods suggested by Kutta and Joukowski. Instead, Munk reversed the process. He decided to start with an empirically proven airfoil and then fit an analysis to it. Starting from a known design allowed him to validate the theory, something that had eluded many theorists. His theory was not perfect, but it allowed for an easier and more accurate prediction of wing performance. Over the next few years, a small group of scientists and engineers with better analytical capabilities and backgrounds joined the LMAL staff. Although conflicts between the empiricists and the theorists never disappeared totally, empirical and theoretical results came increasingly into agreement, as both the experimental tools and the analytical methods improved. Inexorably, the two camps pulled together into a mature discipline.

By the early 1930s, aeronautical research and development had come of age in the United States. Vigorous debates about the quality of the direction being followed by the NACA still took place, notably the fiery public back and forth in 1930 and 1931 between *Aero Digest*’s opinionated editor Frank Tichenor, the NACA’s main detractor, and *Aviation*’s Dr. Edward P. Warner, its principal defender. (Their exchange of editorials is included in this chapter’s documents.) But, in the opinion of most observers and colleagues in the aeronautics community, the program of research being pursued by the NACA by the 1930s, in cooperation with the military air services, the aircraft industry, and the universities, seemed to be exactly what the country needed not just for better airplanes but for global aeronautical hegemony.

In two decades’ time, American aeronautical research had grown from having virtually nothing in the way of laboratory facilities to a position of preeminence in the world, with more than twenty major wind tunnels in operation for universities, aircraft manufacturers, the military, and the NACA. Equally important were the bright, trained, and experienced researchers who could effectively use these experimental tools and increasingly powerful analytical methods—combined with their own innate creativity and vision—to generate new knowledge and solve the fundamental problems facing a rapidly growing technology. The stage was set for an honest-to-goodness revolution in aerodynamics and aircraft design.

The Documents

Document 2-1(a–d)

(a) Wilbur Wright, letter to George A. Sprat, 15 December 1901.

(b) Wilbur Wright, letter to Octave Chanute, 15 December 1901.

(c) Wilbur Wright, letter to Octave Chanute, 23 December 1901.

(d) Wilbur Wright, letter to Octave Chanute, 19 January 1902.

All of the above documents are found in
***The Papers of Wilbur and Orville Wright*, 1899–1948,**
Marvin W. McFarland, ed. (New York, NY: McGraw-Hill), 1953.
The originals are in the Library of Congress, Washington, D.C.

The successful flights of Wilbur and Orville Wright on 17 December 1903 were due in no small part to the brothers' systematic pursuit of aeronautical knowledge. Recognizing that tables developed by John Smeaton and Otto Lilienthal—tables the brothers had used in designing their disappointing 1900 and 1901 gliders—included significant errors, the Wrights designed and built novel research devices and used them systematically to generate reliable data that pointed to a successful design. As he did throughout the project, Wilbur Wright frequently communicated their progress and questions with several prominent figures in early aviation. These letters, a sample of many written during the Wrights' wind tunnel, or "trough," experiment period, discuss their test results, how these results compared to Lilienthal's data, and their efforts to obtain "a perfectly straight current of wind" for the tests. While the work was distinctly empirical in nature, the details of measurement and experiment design, especially those noted in Wright's 15 December 1901 letter to Octave Chanute, clearly show the brothers' attention to accuracy and repeatability—vital elements of any empirical program. Also of interest is Wilbur's 23 December 1901 rejection of Chanute's offer to try and interest Andrew Carnegie in supporting the Wright's work (in a 19 October 1901 letter not included herein) and his pragmatic analysis of an aviation competition in a letter to Chanute dated 19 January 1902.

Document 2-1(a), letter from Wilbur Wright to George A. Sprat, 15 December 1901.

We were pleased to receive your letter and the photograph of your new testing machine. It seems quite ingeniously designed and I think should give good results. As you say, the greatest trouble will probably be with the changeableness of the wind. If I understand you properly, the machine is intended for locating the center of pressure at any angle (or rather locating the angle for any center of pressure), and for finding the direction of the resultant pressure as measured in degrees from the wind direction, so that the ratio of lift to drift is easily obtained, the lift being the cotangent and the drift being the tangent of the angle at which the arm stands. Does the machine also measure the lift, in terms of per cent of the pressure at 90°? so that you can make tables like that of Lilienthal?

I think I told you in my last that we had been experimenting with a lift measuring machine. We have carried our experiments further and have made a measurement of the lifts of about 30 surfaces at angles of 0°, 2 ½°, 5°, 7 ½°, 10°, 12 ½°, 15°, 17 ½°, 20°, 25°, 30°, 35°, 40°, & 45°. The results have rather surprised us as we find at angles of 7° to 15° *with some surfaces* a greater lift than Lilienthal gives in his table. Our #7 surface, which is a rectangle 1:6 with a depth of curve of 1⁄12 the chord, has a *lift* of one hundred and nineteen per cent at 17 ½ degrees. Lilienthal only claims about 80 per cent. But at 3° our measurement is way below him. I will try to send you a blueprint showing the lifts of some of the surfaces we have tested. Some surfaces which lift big at very small angles are no good at large angles & *vice versa*. We have not attempted to trace the travel of the center of pressure except that by holding some of the surfaces between the tips of our fingers we were able to roughly determine which ones tended to reverse and which did not. It seems that surfaces with rather flat upper sides and thickened front edges lift more *at small angles* than plain curves and have little reversal of the travel of the center of pressure. Thickening the front edge does not seem to add near as much to the drift as I expected though it adds some.

We have found less drift with surfaces 1⁄20 deep than with curves 1⁄12 deep. What is your experience?

Document 2-1(b), letter from Wilbur Wright to Octave Chanute, 15 December 1901.

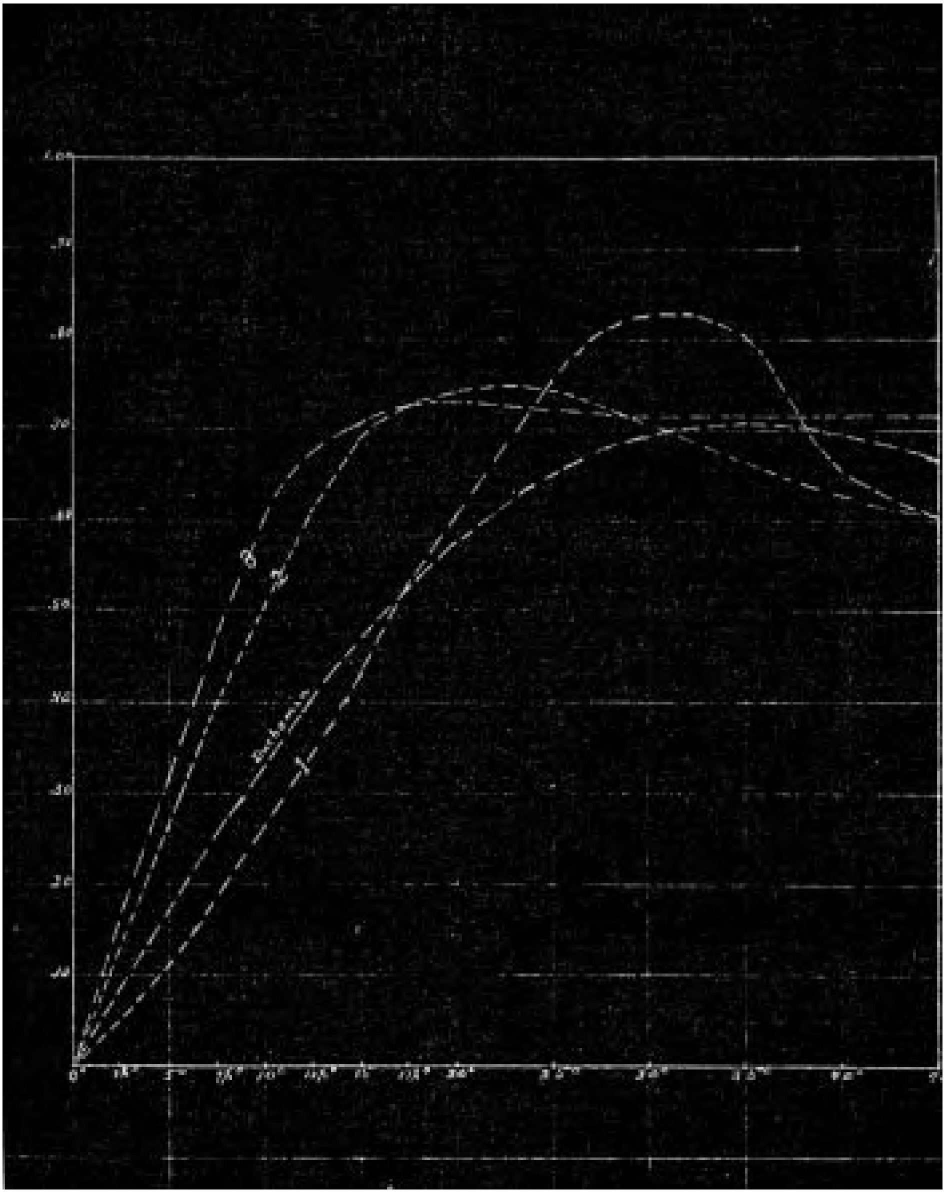
I have your letter of 11th with enclosures and have read all with much interest. Moedebeck evidently is a balloonist rather than an aviator. Mr. Lilienthal says that the results obtained by his brother with the apparatus *exactly coincided* with their joint measurements. My study of Otto Lilienthal's writings leads me to question whether this is not too strong a term. After Lilienthal began gliding I do not recall that he ever recommended 3° as being a specially desirable angle of incidence. In his calculations of glides he invariably uses angles of from 9° to 12° incidence. If he had found the great advantage in the smaller angles which the tables pronounced best, he would surely have mentioned it. From this I am led to think that Lilienthal himself had noticed that there was a discrepancy between his glides and his tables, at small angles especially. There is also some question whether a surface about 8 ft. x 20 ft., that is, 1:2.5, would give the exact result obtained with the surface of very different aspect which was used in the experiments on which the tables were based.

Prof. Marvin's article has evidently been prepared with some care and is well worth close study. I quite agree with him that data of a glide in which the forces are not in equilibrium throughout the glide or the portion of it on which the data are based must be worthless for purposes of calculation. You will remember that at Kitty Hawk we said that the #3 glide of Aug. 8th, A.M., was really the only one of much value for purposes of calculation because in it, alone, the speed from the instant of starting to the instant of landing was very nearly uniform. The angle of incidence varied scarcely at all; the line of motion was exactly opposite to the direction of the wind so that the conditions were equivalent to a glide of about double the speed in still air. Owing to the practical impossibility of obtaining correct data I have never considered glides of very great value for purposes of calculation.

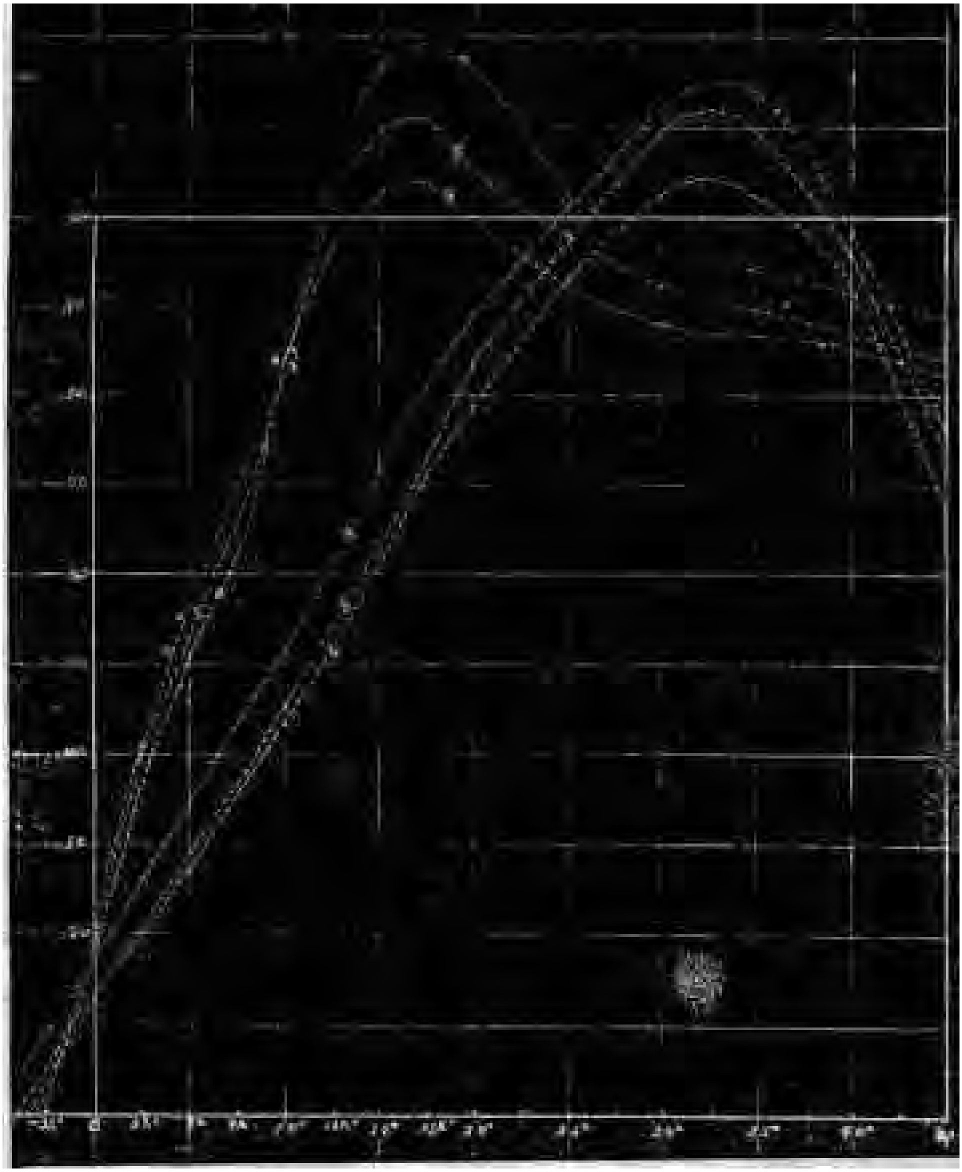
I notice that Prof. Marvin holds about the same view on the tangential that I held last July, *viz.*, that the front edge must be lower than the rear edge or the bird or machine can not go forward. But the sight of a buzzard which maintained its speed with its wings constantly pointed *above* the horizon, and the fact that our machine pulled less than the weight X tang. of the angle of incidence, in spite of the head resistance of the framing, led me to suspect that Lilienthal might be right about the tangential. Our recent experiments are so clear on this point that I can no longer doubt that a suitably arched surface can glide forward in a descending course of ten degrees with the front edge of the surface pointed as much as *five degrees above the horizon*.

I send you blueprints of some of the preliminary charts of our recent measurements of lifts. They were made on common paper so that the blueprints from them scarcely show the small squares, and this makes it hard to read the exact values, but you can see the general result. The lines run *exactly according to our observations* at the points at which measurements were made, *viz.*, 0°, 2 ½°, 5°, 7 ½°, 10°, 12 ½°, 15°, 17 ½°, 20°, 25°, 30°, 35°, 40°, & 45°. In connecting these points we have used our best judgment as to the course in the intervening spaces. I am not sure but that there is room for some improvement as to this. I send three of our series charts and one large one with a selection of surfaces of various types.

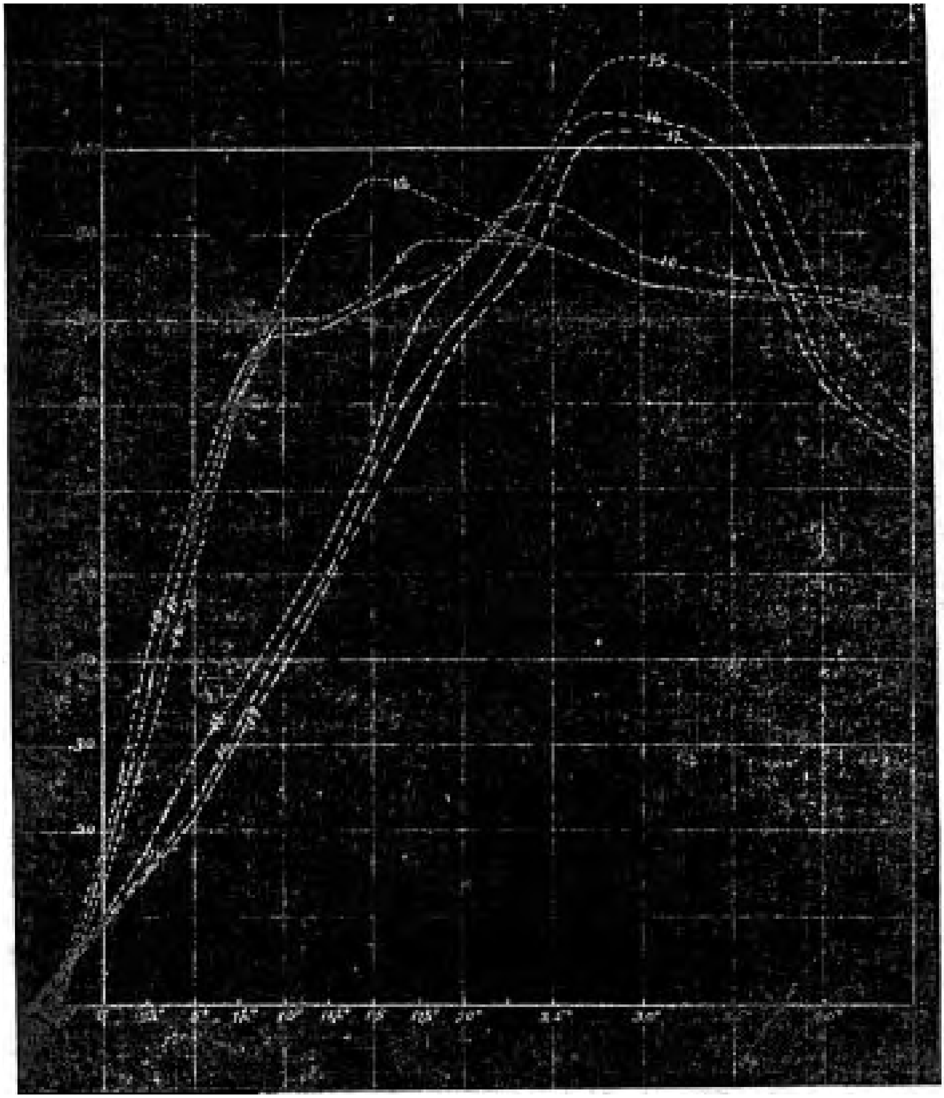
The aspects and curvatures are shown on the back of the large sheet. On the whole the finished charts give me a better impression of the accuracy of the



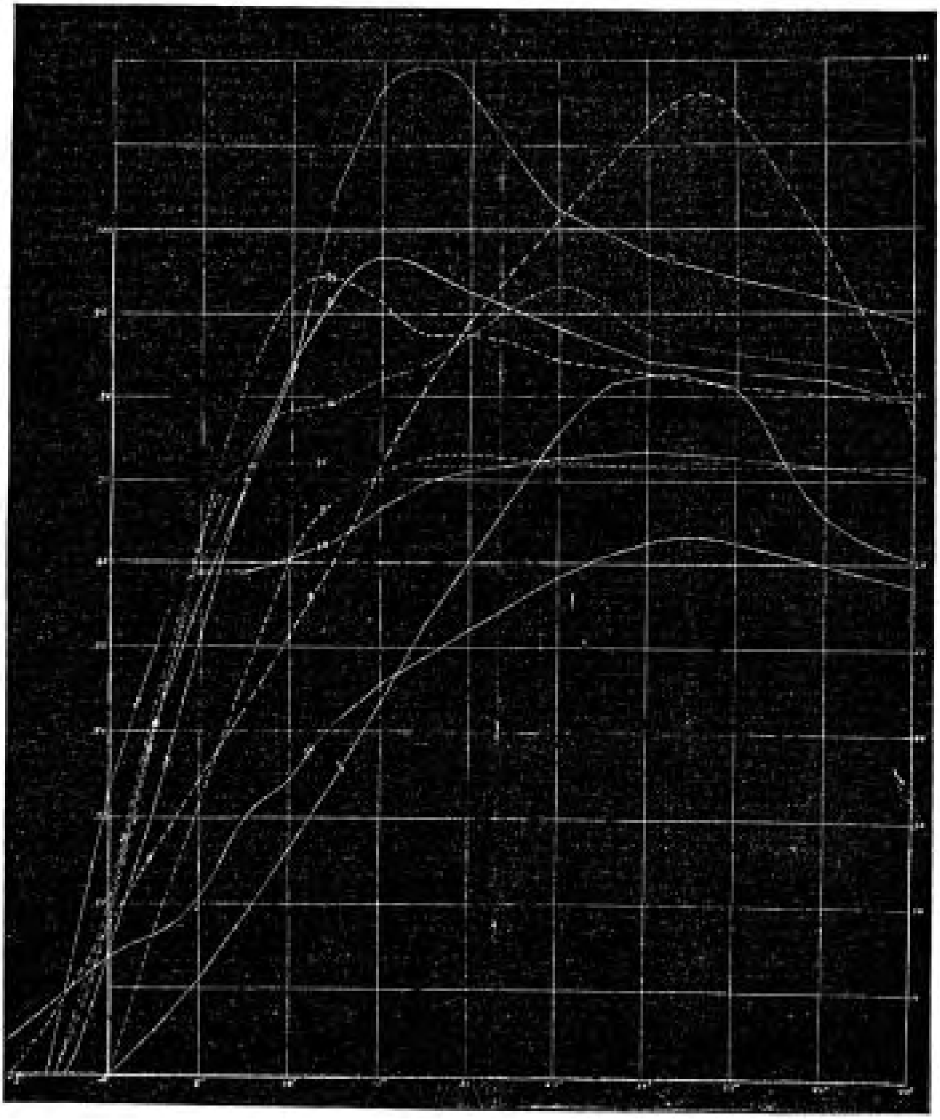
results than I had after plotting the first few lines, as I find that many kinks in the lines, which at first seemed to be due to errors of the observations or imperfections in the machine, are now seen to be due to the surfaces themselves. Thus when #7 was plotted we found an uncalled-for depression at 10° and a hump at 12 ½°, and though we verified the observations we were not entirely easy in our minds till we plotted #8 and #9 and found that they had exactly corresponding depressions and humps. The fact that the hump on #9 comes exactly at the depression on #7 makes #9 the greater lifter at 10°.



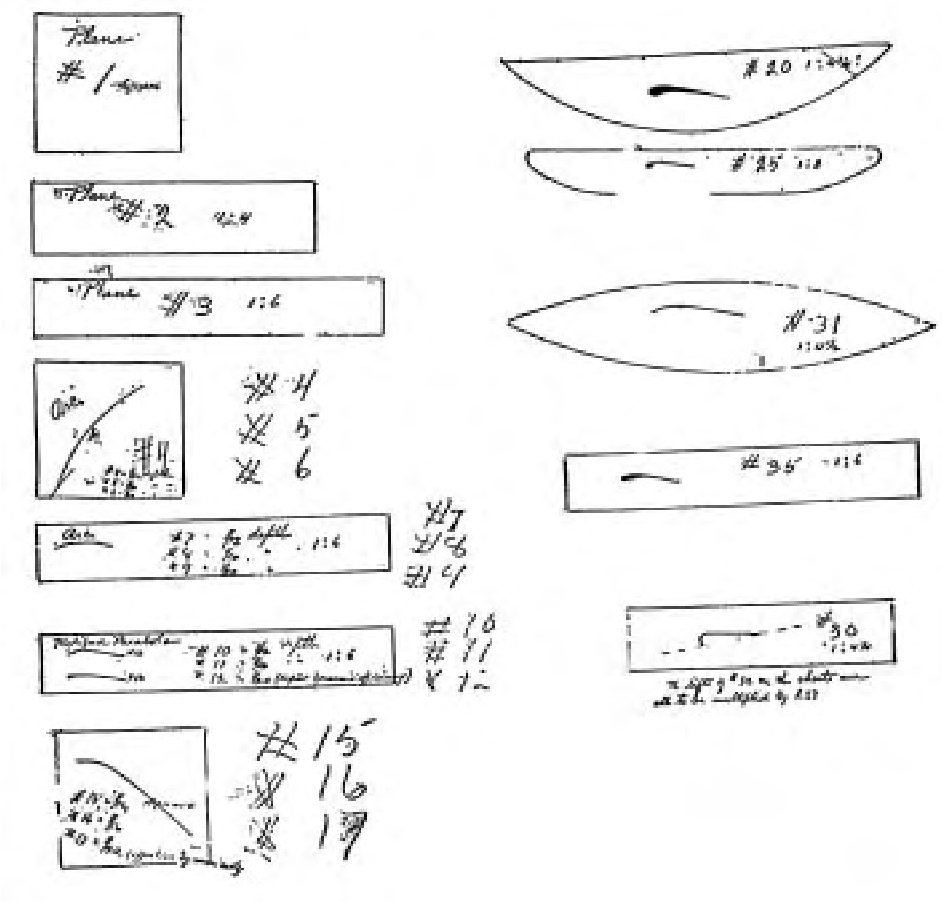
You will also note that #4, #5, & #6 have corresponding depressions at 10°–12° and humps at 17°–20°. When we first measured #10 we struck a snag at 12 ½°, for the machine, which at the lower angles had been recording a reasonably regular increase in lift with each successive observation, suddenly refused to indicate any increase and, though we examined the machine carefully and verified the angle of incidence, we could not believe that the observation was correct till we had remeasured 10° also. Afterward we found that this was a characteristic of all surfaces having the curvature well to the front. Arcs rise in regular peaks while parabolic surfaces show summits like volcanoes, the crater being more or less marked according to the depth of curvature.



It has been a great advantage we think to make a systematic measurement of several typical series of surfaces rather than to work blindly on all sorts of shapes, as a study of the series plates quickly discloses the general principles which govern lift and tangential and thus renders the search for the *best* shapes much easier. A mere glance shows that while increasing the ratio of breadth to length does not increase the maximum lift to any great extent it does cause the maximum to be reached at a smaller angle; and the wider the spread from tip to tip the smaller the angle at which large lifts can be obtained.



By comparing #1, #4, #5, & #6 it is seen that the effect of curving the surface is to give a steady increase in the lift of all angles without affecting the angle of maximum pressure. Varying the depth of curvature has a less marked effect than the experiments of Lilienthal would indicate in the matter of lift, but when we come to consider tangential the difference is very marked. The great advantage of moving the maximum curvature well forward is in the matter of center of pressure, though it seems also to cause an increase in lift at smaller angles, and in general gives a slightly more favorable tangential at angles of 4°–10°. Thickening the front edge has a marked tendency to give big lifts at small angles—*vide* #20, #25, & #35; and is even better than moving the curvature forward in its effect on center of pressure. It somewhat increases the head resistance, however, though not near so much as the great increase in thickness might be expected to cause.



The charts showing the effect of superposing are not completed, but there seems to be some indication that they will tend to establish a general law that (eliminating connections) the lift and tangentials of a set of superposed or following surfaces spaced about their length apart [are] approximately equal to [those] of a single surface of similar profile or curvature having a breadth equal to that of one surface and a length equal to the sum of their lengths. That is, two 1:6 surfaces would give the same results as one 2:6 surface; and four of them that of a 4:6 surface. From this it would appear that superposing reduces the efficiency of the individual surfaces. In considering a double-deck or triple-deck machine, as compared with a single surface of equal area, it would seem the single surface can be cut up as in Fig. 1 and the parts superposed without loss, but if cut as in Fig. 2 there is a loss. Superposing may be used to reduce the fore-and-aft dimension but not the lateral.

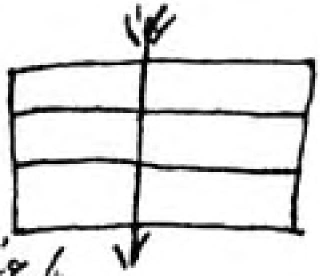


Fig 1.

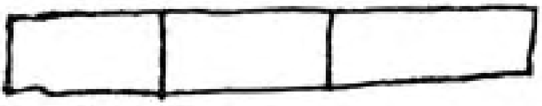
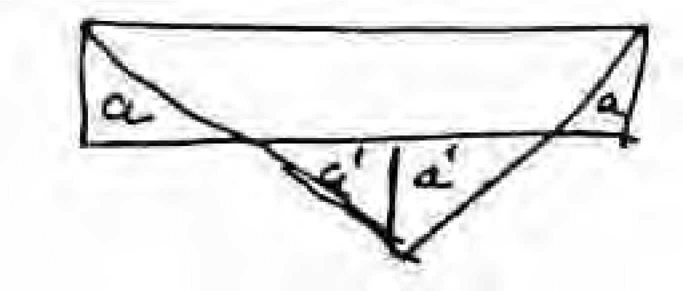


Fig 2.

A somewhat similar law seems to hold when comparing rectangles with segments or triangles. Surface may be cut off at one point and added at another without affecting the efficiency of the surface. Thus if the corners be cut off at *aa* and added at *a'a'* thus forming a triangle, the effect remains about the same, so long as the spread from tip to tip remains the same. It is at least roughly true that both in superposing and in reshaping a single surface the efficiency depends on the ratio of maximum breadth to area, rather than on the ratio of breadth to length.

I regret that we did not have time to carry some of these experiments further, but having set a time for the experiments to cease, we stopped when the time was up. At least two thirds of my time in the past six months has been devoted to aeronautical matters. Unless I decide to devote myself to something other than a business



career I must give closer attention to my regular work for a while. I hope at some later time to resume these investigations and also to carry out a plan I have considered of obtaining an accurate measurement of the value of P90.

You will note that #7 reaches a lift of 119 per cent at 17 ½°, a greater amount than any preceding investigation has found, so far as I am aware. And that #4 reaches almost as high at 30°. By means of an entirely different instrument I have confirmed the fact that the *normal pressure* of #4 at 20° (twenty) is slightly more than equal size at 90°. At 30° the normal pressure of #4 is much greater than the normal plane. At about 45° the normal pressure of #4 is equal to the normal pressure of a plane at about 70°. If these high values of #7 and #4 at 17 ½° and 30° are too high, then the lift of #1 at 17 ½° is also too high in the same ratio. In fact all the measurements of all the surfaces were made under conditions which make all the measurements true if any of them are true.

I will return the Marvin document in a few days.

[p.s.] #12 has the highest dynamic efficiency of all the surfaces shown.

Document 2-1(c), letter from Wilbur Wright to Octave Chanute, 23 December 1901.

I am returning the Marvin papers herewith. I should have sent them much sooner but that I hoped to be able to complete some notes to send with them. I find however that it is more difficult than I expected and though I have made several attempts I have not yet been able to get everything shaped up to suit my ideas of the real operation of the forces which arise in glides. I think that the *real* angle of incidence should be not the angle bounded by the chord of the surface and the line of the path, but an angle bounded by the line which marks the negative angle at which lift begins and the line of the *relative wind*. The true normal should be perpendicular to the line of no lift instead of to the chord, and the tangential is the deflection of the direction of the *resultant pressure* from this true normal. You will see that all this greatly increases the complication of calculations but I see no other way of arriving at theoretically perfect results. For rough work much of this refinement would be unnecessary, but in *defining the meaning of terms* we should aim to get them *exactly* as they really are. I will send you my ideas on some of these points as soon as I can get them properly straightened out. I get lost now and then.

I return the Langley letter which I have read with regret. I was not unprepared for his decision as some remarks Mr. Huffaker made last summer gave the impression that Lilienthal was not in high favor among the Washington group of workers for some reason. Mr. Huffaker seemed to think that Lilienthal had been overestimated. Since seeing his book I can not help thinking that he is underestimated, and that he will stand even higher when the doubts with which some of his most important discoveries have been accepted are finally cleared away. It seemed to me that the publication of his book in English would not only be of very great value to all aeronautical workers but it would be a well deserved tribute to the memory of a man who spent much money, an immense amount of his time, and finally his life in carrying out investigations which he gave freely to the world.

Your offer to assist in figuring out the results of our recent experiments is thankfully received. The labor itself is not so tremendously great, though there are several days' work required; but I have felt the need of a verification of our calculations to guard against blunders. If you desire I will send you our data as read from the machine, with directions for translating them into per cent of the pressure on a square plane of equal area at 90°. I will also send you photographs of our instruments and try to make it clear just how they operated. As to the accuracy of the results I think I am very safe in saying that the possible error is less than one twentieth. I think the average is much closer than this. We spent nearly a month getting a *straight* wind, but finally were able to get a current whose direction did not vary one eighth of one degree. A possible error of one twentieth, or 5%, is by no means insignificant but it does not greatly reduce the practical value of the tables. For purposes of comparing the lifts of different surfaces at the same angle or different angles, or of comparing the relative lifts of the same surface at different angles, the tables will be more accurate still, as some of the most serious sources of error are eliminated when the exact value of the common resistance against which the surfaces are weighed is not required to be fixed. I think about 2% would easily cover the errors which could arise from errors in mounting the surface at the exact angle desired, variations in wind direction, and errors in reading. This of course does not include mere blunders.

Our measurements of the tangential include the edge resistance of the surfaces and I can devise no way of eliminating this from the measurements. On the whole I am inclined to think that it is possible with large surfaces to get rather more favorable tangentials than the small surfaces show. The thickness of our regular surfaces was about three per cent of the fore-and-aft dimension. This you will notice is more than the relative thickness of the largest spars of our gliding machines. We did not use thinner material because it was deemed to be of the utmost importance that the surface should be sufficiently strong and stiff to prevent any twisting or warping under pressure of the wind. A distortion too small to be noticed and almost too small to be measured might cause quite serious errors in measurements where great exactness was desired. I am inclined to think that this has been a more serious matter than most investigators have supposed.

It was with very great reluctance that we discontinued our experiments at the time we did, but there were so many things yet to be investigated that it was very evident that we would be unable to spare the time to carry them through at the present. We got all that we originally set out for, so we thought it a favorable time to take a recess. Then, too, we saw that any further time consumed now would seriously impair our chance of a trip to Kitty Hawk next fall.

As to your suggestion in regard to Mr. Carnegie, of course nothing would give me greater pleasure than to devote my entire time to scientific investigations; and a salary of ten or twenty thousand a year would be no insuperable objection, but I think it possible that Andrew is too hardheaded a Scotchman to become interested in such a visionary pursuit as flying. But to discuss the matter more seriously, I will say that several times in the years that are past I have had thoughts of a scientific career, but the lack of a suitable opening, and the knowledge that I had no special preparation in any particular line, kept me from entertaining the idea very seriously. I do not think it would be wise for me to accept help in carrying our present investigations further, unless it was with the intention of cutting loose from business entirely and taking up a different line of lifework. There are limits to the neglect that business will endure, and a little pay for the time spent in neglecting it would only increase the neglect, without bringing in enough to offset the damage resulting from a wrecked business. So, while I would give serious consideration to a chance to enter upon a new line of work, I would not think it wise to make outside work too pronounced a feature of a business life. Pay for such outside work would tend to increase the danger. The kindness of your offers to assist, however, is very much appreciated by us.

Document 2-1(d), letter from Wilbur Wright to Octave Chanute, 19 January 1902.

I am sending you herewith photo and description of our pressure-testing machine. It is our belief that the method and construction employed entirely avoid errors from the following sources: (1) Variation in wind velocity; (2) Variation in temperature and density of the atmosphere; (3) Travel of center of pressure; (4) Variation in angle of incidence owing to movements of the mounting arms. The first two causes gave Mr. Langley trouble; while the 3rd & 4th vitiate somewhat the *natural wind* experiments of Lilienthal. Gravity and centrifugal force are also rendered nugatory.

Our greatest trouble was in obtaining a perfectly straight current of wind, but finally, by using a wind straightener, and changing the resistance plane to a position where its ill influence was much reduced, and also by breaking it up into a number of narrow vertical surfaces instead of a single square, we obtained a current very nearly constant in direction. The instrument itself was mounted in a long square tube or trough having a glass cover. After we began to make our record measurements we allowed no large object in the room to be moved and no one except the observer was allowed to come near the apparatus, and he occupied exactly the same position beside the trough at each observation. We had found by previous experience that these precautions were necessary, as very little is required to deflect a current a tenth of a degree, which is enough to very seriously affect the results. I will send another batch of data in a few days.

Your letter from St. Louis of course interested us very much. The newspapers of yesterday announce that the fair will be held in 1903 as originally planned. If this be final there will be little time for designing and building a power machine which is, I suppose, the only kind that could hope to be awarded a prize of any size. Whether we shall compete will depend much on the conditions tinder which the prizes are offered. I have little of the gambling instinct, and unless there is reasonable hope of getting at least the amount expended in competing I would enter only after very careful consideration. Mathematically it would be foolish to spend two or three thousand dollars competing for a hundred thousand dollar prize if the chance of winning be only one in a hundred. However we shall see about the matter later.

Meanwhile it will be just as well for me to postpone the paper on our late experiments on pressures & tangentials till we have decided whether or not we shall compete, as it would be hardly advisable to make public information which might assist others to carry off the prize from us. If the exposition authorities should deem it advisable to offer some preliminary prizes for papers on such subjects with a view to getting into the hands of all the competitors the best possible information and thus rendering the final contest of machines more exciting, I would place our tables in competition, but otherwise we ought to delay publication for a short time at least. This injection of the mercenary idea into the flying problem is really a nuisance in some respects.

Document 2-2(a–c)

(a) Washington Irving Chambers, excerpt from "Report on Aviation," 21 September 1912, *Annual Report of the Secretary of the Navy for 1912* (Washington, DC, 1912).

(b) Jerome C. Hunsaker, Assistant Naval Constructor, *Report on Facilities for Aeronautical Research in England, France, and Germany, Part III—Germany*, undated (ca. November 1913), NASA Record Group 255, Hunsaker Biography File, Entry 3, Box 12, National Archives, Washington, D.C.

(c) Albert F. Zahm, excerpt from *Report on European Aerodynamical Laboratories* (Washington, DC: Smithsonian Institution, 1914).

A few months after the untimely death of Wilbur Wright in 1912, Captain Washington Irving Chambers of the United States Navy submitted "Report on Aviation" to the Navy Department's Bureau of Navigation. Chambers began his report by outlining possible uses of aircraft in naval warfare. Concerned about the lagging state of aviation research in the United States, especially when compared to what was being carried out in Europe, Chambers concluded his report with a detailed proposal for the establishment of "a national aerodynamical laboratory" in the United States. It is this part of the document that is reproduced here. While Chambers's specific recommendations were not acted on at the time, his report stands as a far-sighted statement of the need for a national research establishment.

Following the success of the Wright Brothers, European interest in aeronautical research intensified, but, incredibly, it languished for a decade in the United States. Between 1903 and the start of World War I, no less than ten wind tunnels began operation in Europe, while only two were built in America. Much of the European work was due to a realization, particularly after the Wrights' 1908 demonstration flights in France, that airplanes could provide a military advantage, something of great value in an increasingly antagonistic Europe. In England, Italy, and Germany, governments invested in both airplanes and new aeronautical laboratories, such as the one at the National Physical Laboratory in London and a laboratory at Göttingen, Germany, that featured an innovative, closed-circuit wind tunnel. Private funds underwrote new facilities in Russia and France, including two laboratories built by Gustav Eiffel in and near Paris. The United States, on the other hand, cared little about the looming catastrophe in Europe, preferring to remain neutral and, thus, seeing little need for an investment in military aviation. The long Wright-Curtiss patent fight over ownership of rights to the airplane further dampened America's enthusiasm for airplanes and aeronautical development.

Within the military, a few farsighted naval officers, including Washington Irving Chambers, David W. Taylor, and Jerome C. Hunsaker, realized the long-term importance of aviation to the military and lobbied for an aeronautical laboratory. An investigative board chaired by Chambers recommended a national aerodynamic laboratory in its 1912 "Report on Aviation." This report includes considerable detail about the nature of the work to be done in such a laboratory and the necessary facilities, and it goes so far as to suggest a joint military-civilian agency "advisory committee" to direct the operation. Although Chambers' recommendation for joint military-civilian management angered his superior officers—unlike Taylor, Hunsaker, and other pioneers of naval aviation, Chambers was never promoted to rear admiral—his report described a research organization very close to the one adopted for the National Advisory Committee on Aeronautics three years later.

The Chambers report also inspired the navy to take action on its own. Taylor, assisted by two lieutenants and Zahm, was developing a design for a closed-circuit wind tunnel. With encouragement from the Chambers Report, Taylor managed to obtain the funds to build it, and the eight- by eight-foot Washington Navy Yard tunnel, the first wind tunnel built by the United States Government, began operation in March 1914.

While America did not invest in aeronautical laboratories to the extent that European nations did before World War I, a few interested parties stayed abreast of the European developments and pressed for the establishment of such facilities in the United States. Supported by the Smithsonian Institution, Catholic University Professor Albert F. Zahm and Assistant Naval Constructor Jerome C. Hunsaker, two of the leading figures in American aviation, visited the major European research facilities in 1913 to evaluate their capabilities and progress in aeronautics. Hunsaker first submitted his findings to his superiors in the navy in an extensive three-part report. Part III of his *Report on Facilities for Aeronautical Research in England, France, and Germany* covers six facilities and gives a good picture of the state of the German aeronautical art. The work of Ludwig Prandtl at the University of Göttingen particularly impressed the perceptive Hunsaker. While he found the Göttingen work to be "characterized by poor equipment made the most of by men of extraordinary ability," he noted that the Kaiser Foundation was underwriting a new wind tunnel there. Hunsaker's report also mentions Prandtl's theoretical work in computing dirigible resistance using hydrodynamic theory, work that would be a major foundation for the development of theoretical aerodynamics.

Zahm's *Report on European Aeronautical Laboratories*, a public report of the same inspection trip published by the Smithsonian Institution in 1914, described the European research laboratories and apparatus in considerable detail, but this report did not contain the kind of analysis and insightful evaluation of the facilities and work that can be found in Hunsaker's report. Nevertheless, Zahm's report showed that America was considerably behind the Europeans. This report, being a published document, was widely circulated in aviation and political circles, and it proved to be the catalyst that spurred the creation of the National Advisory Committee for Aeronautics.

Document 2-2(a), Washington Irving Chambers, excerpt from "Report on Aviation," 21 September 1912.

INFLUENCE OF FOREIGN LABORATORIES

Little more than a year ago our knowledge of the effect of air currents upon aeroplane surfaces was almost entirely a matter of theory. The exact information available was so meager that aeroplanes were built either as copies, slightly modified, of other machines, or else by way of haphazard experiment. This state of affairs pertains to some extent in the United States today, although in Europe aeroplane construction is now largely based on scientific data obtained at notable aerodynamic laboratories.

The intuitive, hasty, and crude methods of the pioneer can not succeed in competition with the accurate and systematic methods of the scientific engineer, and it is beginning to dawn upon our perceptions that through lack of preparation for the work of the scientific engineer, i.e., through delay in establishing an aerodynamic laboratory, a waste of time and money, a decline of prestige, and an unnecessary sacrifice of human life has already resulted.

Students of aviation do not need to be informed of the practical necessity for aerodynamic laboratories. They have repeatedly pointed out, in aeronautical publications, the immense commercial advantages to be anticipated from the establishment of at least one in this country, and they have naturally expected that some philanthropic patriot of wealth and scientific interest would come to the rescue with a suitable endowment fund that would enable such work to be started in short order without Government aid. The fact that no patriot has responded is disappointing, in view of the large private donations that have done so much for aviation in France, but in my opinion, it simply indicates something lacking in the manner of disseminating information concerning the importance of the subject. I am not willing to believe that our people will refuse to establish one when they are fully acquainted with the advantages to humanity and to sane industrial progress, and when a reasonable concrete proposition is advanced for their consideration.

It is now my purpose to submit such a proposition, and, in doing so, I will follow briefly, in general outline, the ideas advanced in an address to the Fifth International Aeronautic Congress by one of the greatest authorities in the world, the Commandant Paul Renard, president of the International Aeronautic Commission.

A NATIONAL AERODYNAMIC LABORATORY

Before considering the character of the work to be done and some details of the needed plant, it will facilitate matters to show what should not be done at such a laboratory.

There are those who dream of supplying the laboratory with all the instruments known to mechanics, to physics, and even to chemistry, in order to have a creditable and complete national institution. They would concentrate in one locality all the scientific instruments and acumen available, with the false idea that economy would result. This would be a grave error.

The financial resources, however great, are sure to be limited, and a too ambitious or a superfluous installation would squander the sources of power and indirectly menace the initiative of other industries. The character of the new work to be done demands that everything should be rejected that can be dispensed with readily in order that appliances specially needed in the new work may be provided and that these appliances be of the latest and most efficient types.

For the sake of economy, not only of money but of time and intellectual energy, tests and experiments that can be executed as well or better elsewhere by existing establishments should be avoided. For example, it is unnecessary to install a complete set of instruments and implements for testing the tensile strength of materials or their bending and crushing strength. Many other establishments permit of such work. If the laboratory is to be located in Washington, where certain advantages exist, such work could be readily done at the navy yard, where other facilities exist such, for instance, as the testing of models for hydroaeroplanes and flying boats. The Bureau of Standards and Measures and other Government branches in Washington also offer facilities which it would not be wise to duplicate in such a laboratory.

I do not think that such an institution should be burdened with measuring the power of motors or preoccupied with the details of their performances. This may be done at various other Government establishments, and it is understood that the Automobile Club of America is also equipped for this work.

Nor is it necessary to have a complete chemical laboratory under the pretext of studying questions relating to the chemistry of fuel or the permeability of balloon envelopes.

I do not wish to convey the idea that an aerodynamic laboratory should be deprived entirely of such facilities and that it should be obliged to seek minor information from other establishments when that information may be more economically obtained by a duplicate plant on a small scale. Such duplicate conveniences, however, should be regarded as strictly accessory; but it should be well understood that whenever important researches can be prosecuted as well or better elsewhere, dependence should be placed on those other establishments where such work is a specialty.

TWO DISTINCT CLASSES OF WORK

An aerodynamic laboratory should be devoted to (1) experimental verification, (2) experimental research. The first is concerned with testing the qualities of existing appliances, propellers, sustaining surfaces, control mechanism, etc. Usually these tests are made at the request of interested parties (as is now the case with water models at the navy-yard model basin). A constructor or a designer will bring, for example, a propeller and will wish to know its power or thrust at a given speed on the block or on a moving appliance under the conditions of flight, or he may bring several propellers to compare their performances and to ascertain what power they absorb at different speeds.

One of the very successful appliances devoted to this work at St. Cyr is a movable car, in which an aeroplane may be mounted and tested at speeds in perfect safety as to its strength, its efficiency, and the suitability of its control mechanism. This device is specially adapted to make actual service tests of sustaining surfaces, in other words, to try out in perfect safety the relative efficiencies of finished aeroplanes. It is a most important adjunct, as it supplements and rounds out the important research work on models in the closed laboratory.

Tests of this character, i.e., verification tests, constitute, so to speak, standard work. They are performed at the request of manufacturers, clubs, independent investigators, and other interested parties on condition of payment for the actual cost of the work. They therefore contribute to the support of the establishment.

The tests of verification, however, notwithstanding their great utility, do not constitute either the most important or the most interesting work of the laboratory. The research work, which prosecutes continuously and patiently systematic, thorough, and precise investigation of new ideas, or of old ideas with new applications, with the specific intention of discovering laws and formulas for advancing the progress of aerial navigation, is of greater importance, because it is the short cut to substantial efficiency, economy, improvement, and prestige.

This work is concerned with developing adequate methods of research in all branches of aerial navigation and in furnishing reliable information to all students, engineers, inventors, manufacturers, pilots, navigators, strategists, and statesmen. The knowledge thus gained should be disseminated regularly through publications, lectures, open-air demonstrations, and by exhibitions of apparatus, instruments, materials, and models—in fact, by all the facilities of the aerodrome, the showroom, the library, and the lecture room.

An exact knowledge of aerodynamics can best be acquired in such a laboratory by experimentation with standard scale models in air tunnels such as those used by M. Eiffel and others. In this way reliable data is obtained of the air resistance to be encountered and the efficiency at various velocities, the amount of lift, the effect of varying impact at different angles of attack on the stability—in fact, all the exact data which, reduced to curves and diagrams, enables the engineer to design a machine in a scientific manner. From such data the performance of a new machine can be closely predicted. The performance of the finished product can be verified later as before described.

Much of the research work will be prosecuted at the request of technical men outside of the institution, to whom the laboratory should offer, gratuitously as far as possible, its material and personal resources.

THE COUNCIL AND ORGANIZATION

To obtain benefit from these researches it will be necessary to know that they are worth the time and expense, and a body of men—a council or a board of governors—should be authorized to accept or reject requests for this work. This will be a delicate task, but the principal duty of the council should be to establish and to correct from time to time a program of the research work to be executed by the director and his staff and to coordinate the work to the best advantages within the limits of the money available. The disbursement of the Government funds, however, and the responsibility therefore should be entirely under the director.

With the actual state of aerial navigation and its deficiencies as a guide it will be the policy of the council to concentrate effort upon such points as seem most important, promising, and interesting for the time being.

I do not think there would be any doubt, if we had the laboratory in working order now, but that all questions relating to improvement in stability, automatic control, and safety in general would have the right of way.

The council or board, which in England is called the "advisory committee," should be representative of other Government departments than that employing the director, and should be independent of the director and his administrative staff. It might be possible for the director to act as a member of the council and, if so, it would conduce to harmony and expedition.

The council should not be a large body, but should be composed mostly of specialists of unquestioned ability, men interested in the development of aerial navigation in various branches of the Government and in its useful and safe adaptation to commerce and sport.

Whatever the ability of this council it should not be allowed to pretend that it has a monopoly of aeronautic acumen. Many brilliant and worthy ideas may originate outside of the establishment which it will be wise to investigate. And to avoid any possibility of the council being charged with narrow prejudice, it is indispensable that it be not composed entirely of specialists. In a few words, it should comprise representative men who are also learned and technical men, with broad vision and reputation, whose presence will guarantee to industrial investigators that their ideas will be treated in an unpartisan or unbiased spirit. I will not attempt to suggest the composition of this council or board, but it is evident that the Army and Navy should each be adequately represented on it.

ENDOWMENTS, PRIZES, AND REWARDS

If the laboratory should obtain, in addition to the funds required for prosecuting researches by its staff, any endowments of financial aid in excess of immediate needs (and I am confident it will eventually), it would accomplish useful work by offering prizes and granting rewards for important results achieved outside of the institution. The division of rewards would be one of the functions of the council, and it is possible that this would be one of the best uses of such resources, after the success of the laboratory is assured.

The complete role of an ideal aerodynamic laboratory can be summed up now in a few words in the natural order of establishment: (1) Execution of verification tests by means of nominal fees; (2) facilities to technical men for prosecuting original research; (3) execution of researches in accordance with a program arranged by the council, and (4) reward of commendable results accomplished outside of the laboratory.

NATURE OF THE PLANT

Researches and tests can be made on either a large or a small scale, preferably on both.

The use of small models can be made prolific in results because of the comparatively small cost, provided we understand the laws governing transformation into the full sized products. For model work a large plant is unnecessary. M. Eiffel has done very valuable work in a very small establishment.

Certain classes of tests with large models, such, for example, as the block test of propellers, do not require much space. But the conditions are altered when such tests are made on a machine in motion. These more difficult tests are absolutely indispensable and very important to the usefulness of an official laboratory.

Experiments and tests with small models being comparatively inexpensive, private establishments often undertake their execution, but when we attempt to draw conclusions from their results we are obliged to admit that the laws of comparison with full-sized machines are debatable the world over. Comparisons are sensibly true between small surfaces and larger surfaces that have been extended proportionately to the square of the linear dimensions, even to surfaces five or ten times larger, but when we pass to much larger surfaces, as we are obliged to, we are forced to adopt formulas with empirical coefficients, about which there is indefinite dispute.

The difficulty can be overcome only by precise experiments upon large surfaces, and such experiments, whatever the manner in which they are performed, will be costly. If privately executed, the financial returns would not cover the cost.

The laboratory should comprise, therefore, two distinct parts, one devoted to experiments on small-scale models and the other to experiments on surfaces of large dimensions. But in both parts precise and thorough work is necessary.

When we have studied separately each element of an aeroplane, for example, it will be necessary to test the complete apparatus. An aerodrome annex is therefore necessary, or, at least, the laboratory should be located in proximity to an aerodrome of which it can make use. In order that the observations may not only be qualitative but quantitative, it will be necessary to follow all the movements of the complete machine to know at each instant the speed, the inclination, the thrust of the propellers, the effective horsepower, and, in fact, to conduct a true open-air laboratory for aircraft after the manner of certain tests that have been prolific of results in France.

The English have established close relations between the royal aircraft factory and their laboratory, the function of the former being the reconstruction and repair of aeroplanes, the test of motors, and the instruction of mechanics.

LOCATION OF THE LABORATORY

The location of the model-testing plant, the headquarters of the administration staff, requires comparatively small space, and there is no reason why it should be remote from a city or from intellectual and material resources. It is advantageous to have it easy of access to many interested people who are not attached to it.

The location of the open-air laboratory should obviously be at an aerodrome as near as may be convenient to the model-testing plant or headquarters. Close proximity of the two parts is desirable, but not necessary. The high price of land near a large city obliges the aerodrome annex of foreign plants to be located at a distance, but we are fortunate in having here at Washington ideal conditions for the location of both parts. The model laboratory should obviously be located on the site of Langley's notable work at the Smithsonian Institution, where the nucleus, an extensive library of records, and a certain collection of instruments, are still available. The National Museum is also an ideal location for the historical collection of models that will result.

No more ideal location for the annex, the open-air laboratory, or aerodrome exists in all the world than that afforded by the as yet undeveloped extension of Potomac Park. This is Government property which is of doubtful utility as a park only, but which would be of immense utility and interest as a park combined with a scientific plant of the character under consideration.

There is no reason why the public should be excluded from such a practice field, but there is much to recommend that it be open to the public under proper regulations as to the traffic, especially on occasion of certain tests or flights of an educational value. It is of sufficient area, about 1 square mile. It is about 2 miles long, is almost entirely surrounded by broad expanses of water, and, while convenient of access, is so situated that the public may be readily excluded when tests of a dangerous character are in process of execution. The fine driveways that will be required as a park will offer excellent facilities for the practice work of the aerodrome and for the moving test cars that should be supplied.

One of the most attractive features of this location is the advantage it offers as an ideal aerodrome for both the Army and the Navy, for both land and water flying and the opportunity it affords for cooperation in all branches of the work of instruction and experimentation. Furthermore, it is near to the shop facilities of the navy yard, the accommodations of the Washington Barracks, the conveniences of various Government hospitals, and it would doubtless add to the information and interest of the near-by War College Staff and the General Board of the Navy. Its location would enable our statesmen in Congress and a great number of officials in all departments to keep in touch at first hand with the progress of aeronautics, with the quality of the work done, and with the manner in which the money appropriated was being expended. The educational facilities afforded by the work and by the lectures would be invaluable to the course of instruction for Army, Navy, and civil students of aeronautics.

As Washington is a mecca for business people of all parts of the country, a laboratory located here would be convenient in a commercial sense, especially in view of its southerly location, which renders the open aerodrome available for use throughout the greater part of the year. The only objection that I can see to the Potomac Park extension is that the ground will require a considerable clearing, but the trees on the harbor side of the location would not necessarily require removal.

THE APPARATUS NEEDED

It is useless to discuss here the various instruments and methods that have been a source of some dispute abroad. All have some good feature, but time has shown where some of the cumbersome and unnecessary installations may be eliminated to advantage and where others may be improved. The new plant of M. Eiffel, at Auteuil, may be regarded as a model for the wind tunnel and the aerodynamic balance. A duplicate of that plant alone would be of inestimable value. The last volume published by M. Eiffel is a forcible example of the value of his discoveries by this method with respect to the angle of incidence and the displacements of the center of pressure. It seems to merit the utmost confidence, although the details of his installation differ from those at Chalais, at Koutchino, at the Italian laboratory, and others. This method permits of testing the resistance of body structures, the sustaining power of surfaces, the tractive power of propellers, and the influence of transverse or oblique currents. If "free drop" apparatus at uniform speed be regarded as indispensable to obtaining the coefficients of air resistance to solid bodies of different shapes, it is possible that the interior of the Washington Monument could be used to advantage, as was the Eiffel Tower, without disturbance of the main function of that noble structure. This would be an excellent place from which to observe the stability or action of falling models cast adrift at an altitude of 500 feet under varying atmospheric conditions. The free drop of full-sized models would of course require the use of kites or captive balloons.

The moving car previously referred to for tests of verification would be the most useful open-air plant and would soon repay the outlay required by the value of the information obtained from its use. A miniature duplicate of this method for preliminary tests on models with a wire trolley would be of value in a hall of large dimensions. It would be useful in winter work but not invaluable.

The track of the open-air vehicle at St. Cyr is too restricted to give the best results. The car can not circulate continuously at high speed and maintain the speed for a sufficient length of time. An ideal endless track may readily be arranged at the Potomac Park extension, preferably of rectangular form with rounded corners. A railway track would be preferable, but excellent results could be obtained from auto trucks run on macadamized roadbeds. Good results could be obtained by the use of suitable hydroaeroplanes or flying boats suitably equipped with instruments.

At the aerodrome annex ample facilities should be provided for measuring the wind velocity at various heights and at different points. The convenient installation of recording anemometers and the employment of kites or captive balloons should be considered.

A branch of the United States Weather Bureau could readily be established at the aerodrome here in connection with the investigation of meteorological phenomena affecting the movements of aeroplanes in flight and as an adjunct to the national laboratory.

Exactly measured bases and posts of observation are also required, as well as instruments of vision or photographic apparatus, to permit of following machines in their flights and of preserving the records for study.

One of the most useful installations for recording advanced information is an actual aeroplane itself equipped with instruments adapted to record, while in flight, much of the information that is desired. Such machines are already in use in France and in England.

It will be in perfect harmony and convenient to the laboratory to obtain all the services of an aircraft factory from the Washington Navy Yard, where facilities already exist for the reconstruction and repair of aeroplanes, the test of motors, and the instruction of mechanics. But this should not be allowed to interfere with our policy of relying upon private industry for the purchase of new machines, for the sake of encouraging the art among private builders.

It will suffice to merely mention the hangars or sheds required of the local accessories, such as drafting room, office, and minor repair shops. The character and location of these present no difficulties, but they should not be made the principal part of the institution as they are in several elaborately equipped foreign laboratories. The power plant, however, is a subject for careful consideration and the economy effected by M. Eiffel in his new installation at Auteuil is worthy of study.

COST

I have seen estimates varying from $250,000 to $500,000 for such a plant, but inasmuch as $100,000, with an annuity of $3,000 donated by M. Henry Deutsch de la Meurthe to the University of Paris for the establishment of the aeronautical laboratory at St. Cyr, seems to have been sufficient for a very creditable though somewhat deficient plant, I will venture an opinion that $200,000 would be sufficient in our case. Although the same plant would cost more in this country, I assume that some of the buildings required are already available at the Smithsonian Institution. If located elsewhere the cost would be considerably more than the sum named.

A COMMISSION RECOMMENDED

Inasmuch as more definite information regarding the actual cost of a dignified and creditable but modest and sufficient installation should be obtained, and as the details of the plan, the scope, the organization, and the location of such an important undertaking should not be left to the recommendations of one man, I respectfully recommend that a commission or board be appointed to consider and report to the President, for recommendation to Congress, on the necessity or desirability for the establishment of a national aerodynamic laboratory, and on its scope, its organization, the most suitable location for it, and the cost of its installation.

W. Irving Chambers

Document 2-2(b), J. C. Hunsaker, Report on Facilities for Aeronautical Research in England, France, and Germany, Part III—Germany, *undated (ca. November 1913).*

Part III—Germany

(a) Modellversuchsoustalt fur Luftschiffart und Flugtechnik and der Universitat, Göttingen,

Director: Dr. Prandtl.

In 1907, the Motorluftschiff-Studien-Gesellschaft was founded to promote the German air craft industry. Various banks, commercial houses, and industrial works contributed heavily. The parent society later floated stock companies to build the German Wright aeroplane, and the Parseval air ship, and founded aero clubs in the principal cities. Money was allotted to the University of Gottingen for experimental research, and in 1908-9 a laboratory was built there. The director of the laboratory was Dr. Prandtl, head of the department of applied mechanics, at the University. He has given a great part of his time to aeronautical research since that date while still holding his position at the University and delivering lectures there on mathematics and mechanics. He has had only the assistance of his students and a few mechanics. There have usually been two students in the laboratory working for a doctorate. These men remained only a year or two and were replaced by new men. In view of the changing personnel, the excellence and continuity of the research is most remarkable. However, it is not likely that men of the ability of Fuhrmann, Fokke, and others could have been hired as assistants for a reasonable sum.

The Gottingen work is characterized by poor equipment made the most of by men of extraordinary ability.

In June, 1913, the Moroluftschiff-Studien-Gesellschaft had expended its capital and had accomplished its purpose.

In the first place, the German Wright Company has been distanced by its competitors in the aeroplane field, which is proof that the society's object of creating the industry has been attained. In the second place, the Parseval air ship, made by the Luftfahrzeug Gesellschaft, has been successfully developed and is financially profitable. Germany leads the world in air ship building. Accordingly the M-S-G has gone into liquidation and the Gottingen laboratory grant is stopped.

However, model research will be continued by a large grant from the Kaiser Foundation. The old wind tunnel will be abandoned, and a new and more powerful one built. It is expected that new buildings will be erected in Gottingen and that Dr. Prandtl will leave the University to be the director. Dr. Prandtl is now inspecting the aeroplane laboratories of Europe. I met him in Paris at the Eiffel laboratory, and in England at the National Physical Laboratory. Both wind tunnels he considers good. He is not yet ready to announce the type of his new tunnel, but he will not reproduce his old one.

It seems that with a closed circuit great difficulty is ahead in securing a uniform stream of air if high speeds are used.

The closed circuit tunnel at Gottingen gives a uniform stream of air of velocity 10 meters per second but nearly two years have been spent adjusting baffles, screens, and honey combs to attain a good result.

The tunnel and its work are well known to the readers of aeronautic literature. Descriptions will be found in:—Jahrbuch der Motorluftschiff-Studien-Gesellschaft for the years 1908-1910-1911-1912, published by Gustav Braunbeck, Berlin.

Also in—Jahrbuch der Luftfahrt by A. Vorreiter, 1910-11-12, published by J.F. Schmanns, Munich.

Also in—Zeitschrift—fur Flugtechnik und Motorluftschaffahrt, a periodical published by Oldenburg, Berlin.

The balance used by Dr. Prandtl is very delicate, and also very complicated. The best description is given by Vorreiter. Its precision is less than 3/4%.

A special device is used for model propeller testing in connection with the regular balance.

A great deal of work has been done on balloon shapes, aeroplane wings and model propellers.

A most interesting piece of work has been the computation of the head of resistance of a model dirigible by hydrodynamic theory and its verification by experiment. The discrepancy found is considered to be skin friction.

A research on propellers has been made to determine the distribution of pressure over the blade in motion. A model in wax was covered with copper by electro deposit and the wax melted out. The hollow copper model then had holes bored at intervals over the blades. One hole at a time was opened and the internal pressure transmitted through a hollow shaft and slip joint to a manometer.

The form of a Parseval air ship and the arrangement of fins and rudders is the result of model research at Gottingen. As Count Zeppelin is an officer of the M-S-G, considerable work has been done on Zeppelin models but none of it is published.

As the wind tunnel is not to be duplicated in the new laboratory no detail criticism of it is given here. The general statement can be made that an irregular flow is created which must be smoothed out by a multiplicity of obstructions in the channel. That irregularity has been removed is more to the credit of the personnel of the laboratory than to the design of the wind tunnel.

It appears that there is no relation between model research at Gottingen and full scale experiment. The laboratory is devoted primarily to scientific research and at odd times undertakes industrial testing for private firms.

(b) Zeppelin Versuchsanstalt, Friederichshafen.

The Zeppelin company's works are not open to visitors, and I was unable to secure admission. It is generally known that the company has a very complete engine testing plant and is equipped with a complete meteorological station including an outfit of pilot balloons for sounding the upper air levels.

In addition there is machinery for testing the strength of materials of construction.

A large and rough wind tunnel of rather blast of wind is reported to be used to study the stability of route of air ship models.

(c) Technische Hochschule Aachen, Aachen.

Director of laboratory—Prof. Reisswer.

Under Prof. Reisswer at Aachen courses in aeronautics and aerodynamics have been given to students who elected to attend. No design work was given. A large wind tunnel of square section 2 m. by 2 m. has been built. The tunnel is open to the outdoor air at one end and at the other is a powerful exhauster. A velocity of 30 meters per second can be maintained and provided there is no wind outside, the current is very uniform. There are no baffles or honey combs in the channel. A 75 H.P. motor is used.

It is noted that Eiffel obtains 30 meters per second in a 2 meter channel with less than 50 H.P.

When there is any wind outside the air stream is not uniform. Much time is lost waiting for a calm day and an advantage must be taken of still times at dawn and evening.

No expensive balance has been designed, and work is largely confined to propeller testing. The model propeller attached to the axis of a small electric motor is suspended by two wires in a canal. The torque is measured by the difference in pull on the two wires. The thrust is measured by the pull on a third wire led up stream.

There is no permanent staff working with the tunnel and no great amount of work is done at present. This winter one of the assistants from Gottingen will take charge.

(d) Versuchsanstalt für Flugwesen der Kgl. Technischen Hochschule zu Berlin, Spandauer Weg, Reinickendorf-West, Berlin.
Director:—Major Von Parseval.
Asst. Dr. Quittner.

The laboratory is in process of construction under the direction of Maj. Von Parseval, the inventor of the Parseval dirigible.

An office building of 4 rooms has been erected beside an old wooden air ship shed of the Parseval Company.

This shed is 25 by 70 by 22 meters high.

Down the center is built a trestle 5m high and 70m long carrying a track on which a dynamometer carriage runs.

The carriage is drawn by a cable leading over suitable blocks to two sets of sand bags. The first set fall and accelerate the car in a few meters while the second set continue to fall and maintain the car in uniform motion. The weight of sand will be adjusted after trials. Automatic brakes are provided.

The car is some 2m high and is designed to measure lift, drift, and center of pressure of aeroplane wings of span not greater than 10 meters. Forces are measured against hydraulic pressure boxes and recorded graphically on ordinary steam engine indicators from which the springs are removed.

Great precision of measurement is not attempted. It is hoped to study the effect on aeroplane coefficients of change in area.

It is not likely that a high speed can be reached.

A small whirling arm, radius 5.5 meters, is completed. This arm is turned by falling weights. The trouble from a following wind is avoided by giving only two complete turns for a test. Only head resistance forces can be measured. A steam engine indicator drum records the force on the model. A velocity of 12 meters per second is used.

It is expected to study small surfaces to compare their coefficients with those for large surfaces tested on the railway.

The staff consists of the director Major v. Parseval, and his assistant Dr. Quittner, with 3 mechanics.

The courses in aeronautics at the Hochschule include lectures on aeroplane and dirigible construction by Major v. Parseval, with the mathematics and mechanics of flight given by other members of the faculty. No practical design is attempted. Aeronautical studies are optional.

(e) Prof. Dr. Fr. Ahlborn, 23 Ufer Strasse, Hamburg.

Prof. Ahlborn is well known to naval architects for his contributions to "Schiffbau," on stream line flow in water. He first called attention to the inherent stability of the East Indian Zanonia leaf, and was the means of causing Igo Etricn of Vienna to develop the Zanonia form aeroplane.

All of his investigations have been conducted in a tank of water some 3 x 3 x 15 feet long. Models at very low speed were drawn through the water by an electric carriage running above the tank.

He found that fine beech sawdust boiled until it reached the density of water could be used to show stream line phenomena. Various photographic methods were developed which have been described in his papers. At the present time, the Hamburg American Line and the City of Hamburg are to build him an experimental model tank for the further study of fluid motion.

All of Prof. Ahlborn's results are qualitative and serve to illustrate fluid flow at very low speeds. Their application to aeronautics is not apparent.

The Massachusetts Institute of Technology has purchased some 30 slides from Prof. Ahlborn which represent his most important work. Permission is given to copy them provided they are not published.

It seems that work such as Prof. Ahlborn has done should go hand in hand with wind tunnel research. No one has yet been able to delineate the flow of air in a high speed wind tunnel. At the National Physical Laboratory the whole stream becomes cloudy if tobacco smoke is used and the camera only shows a fog. Mr. Eiffel has tried salammoniac vapor without success. At Gottingen cork dust has been tried and abandoned.

(f) Deutschen Versuchsanstalt für Flugwesen, Adlershof, bei Berlin.
Director:—Dr. Bendemann.

Dr. Bendemann conducted the test for the Kaiser Prize for the best German aeroplane motor. This competition showed the necessity for having in Germany a motor testing establishment when impartial comparisons could be made.

The German Society of Mechanical Engineers has now endowed a laboratory of which Dr. Bendemann is to be director.

Five motor testing beds in separate buildings are completed. Five motors can thus be tested at the same time without interference due to noise and flying oil. The main building contains offices and an aeroplane hanger. A tower 100 feet high surmounts it. It is expected to suspend an aeroplane from cables reaching up inside this tower to the top in such a manner that by the method of oscillations the moment of inertia about the various axes may be measured. Work of this nature is not yet commenced.

No wind tunnel will be built.

Experiments will be conducted on full-scale aeroplanes in flight, carrying recording instruments. Work will be started as soon as the proper instruments are completed.

The first experiments will be to send up aeroplanes with a form of recording dynamometer attached to the controls. The force exerted by the pilot and the rapidity of his movements will thus be studied.

Designs are being made for a dynamometer car to run on the railroad and to carry a full size aeroplane. The car will be pushed by a locomotive and high speeds are expected.

There seem to be unlimited funds.

The staff already consists of the Director and 10 assistants.

The laboratory is next to the Johannistahl flying grounds.

[signed] J. C. Hunsaker

Document 2-2(c), Albert F. Zahm, excerpt from **Report on European Aerodynamical Laboratories,** *1914.*

GENERALITIES

Places visited.—During August and September, 1913, in company with Jerome C. Hunsaker, Assistant Naval Constructor, U. S. N., I visited the principal aeronautical laboratories near London, Paris, and Göttingen, to study, in the interest of the Smithsonian Institution, the latest developments in instruments, methods, and resources used and contemplated for the prosecution of scientific aeronautical investigations. Incidentally we visited many of the best aerodromes (flying fields) and air craft factories in the neighborhood of those cities, and took copious notes of our observations. We also visited many aeronautical libraries, book stores, and aero clubs, in order to prepare a comprehensive list of the best and latest publications on aerial navigation and its immediately kindred subjects. In each of the countries, England, France and Germany, we spent about two weeks. We were made welcome at all the places visited, and thus established personal relations which should be valuable in future negotiations with the aeronautical constructors and investigators in those countries. But these incidental visits and studies, though they may prove serviceable, do not seem germane to the present report. Neither does it seem advisable to take more than passing notice of the aeronautical laboratories themselves in those manifold details which have been already published in large and comprehensive reports now accessible in the Smithsonian Library.

Organization, resources, and scope of the laboratories.—The laboratories examined by us are in particular (1) the aeronautical research and test establishments of the British government near London; (2) the Institut Aerotechnique de St. Cyr, and the Laboratoire Aerodynamique Eiffel, both near Paris; (3) the Göttingen Modelversuchsanstalt at Göttingen; and (4) the newly organized laboratory adjoining the flying field at Johannisthal near Berlin, known as the "Deutsche Versuchsanstalt für Luftfahrt zu Adlershof."

These establishments resemble each other in some important features, but differ in others. All are devoted to both academic and engineering investigations. All are directed by highly trained scientific and technical men. The directors are not merely executives; they are the technical heads—scientists or engineers specifically qualified by superior training in aeronautical engineering and its immediately cognate branches—who initiate the researches, and assist their technical staffs in devising apparatus, interpreting results, and making systematic reports.

The establishments differ in their organization, resources, and equipments, and, to a considerable extent, in the scope and character of their investigations. Of the five institutions mentioned, the one in England and the one at Göttingen are now supported largely by governmental appropriations; and the other three are maintained by private capital, allotted as required, or accruing from fees or endowment funds. Again, the laboratories near London, at St. Cyr, and at Adlershof are practically unlimited in the scope of their researches, while Eiffel's and the Göttingen laboratory have confined their activities substantially to wind-tunnel experiments.

The aeronautical researches of the British Government are in charge of the British Advisory Committee for Aeronautics, a self-governing civilian organization which was appointed by the Prime Minister of England to work out theoretical and experimental problems in aeronautics for the army and navy, and comprises twelve to fourteen expert men, under the presidency of Lord Rayleigh. This committee initiates and directs investigations and tests at the Royal Air Craft Factory, at the National Physical Laboratory, at the Meteorological Office, at Vickers Sons, and Maxim's etc. It expends, in performing its regular functions, a sum exceeding the income of any private aeronautical laboratory and received directly from the government treasury.

The committee is primarily occupied with work for the government, but also performs researches and tests for private individuals, for suitable fees, but without guaranteeing secrecy as to the results. The work of the committee is manifold and comprehensive. Whirling-table measurements, wind-tunnel measurements, testing of engines, propellers, woods, metals, fabrics, varnishes, hydromechanic studies, meteorological observations, mathematical investigations in fluid dynamics, the theory of gyroscopes, aeroplane and dirigible design—whatever studies will promote the art of aircraft construction and navigation may be prosecuted by this committee. A detailed program and the results of actual investigations have been published in the annual report of this committee.

M. Eiffel has paid from his personal fortune all the expenses of his plant and elaborate researches, though it is understood that he may sometimes charge nominal fees for investigations made for private individuals who wish exclusive rights to the data and results obtained. The general director of the laboratory is Eiffel himself—who initiates the researches and publishes the results. He has in immediate charge two able engineers, MM. Rith and Lapresle, aided by three trained observers who are skilled draughtsmen. Two mechanics and one janitor complete the personnel. The work of the laboratory is all indoors, and is confined to researches in aerodynamics alone, or more specifically to wind-tunnel measurements and reports thereon.

The institute at St. Cyr was founded by Deutsch de la Meurthe, who gave $100,000 for the original plant and has provided $3000 per year, during his life, for maintenance. It was presented by him to the University of Paris, and is now under the general direction of the professor of physics, M. Maurain, aided by a technical staff and a large advisory council of eminent engineers, scientists, and officers of the university, officers of the French government, and members of various clubs and aeronautical organizations. The staff comprises the director in charge and his assistant, together with such students, two or three at a time, as may come as temporary volunteers from the University of Paris.

The institute conducts large-scale experiments in the open field as well as indoor researches, makes investigations for general publication or for private interests, on payment of suitable fees, and permits private persons to conduct researches in the laboratory. The work is practically unlimited, as is the case in the English aeronautical laboratories. A special feature of the institute is its three-quarter mile long track with electric cars for tests on large screws, large models, and full-size aeroplanes.

The Göttingen aerodynamical laboratory was begun as a private enterprise, but is now to be enlarged and maintained in part by financial aid of the Kaiser Foundation. The original building, with its wind tunnel, was erected in 1908 after the plans of its director, Prof. Prandtl of the University of Göttingen, at a cost of 20,000 marks supplied by the Motorluftschiff-Studien-Gesellschaft. Its available income is said to be $7,000 a year. The enterprise was inaugurated on a small scale because of the uncertainty, at that date, as to the practical value of such an establishment. The work of this laboratory, as in Eiffel's, has been practically limited to wind-tunnel experiments, though Prof. Prandtl has written some valuable theoretical investigations, and is reported to be undertaking large-scale experiments in the open air by use of a car on a level track, as at St. Cyr.

The Deutsche Versuchsanstalt für Lutftfahrt zu Adlershof has been recently founded by the Verein Deutscher Ingeniure. The laboratory adjoins the great Flugplatz, with its two square kilometer flying field surrounded by numerous air craft factories, scores of hangars, an aero club house, and a grand stand. Major Von Tschudi, a retired German officer, is general manager of the organization which operates the flying field in the interest of all aero manufacturers and experimentalists, whether civilian or governmental. Dr. Eng. F. Bendeman is director of the laboratory and has ten assistants, comprising, among others, Dr. Fuhrman, who was former assistant in the Göttingen laboratory. I have not ascertained the financial resources of the laboratory, but a prelude to its present operations was a competition involving some three score German aeronautical motors, for the Kaiser Prize, and additional contributions from the country at large, aggregating in all 125,000 marks. It is understood that the laboratory is liberally supported, is unlimited in the scope of its work, and will conduct both indoor researches and field experiments similar to those at St. Cyr.

After this general view, a technical account of the foregoing aeronautical establishments may be useful.

Document 2-3(a–c)

(a) U.S. Congress, "An Act Establishing an Advisory Committee for Aeronautics," Public Law 271, 63rd Congress, 3rd Session, passed 3 March 1915.

(b) George P. Scriven, "Letter of Submittal" (Chairman's Letter for the first *Annual Report*, 9 December 1915), *NACA Annual Report for 1915* (Washington, DC).

(c) George P. Scriven, "Existing Facilities for Aeronautic Investigation in Government Departments," *NACA Annual Report for 1915* (Washington, DC).

Zahm's *Report on European Aeronautical Laboratories*, published by the Smithsonian Institution in 1914, revealed just how far the United States trailed Europe in aeronautical research. Armed with this new information, the Smithsonian's regents recommended that Congress establish a national advisory committee for aeronautics, based largely on the British Advisory Committee for Aeronautics organization, in February 1915. The navy, content with its own aerodynamical laboratory at the Washington Navy Yard, did little to support the recommendation, but it ended up in an ironic supporting role nevertheless. The text that established the National Advisory Committee for Aeronautics comprised only two paragraphs, and Congress included them in a Naval Appropriations Act that it passed on 3 March 1915. While only $5,000 per year for five years was appropriated, this brief rider set the tone for the organization, stating, "That it shall be the duty of the Advisory Committee for Aeronautics to supervise and direct the scientific study of the problems of flight, with a view to their practical solution, and to determine the problems which should be experimentally attacked, and to discuss their solution and their application to practical questions." Over the next two decades, that small investment in the NACA would grow into several million dollars, and the NACA would build a superb aeronautical laboratory and research program.

Congress defined the NACA to include members from the army, navy, Smithsonian Institution, weather bureau, and bureau of standards, as well as members from the broader aeronautical community. In so doing, it intended to coordinate both military and civilian aeronautical research and development through one agency that was charged with the authority to conduct research in any laboratory that might be assigned to it. During its earliest years, the NACA sponsored research in the Washington Navy Yard tunnel and elsewhere, such as William F. Durand's propeller research at Stanford University, but the committee members soon realized that they needed a comprehensive laboratory and staff that could support all facets of aeronautical research. Indeed, one of the NACA's first tasks was to examine the available facilities in the United States. The 1915 annual report outlined the capabilities and work loads of five government agencies in a section entitled "Existing Facilities for Aeronautic Investigation in Government Departments," and concluded "that utilizing all facilities at present available, the progress that can be made will be fragmentary and at best lack the coordination that is necessary to accomplish . . . the important work now in sight." Armed with this conclusion, the NACA's first chairman, Army Brigadier General George P. Scriven, proposed a joint military-civilian facility, but Navy Secretary Josephus Daniels objected, fearing it could jeopardize the navy's research plans. With its Washington Navy Yard wind tunnel in operation and Jerome Hunsaker involved in research at MIT, the navy was already actively engaged in aerodynamical research in 1915, and Daniels saw no need to change course. Nevertheless, he allowed an $82,516 NACA appropriation, which included $53,580 for construction of a new laboratory, to be attached to a 1916 Naval Appropriations Act. (The following year's Naval Appropriations Act provided the NACA with an additional $107,000, but subsequent NACA funding came from civil appropriations acts.) Given the navy's reticence, the NACA worked with the army to find a site where they could build and share a flying field, and where the NACA could build its laboratory. They settled on a location near Hampton, Virginia, and construction at Langley Field began in April 1917, just as the United States entered World War I.

Document 2-3(a), U.S. Congress, "An Act Establishing an Advisory Committee for Aeronautics," 1915.

Public Law 271, 63d Cong., 3d sess., passed 3 March 1915 (38 Stat. 930).

An Act making appropriations for the naval service for the fiscal year ending June thirtieth, nineteen hundred and sixteen, and for other purposes.

An Advisory Committee for Aeronautics is hereby established, and the President is authorized to appoint not to exceed twelve members, to consist of two members from the War Department, from the office in charge of military aeronautics; two members from the Navy Department, from the office in charge of naval aeronautics; a representative each of the Smithsonian Institution, of the United States Weather Bureau, and of the United States Bureau of Standards; together with not more than five additional persons who shall be acquainted with the needs of aeronautical science, either civil or military, or skilled in aeronautical engineering or its allied sciences: Provided, That the members of the Advisory

Committee for Aeronautics, as such, shall serve without compensation: Provided further, That it shall be the duty of the Advisory Committee for Aeronautics to supervise and direct the scientific study of the problems of flight, with a view to their practical solution, and to determine the problems which should be experimentally attacked, and to discuss their solution and their application to practical questions. In the event of a laboratory or laboratories, either in whole or in part, being placed under the direction of the committee, the committee may direct and conduct research and experiment in aeronautics in such laboratory or laboratories: And provided further, That rules and regulations for the conduct of the work of the committee shall be formulated by the committee and approved by the President.

That the sum of $5,000 a year, or so much thereof as may be necessary, for five years is hereby appropriated, out of any money in the Treasury not otherwise appropriated, to be immediately available, for experimental work and investigations undertaken by the Committee, clerical expenses and supplies, and necessary expenses of members of the committee in going to, returning from, and while attending meetings of the committee: Provided, That an annual report to the Congress shall be submitted through the President, including an itemized statement of expenditures.

Document 2-3(b), George P. Scriven, Chairman's Letter for the first NACA Annual Report*, 1915.*

LETTER OF SUBMITTAL.
NATIONAL ADVISORY COMMITTEE FOR AERONAUTICS,
STATE, WAR, AND NAVY BUILDING
Washington, D. C., December 9, 1915.

The PRESIDENT:

In compliance with the provisions of the act of Congress approved March 3, 1915 (naval appropriation act, Public, No. 273, 63d Cong.), the National Advisory Committee for Aeronautics has the honor to submit herewith its annual report for the period from March 3, 1915, to June 30, 1915, including certain recommendations for future work and a statement of expenditures to June 30, 1915.

The committee was appointed by the President on April 2, 1915, and held its first meeting for organization on April 23, 1915. On June 14 the President approved rules and regulations which had been formulated by the committee for the conduct of its operations.

By the act establishing the committee, an appropriation of $5,000 a year for five years was made immediately available. Of the appropriation for the first year, ending June 30, 1915, there was expended a total of $3,938.94, as shown by the itemized statement in the accompanying report, and the unobligated balance of

$1,061.06 was covered into the Treasury as required by law.

In order to carry out its purposes and objectives, as defined in the act of March 3, 1915, the committee submits herewith certain recommendations and an estimate of expenses for the fiscal year ending June 30, 1917. The estimates in detail were submitted through the Secretary of the Navy.

Attention is invited to the appendixes of the committee's report, and it is requested that they be published with the report of the committee as a public document.

It is apparent to the committee that there is a large amount of important work to be done to place aeronautics on a satisfactory foundation in this country. Competent engineers and limited facilities are already available and can be employed by the committee to advantage, provided sufficient funds be placed at its disposal, as estimated for the fiscal year 1917.

What has been already accomplished by the committee has shown that although its members have devoted as much personal attention as practicable to its operations, yet in order to do all that should be done, technical assistance should be provided which can be continuously employed. There are many practical problems in aeronautics now in too indefinite a form to enable their solution to be undertaken. The committee is of the opinion that one of the first and most important steps to be taken in connection with the committee's work is the provision and equipment of a flying field together with aeroplanes and suitable testing gear for determining the forces acting on full-sized machines in constrained and in free flight, and to this end the estimates submitted contemplate the development of such a technical and operating staff, with the proper equipment for the conduct of full-sized experiments.

It is evident that there will ultimately be required a well-equipped laboratory specially suited to the solving of those problems which are sure to develop, but since the equipment of such a laboratory as could be laid down at this time might well prove unsuited to the needs of the early future, it is believed that such provision should be the result of gradual development.

The investigations which the committee proposes in its program for the coming year can only be carried out to a satisfactory degree, with the limited facilities already existing, provided sufficient funds are made available. The estimates of the committee are based on such line of action, and on the assumption that a flying field can be placed at its disposal on Government land. If, however, such facilities be not practicable at this time, some progress may still be made by the utilization of the facilities of the Government aeronautic stations at Pensacola and San Diego.

The estimate of expenses for the fiscal year ending June 30, 1917, is as follows:

For carrying into effect the provisions of the act approved March third, nineteen hundred and fifteen, establishing a National Advisory Committee for Aeronautics, there is hereby appropriated, out of any money in the Treasury not otherwise appropriated, for experimental work undertaken by the committee, assistants and the necessary unskilled labor, equipment, supplies, office rent, and the necessary traveling expenses of the members and employees of the committee, personal services in the field, and in the District of Columbia: Provided, That an annual report to the Congress shall be submitted through the President, including an itemized statement of expenditures, $85,000.

The committee, therefore, submits its report, recommendations, and estimates to your favorable consideration.

Very respectfully,
George P. Scriven,
Brigadier General, Chief Signal Officer of the Army,
Chairman.

Document 2-3(c), George P. Scriven, "Existing Facilities for Aeronautic Investigation in Government Departments," NACA Annual Report for 1915.

For the conduct of the work outlined, limited facilities already exist in different Government departments about as described in general terms in the following. These facilities can be augmented by the facilities described as existing the different technical institutions, etc., previously referred to:

A. The Bureau of Standards is well equipped for carrying on all investigations involving the determination of the physical factors entering into aeronautic design, and is prepared to take up such matters as are of sufficient general interest to warrant same.

B. The Navy Department is equipped with a model basin and wind tunnel at the Washington Navy Yard, with adequate shop facilities for carrying on the work in a limited way, and is also constructing at the Washington Navy Yard a plant for the testing of aeronautic motors and devices involved in their operation, which will be in commission at an early date. Also, under the Navy Department steady progress is being made in attacking practical problems involved in the development of the Navy aeronautic service at its station at Pensacola, and theoretical and practical designs are in hand in the Bureaus of Construction and Repair and Steam Engineering.

C. The War Department has limited facilities at the flying school at San Diego, for investigations of interest to that branch of the service, and is able to carry out in a limited way experiments of interest to the service on full-sized machines, for which work it has the assistance of technical experts.

D. The Weather Bureau is well equipped for the determination of the problems of the atmosphere in relation to aeronautics, and Prof. Marvin, a member of the advisory committee, is the chairman of a subcommittee engaged on this problem.

The work, however, will necessarily be limited until the necessary funds for more extensive work become available. There is already available in the records of the bureau much information of value which requires compilation in a form suited to aeronautic requirements, and this work is the subject of a preliminary report included in the annual report of the committee.

E. The Smithsonian Institution has been engaged for a number of years on the compilation of the bibliography of aeronautics, and is prepared to continue this work for at least two years more with the funds at its disposal. The institution has also contributed funds toward the development of the work of the subcommittee of the Weather Bureau in its investigation of the problem of the atmosphere in relation to aeronautics.

CONCLUSIONS.

From the above, it will be apparent that utilizing all facilities at present available, the progress that can be made will be fragmentary and at best lack that coordination which is necessary to accomplish in a direct, continuous, and efficient manner, and as rapidly as practicable, the important work now in sight. If the committee is to be prepared to keep pace with the increasing needs of the very rapid development already under way, stimulated by the unusual conditions existing in Europe, the facilities and technical assistance recommended are essential. While the needs at present are principally those which have an important bearing on military preparedness, the committee is of the opinion that aeronautics has made such rapid strides that when the war is over there will be found available classes of aircraft and a trained personnel for their operation, which will rapidly force aeronautics into commercial fields, involving developments of which today we barely dream.

Respectfully submitted.

George P. Scriven,
Brigadier General, Chief Signal Officer of the Army,
Chairman.

Document 2-4

Jerome C. Hunsaker, excerpt from "The Wind Tunnel of the Massachusetts Institute of Technology," *Reports on Wind Tunnel Experiments in Aerodynamics* (Washington, DC: Smithsonian Institution, 1916).

The Massachusetts Institute of Technology became interested in aeronautical research and the idea of creating a course of study in aeronautical engineering in 1913—about the same time the navy began to pursue aeronautical research. MIT President Richard Maclaurin approached the navy about a cooperative effort and asked that Jerome Hunsaker, who had taken a masters degree at MIT in 1909, be assigned to the school for three years. The navy agreed, and Hunsaker reported to the school early in 1914, but only after his return from a previously scheduled European inspection trip with Albert Zahm.

While visiting the National Physical Laboratory in England during that trip, Hunsaker obtained plans for the NPL's four- by four-foot open tunnel and its latest balance instrument. Using the NPL plans, Hunsaker and MIT erected a duplicate tunnel on campus in 1914 and began experimental work. Two years later, the Smithsonian Institution published *Reports on Wind Tunnel Experiments in Aerodynamics*, written by Hunsaker and five other MIT professors. One of the earliest comprehensive reports on organized American aeronautical research, it describes the tunnel and some fundamental research being conducted. More importantly, it shows a degree of cooperation on the working level between several government agencies—cooperation that was not readily apparent at the executive level as the debate over a national aeronautical laboratory peaked.

Document 2-4, Jerome C. Hunsaker, excerpt from "The Wind Tunnel of the Massachusetts Institute of Technology," 1916.

An aeroplane or airship in flight has six degrees of freedom, three of translation and three of rotation, and any study of its behavior must be based on the determination of three forces—vertical, transverse, and longitudinal—as well as couples about the three axes in space. Full-scale experiments to investigate the aerodynamical characteristics of a proposed design naturally become mechanically difficult to arrange. The experimental work is much simplified if tests be made on small models as in naval architecture, and a further simplication is made by holding the model stationary in an artificial current of air instead of towing the model at high speed through still air to simulate actual flying conditions.

The use of a wind tunnel depends on the assumption that it is immaterial whether the model be moved through still air or held stationary in a current of air of the same velocity. The principle of relative velocity is fundamental, and the experimental discrepancies between the results of tests conducted by the two methods may be ascribed on the one hand to the effect of the moving carriage on the flow of air about the model and to the effect of gusty air, and on the other hand to unsteadiness of flow in some wind tunnels.

The wind tunnel method requires primarily a current of air which is steady in velocity both in time and across a section of the tunnel. The production of a steady flow of air at high velocity is a delicate problem, and can only be obtained by a long process of experimentation. A study was made of the principal aerodynamical laboratories of Europe from which these conclusions were reached: (1) That the wind tunnel method permits a leisurely study of the forces and couples produced by the wind on a model; (2) that the staff of the National Physical Laboratory, Teddington, England, have developed a wind tunnel of remarkable steadiness of flow and an aerodynamical balance well adapted to measure with precision the forces and couples on a model in any position; and (3) that the results of model tests made at the above laboratory are applicable to full scale aircraft.

Consequently it was decided to reproduce in Boston the four-foot wind tunnel of the National Physical Laboratory, together with the aerodynamical balance and instruments for velocity measurement. Dr. R. T. Glazebrook, F. R. S., director of the National Physical Laboratory, most generously presented us with detail plans of the complete installation, including the patterns from which the aerodynamical balance was made. Due to this encouragement and assistance we have been able to set up an aerodynamical laboratory with confidence in obtaining a steady flow of air of known velocity. The time saved us by Dr. Glazebrook, which must have been spent in original development, is difficult to estimate.

The staff of the National Physical Laboratory have developed several forms of wind tunnel in the past few years. In 1912–13 Mr. Bairstow and his assistants conducted an elaborate investigation into the steadiness of wind channels as affected by the design both of the channel and the building by which it is enclosed. The conclusions reached may be summarized as follows:

(1) The suction side of a fan is fairly free from turbulence.

(2) A fan made by a low pitch four-bladed propeller gives a steadier flow than the ordinary propeller fan used in ventilation, and a much steadier flow than fans of the Sirocco or centrifugal type.

(3) A wind tunnel should be completely housed to avoid effect of outside wind gusts.

(4) Air from the propeller should be discharged into a large perforated box or diffuser to damp out the turbulent wake and return the air at low velocity to the room.

(5) The room through which air is returned from the diffuser to the suction end of the tunnel should be at least 20 times the sectional area of the tunnel.

(6) The room should be clear of large objects.

The wind tunnel of the Institute of Technology was built in accordance with the English plans, with the exception of several changes of an engineering nature introduced with a view to a more economical use of power and an increase of the maximum wind speed from 34 to 40 miles per hour.

Upon completion of the tunnel an investigation of the steadiness of flow and the precision of measurements was made in which it appeared that the equipment had lost none of its excellence in reproduction in the United States.

As will be shown below, the current is steady both in time and across a cross-section within about 1 per cent in velocity. Measurements of velocity by means of the calibrated Pitot tube presented by the National Physical Laboratory are precise to one-half of 1 per cent. Force and couple measurements on the balance are precise to one-half of 1 per cent for ordinary magnitudes. Calculated coefficients which involve several measurements of force, moment, velocity, angle, area, and distance, as well as one or more assumptions, can be considered as precise to within 2 per cent. It is believed that it is not practicable to increase the precision of the observations to such an extent that the possible cumulative error shall be materially less than the above.

DESCRIPTION OF WIND TUNNEL

A shed 20 by 25 by 66 feet houses the wind tunnel proper, 16 square feet in section, and some 53 feet in length (pl. 1). Air is drawn through an entrance nozzle and through the square tunnel by a four-bladed propeller, driven by a 10 H. P. motor. Models under test are mounted in the center of the square trunk on the vertical arm of the balance to be described later.

The air entering the mouth passes through a honeycomb made up of a nest of 3-inch metal conduit pipes 2 feet 6 inches in length. This honeycomb has an important effect in straightening the flow and preventing swirl.

Passing through the square trunk and past the model, the air is drawn past a star-shaped longitudinal baffle into an expanding cone. In this the plans of the National Physical Laboratory were departed from by expanding in a length of 11 feet to a cylinder of 7 feet diameter. This cone expands to 6 feet in the English tunnel. M. Eiffel affirms that the working of a fan is much improved by expanding the suction pipe in such a manner as to reduce the velocity and so raise the static pressure of the air. Since the fan must discharge into the room, the pressure difference that the fan must maintain is thus reduced. Also with a larger fan the velocity of discharge is reduced, and the turbulence of the wake kept down.

The propeller works in a sheet metal cylinder 7 feet in diameter, and discharges into the large perforated diffuser. The panels of the latter are gratings and may be interchanged fore and aft. The gratings are made of 1 ½-inch stock with holes 1 ½ by 1 ½ inches. Each hole is then a square nozzle one diameter long. The end of the diffuser is formed by a blank wall. The race from the propeller is stopped by this wall and the air forced out through the holes of the diffuser. Its velocity is then turned through 90 degrees. The area of the diffuser holes is several times the sectional area of the tunnel, and the holes are so distributed that the outflow of air is fairly uniform and of low velocity (pl. 2, fig. 1).

A four-bladed black walnut propeller (pl. 2, fig. 2) was designed on the Drzwiecki system and has proved very satisfactory. In order to keep down turbulence a very low pitch with broad blades had to be used. To gain efficiency such blades must be made thin. It then became of considerable difficulty to insure proper strength for 900 R. P. M. as well as freedom from oscillation.

The blade sections were considered as model aeroplane wings and their effect integrated graphically over the blade. The blade was given an angle of incidence of 3 degrees to the relative wind at every point for 600 R. P. M. and 25 miles per hour. The pitch is thus variable radially.

To prevent torsional oscillations, the blade sections were arranged so that the centers of pressure all lie on a straight line, drawn radially on the face of the blade.

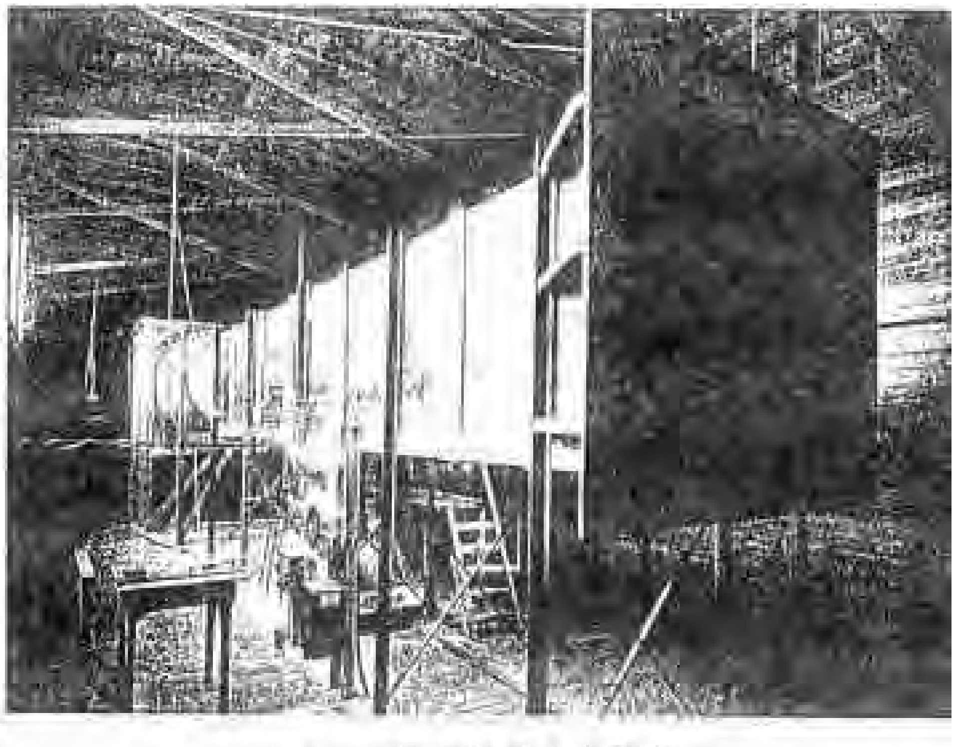


FIG. [illegible] SIDE OF [illegible] TUNNEL

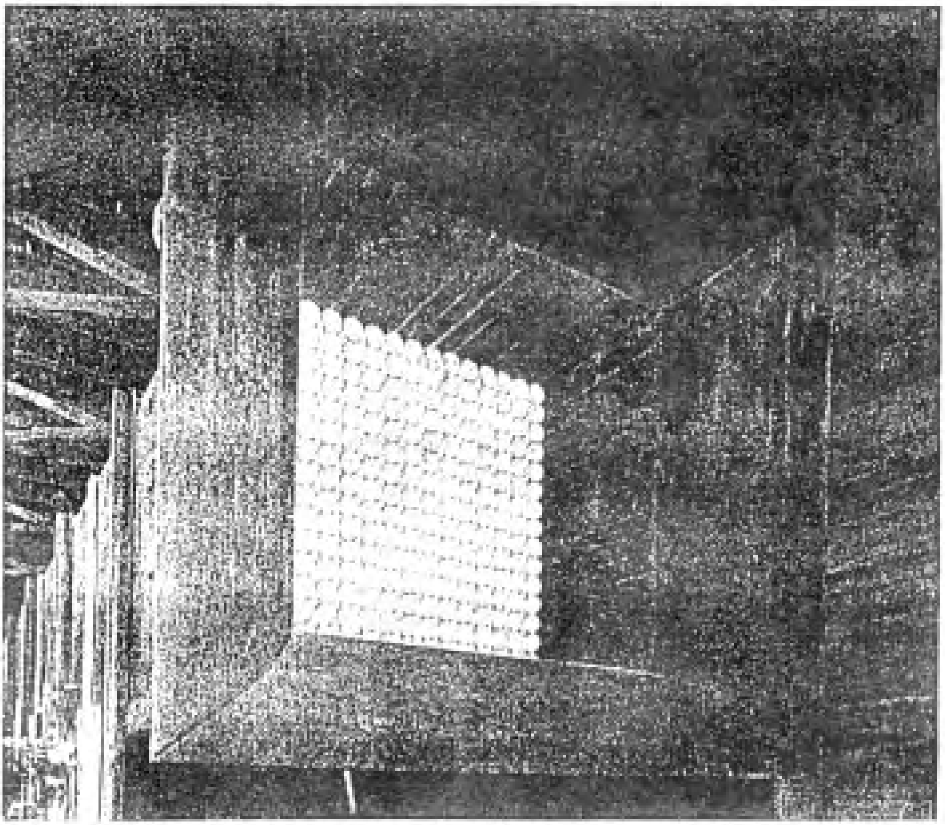


Fig. 2. ENTRANCE NOZZLE, SHOWING END OF HONEYCOMB

This artifice seems to have prevented the howling at high speeds commonly found with thin blades. The propeller has a clearance of ½ inch in the metal cylinder.

The propeller is driven by a “silent” chain from a 10 H. P. interpole motor beneath it. The propeller and motor are mounted on a bracket fixed to a concrete block and are independent of the alignment of the tunnel. Vibration of the motor and propeller can not be transmitted to the tunnel as there is no connection.

The English plans for power contemplate a steady, direct current voltage. Such is not available here. A 15 H. P. induction motor is connected to the mains of the Cambridge Electric Light Company. This motor then turns at a speed proportional to the frequency of the supply current for a given load. Fluctuations of voltage are without sensible effect, and the frequency may be taken as practically constant.

The induction motor is directly connected to a 12 H. P. direct current generator, which is turned at constant speed and which generates, therefore, a constant direct current voltage for given load.

By change of the generator field rheostat and motor field rheostat the propeller speed can be regulated to hold any wind velocity from 4 to 40 miles per hour. The control is very sensitive. Left to itself, the speed of the wind in the tunnel will vary by 2 per cent in 2 or 3 minutes. This variation is so slow that by manipulation of the rheostats the flow can be kept constant within ½ per cent. The cause of the surging of the air is not understood, but is probably due to hunting of the governor of the prime mover in the Cambridge power house causing changes in frequency too small to be apparent. The gustiness of outdoor winds seems to have no effect, although the building is not airtight.

AERODYNAMICAL BALANCE

The aerodynamical balance (pl. 3) was constructed by the Cambridge Scientific Instrument Company, England, to the plans and patterns of the National Physical Laboratory. The balance is described in detail by Mr. L. Bairstow in the Technical Report of the Advisory Committee for Aeronautics, London, 1912–13. For details of operation and the precision of measurements reference may be made to the original article.

In general, the balance consists of three arms mutually at right angles representing the axes of coordinates in space about and along which couples and forces are to be measured. The model is mounted on the upper end of the vertical arm which projects through an oil seal in the bottom of the tunnel.

The entire balance rests on a steel point, bearing in a steel cone. The point is supported on a cast-iron standard secured to a concrete pillar, which in turn rests on a large concrete slab. The balance is then quite free from vibration of the floor, building, or tunnel.

The balance is normally free to rock about its pivot in any direction. When wind blows against the model, the components of the force exerted are measured by determining what weights must be hung on the two horizontal arms to hold the model in position. Likewise the balance is free to rotate about a vertical axis through the pivot. The moment producing this rotation is balanced by a calibrated wire with graduated torsion head.

Force in the vertical axis is measured by means of a fourth arm. The model for this measurement is mounted on a vertical rod which slides freely on rollers inside the main vertical arm of the balance. The lower end of this rod rests on one end of a horizontal arm having a knife edge and sliding weight.

For special work on moments, the interior vertical rod is replaced by another having a small bell crank device on its head which converts a moment about the center of the model into a vertical force to be measured as above (pl. 4).

In this way provision is made for the precise measurement of the three forces and the three couples which the wind may impress on any model held in any unsymmetrical position to the wind.

The balance is fitted with suitable oil dash pots to damp oscillations, and devices for limiting the degrees of freedom to simplify tests in which only one or two quantities are to be measured. The balance can be adjusted to tilt for 1/10,000 pound force on the model. In general, the precision of measurements is not so good as the sensitivity, and in the end is limited by the steadiness of the wind and the skill of the observer.

The weights and dimensions of the balance were verified by the National Physical Laboratory, where also the torsion wires were calibrated.

For ordinary forces, weighings may be considered correct to 0.5 per cent. Naturally for very small forces, such as the rolling moment caused by a small angle of yaw, the measurements can not be so precise.

[*Not included here are the final sections of Hunsaker's report, concerning the alignment of the wind tunnel and the means by which tunnel instrumentation measured air velocity.*]

Document 2-5

G. I. Taylor, "Pressure Distribution Over the Wing of an Aeroplane in Flight," British Advisory Committee for Aeronautics, *Reports and Memoranda*, No. 287 (London, 1916).

Early airplane designers found themselves confronting a paradox. They could obtain repeatable data on new designs using models in wind tunnels—something almost impossible to achieve in flight—but the forces and moments measured on wind tunnel models did not agree with what an actual airplane experienced in flight. While many recognized the paradox, only a few researchers even approached a possible solution to it prior to the introduction of dynamical similarity concepts in the 1920s. Gustav Eiffel grappled with this problem around 1910, using what he called "augments," developed from his experiments, to adjust model data and predict full-size performance. Although Eiffel concluded that "the calculations are in each case in complete accord with the actual conditions observed in flight," his augments were not sufficiently accurate to be universally applicable. With other wind tunnel work, however, Eiffel showed how measurements of the air pressure distribution across the surface of a wing could be correlated to the lift and drag generated by the wing. This work pointed the way toward a useful design technique, but much more work was needed to correlate performance data of models and full-size airplanes before the technique could be fully exploited.

A major problem in determining this correlation was obtaining useful data from airplanes in flight. No one knew how to measure propeller performance and efficiency accurately, so drag could not be directly determined from engine performance. G. I. Taylor reasoned that Eiffel's pressure-distribution techniques would work on an airplane in flight. He devised a rib with pressure taps that could be mounted in a wing along with a clever means to measure and record pressure-distribution data in flight. Taylor still found that a correction, which he believed was due to skin friction, was necessary, but his device worked, and it produced lift and drag coefficient curves of a similar shape to those obtained from wind tunnel models at all but the lowest lift coefficients.

Document 2-5, G. I. Taylor, "Pressure Distribution Over the Wing of an Aeroplane in Flight," 1916.

SUMMARY.—The comparison of the results of experiments performed with model aerofoils in wind channels with the actual performance of an aeroplane in

flight has proved extremely difficult. It has been possible to find the relationship between angle of incidence and lift coefficient for a full-scale machine, but when attempts are made to find the resistance of the wing of an aeroplane in flight, various causes combine to diminish the accuracy of the result. In the first place the thrust of the airscrew is the subject of considerable uncertainty, for it depends on the power of the engine and on the efficiency of the airscrew, neither of which are known accurately. If the thrust of the airscrew were known, the total resistance of the aeroplane would be known, but considerable uncertainty would still exist as to what proportion of the total resistance is due to the wings.

In spite of these various sources of error, there is strong evidence to show that the resistance of a full-scale aeroplane differs from the predictions made as a result of model experiments.

The measurements here described were undertaken with a view to getting further and more direct information as to the relationship between model and full-scale aerofoils. Simultaneous measurements of the pressures at various points round a certain section of the lower wing of a B.E.2C aeroplane were made by means of an apparatus which registered photographically the heights of the liquid in a number of manometer tubes. Observations were taken at various speeds ranging from 50 to 95 miles per hour.

The pressures were integrated, and the normal and longitudinal forces and moments, which act on the wing in the experimental section, were found.

The inclinometer measurements of the angle of incidence corresponding with various lift coefficients were then used to find the drag coefficient of the section.

The results are shown in Fig. 12. For the purpose of comparison the drag curves obtained from model experiments and also from the performance tests of full-scale aeroplanes, are shown in the same figure. It will be seen that the present measurements agree with the performance tests over the greater part of the curves, but that for small lift coefficients the pressure-integration curve indicates a higher drag than the performance tests. The drag curve for the model lies below the pressure-integration curve over its whole range, but on the other hand, the general shapes of the two curves are very similar.

It will be possible to say more about the relationship between the present measurements and the model results when the pressure distribution around a similar section of a model has been measured. This work is now being carried out at the National Physical Laboratory.

The pressure distribution has also been used to obtain the moment coefficient. A curve showing the moment coefficients for various lift coefficients is given. (Fig. 11)

1. The section chosen for these experiments was the old B.E.2C section with a hollow undersurface. A brass rib was made to drawing, and eighteen small copper tubes were fixed to it in the positions shown in Fig. 1. These were filled at the

ends with brass plugs, through which holes 1/32nd inch diameter were drilled. These holes were flush with the surface of the wing.

2. The holes were numbered α, β, 1*a*, 2, 3, 4, 5, 6, 7, 8 on the upper surface, and 9, 10, 11, 12, 13, 14, 15 and 1*b* on the lower surface. There was also a hole numbered (1) in the leading edge of the wing. The positions of the holes are given in terms of co-ordinates in Table (1), the axis of x being along the chord and the axis of y the perpendicular through the leading edge. x and y are expressed as fractions of the chord, so that x varies from 0 to 1.

3. The pressures at the holes were measured simultaneously by means of an apparatus shown in Fig. 2, which consisted of twenty glass manometer tubes, connected at their lower ends to a reservoir which was full of alcohol. Their upper ends were connected by means of rubber tubes with the pipes which led through the interior of the wing to the pressure holes. When the aeroplane was at rest the alcohol stood about half-way up the tubes.

4. When the aeroplane was in flight the height of the liquid in the tubes connected with the various holes registered the pressure at the holes. These heights were recorded photographically. A small electric light, some ten inches from the tubes, was used to cast shadows of the columns of liquid on to some bromide paper, which could be wound between two light-tight boxes past the tubes. The bromide paper was pressed up against the tubes by means of a back (shown leaning up against the apparatus in Fig. 2), and the whole apparatus was shut in by a hinged light-tight door (shown open in Fig. 2). To make an exposure, the small electric light was switched on for half a second.

5. The shadow of the meniscus at the top of the alcohol was very sharp, and it was possible to read the height of the liquid in the tube to 1/10th of a mil1imetre. A specimen of the records obtained is shown in Fig. 3.

6. On testing the apparatus, it was found that the maximum error in measuring pressure by means of the photographic records was 0.4 mm. of alcohol, and the probable error was a little more than 0.1 mm. It was found, however, that the error for a given position of the liquid in a tube was always the same. A table of errors was therefore constructed, and by using this table the pressures could be obtained correct to 0.1 mm. of alcohol.

7. The whole apparatus was hung in the aeroplane by rubber suspensions, so that it might not be affected by the vibration of the aeroplane.

8. As a further preventive to possible rapid movements of the alcohol in the tubes the rubber pipes, which served to connect them with the alcohol reservoir, were constricted by passing them through a series of holes in a brass rod. The sizes of these holes were adjusted by means of screws, so that the time taken by the alcohol to come to rest after a sudden change in speed of the aeroplane was about fifteen seconds.

9. In order to measure the height of the meniscus at the top of the fluid in each tube, it was necessary to know what direction in the plane of the photograph was horizontal. For this purpose the two outside tubes in the apparatus were both connected with the static pressure side of the pitot tube. A straight line was drawn on each photograph touching the two outside meniscuses, and this was used as a base line from which the pressures in the other tubes were measured. This line is shown in Fig. 3.

10. The aeroplane had a constant weight of 2,020 lbs. during the experiments; the lift coefficient and angle of incidence should therefore depend only on the reading of the pitot tube. For a given angle of incidence the pressure at each of the holes should be a given fraction of ρV^2, r being the density of the air and V the velocity of the aeroplane.

11. Hence, to each reading of the pitot tube there should correspond a definite pressure at each hole, whatever the density of the air might be. This was specially convenient because observations could be taken at whatever height the air happened to be calmest, without the necessity of considering its density.

12. One of the central tubes was connected with the high pressure side of the pitot tube, and the reading of the liquid in this was used instead of an air speed indicator to obtain an accurate measure of the airspeed of the machine.

13. In order to find what value of ρV^2 corresponded with a given pitot reading, it was necessary to fly the machine at known speeds through air of known density, and to take the corresponding readings of the liquid in the pitot tube. This was done on a speed course of known length, and it was found that at 60 and at 80 miles per hour the pressure difference between the static pressure tube and the pitot tube was $0.475\rho V^2$. When a pressure head is placed facing the wind in a wind channel, this pressure difference is $\frac{1}{2}\rho V^2$. The discrepancy between the two seems to be due to the disturbing effect of the wings of the machine, which probably increases the pressure in the static pressure tube without affecting the pressure in the pitot head.

14. The pressure at each hole is expressed in all cases as a fraction of ρV^2. This fraction is found by multiplying the ratio of the head of liquid in the tube connected with the hole to the head in the tube connected with the pitot tube by 0.475. The readings for different air speeds of the aeroplane are shown graphically in Figs. 4, 5, and 6. In these diagrams each hole, as numbered on the right-hand side of each curve, is represented by a special mark, and smooth curves have been drawn through the points in order to eliminate accidental errors as far as possible. The magnitude of the accidental errors can be estimated by noticing the distances of the observed points from the smooth curves. In the case of hole 2, for instance, the accidental error which may be expected is about $0.003\rho V^2$, while in the case of holes 10, 1*b*, and 15 it is much greater, being about $0.01\rho V^2$.

15. In the case of hole 10 the source of error was detected. It was found that the fabric was inclined to come away from the rib in the immediate neighbourhood of this hole, so that the pressure recorded was probably intermediate between the pressure outside and the pressure inside the wing. In the cases of holes 1*b* and 15 it seems probable that the errors are due to the peculiar conditions of air flow, which seem to exist immediately underneath a turned-down leading edge.

16. *Pressure integration.*—Having determined the pressure at each hole as accurately as seems possible from present observations, it remains to estimate by integration the forces which act on the aerofoil in the neighbourhood of the section chosen for the pressure distribution experiments.

17. The integrations were effected graphically by the method used by Jones and Patterson in the case of a model aerofoil. Curves were plotted for various speeds of the aeroplane, ranging from 50 to 95 miles per hour by intervals of 5 miles per hour, showing the pressure round the aerofoil as a function of x and as a function of y. These curves, when integrated by means of a plantimeter give the normal and longitudinal force coefficients.

18. Curves were also plotted showing the relationship between (pressure multiplied by x) and x. These curves give the part of the moment coefficient due to the component of pressure perpendicular to the chord. The part of the moment coefficient due to the component parallel to the chord is so small as to be negligible.

19. The points on the pressure curves corresponding to the various holes are taken from the curves of Figs. 4, 5, and 6. In drawing the pressure curves shown in Figs. 7, 8, and 9, it will be seen that it is possible to vary their forms to a certain extent, while at the same time keeping them to the determined points. When the first set of experiments was made there were no pressure holes between 1 and 1*a*. Under these circumstances it was found that very considerable variations in the areas of the curves in Fig. 8 were possible; accordingly the holes a and b were constructed. By making the curves pass through the points corresponding with these two new holes, and bearing in mind certain geometrical limitations, which the curves must comply with, it was found that the differences in their area, due to various possible ways of drawing the curves were not large enough to affect the results.

20. The geometrical limitations alluded to above are obvious. In the case of the normal force and moment curves of Figs. 7 and 9, the curves must lie between $x = 0$ and $x = 1$, and the values of x must increase steadily in going along the curve from the point corresponding with the nose to the point corresponding with the trailing edge. In the case of the longitudinal force curves of Fig. 8, the values of y must increase steadily along the curves from $y = 0$ to $y = 0.0793$.

21. Besides these geometrical limitations there is a hydro-dynamical one. The pressure can not be greater, at any point on the wing, than $\frac{1}{2}\rho V^2$ above the pressure in the surrounding undisturbed air; and, moreover, it appears certain that, at

some point near the nose of the wing, the pressure will attain this value. It seems probable that the pressure in the pitot tube is $\frac{1}{2}\rho V^2$. In this case the pressure in the static pressure tube would be $\frac{1}{2}\rho V^2 - 0.475\rho V^2$, or $0.025\rho V^2$, above the pressure in the surrounding air; and since the pressures measured by the instrument are the differences between the pressures in the holes and the pressure in the static pressure tube, the maximum possible pressure on the diagrams should be $0.475\rho V^2$. On looking at Figs. 4 and 6 it will be seen that the point of maximum pressure, which presumably corresponds with the point where the air divides to pass over and under the wing, crosses hole 1, in the nose, when the aeroplane is flying at 53 miles per hour, while at about 80 miles an hour it crosses hole a. At the highest speed attained—95 miles per hour—it had not crossed the next hole, β.

22. The normal force, longitudinal force, and moment coefficients derived by integrating the curves are given in the 6th, 7th and 8th columns of Table (2). They are denoted by the, symbols $[k_z]_s$, $[k_x]_s$, and $[k_m]_s$, the “s” being used to show that the coefficients apply to the experimental section only. In order to get the lift and drag coefficients of the section explored, it is necessary to find the angle of incidence of the section. The aeroplane was measured and it was found that the angle of incidence of the wing in the neighbourhood of the experimental rib was 18´, or 0.3° less than the mean angle of incidence. The mean angle of incidence has been measured for different mean lift coefficients by means of an inclinometer, and the relationship between them is shown graphically in Fig. 10.

23. The mean lift coefficients for various speeds were obtained by dividing the weight of the aeroplane by ρV^2 and by the area of the wings. The weight of the aeroplane was 2,020 lbs. The area of its wings was 384 square feet. Hence if V be expressed in miles per hour, the mean lift coefficient

[illegible]

The mean angle of incidence, θ, was found from the curve in Fig. 10. The angle of incidence of the experimental rib was 0.3° less than this. These angles are tabulated for various speeds in column 5, Table (2).

24. The lift and drag coefficients of the section, $[k_l]_s$ and $[k_d]_s$, are related to $[k_z]_s$, and $[k_x]_s$ by the relations

$[k_l]_s = [k_z]_s \cos(\theta - 0.3°) - [k_x]_s \sin(\theta - 0.3°)$,

$[k_d]_s = [k_x]_s \cos(\theta - 0.3°) - [k_z]_s \sin(\theta - 0.3°)$,

They are tabulated in columns 9 and 10 of Table (2).

25. In order to find the “scale effect” between the forces on the experimental section and those on the same section of a model, curves of the lift and drag coefficients of the section should be drawn for various angles of incidence. On the other hand, in order to compare the drag of the section with the drag of the whole wing,

as calculated from the performance of an aeroplane, it is in some ways more convenient to draw curves representing the drag of the section for different values of the lift coefficient of the whole machine. For this purpose it is necessary to correct the lift coefficient of the whole machine to what it would be if the mean angle of incidence were the same as that of the experimental section. This is done by taking the lift coefficient corresponding with the angle of incidence of the experimental section from the curve in Fig. 10 instead of the actual mean lift coefficient given in column 2 of Table (2). These corrected lift coefficients are given in column 4.

26. *Results.*—The results have been plotted in the form of curves representing the lift, drag, and moment coefficients of the experimental section for various values of the lift coefficient of the whole aeroplane. These curves are shown in Figs. 11 and 12. In the case of the drag curve it must be remembered that the drag obtained from pressure integration, shown as the full curve in Fig. 12, is less than the actual drag because no account is of tangential forces (*i.e.*, skin friction) in pressure integration. The effect of skin friction may be allowed for roughly by adding 0.0035 to the drag coefficient. After making this allowance the final drag curve resulting from these experiments is shown as curve B, Mg. 12.

27. For the purpose of comparison the drag curves obtained from model experiments (curve D) and also from the performance tests of full-scale aeroplanes (curve C) are shown in the same figure. It will be seen that pressure distribution experiments indicate a higher drag than that obtained by either of the other methods, but it must be remembered that curve B represents the drag on a certain section of the lower plane of a biplane, while curves C and D apply to the mean drag of both wings. It is quite possible also that the drag at the ribs is greater than the drag at the intermediate points of the wing where the fabric sags below the true wing section; this is not probable, however, in view of the fact that model tests show that the drag of a scalloped wing is the same as that of a smooth wing.

28. It is not possible to compare the moment coefficient of a model with $[k_m]_s$ because no measurements of the forces acting on a biplane fitted with B.E.2C section have been made. On the other hand it has been found that the moment curve for a single B.E.2C section wing is practically identical with the moment curve for R.A.F. 6. The moment coefficients of the lower wing of a biplane of R.A.F. 6 section have been measured, and it seems probable that they will be nearly the same as the moment coefficients we require.

29. The moment curve for the lower wing of a biplane of R.A.F. 6 section is shown as a dotted curve in Fig. 11. It will be seen that it lies very near the moment curve for the experimental section.

TABLE 1.

TABLE SHOWING THE x AND y CO-ORDINATES OF THE EXPERIMENTAL TUBES.

Tube.	x	y
1	0·0015	0·0052
1a	0·013	0·0230
2	0·040	0·0408
3	0·086	0·0592
4	0·185	0·0748
5	0·385	0·0772
6	0·584	0·0684
7	0·783	0·0478
8	0·930	0·0221
9.	0·931	0·0025
10	0·785	0·0077
11	0·583	0·0151
12	0·387	0·0212
13	0·193	0·0215
14	0·099	0·0135
15	0·051	0·0073
1b	0·026	0·0034
α	0·0016	0·0105
β	0·0077	0·0172

TABLE 2.

Complete Aeroplane.					Experimental Portion.				
1. Air Speed m.p.h.	2. k_l	3. Angle of incidence, θ	4. k_l at $\theta - 0·3°$.	5. Angle of incidence, $\theta - 0·3$.	6. $[k_z]_r$	7. $[k_x]_r$	8. $[k_m]_r$	9. $[k_l]_r$	10. $[k_d]_r$
		°		°					
50	0·407	8·65	0·397	8·35	0·322	−0·0126	−0·1096	0·320	0·0343
55	0·337	6·40	0·327	6·10	0·275	−0·0031	−0·1026	0·272	0·0249
60	0·283	4·47	0·272	4·17	0·230	+0·0024	−0·0917	0·230	0·0191
65	0·241	3·56	0·230	3·26	0·195	0·0060	−0·0826	0·194	0·0171
70	0·208	2·68	0·197	2·38	0·164	0·0083	−0·0736	0·163	0·0147
75	0·181	1·93	0·170	1·63	0·139	0·0102	−0·0689	0·139	0·0142
80	0·159	1·37	0·147	1·07	0·119	0·0115	−0·0624	0·119	0·0138
85	0·141	0·91	0·129	0·61	0·103	0·0123	−0·0603	0·103	0·0134
90	0·126	0·53	0·114	+0·23	0·187	0·0132	−0·0556	0·087	0·0136
95	0·113	0·19	0·101	−0·11	0·075	0·0149	−0·0563	0·075	0·0148

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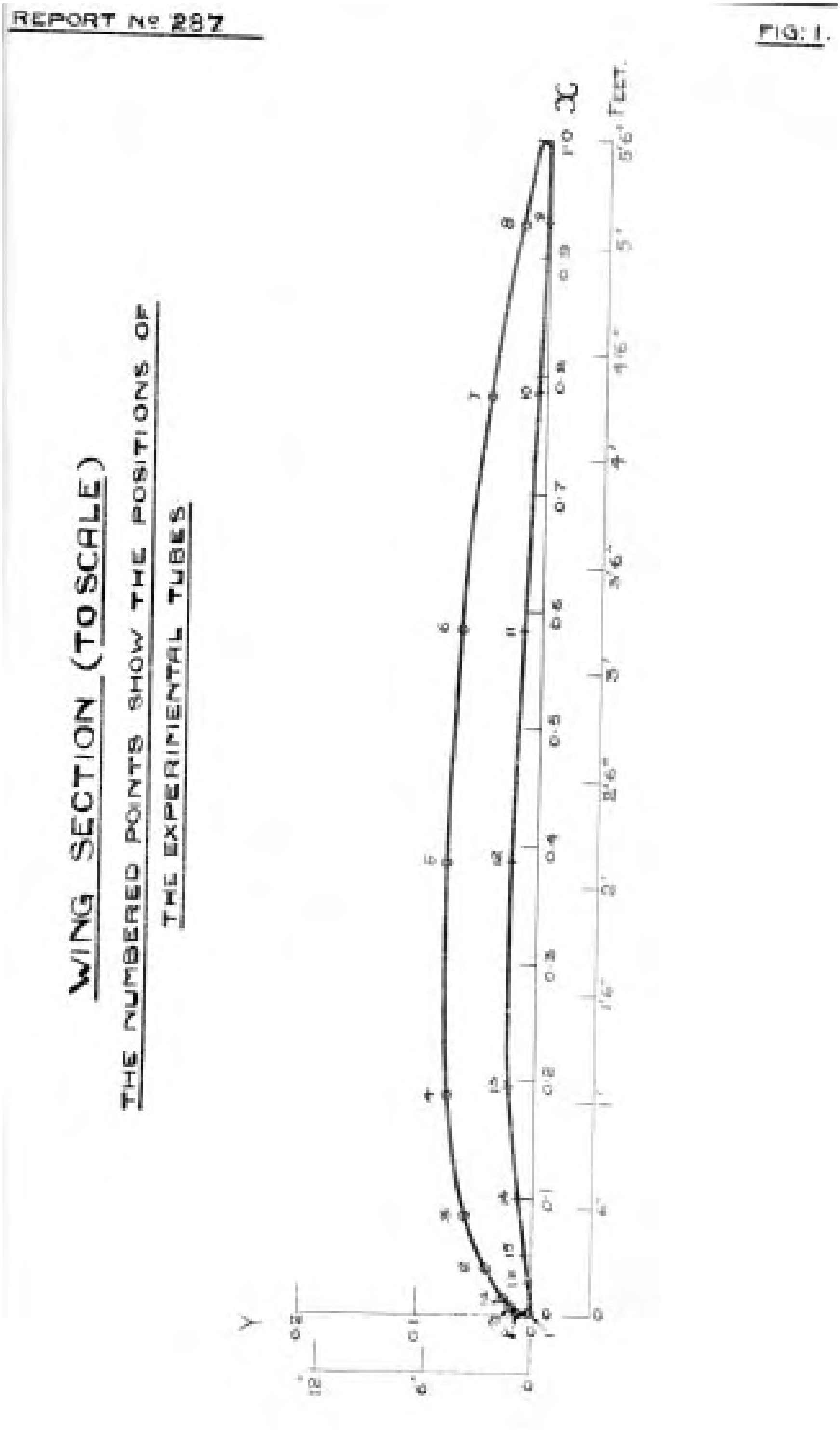

REPORT No 287
FIG: I.
WING SECTION (TO SCALE)
THE NUMBERED POINTS SHOW THE POSITIONS OF THE EXPERIMENTAL TUBES
X
Y
0
0·1
0·2
0·3
0·4
0·5
0·6
0·7
0·8
0·9
1·0
6"
1'
1'6"
2'
2'6"
3'
3'6"
4'
4'6"
5'
5'6" FEET.
0·1
0·2
6"
12"
1
2
3
4
5
6
7
8
9
10
11
12
13
14
15
16
17
18

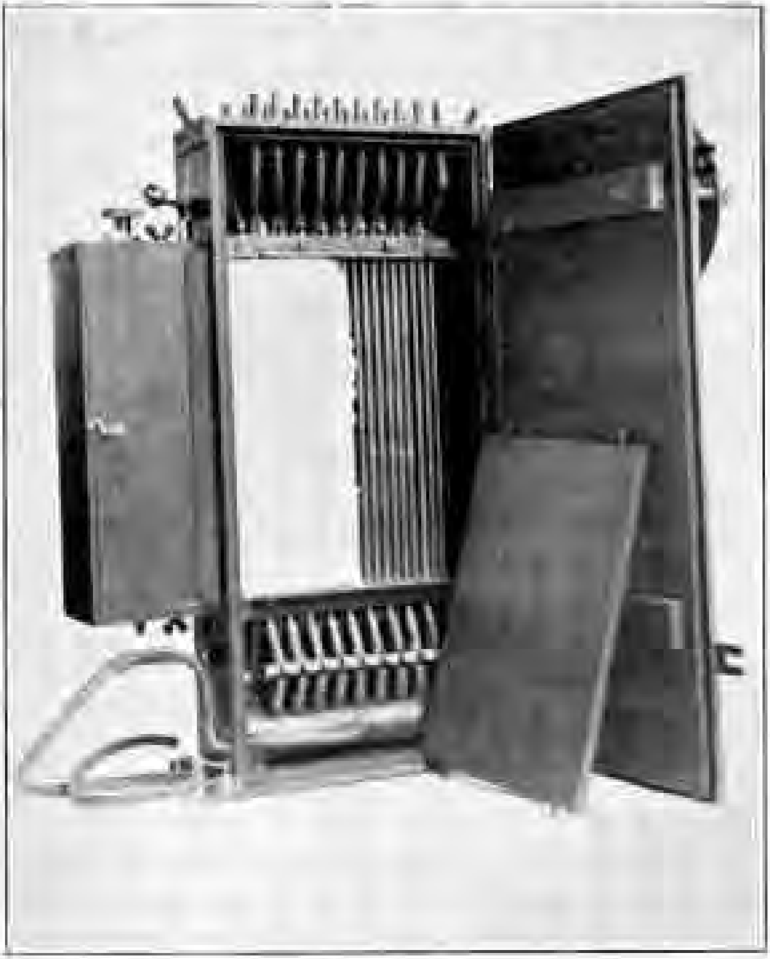
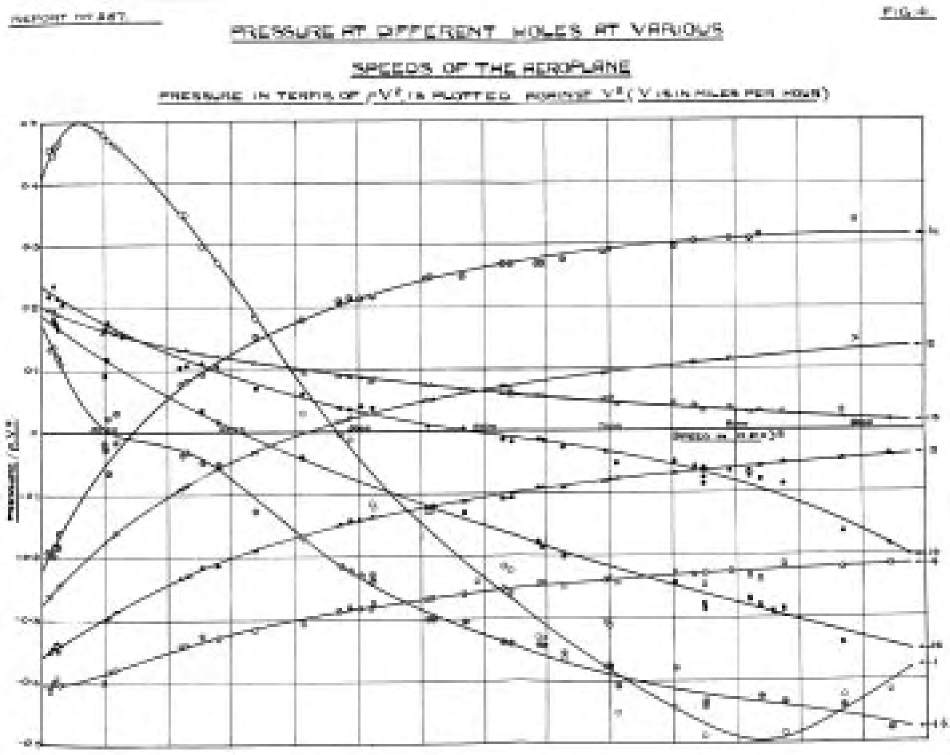


Fig. [illegible]

Fig. 8.

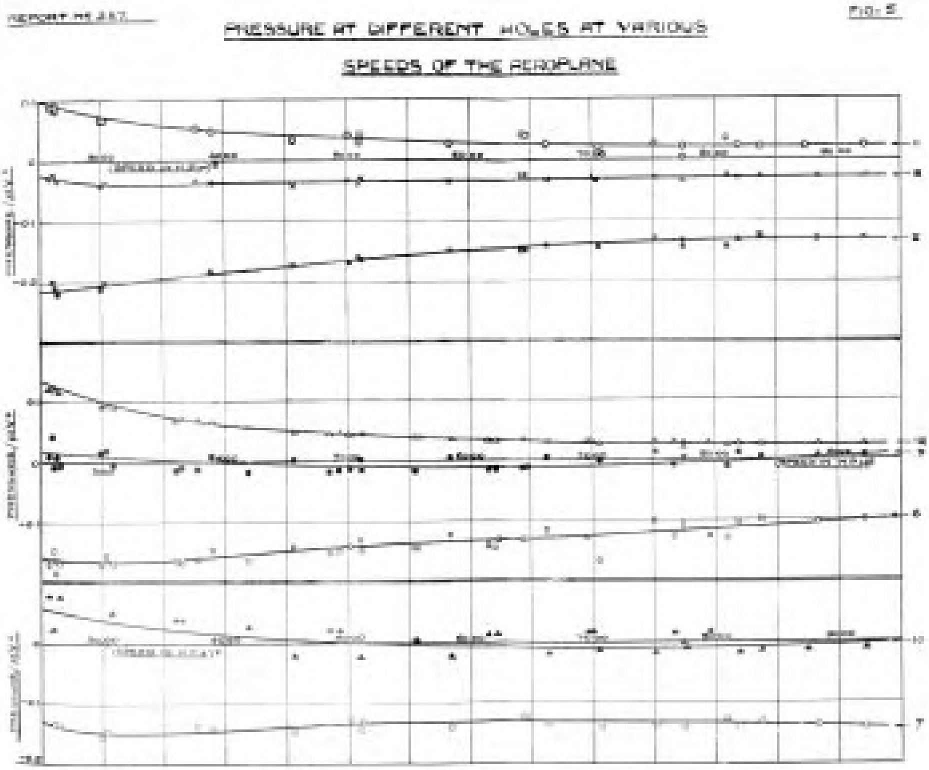


REPORT No 287.

FIG. 4.

PRESSURE AT DIFFERENT HOLES AT VARIOUS SPEEDS OF THE AEROPLANE

PRESSURE IN TERMS OF ρV² IS PLOTTED AGAINST V² (V IS IN MILES PER HOUR)


REPORT No 257
FIG. 5
PRESSURE AT DIFFERENT HOLES AT VARIOUS
SPEEDS OF THE AEROPLANE
SPEED IN M.P.H.
PRESSURE / ½ρV²

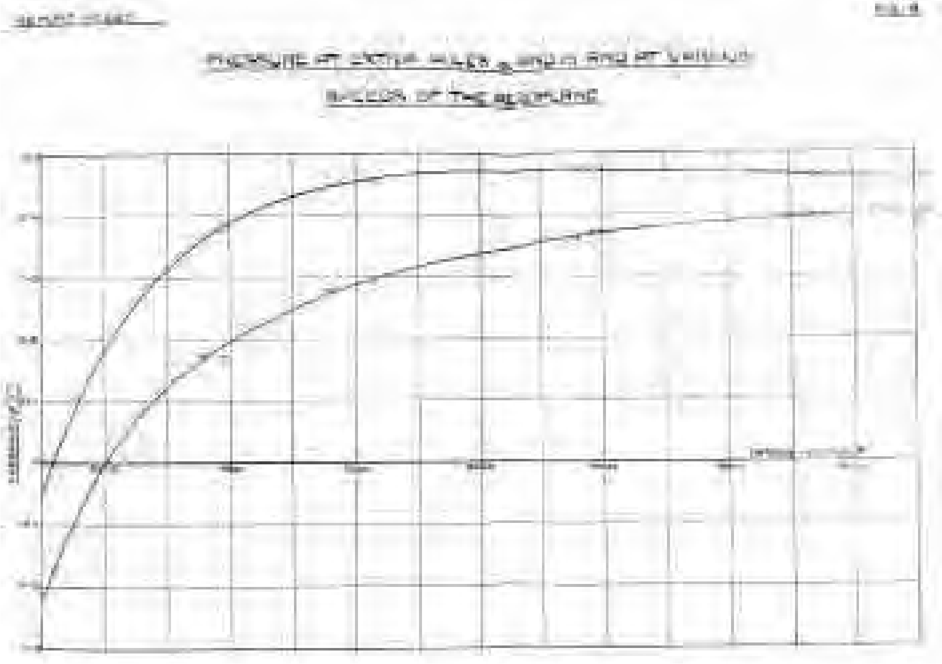
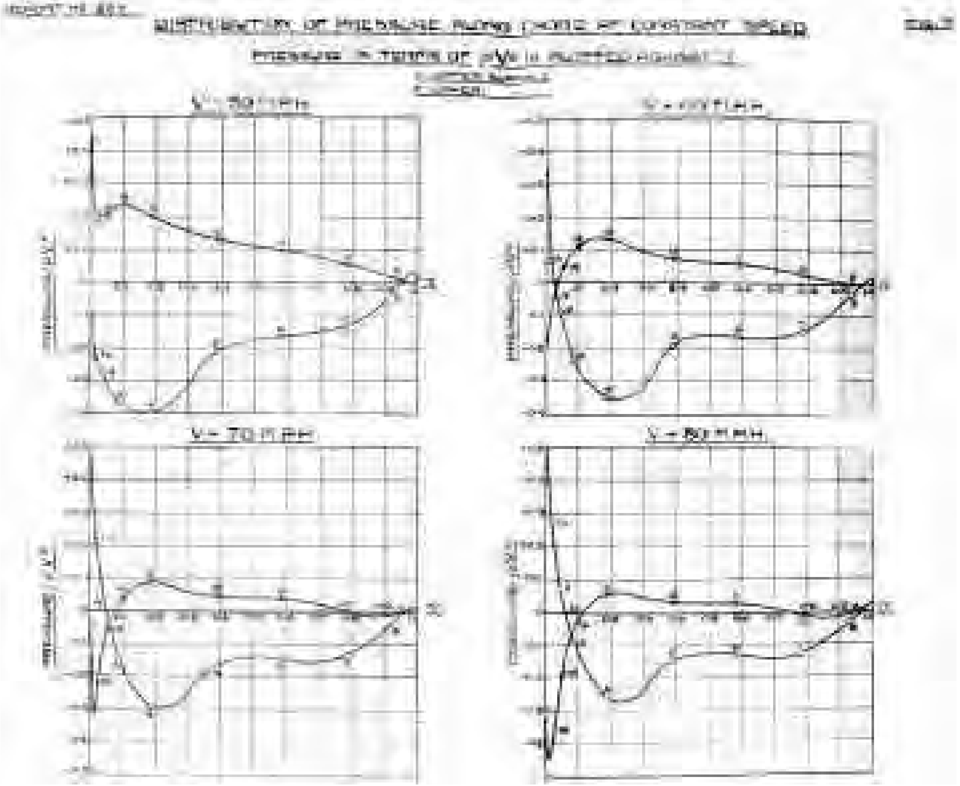


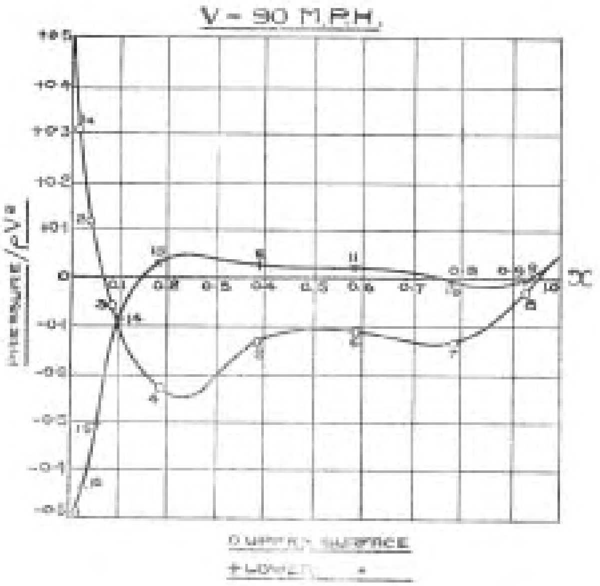
FIG. 6
SPEEDS OF THE AEROPLANE

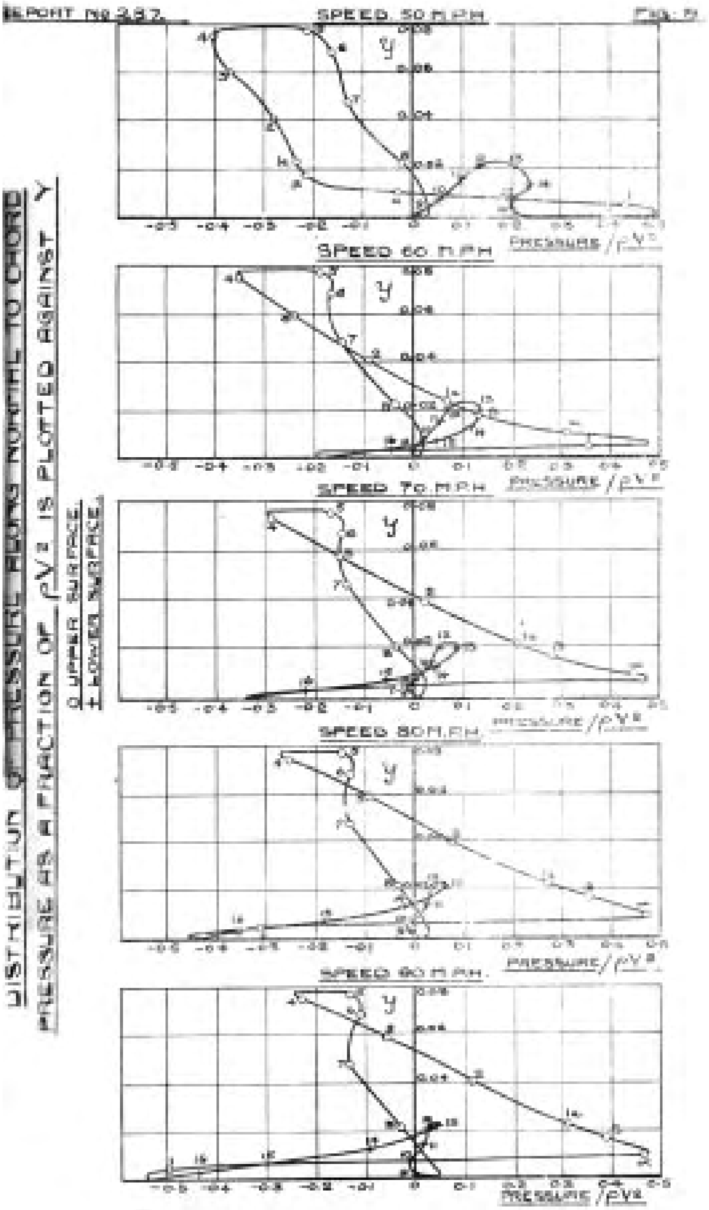

DISTRIBUTION OF PRESSURE ALONG CHORD AT CONSTANT SPEED
V = 50 M.P.H.
V = 70 M.P.H.
V = 80 M.P.H.

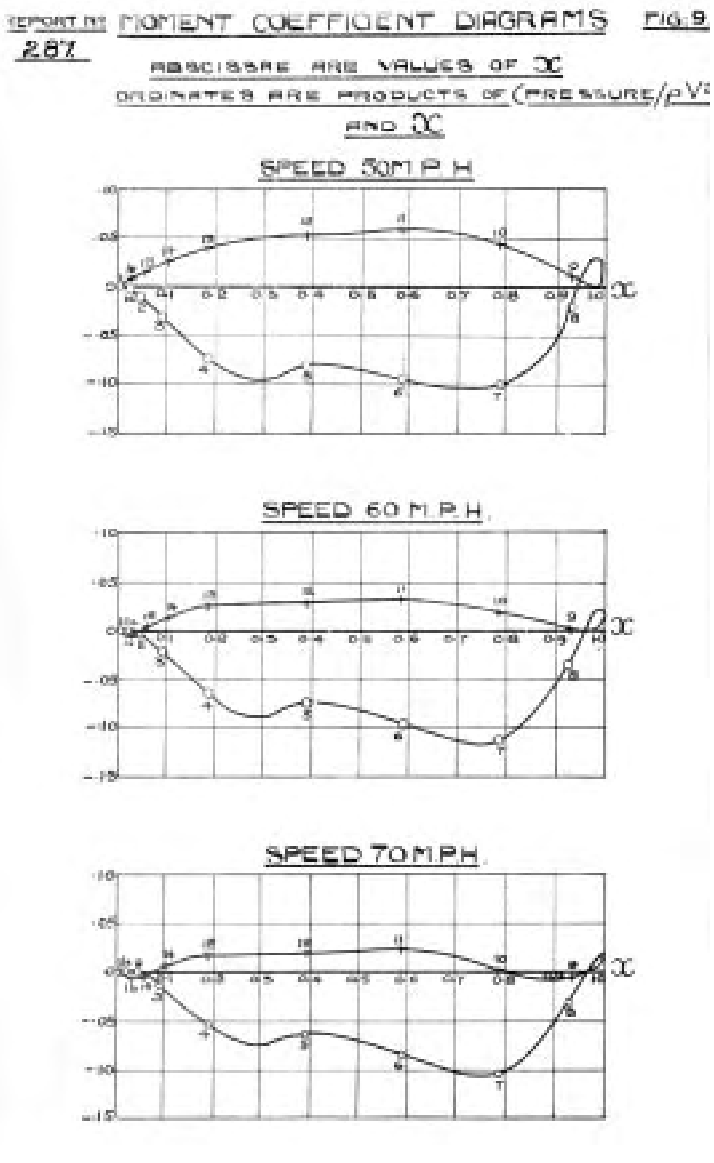
REPORT No 287

FIG: 7.A.

DISTRIBUTION OF PRESSURE ALONG
CHORD AT CONSTANT SPEED
PRESSURE IN TERMS OF ρV^2 IS PLOTTED
AGAINST x.




REPORT No 287.
FIG. 5
DISTRIBUTION OF PRESSURE ACTING NORMAL TO CHORD
PRESSURE AS A FRACTION OF ρV² IS PLOTTED AGAINST Y
O UPPER SURFACE
+ LOWER SURFACE
SPEED 50 M.P.H
SPEED 60 M.P.H
SPEED 70 M.P.H
SPEED 80 M.P.H
SPEED 90 M.P.H
y
0.08
0.06
0.04
0.02
-0.5
-0.4
-0.3
-0.2
-0.1
0
0.1
0.2
0.3
0.4
0.5
PRESSURE / ρV²

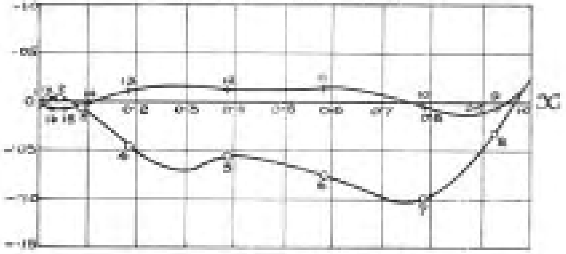

REPORT No 287
MOMENT COEFFICIENT DIAGRAMS
FIG. 9
ABSCISSAE ARE VALUES OF x
ORDINATES ARE PRODUCTS OF (PRESSURE/ρV²) AND x
SPEED 50 M.P.H.
SPEED 60 M.P.H.
SPEED 70 M.P.H.
·10
·05
0
−·05
−·10
−·15
0·1
0·2
0·3
0·4
0·5
0·6
0·7
0·8
0·9
1·0
x

REPORT No 287 FIG: 9.a

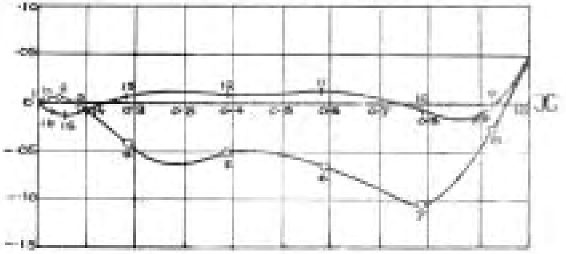
MOMENT COEFFICIENT DIAGRAMS

ABSCISSAE ARE VALUES OF x
ORDINATES ARE PRODUCTS OF (PRESSURE/ρV^2)
AND x

SPEED 80 M.P.H.

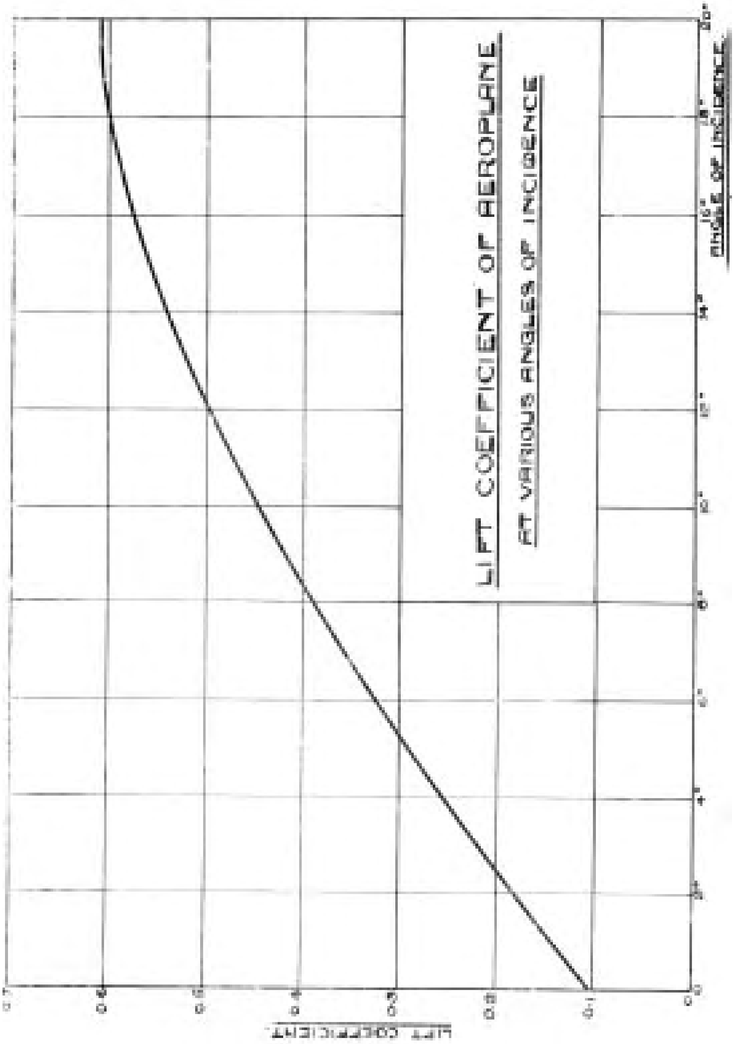


SPEED 90 M.P.H.



REPORT No 287.

FIG 10.

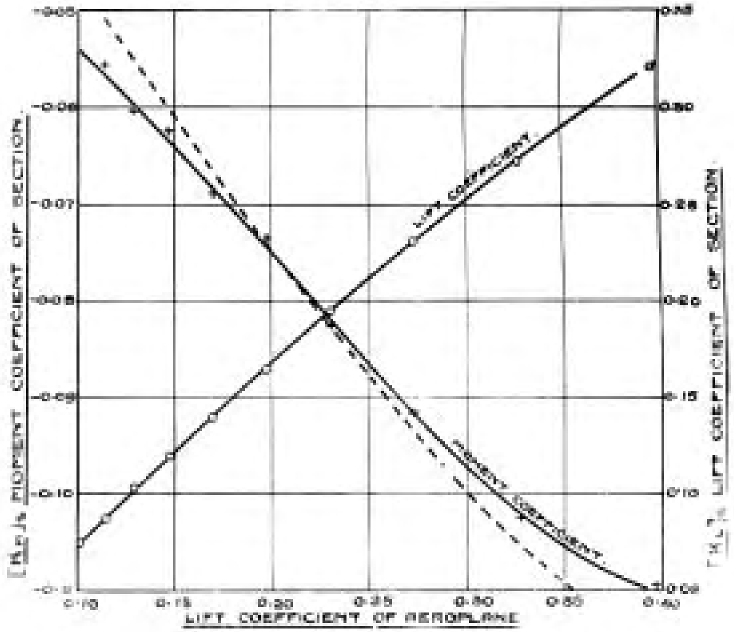


REPORT Nº 287

FIG: 11.

CURVES SHOWING LIFT COEFFICIENT (o) AND MOMENT COEFFICIENT (x), OF THE SECTION, PLOTTED AGAINST THE LIFT COEFFICIENT OF THE AEROPLANE.

THE DOTTED CURVE SHOWS THE MOMENT COEFFICIENT OF THE LOWER PLANE OF A BIPLANE WITH WINGS OF R.A.F. SECTION, OBTAINED FROM MODEL EXPERIMENTS.



REPORT No 887 FIG. 12

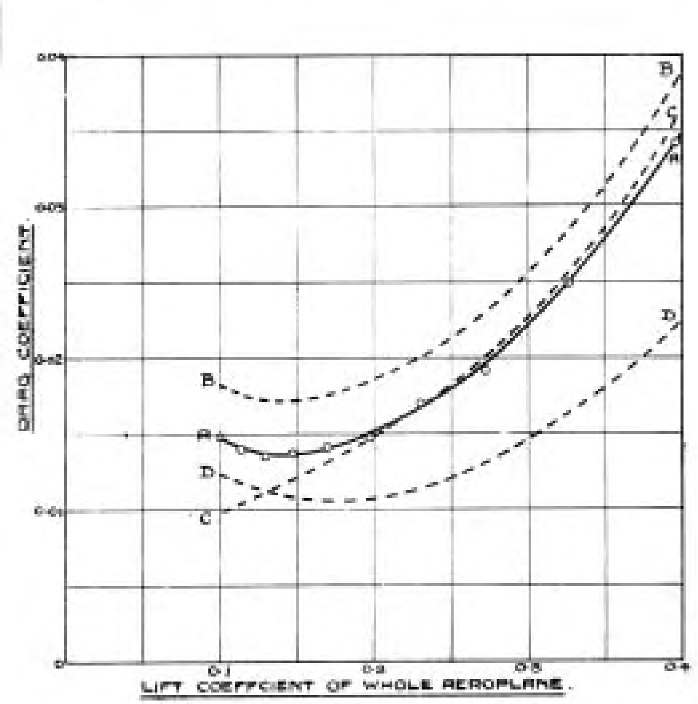
DRAG CURVES.

A. DRAG COEFFICIENT OF SECTION FOUND BY PRESSURE INTEGRATION, ($[K_D]_S$)

B. CURVE (A) WITH 0·0035 ADDED FOR SKIN FRICTION ($[K_D]_S + 0{\cdot}0035$).

C. MEAN DRAG CURVE OF WINGS OBTAINED FROM AEROPLANE PERFORMANCE.

D. DRAG COEFFICIENT OF MODEL.



Document 2-6

Henry T. Tizard, "Methods of Measuring Aircraft Performances," Aeronautical Society of Great Britain, *Aeronautical Journal*, No. 82 (April–June 1917).

To be useful, aircraft flight testing demands accuracy, repeatability, and continuity in two crucial areas: flight instruments and test flight methods. As World War I progressed in Europe, the fledgling air forces and airplane manufacturers increasingly realized that no standards or consistency existed for such basic performance parameters as rate of climb and altitude. Even the idea of "level flight" was an uncertain concept due to the variations experienced with aneroid altimeters. If, however, airplanes were to be thoroughly evaluated and their performance envelopes determined for the pilots who would fly them, some standards were essential. England's Royal Aircraft Factory led the effort in establishing what H. T. Tizard called "the general principles of . . . scientific testing of airplanes," principles quickly adopted by the Royal Flying Corps, in which Tizard served as a pilot with the rank of captain.

Tizard's "Methods of Measuring Aircraft Performances" was notable in two areas. First, he explained the effects that normal variations in air temperature, pressure, and density had on the accuracy of contemporary aircraft instruments, particularly altimeters and airspeed indicators, and he discussed methods to recognize and minimize errors. Perhaps of greater interest are his comments regarding the flyers—those now known as test pilots. Here we see possibly the earliest published recognition that test pilots play a unique role in aircraft development that requires special training. Tizard mentioned this repeatedly in his paper, noting that, "it is . . . the flyer on whom the accuracy of the tests depends. I feel that too great stress can not be laid on this; he is the man who does most of the experiments, and . . . he requires training and a great deal of practice."

Tizard himself went on to a distinguished career in aeronautical research. Knighted for his contributions, in 1935 Sir Henry Tizard was appointed the chair of the RAF's newly established Committee for the Scientific Survey of Air Defense. In that role he sponsored R. A. Watson-Watt's development of Radio Direction Finding, or "radar," and helped to develop the vital air defense system that defeated the World War II German aerial assault in the strategically crucial 1940 Battle of Britain.

Document 2-6, Henry T. Tizard,
"Methods of Measuring Aircraft Performances," 1916.

AEROPLANE TESTING.

The accurate testing of aeroplanes is one of the many branches of aeronautics which have been greatly developed during the war, and especially during the last year. For some months after the war began a climb to 3,000 to 5,000 feet by aneroid and a run over a speed course was considered quite a sufficient test of a new aeroplane; now we all realise that for military reasons certainly, and probably for commercial reasons in the future, it is the performance of a machine at far greater heights with which we are mainly concerned. In this paper I propose to give a short general account of some of the methods of testing now in use at the Testing Squadron of the Royal Flying Corps, and to indicate the way in which results of actual tests may be reduced, so as to represent as accurately as possible the performance of a machine independently of abnormal weather conditions, and of the time of the year. For obvious reasons full details of the tests and methods employed can not yet be given. So far as England is concerned, I believe that the general principles of what may be called the scientific testing of aeroplanes were first laid down at the Royal Aircraft Factory. Our methods of reduction were based on theirs to a considerable extent, with modifications that were agreed upon between us; they have been still further modified since, and recently a joint discussion of the points at issue has led to the naval and military tests being coordinated, so that all official tests are now reduced to the same standard. It should be emphasised that once the methods are thought out scientific testing does not really demand any high degree of scientific knowledge; in the end the accuracy of the results really depends upon the flyer, who must be prepared to exercise a care and patience unnecessary in ordinary flying. Get careful flyers whose judgment and reliability you can trust and your task is comparatively easy; get careless flyers and it is impossible.

At the outset it may be useful to point out by an example the nature of the problems that arise in aeroplane testing. Suppose that it is desired to find out which of two wing sections is most suitable for a given aeroplane. The aeroplane is tested with one set of wings, which are then replaced by the other set, and the tests repeated some days later. The results might be expressed thus:—

	A Wings.	B Wings.
Speed at 10,000 ft.	90 m.p.h.	93 m.p.h.
Rate of climb at 10,000 ft.	25ft. a minute.	300ft. a minute.

Now the intelligent designer knows, or soon will know, that firstly an aneroid may indicate extremely misleading "heights," and secondly, that even if the actual height above the ground is the same in the two tests the actual conditions of atmospheric pressure and temperature may have been very different on the two days. He will therefore say, what does that 10,000 mean? Do you mean that your aneroid read 10,000 feet, or do you mean 10,000 feet above the spot you started from, or 10,000 feet above sea level? If he proceeds to think a trifle further he will say—what was the *density* of the atmosphere at your 10,000 feet; was it the same in the two tests? If not, the results do not convey much. There he will touch the keynote of the whole problem, for it is on the density of the atmosphere that the whole performance of an aeroplane depends; the power of the engine and the efficiency of the machine depend essentially on the density, the resistance to the motion of the machine through the air is proportional to the density, and so finally is the lift on the wings. None of these properties are proportional solely to the pressure of the atmosphere, but to the density, that is the weight of air actually present in unit volume. It follows that it is essential when comparing the performances of machines to compare them as far as possible under the same conditions of atmospheric density, not as is loosely done at the same height above the earth, since the density of the atmosphere at the same height above the earth may vary considerably on different days, and on the same day at different places.

At the same time, in expressing the final results, this principle may be carried too far. Thus, if the speed of a machine were expressed as 40 metres a second at a density of 0.8 kilogrammes per cubic metre, the statement, though it may be strictly and scientifically accurate, will convey nothing to 99 per cent of those directly concerned with the results of the test. The result is rendered intelligible and indeed useful by the form "90 m.p.h. at 10,000 feet," or whatever it is. With this form of statement, in order that all the statements of results may be consistent and comparative, we must be careful to mean by "10,000 feet" a certain definite density, in fact the average density of the atmosphere at a height of 10,000 feet above mean sea level. This is what the problem of "reduction" of tests boils down to; what is the relation between atmospheric density and height above sea level? This knowledge is obtained from meteorological observations. We have collected all the available data—mostly unpublished—with results shown in the following table:—

Table I.

MEAN ATMOSPHERIC PRESSURE, TEMPERATURE AND DENSITY AT VARIOUS HEIGHTS ABOVE SEA LEVEL.

Height inkilometers	Height in equivalent feet.	Mean pressure in millibars.	Mean temp. in absolute degrees centigrades.	Mean density in kgm. per cubic meter.
0	0	1,014	282	1.253

1	3,280	900	278	1.128
2	6,560	795	273	1.014
3	9,840	699	268	0.909
4	13,120	615	262	0.818
5	16,400	568	255	0.735
6	19,680	469	248	0.658
7	22,960	407	241	0.589

These are the mean results of a long series of actual observations made by Mr. W.H. Dines, F.R.S. It is convenient to choose some density as standard, call it unity, and refer all other densities as fractions or per centages of this "standard density." We have taken, in conformity with the R.A.F., the density of dry air at 76m.m. pressure and 16 degrees centigrade as our standard density; it is 1.221 kgm. per cubic metre. The reason this standard has been taken is that the air speed indicators in use are so constructed as to read correctly at this density, assuring the law:

$$p=\tfrac{1}{2}\rho V^2$$

where V is the air speed, p the pressure obtained, ρ the standard density.

In some ways it would doubtless be more convenient to take the average density at sea level as the standard density, but it does not really matter what you take so long as you make your units quite clear. Translated into feet, and fraction of standard density, the above table becomes:—

Table II.

Height in feet.	Per centage of standard density.
0	102.6
1,000	99.4
2,000	96.3
3,000	93.2
4,000	90.3
5,000	87.4
6,000	84.6
*6,500	83.3
7,000	81.9
8,000	79.2
9,000	76.5
*10,000	74.0
11,000	71.7
12,000	69.5
*13,000	67.3

14,000	65.2
15,000	63.0
16,000	61.1
*16,500	60.1
17,000	59.1
18,000	57.1
19,000	55.2
20,000	53.3

*6,500 feet is introduced as corresponding roughly to the French test height of 2,000 metres. 10,000 feet similarly corresponds roughly to comparing aeroplane test performances to the French standard of 3,000 metres, and similarly for 13,000 and 16,500 feet.

Let us briefly consider what these figures mean. For example, we say that the density at 10, 000 feet is 74 per cent of our standard density, but it is not meant that at 10, 000 feet above mean sea level the atmospheric density will always be 74 per cent of the standard density. Unfortunately for aeroplane tests this is far from true. The atmospheric density at any particular height may vary considerably from season to season, from day to day, and even from hour to hour; what we do mean is that if the density at 10,000 feet could be measured every day, then the average of the results would be, as closely as we can tell at present, 74 per cent of the standard density.

The above table may therefore be taken to represent the conditions prevailing in a "normal" or "standard" atmosphere, and we endeavour, in order to obtain a strict basis of comparison, to reduce all observed aeroplane performances to this standard atmosphere, i.e., to express the final results as the performance which may be expected of the aeroplane on a day on which the atmospheric density at every point is equal to the average density at the point. Some days the aeroplane may put up a better performance, some days a worse, but on the average, if the engine power and other characteristics of the aeroplane remain the same, its performance will be that given.

It must be remembered that a standard atmosphere is a very abnormal occurrence; besides changes in density there may occur up and down air currents which exaggerate or diminish the performance of an aeroplane, and which must be taken carefully into account. They show themselves in an otherwise unaccountable increase or decrease in rate of climb or in full speed flying level at a particular height.

We now pass to the actual tests, beginning with a description of the observations which have to be made and thereafter to the instruments necessary. The tests resolve themselves mainly into

(a) A climbing test at the maximum rate of climb for the machine.

(b) Speed tests at various heights from the "ground" or some other agreed low level upwards.

Experience agrees with theory in showing that the best climb is obtained by keeping that which is frequently called the air speed of an aeroplane, namely, the indications of the ordinary air speed indicator, nearly constant whatever the height. [In other words ρV^2 is kept constant.] We can look at this in this way. There is a limiting height for every aeroplane above which it can not climb; at this limiting height, called the ceiling of the machine, there is only one speed at which the aeroplane will fly level, at any other air speed higher or lower it will descend. Suppose this speed is 55 m.p.h. on the air speed indicator. Then the best rate of climb from the ground is obtained by keeping the speed of the machine to a steady indicated 55 m.p.h. Fortunately a variation in the speed does not make very much difference to the rate of climb; for instance, a B.E.2C with a maximum rate of climb at 53 m.p.h. climbs just as fast up, say, to 5,000 feet at about 58 m.p.h. This is fortunate as it requires considerable concentration to keep climbing at a steady air speed, especially with a light scout machine; if the air is at all "bumpy" it is impossible. At great heights the air is usually very steady, and it is much easier to keep to one air speed. It is often difficult to judge the best climbing speed of a new machine; flyers differ very much on this point, as on most. The Testing Squadron, therefore, introduced some time ago a rate of climb indicator intended to show the pilot when he is climbing at the maximum rate. It consists of a thermos flask, communicating with the outer air through a thermometer tube leak. A liquid pressure gauge of small bore indicates the difference of pressure between the inside and outside of the vessel. Now, when climbing, the atmospheric pressure is diminishing steadily; the pressure inside the thermos flask tends therefore to become greater than the outside atmospheric pressure. It goes on increasing until air is being forced out through the thermometer tubing at such a rate that the rate of change of pressure inside the flask is equal to the rate of change of atmospheric pressure due to climbing. When climbing at a maximum rate, therefore, the pressure inside the thermos flask is a maximum. The pilot therefore varies his air speed until the liquid in the gauge is as high as possible, and this is the best climbing speed for the machine.

What observations during the test are necessary in order that the results may be reduced to the standard atmosphere? Firstly, we want the time from the start read at intervals, and the height reached noted at the same time. Here we encounter a difficulty at once, for there is no instrument which records height with accuracy. The aneroid is an old friend now of aeronauts as well as of mountaineers, but although it has often been tentatively exposed, it is doubtful whether 1 per cent of those who use it daily realise how extraordinarily rare it is that it ever does what it is supposed to do, that is, indicate the correct height above the ground, or starting place. The faults of the aeroplane aneroid are partly unavoidable and partly due to those who first laid down the conditions of its manufacture. An aneroid is an instrument which in the first place measures only the pressure of the surrounding air. Now if p_1 and p_2 are the pressures at two points in the atmosphere, the difference of height between these points is given very closely by the relation,

$$h = k\theta \log_e \frac{p_1}{p_2}$$

where θ is the average temperature, expressed in "absolute" degrees, of the air between the two points and k is a constant. It is obvious that if we wish to graduate an aneroid in feet we must choose arbitrarily some value for θ. The temperature that was originally chosen for aeroplane aneroids was 50 degrees Fahrenheit, or 10 degrees centigrade. An aneroid, as now graduated, will therefore only read the correct height in feet if the atmosphere has a uniform temperature of 50 degrees Fahrenheit from the ground upwards, and it will be the more inaccurate the greater the average temperature between the ground and the height reached differs from 50 degrees Fahr. Unfortunately 50 degrees F. is much too high an average temperature; to take an extreme example it is only on the hottest days in summer, and even then very rarely, that the average temperature between the ground and 20, 000 feet will be as high as 50 degrees F. On these very rare occasions an aneroid will read approximately correctly at high altitudes; otherwise it will always read too high. In winter it may read on cold days 2,000 feet too high at 16,000 feet, i.e., it will indicate a height of 18,000 feet when the real height is only 16,000. It is always necessary therefore to "correct" the aneroid readings for temperature. The equation

$$H = \frac{273 + t}{283} \times h$$

gives us the necessary correction. Here H is the true difference in height between any two points, t the average temperature in degrees centigrade between the points, and h the difference in height indicated by aneroid. It is convenient to draw a curve showing the necessary factors at different temperatures, some of which are given in the following table:—

Table III.
Aneroid Correction Factors

Temperature.	Correction factor.
70° Fahr.	1.040
50°	1.000
30°	0.961
10°	0.922
-10°	0.883

For example, if a climb is made through 1,000 feet by aneroid and the average temperature is 10 degrees Fahr., the actual distance in feet is only 1,000 x 0.922 = 922 feet. The above equation is probably quite accurate enough for small differences of height—up to 1,000 feet say—and approximately so for bigger differences.

The magnitude of the correction which may be necessary shows how important it is that observations of temperature should be made during every test. For this purpose a special thermometer is attached to a strut of the machine, well away from the fuselage, and so clear of any warm air which may come from the engine. The French, I believe, do not measure temperature, but note the ground temperature at the start of a test, and assume a uniform fall of temperature with height. This, undoubtedly, may lead to serious errors. The change of temperature with height is usually very irregular, and only becomes fairly regular at heights well above 10,000. [*Tizard illustrates this with a chart entitled "Variations of Temperature with Height," omitted here, which displays the data with a series of curves.*]

The aneroid being what it is, one soon comes to the conclusion that the only way to make use of it in aeroplane tests is to treat it purely as a pressure instrument. For this reason it is best to do away with the zero adjustment for all test purposes and lock the instrument so that the zero point on the height scale corresponds to the standard atmospheric pressure of 29.9 inches or 760mm. of mercury. Every other height then corresponds to a definite pressure; for instance, the locked aneroid reads 5,000 feet when the atmospheric pressure is 24.8ins., and 10,000 feet when it is 20.7ins., and so on. If the temperature is noted at the same time as the aneroid reading, we then know both the atmospheric pressure and temperature at the point, and hence the density can be calculated, or, more conveniently, read off curves drawn for the purpose. The observations necessary (after noting the gross aeroplane weight, and nett or useful weight carried) are therefore, (i.) aneroid height every 1,000 feet, (ii.) time which has elapsed from the start of the climb, and (iii.) temperature; to these should be added also (iv.) the air speed, and (v.) engine revolutions at frequent intervals. The observed times are then plotted on squared paper against the aneroid heights and a curve drawn through them. From this curve the rate of climb at any part (also in aneroid feet) can be obtained by measuring the tangent to the curve at the point. This is done for every 1,000 feet by aneroid. The true rate of climb is then obtained by multiplying the aneroid rate by the correction factor corresponding to the observed temperature. These true rates are then plotted afresh against standard heights and from this curve we can obtain the rates of climb corresponding to the standard heights 1,000, 2,000, 3,000, etc. Knowing the change of rate of climb with height, the time to any required height is best obtained by graphical integration. The following table gives the results of an actual test:—

Table IV.

Machine Engine

Date 27/12/16.

From Curve

Height in Aneroid ft.	Observed temp Fahr.	Per centage of standard density	Observed time	Rate of climb in Aneroid ft.	Real rate of climb (corrected for temp.)	Standard height	% of standard density	Time	Rate of climb
0	36°	0.00							
1,000	37°	101.0	1.00	835	814	1,000	99.40	1.20	775
2,000	38°	97.2	2.10	735	718	2,000	96.30	2.56	685
3,000	36°	94.0	3.70	640	655	3,000	93.26	4.11	610
4,000	36°	90.7	5.40	560	544	4,000	90.25	5.85	545
5,000	36°	87.4	7.25	540	495	5,000	87.32	7.80	490
6,000	33°	84.7	9.40	450	435	6,000	84.50	9.96	435
7,000	30°	82.1	11.90	405	389	7,000	81.80	12.40	385
8,000	26°	79.9	14.25	365	347	8,000	79.16	15.14	345
9,000	22°	77.6	17.00	330	312	9,000	76.55	18.20	310
10,000	23°	74.7	20.25	310	294	10,000	74.00	21.61	280
11,000	21°	72.2	23.60	280	264	11,000	71.70	25.41	245
12,000	20°	69.8	27.40	230	216	12,000	69.50	29.81	210
13,000	17°	67.7	31.90	195	182	13,000	67.32	35.13	170
14,000	12°	65.9	37.90	150	139	14,000	65.17	41.33	130
15,000	8°	64.1	45.25	110	101	14,500	64.11	46.23	105

The corrections are often much greater than those necessary in the above case.

It will be noticed that the rate of climb of this machine is approximately halved for a difference in height of 5,000 feet. Now it is possible to get a difference in density near the ground of as much as 15 per cent between a hot day in summer and a cold day in winter. This corresponds to a difference in height of 5,000 feet, so that this machine would climb off the ground on a hot day at only half the rate that it would on a very cold day. Variation in atmospheric density, combined with the errors of an aneroid, fully account for the observed difference between a "good climbing day" and a "bad climbing day."

At least two climbing tests of every new machine are carried out up to 16,000 feet or over by aneroid. If time permits three or more tests are made. The final results given are the average of the tests and represent as closely as possible the performance on a standard day, with temperature effects, up and down currents, and other errors eliminated.

If we produce the rate of climb curve upwards it cuts the height axis at a point at which the rate of climb would be zero, and therefore the limit of climb reached. This is the "ceiling" of the machine.

SPEEDS.

His 16,000 feet, or whatever it is, reached, the flyer's next duty is to measure the speed flying level by air speed indicator at regular intervals of height (generally every 2,000 feet) from the highest point downwards. To do this he requires a sensitive instrument which will tell him when he is flying level. The aneroid is quite useless for this purpose, and a "statoscope" is used. The principle of this instrument is really the same as that of a climbmeter. It consists of a thermos flask connected to a small glass gauge, slightly curved, but placed about horizontally. In this gauge is a small drop of liquid, and at either end are two glass traps which prevent the liquid from escaping either into the outside air or into the thermos flask. As the machine ascends the atmospheric pressure becomes smaller, and the pressure in the flask being then higher than the external pressure, the liquid is pushed up to the right hand trap, where it breaks, allowing the air to escape. On descending the reverse happens; the liquid travels to the left, breaks, and air enters the flask. When flying truly level the drop remains stationary, moving neither up nor down. The instrument is made by the British Wright Co.

The flyer or the observer notes the maximum speed by the air speed indicator, i.e., the speed at full engine throttle. At one or more heights also, he observes the speeds at various positions of the throttle down to the minimum speed which will keep the machine flying at the height in question. The petrol consumption and the engine revolutions are noted at the same time, as well, of course, as the aneroid height and temperature. Accurate observation of speeds needs very careful flying—in fact much more so than in climbing tests. If the air is at an all bumpy observations are necessarily subject to much greater error, since the machine is always accelerating and decelerating. The best way to carry out the test seems to be as follows. The machine is flown first just down hill and then just up hill and the air speeds noted. This will give a small range between which the real level speed must lie. The flyer must then keep the speed as steadily as possible on a reading midway between these limits, and watch the statoscope with his other eye. If it shows steady movement, one way or the other, the air speed must be altered accordingly by 1 m.p.h. In this way it is always possible at heights where the air is steady to obtain the reading correct at any rate to 1 m.p.h., even with light machines, provided always sufficient patience is exercised. The r.p.m. at this speed are then noted.

One difficulty, however, can not be avoided. If at any height there is a steady up or down air current, then though the air may appear calm, i.e., there may be no "bumps," the air speed indicator reading may be wrong, since to keep the machine *level* in an up current it is necessary to fly slightly down hill relatively to the air. Such unavoidable errors are, however, eliminated to a large extent by the method of taking speeds every 2,000 feet, and finally averaging the results.

We must now consider how the true speed of the aeroplane is deduced from the reading of the air speed indicator. It is well known that an air speed indicator reads too low at great heights—for example, if it reads 70 m.p.h. at 8,000 feet the real speed of the machine through the air is nearer 80 m.p.h. The reason for this is that the indicator, like the aneroid, is only a pressure gauge—a sensitive pressure gauge, in fact, which registers the difference of pressure between the air in a tube with its open end pointing forward along the lines of flight of the machine, and the real pressure (the static pressure) of the external air. This difference of pressure is as nearly as we can judge by experiment = $\frac{1}{2} pV^2$ (where p is the density of the air and V the speed of the machine), provided that the open end of the tube is well clear of wings, struts, fuselage, etc., and so is not affected by eddies and other disturbances. Now assuming this law, air speed indicators are graduated to read correctly, as I have said above, at a density of 1.221 kgm. per cubic metre, which we have taken as our standard density and called "unity." It corresponds on an average to a height of about 800 feet above sea level.

Then suppose the real air speed of an aeroplane at a height of "h" feet is V m.p.h., and the indicated air speed is 70 m.p.h., this means that the excess pressure in the tube due to the speed is proportional to 1×70^2,

$$\text{or } \rho \times V^2 = 1 \times 70^2,$$

where ρ is the density at the height in question, expressed as a fraction of the standard density. To correct the observed speed, we therefore divide the reading by the square root of the density. Thus, observation of the maximum speed of an aeroplane at a height of 8,000 feet by the locked aneroid gave 80 m.p.h. on the indicator, the temperature being 31 degrees Fahr. From the curve we find that the density corresponding to 8,000 feet and 31 degrees is 0.85 of standard density. The corrected airspeed is therefore:—

[illegible]

This "corrected" air speed will only be true if the above law holds, that is to say, if there are no disturbances due to the pressure head being in close proximity to struts or wings. It is always necessary to find out the magnitude of this possible error, that is, to calibrate the air speed indicator, and the only way to do this is to measure a real air speed of the aeroplane at some reasonable altitude for easy observation by actual timed observations from the ground, and from these timed results check those deduced from the air speed indicator readings. This calibration is the most important and difficult test of all, since on the accuracy of the results depends the accuracy of all the other speed measurements. It can either be done by speed trials over a speed course close to the ground, or when the aeroplane is flying at a considerable height above the ground. In the Testing Squadron we have till lately attached more importance to the latter method, mainly because the conditions approximate more to the conditions of the ordinary air speed measurements at different heights, and because the weather conditions are much steadier and the flyer can devote more attention to flying the machine at a constant air speed than he can when very close to the ground.

One method is to use two camera obscuras, one of which points vertically upwards and the other is set up sloping towards the vertical camera. At one important testing centre the cameras are about ¾ mile apart, and the angle of the sloping camera is 45°. By this arrangement, if an aeroplane is directly over the vertical camera it will be seen in the field of the sloping camera if its height is anywhere between 1,000 and 16,000 feet, although at very great heights it would be too indistinct for measurements except on a very clear day. The height the tests are usually carried out is 4,000 feet to 6,000 feet.

The aeroplane is flown as nearly as possible directly over the vertical camera, and in a direction approximately at right angles to the line joining the two cameras. The pilot flies in as straight a line and at as constant an airspeed as he can. Observers in the two cameras dot in the position of the aeroplane every second. A line is drawn on the tables of each camera pointing directly towards the other camera, so that if the image of the aeroplane is seen to cross the lines in the one camera it crosses the line in the other simultaneously. From these observations it is possible to calculate the height of the aeroplane with considerable accuracy; the error can be brought down to less than 1 part in 1,000 with care. Knowing the height, we can then calculate the speed over the ground of the aeroplane by measuring the average distance on the paper passed over per second by the image in the vertical camera. If x inches is this distance, and f the focal length of the lens, the ground speed is $x \times h/f$ feet per second.

It is necessary to know also the speed and direction of the wind at the height of the test. For this purpose the pilot or his observer fires a smoke puff slightly upwards when over the cameras, and the observer in the vertical camera dots in its trail every second. The height of the smoke puff is assumed to be the same as that of the aeroplane—it probably does not differ from this enough to introduce any appreciable error in the results. The true speed through the air is then found.

[*Tizard then shows this graphically in the form of a simple ABC triangle in which length AB represents the ground speed of the aeroplane as measured in the camera, CB represents the velocity and direction of the wind, and the length AC represents the true air speed of the machine.*]

The tests are done in any direction relative to the wind, and generally at three air speeds, four runs being made at each air speed.

The advantages of this method are:—

(1) Being well above the earth the pilot can devote his whole attention to the test.

(2) Within reasonable limits any height can be chosen, so that it is generally possible to find a height at which the wind is steady.

(3) It does not matter if the pilot does not fly along a level path so long as he does so approximately. What is more important is that he should fly at a constant air speed.

(4) It is not necessary that there should be any communication between the two cameras, although it is convenient. The two tracks are made quite independently, and synchronised afterwards from the knowledge that the image must have passed over the centre line simultaneously in the two cameras.

The main disadvantage is that somewhat elaborate apparatus is necessary, but this is of not much importance in a permanent testing station.

There are often periods in war time, however, when an aeroplane has to be tested quickly, and low cloud layers and other causes prevent the camera test from being carried out. It is then necessary to rely on measurements of speeds near the ground for the calibration of the air speed indicator. In this method the aeroplane is flown about 50 feet off the ground, and is timed over a measured run. There are two observers, one at each end of the course; when the aeroplane passes the starting point the observer sends a signal and starts his stop-watch simultaneously; the second observer starts his stop-watch when he hears the signal, and in his turn sends a signal and stops his watch when the aeroplane passes the finishing point. By this double timing, errors due to the so-called "reaction time" of the observers are practically eliminated, for the observer at the end of the course tends to *start* his watch late, while the first observer *stops* his late. The mean of the two observations gives the real time. Four runs, two each up and down the course, are done at each air speed, the pilot or his observer noting carefully the average air speed during the run. Observations of the atmospheric pressure and temperature from which the density can be obtained are also taken. The average strength and direction of the wind during each trial are noted from a small direct reading (or recording) anemometer and the speed corrected in the same way as in the camera tests. If there is a strong cross wind the aeroplane may have to be pointed at a considerable angle to the course, and this makes the test a very difficult one to carry out well. Generally speaking, it is only reliable when the wind is quite light, not more, at any rate, than 10 m.p.h. Even this is too strong if it is a cross wind.

A further difficulty is that at high speeds, over 100 m.p.h., an aeroplane may take quite a considerable time to accelerate up to a steady speed, and so it must fly level for a long distance each end before reaching the actual course. At the testing station previously alluded to the course is a mile long, and there is a clear half mile or more at each end, but it is doubtful whether even this distance is enough for the machine to attain steady speed before the starting point. Finally, the flyer of a single-seater is generally too busy watching the ground to do more than glance at his air speed indicator more than a few times during the run. Doubtless it would be better in such a case to use some form of recording air speed instrument, although then other difficulties would arise.

Having gotten the true air speed from camera or speed course tests, and knowing the density at the height at which the test was carried out, we obtain what the air speed indicator should have read by multiplying the measured air speed by the square root of the density. By comparing this with the actual reading of the indicator we obtain the necessary correction. The whole procedure may be shown best by a table giving part of the results of a camera test made at the beginning of the year.

A summary of the complete speed tests may now be given. Firstly, the air speed and engine revolutions are noted flying level at full throttle every 2,000 feet approximately, by aneroid. From the aneroid reading and the temperature observations at each height the density is obtained. The reading of the air speed indicator is then first corrected for instrumental errors by adding or subtracting the correction found by calibration tests over the cameras or speed course. This number is then again corrected for height by dividing by the square root of the density. The result should give the true air speed, subject, of course, to errors of observation. The numbers so obtained are plotted against the "standard" heights, i.e., the average height in feet corresponding to the density during the test. A smooth curve is then drawn through the points and the air speeds at standard heights of 3,000, 6,500, 10,000, 13,000, and 16,500 read off the curve. These heights are chosen because they correspond closely with 1, 2, 3, etc., kilometres. The indicated engine revolutions are also plotted against the standard heights, because these observations form a check on the reliability of the results; also the ratio of speed to engine revolutions at different heights may give valuable information with regard to the propeller.

[*At this point Tizard includes a set of tables that present data from tests of air speed at height, showing the need for the outlined adjustments in order to obtain reliable and accurate results. Tizard also presents another set of figures showing curves drawn from the calculated data. In it the air speeds lie very closely on a smooth curve except at one point—about 10,000 feet—where the author believes they were probably affected by a downward current of air.*]

In a brief paper it is impossible to do more than explain the more important of the "performance" tests of aeroplanes, considered solely as flying machines. For military purposes a number of tests are necessary, some of which can not easily be reduced to figures. Nor can it be supposed for an instant that the methods outlined here are final; aeroplane testing, like all other work connected with aeroplanes, is only in its infancy; and as time goes on, and knowledge accumulates, better methods and instruments will evolve. There are some who lay considerable emphasis on the necessity of every test instrument being self-recording, and although this scheme appears at first sight Utopian and would relieve the pilot of a single-seater of considerable trouble, there are many objections to it when considered in detail, not the least of which is the difficulty of getting new and elaborate instruments made at a time when all manufacturers are fully engaged on other important work. When an observer can be taken I would personally place much more reliance on direct observations at the present time, and one great advantage of direct observation is that the results are there, and no time is lost through the failure of a recording instrument to record, a circumstance which is not unknown in practice. So far as we use recording instruments, we use them only as a check on direct observations, although we may probably adopt recording air speed indicators for the calibration tests of single seaters. But whether recording or direct reading instruments are used, it is as I said before, the flyer on whom the accuracy of the tests depends. I feel that too great stress can not be laid on this; he is the man who does most of the experiments, and like all experimenters in every branch of science, he requires training and a great deal of practice. Although the methods themselves may be greatly changed, this much may perhaps be claimed, that the general principles on which they are founded are sound, and will only be altered in detail. The importance of the work can hardly be exaggerated; model experiments are notoriously subject to scale and other corrections, which if not carefully scrutinised may be very misleading, and it is only by accurate full-scale work in addition that we can hope to maintain a steady improvement in the efficiency of aeroplanes.

[*The published paper includes the following transcript of the discussion after its presentation by Captain Tizard.*]

FIFTH MEETING, 52nd SESSION.

An ordinary general meeting of the Society was held in the Theatre of Royal Society of Arts, London, on Wednesday, March 7th, 1917, at 8:00 p.m. There was a large attendance of members and guests. The chair was to be the Right Hon. Lord Sydenham, G.C.I.E., F.R.S.

Captain H. J. Tizard, of the R.F.C., Associate Fellow, read a paper, illustrated by slides, on “Methods of Measuring Aircraft Performances.”

On the conclusion of the lecture a discussion followed.

Squadron-Commander Busteed: I regret that the Naval Testing Department is not as far advanced as one would like it to be. A good deal of useful work has been done, but the R.N.A.S. and R.F.C. Department had adopted different density standards, though these were now the same.

I appreciate the necessity for instruments, but my experience goes to show that the machine instruments were most required for single-seaters, and unfortunately, after the pilot had managed to get in, there was very little room for them. They were also a source of trouble in getting tests through quickly; readings taken by pilots had proved very fair.

Lieutenant G. H. Millar, R.N.V.R., said that in his opinion it was a pity that the standard atmosphere which had been adopted was a purely empirical one; he would have preferred one based on a given temperature and pressure at sea-level and a uniform rate of fall of temperature. Some months previously he had calculated such a standard atmosphere, taking as the condition at sea-level a pressure of 760mm. and a temperature of 15 deg. C., with a fall of 1.5 deg. C. per 1,000 ft. rise, and the curve of density against height thus obtained did not differ greatly from that given in this paper, the difference varying between 400 and 700 ft. for height from 0 to 20,000 ft. By reducing the assumed ground temperature the curves could be brought nearly into coincidence. The advantage of such a standard over the empirical one was that it could be calculated at any time by remembering two constants. He also thought that the unit of density should cetainly be the density at zero height for the standard atmosphere adopted. No advantage was gained by using the density for which the speed indicators were initially calibrated, since the instrument had to be calibrated in the machine in any case, and in practice instruments were found to be anything up to 20 per cent out. With regard to calibration in the machine, calibration at height had the disadvantage that the speed range of the machine was reduced, unless the machine was flown slightly downhill for the higher speeds. He had found that it was best to take four pairs of runs at different speeds over as wide a range of speed as possible, and even with quite rough methods of timing, the four spots usually came very nearly on a straight line. He was inclined to doubt Captain Tizard's statement that the best climbing speed was the same at all heights, although probably little was sacrificed by climbing throughout at the "ceiling" speed. He stated that terms were badly needed for the quantities $^{v}/_{\sqrt{\rho}}$ and $^{n}/_{\sqrt{\rho}}$ where "v" was the speed, "n" the r.p.m., and "ρ" the density. These quantities were of great importance in considering the aerodynamic properties of the machine and propeller (apart from the engine) and the relations between them, and the angle of incidence and angle of ascent or descent were independent of the height or density. He wished to express his admiration for the thoroughness with which the R.F.C. tests were carried out.

Captain Grinsted: The principle of the methods of testing of aeroplanes and of the reduction of the results to a standard basis as now established and improvements in accuracy of testing can now be made only by improving the instruments by which measurements are made.

Captain Tizard objected to the use of an aneroid as a height-measuring instrument, and preferred to use it simply as a pressure-measuring instrument.

Even as such the aneroid is not perfect, and in saying that it measures pressure it is given too good a character. Owing to its lag it does not give a correct measurement of pressure when the rate of change of pressure is at all rapid. I should like to know if Captain Tizard has found difficulty in obtaining instruments sufficiently free from lag for the purpose of accurate aeroplane testing.

The measurement of performance is now confined to tests of speed and climb. There are other things of importance, such as the rate at which the aeroplane can be brought on to a given bank or its direction of flight turned through a given angle which should be measured when comparing performances of aeroplanes. I should like to know if Captain Tizard has considered methods of making such measurements.

Mr. Bertram Cooper: I should like to ask Captain Tizard if he could tell us something about the lag of the climbmeter. We have had several "lags" mentioned tonight, but not this "lag," which seems to me to be a pretty serious one.

The action of the instrument depends on the accumulation of pressure in the bottle, which is relieved by the leak. It will be clear, therefore, that the reading will always lag behind the real state of affairs at the spot where it is made, the exact amount depending on how fast the upward or downward journey to that spot was made. For instance, a pilot could stall his machine when seeking his best "climb" and the instrument would still tell him he was climbing when he was, in fact, falling owing to stalling. Moreover, the error here would be aggravated by the "gravity error" on the liquid. This liquid would be relatively lighter owing to the falling, and would consequently tend to show a rate of climb in excess of that actually appropriate to the pressure difference that existed. And this leads me to ask Captain Tizard what is the most serious error in practice, the "lag" error or the "gravity" error? I notice he said that the instrument was not satisfactory near the ground. I take it that is chiefly because of the gravity error and "bumpy" flying, but is not the "lag" error serious at all heights?

Captain Farren: The methods of measuring aeroplane performance described by Captain Tizard are, as he said, only different in some minor points from those in use at the R.A.F., where they serve both for the testing of new types and for reducing the full-scale experiments on aeroplane resistance, etc., which have been going on there for some time. The methods were, in fact, arrived at to a great extent by discussion between the R.A.F. and the Testing Squadron. The same standards of density are used, and we agree with him generally on the superiority of ordinary instruments and good observers over automatic recording instruments. We have not had so much experience as he has had with single-seaters, which are rather a different problem from two-seaters, demanding much more skill from the pilot, but it seems that even here automatic recorders have disadvantages.

With regard to measuring speeds at heights, Captain Tizard is of the opinion that it is not possible to fly level except by using a statoscope. My experience is that in the case of certain very expert pilots a flight taking as long as ten minutes can sometimes be made, during which the aneroid shows no appreciable movement. (The aneroids used are very high-class instruments, with 20 ft. divisions.) I realise that this does not really mean that no height is gained or lost in the test, because every aneroid is known to possess lag. But under these circumstances on the average the height difference between the beginning and end of the run can not be more than about 100 ft. in a length of about 12 miles—corresponding to a slope of 1 in 600 or so, which represents a correction to the speed of well under the error of observation. But undoubtedly the statoscope gives generally much better results. The instrument in use at the R.A.F. is similar in principle to the one shown, but very much smaller, occupying a space 1 in. by 1 in by 6 ins, approximately. This gives very satisfactory results in use.

With regard to the rate-of-climbmeter—which, it may be interesting to know, was christened the "coffeeometer" by the pilots at the R.A.F., on account of the thermos flask used on the first instrument!—this was first shown me by Captain Dobson (then of the Testing Squadron) in July, 1916. A search in the Instrument Stores at the R.A.F. brought to light an exactly similar instrument—made in Germany! It is apparently a standard balloon instrument, but the credit of introducing it into aeroplane testing is due to the Testing Squadron. This instrument again has been much reduced in size, and occupies about the same space as the statoscope, referred to above. In use it suffers from one disadvantage—any vertical acceleration, such as that which occurs as the result of a change in speed, causes the indicating column to move on account of the change in effective gravity. As a result only very gradual changes in speed must be made in searching for the best climbing speed. An attempt has been made to develop a dial indicator (in which the defect would not appear), but without success, on account of the large volume of air enclosed in the diaphragm.

Captain Tizard laid stress on the necessity for very careful work on the part of the pilot. I think too much emphasis can not be put on this point. We are now emerging from the middle age of aeronautics—when flying was, to the ordinary man, a kind of magic, practised by a sort of superman who daily carried his life in his hands, but nevertheless continued to survive in spite of the apparently rash things he habitually did. To some extent the fear of the passing generation of flyers that flying would become cheapened and commonplace helped to keep alive this idea. They saw their living vanishing. It must be admitted that their fears were justified. It is difficult to estimate aright the value of their work. We are too near to see it in its true place. I think we can be sure that history will not be unjust to them or stinting in its acknowledgments. But it is evident that nowadays it is becoming easier and easier to fly—also less risky. The "magic" has gone. In its place we find a new branch of engineering—a new science. For accurate and useful work nowadays skill and nerve are still essential, but to these care, thoroughness, and training in making accurate observations must be added. Everyone who has had anything to do with aeroplane testing knows how it was common talk that A. always got a better climb out of a machine than anyone else. Perhaps he did it by willpower or some other occult practice; anyhow, it was beyond us. Naturally A. gained in many ways, and it can not be reckoned against him that he did not make any special efforts to dispel the idea. Pilots are human. But the real truth is that A. was possessed of a power of accurate and thorough workmanship, which always, in any kind of work, brings the best results.

Aeroplane testing, as a part of aeroplane designing, demands for satisfactory results the highest training. It occupies no special place by virtue of this—it merely comes into line with the rest of engineering. Now, one can learn to fly in a month—even in England in war time—but an engineer's training requires years. It is evidently necessary, therefore, that engineers—men with scientific training and trained to observe accurately, to criticise fairly, to think logically—should become pilots, in order that the development of aeroplanes may proceed at the rate at which it must proceed if we are to hold that place in the air to which we lay claim—the highest.

I wish to add the following remarks:—

In the years immediately preceding the war aeronautics suffered very much from a lack of full-scale experiments. Money was but grudgingly given, and the foresight of the Government in this matter was not conspicuous. As a nation we are remarkable for our inertia. After the outbreak of war for some time little improvement was evident, but gradually the state of affairs became better. At the present moment we are in a fairly good position, but it is necessary to make provision for "after the war." At the moment experiments are not killed—as they used to be—for lack of money. But after the war the inevitable reaction will almost certainly mean that a partial slump will occur. Money will be scarce and aeronautics will suffer in company with other activities. It is here that the trade must help. They must realise that if they are to build up aeronautics as a branch of engineering they must be prepared to experiment thoroughly. They must provide money and manufacturing facilities for testing and for full-scale experiments. Men will not be lacking. In no branch of engineering have we ever had to want for men—aeronautics has special attractions which will ensure a steady supply of the best. But only if the prospects are sufficiently attractive. A stinting policy here will only result in other countries beating us. It has been our unhappy experience in the past in more than one science to see our brains and our energies wasted owing to lack of encouragement from those who could and should have given it. We have seen other countries gifted with more foresight take our ideas—and our men—and forge ahead. Eventually we have generally managed to regain some of our losses.

But in the keener struggle which is to come in every trade we must not go back to the old tactics, or we shall not find Fate so kind to us. It is to be hoped that Captain Tizard's lecture will cause aeroplane manufacturers to see that if they are prepared to treat their productions as other engineers do, to provide for testing and experiment on a liberal scale as is done in every other kind of profession, then they will reap their reward.

Captain TIZARD replied.

Lord SYDENHAM expressed on behalf of those present their indebtedness to Captain Tizard for his interesting and valuable paper.

A vote of thanks was then offered to Lord Sydenham for presiding, and the meeting terminated.

Document 2-7(a–b)

(a) Jerome C. Hunsaker, Assistant Naval Constructor, letter to H. M. Williams, Managing Editor, Aviation and Aeronautical Engineering, 11 March 1918, Hunsaker Collection, Box 1, Folder 1, File A.

(b) "Education in Advanced Aeronautical Engineering," *NACA Annual Report for 1920* (Washington, DC), p. 20.

The following two documents illustrate the nascent state of American aerodynamic education at the end of the second decade of the twentieth century. In the spring of 1918, Jerome Hunsaker was busy overseeing the design of airplanes, airships, catapults, and aircraft engines for the U.S. Navy. At that time he wrote to H. M. Williams, the managing editor of *Aviation* magazine, to praise their decision to publish a much-needed textbook on aerodynamics and aeronautical engineering. Two years later in its sixth annual report, the NACA published a resolution adopted in April 1920 calling for both the military services and American universities to establish courses in "advanced aeronautical engineering."

Document 2-7(a), letter from Jerome C. Hunsaker to H. M. Williams, 1918.

March 11, 1918.

Dear Mr. Williams:

I am very glad indeed to learn that you are to bring out in book form the "Course in Aerodynamics and Aeronautical Engineering" by Klemin and Huff, as published serially in your paper.

I know there is a real need for a thorough treatment of the subject such as this course presents. A large part of the work is fundamental and hence will not quickly pass out of date as is unfortunately the case with a great deal of technical literature.

Let me add to my good wishes for the success of the book, my suggestion that you expedite the printing.

Very truly yours,
Jerome C. Hunsaker
Asst. Naval Constructor, U.S.N.

Document 2-7(b), "Education in Advanced Engineering," NACA Annual Report for 1920.

At the semiannual meeting of the full committee in April, 1920, consideration was given to the question of education in advanced aeronautical engineering. This meeting was attended by all the members of the committee connected with universities: Drs. Ames, Durand, Hayford, and Pupin, and it is deemed worthy of special notice that each of these members individually expressed his approval of the resolution which was adopted at that meeting in the following terms:

Whereas it is deemed essential to the development of aviation in America for military and naval purposes that advanced instruction in aeronautical engineering be given to military and naval officers at a competent educational Institution; and

Whereas the public demand for such instruction will in all probability not be sufficient to justify or permit the offering of such advanced courses in more than one institution at the present time; and

Whereas such an advanced course is now being given at the Massachusetts Institute of Technology; and

Whereas it is deemed further essential that actual experience with aerodynamic research should form a part of such advanced Instruction: Therefore be it

Resolved, That the National Advisory Committee for Aeronautics hereby recommends to the Secretary of War and to the Secretary of the Navy the adoption of a continuing policy for the instruction of officers in advanced aeronautical engineering, and that for the next three years classes of 15 Army officers and 15 Navy officers be detailed annually to take such instruction in advanced aeronautical engineering at the Massachusetts Institute of Technology at the expense of the War and Navy Departments, respectively.

Resolved further, That, in connection with the course in advanced aeronautical engineering, the National Advisory Committee for Aeronautics cooperate in every way with the Massachusetts Institute of Technology by offering to its faculty and students the facilities for investigations in aerodynamics and experimental work on actual airplanes at the committee's research laboratory, Langley Field, Va.

Resolved further, That the National Advisory Committee for Aeronautics offer to give at various engineering universities courses of lectures in advanced aeronautical engineering by members of its engineering staff.

Resolved further, That the National Advisory Committee for Aeronautics recommend that educational institutions generally not consider the establishment of courses in aeronautical engineering at the present time, as it is the opinion of the committee that the demand for such instruction outside of the Government service is not sufficient, and competent instructors for such courses are not available.

This resolution was transmitted to the Secretary of War and to the Secretary of the Navy. The War Department, acting on the committee's recommendation, secured the necessary authority from Congress to detail 25 officers for special instruction at the Massachusetts Institute of Technology. It is understood that the Navy has not secured similar authority. The committee therefore strongly recommends to Congress that similar authority be given for the detail of naval officers for such special training. At the present time both services are weak in respect to the number of officers sufficiently educated in aeronautical engineering. The committee considers that the diligent prosecution of a continuing program of education will be of great value within a few years in the development of military and naval aviation.

Document 2-8(a–b)

(a) United States Army, excerpts from "Full Flight Performance Testing," *Bulletin of the Airplane Engineering Department, U.S.A.* 1, No. 2 (July 1918): 20–49.

(b) "Special Aerodynamic Investigations," *NACA Annual Report for 1919* (Washington, DC), pp. 27–28.

Early in World War I, military officials saw that the NACA's Langley Laboratory would not be ready in time to meet its research needs. Accordingly, the U.S. Army built a temporary facility at McCook Field, in Dayton, Ohio, and moved its Airplane Engineering Department personnel from Langley. Because the army was interested in practical research to quickly identify and solve problems that directly affected aircraft production, performance, and reliability, much of McCook's work involved flight testing. Recognizing that "the possibilities of error in full flight testing are very great," one of McCook's first investigations examined methods to standardize test methods and ascertain the accuracy of instruments such as altimeters and airspeed indicators.

"Full Flight Performance Testing" was one of McCook's earliest publications, and it shows the army's practical approach to flight research. The following excerpts include the report's introduction and the second section that describes the basic types of instruments recommended by the McCook Field engineers for recording aerodynamic data from flight tests.

Noting the success of McCook's approach, as well as the slow progress of construction at Langley, the NACA sought to work with the army at McCook. The committee, which included the commanding officer at McCook Field, Colonel Thurman H. Bane, approved a broad program of "scientific work" at McCook. Outlined under "Special Aerodynamic Investigations" in the *NACA Annual Report for 1919*, this work marked the beginning of a fruitful cooperation between the Army and the NACA.

Document 2-8(a), United States Army, excerpts from "Full Flight Performance Testing," 1918.

AERONAUTICAL RESEARCH DEPARTMENT REPORT

The possibilities of error in full flight testing are very great, both as regards the use of instruments, the methods of observation, and the corrections applicable for varying atmospheric conditions. This article has been written as a definite summary of the subject to facilitate standardization of the methods of testing and recording of results. It deals solely with standard performance tests and climb and speeds at varying altitude, with no consideration of stability, controllability or radiator and engine performance.

In Section I is included a review of such physical data as is required for a complete understanding of the subject. The points considered in this section are: formulae for density of air; density in grammes per cubic meter and pounds per cubic foot; reduction to per centage of standard density; standard atmosphere; density values at various relationship between pressure, temperature and altitude at constant temperature; Halley's formula for temperature, and corrections; Bureau of Standards altitude pressure curve; calibration chart for altimeter, and an alignment chart for altitude.

In Section 2 are described the commonest and most useful forms of instruments employed in performance testing, with their principles and calibration. These include air speed indicators whose utility is obvious; barographs and altimeters for measuring pressures and allowing altitudes to be deduced therefrom; strut thermometers so that necessary temperature corrections may be made; tachometers whereby the r.p.m. of the motor may be obtained; statoscopes to enable the pilot to fly level at altitudes; recording drums used on various types of measuring instruments, and anemometers and wind vanes.

Finally, in Section 3 are described the methods employed in the calibration of the air-speed meter, in measuring climb and speed at altitude, and the various methods of correcting and recording results.

In the Appendix a standard form for recording results is submitted. This form has been adopted for use at McCook Field.

Simplicity of presentation rather than an exhaustive, scientific treatment has been sought.

[. . .]

Section 2

AIR SPEED INDICATORS

Several types of air speed indicators, based on a number of principles, have been employed in the past, such as pressure instruments with a plate balanced by a spring, rotating vane anemometers, and hot wire anemometers. For a number of practical and theoretical reasons, instruments of this type have been discarded, and attention is now concentrated on instruments measuring differences of pressure transmitted from two Pitot tubes or Pitot and Venturi tubes.

Pitot and Venturi Tubes

Since all air speed indicators based on pressure differences are of the Pitot, or Pitot and Venturi type, a simple explanation of the principles involved will be included.

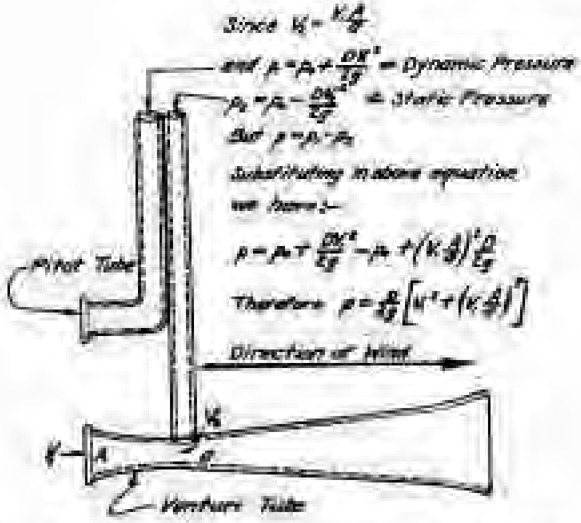


Fig. 9—Combination of Pitot and Venturi tubes

(a) *Pitot tube*.—In Fig. 10 is shown a Pitot tube in diagrammatic form. It consists of two concentric tubes, the inner open to the wind, the outer closed and communicating with the current of air only by a series of fine holes. The tubes are connected to the two arms of a pressure gauge, which measures the difference in pressure between them. The inner tube, open to the wind, brings the air impinging on it to rest, and the pressure on it is, therefore, a measure of both the static pressure in stream and of the kinetic energy head of the stream. If p is the static pressure of the stream, V the velocity, the total pressure on the inner tube will be given by

$$p+\frac{DV^2}{2g}$$

The outer tube, on the other hand, being closed to the wind, will, if the holes are small enough, read the static pressure of the air flow p.
Hence the differences in pressures read on gauge will be

$$\frac{DV^2}{2g}$$

and the gauge reading will be a measure of the velocity.
Pitot tubes with suitable gauges are widely used in laboratory practice, but owing to the small difference in head $^{DV^2}/_{2g}$, the forces acting on the gauge are very small and hard to record.

(b) *Combination of Pitot and Venturi.*—To increase the pressure differences, and thus get practicable forces on the gauges, the Venturi tube is coupled with the pressure part of the Pitot. Such a combination is shown diagrammatically in Fig. 9. Here the velocity at the throat will be considerably greater than that acting on the suction side of a Pitot, and therefore has a considerably greater effect. The mathematical theory of the Venturi is a little more complicated than that of the Pitot and the theoretical suction heads are not always in accord with practical results. From the simple formula of Fig. 9 it can be seen that the gauge readings will be proportional to $^{DV^2}\!/_{2g}$, hence are a measure of the velocity.

Correction for density and reduction to standard density in air speed meters.—From the preceding considerations of the Venturi and Pitot tubes, it is seen that the forces on the gauge are proportional to A D V², where D is the density, V the velocity and A some constant depending on the instrument. The airspeed reading equation is therefore

$$R = ADV^2$$

Airspeed meters being calibrated at 16 deg. C. and 760 mm., they will only be correct at the standard density D corresponding to this condition.

If the instrument gives a certain reading V_1 at density D_1 then true reading V_t

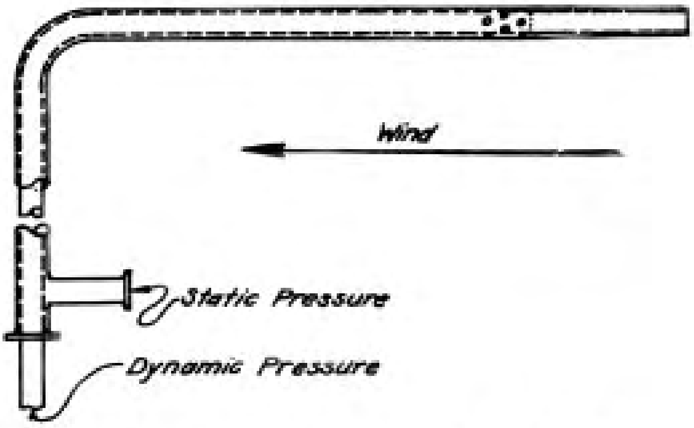


Fig. 10—Standard form of Pitot tube

will be given by equation: $AD_1V_t^2 = AD_0V_1^2$

and $V_t = V_1\sqrt{\frac{D_0}{D_1}}$

Since $\frac{D_0}{D_1} = \frac{273 + t}{273 + 16} \times \frac{760}{P}$

[illegible]

The correction can be applied either by computation or from the curves of Figs. 2 and 3 where densities for varying temperatures and pressures are given as per centages of the standard density, as well as the values of the ratios ÷D0/D1. On certain occasions it may be quicker to use table 3.

Approximate air speed correction at heights.—A very useful table furnished by the Technical Department, British Aircraft Production, allows air speed corrections at heights to be made with fair accuracy, on the assumption of certain standard conditions. Its use is not recommended for the computation of performance results, but may be very handy as a check. The table employs mean value of the density at various altimeter heights. Ground temperature of 16 deg. C. and pressure of 760 mm. are assumed, and a lapse rate of 1.75 deg. C. per 1000 ft. ascent.

Table 3

Multiplying Factors to Reduce Speed Readings at Varying Pressure and Temperatures to Standard Density Pressure in Millimeters of Mercury

Temperature, degrees centigrade	200	250	300	350	400	450	500	550
- 40∞	1.750	1.566	1.429	1.324	1.236	1.168	1.109	1.057
-30	1.790	1.599	1.459	1.351	1.265	1.191	1.130	1.080
-20	1.830	1.631	1.492	1.380	1.290	1.218	1.155	1.100
-10	1.852	1.661	1.521	1.407	1.318	1.239	1.175	1.121
0	1.899	1.673	1.550	1.430	1.340	1.264	1.120	1.142
10	1.924	1.729	1.574	1.459	1.365	1.286	1.220	1.162
20	1.961	1.757	1.602	1.483	1.388	1.308	1.241	1.182
30	1.990	1.787	1.630	1.508	1.410	1.330	1.261	1.204
40		1.815	1.661	1.533	1.437	1.354	1.281	1.224
Temperature, degrees centigrade	600	650	700	750	800	850	900	950
- 40∞	1.010	.944	.936	.904				
-30	1.031	.982	.956	.925	.894			
-20	1.051	1.012	.975	.942	.911			
-10	1.075	1.031	.995	.960	.930	.902		
0	1.094	1.051	1.012	.978	.949	.920	.893	
10	1.115	1.070	1.030	.996	.965	.937	.910	
20	1.132	1.089	1.050	1.012	.982	.954	.926	
30	1.151	1.098	1.068	1.030	.997	.979	.941	
40	1.170	1.120	1.078	1.050	1.013	.984	.958	

Typical Recording Air Speed Meter

In the Toussaint-Lepère air speed meter the dynamic pressure of the wind is measured by a combination of Pitot tube and Venturi meter. This pressure is transmitted to a clock work recording device by a gauge consisting of bellows and a tension spring.

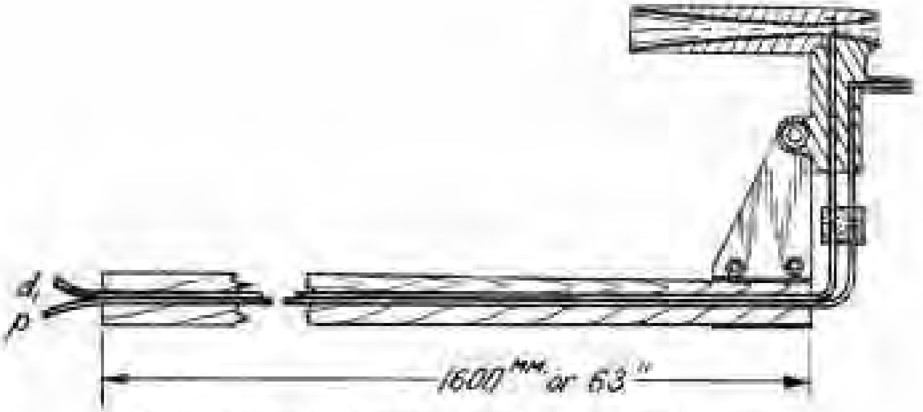


Fig. 11—Antenna and support arm

The Pitot tube and Venturi meter are combined in a small casting conveniently called by the French *antenna* and similar to that of many other speed indicators. This antenna is shown in Fig. 11. The Venturi is carefully proportioned to give the maximum possible suction with a given air speed. The antenna is supported by a long, slender, hollow arm of light wood which contains the tubes transmitting the pressure to the recording device as shown in Fig. 12. It is fastened to this arm by a light, adjustable clip, in order that the antenna may be turned directly into the wind.

The recording device is shown in Fig. 14 and diagrammatically in Fig. 13. It has the ordinary clockwork drum and pen. These are described elsewhere. The gauge consists of two movable circular plates S1 and S2, rigidly connected by a rod ab. The plates form the tops of the bellows f1 and f2. The sides of these bellows are made of thin rubber that is very flexible, the bottoms are formed by the fixed plates m and n. The suction from the Venturi is led to the airtight chamber c-c, and so acts on top of the plate S1. The pressure from the Pitot is led to the under side of the plate S2. The top of S2 and the bottom of S1 are open to the air inside of the box. Thus a variation of that pressure causes no motion of the rod ab which is moved only by the difference of the pressure transmitted from the antenna. The rod ab is constrained to move vertically by the form bar linkage a-d-c-b. The link bc carries on one end the marking pen g; on the other a counter weight for the movable parts of the instrument. At the end of this link is fastened the spring R, whose tension balances the pressure of the pen. This spring is so placed that the displacement of the pen is nearly proportional to the wind speed. The recording apparatus is enclosed in a box about 9 in. x 6 in. x 5 in., total weight about 4 ¼ lbs. The apparatus slides out of this box to facilitate adjustment of paper on the drum.

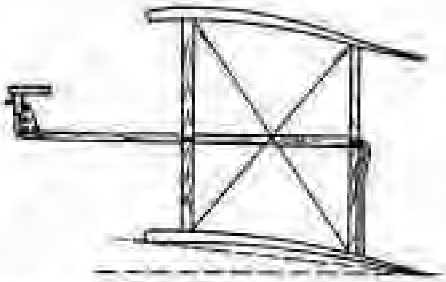


Fig. 12—Installation of Toussaint-Lepère air speed meter

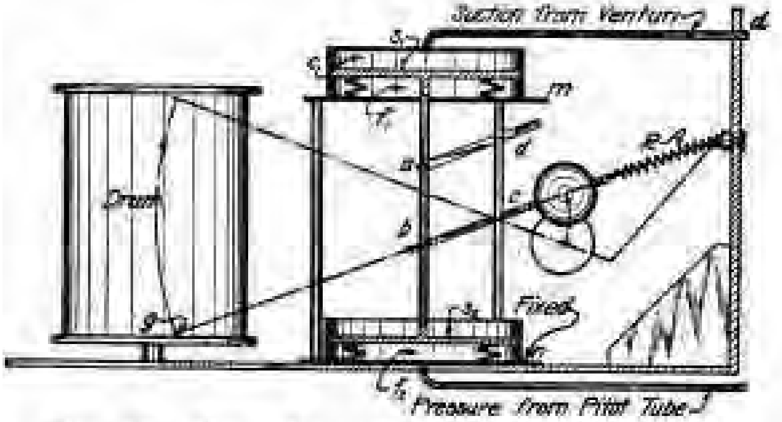


Fig. 13—Recording apparatus of Toussaint-Lepère air speed meter

The complete speed indicator must be calibrated and a chart or table made for converting the readings on this drum into true wind speeds. This chart of course is only correct for readings in air of standard density.

The recording apparatus is suspended in the air by elastic cords or may be held by the passenger in a two-seater. The antenna must not be placed near any obstructions or disturbance including the slip-stream, body, etc. The supporting arm is fastened to any convenient part of the airplane such as a strut by tape or a fitting (see Fig. 21). The antenna is then adjusted to point directly into the wind. With this instrument as with all air speed meters, a test run in flight must be made over a measured course to determine the effect of interference of the plane upon the air flow to the antenna, and to find the correction due to this interference.

The Foxboro-Zahm Direct Reading Air Speed Meter

In Figs. 15 and 16 are shown views of a very widely used combination of the Foxboro indicating box and the Zahm Pitot-Venturi tube (now adopted as standard by the Signal Corps). The pressure lead of the Pitot enters the small cylinders located in the indicating case which in itself is made air-tight by a gasket under the cover. The, suction of the Venturi is transmitted to the case itself. When a difference of pressure exists between the inside and outside of the two cylinders, they elongate or contract. The motion is transmitted to the pointer by means of links to a circular rack which engages a pinion on the spindle.

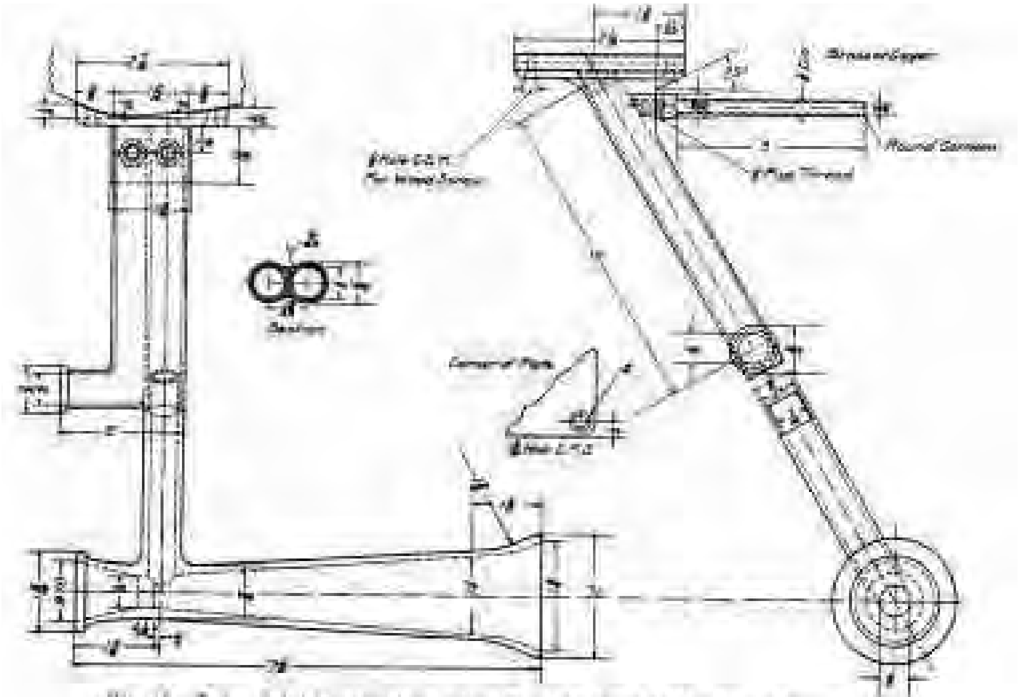


Fig. 16—Zahm Pitot-Venturi tube adopted as standard by the Signal Corps

It may be useful to include also the old type Foxboro head which is widely used. This is shown in Fig. 17. The tube in front presents a large opening to the wind. In this opening is fitted a conical guard pointing into the wind, behind which is located the opening to a small pipe. This pipe is the only outlet from the cup-shaped opening, and it transmits the dynamic pressure to the gauge. The wind passing the tube creates a suction in the space inclosed by the frustrum of the cone. A small pipe, seen in the photograph at the base of the cone to the left, transmits this suction to the gauge.

In airspeed meters, since the movement of the aneroid boxes is proportional to the square of velocity, the scale on the dial is not uniformly graduated; and were it not for a compensating device, the divisions of the scale for the higher velocities would increase rapidly as the velocity increased. The small springs fastened to the aneroid boxes shown in Fig. 15 restrict the movement of the boxes and shorten the scale divisions.

Table 4
Air Speed Corrections at Heights

Apparent speed instrument reading m.p.h.	Corrected Speeds at Heights (m.p.h.)			
	6500 ft.	10000 ft.	15000 ft.	20000 ft.
40	44	46	50	54
45	49	52	56	60
50	65	58	62	67
55	60	63	68	74
60	66	69	75	80
65	71	75	81	87
70	76	81	87	94
75	82	86	93	100
80	87	92	99	107
85	93	98	106	114
90	98	104	112	120
95	104	109	118	127
100	109	115	124	131
105	115	121	130	140
110	120	127	137	147
115	120	132	143	154
120	131	138	140	161
125	137	144	155	[170]
130	142	150	161	171
135	147	156	168	171
140	153	161	174	187
145	158	167	180	194
150	161	173	186	201
155	169	179	193	207

Miscellaneous Air Speed Meters

The British R.A.F. IV-A air speed indicator head is of the Pitot type modified in construction. The dynamic pressure tube and the static pressure tube are entirely separate, held parallel about 2 in. apart by a small fitting. The dynamic tube is just a plain tube open to the wind, but the static pressure tube is closed by a small streamlined cap. The holes are drilled well back along the cylindrical part of the tube. The leads are separate and the head is very easy to make. The gauge, shown diagrammatically in Fig. 18, is typical of most of those used for this type of air speed indicator. The whole gauge is made airtight by rubber gaskets. Inside there are two diaphragms A and B made of thin flexible metal. The top of B and the bottom of A are fixed to the case. The movable ends push through small rods to the cross-arm C on the spindle D. At the end of this spindle is an arm E which engages a quadrant suitably geared to the pointer. The motion of the pointer is opposed by a light hairspring. The dynamic pressure is led to the inside of the diaphragms, the static merely inside of the case. The diaphragms therefore tend to expand and so the gauge is sensibly independent of gravity and centrifugal force, and entirely free of the pressure in the cockpit. Other makers of gauges have different managements of diaphragms and different mechanism, but the principle is the same.

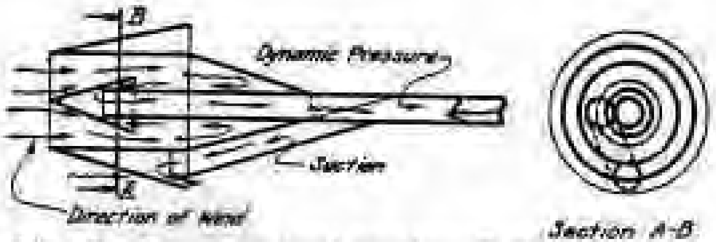


Fig. 17—Longitudinal section of Foxboro head

Another British instrument, the Ogilvie indicator-head is merely a Pitot tube. The gauge is different from the usual type. It has a single airtight chamber divided into two parts by a flexible rubber diaphragm. The static pressure is on one side, the dynamic on the other. A light silk thread is attached to the center of this diaphragm. The thread is kept taut at all times by a very light hairspring. The whole mechanism is very delicate, almost too fragile for rugged work. Later types of Ogilvie indicators have a gauge made similar to the R.A.F. IV.-A.

Badin Double Venturi Head

The Badin type of head is a double Venturi meter as in Fig. 19. The small inner meter has its exit at the throat of the outer meter. This greatly increases the suction at a given wind speed; a very desirable quality, especially on slow speed machines or dirigibles. The Badin system appears to have only the suction lead from the head to the gauge. This is not good practice as the total pressure in the cockpit may be quite different from the static pressure at the head.

The Sperry Venturi speed meter is also of the double Venturi type. There are three leads for pressure difference. One, the suction, from the throat of the small Venturi, one from an open tube pointing back along the outside of the outer Venturi, and the third from the front edge of the case of the outer Venturi. Readings may be taken between the first and either of the others. There seems to be a distinct disadvantage in using the tube pointing back for this gives a reading less than the static pressure and so reduces the available pressure difference. For use on high speed airplanes there is a Sperry head of the Pitot tube type.

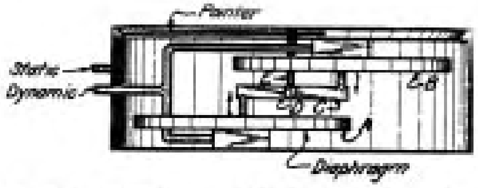


Fig. 18—Section of British R. A. F. Mark IV. air speed indicator gauge

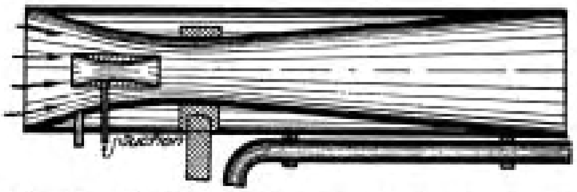


Fig. 19—Badin double throat Venturi meter

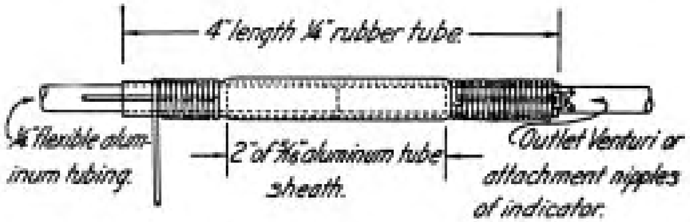


Fig. 20—Showing method of connecting aluminum tubing to Venturi meter and air speed meter

Important Practical Points

Attachment of Pitot tube heads.—A simple attachment as shown in Fig. 21 is customary, but this is open to the objection that the air stream is interfered with by the strut. It is much better to offset the instrument from the strut.

Connecting up Venturi-Pitot tube with airspeed indicating instrument.—The Venturi-Pitot tube is connected with the airspeed indicator by flexible aluminum tubing. In using the aluminum tubing sharp bends and kinks must be avoided. It is absolutely necessary that the connections at all joints be airtight. For this reason the following method is recommended for making connections between the aluminum tubing and the outlets of the Venturi, or the nipples of the indicators, or between sections of the tubing. (See Fig. 20.)

(1) Slip a 4 in. length of standard rubber tubing, ¼ in. bore over the 2 in. length of the 5⁄16 dia. aluminum sheath, so that the ends of the rubber tube extend 1 in. beyond the extremities of the sheath.

(2) Butt the ends of the aluminum tube and the connection, and slide the sheath in the rubber tube over the joint so that the joint comes at the middle of the sheath.

(3) Bind the two ends of rubber tubing with wire. First tie the wire near the sheath with a simple knot, leaving one short end free, which is pressed down along the tube and bound under. The wire is wrapped around the tube and when the wrapping is finished the two ends of the wire are twisted together and cut off, leaving a ¼ in. stub to prevent slipping. In binding the rubber care should be taken not to cut it.

Document 2-8(b), "Special Aerodynamical Investigations," NACA Annual Report for 1919.

SPECIAL AERODYNAMIC INVESTIGATIONS.

In the summer of 1919 the executive committee approved a program of scientific work to be carried out at McCook Field, Dayton, Ohio, under the supervision of its aerodynamical expert, Dr. George de Bothezat. This work involved: First, the theoretical analysis of the full performance of an airplane in steady flight; second, the development of new instruments and methods in order to measure in a single test flight the full performance characteristics of an airplane; third, the analysis of the full performance record of an airplane and deductions there from as to how the efficiency of an airplane can be increased by minor changes; fourth, the making of such minor changes in a given type of airplane to be followed by a second full performance test; fifth, the checking of the results against the original theory and the necessary modifications of the theory to permit in the future the determination of all the performance characteristics of an airplane in steady flight by mathematical calculation.

Dr. de Bothezat has been stationed at Dayton since August, 1919, and a small staff has been selected from members of the engineering division of the Air Service and assigned by the commanding officer at McCook Field to work with him in the prosecution of this work. The present commanding officer at McCook Field, Col. Thurman H. Bane, is also a member of the National Advisory Committee for Aeronautics. This work has been successfully inaugurated with the hearty cooperation of the officials and civilian engineers of the Air Service.

Document 2-9(a–c)

(a) "Office of Aeronautical Intelligence," *NACA Annual Report for 1918* (Washington, DC), pp. 24–25.

(b) John J. Ide, Technical Assistant in Europe, to the NACA, excerpts from "Report on Visit to England, July 1–22, 1921," 4 August 1921, Ide Collection, Box 8, National Air and Space Museum, Washington, D.C.

(c) John J. Ide, Technical Assistant in Europe, to the NACA, "Wind Tunnel at Issy-les-Moulineaux," 8 December 1921, Ide Collection, Box 8, National Air and Space Museum, Washington, D.C.

While the young NACA actively pursued the construction of a laboratory to seek new aeronautical knowledge as America fought in a world war, the committee recognized that considerable progress continued to be made in other countries and that some means of managing the growing body of "scientific and technical data relating to aeronautics" was essential. Thus, the committee established the Office of Aeronautical Intelligence in 1918. As outlined in the *NACA Annual Report for 1918*, the Office of Aeronautical Intelligence worked closely with the military and naval intelligence offices, including "special committees stationed at London, Paris, and Rome to collect information regarding all phases of the scientific and technical study of war problems."

In December 1918, a naval reserve ensign by the name of John J. Ide wrote the chief of naval operations (aviation) requesting that he be assigned to the naval attaché at the American Embassy in Paris to follow the progress of European aviation. Although the navy originally denied his request, Ide persisted and ultimately received a Paris posting in 1921 as the NACA's technical assistant in Europe. In this role, Ide repeatedly visited manufacturers and aeronautical laboratories throughout Europe, and he submitted many detailed reports on airplanes, wind tunnels, instruments, engines, and other aviation-related matters to the committee on a regular basis. The two documents reproduced herein are typical of Ide's reports. The reader will note that Ide took great care not to judge the merits or applicability of what he observed, but rather to report what he had learned as factually as possible.

The NACA treated these reports as confidential, but the information proved useful to the American military, manufacturers, and the staff at Langley. For an example, note Ide's description of an optical method to observe propeller blade bending under the "Aerodynamical Department" section of his 4 August 1921 report. While not totally successful in England, there can be little doubt that this idea influenced the Langley researchers who developed a successful technique to optically measure blade bending in the Propeller Research Tunnel a few years later.

Document 2-9(a), "Office of Aeronautical Intelligence," NACA Annual Report for 1918.

OFFICE OF AERONAUTICAL INTELLIGENCE.

In January, 1918, the need for a central governmental depository in Washington for scientific and technical data relating to aeronautics was recognized, and the Aircraft Board suggested that the National Advisory Committee for Aeronautics was the logical governmental agency for the collection and classification of such data to be made available to the military and naval air services in this country. This committee, accordingly, established an Office of Aeronautical Intelligence and adopted rules and regulations for the handling of its work.

The committee has made the necessary arrangements at home and abroad for the collection of such data. There are many sources of obtaining such information, the chief at the present time being the research information committee, organized under the National Research Council in January 1918, by funds provided by the Council of National Defense. It consists of the Director of Military Intelligence, Director of Naval Intelligence, and Dr. S. W. Stratton as chairman.

The purpose of the research information committee is to serve as a collector and distributor of scientific and technical information regarding all war problems. Special committees stationed at London, Paris, and Rome collect information regarding all phases of the scientific and technical study of war problems and transmit the same to the central committee in Washington for distribution to the interested services. Similarly, these special committees receive information from Washington and transmit the same to the interested services abroad.

Since February, 1918, Dr. William F. Durand, chairman of the National Advisory Committee for Aeronautics, has served as scientific attaché to the American Embassy in Paris, representing the National Research Council on this research information service, and has, in addition, acted as special representative of the Aircraft Board at the International Aircraft Standardization Conferences in London in February and in October, 1918, besides serving as a special liaison officer in aeronautical matters between France and the United States.

In September, 1918, Dr. W.C. Sabine, head of the department of technical information of the Bureau of Aircraft Production and a member of the National Advisory Committee for Aeronautics, was placed in charge of the Office of

Aeronautical Intelligence of the National Advisory Committee for Aeronautics, with the title of director of scientific and technical data.

Many valuable documents dealing with important research problems in aeronautics have been secured by the Office of Aeronautical Intelligence, and copies have been distributed to those concerned with the problems involved.

The committee has established in connection with its Office of Aeronautical Intelligence, and particularly for the use of its engineering staff, a small selected library, containing the most useful and valuable aeronautical and technical books and publications.

Document 2-9(b), John J. Ide, excerpts from "Report on Visit to England, July 1–22, 1921."

American Embassy,
7 Rue de Chaillot, Paris, XVIe
August 4, 1921.

CONFIDENTIAL

From: Technical Assistant in Europe, U.S.N.A.C.A..
To: National Advisory Committee for Aeronautics, Washington, D.C.
Subject: Report on Visit to England, July 1–22, 1921.

On July 1st I went from Paris to London.

R.A.F. Pageant.

On July 2nd I witnessed the R.A.F. Pageant at Hendon. The Pageant was a remarkable display of the proficiency attained by the Royal Air Force in formation flying, fighting, stunting, and bombing. The airplanes used, with one exception, were standard service types developed during or shortly after the war. The exception was the Siddeley "Siskin," which replaced the Westland "Wagtail" in the mock duel with the Nieuport "Nighthawk."

Siddeley "Siskin".

The Siddeley "Siskin" (Figs. 1 and 2) has been built by the Armstrong-Siddeley Company of Coventry. It is a single-seater fighter, originally designed to take a 320 HP A.B.C. "Dragonfly" radial engine. After the failure of this engine to fulfill expectations, the design of the "Siskin" was slightly changed to accommodate the Siddeley 300 HP 14 cylinder radial engine. This engine, although considerably heavier than the "Dragonfly," is very reliable and develops its rated power. The "Siskin" clearly outmaneuvered the "Nighthawk" in the mock fight.

As seen from the illustrations, the "Siskin" has one pair of inclined struts on each side of the fuselage. Ailerons are fitted only to the upper plane which has a certain amount of overhang. Two sets of struts run from the fuselage to the upper plane, forming two W's.

Tests have been discontinued for the present with a single cylinder water-cooled engine of 8 x 11 in. bore and stroke. It has developed 120 HP and there are rumors that Beardmore is going to construct a slow speed ungeared airship engine of six of the cylinders, the total weight to be 1600 lbs. Tests are proceeding with direct fuel injection with a monosoupape air-cooled cylinder. The fuel is forced into the cylinder under pressure, mixing with air sucked in through the valve.

I saw a Siddeley 150 HP 7 cylinder radial engine being tested. The Siddeley radials, designed by Capt. Green, are very highly considered at the R.A.E. While fairly heavy, the 150 HP model weighing 400 lbs. complete, they are very reliable.

The R.A.E. is still occupied with redesigning the A.B.C. "Dragonfly" engine. As redesigned, the aluminum heads overlap the steel cylinders by 1 inch. The heads have fins to assist cooling. The crankshaft and master rod have been made heavier, and the induction system is quite new. Six of these engines have been finished to be fitted to Nieuport "Nighthawks."

A universal engine test bench, similar to the standard type but lightened, has been constructed for the purpose of being installed in a Handley Page, the fuselage of which has been fitted with an aluminum lined chamber to take the bench. A small propeller, placed in the nose of the fuselage, is to be connected with the engine under test. Mr. Smith stated that it was possible that tests with this apparatus might not be carried out as one of the engine testing rooms is to be converted into an altitude chamber, Squadron Leader Norman having studied the installation at the Bureau of Standards while in America.

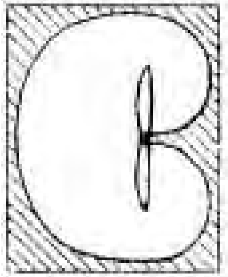
Aerodynamical Department.

I was taken through the 7 x 7 ft. and the 4 x 4 ft. wind tunnels. The screen fitted in the newer 7 x 7 ft. tunnel, which has an engine of 200 HP, effects a saving of power in the order of 15% by smoothing out the air flow. This screen consists merely of a brick wall in which there is an air space between each brick.

Considerable work has been done in propeller design for wind tunnels. Contrary to expectations, a four-bladed propeller gave more even flow than one with six blades.

Observations have been made of the bending of propeller blades. Small white squares have been marked along the otherwise black blades of a propeller. The bending effect at various speeds has been observed by means of reflected light. These experiments have come to a standstill owing to inability to express mathematically the curve of the blade.

A small propeller has been enclosed in a box suitably shaped on the inside so that the propeller makes its own air. The section of the box is shown in the sketch given herewith.



Through windows on the side of the box the behavior of the air stream is observed by the movements of threads suspended from another thread.

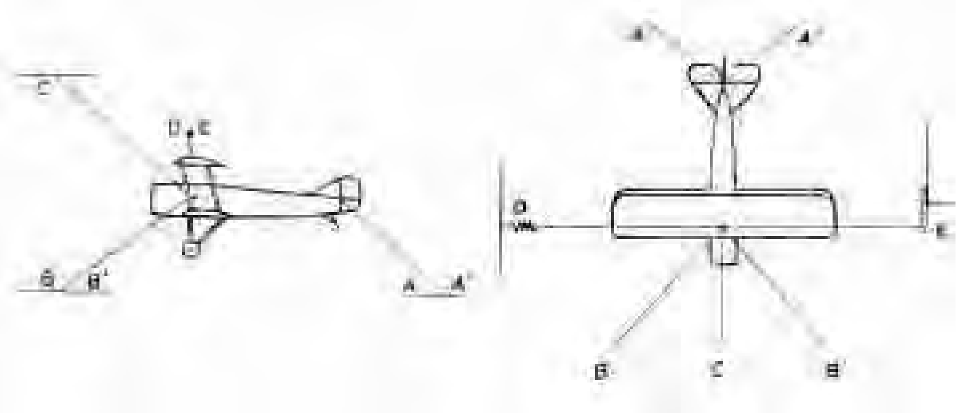
The experiments in recording the pressures along propeller blades have been interrupted by an accident, the plate holding the manometers having broken loose and smashed them. Contrary to previous reports, no difficulty was experienced with fastening the tubes to the propeller blades.

[. . .]

<u>National Physical Laboratory.</u>

On July 19th I visited the National Physical Laboratory at Teddington, where I was taken through the wind tunnels by Mr. Nayler.

The 7 x 14 ft. wind tunnel has been completed and is in operation. The power is supplied by two 200 HP engines with a synchronized gear which has been developed by Vickers. A perforated brick wall, similar to that used at the R.A.E., divides the room into two sections. The principal advantage of this wall according to Mr. Nayler, is the fact that the wind tunnel can be made very much shorter with it than without it, thus saving a considerable amount of space. No balance has yet been installed in this tunnel. At present the 7 x 14 ft. tunnel is being used for the measurement of rotary derivatives, such as the rolling due to rolling (Lp), the yawing due to rolling (Np). and the rolling due to yawing (Lr). A model of an S.E.5 suspended on wires as shown in the accompanying sketch, is used for these measurements.



The amplitude of yaw or roll is ascertained by measuring the movement of a spot of light reflected from a lamp by a mirror on the side of the fuselage to a strip of thin paper placed at the observer's station at the side of the tunnel. The model is placed in motion by moving the arm (E), to which one end of the wire running thwartships above the model is secured, there being a spiral spring at the other end of this wire.

One of the 7 x 7 ft. wind tunnels is being used for pressure plotting over a model rigid airship hull of the R 33 type. Mr. Nayler stated that this test is a result of the fact that the bow girders of several Zeppelins collapsed when making turns at high speeds. Although the British had as yet experienced no trouble from this source, it was desired to ascertain the pressures to which the airship was subjected.

In another 7 x 7 ft. tunnel there is being conducted a series of tests for thrust and torque of a family of air screws. There are six air screws in the family, all having the same section but with various pitch diameter ratios. The diameter of all the air screws is 3 ft. 6 in.

Mr. Nayler also conducted me through the material testing laboratory. A new method of fatigue testing of metals has been developed by Mr. Gough of the N.P.L. It is expected that a report of this method will shortly be published in "The Engineer," as the manuscript has been accepted.

By Mr. Gough's method, the ultimate strength of the material under test can be determined in about 15 minutes instead of the considerable number of hours necessary by the present methods. The new method is based upon the principle that a change in molecular construction resulting in an increase in the amplitude of vibration occurs after a short period of test. Light is employed for the measurement of the vibrations.

On July 22d I returned to Paris from London.

Respectfully,
[signed] John Jay Ide.

Document 2-9(c), John J. Ide,
"Report on Wind Tunnel at Issy-les-Moulineaux," 1921.

December 8, 1921

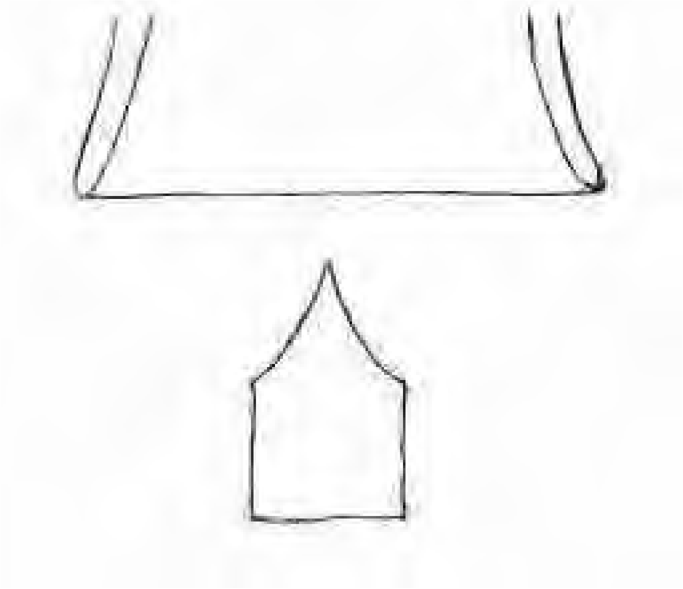
From: Technical Assistant in Europe, U.S.N.A.C.A.
To: National Advisory Committee for Aeronautics, Washington, D.C.
Subject: Wind Tunnel at Issy-les-Moulineaux.

I recently visited the wind tunnel under construction at the headquarters of the French Aeronautical Technical Service at Issly-les-Moulineaux outside the gates of Paris. This tunnel is housed in a building of brick, steel and glass about 100 ft. wide and 210 ft. long which was almost completed at the time it was decided to use it for a wind tunnel. The building is not particularly well adapted for the purpose having numerous columns and trusses which project into the interior and impede the smooth flow of air around the tunnel. Also, not much advantage can be taken of good atmospheric conditions by admitting outside air as there is only one large door which is at the entrance cone end.

The tunnel itself, of the Eiffel type, is constructed entirely of reinforced concrete and it was made to the designs of the Aerodynamical Section of the Technical Service by the company of which M. Caquot, formerly director of the Technical Section, is the head. The tunnel is in a large cement pit about eight feed deep and taking up the entire floor space of the building with the exception of gangways along the sides and ends. The tunnel is supported by two concrete walls running longitudinally and about eight feet high leaving a free air passage under the tunnel. The entrance nozzle is set back very far from the front end of the building. The section of the outer walls of the collector has been made square instead of circular and of a width equal to the diameter of the front end of the collector. This permits the outer walls of the collector to be parallel to the walls and floor of the room and also eliminates any break in contour caused by the experimental chamber. Aft of the latter, the outer walls are gradually faired into the diffuser.

The diameter of the tunnel at the experimental section is 3 meters (9.84 ft.) and at the propeller end 7 meters (22.96 ft.). The propeller itself is to have 6 blades with square tips and each blade is to be an arc of a circle. The pitch of the blades will be variable, though not while in motion. It was stated that the efficiency of the propeller was in the neighborhood of 75 per cent. The propeller will be driven by an electric motor of 1000 H.P. which it is expected will enable a speed of 80 meters (262 ft.) per second to be realized.

Behind the diffusor is the concrete stand for the motor. In order to change the direction of the air leaving the diffusor the two forward faces of the stand are curved thus in plan:



It is proposed to continue these curves upward and outward by wooded partitions.

Entrance to the experimental chamber is by means of a passage with a staircase arranged in the thickness of one of the supporting walls of the tunnel. There are two doors to insure an air lock. The chamber itself is very large being roughly a cube of about 20 ft., a side into which project the ends of the diffusor and collector. The only natural light is that which comes through the latter. In the centre of the floor is a square trap closed by a platform. When it is desired to have large models, etc. brought into the experimental chamber, movement of a lever lowers the platform sufficiently for it to be rolled clear of the opening on rails attached to the inner faces of the two longitudinal walls below the tunnel.

Arrangements will be made so that the tunnel can be partially or completely closed at the experimental section if desired. There are a number of hooks flush with the concrete surface of the diffusor and collector cones near the experimental section. These hooks can be pulled outward and to them can be attached the supports of the cones of a small tunnel of 80 cm. (2.62 ft.) diameter placed concentrically with the large tunnel. By using the same propeller and the power available for the large tunnel it is expected that a speed of 400 meters (1312 ft.) per second will be reached by the 80 cm. tunnel.

The model suspension will be by wires instead of by a spindle. It is understood that the balance will be of a dynamometric type having various gauges to which the wires supporting the models will be attached, according to the strength of the forces to be measured.

It is hoped to have the wind tunnel completed by July 1922.

[signed] John Jay Ide.

Document 2-10(a–b)

(a) D. W. Taylor, letter to Frederick C. Hicks, 9 December 1919, Hunsaker Collection, Box 3, File H.

(b) Edward P. Warner, excerpts from "Report on German Wind Tunnels and Apparatus," October 1920, Box 18, Folder 24, McDermott Library Archives, University of Texas–Dallas. [Also published, with the same title, in *Aerial Age Weekly* (15 November 1920): 275–277.]

While the NACA worked to gather information through its Office of Aeronautical Intelligence, other Americans with a particular interest in foreign developments did all they could to stay abreast of these activities as well. U.S. Navy officer Jerome Hunsaker was among the first to cultivate friendships with aeronautical experts outside the United States. After a tour of the primary European laboratories in 1913, he maintained these relationships during and after World War I. As the 9 December 1919 response—from his superior Rear Admiral D.W. Taylor to an inquiry from Congressman Frederick C. Hicks—shows, the expert intelligence provided by sources like Hunsaker proved useful to politicians and policy makers as well as researchers. Of particular interest are the comments on Japan's purchase of French technology to aid its own fledgling aviation industry. Although relative newcomers to aeronautics in 1919, Japanese progress over the next two decades would provide the island nation with formidable aerial weapons for World War II.

Edward P. Warner, an aviation consultant who had been the NACA's first chief scientist, prepared an independent report to the NACA covering his 1920 inspection of German laboratories at Göttingen, Aachen, and Dessau that he also published in *Aerial Age Weekly* later that year. Coverage of the Aachen and Dessau installations was brief, but Warner went into considerable detail concerning Ludwig Prandtl's second wind tunnel at Göttingen. The detail was warranted, for this was the first truly modern, closed-circuit wind tunnel, with efficient turning vanes in the corners and a variable cross-sectional area for improved air management, lower power consumption, and reduced turbulence. Prandtl's balance apparatus for this tunnel also impressed Warner with its accuracy and simplicity of operation.

Document 2-10(a), letter from D. W. Taylor to Frederick C. Hicks, 1919.

December 9, 1919

My dear Mr. Hicks:

Referring to your letter of the 4th inst., relative to the Aviation Program of Foreign Countries, my information in regard to Aviation in Japan is not very full, but it is possible that Captain Craven can give you something additional. The last we have heard about Japan was that they had a mission of some 24 officers of the army and navy, together with civilian professors representing the engineering colleges in Japan, which has made an inspection of aviation in England, France, Italy, and the United States and has now returned to Japan and, presumably, will make definite recommendations.

There is a rumor that the Japanese have purchased the rights to manufacture one or two French aviation engines developed during the War and are equipping one of their arsenals to turn out this engine. I understand that French experts have gone, or will go, to Japan to assist in this development. The Japanese are also supposed to be purchasing in France planes left over as excess War material. I have no information that they have purchased anything in the line of large flying boats. I know that they made no attempt to do so while in the United States.

With regard to Italy, I don't believe that the Government has under way any military or naval program, but the Caproni Company is attempting to develop their large bomber for passenger carrying with the idea of running a commercial business for the expected tourists trade. The Italian Government is also rebuilding one or more of their semi-rigid airships so as to fit them to carry a large number of passengers. This appears to be an attempt to make some use of the excess stocks of airships left over from the War.

In France such development as is proceeding appears to be entirely directed toward commerce, and the French have arranged for a subsidy to concerns which will maintain machines and aviators available for military purposes in time of war. The French also are commencing the construction of rigid airships primarily for commercial travel between France and Africa, but I understand the Government's connection is very close as the ships are being designed by the Technical Section of the Army, but are to be operated for commercial purposes.

In England all seems to be in turmoil. As you know, General Seely under Secretary of State for Air has resigned, giving as his reason the impossibility of the existing arrangement which places the Royal Air Force under Mr. Churchill, the War Minister. The opposition press intimates that Mr. Churchill having absorbed the Royal Air Force is attempting by political maneuver to obtain the post of Minister of Defense, and absorb the Admiralty in addition, thus carrying consolidation to the limit. The Royal Air Force started off the beginning of the year with an ambitious program for the development of civil aviation, but due to the economic crisis their funds have been cut to about one half the original credit, which appears to be no more than enough to wipe out outstanding war obligations and close out their various contracts. The result is that building has stopped on practically all types, in particular the great program of rigid airships is suspended for lack of funds. The only new airship building in England is R-38 which is being completed for the United States Navy.

The Royal Air Force is under fire from three sides; from the public for gross extravagance and bad administration and waste of public funds, because they did not cancel war-time contracts with a firm hand, but have permitted certain favored concerns to continue the production of aircraft contracted during the War with a result that now the cut has come, the layoff of men is demoralizing, and the amount of money already spent is wasted. The second attack comes from the Aeronautical trade or industry, which objects to military control in the development of commercial aviation and complains of the lack of results and positive action in stimulating this development as promised. The third attack comes from those interested in the efficiency of the Fleet with a charge that since the Air Ministry has taken control no progress has been made in Naval Aviation, but on the contrary conditions have become progressively worse. The development of large flying boats has been allowed to come to a stop because of lack of sympathy with the type in the Royal Air Force. The flying boat type and the construction of airships of the Zeppelin type for use of the Fleet has been stopped for lack of funds because the Air Ministry has frittered away its fund on other things which are of no benefit to the Navy, so it is alleged.

The net result in England is a very pretty row and it is difficult to see exactly what is going on because of the smoke. I enclose, herewith, extracts from the British aeronautical press whose tone will give you an idea of conditions.

With regard to your request for reports of the National Committee for Aeronautics, I forward, herewith, the reports mentioned. The correct title of this Board is "National Advisory Committee for Aeronautics," as given by the Sundry Civil Act for 1920. This Committee was originally called "Advisory Committee for Aeronautics" in the Naval Bill of 1916 which carried funds for the first year. This title was changed because of confusion with a British Committee of identical title.

I am not in very close touch with the Helium Plant which comes under Admiral Griffin, but I understand that a Helium Board which coordinates the interests of the Army and the Navy in this matter is directing the development of this project, and that things are going along in accordance with the agreed plan, and so far as I know without any friction or difficulty. You know there have been some difficulties in the past with the Bureau of Mines of the Interior Department which was urging a technical process for the separation of Helium which other experts did not favor in war time as it was experimental. Possibly, what you have heard is a result of this.

Very respectfully,

[signed] D. W. Taylor.

Document 2-10(b), Edward P. Warner, excerpts from "Report on German Wind Tunnels and Apparatus," 1920.

REPORT ON GERMAN WIND TUNNELS AND APPARATUS

By

Edward P. Warner

The National Advisory Committee for Aeronautics, in view of the important research work that has been conducted in the German wind tunnels at Göttingen, Aachen, Dessau, and Friedrichshafen, requested Professor Edward P. Warner, the Committee's Acting Technical Assistant in Europe, to submit the following report, descriptive of the above mentioned wind tunnels, together with methods of operation and details of the apparatus used.

It is appropriate that any discussion of aerodynamical work in Germany should begin with Göttingen and with Prof. Prandtl, where the first serious work of the kind was undertaken, before the war, and where the most extensive and interesting results have been obtained both in respect of wind tunnel testing and of purely mathematical investigations.

There are, at the present time, two wind tunnels at the Göttingen laboratory, one being the original 1-meter tunnel of pre-war days, but moved into a new building; the other a newer installation 2 meters in diameter. During the war both tunnels were kept in service, but that is impossible with the present shortage of funds and of employees, and the small tunnel is now seldom used. The small tunnel was described by Dr. Zahm in his report to the Smithsonian Institution in 1913, and need not here be gone into in detail. The newer and larger one is built on essentially the same principle, but with substantial modification in detail. In the first place, the plane of the closed circuit which the air follows has been turned from the horizontal to the vertical, the return passage being underground. The whole tunnel, except the portion immediately around and adjacent to the test section, is made of concrete, so there is no question of air-tightness.

The section, 2 meters in diameter at the throat, expands in each direction to a diameter of 4.5 meters before the first turns are reached, and this size is maintained all the way around the return, making an area ratio of 5 to 1. The air is still guided around the turns by means of vanes, but these vanes are no longer adjustable in position or inclination to secure regularity of flow, such adjustment having been found unnecessary, nor are they now made in honey comb form.

They are cast in the concrete, are of crescent form with a maximum thickness of about one-fifteenth of the chord, which is about 18 inches, and are spaced approximately 8 inches apart. The vanes are not arranged to cause all the particles of air to swing about a common center, as might perhaps be expected, but are all of the same size and all curved to the same radius, so that the outside boundary of the tunnel comes to a corner and the section is increased at the turns.

The stream is not enclosed at its throat, although a tube is available which can be put into place to partially restrain the flow if desired. The stream being enclosed at every other point of its travel, the pressure at the throat is the atmospheric pressure in the free air, while that in the return passage is raised above atmospheric, and no experimental chamber or air-lock is necessary.

The only means provided for regularizing the flow is a honeycomb placed in the large section of the tunnel, before the contraction to the throat begins. This honeycomb is made up of metal plates separated by corrugated strips, so that the cells have a rather eccentric form, approximately semi-circular. The mean effective radius of these cells is about ⅜", the length 8 inches. Prof. Prandtl lays great stress on the value of placing the honeycomb where he has it, before the entrance cone, in order to avoid turbulence and minute eddies which he believes are produced by a honeycomb of the ordinary type. Admittedly there is some loss in regularity of flow as ordinarily judged when the honeycomb is moved farther away from the throat, yet the regularity at Göttingen seems to be very good. Incidentally, I found Capt. Toussaint quite convinced of the merits of Prandtl's placing of the honeycomb and preparing to adopt a similar disposition at St. Cyr, and the matter was also receiving serious consideration at the N.P.L.

The current is produced by a four-bladed wooden propeller of ordinary type, driven by a 300 H.P. electric motor. The maximum wind speed obtainable is 50 m. per sec., which, if the motor is working at rated power without overload, corresponds to an energy factor of 1.36 (a result not in any way remarkable). The motor is outside the tunnel and drives the propeller by a shaft passing through the wall at the turn.

The forces and moments on the models are ultimately weighed on ordinary platform balances, the only special apparatus being that which transmits the forces from the roof of the tunnel, or, more properly, from the platform erected above the airstream, down to the balances which are placed conveniently for the observers standing on the floor. The apparatus is primarily of interest in that the support in all cases is solely by wires, no spindles being used under any conditions. Furthermore the measurements of lift and drag are direct and entirely separate, no moments entering in until the balances are reached.

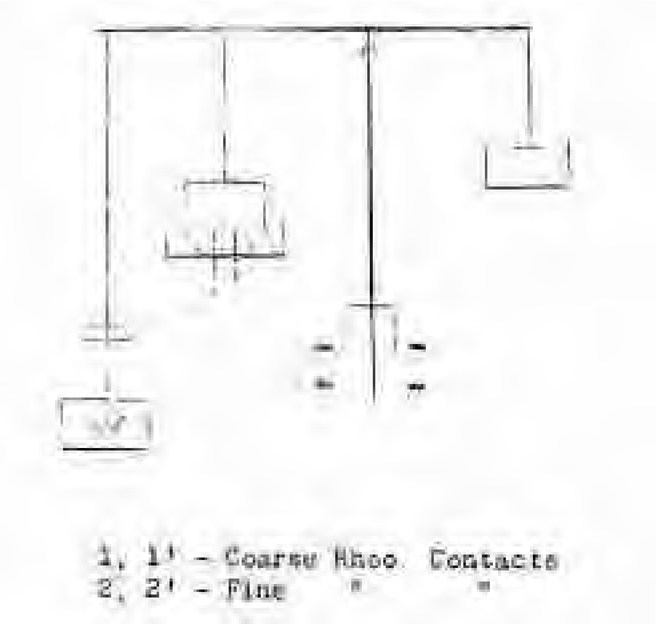
In the case of an airship model or other similar streamline body, the support is by five wires. Four of these are arranged in two pairs the two members of a pair attaching to the model at the same point and then diverging, their plane being perpendicular to the direction of the wind. One pair is attached about a quarter of the way back from the nose of the model, the other about three-fifths of the way back. The fifth wire is attached at the same point as the forward pair, and runs forward exactly parallel to the wind direction. At a point about two feet forward of the nose of the airship this wire terminates in a small ring to which are also fixed two other wires, one running vertically upwards and the other obliquely downwards and forwards to the floor at the end of the entrance cone. This diagonal wire is simply fixed to a screw-eye in the floor, while the vertical wire is attached to a crank on a rocking beam, another offset crank on which bears against a vertical rod running down to the pan of the platform balance. The pull in the vertical wire can thus be weighed directly, and, knowing the directions of the other two wires attached to the ring, the pull in the horizontal wire, which is equal to the drag of the model, can be calculated. As a matter of fact, I believe, although I am not certain, that the oblique wire runs off at just 45°, so that the balance reads the drag directly with ordinary weights. The total pull in each pair of vertical wires is similarly measured, the two members of a pair being attached to two cranks on a single beam so that the total is obtained directly. The lift on the model is then equal to the sum of the two readings (correction having been made for the tensions due to the weight of the model), and the pitching moment can be directly calculated from their difference. The angle of attack is adjusted by raising and lowering the rear beam and wires. Of course, as the angle is changed the horizontal distance between the points of attachment of the two sets of wires changes, and, if the rear beam were raised or lowered vertically, it would be impossible for both pairs of wires to remain truly vertical. The two beams are therefore connected together by a link forming, with the two sets of wires and the line connecting their points of attachment in the model, a parallelogram. The five wires suffice to restrain the model from all motions except those in roll, and these are resisted by its own weight. As an additional safeguard against rolling, a wire may be attached to the lower surface of the model and run downwards and backwards over a pulley, a heavy weight being hung at its free end, thus introducing a constant correction to both lift and drag. Despite this precaution, the model of an airship which was being tested while I was at the laboratory several times started oscillating badly and it was necessary to reach into the current with a stick and steady the model just before the final balance readings were taken.

In the case of wings, the same balances are used and the method is essentially the same, but there are differences in details of attachment. The model is made with four hooks cut from thin sheet metal mounted on it, three of these hooks being distributed along the leading edge and the fourth carried by a rod which projects about one and a half chord lengths to the rear of the trailing edge. The two hooks mounted near the tips on the leading edge carry wires running vertically to the forward lift beam, the center hook carries the wire which runs forward to measure the drag, and the rear hook, on the rod, forms the point of attachment for two wires running off in a V to the rear lift beam. The model is then definitely fixed except in regard to yaw. Should any particular model show a tendency to oscillate in that respect it can easily be checked by running two wires from each corner of the leading edge to the forward lift beam, the whole set of four then appearing from the front as a W. This is to be done as a regular practice at the Zeppelin laboratory.

The spindle correction for wings is obtained by substituting for the model a T made of two pieces of stream-line wire about ⅝" x $^3/_{16}$" in section. The cross-arm of the T has a length equal to the span of the aerofoil, the shank a length equal to the distance between the leading edge of the aerofoil and the rear point of attachment used for the determination of pitching moments. This T is hung up in exactly the same manner as the model, and the drag measured. The resistance of the stream-line wires being known with fair accuracy by computation and by previous experiment, the effect of the supporting wires can be determined at once by subtraction. In the case of an airship model the method is the same, but a single pointed rod is used in place of the T, the rod being held parallel to the wind direction. The method is not absolutely satisfactory in this case, as no allowance is made for the interference between the supporting wires and the model, which is quite different from the interference between the supporting wires and the rod, but the per centage error from this cause is undoubtedly very small.

The wing models in use at Göttingen are all made of plaster, scraped to form while still soft. One or, more usually, two aluminum plates are used as the core. The finish is excellent, as good as I have ever seen on plaster models, although I think it is no better than on the best of the models made for the Garden City laboratory. The standard size of wing model at Göttingen is 1 m. by 20 cm., an aspect ratio of 5.

The method of regulating the wind speed is of particular interest, as it is entirely automatic and seems to work with absolute perfection. The speed is fundamentally measured, in accordance with the usual practice by measuring the pressure difference between the large and the small sections of the tunnel. The pressure difference is measured by leading the pressure which differs from atmospheric in under a cup the rim of which dips in oil, and by weighing the "lift" on this cup with a balance, just as is done at the Leland Stanford tunnel. In this case, since the pressure under the cup is greater than atmospheric, the weights have to be hung on the same side of the balance axis with the cup. On the other arm of the balance is an oil dash-pot, while a long pointer runs downwards from the balance axis and moves between two contacts. As the pointer makes contact on one side or the other a servo motor is started in one direction or the other, steadily moving a rheostat and increasing or decreasing the motor speed, in such a way as to bring the wind speed back to normal, until the contact is broken and the pointer once more floats freely. In order to secure a greater range and a more sensitive maintenance of speed, the pointer is made with a subsidiary contact in the form of a light flat spring on each side of the main contact, and these subsidiary contacts actuate a fine rheostat. If the deviation from normal speed is really large, the unbalancing of the beam overcomes the resistance of the light contact spring and the beam moves, far enough to bring the main pointer into contact with its stops and actuate the coarse rheostat. In order to prevent the fine rheostat being brought up against its stops, a contact is automatically made when the handle draws near to the limit of its travel and this moves the coarse rheostat one step. The whole adjustment is now automatic. There is a push-button starter, and, the motor once started, the operator has only to hang the proper weight on the balance (the weights are marked directly in meters per sec.) and go away and leave it.



Although it appears that not much stability work has been done at Göttingen, a balance for measuring all forces and moments simultaneously is now under construction and is nearly completed. For this purpose, the model is hung from a platform, the model being heavily enough ballasted to keep all the suspension wires taut. These suspension wires are rigidly attached to the platform. The platform is held in position by six wires, two running vertically, two parallel to the wind stream, and the remaining pair horizontally and perpendicular to the tunnel axis, and the simultaneous measurement of the tensions in all these wires permits calculation of the six forces and moments acting. This is much the same in principle as the roof balance which has long been used for certain special experiments at the N.P.L. and to the all-wire-support roof balance which is now being designed at the R.A.E. The most interesting and original feature of the Göttingen balance is the automatic adjustment of the six balances, all of which are arranged in a row before the operator.

[*At this point the author goes into great detail about the design of the wind tunnel balance, which is omitted here.*]

No propeller testing has been done at Göttingen recently, but a propeller balance of unique type has been designed and partially constructed. The propeller under test will be driven through bevel gears and a vertical shaft from a motor outside the wind-stream, and the whole apparatus, motor, transmission, and propeller, will be carried on floats in a tank of water or oil. The thrust is to be measured by the combined pull in two wires running parallel to the wind direction, the torque by the pull which has to be applied to a vertical wire attached to one side of the floating platform in order to keep that platform horizontal. The cross-wind force and pitching and yawing moments can also be obtained with ease if desired by running other wires in suitable directions. In order that the measurements of torque may be sensitive, the inherent stability of flotation must be small, so that the angle of tilt due to the torque would be large were it not for the restraining wire. In order to accomplish this the metacentric height must be reduced or, since metacentric height is equal to I/V and V is fixed by the weight of the apparatus, the moment of inertia of the water-line section about a longitudinal axis must be made as small as possible without actually bringing the metacenter below the center of gravity. This is done in the Göttingen apparatus by making the floats proper of such a size that they will be completely submerged and the surface of the water will only be broken by the tubes which connect the floats to the platform. The propellers tested are to be one meter in diameter.

Great confidence is felt at Göttingen that the Prandtl theory of wing action, together with the work done along the same lines by Munk and Betz, now furnishes a practical tool for the engineer. There is no question of the consistency of the results obtained within a certain field, and notably in predicting the effect of changes in aspect ratio. A series of tests on wings of the same section but of different aspect ratios was recently made, and when the results obtained were reduced to aspect ratio 5 by Prandtl's formulae all the points lay, well within the experimental error, on a single curve. In view of this, Prandtl considers further testing for aspect ratio effect on new sections as quite unnecessary.

Aerodynamic Work at Aachen.

A regular course in aerodynamics is given by Prof. Karman at the Technische Hochschule at Aachen, and there are a number of students undertaking investigations in that field. The laboratory equipment, however, is very limited. The only wind tunnel available is on the roof of the building and takes its air from the free atmosphere, with no protection from gusts. It is also very short, and the flow is so irregular that it would be impossible to work with wings or, indeed, to do any work at all with a balance. The tunnel is two meters in diameter and has a 100 H.P. motor, realizing a speed of 33 meters per sec. There is no expansion in the exit cone, the sirocco fan which produces the current being of the same diameter as the throat of the tunnel. The experimental chamber is enclosed as in the Eiffel type, and the irregularity of flow in the stream is attested by a pronounced circulation of air all around the chamber. The honeycomb cells appear too large for best results, being about eight inches in diameter and two feet long. The most interesting thing about the Aachen tunnel itself is its noise-making characteristic. Up to about 25 meters per sec., while not by any means silent, the amount of noise is not unusual for the corresponding size and speed. At that speed there is a sudden change, and above 25 m. per sec. there is an ear-splitting shriek totally unlike anything that I ever heard from a wind tunnel before. The most peculiar thing about it, however, is that if one thrusts an arm into the stream until the hand is near its middle the high-pitched noise instantly stops, recommencing as soon as the arm is withdrawn. The question of noise in wind tunnels is at present considered as a vital one all over Europe, having come up for discussion without any effort on my part at Aachen, St. Cyr, Teddington, and Farnborough, and a multitude of different causes are ascribed. The problem is a very interesting one allied to, but more complex than, the general question of propeller noise, on which Bryan and Lanchester have already done some work and on which the former presented a paper to the British Association last year. Apparently no one has even attempted to explain completely the strange behavior of the Karman tunnel, but it is evidently due to the formation and dissipation of eddies with an accompanying strong reduction of pressure and density. The sound is not unlike the whine of a high-velocity bullet as it passes overhead.

At the present time the Aachen tunnel is working chiefly on the heat dissipation from and the flow of air through radiators. In carrying out these experiments a section of the radiator is closed off from the remainder, so that no water can flow through it, and minute holes are made in the sides of these isolated passages. Connecting the holes thus made to a gauge, the static air pressure is found at several points between those where the air enters and where it leaves the radiator, and the resistance can be studied and computed from the pressure gradient thus found, in the manner commonly employed in examining the flow of fluids through pipes. Water is pumped through the radiator at variable speeds and variable inlet temperatures and the heat dissipation is computed in the usual way.

Prof. Karman anticipates doing some work at very high velocities with the aid of a l00 H.P. centrifugal compressor delivering air at a pressure of one-half atmosphere, but no start has been made as yet.

Karman is now working on the theory of turning wings, or wings which rotate simultaneously, but not necessarily with constant or equal angular velocities, about an axis in the plane of the chords and about another axis parallel to the first but at a considerable distance from the plane of the chords. If the two rotations are properly adjusted to each other there is a lift and also a driving force, the action being something similar to that of flapping flight and eliminating the need for a propeller. The investigations now being made are directed towards finding the most efficient type of rotation, and will be followed by actual experiments with such wings. Another professor at Aachen is specializing in the study of the equations of viscous flow and attempting to apply them to aircraft parts, apparently proceeding somewhat along the lines on which Bairstow is now working.

A small water-channel is being used by one of Karman's research students for photographing the flow through diverging and converging passages of various sorts with a view to analyzing the flow in the exit and entrance cones of wind tunnels. Aluminum powder is used for making the flow visible. The most striking thing about the results so far obtained is the abrupt change in type of flow which appears about ten seconds after the motion has commenced, the flow initially "filling" a diverging cone and then breaking away from the walls, passing straight down the center as a stream of approximately constant cross-section with a region of dead-water on each side. Unfortunately, no moving pictures have as yet been taken to show the exact mechanism of the change. The alteration of the flow probably depends at least in part on the cessation of acceleration and the establishment of steady conditions.

During the war Prof. Karman was in Austria and had there a very large wind tunnel, which, however, does not appear to have been run for very long or in a very scientific manner. The tunnel was 3.2 meters in diameter, and propellers up to a diameter of 2.8 meters were tested, the ratio between these figures being far larger than has ever been considered safe elsewhere. The speed attained was 40 meters per second. The propeller thrust and torque were measured by the pendulum method, the whole apparatus being carried by four parallel wires. The tunnel was apparently rather short in proportion to its diameter, and the motor driving the propeller under test and the supporting wires were placed outside the mouth, a long shaft projecting along the entrance cone to drive the propeller. The whole tunnel was on wheels so that it could be moved to one side when the pendulum apparatus and driving motor were to be used for making static tests. The cost of the tunnel was one million kroner, or $200,000 at normal rates of exchange.

The Junkers Laboratory at Dessau.

This wind tunnel is hardly to be compared with Prandtl's, as the aim in its construction was entirely different. The Junkers laboratory is admittedly solely commercial in its aims, its only object being to furnish data for improving the Junkers airplanes. None of the work done there has been published, and there is no intention of publishing any. The experiments at Dessau have been chiefly concerned with the development of improved sections for cantilever wings, although a little stability testing of an elementary sort has been undertaken, and the constant attempt has been to speed up and systematize the making of routine tests with a moderate degree of accuracy, rather than to seek increased refinement in measurement.

The Junkers tunnel is octagonal in section, but the octagon is not a regular one, the breadth of the stream being 1.2 meters, the height only 0.9. A fan is used instead of a propeller for producing the air current. The tunnel is of true Eiffel type as regards the experimental chamber, and a couple of inches of water is left standing on the floor at all times to minimize leakage and to suppress the dust. The drive is by an electric motor of 100 H.P., and the maximum wind-speed is 39 meters per second, giving an energy ration of .497/.

The steadiness of flow in the Dessau laboratory seems distinctly inferior to that at Göttingen, and there is no question that there is more disturbance of the air in the room and spreading of the stream. The honeycomb is similar to that used by Prof. Prandtl as far as the form and arrangement of the cells are concerned, but it is placed at the beginning of the throat instead of at an enlarged section. The cells are ½" in mean diameter and 2" long.

The balance at the Junkers laboratory is of interest chiefly because it is completely autographic. The measurements depend entirely on springs, no weights being used. The balance carriage slides longitudinally and is carried on two steel rods running parallel to the wind direction. The weight of the carriage is considerable, and the friction in sliding on fixed rods would be intolerably large, but this friction is practically eliminated by rotating the rods at high speed through a belt drive—a very ingenious and successful method. The longitudinal motion of the carriage is proportional to the drag, and is transmitted through a linkage to a pencil which moves vertically over a record-sheet. The lift is measured by the tilt of a beam which carries the model and which is also restrained by springs, and is also transmitted through a linkage to a pencil making its record on the same sheet as the first one. The angle of attack is changed steadily and progressively by a motor which winds up a wire attached to the trailing edge of the model, and which, at the same time, rotates the drum carrying the record-sheet at a constant speed. The making of a complete test requires only about twenty minutes. No attempt is made to keep the speed constant automatically, the rheostats being set at a beginning of a run and then left alone. It is assumed that the changes in speed due to voltage changes are negligible.

The wing models tested are made of wood, and painted so that I could not examine the method of construction in detail. A very large number of thick wings have been tested, at least a hundred models being stored in their cabinets at the time of my visit, and work is still continuing in the effort to improve the present Junkers wing. The best results obtained to date with thick wings at Dessau are: max. $^{L}/_{D}$ 17.0, max. Lc .65, at 35 meters per second. The maximum lift coefficient is not in any way remarkable, and the high maximum $^{L}/_{D}$ is largely accounted for by the way in which the model was supported. All tests are made with the aerofoil carried by a stream-line spindle about $^{3}/_{16}$" wide passing into the model at the center of the span and near the leading edge, and it is well known that such a support, when its resistance is corrected for in the usual approximate fashion, leads to values of the drag which are far below the correct figures.

The Zeppelin Wind Tunnel at Friedrichshafen.

The only remaining wind tunnel of any importance is that which is under construction, but not yet completed, for the Zeppelin Airship Company. To be sure, there is a tunnel at Adlershof, but this is old and little used, and was fully described in the report submitted by Dr. Zahm after his tour in 1913 and published by the Smithsonian Institution.

The Zeppelin tunnel has been designed by, and is being built under the supervision of Dr. Max Munk, and is naturally similar in many respects to the Göttingen installation, since it was at Göttingen that Dr. Munk received all his aerodynamical training and much of the apparatus there was designed by him. The Zeppelin tunnel, however, is considerably larger than either of those at Göttingen, and will be the largest tunnel in the world from the time of its opening until the new 7 x 14-foot channel at the N.P.L. goes into action. The diameter of the wind-stream at Friedrichshafen is 3 meters. The drive will be by two Maybach engines coupled together in tandem, delivering a total of 500 H.P., and a speed of 30 meters per sec. is anticipated.

The most original feature of the tunnel design lies in the provision of air-tight gangways, of much larger section than the tunnel itself or even than the large end of the exit cone, for the return of the air. The principle is the same as a Göttingen, but the mechanical execution is somewhat different, largely because of the great size of the laboratory. There are two return passages, one on each side of the tunnel, instead of a single one, and these passages take the form of gangways each about three meters wide and six meters high, with air-tight windows and air-locks and without any guide vanes at the corners. The passages are shaped at the ends so that there will be as gradual a change of section and of direction as possible, the whole, including the entrance cone, being made in concrete. The exit cone, however, is of wood. Since the gangways are air-tight, the throat section can be open to the atmosphere, just as at Göttingen, and admission to the test room is perfectly free at all times, no air-lock or other control being necessary. In order to permit access to the test chamber there is a subway under each of the gangways.

The slope of the exit cone is small, the vertex angle being 7-½°, and the length is not sufficient to give as large an area ratio as is the usual custom. The length of the cone is to be 15 meters and the diameter of the propeller 5 meters, the propeller being driven with the same gear reduction as is employed in the Maybach engines on Zeppelin airships.

No honeycomb has been fitted as yet, and Dr. Munk hopes to avoid the use of any straightening device, but his hopes apparently have no sound basis.

The measuring instruments will be of the same general type as those at Göttingen, but naturally much stronger. The whole weighing apparatus is to be installed on an overhead platform where the observers will work, the support being by steel rails carried by concrete piers. The most striking feature about the balance is its construction, which follows Zeppelin airship lines faithfully, the cross-beams used for weighing the lift being built-up duralumin lattice girders. The total weight of the moving parts will certainly be less than in any other balance ever built for so large a VL. Of the two bridges, the forward one will be fixed rigidly to the rails, but the other will be mounted on wheels and will be allowed to move longitudinally as the angle of attack is changed so that the horizontal distance between the two bridges will always remain equal to the horizontal distance between the lower ends of the two sets of wires. The adjustment of the angle of attack is by raising one of the bridges, the supports for which slide on vertical rods. The bridge is raised with a cable which is wound up on a drum by turning a graduated handwheel.

The speed is to be controlled automatically by the same method as that employed at Göttingen, the servo motor operating the engine throttles instead of a rheostat.

The Friedrichshafen laboratory will work for the parent airship company and for all its subsidiaries, of which there are a considerable number, including the airplane firm at Seemoos (formerly located at Lindau) where the Dornier flying boats are built, and the aircraft company at Staaken, for which Rohrbach is the chief engineer.

September, 1920.

Document 2-11

"Free Flight Tests," *NACA Annual Report for 1918* (Washington, DC) pp. 20–21.

Like the British Royal Aircraft Factory, the NACA sought to develop "as complete tests as possible of the performance, in all respects, of airplanes while in the air under normal conditions," and it established a subcommittee on free flight tests to pursue that end. There was, however, a larger goal in the committee's mind. As this portion of the *Annual Report for 1918* indicates, the committee specifically wanted to develop the means to correlate the "information gained from all sorts of tests on the ground" with flight-test data to obtain not only a validation of ground-based methods, but a synergy of laboratory and flight testing as well.

Document 2-11, "Free Flight Tests," NACA Annual Report for 1918.

Free Flight Tests

The general purpose of the work of the subcommittee on free flight tests is to obtain as complete tests as possible of the performance, in all respects, of airplanes while in the air under normal conditions. The general purpose of these tests is to supplement and make more valuable the information gained from all sorts of tests on the ground, including tests of engines and tests of airplane parts and airplane models in wind tunnels. It is obvious that the actual performance in the air, when it becomes known, is the best possible basis for future progress.

The committee now has, in a late stage of development, instruments for recording in the air the torque and revolutions per minute of the engine, the thrust of the propeller, the air speed, the angle of attack, and the inclination of the wing chord to the true horizon. It is proposed to complete this development as promptly as possible, and to get these instruments in action in the air, presumably on a D.H.4 airplane, to determine the power-plant performance and the relations in the air between the lift, drag, air speed, and angle of attack.

When such tests have been successfully demonstrated as possible, by making them, the next steps on the program of the committee are to analyze the results and show what conclusions can be drawn from them.

The committee then proposes, in due time, to extend the free-flight tests to such quantities as will help to develop the stability characteristics of airplanes, possibly to furnish some information as to the stresses in various parts of an airplane in operation.

To secure the necessary degree of accuracy and reliability in free-flight observations, the new instruments have in each case been so designed as to give a continuous autographic record.

Document 2-12(a–b)

(a) Edward P. Warner, F. H. Norton, and C. M. Hebbert, *The Design of Wind Tunnels and Wind Tunnel Propellers*, NACA Technical Report No. 73 (Washington, DC, 1919).

(b) F. H. Norton and Edward P. Warner, *The Design of Wind Tunnels and Wind Tunnel Propellers, II*, NACA Technical Report No. 98 (Washington, DC, 1920).

Although American aeronautical researchers usually chose to pursue a more empirically based path in designing wind tunnels than did their German counterparts, they were nevertheless interested in a thorough understanding of these machines and the aerodynamic phenomena they could produce. During the NACA's early years, Frederick Norton and Edward Warner led an investigation at Langley to determine the key aspects of wind tunnel design that engineers could use with confidence. Their report, *The Design of Wind Tunnels and Wind Tunnel Propellers*, published in two parts, clearly reveals the empirical approach to design optimization employed by the Americans for many years, and it shows the limited state of theoretical aerodynamics in the United States at the time. The publication of these two technical reports also demonstrates the NACA's commitment to making useful information about experimental tools and methods as well as developments in aircraft design available to the aviation community.

Document 2-12(a), Edward P. Warner, F. H. Norton, and C. M. Hebbert, **The Design of Wind Tunnels and Wind Tunnel Propellers**, *NACA Technical Report No. 73, 1919.*

THE ELEMENTARY THEORY OF THE FLOW OF AIR THROUGH WIND TUNNELS.

If the air flowing through a wind tunnel and back through the room from the exit to the entrance of the tunnel followed Bernouilli's theorem with exactness, there would be no change in the energy, possessed by a given particle of air, except for the loss due to friction as the kinetic energy lost on issuing from the tunnel would be restored in the form of pressure energy. The power required to maintain the flow would then be

$$P=m \times h_f$$

where h_f is the head (in feet of air) lost by friction and m is the mass of air flowing per second. As the same mass of air must pass every point in the tunnel, the product of mean air speed by cross-section area must be a constant for its whole length, neglecting compressibility and changes in temperature during the passage. Since the major part of the frictional losses occur in the reduced section of the tunnel (provided that it is not very short and that the diffuser is not so constructed as excessively to hamper the travel of the air from the tunnel back into the room), h_f would be practically independent of the size and angle of the exit cone, and the power consumed would also be independent of these factors.

As a matter of fact, the conditions of flow are not simple enough to permit the direct application of Bernouilli's theorem. Borda has shown that the loss of energy when fluid moving at high velocity in a pipe is discharged abruptly into a large room or reservoir is equal to the kinetic energy initially possessed. The kinetic energy is not converted into pressure energy as the theory indicates that it should be, and it is therefore profitable to use an exit cone of considerable length, in order that part of the kinetic energy may be saved by conversion into the potential form before the sudden discharge into the room. The length to which it is desirable to prolong the cone is limited by the growing loss by friction within the exit cone itself. A more rapid conversion of the kinetic energy by increasing the vertex angle of the exit cone is forbidden by the unwillingness of the air to change its course suddenly and follow the walls of the exit cone. If the vertex angle be made too large the effect is almost the same as that of an abrupt increase in cross section. Eiffel, as the result of an elaborate theoretical and experimental research on tunnels having exit cones generated by straight lines, has come to the conclusion that the vertex angle of the exit cone should be not more than 7°, and that the diameter at the large end of the exit cone should be three times that at the small end. It is necessary to base the dimensions of a tunnel on a compromise, as the arrangement which would give the absolute maximum of efficiency would have to be housed in a building of prohibitive size. The overall length can be materially reduced at the cost of a slight increase in power, and the first cost of the building, depending on its dimensions, must be balanced against the cost of operation, which varies with the power of the motor and so with the efficiency of the tunnel. The relations to be observed among the various dimensions of the tunnel and the angles of the cones will be discussed more fully elsewhere. Knowing the power consumed by a tunnel, its diameter, and the speed of the air, the total losses can easily be computed for that particular speed, and the magnitude of the figure thus obtained will serve as a measure of the efficiency of operation of the tunnel. Since, however, the losses vary with the speed, they can not be compared directly for two tunnels unless they are run at the same speed. The factor most commonly used for comparisons between tunnels is the ratio of the kinetic energy possessed by the air passing through the tunnel in unit time to the work done by the motor in unit time. This is sometimes called the "over-all efficiency," but is herein alluded to as the "energy ratio." The term efficiency in this connection is misleading, as the two quantities introduced into the ratio are not directly connected, but merely happen to have the same dimensions and so to be convenient for the purpose. Furthermore, the value of the ratio is very commonly more than 1, and is sometimes very much more.

To determine the manner in which the power consumed varies with speed, and so determine the validity or otherwise of the above relation, as well as to find the relation which must be preserved among the various factors in order that geometrically similar tunnels may be strictly comparable, the Theory of Dimensions may be used. The method pursued need not be gone into in detail, as it has been described many times before, and it will suffice to summarize the results. It appears that, if the compressibility of the air and the action of gravity on it be assumed to be of negligible importance at the speeds employed, the power consumed is proportional, for geometrically similar tunnels, to the cross-section area and to the cube of the speed, provided that $^{VD}/_{v}$, where V is the air speed, D the tunnel diameter, and v the coefficient of kinematic viscosity, is maintained constant. Experiments conducted with a model tunnel at Langley Field and fully described elsewhere in this report, as well as those carried on by Durand, Castellazzi, and others, show that the "energy ratio" varies but little with changes of $^{VD}/_{v}$ and it is therefore safe to apply the results of model experiments to full-sized tunnels, even though the speeds may not be strictly in inverse ratio to the diameters. In general, the "energy ratio" increases as $^{VD}/_{v}$ increases, and it therefore requires less power to drive a tunnel than would be predicted from a direct application of the results of tests on a model of the tunnel and propeller.

The useful work done by a propeller is equal to the product of the thrust by the speed of flow of the fluid through the propeller disk. The thrust of a wind tunnel propeller is then

$$\frac{m \times h_f}{V'}$$

where V' is the speed of the air past the propeller, and this equation holds good whether Bernouilli's theorem is followed or not, so long as hf is the total loss of head from all causes.

$$m = {}^{\rho}/_{g} \times A' x V'$$

A' being the cross-section area at the propeller, and the propeller thrust is therefore equal to the weight of a column of air having a height equal to the total loss of head and a cross-sectional area equal to the disc area of the propeller. Since the power is proportional to the cube of the speed, the thrust varies as its square.

If the factors causing departures from Bernouilli's theorem are neglected, the useful work done in moving the air against friction will be, as already mentioned, independent of the degree of expansion of area in the exit cone, and so of the diameter of the propeller. Under these conditions, in fact, the advantage in respect of power consumed would rest with the short exit cone and small propeller, as the propeller efficiency is highest for a large value of the "slip function" and this is obtained by making the speed of the air through the propeller high and keeping down the diameter of the propeller. Assuming that the output of work is the same in all cases, the thrust will be inversely proportional to the speed of air through the propeller, or directly proportional to the disk area.

LAWS OF SIMILITUDE FOR WIND TUNNEL PROPELLERS.

It is obvious from a study of the Drzewiecki theory of propeller action that a series of propellers of similar blade form and width-diameter ratio, all working at the same true angle of attack, will give thrusts approximately proportional to N^2D^4, where N is the engine speed in revolutions per unit time and D the propeller diameter. This proportion can be demonstrated by the Theory of Dimensions to hold exactly true for geometrically similar propellers of perfect rigidity, but it is very nearly correct even where propellers of different pitches are concerned. It has been shown that the thrusts of a series of propellers designed to drive the same wind tunnel or geometrically similar tunnels is proportional to N^2D^4, and also to the cross-section area, which, in turn, varies as D^2. It follows from these two relations that N^2D^2 must be a constant, and the peripheral speed of the propeller required to draw air through a wind tunnel at any particular speed will therefore be quite independent of the diameter of the propeller if the power required is independent of that diameter. It follows as an obvious corollary that, if the power required is not independent of the degree of expansion in the exit cone, the peripheral speed of the propeller will be least under the same conditions as those for which the power required has its minimum value.

It is easily demonstrable that the stresses, both those due to centrifugal force and those due to bending by the air pressure, in a series of geometrically similar propellers depend only on the peripheral speed, and that they vary as the square of that quantity. There is therefore a limiting peripheral speed which can not be exceeded with safety. For wooden propellers, it is unsafe to run the peripheral speed much beyond 60,000 feet per minute, or 305 meters per second, and it is better to stay well inside this figure. In the case of an airplane or airship where large power must be taken on a single propeller the peripheral speed can be reduced by gearing down, as the engine speed decreases more rapidly than the propeller diameter increases. In the wind tunnel, it has just been shown that this is not the case, and that the peripheral speed, and so the stress, actually increases if the propeller diameter is enlarged beyond a certain point. There is then a clearly defined upper limit to the power which it is safe to apply to driving the propeller in any given wind tunnel, and therefore a limit to the maximum speed attainable. This maximum can only be raised by reducing the losses and so improving the over-all efficiency of the plant.

Since the power required to secure a given speed with a given "energy ratio" is proportional to the cross-sectional area of the tunnel, and is also proportional to VN^2D^4, the propellers in a series of tunnels of different diameters operating, at the same speed and having the same "energy ratio," all work at the same value of N^2D^2, and so of the peripheral speed. This leads to the rather astonishing conclusion that the peripheral speed necessary to produce a given air speed depends only on that air speed and on the energy ratio, and is not at all affected by the size of the tunnel or of the propeller (except indirectly, in so far as these factors have an effect on the energy ratio). For any value of the energy ratio, then, there is a limiting air speed which can not be exceeded without running the peripheral speed up beyond the limits of safety, and this speed is the same for large tunnels as for small, although the actual power consumed of course varies with the tunnel diameter. In order to realize the highest possible wind speed the power coefficient of the propeller must be made as large as possible. This can be done by using many blades and by making them of high sections set at relatively large angles of attack. If the velocities desired are too high to be obtained in this way, it will be necessary to use two or more propellers arranged in tandem, acting like a multi-stage compressor.

It has been shown that

$$P=K_1D_1{}^2V_1{}^3$$

and also that

$$P=K_2V_2N^2D_2{}^4$$

where the subscripts 1 and 2 denote, respectively, the conditions existing in the experimental chamber and at the propeller, and K_1 and K_2 are experimental constants depending on the type of tunnel and propeller. Since $D_1{}^2V_1 = D_2{}^2V^2$, if the velocity across the exit cone at the propeller is uniform, the first of these relations may be written

$$P=K_1D_2{}^2V_2V_1{}^2$$

Dividing this by the second of the relations above,

$$K_2N^2D_2{}^2=K_1V_1{}^2$$

and

[illegible]

The ratio of the air speed to the peripheral speed is thus a constant for a given tunnel and its value for any particular tunnel depends only on the type of installation—not at all on its size. Values of V_1/ND_2 for a few tunnels are tabulated herewith:

Name.	V_1(m./sec.).	N(r.p.s.).	D_2(m).	V_1/ND_2.
Eiffel, Auteuil31.8	3.83	3.80	2.18	
Leland Stanford, Jr.	24.0	6.77	3.35	1.06
Langley Field, model	41.5	68.3	0.610	1.00
N. P. L., 4-foot	15.24	22.5	1.6761	0.40
Curtiss, 4-foot	34.5	22.92	2.44	0.62
Curtiss, 7-foot	42.8	20.00	3.66	0.58
McCook Field	221.0	29.50	1.52	4.92

[1] This tunnel was square and the ratio of V1 to V2 is therefore equal to the ratio of the cross-section areas and not to that of the squares of diameters at the minimum section and at the propeller.

Castellazzi's experiments.

Number of blades.	Blade width, diameter.	V_1(m./sec.).	N (r. p. s.).	D_2(m).	$V1/ND_2$.
24	0.0435	25.0	17.25	0.600	2.41
24	.0300	25.0	19.17	.600	2.17
16	.0650	25.0	16.67	.600	2.50
12	.0435	25.0	20.50	.600	2.03
8	.0650	25.0	19.33	.600	2.15
6	.0650	25.0	22.17	.600	1.88

It will be noted that the highest value of V_1/ND_2 in this table, with one exception, is 2.50, and this value was obtained in a tunnel of very efficient type in combination with a propeller having a total blade width equal to one-third of its circumference. Analysis by the Drzewiecki method leads to the belief that it will be possible to raise V_1/ND_2 to 3, but that this figure can hardly be exceeded with propellers resembling those now in use. The exception mentioned above, the small tunnel at McCook Field, has a fan of special type and will be discussed later.

If the allowable peripheral speed be taken as 285 meters per second, ND^2 is 90.6 meters per second. If V_1/ND_2 be assumed to be 3, the limiting value for V is 271.8 meters per second, or 607 miles an hour. This is a considerably higher speed than has yet been attained, or than is ever likely to be desired in connection with the study of aircraft. If higher speeds should be needed they can be secured either by the use of a multiplicity of propellers in series or, up to a certain point, by the use of a fan with an abnormally large hub and short blades entirely filling the periphery of the hub, as in the McCook Field tunnel, where the hub diameter is two-thirds of the total diameter. If V_1/ND_2 is raised to 5, a value only a little higher than that in the McCook Field tunnel, the limiting air speed for the peripheral speed given above is increased to 453 meters per second, or 1,012 mile an hour. ([*Footnote:*] In this analysis the change of density of the air, due to decrease of static pressure with increasing speed, is neglected. This does not lead to a very large error, as both the propeller thrust and the frictional resistance to the passage of the air increase with the air density, the former varying more rapidly than the latter.)

The assumption has so far been made that the air has a free passage across the whole area swept by the propeller. Of course the hub always blocks off a part of this area, but it has usually been an insignificant fraction. If the propeller diameter is n times the hub diameter, the proportion of the area blocked off is $\frac{1}{n^2}$, and the speed of the air across the propeller blades, assuming a uniform distribution everywhere outside the hub, is increased in the ratio $\frac{1}{1-\frac{1}{n^2}}$.

If the propeller be made, as is the common practice, with a constant blade width, and if the lift coefficient be assumed constant all along the blade, the portion of the total thrust given by the part of the blade inside of any given point is very nearly proportional to the cube of the radius at that point. For example, one eighth of the thrust would be given by the inner half of the blades if they extended clear to the center, with no hub at all. The use of a hub, or the covering up of part of the blades with a "spinner," therefore decreases the thrust in the ratio $1-\frac{1}{n^2}$. Since useful power is equal to this product of the thrust by the speed across the propeller disk, the net change in power, due to hub or spinner, is

$$\frac{1-\frac{1}{n^3}}{1-\frac{1}{n^2}} = \frac{n^3-1}{n^3-n} = 1+\frac{1}{n^2+n}$$

The increase in power coefficient by the use of a spinner, the propeller pitch being adjusted to give the same angle of attack of the blades with or without the spinner, is 5 per cent for a spinner of hub one-quarter the diameter of the propeller, 17 per cent when the ratio is one-half, and 27 per cent when, as in the McCook Field tunnel, it is two-thirds. Furthermore, the use of a very large hub makes it possible to use more blades and make their total width a larger fraction of the circumference of the circle swept by the blades. In the McCook field fan there are 24 blades, and their total width is approximately equal to the circumference of the hub.

Where very high speeds are desired, as in the calibration of air-speed meters, a throttling insert has sometimes been used to reduce the section of a large tunnel. The effect is to increase the speed, but usually much less than is expected. If the "energy ratio" remained constant, halving the diameter of the tunnel would increase the speed available with a given expenditure of power by 59 per cent. A change of this sort usually, however, diminishes the energy ratio unless the tunnel is of the type combining a long straight portion with conical ends, and permitting the extension of the cones back into the straight cylindrical part. The use of a throttling insert in a tunnel with a short experimental chamber, like those used by Eiffel and Crocco, is almost certain to lead to a large drop in energy ratio, and the increase of speed by halving the diameter in such a laboratory would probably be less [than] 50 per cent. Furthermore, it is necessary for best results that the propeller ordinarily used be replaced by one especially designed for use in conjunction with the throttling insert. If the diameter of the tunnel be halved the area at the smallest section is divided by four, and, even with an increase of 59 per cent in speed at the throat or in the experimental chamber, the speed of the air past the propeller is reduced by 60 per cent. Since the propeller diameter and its normal rotational speed to develop the rated power are unchanged, the propeller for use with the throttling insert must have a much smaller effective pitch than that employed with the full section, if the maximum of efficiency is to be obtained.

RELATIVE ADVANTAGE OF SMALL AND LARGE TUNNELS.

It has just been shown that the gain in speed by reducing the diameters by the use of throttling insert is disappointingly small. This leads naturally to a study of the best size of wind tunnel to be employed, and of the relation between speed and size which should be sought.

In the construction of aerodynamical laboratories, as the attempt has been made to approach ever more nearly to full-flight conditions, two divergent schools of practice have grown up. The first, best represented by the National Physical Laboratory in England, has constantly increased the diameter of the wind stream, and so increased the size of model which may be tested, but has remained content with relatively moderate wind speeds. The second, on the other hand, has concentrated its efforts on the pumping of the air across a small section at enormous velocity.

In comparing the merits of the high speed and the large diameter tunnels, there are three points which must be borne in mind. In the first place, the highest possible value of LV (LV being the criterion of dynamic similarity) is to be obtained with a minimum expenditure of power. Secondly, the interference between the model and its support is to be reduced to a minimum, and, finally, that disposition should be favored which enables us to secure the greatest accuracy in the construction of the models.

It has been shown that

$$P=KAV^3+K_1D^2V^3$$

where D is the diameter of the tunnel and K_1 is a constant.

In order to avoid interference between the model and the walls of the tunnel, the ratio of maximum span to tunnel diameter must not exceed a certain value (usually about 0.4). Setting L, the span of the model, proportional to D, we can then modify the above equation:

[illegible]

The power required to drive the fan will therefore be least, for any given value of *LV*, in that tunnel where the diameter is largest and the speed is smallest.

The relative magnitude of the interference between the model and its support, the so-called "spindle effect," depends on the ratio of the spindle diameter to the linear dimension of the model. Its reduction is a matter of very vital importance, the spindle correction undoubtedly being the largest single source of error in most wind-tunnel tests.

The bending moment in the spindle at any point (say one chord length from the wing tip) is proportional to the product of the span by the force acting on the model.

$$M=C_1LF=C_2L(\mathrm{L}^2\mathrm{V}^2)=\mathrm{C}_2\mathrm{L}^3\mathrm{V}^2$$

If *d* is the diameter of the spindle, the relations between the bending moment, fiber stress, and deflection may be written:

[illegible]

[illegible]

if the material of the spindle be the same in all cases.

If the maximum fiber stress be limited to a definite value,

[illegible]

[illegible]

The ratio of spindle diameter to model size, and consequently the spindle interference, will therefore be greatest in the high-speed, small diameter tunnel.

If, as is usually the case, it is stiffness and not strength which prescribes the diameter of the spindle, and if the deflection be limited to a determined value, the required spindle size is given by the equation:

[illegible]

[illegible]

For a given value of LV, then, $^{d}\!/_{L}$ will be least when the speed is low and the tunnel diameter large. The advantage of the large tunnel on this score is even greater than appears at first, as a larger spindle deflection is permissible with a large tunnel than with a small one. In fact, the permissible deflection increases nearly as rapidly as does the tunnel diameter.

In respect of the third consideration, accuracy of construction of the model, the superiority of the large tunnel, permitting the use of a large model, is so manifest as hardly to call for discussion. A model of 3-foot span can include many parts, such as fittings and wires, which it is quite hopeless to put on one of half that size.

So far, the advantage has rested with the large diameter in every particular. It has one disadvantage in that the size and weight of the balance are much increased, longer weighing arms, heavier counterweights, and a general strengthening up of the apparatus are necessitated. Furthermore, the initial cost of the building to house a large tunnel is very high. In the writer's opinion, however, the advantages far outweigh the drawbacks, and any future development of wind tunnels for model testing should proceed along the lines of increasing the diameter rather than the speed.

All that has been said against high speeds applies, of course, only to tunnels for the testing of models. Speeds equal to the speeds of flight of airplanes are essential for the calibration of instruments.

DESIGN OF WIND TUNNEL PROPELLERS BY THE DRZEWIECKI THEORY.

It is possible, if the rate of flow of the air through a wind-tunnel propeller be known, to predict the performance of the propeller by the Drzewiecki theory. Indeed, the application of that theory to wind-tunnel propellers is rather simpler than its application to the airplane, as there is no in-draught correction to contend with. If the velocity at the minimum section of the tunnel is given, the velocity through the propeller can be computed with absolute accuracy on the assumption that the distribution across the exit cone is uniform. This assumption can only justify itself in the results of the analysis derived from it as a basis.

The best way of checking the accuracy of the analytical method of design is to apply it to a propeller already working satisfactorily. This has been done with the propeller used in the model wind-tunnel experiments described in a later section of the report. The angle of the relative wind to the plane of the propeller can be computed from the wind speed, and it is then possible, knowing the angles of blade setting, to work and find the angle of attack of each blade element. Having this, the power consumed by the propeller and its efficiency can be found in the usual way. This was done for two cases. In the first case the tunnel was of the Eiffel type, with an enlarged experimental chamber, and the calculated power checked the actual consumption within the experimental error (about 2 per cent, owing to uncertainty as to motor losses). In the second case the air stream was inclosed throughout, a cylindrical tube being carried across the experimental chamber, and the power consumed was about 15 per cent more than that calculated. It is considered that both of these tests showed a very fair check and that the use of the Drzewiecki theory for design is amply justified. The average error, both in these and in other cases which have been tried, is in the direction of underestimation of the power consumption.

In designing a propeller for a new tunnel it is necessary to make an estimate of the energy ratio, and so of the speed for a given power. If the estimate is too low, the propeller pitch will be made too low, and the propeller will work at an inefficiently small angle of attack. The speed will be higher than that estimated, but still not so high as it would be with a proper propeller. If the propeller blades are made too narrow, or if too few blades are used, the full power of the motor will not be absorbed at the rated revolutions per minute. The speed will then fail to reach the value expected for the rotational speed realized, the angle of the relative wind to the plane of the propeller will fall below the estimated value, and the angle of attack of the blade elements will become inefficiently large. Any change of this sort from the designed conditions of operation tends to correct itself, as the larger angle of attack increases the power consumed and the thrust given by the propeller. This in turn speeds up the air and brings the angle of attack to a lower value. It is for this reason that fairly satisfactory results have so frequently been secured with propellers chosen almost at random, but the best efficiencies can only be obtained with a propeller designed especially for the conditions under which it is to operate. The commonest faults in the design of wind-tunnel propellers have been either to overestimate the energy ratio for a projected tunnel or to underestimate the total blade width required for the absorption of the given power at the most efficient angle of attack. The result in both eases is to cause the blades to work at too large an angle of attack.

There is some doubt as to the manner in which the angle of attack should vary along the blades. Most wind-tunnel propellers in which the Drzewiecki sytem was used at all have been designed for a constant angle of attack, but since, as was just noted, the propellers have usually been made too small to absorb the full power of the motor, they actually work at an angle of attack larger than that desired and increasing from the tip to the root of the blade. In the design of a propeller for the Langley Field wind tunnel, the opposite disposition has been

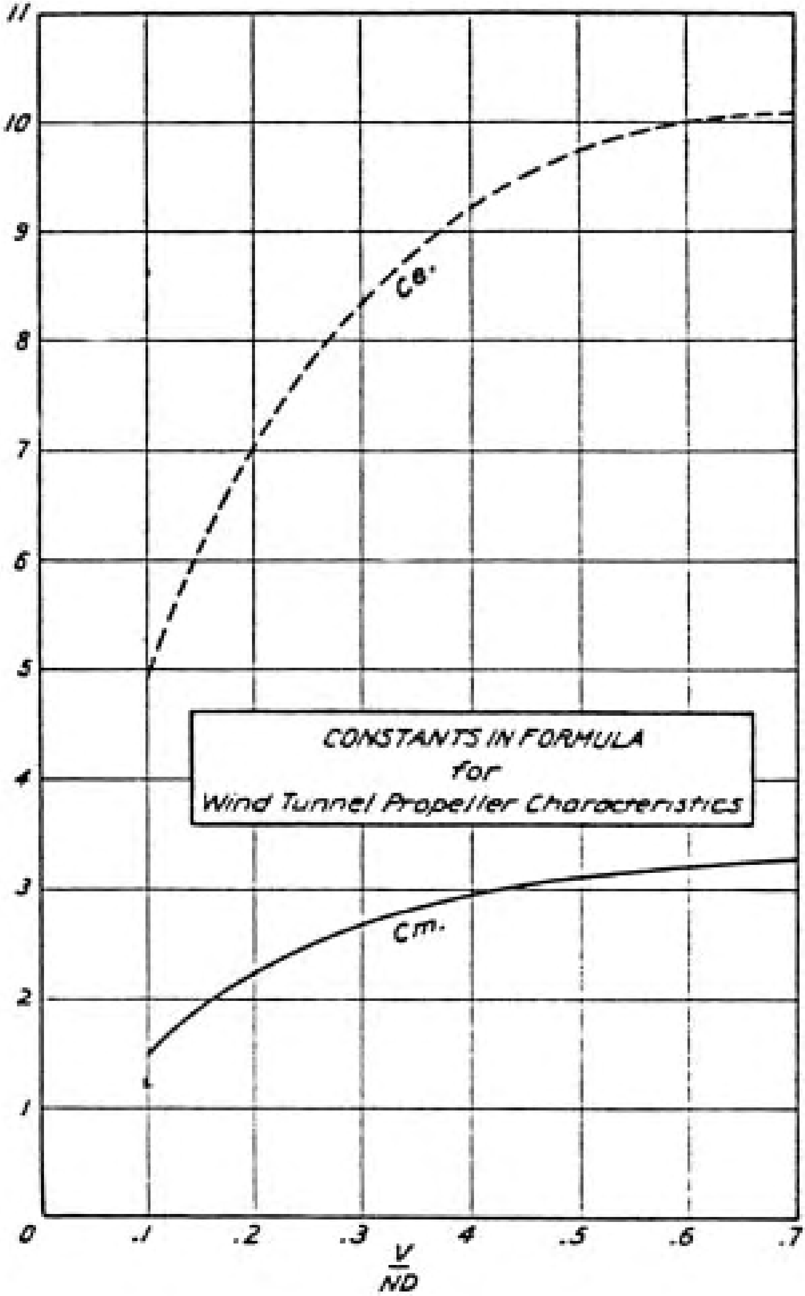

CONSTANTS IN FORMULA
for
Wind Tunnel Propeller Characteristics
CB'
Cm.
0
1
2
3
4
5
6
7
8
9
10
11
.1
.2
.3
4
.5
.6
.7
V/ND

FIG. 1.

deliberately chosen, the angle of attack being made largest near the tips and decreased toward the hub in order that the air may be drawn out along the sides of the exit cone and in order that the larger part of the thrust may come on the most efficient portion of the blades. No experimental data on the effect of this arrangement of the blade sections are available as yet.

In order to make it easy to estimate the number of blades and the blade width required in a propeller for a tunnel, assuming that the wind speed, power consumption, and revolutions per minute are known, a number of propellers have been computed for a variety of conditions and the results expressed by a formula and a curve. The power is given by the formula

$$P = \frac{\text{[illegible]}}{C \times 10^8}$$

where *P* is the horsepower input of the motor, *V* the air-speed through the propeller in meters per second, *b* the blade width in centimeters, *N* the revolutions per minute, *n* the number of blades, *D* the propeller diameter in meters, and *C* a constant, the magnitude of which depends on the pitch of the propeller. *C* is plotted against $^{V_2}/_{ND_2}$ in figure 1. If English units be used, *V* being given in miles an hour, *D* in feet, and *b* in inches, a factor 10^9 replaces 10^8 in the denominator of the power formula given above, and C is given by the dotted curve in figure 1. The theoretical basis for the derivation of this formula is the same as that for a formula derived by the writer, and previously published, for the power consumption of airplane and airship propellers.

The efficiency of wind-tunnel propellers is usually very low, and the maximum attainable depends largely on the magnitude of the pitch ratio. In the propeller designed for the Langley Field tunnel the calculated efficiency is 58 per cent. In figure 2, probable propeller efficiencies have been plotted against $^{V_2}/_{ND_2}$. The efficiencies there predicted may be exceeded when the peripheral speed is low, so that thin sections can be used over the whole length of the blade, or when a very large hub or spinner is used to cover up the less efficient parts. In order to give an idea of the range of values of $^{V_2}/_{ND_2}$ employed in successful tunnels, a few are tabulated below, the data being taken from the table under "Laws of Similitude for Wind Tunnel Propellers".

	$^{V_2}/_{ND_2}$
Leland Stanford, Jr.	.0265
N. P. L., 4-foot	.27
Curtiss, 4-foot	.20
Curtiss, 7-foot	.20
Langley Field model	.25
McCook Field	.48

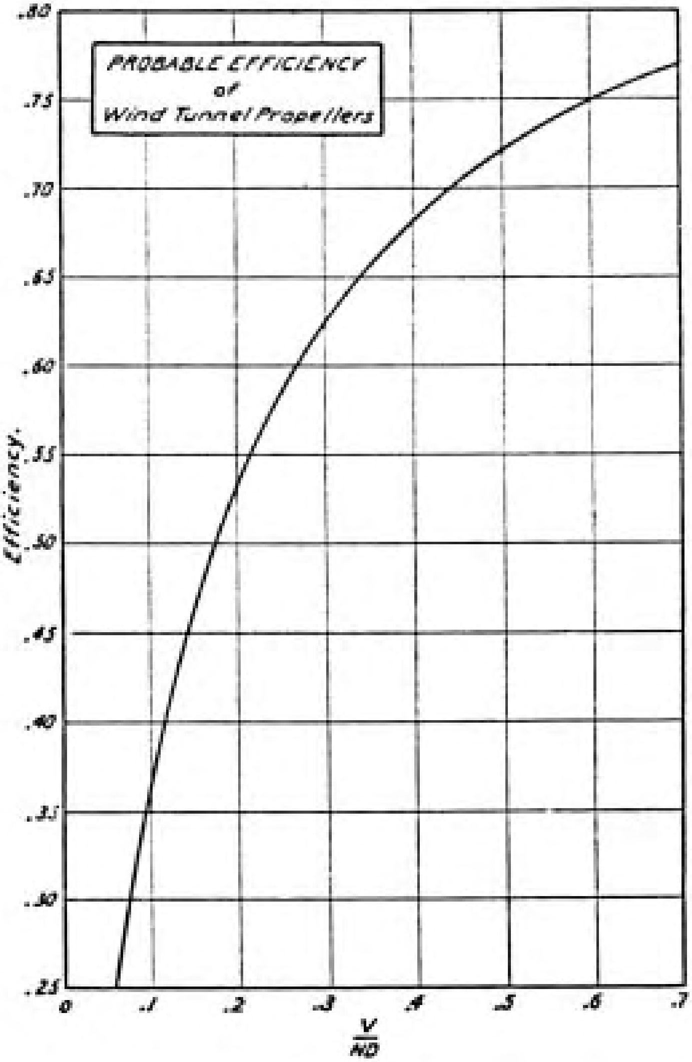


FIG. 2.

THE FORMS OF ENTRANCE AND EXIT CONES.

There has been a great deal of discussion and dispute as to the best form for the cones in which the air acquires and loses its speed, and further experiment is desirable. The effect which changes in the form of these cones have on the efficiency is, however, much less than has commonly been supposed, judging from experiments recently performed at Langley Field and reported in another part of this paper.

In the absence of data to indicate the best form, most of the wind tunnels which have been constructed have used, at least on the exit side of the experimental chamber, the frustrum of a right cone generated by a straight line. This was true of the N.P.L. and all their imitators, and it has been true also of most of the tunnels designed with an eye to the results of the experiments of Crocco and Castellazzi, and using long exit cones of very gradual slope. A surface of this type has at least the advantage of being easy to generate and to fabricate from wood or sheet metal. There is, however, no particular reason to believe that it is the most efficient that can be constructed from an aerodynamical point of view. Eiffel and his followers, on the other hand, have always used cones of curving form. It seems fair to assume that the loss in diverging nozzle is partially dependent on the deceleration of the fluid, and that the loss will usually be least where the deceleration is least. It is obvious, furthermore, that the flow through the exit cone will be smoothest and least turbulent when the form of the cone is smooth, and that any abrupt change of slope of the walls, such as that at the juncture of the parallel portion of the tunnel with an exit cone generated by a straight line, is liable to cause the lines of flow to break away from the contour of the tunnel wall, and to establish a region of "dead-water" and turbulence around the periphery of the exit cone. The smoothness of a curve can best be judged by taking differences, or, if the equation of the curve is known, by plotting the derivative. This was done in designing the cones for the Leland Stanford, Jr., tunnel. The plotting of the curve of acceleration for a tunnel will then serve the double purpose of indicating the smoothness of the curve and of giving the maximum rate at which the velocity of the air is changing, and so the maximum force necessary for accelerating the moving stream.

A curve of velocity against distance along the axis of the tunnel can be drawn on the assumption that velocity is inversely proportional to the square of the diameter of the tunnel. This, of course, is true only for velocity parallel to the axis, and entirely neglects the radial component. In order to obtain the acceleration from this curve, the derivative giving acceleration is written

$$\frac{dv}{dt} = \frac{dv}{dx} \times \frac{dx}{dt} = v \times \frac{dv}{dx}$$

The acceleration at any point along the tunnel is therefore equal to the product of the ordinate of the curve just described by its slope at that point. These factors can be found graphically or, in the case of a curve for which the equation is know, analytically.

In the case of a straight cone, for example, the formula for diameter at any point is

$$D = D_1 + (D_2 - D_1) \times \frac{x}{l}$$

where D_1 and D_2 are the diameters at the small and large ends of the cone, respectively, l the length of the cone, and x the distance from the small end. Then

[illegible]

and

[illegible]

The acceleration is equal to the product of these expressions, or

[illegible]

If the exit cone is generated by rotating about the axis of the tunnel a parabola having its vertex at the junction of the exit cone with the straight portion the formula for diameter of the cone at any point becomes

The acceleration may then be obtained by the same steps just employed for the straight cone.

[illegible]

[illegible]

[illegible]

[illegible]

In figure 3, the velocity and acceleration, as well as the cone diameter, are plotted against *x* for cones of these two forms. The units are meters and seconds, and the curves relate to a tunnel having an exit cone tapering in diameter from 1.5 meters to 3 meters in a length of 6 meters, and a wind speed of 50 meters per second. It appears that the straight cone is far inferior, judged by the criteria laid down above, to that of parabolic form. The maximum acceleration for the first is more than two and a half time that for the second, and there is a large discontinuity in the acceleration curve for the straight cone, as might be anticipated from the discontinuity in the slope of the side of the tunnel. The parabolic form gives zero acceleration at the point where the air emerges from the exit cone. There is some question as to the desirability of using a reverse curve which will have tangents parallel to the axis of the tunnel at both its ends, and so securing zero acceleration at both ends of the exit cone. The air has to be slowed down some time, and there would seem to be little advantage in bringing it to a constant velocity as it leaves the retaining walls of the exit cone if it is to be decelerated again the instant that it is free from those walls. Also, the current of air, since it is to be turned through an angle of 180° and travel back through the room to the entrance of the tunnel, must acquire a radial velocity either inside the exit cone or immediately after it has left it. No gain is apparent from a construction which permits the air to acquire a certain amount of radial velocity and then straightens it out again, only to force it to turn outward once more a few feet father along its path. The effect of a reversal in the curve of the walls near the large end of the exit cone is certainly slight, as very good results have been obtained both with and without such a reversal.

The form of the entrance cone appears to have but little effect on the "energy ration," and this is in accord with the results of hydraulic experiments, where it is always found that the loss in a converging nozzle is much less than that in a

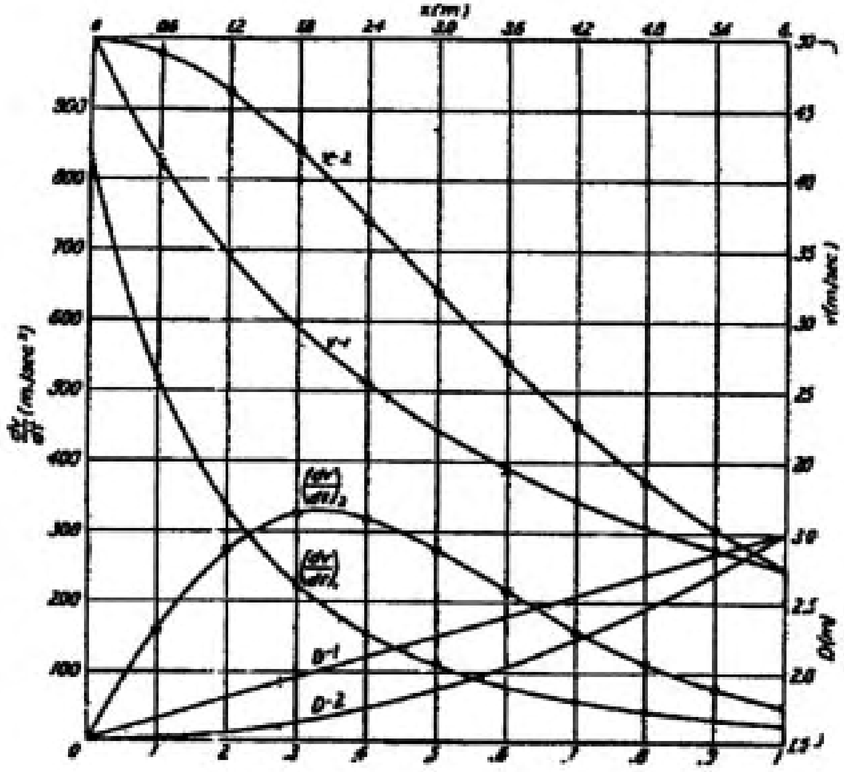


FIG. 3.—Velocities and accelerations of fluid in exit cones.

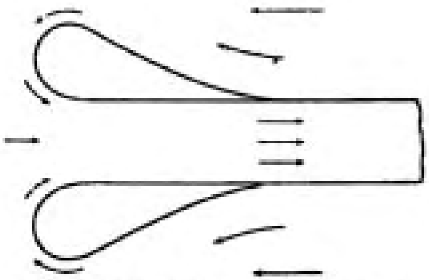


FIG. 4.—Fairing of entrance to N. P. L. tunnel.

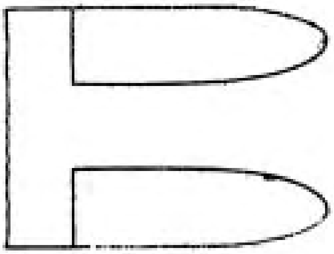


FIG. 5.—Proposed fairing of entrance cone.

diverging one, and that the nozzle can converge very abruptly without seriously increasing the loss.

Most of the European experiments on model tunnels have been made with straight entrance cones. While these are probably as efficient as any other type, they must have avena contracta near the large end, causing turbulence which persists into the experimental chamber, and there is further eddying and disturbance due to the turning of the air around a sharp corner at the small end of the cone. To avoid these difficulties and to secure as steady a flow as possible in the experimental chamber it is the almost universal practice, in actual tunnels, to make the entrance cone of curing form. It has been found at the National Physical Laboratory that even if the entrance cone, or bell-mouth, as it is called there, is curved around until a tangent to the wall at the large end is perpendicular to the axis of the tunnel, there still are marked and persistent eddies in the neighbor-

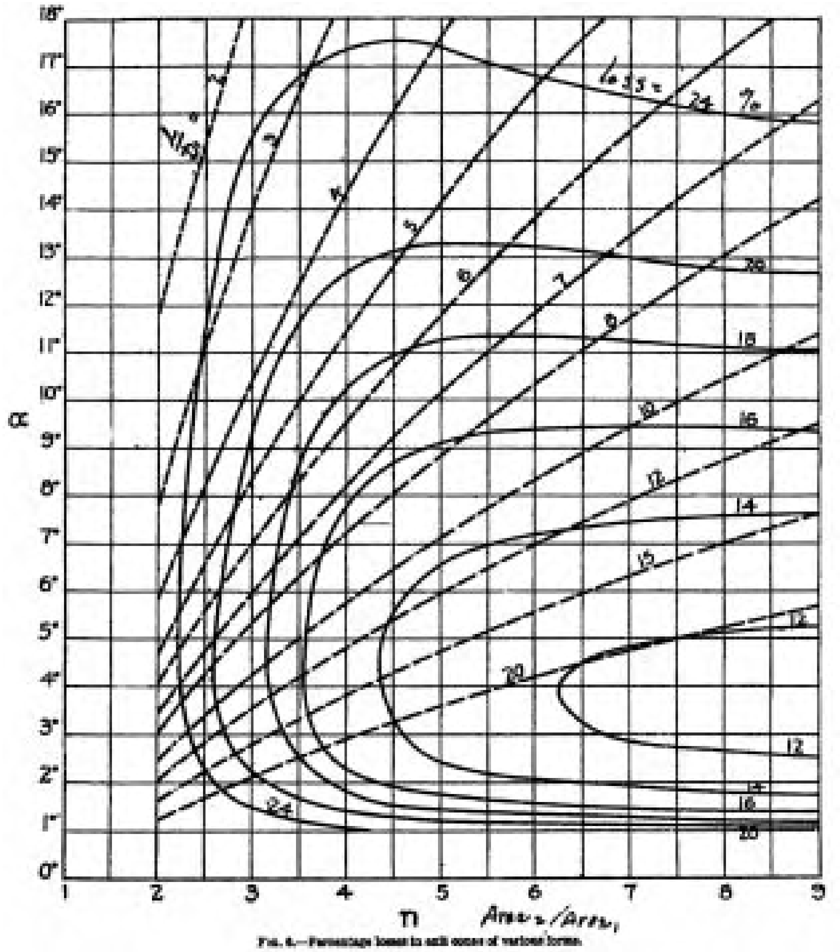


FIG. 4.—Percentage losses in exit cones of various forms.

hood of the sharp edge. To entirely eliminate this edge it is not the practice at the N.P.L. to carry the bell-mouth around, as shown in figure 4, until it meets the straight portion of the tunnel. This method has not been adopted at Langley Field, as it is desired to make some experiments on the full-sized tunnel with the normal entrance cone, but provision has been made for building a fairing to extend clear around to the experimental chamber, as shown by the dotted lines in figure 5, so giving the air a perfectly smooth passage.

THE THEORY OF LOSSES IN THE EXIT CONE.

The losses in the exit cone of a wind tunnel arise from three sources. The first is the friction against the walls, and is best determined by Fritzsche's formula for fluid friction. The second is the diverging angle of the cone, which, as already noted, always leads to a loss of energy as compared with the ideal conditions expressed by Bernouilli's theorem. The magnitude of this loss is determined with satisfactory accuracy by a formula devised by Fliegner. Finally, there is a loss due to the sudden release of the air from the exit cone and its passage into the room, where its velocity drops almost to zero. This loss was shown by Borda to be equal to the kinetic energy possessed by the air at the large end of the cone. These losses, and their relation to the factors entering into wind tunnel design, together with all the losses in other parts of the tunnel, have been fully addressed by Eiffel, and it is not necessary to repeat his work here. For the benefit of those designing tunnels, however, a set of curves has been plotted which make it possible to read off at once the loss in a straight conical exit cone of any type, and to determine, given the limiting conditions, such as the size of a building to house the tunnel, the characteristics of the best exit cone of that particular case. Since from 80 per cent to 90 per cent of the total losses in a tunnel (not including those in the propeller) occur in the exit cone, the problem of designing a tunnel with a high energy ratio is essentially a problem of reducing the losses in the exit cone.

In figure 6 the ordinates are the vertex angles of exit cones, the abscissae, the ratio of the cross-section area at the large end of the cone to the cross-section area where models are tested, at the throat or in the experimental chamber. The family of curves drawn in full lines are curves of equal loss, and the number which one bears expresses the loss in the exit cone as a per centage of the kinetic energy possessed by the air at the smallest section of the tunnel. For example, if there were no losses except those in the exit cone, a tunnel having an exit cone of form corresponding to any point on the curve marked 20 would have an energy ratio of 5. The nearly straight dotted lines running across the sheet diagonally correspond to various constant lengths of exit cone, and they are marked with the ratio of length to diameter at the small end.

To illustrate the use of this chart in choosing an exit cone a few illustrative examples will be given.

1. A tunnel is to be 2 meters in diameter. In order to keep the size and cost of the building within reasonable limits, it is desired that the length of the exit cone shall not exceed 20 meters. Subject to this limitation, the cone is to be chosen for maximum efficiency.

The ratio of length to diameter here is 10. Passing along the dotted line bearing that number, it is seen that it cuts the curve of 16 per cent loss at two points and that it does not cut the 14 per cent curve at all, but that it approaches nearest to the latter at the point ($\alpha = 6.8$ degrees, $n = 4.8$). It is usually best to make n a little smaller than the value for the minimum loss in the exit cone, as a reduction in n in the diameter at the large end of the cone and so in the propeller diameter, and it has already been shown that this is favorable to propeller efficiency. It would probably be best, in this case, to take $n = 4.3$, $\alpha = 6.1$ degrees, or some other combination in that immediate neighborhood.

2. A very large wind tunnel is to be built, and, in order that the propeller diameter may not be unreasonably large, as well as to keep down the height of the building, the propeller diameter is limited to twice the diameter of the tunnel at the minimum section.

If the ratio of diameters at the ends of the exit cone is 2, $n = 4$. Drawing a vertical from the scale of abscissae at this point, it is seen that it approaches nearest to the 14 per cent curve at ($\alpha = 4.5$ degrees). The length of the exit cone for this angle is 13 times the minimum diameter. It would not be advisable, under these conditions, to choose the cone for the absolute maximum efficiency, as the length could be decreased 4 ½ diameters at a cost of only 5 per cent increase in total power by increasing α to 6.7 degrees. Since the curvature of the constant power curves is not abrupt, the conditions can be changed considerably from those for minimum loss without very much affecting the efficiency, and it is almost always worthwhile to make some concession of efficiency in order to reduce the dimensions of the building and of the tunnel itself.

EXPERIMENTS ON MODEL WIND TUNNELS.

The first set of experiments conducted dealt with a model of the wind tunnel for Langley Field, as it was originally planned. The tunnel was of the Eiffel type, with a large experimental chamber, and this chamber was reproduced to the proper scale in the model. All of the models used were one-fifth the size of the large tunnel, the experimental chamber being 30.5 cm. in diameter in the models. The entrance and exit cones were made of plaster over a base of wall board, and were shellacked, so that a very smooth surface was secured. The plaster was scraped to form, as soon as it is set, with a steel template rotated about a shaft running along the axis of the tunnel. An exit cone is shown, with the template in place and ready to apply the plaster, in figure 7. The drive was by belt from a 2-horsepower induction motor, and the propeller was four-bladed. The blade had

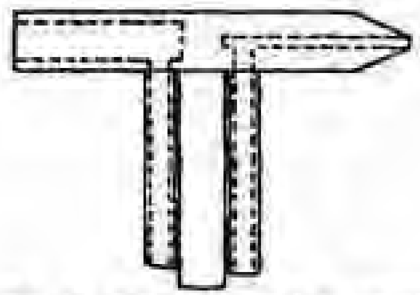


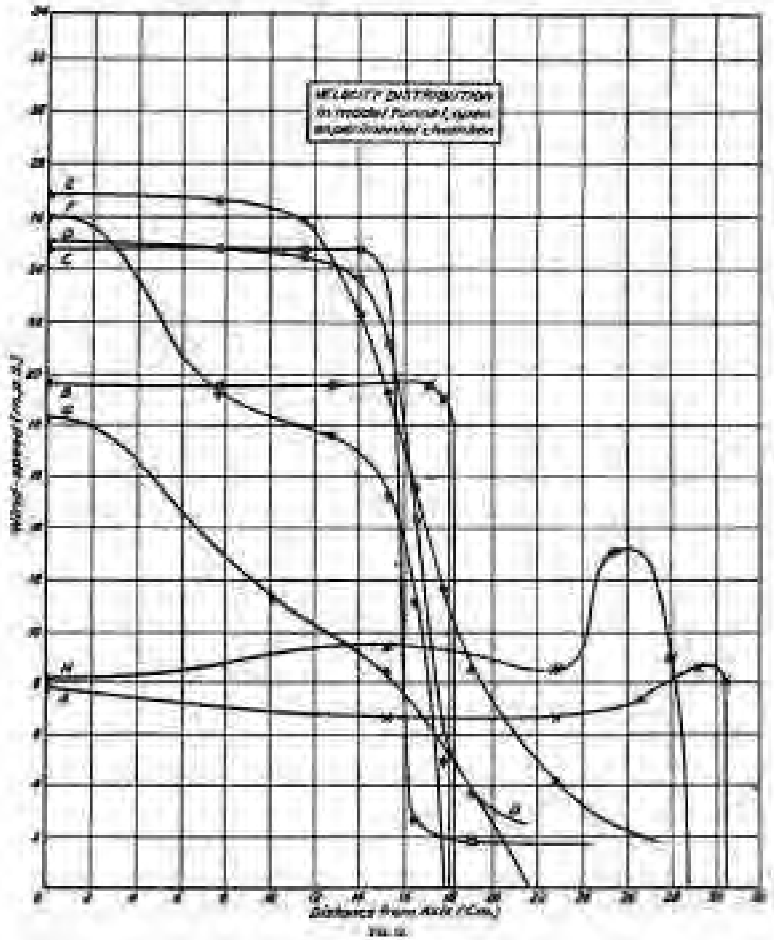
FIG. 11.—Pitot tube used in experiments with model tunnels.

a constant width of 4.5 cm. The speed of the propeller was measured with a Veeder liquid tachometer, and the power consumption with a polyphase watt-meter. The tunnel and instruments ready for use are shown from two points of view in figures 8 and 9, the propeller in figure 10. Figure 11 illustrates the Pitot tube used for measuring the wind speed. A hole 1 mm. in diameter is bored in the tapering end, and communicates with one of the two hypodermic tubes passing down the shank. The static pressure is secured inside a hole 2.5 mm. in diameter drilled from the other end of the tube, and this hole communicates with the other hypodermic tube. The piece between the two hypodermic tubes is a solid rod to provide stiffness. Since the static pressure points to the rear, the pressure in that side of the gauge is less than the true static, and the readings are higher than they theoretically should be. The tube was calibrated against a standard Pitot in the wind tunnel of the Bureau of Standards, and was found to have a constant of 1.167 (i.e., the readings of the small Pitot tube were 16.7 per cent higher than they theoretically should have been). This Pitot tube was very insensitive to rotations in all planes, as it could be turned 20 degrees without affecting the reading more than 8 per cent. This is a great advantage where, as in traversing the cones, the direction of flow of air is uncertain. The dynamic head on the Pitot tube was measured by an alcohol gauge, shown in figure 12. Only one side of the gauge is ordinarily used. Since the glass tube is raised and lowered by a micrometer screw so that the meniscus of the alcohol stands opposite the same mark on the tube for each reading there are no corrections, such as are required in the ordinary Krell manometer, for varying diameter of the glass tube or for changing level of the fluid in the reservoir.

The mode of procedure in each complete test was to make traverses of the entrance and exit cones and the experimental chamber at several points, measuring the wind speed at several radii, and then to make runs at a number of different speeds, measuring the wind speed at the point where a model would be placed for test and the power consumption. The energy ratio and the manner of its variation with speed was determined from this set of runs.

The traverses for the original model are plotted in figure 13. The points *A* and *B* were in the entrance cone, *A* being at the large end of the cone, *B* midway between the ends. *C* was in the experimental chamber, 5 cm. from the entrance cone side. *D* was 20 cm. downstream from the entrance cone and E was 46 cm. from the entrance cone, 15 cm. from the exit. *F, G,* and *H* were in the exit cone, and were equally spaced along its length, *H* being in front of the propeller and as close as it could be placed without danger of having the tube struck by the blades. The exit cone in this model was parabolic in form. The location of the point *D* corresponded to that at which the model is to be placed in the full-sized tunnel.

The speed in the entrance cone had a maximum at the center and one near the wall, the one near the wall being higher than that in the center. The maximum occurred with 2 cm. of the wall. On going still farther out the speed dropped rapidly, due to friction. The velocity in the experimental chamber near the entrance cone was constant, as nearly as could be detected, over 90 per cent of the diameter of the stream. On going farther downstream the velocity distribution became more irregular, the speed being a maximum at the center and dropping off steadily toward the edges of the stream. The ratio of the velocity 75 per cent of the way out to the edge of the stream to that at the center was 1.00 at C, 0.97 at *D*, and



0.96 at *E*. The edge of the stream was not sharply defined, even very near to the point of issuance from the entrance cone, and at E, three-quarters of the way across the chamber, the velocity dropped off in a smooth curve from very near the center of the stream out to far beyond its normal boundaries.

The velocity distribution in the exit cone was exceedingly strange. The velocity dropped off rapidly from a maximum at the center, so that the stream appears actually to contract rather than to expand in the exit cone. As in the entrance cone, there was another maximum near the wall, but it was farther from the wall than was that at the entrance, and the velocity was much lower than at the center. Directly in front of the propeller the velocity at the center dropped sharply, due to the hub, and varied in an irregular manner over the rest of the section. The flow at this point was so turbulent and so varying in direction that the measurements of velocity may contain considerable errors.

In order to make direct observations on the sharpness of definition of the edge of the stream in the experimental chamber and to determine the general nature of the flow in the chamber an observer got inside and sounded the flow with a thread. It was evident that the air in the whole chamber was much stirred up, and that the flow near the nominal edge of the stream was extremely turbulent, except in the immediate neighborhood of the entrance. Even in the farthest corners of the chamber, at a distance from the center of the stream equal to more than three times its nominal diameter, there was still a distinct movement of the air. The motion everywhere was very unsteady, the direction of flow at a given point changing 60 degrees or more almost instantaneously. The best defined part of the circulation was near the small end of the exit cone, where two strong vortices rotating in opposite directions existed in the corners of the chamber. The examination of the flow was not extended to points above and below the stream in this neighborhood, so it is not certain whether or not a complete vortex ring, surrounding the opening into the exit cone, existed. The results of this examination of the flow in the experimental chamber made it clear that the balance would have to be shielded in some way from the air currents if any accurate work was to be done. In Eiffel's tunnel, partial shielding of the balance is accomplished by placing it on a platform which, however, extends across only a small proportion of the width of the room, and can hardly act as a complete protection from air-currents for the measuring instruments.

The power curve is plotted in figure 14 (curve No. 1) and the curve of speed against revolutions per minute in figure 15 (curve No. 1). The energy ratio varied too little and too irregularly to make it worthwhile to plot a curve. Its mean value was 0.90, making no allowance for propeller losses. If the propeller efficiency be assumed to be 57 per cent (the value calculated by the Drzewiecki method), the energy ratio for the tunnel proper becomes 1.58.

In view of the irregularities of flow found in the experimental chamber it was decided to try next the effect of inclosing the stream in a cylindrical tube during its passage across the experimental chamber. No attempt was made to make the tube air-tight, the static pressure inside the tube being equal to that in the experimental chamber, which was carefully made air-tight. Curve No. 2 in figure 14, and also in figure 15, correspond to this case, and the traverse of the stream at points corresponding with those taken for the original model are plotted in figure 16.

Comparing these traverses with those in figure 13, it is seen that the nature of the distribution in the entrance cone is practically unaffected. The velocity at point *C* was a little less regular than for the case of the unconstrained stream, showing an increase near the walls similar to that which characterized the entrance cone. At *D* and *E*, however, the velocity was much more even with the inclosing tube than without it, being constant within 1 per cent over 75 per cent of the diameter. Evidently, from the standpoint of steadiness of flow, the inclosed type of tunnel is superior to the Eiffel type.

In the exit cone the effect of surrounding the stream with a definite boundary was still more apparent. At *F* the velocity three-quarters of the way from the center to the walls was 94 per cent of that at the center, as against 67 per cent in the original model. At *G* the corresponding figures were 82 per cent and 40 per cent. At *H* there was, as in the first case, a minimum at the center and two maximums, the distribution of velocity being reasonably uniform across the outer 70 per cent of the blade, which is the most effective portion.

It is reasonable to suppose, in view of the better filling of the exit cone and of the generally improved velocity distribution, that the energy ratio would be increased by inclosing the stream, and this supposition was fully justified by the power measurements. For a given rate of rotation of the propeller the wind speed was increased while the power consumption for a given wind speed was decreased just about 50 percent. The energy ratio with the inclosing tube was 1.83 for the whole installation, or, making due allowance for the propeller losses, 3.20 for the tunnel alone.

It is evident that the enclosure of the stream improves the results in every way. The experiments, so far as power consumption is concerned, check very well with those obtained in some similar experiments on model tunnels, carried out by Lieut. Castellazzi. Lieut. Castellazzi found that the efficiency was decreased 40 per cent by the use of an open experimental chamber. The experimental chamber used in his experiments was round in cross section and was twice as large in diameter as the entrance and exit cones where they entered the chamber, and the slightly greater loss in efficiency found in the experiments conducted at Langley Field may be accounted for by the larger size and more irregular form of the experimental chamber there employed.

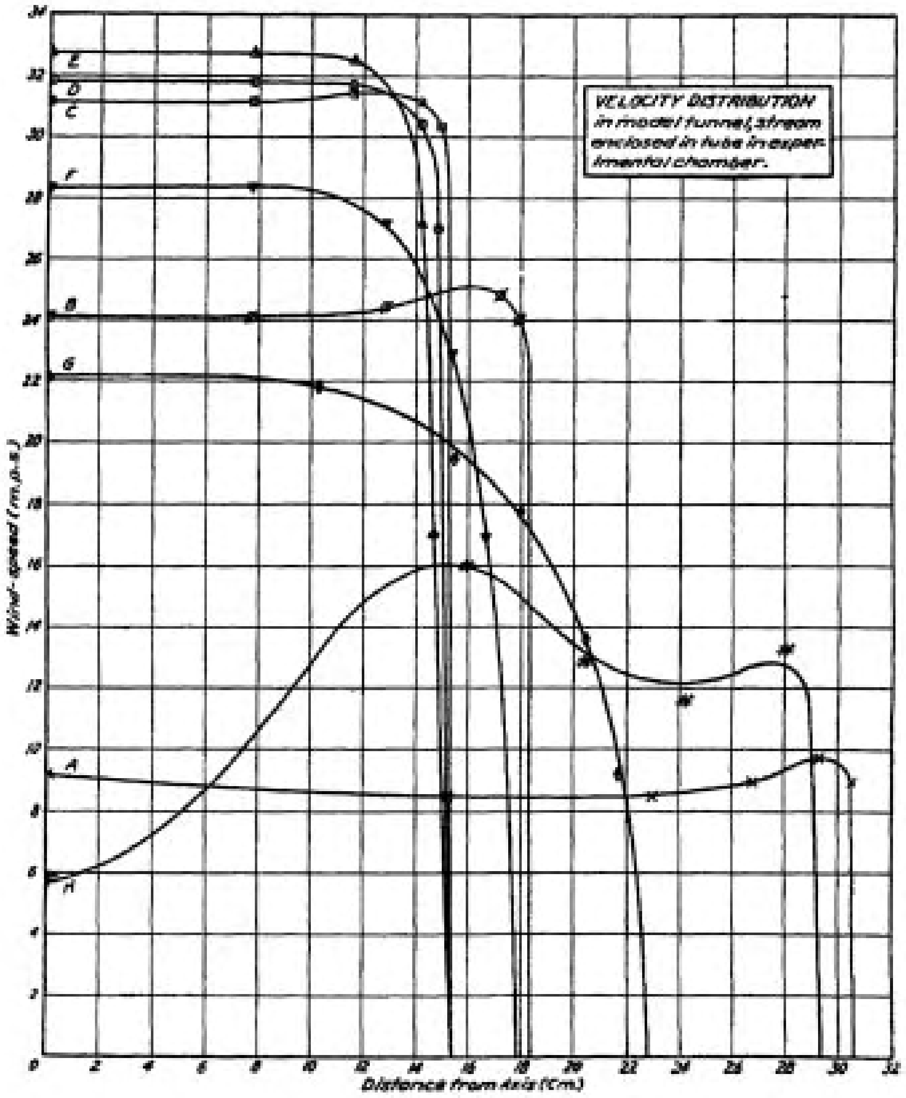


Fig. 18.

EFFECTS OF VARIATION IN EXIT CONE FORM.

The next series of experiments dealt with the effect of alterations in exit cone form. It was originally the intention to make a number of cones of different forms, but this plan was abandoned after two had been tried, and the experiments cover only the parabolic and straight forms of cone. These are as widely different from each other in respect of their acceleration curves as are any two forms which would be likely to be used.

The curves of power and revolutions per minute with the straight cone are plotted as curve No. 3 in figures 14 and 15. The mean energy ratio is 1.83 for the combination of tunnel and propeller, or 3.20 for the tunnel alone, values identical with those for the parabolic cone. It is evident from the curves that the effect of changing the exit cone from a parabolic to a straight form was very slight. The parabolic form seems to have a slight advantage at high values of VD and to be inferior at low values, but the difference between the two curves is in no case in excess of the possible experimental error. In view of these results it appears that the efficiency of a tunnel is not affected appreciably by exit cone form or by the nature of the acceleration in the cone, but only by its length, mean angle, and total expansion ratio.

The large acceleration suddenly imposed on the air at the juncture between the parallel-sided portion of the tunnel and a straight exit cone might be expected to cause turbulence, so that the flow would be less regular than with a parabolic or other smoothly curving form. No experimental data are available on this point as yet, as the experiments were temporarily halted by an accident to the propeller before traverses and investigations of the flow had been carried out with the straight cone.

OBSERVATIONS OF THE NATURE OF THE FLOW THROUGH THE PROPELLER.

The most noticeable feature of the flow behind the propeller is the great rapidity with which the slip stream spreads. Instead of contracting, as in the case of an airplane propeller, where the direction of inflow is unrestricted, the stream expands immediately on passing clear of the cone, the air changing its direction so that there is a strong movement of the air, in a direction approximately at right angles to the axis of the tunnel, at a distance of 30 cm. back and 50 cm. out radially from the edge of the exit cone.

The flow in the throat and cones was very steady at all points except near the edges of the stream. The velocity head varied with a total amplitude of oscillation of about 2 per cent of the head and a period of from 20 to 40 seconds. On passing the propeller the pulsations of velocity became much more marked. The period of the pulsations close behind the propeller was about half a second, and the maximum velocity was estimated to be about 50 per cent greater than the minimum, although no means of measuring and making a continuous record of a rapidly varying velocity were available. On going farther away from the propeller along the lines of flow of the air, the pulsations steadily increased in violence and the period lengthened, until at a distance of about 80 cm. to the rear of the propeller, the flow consisted of a violent gust about every second, the velocity in the intervals between these gusts being so low as to be hardly perceptible. These observations on the nature of the flow and its variations held in a general way for all the models tried, but the pulsations of velocity were much more marked for the case where the experimental chamber was left open than for that where it was inclosed in a tube.

EXPERIMENTS ON THE EFFECT OF DISKS AND SPINNERS ON THE PROPELLER.

In order to secure some idea of the effect of enlarging the hub of a propeller or of attaching a spinner, some experiments were made with disks of wall board attached in front of and behind the propeller, and also with a paper cone projecting from the propeller into the exit cone. The results of these tests do not fairly represent what might be secured with a good spinner and a propeller especially designed for it, as the propeller pitch should be increased when a spinner is incorporated or the hub is enlarged, but they will give some idea of the effect.

The effect of placing a disk in front of the central portion of the propeller, the rear not being covered and the blades not being housed in any way, was to decrease the wind speed and increase the power consumption. The inner parts of the blades acted as a centrifugal blower, taking air in from the rear and throwing it our radially. The increase in power, with a disk half the diameter of the propeller, was 9 per cent, the decrease of speed with the same disk 19 per cent. With a disk only one-fifth the diameter of the propeller the speed was decreased 5 per cent. These measurements were made at a speed of 10 meters per second and with the parabolic exit cone. The relative loss by the addition of a disk was greater with the straight cone and at high speeds, the addition of a disk four-tenths the diameter of the propeller causing an increase of 28 per cent in power and a decrease of 19 per cent in speed at a speed of 34 meters a second with the straight exit cone. The energy ratio was decreased 59 per cent. All subsequent tests were made with the straight cone, and the losses would probably be less with other forms.

The addition of another disk of equal size behind the propeller, so preventing any flow in from the rear and out toward the tips, improved the performance as compared with the single disk in front of the propeller, but remained inferior to the original case with no shielding at all. The power was increased only 6 per cent as compared with the original case without nay disks, but the speed was decreased 16 per cent and the energy ratio fell off 44 per cent. When the rear disk alone was in place, so that any air thrown radially outward had to come from inside the exit cone, the power was increased 6 per cent, the velocity decreased 5 per cent, and the energy ratio decreased 19 per cent, using the model without disks as a standard in all cases. The disk behind the propeller therefore gave better results than did complete sheathing, either in the form of disks or faired by a cone in front.

The addition of a cone, having a diameter equal to two-fifths the diameter of the exit cone at its large end and an altitude of one and a quarter times its own diameter, in front of the propeller decreased the power about 2 per cent and increased the speed 7 per cent as compared with the values for the disks alone, but the energy ratio was still 30 per cent lower than for the original case. It seems strange at first that the entire blocking off of a considerable portion of the blades should increase the power consumption for a given number of revolutions per minute, but the phenomenon can be accounted for by the higher air speed past the propeller when the area of the exit cone is constricted by enlarging the hub. The theory of the effect of an enlarged hub or spinner has been discussed in another section of this report.

It appears that the addition of a spinner or the enlargement of the hub caused serious loss in every case where it was tried with the straight cone. The loss with a parabolic cone is much less, and it is likely that, with a propeller properly designed to allow for the increased velocity due to the blocking off of part of the area of the exit cone by the spinner, results as good as those in the original case could be obtained. It may even be that they could be materially improved on, but this does not seem very probable in view of the uniformly poor results shown in these experiments, where the presence of the spinner can hardly have decreased the propeller efficiency more than 10 per cent (a loss which, as already noted, could be prevented by the adoption of a propeller designed especially for the new conditions). The loss in propeller efficiency, therefore, would not be sufficient entirely to account for the decrease of energy ratio. The principal value of a very large hub is to increase the power coefficient of the propeller and make possible the reduction of the peripheral speed for a given wind speed.

Document 2-12(b), F. H. Norton and Edward P. Warner, The Design of Wind Tunnels and Wind Tunnel Propellers, II, *NACA Technical Report No. 98, 1920.*

SUMMARY.

This report is a continuation of National Advisory Committee for Aeronautics Report No. 73, and was undertaken at the Langley Memorial Aeronautical Laboratory for the purpose of supplying further data to the designer of wind tunnels. Particular emphasis was placed on the study of directional variation in the wind stream. For this purpose a recording yawmeter, which could also be used as an air speed meter, was developed, and gave very satisfactory results. It is regrettable that the voltage supplied to the driving motor was not very constant, due to varying loads on the line, but as this motor was of a lightly loaded induction type, the variation in speed was not as large as the variation in voltage. The work was carried on both in a 1-foot model and the 5-foot full-sized tunnel, and wherever possible a comparison was made between them. It was found that placing radial vanes directly before the propeller actually increased the efficiency of the tunnel to a considerable extent. The placing of a honeycomb at the mouth of the experimental portion was of the greatest aid in improving the flow, but, of course, somewhat reduced the efficiency. Several types of diffusers were tried in the return air, but only slight improvement resulted in the steadiness of flow, they not being nearly as effective as the honeycomb.

APPARATUS.

The efficiency of the tunnel and the slip of the propeller were determined by the same method as described in Report No. 73, but to better record the fluctuations in velocity and direction a recording instrument was constructed. This instrument, as shown in Figs. 1 and 2, consists of a thin mica diaphragm whose movement rotates a very light spindle containing a small silvered mirror. Light from an illuminated slit is transmitted by a lens to this mirror and the reflected beam is then focused on a moving photographic film so that any movement of the mica diaphragm is recorded as a continuous curve. By this method any small and rapid variation in the air flow of the tunnel is indicated and recorded by means of a Pitot-static tube which is connected to the two compartments separated by the diaphragm, and any change in direction is recorded in the same way by connecting the sides of a yawhead to the compartments on opposite sides of the mica diaphragm. The Pitot and the connecting tubes are made comparatively large so that any rapid fluctuation in velocity can be immediately transmitted to the diaphragm without damping or lag. Over 50 records were taken but only a few

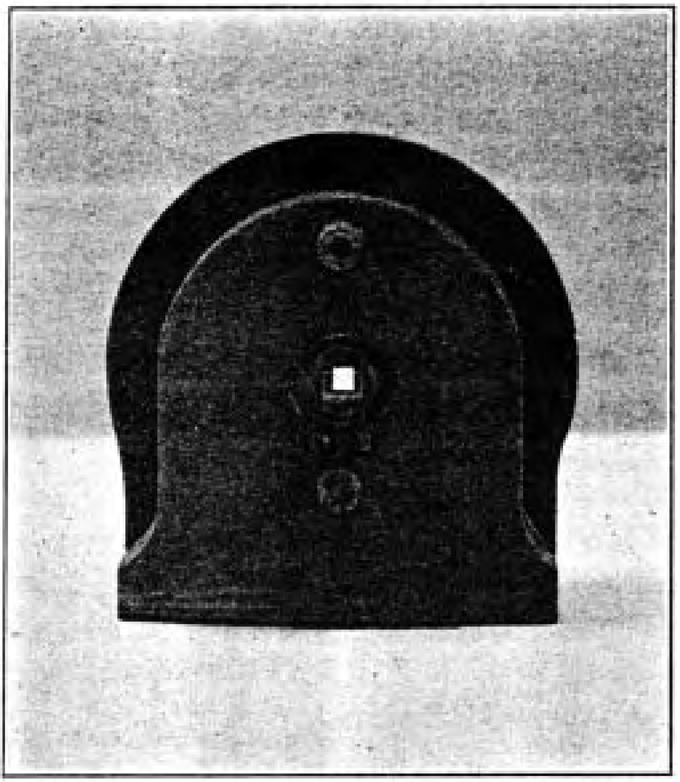


FIG. 1.—RECORDING AIR SPEED METER.

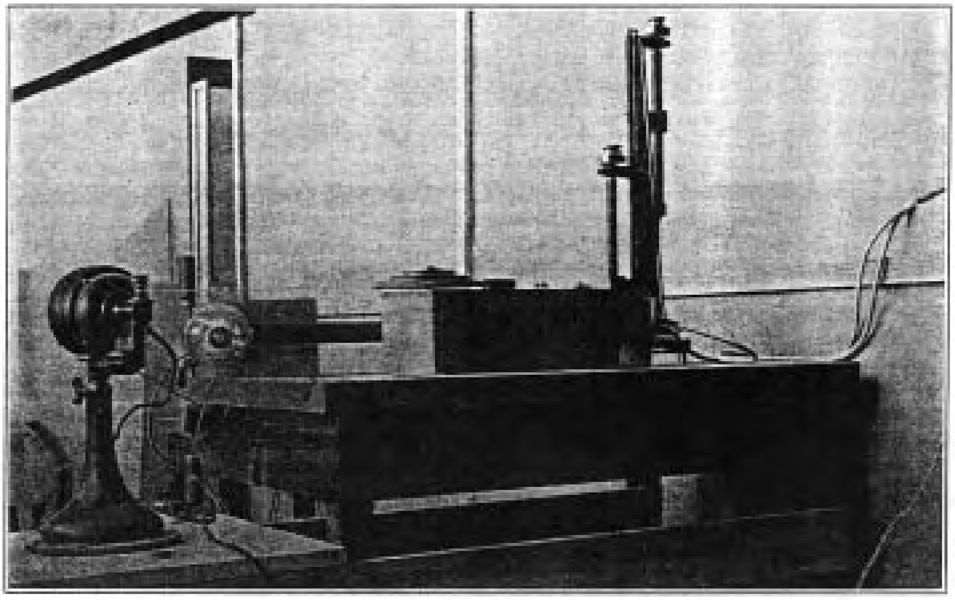


FIG 1.—RECORDING AIR SPEED METER WITH ILLUMINATING AND RECORDING APPARATUS.

typical ones are reproduced here. Numerous experiments on the efficiency of tunnels and on speed fluctuation have previously been made in England.

EFFICIENCY AND SLIP WITH NEW PROPELLER.

In order to give a more even flow of air in the exit cone a new propeller was designed for the model tunnel having a larger pitch at the tip so that the air in this portion would be drawn through with a relatively greater velocity. In every other respect this propeller is very similar to the propeller used in the test described in Report No. 73, which, owing to a piece of wood being dropped in the running tunnel, was completely destroyed. In Fig. 3 is shown the efficiency of this propeller when working in a parabolic cone and in a straight cone. It will be noted that in the same way as with the first propeller the straight cone is considerably more efficient at high speed than the parabolic cone. In Fig. 4 is shown the slip of this propeller in the parabolic cone and in the straight cone and it is noted that the slip is less at high speed for the straight cone. It is then evident that the straight cone is aerodynamically superior to the parabolic cone, in addition to being easier to build.

EFFECT OF SIZE OF THE ROOM.

All the test runs described in Report No. 73 were conducted in a large room, approximating to free-air conditions. In the tests described in this report a temporary room was built around the tunnel, representing to scale the building provided for the 5-foot N.A.C.A. wind tunnel; and all runs except those shown in Figs. 3 and 4 were made in this model room. The cross section of the model room and the

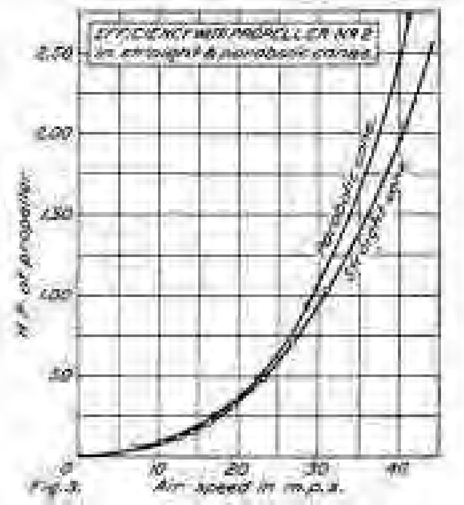


Fig. 3.

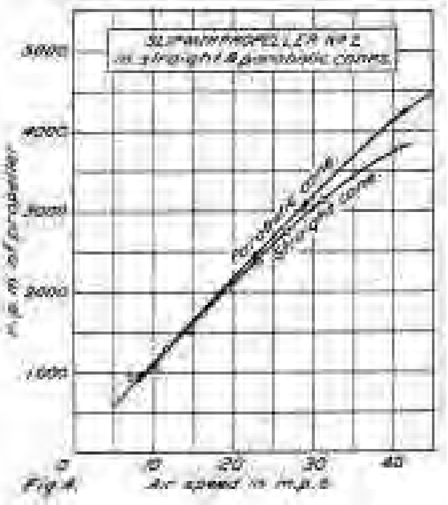
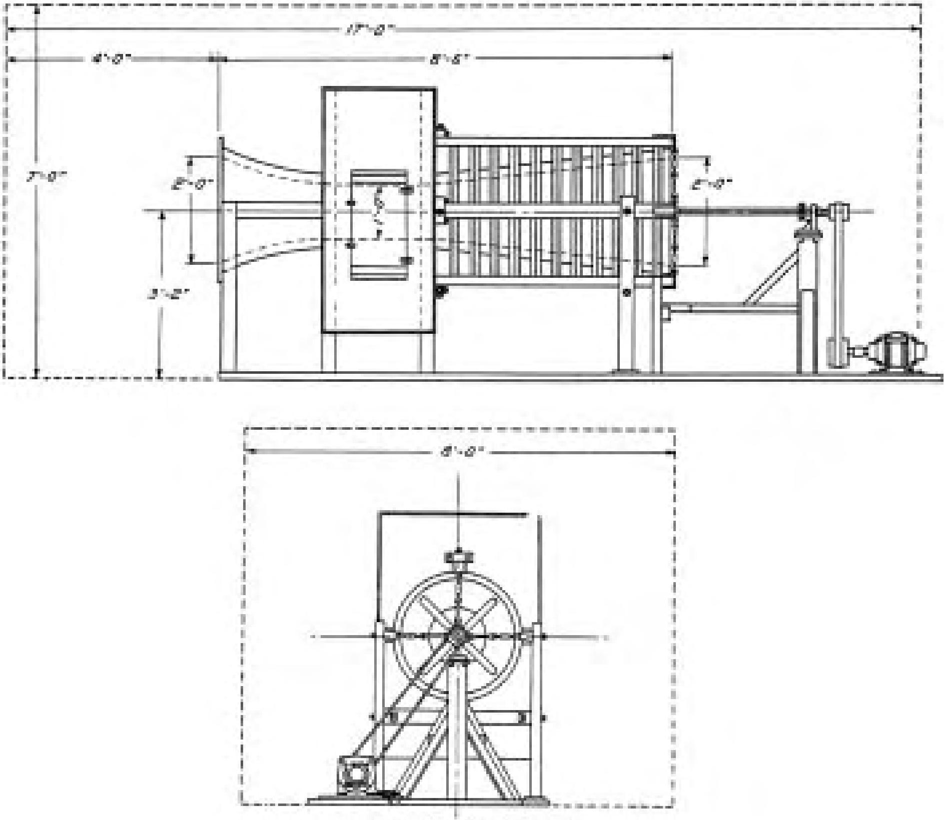


Fig. 4.

wind tunnel are shown in Fig. 5. For the same power this room decreased the air speed from 69 to 59 miles per hour or a decrease of 14.5 per cent. In the small room the maximum variation of speed was ±7 per cent and the maximum variation in direction was ±10°. The air speed records show that for the first 20 seconds after starting, in the large room, and for the first 10 seconds in the small room, the air speed is very steady, and that the fluctuations suddenly appear at a definite time and will be indicated on the record. This appearance of sudden fluctuations seems to indicate that the large part of the speed fluctuations are due to the disturbed air from the propeller as it returns through the room to the entrance cone.

EFFECT OF RADIAL VANES.

Eight radial vanes 3 mm. thick and 450 mm. deep were placed symmetrically in the exit cone immediately before the propeller. These vanes joined in the center in a stationery spinner which was of the same diameter as the propeller base. (Fig. 6.) These vanes actually increased the speed of the air in the tunnel for the same power by 5 per cent, but the fluctuations in direction and velocity remained unchanged. In order to determine what part of the vane gave the increased efficiency, 25 mm. was cut off of the outer end of each vane and the run repeated which gave a 3 per cent increase in speed for the same power over the tunnel with no vanes. Again the vanes were cut off on the end 75 mm. and in this case the same speed was obtained as with the tunnel without vanes. This seems to show that it is the whole area of the vane which acts as a straightener for the air flow and that no particular part is especially valuable in increasing the efficiency of the tunnel. Eight additional vanes 3 mm. thick were then placed along the inner surface

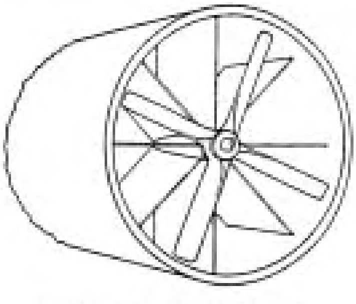


MODEL WIND TUNNEL AND ROOM
Fig. 5

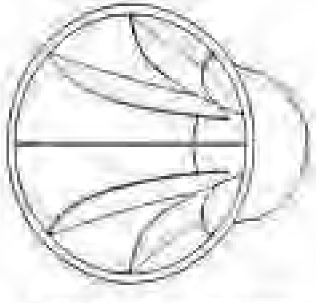
of exit cone, each vane being 75 mm. wide. This distribution of vanes decreased the speed by 12 percent for the same power and the variation in speed was ±6 percent and the variation in direction was ±10°. The same vanes were then placed in the entrance cone, as shown in Fig. 7, and in this case the speed was decreased by 8 percent and the variation in direction was ±8°. With this type of vane in both the exit and entrance cone the speed was decreased by 20 percent for the same power and the variation in direction was ±8°. It is evident from these tests that the narrow vanes in either the exit or entrance cones are of little value in any way.

EFFECT OF PLACING SCREEN ACROSS THE TUNNEL.

A section of chicken wire of 25 mm. mesh was placed across the exit cone 45 centimeters ahead of the propeller. The use of the chicken wire decreased the speed by only 3 per cent, so it does not seem that this distribution of screen would be of any great harm to the efficiency of the tunnel and it is of great use in preventing small objects from being drawn into the propeller. A piece of window screen placed at the beginning of the straight portion of the tunnel decreased the



RADIAL VANES IN EXIT CONE
Fig. 6.



VANES IN ENTRANCE CONE
Fig. 7

speed by 14 per cent and the fluctuation in speed was -12 per cent and was -10 per cent in direction, showing that the screen in no way helps the steadiness of flow for the particular condition of this test. Screens have been used to advantage in other tunnels. With window screen at the mouth of the entrance cone the speed was decreased by only 7 per cent.

EFFECT OF PLACING SPINNERS BEFORE THE PROPELLER.

A spinner 75 mm. in diameter and 450 mm. long was supported by steel wires before the propeller, as shown in Fig. 8. The use of this spinner seemed to have no material effect on the air flow.

THE EFFECT OF EXTENDING THE EXIT CONE BEYOND THE PROPELLER.

By extending the exit cone as shown in Fig. 9, there was no change of the air flow inside the tunnel, but the tangential flow, which had been noticed before with the propeller, was somewhat straightened out, and the air flow was more directly to the rear through the extension of the cone. A cylinder was then attached to the propeller end of the tunnel as shown in Fig. 10, which decreased the air speed

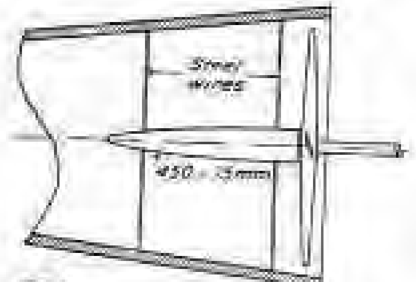


Fig. 8

SPINNER

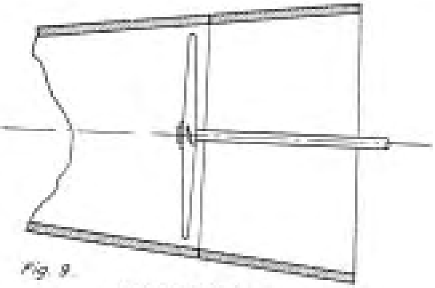


Fig. 9

CONICAL EXTENSION

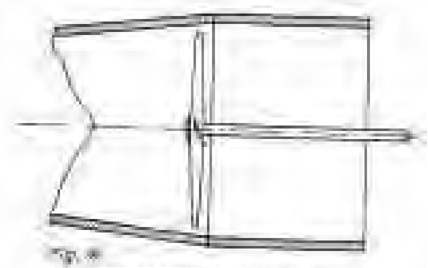


Fig. 10

CYLINDRICAL EXTENSION

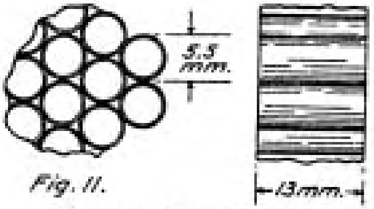


Fig. 11.

SECTION OF HONEYCOMB

about 5 per cent, the air issuing from the tunnel at a considerably higher velocity and in a more compact stream, the borders of the stream still being sharply defined at a distance of 20 feet. As extensions of this kind mean a larger and longer building for the wind tunnel there would certainly be no advantage in using them.

EFFECT OF HONEYCOMBS.

A honeycomb was constructed as shown in Fig. 11 and was placed at the entrance to the straight portion of the tunnel. Owing to the difficulty in obtaining thin-walled metal tubing and to the expense of constructing honeycombs of this type, only this one was tried. It is quite evident, however, even from this one test that the honeycomb is of the greatest importance in straightening out the flow. The speed is reduced 19 per cent and the energy ratio 45 per cent by this honeycomb, but the maximum speed variation was only ±2 per cent and the variation in direction was reduced to ±0.5°.

In order to show more clearly the great increase in steadiness of flow, a curve taken with a recording yawmeter is shown for the open tunnel and for the tunnel

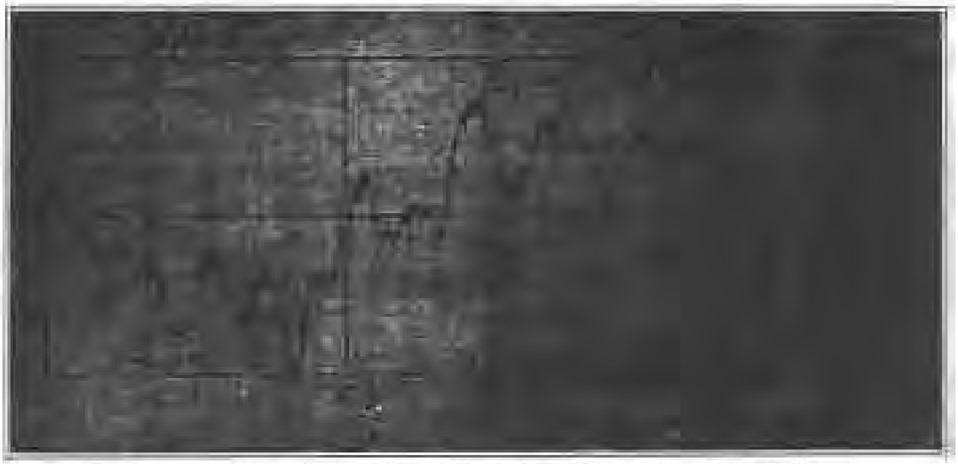


FIG. 12—VARIATION IN DIRECTION IN THE MODEL TUNNEL WITH NO HONEYCOMB.

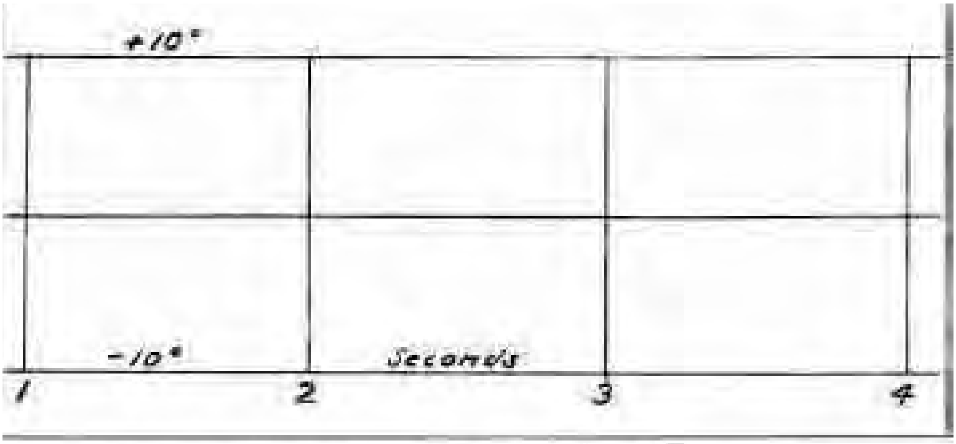
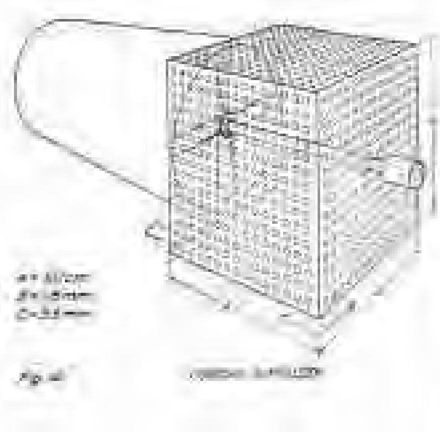


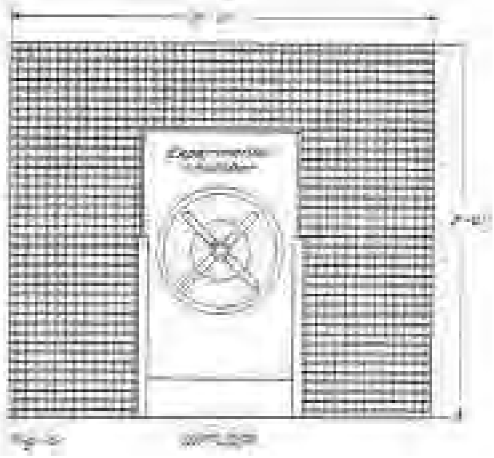
FIG. 13.–VARIATION IN DIRECTION IN THE MODEL TUNNEL WITH A HONEYCOMB

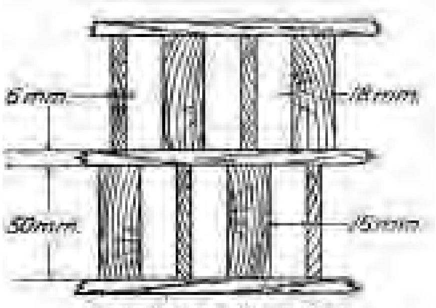
containing the honeycomb. (Figs.12 and 13.) It is evident from these how great is the advantage of the honeycomb. As the length diameter ratio in the tubes of this honeycomb are only 2 ½ it is quite possible that by using longer tubes the flow would be even better and the reduction in speed should not be appreciable. There seems to be no doubt from these tests that the honeycomb is absolutely essential in most wind tunnels.

EFFECT OF DIFFUSERS.

The first diffuser tried is shown in Fig. 14 and consists essentially of a cubical box of which both sides are perforated with small holes, whose diameter is equal to the thickness of the wall of the box and whose spacing between centers is about twice that of the diameter of the hole. This box was connected rigidly to the rear







SECTION OF DIFFUSER
Fig. 16

of the exit cone so that all the air passing through the propeller must escape through these small holes. It was hoped in this way to break up any pulsations which would originate from the propeller. This arrangement decreased the speed of the tunnel by 7 per cent and the maximum variation of speed was ±6 per cent and the direction variation was ±5°, so that it would seem that the flow is slightly straightened, but nowhere near as much as with the honeycomb. A second diffuser was tried as shown in Fig. 15, which consists of a latticework across the tunnel room at the experimental chamber consisting of 50 mm. square cells having a 6 mm. wall with a length 2 1/4 times their diameter. This diffuser only reduced the speed of the tunnel by 2 per cent, and the maximum variation was ±7 per cent, and the variation in direction was ±5°. Although this diffuser has very little effect on the efficiency of the tunnel, at the same time it does not much improve the steadiness of flow. A third diffuser was constructed as shown in Fig. 16 and placed in the same position as the last. This diffuser decreased the air speed for the same power about 5 per cent, the variation in velocity was ±5 per cent, and the variation in direction was ±4°, showing only a slight improvement over the open room. It seems strange that these diffusers did not improve the air flow more, as the British have found that diffusers greatly improve the flow in their tunnels. The results of these tests would not, however, justify the use of a diffuser in a full-sized tunnel because of the rather large expense of construction of such a piece of apparatus.

EFFECT OF PERFORATING THE STRAIGHT PORTION OF THE TUNNEL.

In order to determine the effect on air flow of opening the doors in the cylindrical portion of the tunnel and in using small holes for the introduction of apparatus, various tests were made on the model in order to see how this would effect the efficiency and steadiness of flow. Also the velocity of the air in the experimental chamber was determined by a small anemometer. A slot was first cut in the cylinder parallel to its axis and one-fifteenth of the diameter wide, running the whole length of the experimental chamber. The air flow extended out about the width of the slot from the walls of the cylinder, and beyond this there was no flow in the chamber and the efficiency of the tunnel was not appreciably affected. This slot was then increased in width to one-sixth of the diameter of the tunnel, thus decreasing the efficiency of the tunnel very slightly, and the flow of air extended about one-sixth of the tunnel diameter into the experimental chamber nearest the exit cone, but this air flow was less marked as the distance to the entrance cone was decreased. When the width of the slot was increased to three-eighths of the tunnel diameter the efficiency was decreased about 15 per cent and the air flow extended two-thirds of the width of the slot into the experimental chamber, near the exit cone, but there was no flow elsewhere in the experimental chamber.

TESTS IN FULL-SIZED TUNNEL.

A few tests were made in the large tunnel in order to afford a comparison with the model. In Fig. 17 is shown the slip in the large tunnel. In comparing this with a similar condition in the model tunnel (Fig. 4) it is seen that for the same air speed the revolutions per minute is 5.7 times as large in the small tunnel as in the large one. Theoretically, the ratio should be exactly 5, but the fact that the model test was run in a proportionately larger room would account for this difference.

As the exact efficiency of the driving motor in the large tunnel is unknown, a curve of horsepower supplied to the motor is plotted against air speed, but to give some idea of the power supplied to the propeller a dotted curve is drawn from the estimated motor efficiency. (Fig. 18.)

In comparing this curve with the one obtained in the model, it is seen that the full-sized tunnel is slightly more efficient, so that results may be taken from models to safely predict the performance of the full-sized tunnels. It is also interesting to notice that the power does not increase as rapidly as the cube of the speed but more nearly as $V^{2.5}$, although, as the efficiency of the motor is not exactly known, the value of the exponent can not be determined very closely.

Records were taken in the full-sized tunnel of variations in velocity, and these are reproduced in Figs. 19 and 20. In the first figure the wind-tunnel motor was connected to a gasoline driven generator of 25 kilowatts and records taken at several

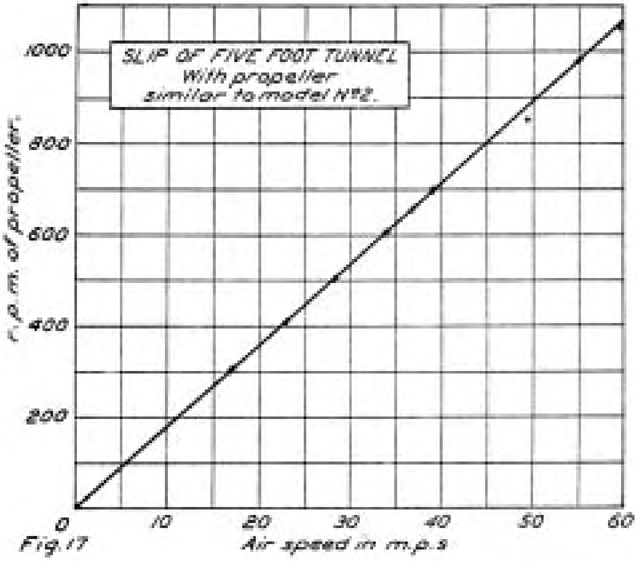


Fig. 17

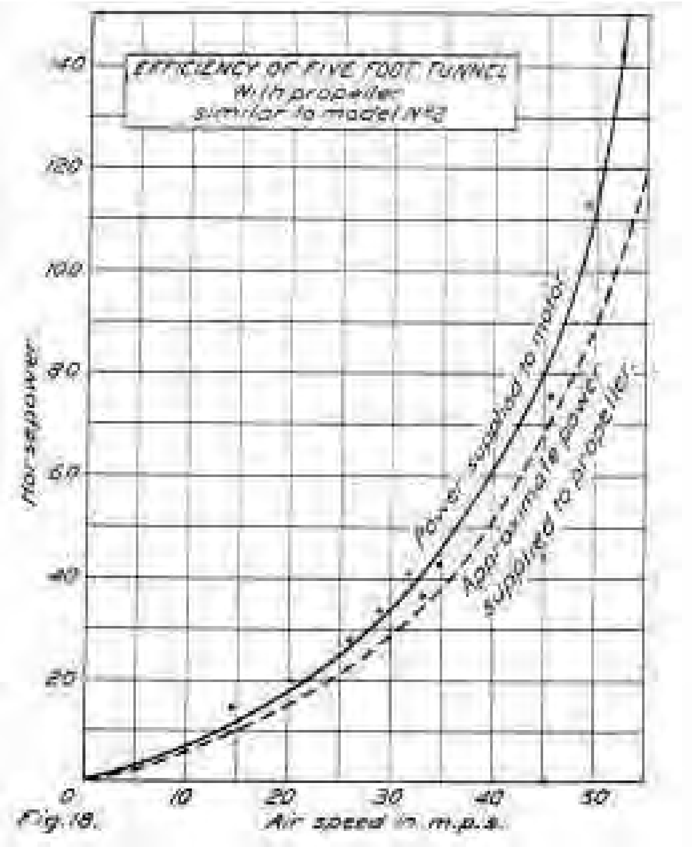


Fig. 18.

speeds. In Fig. 20 the motor was connected to a 300-kilowatt generator driven by a Liberty motor. The most important characteristic of these records is that the magnitudes of the fluctuations do not increase as rapidly as the air speed, so that at the higher speeds, quite contrary to expectations, the velocity is relatively steadier. The maximum variation in air speed at 90 miles per hour was about ±1.5 per cent, whereas in the model it was about ±2 per cent, so that it would seem that the steadiness was about the same in any size of tunnel.

Yawmeter records were also taken in the large tunnel, but were not reproduced, as they show practically a straight line, indicating that the honeycomb was satisfactorily straightening out the flow.

NATURAL PERIODS OF TUNNEL.

A wind tunnel acts as an open organ pipe and its natural period will be given by:

[illegible]

where *l* is the length of the tunnel in feet,and *V* is the velocity of sound, or 1,040 ft./sec.

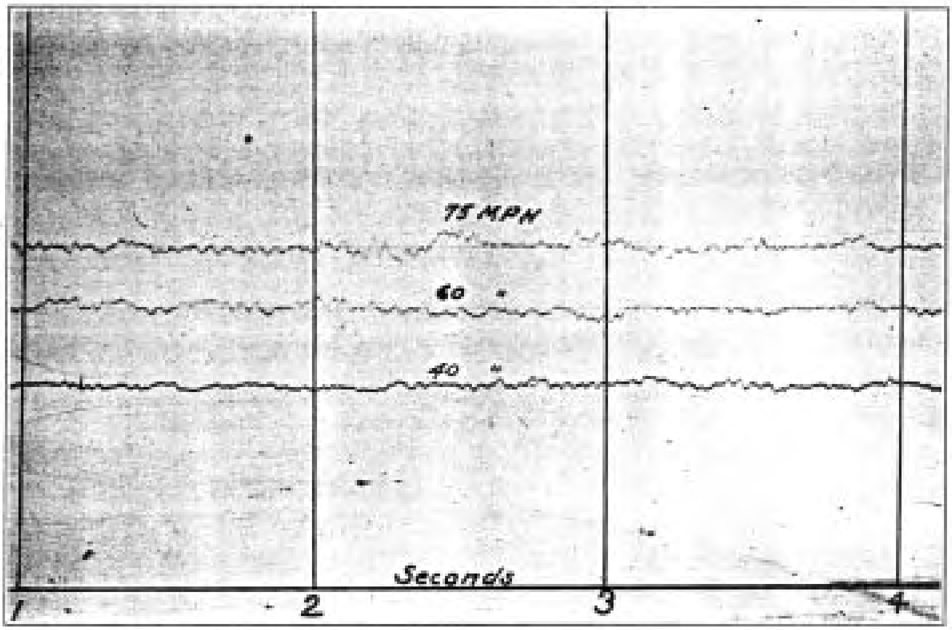


FIG. 19.—VELOCITY VARIATIONS IN LARGE TUNNEL WHEN THE DRIVING MOTOR WAS CONNECTED TO A 25 K. W. GASOLINE GENERATING SET.

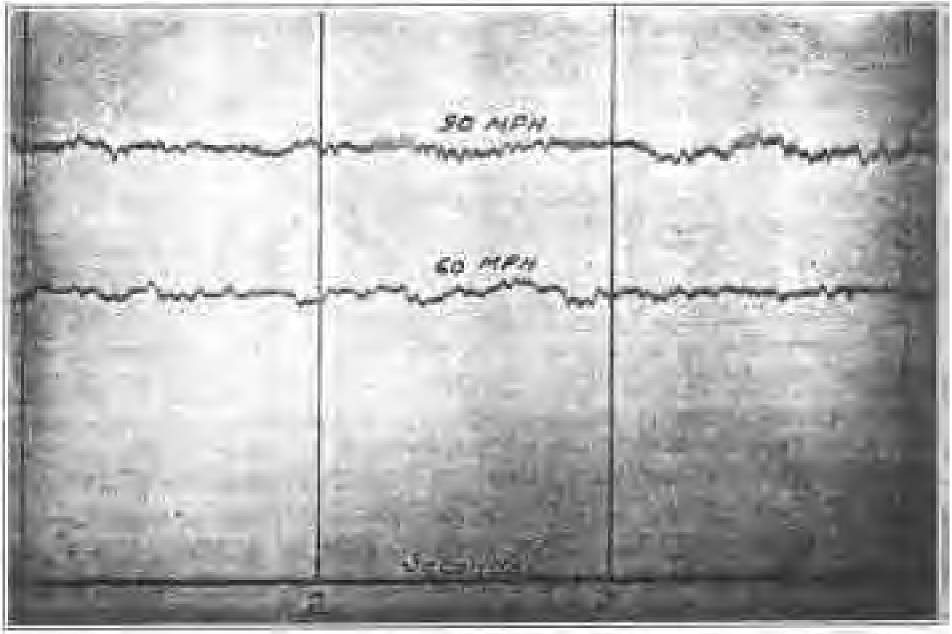


FIG. 20.—VELOCITY VARIATIONS IN LARGE TUNNEL WITH DRIVING MOTOR CONNECTED TO A [illegible] K. W. LIBERTY GENERATING SET.

The model tunnel would then have a period of 0.03 seconds and the large tunnel a period of 0.15 seconds. Vibrations of this nature are very evident audibly in the tunnels at certain speeds, but do not seem to be present on the records, as the pitot tube is very nearly at the node of the vibration. The honeycomb has a considerable influence in damping, these vibrations, which are more of a curiosity than of any practical interest.

AUTOMATIC REGULATORS.

As it is not practical to supply a constant voltage to a wind tunnel, although some tests have been made with storage batteries where an extremely constant speed was required, it is either necessary to keep the voltage constant as nearly as possible by hand regulations or use some type of automatic regulator. In small tunnels it is quite easy to regulate the wind by hand, but in larger tunnels the inertia of the moving parts is so great that there is considerable amount of lag between the change in regulation and the response of the air speed, making hand regulation very difficult. A very complicated regulator has been constructed at Göttingen (N.A.C.A. File No. 5346-10) and seems to hold the velocity quite constant. There are also numerous electrical devices for maintaining a constant motor speed, and some of these regulators will hold the speed within 0.1 per cent. It seems probable, however, that even if the revolutions per minute of the propeller is constant that there will still be fluctuations in the air speed, so that a successful regulator must be actuated by the air flow. There is a great deal of work to be done on such regulators, and the N.A.C.A. intends to carry on work of this kind in the near future.

CONCLUSIONS.

The qualities that should be aimed at in wind-tunnel design in order of their importance are:

1. Constant direction of flow.
2. Constant velocity of flow.
3. Uniform velocity across section.
4. Efficiency.
5. Ease of working around tunnels.
6. Simplicity and cheapness of construction.

A good many of these qualities are contradictory, and the best compromise must be made between them and the type of work that is to be undertaken. For example, a tunnel for testing instruments should have a high efficiency, but need not have a very steady flow. On the other hand, a tunnel for testing wings should have its efficiency somewhat lowered in order to obtain a steady flow. It is quite possible to so arrange the honeycomb and diffusers that they may be removed when it is desired to obtain the highest speed. It would also be of value to make it possible to open the ends of the building, as there are many days when the wind would have little effect on the steadiness, and the efficiency would apparently thus be considerably increased. This arrangement would also make it possible to cool off the air in the building in a very short time, an advantage that would be greatly appreciated in hot weather.

This work seems to show conclusively that a straight exit cone is more efficient than a curved one, and it is certainly cheaper to construct. Diffusers affect the air flow very little, and they do not seem to warrant the expense of construction. Honeycombs, however, are of the greatest value and should be placed in every tunnel.

Document 2-13(a–c)

(a) "Report of Committee on Aerodynamics," *NACA Annual Report for 1919* (Washington, DC), pp. 28–31.

(b) "Report of Committee on Aerodynamics," *NACA Annual Report for 1920* (Washington, DC), pp. 22–28.

(c) "Report of Committee on Aerodynamics," *NACA Annual Report for 1921* (Washington, DC), pp. 31–36.

In its annual reports, the NACA published reports on the activities of its committees (actually subcommittees, as the NACA itself was a committee). These reports outlined the current state of the art and the investigations in progress. This series of reports (1919 to 1921) by the committee on aerodynamics identifies the aerodynamic problems of greatest concern to the NACA during that formative era, as well as the rudimentary state of aerodynamic knowledge. The reader will note how the committee on aerodynamics defined its own role as it proceeded with its research programs during these years, and how the NACA researchers developed cooperative working relationships with McCook Field and the Washington Navy Yard.

Document 2-13(a), "Report of Committee on Aerodynamics," NACA Annual Report for 1919.

REPORT OF COMMITTEE ON AERODYNAMICS.

The committee on aerodynamics is a consolidation, made in April 1919, of the three committees on aircraft design, navigation of aircraft and aeronautic instruments, and free flight tests.

The committee on aerodynamics recommended that in the part of the work at Langley Field within its domain the emphasis should be placed mainly on—

(*a*) Studies, in the laboratories, of propellers from the aerodynamic point of view;

(*b*) Studies, in the laboratories, of the flow of air around parts of airplanes and airships, separately or assembled, and of the forces brought to bear on those parts by the air;

(*c*) Free flight tests on fullsized machines; and

(*d*) The development of instruments, equipment, and methods for making such studies and tests.

This policy has been followed during the year.

The work at Langley Field under these four heads, carried on under the general direction of Edward P. Warner, has been largely one of development and design, although some research work complete in itself has been carried on. The wind tunnel has been under construction throughout most of the year and is now nearing completion, and a number of experiments on model tunnels have been made to determine the best form to adopt for the 5-foot tunnel now under construction. The balance for this tunnel has also been designed and constructed at Langley Field, and is ready for use. As an accessory to the design of the balance, a theoretical investigation of the errors to which such balances are subject and the considerations governing their design has been made and is published as a technical appendix to this report. The most important experimental work complete in itself has dealt with free flight tests, researches having been conducted throughout the summer on two airplanes furnished by the Air Service. These researches have dealt chiefly with the determination of the lift and drag coefficients and with the balance of the airplanes, thus giving data for purposes of comparison with those obtained in model tests. Several minor investigations on the performance of airplanes and on stresses in the airplane have also been undertaken, and two of them are published as appendixes to this report.

The development of autographic recording instruments for free flight tests has continued at the Bureau of Standards under the general direction of John F. Hayford and Lyman J. Briggs for the committee. Of the six new instruments designed five are now complete, namely, the air-speed meter, the angle-of-attack meter, the recording tachometer, the torque meter, and the thrust meter. The first three named have passed through extensive laboratory tests and calibrations, and are substantially ready for use in the air. The torque meter and thrust meter must be subjected to extensive laboratory tests and possible modifications before they will be ready for use. The stable zenith instrument, to be used for recording continuously the angle between the wing chord and the horizontal, has been redesigned as a result of considerable experience with this type of instrument. The redesigned instrument is being built and is nearly complete.

Special investigations have been made and are still in progress at the Bureau of Standards, under the recommendation of the committee, on the Parker variable camber wing for airplanes. This is a promising line of attack of the problem of reducing the landing speeds of an airplane without reducing its flying speed. Incidentally, some valuable information is being secured in regard to the possibility of using a streamline wing and the ordinary type in a biplane combination.

Researches on airplane propellers are being continued at Stanford University under the general direction of Prof. W. F. Durand.

The Bureau of Standards has undertaken four investigations for the aerodynamics committee:

1. Development of open scale altimeter.
2. Diaphragms and elastic fatigue.
3. Altitude correction for air speed indicators.
4. General report on aeronautic instruments.

The first three of these are definite experimental problems while the fourth consists not only of a summary of recent experimental investigations of instruments carried out in cooperation with the committee at the Bureau of Standards, but will go further and embrace a general survey of the state of this subject at the close of the war both in the United States and abroad.

1. The work on the open scale altimeter has progressed to a point where a working model of the essential parts of the altimeter, exclusive of the indicating mechanism, has been assembled and tested. By proper theoretical design of the spring and diaphragm elements an instrument has been made whose performance depends on the material of the steel spring almost entirely and only to a slight degree on the material of the diaphragm. It has been possible to secure steel nearly free from elastic fatigue effects, although such is not the case with the alloys used for the thin flexible diaphragm. As a result this instrument has been shown by test to have less than one-third of the fatigue effects (i.e., discrepancy between increasing and decreasing readings) permitted by the Bureau of Standards specifications for altimeters. The object of this work, which will be continued until completed, is to provide a precision altimeter suitable for altitude determination in aircraft performance tests.

2. Thin metallic diaphragms, usually corrugated for flexibility, form a necessary element in a great variety of engineering instruments, particularly in aneroid barometers for altitude measurement, in air-speed indicators, certain forms of statoscopes and rate-of-climb indicators, balloon manometers, and aviator's oxygen control apparatus. Such diaphragms are never perfectly elastic but show what are known as fatigue effects, failing to recover instantly from the deformations undergone in the normal operation of the instrument; hence, the importance of experiments to select the most promising alloys, to determine the most effective thermal and mechanical treatment in the process of manufacture, and to establish the most efficient geometrical design for the diaphragms when used either singly or in combinations. Up to the present, this investigation has resulted in the development of measuring appliances for detecting the small changes in question by micrometric methods, in the preparation and use of suitable shop equipment for spinning sample diaphragms at the bureau in considerable numbers, in a preliminary study of a variety of alloys, to select those which warrant more detailed study, and in a special study of mechanical seasoning processes. This last phase of the work has led to the conclusion that diaphragms can be seasoned mechanically; that is, artificially aged and thus brought into a permanent state where they will repeat

their performance in successive tests under the same conditions. This is done by repeated deformation of a suitable amount several thousand times, and is done automatically by a mechanism designed for the purpose. Some such seasoning process appears to be a necessary preliminary to the comparative measurement of the effects of different processes of heat treatment, different compositions of the alloy, and different mechanical design. The seasoning process, it will be understood, is not intended to eliminate elastic fatigue, although it does always reduce it somewhat. The object of the seasoning is to secure definite and uniform results, so that those factors which will diminish the fatigue can be analyzed quantitatively.

3. It has hitherto been taken for granted that Venturi tubes, when used for air-speed indicators, will follow a familiar law which states that the suction produced is directly proportional to the density of the air and to the square of the speed.

The object of this investigation is to determine by direct experiment whether this law does apply to Venturi tubes or whether on the contrary, the compressibility and viscosity of the atmosphere may cause some effects which will complicate the correction of these instruments for different altitudes. The experiments are conducted in a so-called vacuum wind tunnel (that is, a very small air-tight wind tunnel in which reduced pressures corresponding to high altitudes may be secured). The conclusion has been reached that the instruments examined are free from the effect of compressibility but not entirely free from the effect of viscosity. These experiments are to be completed and brought to a conclusion which can be expressed numerically for the purpose of correcting such instruments when used in aircraft performance tests.

4. The general report on aeronautic instruments presents the results of investigations made during the war by the Bureau of Standards to determine the characteristic sources of error of the various types of instruments. Tachometers, for example, for measuring the revolutions per minute of the propeller shaft are built in a variety of different types, operating on diverse physical principles. There are the chronometric, centrifugal, magnetic, electric, air viscosity, air-pump, and liquid types, each of which has its own characteristic sources of error. Aside from the ordinary errors met in engineering instruments, such as incorrect calibration, parallax, looseness of friction in the mechanism, elastic hysteresis, and secular changes, those used on aircraft may be further influenced by the physical conditions peculiar to aviation, viz, (1) extreme drop of pressure, (2) extreme change of temperature, (3) vibration, (4) acceleration or inclination. Besides the above results, this report, which is nearly completed, gives a description of the instruments collected by the Bureau of Standards in cooperation with this committee during the war, including those of British, French, Italian, Russian, Danish, and German construction.

In light of that report it will be evident that the objects toward which instrument development work should chiefly be directed in the immediate future may be summarized as follows:

1. Open scale instruments for performance testing of aircraft. These need not necessarily be so compact, light, or rugged as service instruments, and hence offer freedom for such design as will insure the highest accuracy.

2. Instruments for long distance navigation, including an absolute or ground speed indicator, and such a form of gyroscopic stabilizer as may be needed for mounting the instruments.

3. Instruments to guide the pilot in flying through fog, such as more reliable gyro turn indicators and compasses.

4. Better materials for springs and diaphragms and a more systematic determination of the thermal and elastic constants of the materials.

The Bureau of Standards has developed (primarily for the Air Mail Service) a field-marking radio device which enables a pilot to steer directly to the center of his landing field, although it may be obscured by clouds, rain, snow, or fog. This apparatus utilizes the same transmitting equipment for the ordinary radio direction finding signals and for the landing signals. The landing signals are projected vertically as an electromagnetic cone of great intensity, which can be heard satisfactorily at an altitude of three to four thousand feet. The device enables the pilot to first find the approximate vicinity of a landing field and then fly directly to its center, thus making a safe landing in a fog or in the dark. As elevated aerial systems are manifestly dangerous to air navigation, the Air Mail Service experimented extensively in radio transmission with antennas only 20 feet in height, highly directional, and admitting of sharp tuning. The installation of high-powered stations in the vicinity of flying fields is therefore made possible. Efforts are being made to provide and perfect a practical visual signal to take the place of the present audible signal requiring an audibility of 10,000 to overcome engine ignition interference noises. Such a signal will greatly enlarge the field of operation. A new type of gyroscopic nonmagnetic compass, intended to overcome the unreliability of the magnetic compass caused by vibrations and other disturbing influences of an airplane in motion, is now being developed by the Air Mail Service. This compass is now in a usable form for operation on land or sea, and only requires such changes as will adapt it to use on airplanes. It consists essentially of a solid metal ball floating on a film of compressed air and rotating coordinately in fixed relation to the earth's rotation.

Document 2-13(b), "Report of Committee on Aerodynamics," NACA Annual Report for 1920.

REPORT OF THE COMMITTEE ON AERODYNAMICS.

Following is a statement of the organization and functions of the committee on aerodynamics:

ORGANIZATION.

Dr. John F. Hayford, Northwestern University, chairman.
Dr. Joseph S. Ames, Johns Hopkins University, vice chairman.
Maj. T. H. Bane, United States Army.
Dr. L. J. Briggs, Bureau of Standards.
Maj. V. E. Clark, United States Army.
Commander J. C. Hunsaker, United States Navy.
Franklin L. Hunt, Bureau of Standards.
Prof. Charles F. Marvin, Chief Weather Bureau.
Edward P. Warner, Massachusetts Institute of Technology, secretary.
Dr. A. F. Zahm, United States Navy.

FUNCTIONS.

1. To aid in determining the problems relating to the theoretical and experimental study of aerodynamics to be experimentally attacked by governmental and private agencies.

2. To endeavor to coordinate, by counsel and suggestion, the research and experimental work involved in the investigation of such problems.

3. To act as a medium for the interchange of information regarding aerodynamic investigations, in progress or proposed.

4. The committee may direct and conduct research and experiment in aerodynamics in such laboratory or laboratories as may be placed (either in whole or in part) under its direction.

5. The committee shall meet from time to time, on call of the chairman, and report its actions and recommendations to the executive committee.

The committee on aerodynamics by reason of the representation of the Bureau of Standards, the Army, the Navy, technical institutions, and the industry, is in close contact with aerodynamical research and development work being carried on in the United States. Its representation enables it, by counsel and suggestion, to coordinate the experimental research work involved in the investigation of aerodynamical problems, and to influence the direction of the proper expenditure of energy toward those problems which seem of greatest importance.

The committee has direct control of aerodynamical research conducted at the Langley Memorial Aeronautical Laboratory and also directs propeller research conducted at Leland Stanford Junior University under the supervision of Dr. W.F. Durand, and through its membership it keeps in close touch with the work being carried on at the Bureau of Standards, at McCook Field by the engineering division of the Army Air Service, and at the Washington Navy Yard by the Bureau of Construction and Repair, United States Navy.

Two new wind tunnels have been completed and put in operation in the United States within the past year. A new 5-foot wind tunnel at the Langley

Memorial Aeronautical Laboratory has gone into service and has already run at speeds slightly in excess of 110 miles per hour. It is anticipated that speeds of 140 miles per hour will be attained with a new propeller which will be better suited to the characteristics of the electric motor employed. The other new wind tunnel of the year is that constructed by the Curtiss Engineering Corporation at Garden City and is of the true Eiffel type.

The committee on aerodynamics, in directing the research work at the Langley Memorial Aeronautical Laboratory, has adopted a definite policy with reference to research work to be conducted at this laboratory. The policy adopted confines the work to three general problems, and, in order to obtain results which will be of general use, experiments are to be conducted in such a manner that general conclusions and, if possible, general theories may result from them. The following three general problems covering the work of the aerodynamical laboratory for the coming year have been adopted:

(*a*) Comparison between the stability of airplanes, as determined from full-flight test and as determined from calculations based on wind tunnel measurements.

The committee will endeavor to determine the characteristics and peculiarities of certain existing airplanes, and attempt to account for these by calculations based on wind tunnel work. The matter of control will also fall under this heading. The first work conducted will probably be confined to the explanation of the theory of small oscillations and its verification with full-scale work. Later, a study of maneuverability and controllability will follow, as it is felt that in the present state of the art there is not available to airplane designers a rational method of predicting the maneuverability of airplanes from the drawings of the airplanes or from wind tunnel experiments with models.

(*b*) Similar comparison between the performance of airplanes full-scale and the calculations based on wind tunnel experiments.

A great deal of attention has been given by the British to the prediction of performance based on aerodynamic data, but there is still a gap between model and full-scale results which can not be bridged until we have more information. The performance is intimately connected with the propeller, and it is the intention of the committee to have all propeller research conducted at the Aerodynamical Laboratory of Leland Stanford Junior University under the direction of Dr. Durand. An effort will be made to tie in the results obtained at Leland Stanford with the performance work being done at Langley Field. Experiments will also be conducted on models of well-known airplanes to better understand the landing and starting characteristics of airplanes and to determine exactly what it is that makes certain airplanes require a long run.

(*c*) General aerofoil problem, including control surfaces, with particular reference to thick sections and combinations and modifications of such sections.

The committee is to undertake a systematic investigation of thick wing sections, after a thorough analysis of what has been done in this matter, and to duplicate some of the experiments already performed. After the determination of what properties of thick wing sections are of interest, work will then be carried along with a view to systematic variation of the variables which determine the aerodynamic properties of a series. Determination will also be made of the relation between aerodynamic properties of such standard aerofoils and aerofoils of similar profile but of different aspect ratio and taper. It is also desirable to know biplane and other interference effects when the aerofoils are used in combination. A careful study will also be made of recent work, by which it appears possible to predict from a knowledge of the lift coefficient the properties of aerofoils in combination and of different aspect ratio, as well as the influence of a boundary.

Such problems arising in connection with the Army and Navy programs of development as fit in logically with the above program will be referred to the committee on aerodynamics, and the research work covering the problems will be conducted at the Langley Memorial Aeronautical Laboratory.

At the Langley Memorial Aeronautical Laboratory a large number of experiments have been carried on with model wind tunnels in the past year to determine the best form for steadiness of flow and efficiency of operation. The effect of various shapes of cones, experimental chambers, and types of propellers, honeycombs, and diffusers were thoroughly studied. A special recording air-speed meter and recording yaw meter were designed in order to study the steadiness of flow, and it was found that the tunnel with a continuous throat was superior to the open or Eiffel type of tunnel both in efficiency and steadiness of flow. It was also demonstrated that a honeycomb placed in the entrance cone is of the greatest value in straightening the air flow, but a diffuser placed in the return circuit was apparently of little value.

The National Advisory Committee's 5-foot wind tunnel was completed in the spring of 1920 and has been in continuous operation since. This tunnel is designed from the data obtained in the model experiments and is very satisfactory both in efficiency and steadiness of flow. The 10-foot four-bladed propeller is driven by a 200-horsepower variable-speed electric motor. The power for this motor is obtained from gasoline-driven generating sets, and the control system is very convenient, the motor being started and stopped simply by pushing a button in the experimental chamber, and the speed being controlled by a rheostat from the same place.

The balance used in this tunnel is of the modified N.P.L. type, and was constructed in the shop of the National Advisory Committee at Langley Field. Unlike the usual balance, the weight is supported on a half bearing socket, rather than a conical pivot, as this device considerably reduces the friction and will carry a much larger load. It is also possible with this balance to simultaneously read the lift, drag, and pitching moment. As the N.P.L. type of balance is not suited to holding tapered wings, and as a large amount of work of this kind is planned for the future, a simple wire type of balance is being constructed at the present time, similar to that used in the wind tunnel at Göttingen.

It has been the practice in the past when setting up a model to align the chord of the wing with the wind by placing a thin wooden batten on the wing and comparing this batten with a straight line on the floor of the tunnel. But as this method is rather laborious and inaccurate, a new type of aligning apparatus has been designed for this tunnel, consisting essentially of a mechanism for reflecting a beam of light from a plain mirror which is attached parallel to the chord of the wing, so that by rotating the wing the reflected beam of light is brought to a cross line on a small target on the side of the experimental chamber. In this way a wing can be lined up with an accuracy of 0.01° in a very few seconds. As the air speeds used in this wind tunnel are considerably higher than those usually encountered, a special type of manometer was constructed to obviate the necessity of having an extremely long inclined tube. This gauge changes the head of liquid and at the same time the inclination of the tube, so that the fluctuations of the liquid are approximately equal at any speed. A multiple manometer has also been constructed for pressure distribution work on models, containing 20 glass tubes, the inclination of which can be adjusted to any desired angle.

A thorough investigation has been made of the problem of spindle interference and the best manner of protecting the spindle by a fairwater. Different types and lengths of fairwater were tested in order to determine which condition would give the least total interference. An accurate determination of the effective resistance of the spindle was made for various lengths of spindle and for various air speeds so that a complete set of data is available for use on any model tests for the future. In order to provide data for stability calculations a wing was tested through an angle of 360 degrees, and a model of an airplane was tested in the same way. In order to determine the scale corrections for model airplanes a model of the JN4H was constructed with great accuracy, and all details of the airplane were reproduced in the model, including the radiator and motor, but the wires were omitted as it was thought that their resistance could be determined better from tests of the full-sized wires. This model was tested at speeds of 30, 60, and 90 miles per hour in order to determine the corrections that must be applied to it in order to give the full-flight performance which was carefully determined on the full-sized machine.

FREE FLIGHT.

The machines available for the committee's use at the Langley Memorial Aeronautical Laboratory consist of two JN4H training machines and one DH4.

During the summer the machines have been in the air about 60 hours. Numerous small changes have been made on these machines during the different tests, including changing the stagger, changing the angle of the tail plane, and changing the position of the center of gravity by adding weight at the front or rear of the fuselage. A large number of special instruments have been designed and constructed at Langley Field for research in full flight. An accelerometer has been developed for obtaining the loads on an airplane during stunts and landings, and satisfactory results have been obtained with it, which are of considerably greater accuracy than those obtained by other types of instruments. Instruments were also developed for recording the position of and the force on all three controls of the airplane, and variable results have been obtained with these instruments. For obtaining the pressure distribution on the tail of the full-sized machine a special multiple manometer was constructed having 110 glass manometer tubes, all of which could be photographed at one time by an automatic film camera placed in the fuselage. As this instrument will only determine accurately the pressure distribution in steady flight, another manometer is now being constructed consisting of a large number of small diaphragm gauges which will record continuously on a moving film so that the rise and fall of the pressure at various points on the tail surfaces can be recorded during any stunt maneuver.

An air-speed meter and yaw meter have been constructed, working on the optical recording principle, having the actual period of the instrument high and its friction small, so that air-speed records can be obtained of any small or high period fluctuations in the wind velocity. To determine the angular rotation of the airplane during flight, in order to study its stability properties, a kymograph was constructed consisting of a narrow slit which focused the image of the sun on moving bromide paper, and another instrument of the same type has been constructed working on the gyroscopic principle. For obtaining the full-flight lift and drag coefficient a special longitudinal inclinometer was constructed which would give a large scale deflection and would be convenient and accurate to read.

The investigations undertaken consist of the determination of the lift and drag coefficients of the JN4H in free flight, and it is found possible by careful piloting to flay the machine at or slightly beyond the burble point. A thorough experimental investigation has been made of the static longitudinal stability of the airplane and a great many factors have been altered on the full-sized machine, such as changing the angle of the tail plane, changing the center of gravity of the machine, changing the section of the tail plane, and inclining the angle of the propeller axis. A study was made of the angle of attack and the air speed at the wing tips during spins and loops. This was accomplished by placing vanes and air-speed meters at the wing tips and photographing them during the maneuver by means of a camera gun and then plotting the curve of angle and speed against time from the photographs so obtained.

A very extensive investigation of the pressure distribution over the tail of an airplane in free flight has been undertaken. The pressure at 110 points on the left and right hand sides of the tail have been taken independently and the total pressure determined from these two curves. By means of photographic recording methods the time taken for making this investigation in the air is brief, but the computation and plotting of the results are laborious and require a long time for their completion. Runs were made with three positions of the center of gravity and two angles of setting of the stabilizer, as well as one run with celluloid over the crack between the stabilizer and the elevator. In all cases the pressure found over the tail was extremely low and in steady flight the load on the tail would be found very small compared with the load resulting from accelerated flight. A large number of records have been taken with the recording accelerometer designed by the N.A.C.A., these records being taken in the JN4H and several other machines during various stunts and landings. It was found that the maximum acceleration experienced in any stunt was during a roll, where the acceleration reached a maximum of 4.2 g. In order to determine the characteristics of an airplane during circling flight a record of the forces on all three controls was made doing banks of various angles up to 60 degrees and side slips up to 20 degrees of yaw.

The wind tunnel at Leland Stanford Junior University has again been occupied entirely with propeller tests. The results of the research work conducted this year are contained in technical report No. 109. Preparations are being made for tests on propellers at large angles of yaw, which will give data for the analysis of helicopters traveling horizontally.

Dr. George de Bothezat, aerodynamical expert of the National Advisory Committee for Aeronautics, has carried on at McCook Field, with the cooperation of the Engineering Division of the Army Air Service, a special investigation for the measurement of aerodynamic performance. The report on this investigation has been completed and approved as technical report No. 97, entitled "General Theory of the Steady Motion of an Airplane." This investigation involved the design and construction of a new type of barograph. Also in connection with his investigation of airplane performance, Dr. de Bothezat has designed a torque meter and a rate-of-climbmeter, which are under construction. The torque meter is a very simple design, and present indications are that it will be a most serviceable and efficient instrument. The rate-of-climbmeter is not based on a new principle; it is simply a new construction and design embodying the experience obtained in the use of other instruments.

The research work conducted by the Bureau of Construction and Repair of the Navy Department is carried on at the aerodynamical laboratory of the Washington Navy Yard and at the naval aircraft factory, Philadelphia Navy Yard. At the Washington Navy Yard two wind tunnels are in operation, and during the year a large number of airplanes and seaplane models have been given routine tests, and tests on many new aerofoil sections have also been made. Special attention has been given to testing streamline forms and struts. Yawing tests were conducted on the EP and the IE envelopes, which are formed from mathematical curves and have very low resistance. The tests indicate that the yawing moment about the center of gravity of a bare streamlined form varies but little from one shape to another. In connection with the tests on struts, it was shown that the Navy I strut has approximately 15 per cent less resistance than that given for the "Best" strut by the National Physical Laboratory. Wind tunnel tests were also conducted on two airship cars, one of faired contour and the other with facets of the same general contour, the results of which show the great value of fairing. The resistance of the faired car was 15 per cent less than that of the unfaired.

In connection with the wind tunnel at the Washington Navy Yard, a new aerodynamic balance of great interest has been developed. The balance is so designed that all adjustments of weights to bring the balance into equilibrium are automatic, and the time required for testing and the number of skilled operators are thus much decreased.

The Bureau of Construction and Repair has also undertaken the development and construction of the following instruments:

> A precision recording barograph intended for use in airplane trials, and especially for measuring the landing angle of airplanes, for which no wind tunnel test is available. This instrument will have a range of from 0 to 5,000 feet, and will incorporate the desirable features of the present Bureau of Standards precision altimeter.
>
> Two thermometer altimeters and density indicators. These instruments will combine a thermometric element with a pressure element in such a manner as to show at all times the altitude corrected for temperature.
>
> Two instruments intended to measure quantitatively the permeability of gas cells of envelopes without the removal of samples. The construction of these instruments has been suggested by the technical staff of the Bureau of Standards. This instrument is to take the form of a cup of suitable area which is pressed against the envelope at the point where the permeability is to be determined. A current of air is either sucked or driven through the cup, sweeping it out at a known rate. The mixture of gas and air from the cup is then passed through a thermal conductivity cell, and the proportion of hydrogen contained in the mixture is determined from the thermal conductivity of the mixture.

In the high-speed wind tunnel at McCook Field, which is operated under the direction of the technical staff of the Army Air Service, work has been continued along the same general lines as those indicated in technical report No. 83. During the year it is contemplated that tests will be conducted to determine the flow around a sphere and around biplane combinations. It is hoped thus to determine how nearly the action of the visible vapor particles indicate the true air flow about a body, and to visualize the flow around combinations of more than one supporting surface so as to determine the nature of the interference between the upper and lower surfaces should be of the greatest interest. It is also hoped to photograph the vapor action about a sphere over as large an air-speed range as possible. The sphere is to be supported in a manner to produce a minimum disturbance due to the support, and the photographs obtained are to be compared with existing photographs of flow about spheres and with the theoretical streamlines.

It is also hoped that tests will be conducted to determine the effect of rake and tapered wing tips on air flow, as this information may make it possible to further improve the airplane form and nature of taper in wings.

Performance tests are also conducted at McCook Field, and the committee on aerodynamics has requested that special tests be made on longitudinal stability to obtain an index of the dynamic longitudinal stability of the various airplanes used by the Army. The work already done by the staff of the National Advisory Committee for Aeronautics at Dayton with the cooperation of the Engineering Division of the Air Service on five airplanes is but a beginning of longitudinal stability investigation. It is desirable to obtain readings of stick forces and elevator angles on every type of machine in the Army's possession, and to have curves plotted in the same way as in National Advisory Committee's report No. 96.

The investigations carried on at the two wind tunnels of the Bureau of Standards under the direction of Dr. L.J. Briggs have consisted largely in instrument calibration and testing. The principal research has been in connection with the resistance of spheres and projectiles.

The work of the Aeronautic Instruments Section of the Bureau of Standards comprises the investigation, experimental development, and testing of aircraft instruments; also the development of methods of testing, fundamental researches on the physical principles involved in such instruments, and the study of their behavior in actual service.

The more important investigations which have been undertaken by the section during the past year are as follows:

An investigation has been completed and prepared for publication through the National Advisory Committee for Aeronautics on the effect on the performance of Venturi tube air-speed indicators of changes in atmospheric pressure. The results show that in certain instruments commonly used a correction should be applied for the viscosity of the air, a factor which has not hitherto been taken into account. This is of special interest in dirigible work where the air speeds may be low, and also in aircraft performance tests where exceptional precision is required.

An altimeter of exceptional accuracy designed and made at the Bureau of Standards has been completed and submitted to the Army. Another model with additional improvements has recently been designed and is under construction.

At the request of the National Advisory Committee for Aeronautics a fundamental investigation of the factors determining the behavior of flexible diaphragms as used in aeronautic instruments has been undertaken. The irreversible effects which cause the lag in diaphragm instruments has been formulated mathematically. The relation between force and deflection for diaphragms of different sizes, thickness, and materials has been studied graphically, practical methods for spinning diaphragms and building up diaphragm boxes have been investigated, and the possibilities of mechanical seasoning by repeated stress considered.

An improved rate of climb indicator, which indicates directly the rate of climb of aircraft in hundreds of feet per minute, has been completed and tested, and specifications have been prepared for the Army to use in the manufacture of a number of these instruments.

Information regarding instruments available for aerial navigation in cloudy weather or at night or for long-distance flights has been compiled at the request of the National Advisory Committee for Aeronautics and the Air Mail Service by the Aeronautic Instruments Section. This work will be continued and the development of new instruments undertaken.

Other investigations have been the development of a motion-picture apparatus for recording instrument readings during the flight of an airplane; a study of the errors in instruments used for determining the direction of aircraft, such as gyroscopic and liquid inclinometers and banking indicators, gyroscopic and magnetic compasses and turn indicators, a systematic investigation of commercial sphygnomanometers; a paper on the results of investigations on German instruments; a statistical study of the causes of failure in aeronautic instruments.

Assistance has been given the Air Service, the Aero Club of America, and others interested, during the past year, in the world's altitude competition for airplanes. Instruments have been calibrated and the best procedure for determining the altitude attained formulated.

Document 2-13(c), "Report of Committee on Aerodynamics," NACA Annual Report for 1921.

ORGANIZATION.

The Committee on aerodynamics is at present composed of the following members:

Dr. John F. Hayford, Northwestern University, chairman.
Dr. Joseph S. Ames, Johns Hopkins University, vice chairman.
Maj. T. H. Bane, United States Army.
Dr. L. J. Briggs, Bureau of Standards.
Commander J. C. Hunsaker, United States Navy.
Dr. Franklin L. Hunt, Bureau of Standards.
Maj. H. S. Martin, engineering division, McCook Field.
Prof. Charles F. Marvin, Chief Weather Bureau.
C. I . Stanton, Air Mail Service.
Edward P. Warner, Massachusetts Institute of Technology, secretary.
Dr. A. F. Zahm, United States Navy.

FUNCTIONS.

The functions of the committee on aerodynamics are as follows:

1. To determine what problems in theoretical and experimental aerodynamics are most important for investigation by government and private agencies.

2. To coordinate by counsel and suggestion the research work involved in the investigation of such problems.

3. To act as a medium for the interchange of information regarding aerodynamic investigations and developments in progress or proposed.

4. The committee may direct and conduct research in experimental aerodynamics in such laboratory or laboratories as may be placed either in whole or in part under its direction.

5. The committee shall meet from time to time on the call of the chairman and report its actions and recommendations to the executive committee.

The committee on aerodynamics by reason of the representation of various organizations interested in aeronautics is in close contact with all aerodynamical work being carried out in the United States. In this way the current work of each organization is made known to all, thus preventing duplication of effort. Also all research work is stimulated by the prompt distribution of new ideas and new results which adds greatly to the efficient conduction of aerodynamic research. The committee keeps the research workers in this country supplied with information on all European progress in aerodynamics by means of a foreign representative who is in close touch with all aeronautical activities in Europe. This direct information is supplemented by the translation and circulation of copies of the more important foreign reports and articles.

The Aerodynamic Committee has direct control of the aerodynamical research conducted at Langley Field, the propeller research conducted at Leland Stanford University under the supervision of Dr. W.F. Durand, and some special investigations conducted at the Bureau of Standards and at a number of universities.

WIND TUNNEL.

The committee's wind tunnel at Langley Field has recently had several changes made in it which have considerably improved the steadiness of flow. The most important of these is a new electrical system consisting of a synchronous motor-generator set which furnishes power direct to the wind tunnel motor. The speed of the wind tunnel motor is kept at a constant value within ±0.2 of a per cent by means of automatic voltage regulators. The air flow has also been considerably improved by placing a series of vanes around the end of the exit cone so that the air escapes radially. A wire type of balance is now used in this tunnel for all speeds between 30 and 60 meters per second.

It has long been felt that the tests made in the wind tunnel with a model varying much from the usual type are unreliable because of the uncertainty of the scale correction. For this reason the committee is now constructing at Langley Field a compressed-air wind tunnel with a throat diameter of 1.6 meters, a maximum speed of 25 meters per second, and a working pressure of 20 atmospheres. This wind tunnel will give a Reynolds number which is the same as for a full-sized airplane, and although the difficulties of supporting the model are great, the use of a comparatively low velocity and a high pressure have overcome the mechanical difficulties.

There are being constructed at the present time in the United States four other wind tunnels. At the Massachusetts Institute of Technology there are being erected a 1.25 meter and a 2.50 meter tunnel of the open-circuit type and with continuous throats. At McCook Field there is being constructed a high-speed 1.6 meter wind tunnel of the open-circuit type, which is designed for a velocity of 200 miles per hour. The Bureau of Standards at Washington is constructing a wind tunnel with a throat diameter of 3.25 meters. This wind tunnel is novel in that it is built in the open without any housing. The wind tunnel is well surrounded by trees and hills to prevent as far as possible the atmospheric conditions affecting the air flow.

The three-dimensional balance designed by Dr. A.F. Zahm for the Washington Navy Yard wind tunnel has proved very satisfactory. The weights of this balance are automatically actuated by electrically driven lead screws, and the time of making a test is much shorter than with other types of balances.

FREE FLIGHT.

The committee now has in use for aerodynamic research at Langley Field five airplanes; three *JN4H's*, one *VE-7*, and one Thomas-Morse *MB3*. The *JN4H* has been used by the committee extensively in experimental work, mainly because of its strength and the economy of operation. During the past year the flying time of the airplane has been 110 hours, representing 260 flights. Fifty-two per cent of the flying time has been used in actually making measurements in the air. No accidents of any kind have occurred with the committee's airplanes. One forced landing was made due to the sticking of the carburetor float during violent stunting, but the airplane was brought down without damage to itself or the instruments which it contained. Although complete airplanes have not as yet been constructed by the committee, a number of parts, such as wings, tail surfaces, etc., have been designed and constructed at Langley Field for use in free flight research.

INSTRUMENTS.

A number of new pieces of apparatus have been constructed for the wind tunnel, including a machine for forming plaster wings, a new micromanometer, a light balance for measuring the moments of control surfaces, and an instrument for measuring the rolling velocity of wings. It has become more and more evident, as the discrepancies between free flight and model tests have been discovered, that it is necessary to produce in the wind tunnel a slip stream comparable with that on the full-sized airplanes. A very small flexible shaft has been developed which is able to drive the model airplane propeller up to speeds of 30,000 revolutions per minute, which corresponds to the normal speed of a full-sized propeller. The flexible shaft is so small that it disturbs the air flow inappreciably and in this respect is superior to an electric motor or a turbine.

It is realized that all free-flight data must be obtained by recording instruments, first, because events happen so rapidly that observations are difficult to make, and secondly, because the observer is under rather a nervous strain and can not take observations as accurately as he could in the laboratory. For this reason the committee has designed and constructed a considerable number of standardized recording instruments, electrically driven and synchronized, for taking records on interchangeable film drums. With these instruments the only duty of the observer is to change the drums at the end of the record, for the pilot can start and stop all of the instruments with a single switch. The following instruments have been constructed and used during the year:

(1) A new accelerometer more compact and accurate than the previous model.

(2) A recording air speed meter with a high natural frequency and small friction.

(3) A new model of a kymograph.

(4) A multiple manometer which will record on a moving film 30 simultaneous records of varying pressures. The natural frequency of this instrument is very high and the volume does not change appreciably with changes in pressure, which is a very important fact when recording pressures through long tubes.

(5) An instrument for recording annular velocities about a single axis.

(6) A control position recorder for three controls.

(7) A balance for measuring the forces on a trailing wing in flight.

The aeronautic instruments section of the Bureau of Standards has been engaged in an extensive program of research and development work on aircraft instruments in cooperation with the National Advisory Committee for Aeronautics, the Army, the Navy, and to a more limited extent with other Government agencies and private concerns. In addition to the experimental investigations and the development of new instruments a considerable amount of work has been carried out in connection with the routine testing of service instruments.

The investigation of the altitude effect on air speed indicators undertaken at the request of the National Advisory Committee for Aeronautics has been continued and extended. The experiments have been conducted in an improved wind tunnel with a 16-inch throat and mounted in one of the Bureau of Standards altitude chambers. With this apparatus valuable data have been obtained at speeds up to 100 miles per hour and under conditions of pressure and temperature corresponding to altitudes up to 30,000 feet.

Research concerning the action of diaphragms and Bourdon tubes undertaken at the request of the National Advisory Committee for Aeronautics has been continued with the purpose of determining the laws of deflection and of obtaining essential information of value in the design of instruments involving the use of diaphragms and Bourdon tubes.

A series of eight reports dealing with the various aeronautic instruments has been prepared for the National Advisory Committee for Aeronautics and will be found in the Seventh Annual Report.

At the request of the Army and the Navy, the development of the following instruments has been undertaken:

An improved aircraft sextant.
An improved compass.
An improved precision barometer.
A precision altimeter compensated for air temperature.
A precision barograph.
An improved rate of climb indicator.
An improved rate of climb recorder.
A combined statoscope and rate of climb indicator.
A synchronizing type ground speed indicator.
An astronomical position finder.
A horizontal angle indicator.
An improved centrifugal tachometer.
An air speed indicator for dirigibles.
A ballonet volume indicator for dirigibles.
Standard testing sets for field use.

Pursuant of the policy of following the latest developments in aeronautic instruments in foreign countries, a member of the aeronautic instruments section was detailed to investigate the recent developments in England, France, Italy, and Germany. This work was carried on in cooperation with the National Advisory Committee for Aeronautics representative in Europe and our military, naval, and commercial attachés, and much valuable information has been obtained.

A carbon pile tensiometer is being developed for the Navy which allows the accurate recording of tensions at a distance. An instrument has been devised by the Navy for the measurement of the ground speed of an airplane at frequent intervals of time on taking off or landing.

AEROFOIL TESTS.

During the past year the committee has conducted a large number of aerofoil tests in its 5-foot wind tunnel at Langley Field. The main object of these tests was to study the properties of thick aerofoils suitable for internal bracing. The tests were made at 35 meters per second, and in some cases as high as 60 meters per second as it was found that thick wings improve in efficiency with the speed more rapidly than thin wings. Some of the sections developed had at all angles a higher efficiency than the R.A.F. 15 section tested under the same conditions, and yet were more than three times as thick as that section in the center, while the maximum lift coefficients were approximately the same. A number of wings were tested which tapered in plan form, and it was found, contrary to expectations, that heavily tapered wings had the same center of pressure travel and practically the same efficiency as wings of uniform section.

The distribution of pressure was studied over 12 thick aerofoils of various types in order to determine the loading along the spars when the section varied along the span. A new method was devised for constructing pressure distribution models with comparatively little expense.

The effect of placing an aerofoil close to a flat surface representing the earth was thoroughly investigated both at the Massachusetts Institute of Technology and at the Washington Navy Yard. It was found that there was a remarkable increase in efficiency of the wing when close to a flat surface, which accounts for the fact that certain airplanes float for such long distances before landing.

Work has been continued in the McCook Field wind tunnel on various aerofoils at very high speeds, and a further study of vortex motion has been made.

Perhaps the most interesting work which has been carried out on aerofoils is that done by the committee in the testing of large aerofoils when suspended beneath a flying airplane. The aerofoil, constructed in the same way as an ordinary airplane wing of wood and fabric, is pulled up against the lower side of the fuselage in taking off, and when in the air is lowered down by means of a windlass to a distance of 20 or 30 feet or as far as is necessary to got out of the influence of the downwash. The magnitude of the resultant force is measured by a balance in the fuselage and the angle at which the wing trails back from the vertical measures the angle of the resultant. From these figures the lift and drag can be easily computed. At present only small wings of 6 feet span have been tested in this way, but it is evident from the great steadiness with which they trail beneath the airplane that accuracies probably as great as those obtained in the wind tunnel can be reached, although it is necessary to fly in smooth air for this kind of work. The results from the present apparatus although only of a preliminary character show such a good agreement with high speed wind tunnel tests of the same section that it is proposed to use a large bombing machine and trail wings of 30 feet span beneath it. Tests of this nature have not only the same Reynolds number, but also the same velocity, the same size, and the same amount of turbulence as the full-sized airplane, so that the results can be used by designers with perfect confidence.

A number of aerofoil sections have been tested, among which were several of the Göttingen series. The Washington Navy Yard tests check the Göttingen tests as closely as could be expected, the general types of the characteristic curves being very similar in every case. The Göttingen aerofoils tested were: Nos. *173*, *255*, *256*, and *822*.

Tests for scale effect have also been made on the *R.A.F. 15* and *R.A.F. 19* aerofoils.

STRUTS.

An interesting investigation has been made at the navy yard wind tunnel in Washington in the distribution of pressure over a strut. It is concluded that the total drag of the strut is the small difference between the upstream and downstream drag, so that a small error in measuring these will cause a huge error in the total.

STABILITY.

A very complete investigation has been made of the oscillations in flight of the *VE-7* and *JN4H*, the latter airplane with a special tail plane to make it statically stable. The results on the whole are in poor agreement with the theory, due mainly, it is believed, to the fact that the oscillations are large, often over 60 degrees, and that the slip stream has a considerable influence.

Considerable work has been done on static stability and it is becoming more and more evident that the aspect ratio of the tail plane has by far the greatest influence on the stability. It has also been found in actual flight that complete static stability may be obtained when the load is positive upon tail surfaces at all times. A study of the distribution of pressure over the tail surfaces of this surface in steady flight has given valuable information as to the functions of this surface in producing stability.

The lateral stability derivatives Y_v, L_v, and N_v have been determined in free flight for the *JN4H* and comparison has been made with the results from wind tunnel tests. On the whole the agreement is good, the discrepancies being mainly due to the influence of the slip stream and to the fact that in the model the control surfaces were assumed to be in a neutral position, whereas actually they were at a considerable angle.

A mechanical device has been constructed which will illustrate in every particular the dynamic and statical stability of an airplane. By the adjustment of weights the effect of changing the mass, the moment of inertia, the damping, etc., can be produced at will. As yet it has not been possible to obtain any quantitative value for stability with this instrument, but it is hoped that it may be used for quickly finding the stability properties of a new airplane from its known characteristics.

Tests have been made by the Navy on a series of balanced control surfaces with various types of balance. The characteristics of the type in which the axis is placed aft of the leading edge of the movable surface have been investigated at some length.

STRESSES IN FLIGHT.

The distribution of pressure was determined over the horizontal tail surfaces of a *JN4H* during all types of maneuver. In no case did the maximum loading on the tail exceed 6 pounds per square foot, and contrary to the usual expectations this load was in an upward direction. A theory has been devised which will give the loading on the tail surfaces in close agreement with the actual measurements.

The distribution of pressure over the rudder and fin have also been investigated on the same airplane and it was found that the heaviest loads occur in a roll where the loading may go as high as 10 pounds per square foot. It is interesting to notice from the standpoint of fuselage design that the maximum load on the horizontal tail surfaces, the maximum load on the rudder and fin, and the maximum load on the wings may all occur at the same time.

The recent development of very high speed airplanes has shown that very large unexpected loads may occur on the wing surface, several instances causing the stripping of the fabric or crippling of the trailing edge. A Thomas-Morse single seater which has a speed of over 160 miles an hour has been fitted up for measuring the distribution of pressure over the wing surfaces. It is hoped to determine the pressures both in steady flight and during violent maneuvering, and for this purpose the wings have been especially strengthened.

CONTROLLABILITY.

The measurement of and the design for controllability are very important problems and ones which have received but scant attention. In fact, the very definition of controllability is at the present time stated vaguely. The committee is now making an attempt to find some accurate quantitative means of measuring the controllability of various airplanes and to find the effect on controllability of various changes in control surfaces.

The desire for high speed has led many designers to eliminate the external bracing on the horizontal tail surfaces and for this reason a number of airplanes have been constructed with rather thick sections for the tail surfaces. Several airplanes of this type have been found by pilots to be extremely sluggish in responding to the controls; that is, for a certain range about the neutral position the controls have no effect. This condition was investigated in the wind tunnel on a tail plane of this type, and it was found that the elevator must be moved several degrees on either side of its neutral position before the force on the tail is appreciably changed, due to the fact that the elevator seemed to be in the shadow of the thicker portion of the tail surfaces and could have no effect until it was turned out into the free air stream.

The angular velocity and angular accelerations have been measured on a *JN4H* during all types of maneuver, in order to provide designers with data which will be of use in construction of airplanes.

The subject of control, especially lateral control, at low flying speeds has received some attention. It is evident, however, that different airplanes, although varying only slightly in external characteristics, vary tremendously in the amount of lateral control which they have at the stalling speed, and an explanation of this would be of great value. The Navy has recently devised an entirely new type of lateral control which in wind tunnel tests shows great promise.

AIRSHIPS.

Several types of external-pressure pads developed by the Navy have been tested upon the wind of an airplane at Langley Field in order to assure that such opening when cemented to the outside of the wing will give the same reading as a flush hole. One type of pad has proved to be very successful. The possibilities have been considered of measuring the pressure over the surface of an airship during accelerated flight, and as yet no satisfactory method has been devised for entirely eliminating the rather large errors due to the forces acting upon the air column in the long connecting tubes which are necessary in this experiment. The investigation, however, has not be abandoned, and it is believed that the difficulties will be overcome.

Extensive tests have been made on two models of the rigid airship *ZR-1*. These tests were made on the hulls, bare and with six types of control surfaces.

Tests have been conducted at the navy yard wind tunnel in Washington on the effect of fineness, ratio, and length of parallel middle body on airship forms.

PROPELLERS.

Experiments have been conducted in the wind tunnel to measure the drag of various propellers under various degrees of yaw and with different amounts of braking. The drags of propellers are rather small so that the possibility of the save vertical descent of the helicopter without power does not look very probably if the usual type of propeller is used. Test have also been conducted upon a helicopter propeller having blades which are automatically set at a constant angle of attack by means of individual tail planes.

An extensive investigation has been carried out at Leland Stanford University on the properties of propellers at angles of yaw. The results look very promising in connection with the horizontal travel of helicopters, as a considerable horizontal thrust may be obtained with no more power than is required in ordinary flight.

BOMBS.

The Bureau of Standards has been conducting a very extensive investigation of bombs and projectiles not only in their 150-mile wind tunnel but also in a 12-inch air stream from a high-power compressor where speed can be obtained above the velocity of sound. Some very interesting conclusions have been reached in connection with stream lining at very high speeds.

Document 2-14

J. C. Hunsaker, memorandum to NACA, "Recommendations for Research Program—Comparison of Wing Characteristics in Models and Free Flight," 10 November 1920, RA file J, Historical Archives, NASA, Langley.

One of the NACA's most important early research programs began in 1920 after Jerome Hunsaker recommended that the committee pursue a systematic program comparing the wing characteristics of wind tunnel models with those of identical, but full-size, wings in free flight. This proposal, which the NACA embraced and carried out at Langley, led to the establishment of new research methods and produced some of the first reliable data comparing actual and model airplanes. Interestingly, the agreement between model and actual wings was generally good, but the fuselage comparisons revealed considerable discrepancies. Understanding why would require several more years of experiments and analysis and would ultimately help justify larger wind tunnels at Langley. (Note: The duplication of paragraphs numbered "5" in the report is from the original.)

Document 2-14, Jerome Hunsaker to NACA, "Recommendations for Research Program," 1920.

November 10, 1920

From: J. C. Hunsaker, Commander (C.C.), U.S.N.
To: National Advisory Committee for Aeronautics, Room 2722, Navy Building.
Subject: Recommendations for Research Program—Comparison of Wing Characteristics in Models and Free Flight.

1. It is recommended that the contemplated research on comparison between model and full-scale airplanes be made as thorough as possible in order to furnish accurate and complete data on which we may base performance estimates. A research of this nature should include a study of at least three aerofoil sections which are widely separated in their characteristics. The sections recommended by Mr. Norton, i.e., Curtiss (JN-4), R.A.F.-15, Albatross, and U.S.A.T.S.-5 seem to answer all requirements. The problem would be somewhat simplified if a Parasol type of monoplane were available, but perhaps more information may be obtained from a study of a biplane such as the JN4H now available.

2. It is suggested that very accurate scale models of the component parts of this airplane be constructed and tested in the tunnel at various speeds, singly and in combination, with a view to determine the probable scale effect and the interferences.

The wing combinations should be tested carefully and investigated analytically by means of the Gottingen equations (Prandtl and Munk). These equations give very satisfactory results and it is thought that additional constants should be determined for all cases in common use. The German tests have not undertaken a study of tapered wings and, in view of the probable importance of this feature in internally braced designs, it is suggested that a study be made of taper in monoplane and biplane combination as a parallel work. Reference is made to Munk's article in Technische Berichte II-2, for the method of determining the constants.

3. The resistance derivatives should be determined for this machine by experiments on the model and also by calculations based on considerations of the design. An attempt should be made to arrive at some conclusion in regard to the best way to determine each derivative, it being well known that certain derivatives can not be obtained with a necessary accuracy from a direct test. In this respect it is recommend that the findings of Mr. O. Glauert as reported in "Aircraft Engineering" during 1920, be given careful consideration.

4. Additional wind tunnel tests will be suggested by the results of these investigations and should be made in view of the specific requirements.

5. In the full-scale, or free-flight, tests there are several outstanding problems such as:

(a) Determination of the variation of lift and drag coefficients with angle.

(b) Pressure distributions on wings and perhaps on tail surfaces (in addition to the research now under way).

(c) Efficiencies of control surfaces.

(d) Forces on tail surfaces and effect of down wash from different wings on balance and stability.

(e) Stability.

5. The variation of lift and drag with angle should be determined by the usual method of timing a horizontal steady flight over a measured course and checked by gliding flights with the propeller stopped (see Br. A.C.A. R&M Nos. 541 and 603). A method of analyzing the performance in a climb may be found in Zeitschrift F.u.M. June 30, 1920, and it is suggested that a similar scheme be used in this research.

6. It seems desirable to actually determine the effectiveness of the control surfaces by introducing a known moment to be counteracted. This has been done in some German tests reported in Zeitschrift F.u.M. for November 15, 1919, and November 29, 1919. The results apparently justify further experimentation.

7. The study on efficiencies of and forces on control surfaces should determine the variations due to thickness and plan forms of these surfaces. Some very good work has been done in this field and the present research should be made with the view of completing such work.

8. Free flight stability testing has been limited to determinations of period, damping, etc. The previous work of the Advisory Committee furnishes a very good foundation for the present investigation.

9. A comparison of the various reports on free flight tests is sufficient to emphasize the importance of accurate data. Special attention should be given to the determinations of velocity, density, thrust, weight, and center of gravity location. This research will require an immense amount of work, but the results will justify the efforts providing care is taken in planning and executing every test.

[signed] J. C. Hunsaker

Document 2-15(a–i)

(a) Joseph S. Ames, vice chairman, committee on aerodynamics, NACA, letter to A. [F.] Zahm, L. J. Briggs, E. B. Wilson, W. F. Durand, E. N. Fales, J. G. Coffin, H. Bateman, and F. H. Norton, 23 August 1920, RA file 70, Historical Archives, NASA, Langley.

(b) F. H. Norton, acting chief physicist, NACA Langley, response to Joseph S. Ames, 26 August 1920, RA file 70, Historical Archives, NASA, Langley.

(c) A. F. Zahm, Washington Navy Yard, response to Joseph S. Ames, 17 September 1920, RA file 70, Historical Archives, NASA, Langley.

(d) Joseph G. Coffin, Curtiss Aeroplane & Motor Corporation, response to Joseph S. Ames, 18 September 1920, RA file 70, Historical Archives, NASA, Langley.

(e) W. F. Durand, Leland Stanford University, response to J. S. Ames, 24 September 1920, RA file 70, Historical Archives, NASA, Langley.

(f) Ludwig Prandtl, University of Göttingen, letter to William Knight, NACA Technical Assistant (translation), the NACA, 1 May 1920, RA file 70, Historical Archives, NASA, Langley.

(g) "International Standardization of Wind-Tunnel Results," ***NACA Annual Report for 1922*** **(Washington, DC), p. 36.**

(h) G. W. Lewis, director of aeronautical research, NACA, memorandum to Langley Memorial Aeronautical Laboratory, 1 April 1925, RA file 70, Historical Archives, NASA, Langley.

(i) Aerodynamics Department, The National Physical Laboratory [Great Britain], "A Comparison between Results for R.A.F. 15 in N.P.L. Duplex Tunnel and in the N.A.C.A. Compressed Air Tunnel," (summary), n.d. [1925], RA file 70, Historical Archives, NASA, Langley.

As the number and variety of wind tunnels around the world increased, researchers became more concerned that measurements taken in one tunnel might not be reproducible in other tunnels. In 1920, the British Advisory Committee for Aeronautics (ACA) spearheaded an effort to run a series of tests to compare the performance of the world's major wind tunnels. The NACA Committee on Aerodynamics readily endorsed the concept, but it was not sure just what should be measured or how the program should be structured. In an effort to obtain broad input and achieve some consensus in the American aeronautical community, the aerodynamics committee's vice chairman, Joseph S. Ames (for whom the Ames Laboratory would later be named), wrote to eight leading aerodynamics figures, asking them "to outline a program of tests to be made in the wind tunnels of this country and of Europe" that would enable researchers to "connect" data from different tunnels. Responses from F. H. Norton, A. F. Zahm, J. G. Coffin, and W. F. Durand, reproduced herein, uniformly expressed interest in such an effort, but there are interesting differences in their suggestions for the program. Ames received advice and comments from other noted well-known aeronautics personages as well, including Ludwig Prandtl, who offered Ames his suggestions earlier in the year.

The NACA prepared specifications for the American tests and, as the "International Standardization of Wind-Tunnel Results" section from the *NACA Annual Report for 1922* shows, participated fully in the project. Although the standardization tests were initially expected to be a one-time, straightforward proposition, the project rapidly grew in complexity, and the NACA was an active participant in tests for many years. Research Authorization (RA) 70, the in-house authority for the program and the source file for most of these documents, became one of the longest-running projects on the committee's books.

The NACA and ACA published numerous reports that described the various tests and analyzed results. The final document in this section is the summary from a British report by the Aerodynamics Department of the National Physical Laboratory comparing results from the tests of a standard wing section in the N.P.L. Duplex tunnel and the new Variable Density Tunnel (VDT) at Langley. The British expression of confidence in the American VDT is of particular interest in this document.

In the final analysis, however, standardization remained a dream that was never fully realized because of the widely varying capabilities of the world's wind tunnels and the complexities inherent in wind tunnel testing. In another sense, however, the standardization project was a success in that it focused the greatest minds in aerodynamics around the world on the fundamental questions of wind tunnel testing and stimulated an unprecedented degree of cooperation and information exchange.

Document 2-15(a), Joseph S. Ames, letter regarding standardization of wind tunnels, 1920.

August 23, 1920

My dear Sir:

At the last meeting of the Subcommittee on Aerodynamics of the National Advisory Committee for Aeronautics, I was requested to ask you if you would be kind enough to outline a program of tests to be made in the wind tunnels of this country and of Europe with a view to securing what one might call standardization, that is, information which would enable one to connect the data published, as obtained in these different wind tunnels.

This request is being sent to several others, and after replies are received from all, which I trust will be at an early date, I will see that a comprehensive program is prepared for submission at the next meeting of the committee.

Sincerely yours,
NATIONAL ADVISORY COMMITTEE FOR AERONAUTICS
[signed] Joseph S. Ames
Vice-Chairman, Committee on Aerodynamics

Copies to A. H. Zahm, L. J. Briggs, E. B. Wilson, W. F. Durand, E. N. Fales, J. G. Coffin, H. Bateman, and F. H. Norton.

Document 2-15(b), F. H. Norton, reply to Joseph S. Ames, 1920.

August 26, 1920.

From: Langley Memorial Aeronautical Laboratory.
To: National Advisory Committee for Aeronautics.
(Atten. Vice-Chairman, Com. on Aerodynamics)
Subject: Comparison of wind tunnels.
Reference: (a) NACA Let. Aug. 23, 1920, and enclosures.

1. It is evident that for a given model, a given angle of incidence and a given velocity there is only one correct value for Lc and Dc. Tests with the same model in various wind tunnels will not necessarily give these correct values but they will show any large error for any one particular tunnel.

2. The purpose of these tests should be to determine factors for converting past, present and future results, at least approximately, to agree with similar results obtained in any one tunnel. These tests should also determine the merits of the various types of wind tunnel.

3. The main classes of errors which may arise in wind tunnel work are:—First, errors in speed of wind; Second, errors in measuring the forces; and Third, errors in determining the true resistance of the model exclusive of its supports.

4. It is advisable in tests of this nature to make the models as simple and as few in number as possible, and it would seem to me that a standard type of aerofoil would be best suited for this purpose, and I believe that a single test would show everything that would be shown by a more extensive series of tests. In order that the aerofoil may be used in the smaller tunnels it should be made with a span of 18", and a chord of 3" and should be run at a speed of 40 feet per second. It has been suggested that it might be desirable to make tests on a complete model but I do not believe that any more information would be obtained in this way than from the simple aerofoil and the model would have the disadvantage that even a very slight misalignment of the wings would introduce a considerable amount of error in the results.

5. It will be very desirable to determine the steadiness of flow both in direction and velocity for various tunnels. A method for accomplishing this in a rough way has been suggested by using a sphere or cylinder for the test but this method can not give quantitative results. It is suggested that a very complete and valuable record would be obtained by using a recording air speed meter and recording yawmeter in order to obtain records of the actual variation in the flow of each tunnel. Such an instrument has been used very successfully at Langley Field and it is suggested that the same instrument should be used in all tunnels.

[signed] F. H. Norton
F. H. Norton,
Acting Chief Physicist.

Document 2-15(c), Albert F. Zahm, reply to Joseph S. Ames from Washington Navy Yard, 1920.

September 14, 1920

Dear Prof. Ames:

Your letter of August 23 arrived while I was away on vacation.

One of the main objects of wind-tunnel research is to determine the action of the air on and about a model in a stream of indefinite extent flowing uniformly without a pressure gradient, except that caused by the model.

I would suggest first that a very few of the ablest theoretical aerodynamists, such as Prof. Prandtl, be invited to discuss the mathematical theory of the flow in a wind tunnel—not too ideal for practical use—both when empty and when containing a model; and to indicate what corrections should be made to render the wind-tunnel data applicable to a model in a uniform infinite stream, or to a model moving uniformly through an infinite still atmosphere. Prof. Prandtl has written somewhat on this subject, and Dr. de Bothezat has expressed a wish to do so.

After that I would suggest that a few laboratories be invited to determine the action of the air on and about a few very simple models in their tunnels, and thence, after their own peculiar corrections, to derive the air action on the same models in a uniform infinite stream. If the final results agree, the methods of standardization are provided.

The careful testing of a variety of identical models in a variety of tunnels differently manned would probably yield results a little more consistent than those already available. But unless the tests were guided by adequate theory, furnished before hand, it seems improbable that all the observations and precautions would be taken that are necessary to make wind tunnel data strictly comparable.

In case the comparative tests are to begin at once, I would suggest as models a sphere and a thin circular disc in normal presentation to the wind, preferably with its edge champfered [sic] on the back; both to be of specified dimensions. In each case I would have the velocity and pressure distributions, for various fixed wind speeds, mapped throughout the working part of the tunnel both when vacant and when containing the model. The resultant air force on the model, and the pressure distribution all over its surface should be determined. It should be required also to report the temperature and moisture of the air in the tunnel during the test, so that the density and viscosity may be known. Finally, correction should be made to derive the resultant air force on the model in a uniform infinite stream.

In both cases a number of spheres and discs, as nearly geometrically similar as may be feasible, should be used as a check in finding the V1 or scale effect.

If these tests are to be made to establish trustworthy doctrine, rather than to expose the defects of busy routine tunnels, I would suggest that they be limited to research laboratories, with ample equipment, and supervised by high-grade men with sufficient leisure.

Very truly yours,
[signed] A. F. Zahm.

Document 2-15(d), Joseph G. Coffin, reply to Joseph S. Ames from Curtiss Aeroplane Corporation, 1920.

September 18, 1920

Dr. Joseph S. Ames,
National Advisory Committee for Aeronautics,
Washington, D.C.

Dear Sir:—

Your communication of August 23rd must have gone astray as I have received but that of September 14th.

I am extremely interested in the question of standardization tests or rather comparison of tests in various wind tunnels. The following suggestions show in a general way the kind of tests that in my opinion would be useful.

1. Tunnels should show uniform air flow over experimental section as tested by standard type of Pitot.

2. Tests on any standard rings should give identical curves (within experimental area) when reversed.

3. Balance should be checked up for absolute forces.

If these three conditions are satisfactorily met, then:

1. Comparison of results should be made on a standard airfoil. This airfoil should be a species of primary standard sent around from place to place somewhat as an invariable pendulum is used in the determination of "g". It should be of metal to withstand high air speeds.

2. Tests should be made on similar airfoils of geometrical constructions such as shown in sketch below, for example, which can be constructed mechanically and checked up at the various laboratory shops.

3. Comparison tests of an airplane model of "invariable" all-metal type construction.

4. Resistance at zero yaw of streamline airship model which is of a fairly large size, (volume). This brings in the pressure gradient correction, standard surface and shape.

5. No uniformity can be expected unless great and careful attention is paid to spindle and attachment location corrections. These are extremely troublesome and in my opinion deserve the greatest amount of attention at the present time.

6. Tests on a sphere and a cylinder.

7. Attention must be paid to nature of surface. Surface has a model characteristic.

I have had constructed a very carefully made aluminum wing with a RAF-6 upper camber and an absolutely flat under camber. It is provided with holes for end spindle attachment at either end and also for crank spindle attachment at the center. Comparison results with this wing have already been made by M.I.T. and the Bureau of Standards. The results are very interesting.

By sending around an invariable standard wing we eliminate possible surface differences, spindle attachment location differences, as well as differences due to the spindles themselves. Special spindles accompany the model. I believe the Curtiss Company would be glad to cooperate by allowing the use of this standard.

8. Tests on pressure gradients along the tunnel should be made.

9. Tests should be repeated at various air speeds.

After all, what is desired is that any given tunnel, when properly used, should give identical test results. This standardization would go a long way toward attaining this ideal.

I would like to say also that a new type of balance should be developed. I have in mind such a balance which would eliminate to a great extant the difficulties with spindle corrections.

Very truly yours,
[signed] Joseph G. Coffin

Document 2-15(e), W. F. Durand, reply to Joseph S. Ames from Stanford University, 1920.

24th September 1920.

Dr. J. S. Ames
Chairman, Aerodynamic Com.
N.A.C.A.
Washington, D.C.

My Dear Dr. Ames,

I have just returned from a summer trip to the Hawaiian Islands, and find your letter in regard to experimental work in aerodynamical laboratories looking toward a standardization of results. While in the Islands I was beyond reach of mail, and this must be my excuse for the long delay in my reply.

The subject of your inquiry is one in which I have felt much interest and it has seemed to me that the general program of standardization involves two principal features:

(1) The adoption of a system of standard dimensionless units and the expression of results of laboratory research in terms of such units. Already a good start has been made by the N.A.C.A. in this respect, in the work of the technical data division, in reducing to standard terms and forms, results of miscellaneous laboratory research work in this country and abroad.

(2) Some program of test or standardization which would, in effect operate as a test on laboratories and laboratory equipment. Thus a test is made in laboratory A on a series of airfoil sections specified in terms of a drawing or a series of ordinates. Laboratory B desires to check up on Laboratory A and attempts to reproduce the same section. It is very sure that the reproduction will not be exact and may even differ to such a degree as to seriously compromise the comparison. Similarly with

propeller tests. In order to make our results strictly comparable with those made in other laboratories, notable by Eiffel and at the N.P.L., there should in effect be a standardization of laboratory equipment—balance, dynamometers, wind speed measures, etc.

Just how to accomplish such a standardization or comparison in the most effective manner is not entirely clear, but I believe that something could be accomplished by carrying out a series of tests on a series of carefully selected type models, made of metal and therefore invariable in form, and sent bodily from one laboratory to another. Thus if a persistent difference develops as between two laboratories in measuring the same thing, it must obviously be inhere in the laboratory equipment. Or again, if in a dozen laboratories all but one or two are in sensible agreement regarding certain measurements, the presumption is that in the divergent cases some error traceable to the equipment or mode of carrying on the measurements has been introduced. A search for this source of error should then serve to definitely clear up the matter and establish all laboratories on a uniform basis at least as regards this particular feature.

Of course in all laboratory equipment, time changes develop and certain standard forms should be requested from time to time in order to make sure that no secular change of importance is in progress.

The particular features regarding which such laboratory standardization should be carried out are clearly the following:

(a) Air foil sections:
 Lift Force.
 Drift Force.
(b) Models of planes:
 Forces as above.
 Tipping movements.
(c) Propellers:
 Torque.
 Thrust.
 R.P.S.
(d) Wind Speed:
 This is fundamental in all tests.

Regarding (d), I have thought that a special form of wind speed measuring device might be devised, which could be sent bodily from one laboratory to another, and which, through it’s indications in comparison with the method or device used at each laboratory, might serve to give a comparison or a comparative calibration of one laboratory in terms of another.

Regarding (a), (b), and (c), standard metal forms and a standard program of test might be devised, the carrying out of which in each laboratory would thus serve to check one against another and to develop cases which might be specially divergent from the others.

There is much in the way of detail here which would have to be carefully studied, but I believe that something might be done, especially through the agency of a central body such as our Committee.

Hoping that something may develop along these or equivalent lines, and with assurances of our desire to co-operate in all ways practical, I am,

Sincerely yours,
[signed] W. F. Durand

P.S. I have not attempted to discuss this matter in detail, in the thought that a general understanding of the field to be covered and of the general grand strategy would first be desirable.

W.F.D.

Document 2-15(f), Ludwig Prandtl, letter to William Knight from Göttingen, Germany (translation), 1920.

May 1st 1920.

To: Mr. William Knight, Technical Assistant, U.S. Commission for Aeronautics, American Commission, Wilhelmsplatz 7, Berlin.

My Dear Mr. Knight:

I hope you reached Berlin safely. I want to make definite proposals at once for the comparisons to tests made at various laboratories. The following five experiments appear to be, in my opinion, the right ones to propose:

1) Measurement of resistance of a flat circular metal plate, set at right angles to the air flow in order to test the measurement of velocity in the laboratory.

2) Measurement of resistance of a smooth sphere, diameter 20 cm., made as exact as possible, to test the eddying of the air current.

3) Measurement of resistance of a metal wing of about 20 x 100 cm., equipped with the proper fastenings for all the laboratories, which will be sent from one laboratory to another.

4) Measurement or resistance of a wing as constructed from the same drawing by each laboratory, with its own manufacturing means. The drawing may be identical with the one used in experiment 3.

5) Measurement of resistance of a dirigible balloon body model, which may be made of polished wood, to test the uniformity of the air flow lengthwise. (If speed increases down stream, the measurement of resistance will be too large, and vice-versa.)

I shall be obliged if you will negotiate with the various laboratories on the basis of this program, if you agree to it, and collect the statements of the separate laboratories as to the fastenings they require (hooks, screw sockets, etc.) for each model. The models to be turned out in common can then be produced in accordance with those specifications.

I have looked over my old studies, and find that a reprint of only one of the papers you are concerned with is missing; i.e., a paper given at the International Congress of Mathematicians at Heidelberg, 1904. It was a particularly important paper, at least from a historical point of view. If you attach importance to it, you can still probably purchase the Proceedings of that Congress (1905) published by Teubner, Leipzig.

Some other important studies done at my old laboratory deserve your attention. They appeared in the Jahrbuch der Motorluftschiff-Studeingesellschaft 1910/11, 1911/12, and in the Jahrbuch der Luftfahrzeugegesellschaft 1912/13 (Springer, Berline). Everything else of consequence is to be found in the Technische Berichte, or you can get reprints of the papers in questions.

I send you my best regards and hope to hear from you.

[signed] L. PRANDTL.

Document 2-15(g), "International Standardization of Wind-Tunnel Results," NACA Annual Report for 1922.

INTERNATIONAL STANDARDIZATION OF WIND-TUNNEL RESULTS.

During the past year, the committee has entered into an agreement with the Aeronautical Research Committee of Great Britain, through the National Physical Laboratory, to arrange for the conduct of certain definite tests in the wind tunnels of the world. The tests are to be made on standard airfoil and airship models which have been designed and constructed by the National Physical Laboratory. The National Advisory Committee or Aeronautics undertook to arrange for the test in the wind tunnels of the United States. In September 1922, the committee received from the National Physical Laboratory two airship models for comparative tests. These models have been tested under the direction of Dr. A. F. Zahm, at the aeronautical laboratory of the Washington Navy Yard.

The National Advisory Committee has further authorized the testing of standard models in the United States, the models consisting of three cylinders, having length-diameter ratios of 5 to 1, and four models of the U.S.A. 16 wing section, each having an aspect ratio of 6 and the length varying from 18 to 36 inches. The tests on both cylinder models and wing models are to be made over as wide a range of V/L as possible, and to include determinations of lift, drag, and pitching moments every 4° from -4° to +20°. The streamline airship models to be tested will have the proportions of the Navy "C" class airship described in a recent report of the Washington Navy Yard wind tunnel. Four streamline airship models, of 4, 6, 9, and 12 inches diameter, respectively, are to be tested, and are to be supported by spindles of lenticular form, the least diameter of spindle being one-twentieth the diameter of the model, and the fineness ratio of the spindle being 3. After completion of the test in the wind tunnels of the United States, the models will be sent to laboratories in European countries and to Canada for test.

Document 2-15(h), George W. Lewis, memorandum to Langley Memorial Aeronautical Laboratory, 1925.

From: National Advisory Committee for Aeronautics.
To: Langley Memorial Aeronautical Laboratory.
Subject: Results of tests of standard R.A.F. airfoil.

1. The Committee is in receipt of a letter from the Aeronautical Research Committee of Great Britain suggesting that each country publish independently the results of the wind tunnel tests on the standard N.P.L. models. It is further suggested that the results of the tests on the standard R.A.F. airfoil be published as soon as possible, as the Aeronautical Research Committee desires to issue at some future date a complete memorandum comparing the results in the different countries.

2. Before a meeting is called of the representatives of the various laboratories in this country in which the airfoil was tested, it seems desirable that the reports of these tests be circulated among those representatives. There is accordingly enclosed herewith a copy of the report of the Massachusetts Institute of Technology on the tests of the standard R.A.F. model. The report of the Bureau of Standards has already been sent to you, and the report of the Engineering Division of the Air Service will be forwarded as soon as received.

3. It is desired that a meeting of the representatives of the various laboratories be held during the first week in May to consider the preparation of the joint reports on the tests of this model.

G. W. Lewis,
Director of Aeronautical Research.

Document 2-15(i), Aerodynamics Department, National Physical Laboratory [Great Britain], "A Comparison between Results for R.A.F. 15 in the N.P.L. Duplex Tunnel and in the N.A.C.A. Compressed Air Tunnel" (summary), n.d. [1925].

It does not appear justifiable to use any full scale figures for further comparison, since the above results refer to a square-ended monoplane of 6:1 aspect ratio. The only important discrepancy revealed by the above comparison is that occurring at maximum lift, and this difference, at the point where the flow is critical, can not be considered surprising. The agreement, apart from this limited region is surprisingly good, when the extreme difference between the two types of tunnels is taken into consideration, and tends to establish a considerable amount of confidence in the compressed air tunnel results. The marked difference in lift at the stall needs further explanation, and future comparisons on other sections might throw some light upon it. Further information on this point is very desirable if the merits of the compressed air tunnel are to be fairly assessed, on account of the importance of a correct prediction of maximum lift to the designer.

Document 2-16

"Summary of General Recommendations," *NACA Annual Report for 1921* (Washington, DC) pp. 4–5.

By 1921, the NACA had the first portions of its Langley laboratory up and running, and the committee began to take its advisory role as seriously as it had its research role. The *NACA Annual Report for 1921* contained a fascinating section entitled "Summary of General Recommendations" wherein the committee made a number of specific recommendations for federal legislation and policies in support of aviation growth and safety. From what the committee termed "the most urgent need"—that of federal legislation to regulate all facets of aviation—to policies that would encourage and support the "aerological" (weather) service, air mail, aircraft manufacturers, helium production for airships, and "greater provision for the continuous prosecution of research on a larger scale," these recommendations became the nucleus of major federal legislation. The Air Commerce Act, enacted during the decade, and the ensuing regulations played a role in shaping aviation that was at least as important as the NACA's scientific and technical research programs.

Document 2-16, "Summary of General Recommendations," NACA Annual Report for 1921.

SUMMARY OF GENERAL RECOMMENDATIONS.

The more important general recommendations of the National Advisory Committee for Aeronautics are summarized as follows:

LEGISLATION FOR THE DEVELOPMENT OF AVIATION.

The most urgent need for the successful development of aviation at the present time, either for military or civil purposes, is the enactment of legislation providing for the Federal regulation of air navigation, and the establishment of airways and airdromes under Federal regulation. The Federal regulation should include the licensing of aviators, aircraft, and airdromes; the airways should consist of chains of landing fields providing supply and repair facilities and including the necessary meteorological stations, observations, and reports. If the Federal Government will establish and regulate transcontinental airways, as recommended, the committee is confident that air lines for the transportation of passengers or goods will be rapidly established by private enterprise in all parts of the country. The first national airways, however, should be carefully planned to serve military as well as civil needs. The committee reiterates its former recommendations as to the manner of accomplishing the desired results, and urgently recommends the establishment by law of a Bureau of Air Navigation in the Department of Commerce.

EXTENSION OF AEROLOGICAL SERVICE.

The committee emphasizes the importance of extending aerological service (under the Weather Bureau) along airways as established, and recommends that adequate provision of law be made for this service, which is so indispensable to the success and safety of air navigation.

POLICY TO SUSTAIN THE INDUSTRY.

Whatever may have been the faults or the shortcomings of the aircraft industry during or since the war, the fact remains that there must be an aircraft industry, and that it should be kept in such a condition as to be able to expand promptly and properly to meet increased demand in case of emergency. The Government, as the principal consumer, is directly concerned in the matter, and should formulate a policy which would be effective to sustain and stabilize the aeronautical industry and encourage the development of new and improved types of aircraft. In this respect the committee invites attention to the recommendation contained in its special report submitted to the President on April 9, 1921, published as House Document 17, and again recommends the adoption of a policy which, while safeguarding the interests of the Government, will tend to sustain and stabilize the industry.

IMPORTANCE OF MILITARY AVIATION.

Aviation is indispensable to the Army and to the Navy in warfare; and its relative importance will continue to increase. Other branches of the military services are comparatively well developed, whereas aviation is still in the early stages of its development. The demand for greatly reduced expenditures in the military and naval services should not apply to the air services. The committee recommends that liberal provision be made for the Army and Navy Air Services, not only that provision be made for the maintenance and training of personnel, but also that the funds be adequate to insure the fullest development of aviation for military and naval purposes.

SCIENTIFIC RESEARCH.

Substantial progress in aeronautical development, whether for military or commercial purposes, must be based upon the application to the problems of flight of scientific principles and the results of research. The exact prescribed function of the National Advisory Committee for Aeronautics is the prosecution and coordination of scientific research, and, while encouragement may be taken from the progress made, greater provision for the continuous prosecution of research on a larger scale is strongly recommended by the committee.

THE AIR MAIL SERVICE.

The Air Mail Service has demonstrated that airplanes can be utilized with certain advantages in carrying the mail. And it has done more than this, despite the handicap of using, military types of aircraft, poorly adapted to its work or to any civil or commercial purpose, in demonstrating that commercial aviation for the transportation of passengers or goods is feasible. There are several causes which are delaying the development of civil aviation, such as the lack of airways, landing fields, aerological service, and aircraft properly designed for commercial uses. The Air Mail Service stands out as a pioneer agency, overcoming these handicaps and blazing the way, so to speak, for the practical development of commercial aviation. As a permanent proposition, however, the Post Office Department, as its functions are now conceived, should no more operate directly a special air mail service than it should operate a special railroad mail service; but until such time as the necessary aids to commercial aviation have been established it will be next to impossible for any private corporation to operate under contract an air mail service in competition with the railroads. The National Advisory Committee for Aeronautics therefore recommends that provision be made for the continuation of the Air Mail Service under the Post Office Department.

HELIUM AND AIRSHIPS.

The United States has a virtual monopoly of the known sources of supply of helium, and these are limited. Experiments have been conducted by the Bureau of Mines with a view to the development of methods of production and storage, but as yet the problem of storage in large quantities has not been satisfactorily solved. Because the known supply is limited, because it is escaping into the atmosphere at an estimated rate sufficient to fill four large airships weekly, and because of the tremendously increased value and safety which the use of helium would give to airships, particularly in warfare, it is, in the opinion of the National Advisory Committee for Aeronautics, the very essence of wisdom and prudence to provide for the conservation of large reserves through the acquisition and sealing by the Government of the best helium-producing fields. Attention now being given to the development of types of airships to realize fully the advantages which the use of helium would afford should be continued. Such development would give America advantages, for purposes either of war or commerce, with which no other nation could successfully compete.

Document 2-17

F. H. Norton, "The National Advisory Committee's 5-Ft. Wind Tunnel," *Journal of the Society of Automotive Engineers* (May 1921): 1–7.

In spite of World War I and increasing discontent with its Langley Field landlord, the NACA continued work on its first facilities, a five-foot open wind tunnel modeled after Britain's four-foot National Physical Laboratory tunnel and a dynamometer laboratory for engine testing. The Atmospheric Wind Tunnel (AWT), which began operation in 1920, was an obsolete design even before it was built, but it was a proven commodity, and it allowed the NACA's technicians to finally get to work with their own equipment. During the dedication of the Langley Memorial Aeronautical Laboratory (LMAL) on 20 June 1920, praise was lavish. None other than former opponent David Taylor hailed the modest brick wind tunnel building as a "shrine to which all visiting aeronautical engineers and scientists will be drawn." Taylor's statement was a bit exaggerated for the time, but Langley soon began to chart its course and produce significant results. The following year, Frederick H. Norton, Langley's chief physicist, reported on the work being done in the AWT, noting that the work at Langley was "entirely research on the fundamental problems of aeronautics, such as the systematic design of airfoils, the scale effect on models, and the relation of the stability on models to that in free flight."

For one of its most important early programs, the NACA obtained a Curtiss JN4H Jenny for flight tests, and Langley model makers built two models of the plane for wind tunnel testing. One was used for tests measuring lift, drag, and moments, while the other was outfitted for pressure-distribution tests. The wind tunnel data then were compared to measurements obtained during flight tests of the JN4H. Gustav Eiffel had done the first such comparative tests a decade earlier in France, but the Langley program was more extensive and done with considerably greater precision. With such research programs, the NACA began to stake out a crucial role for itself.

Document 2-17, F. H. Norton, "The National Advisory Committee's 5-Ft. Wind Tunnel," 1921.

In the spring of 1919 work was started on a 5-ft. wind tunnel for the National Advisory Committee for Aeronautics at Langley Field, Va., and in the spring of 1920 the tunnel was completed and ready for calibration and for conducting tests. This tunnel has now been in operation for about one year and during this time new apparatus and equipment have constantly been added to increase efficiency and usefulness. While the tunnel is not as large as some that are now in use, it has, it is believed, the highest useful speed of any wind tunnel in the world, that is, it maintains a high velocity flow which is steady in speed and direction, and it possesses satisfactory means for measuring the forces on models at the highest velocities. At present, useful speeds up to 120 m.p.h. can be attained; but, as the propeller was originally designed for other conditions, it is estimated that with a new and higher-pitch propeller a maximum speed of 140 m.p.h. can be reached. Testing models at such high velocities is not a simple matter and a number of new methods and devices had to be developed to accomplish this successfully. For this reason a large portion of the time that the tunnel has been in active operation has been occupied in carefully studying the aerodynamic properties of the tunnel and in constructing apparatus for holding models at the high velocities which can be reached so that only in the last few months has the tunnel been devoted continuously to research work. From now on, the work of the tunnel will be devoted almost exclusively to tests on thick airfoils, including an investigation of their pressure distribution, and to study the stability of model airplanes.

TUNNEL AND BUILDING.

The wind-tunnel building, which is constructed substantially of brick and steel, is approximately 92 ft. long, 43 ft. wide, and 28 ft. high at the eaves. In Fig. 1 is shown a longitudinal section of the building and tunnel giving the principal dimensions of the structure. A heavy concrete foundation runs the whole length of the tunnel and a separate foundation is used for the power plant, so that any vibrations which may be set up by the propeller or motor are not directly transmitted to the tunnel or balance. The interior of the building is smooth and free from obstructions so that the return flow of air from the tunnel will be disturbed as little as possible. Large doors at one end of the building allow the circulation of outdoor air for cooling the building in summer, which is necessary because of the rising temperature due to the power loss in the tunnel at the higher speeds, although, of course, these doors can not be open while the actual tests are being carried on, owing to possible disturbances from the wind. Besides the main room for the tunnel there are several small rooms for offices in the building.

The tunnel itself is of the venturi type with a continuous throat of circular section, and there is an air-tight experimental chamber built about the working section in order that small holes may be opened into the tunnel while it is running, without disturbing the airflow; and this chamber has proved of great convenience in much of the work. The tunnel expands, as shown in Fig. 1, from the 5-ft. diameter working section to a diameter of 10 ft. at the mouth of the entrance and exit cones. This type of tunnel was selected after a considerable amount of investigation had been made upon a model tunnel 1 ft. in diameter, by measuring its efficiency and steadiness while varying many of its characteristics, especially the form of the cones, the type of the experimental chamber, the diffusers, and the honeycombs. It will be noted that, contrary to usual practice, no diffuser is used in the return flow, and this is because the gain in steadiness from its use was found to be very slight on the model, while the cost of the full-sized diffuser would be considerable. Taking into consideration the aerodynamic efficiency of the tunnel, the cost of construction, and the steadiness of the air flow, this type of tunnel was considered to be the best for the proposed investigations, although for some classes of work a larger diameter and slower speed tunnel would be more advantageous.

The cones of the wind tunnel as shown in Fig. 2 are supported from a concrete foundation by heavy steelwork and the surface of the cones is planked with cypress with the inside highly polished. This construction may first seem unduly heavy but when it is realized how great are the vibrations set up by the higher wind speeds and the necessity for having the cones remain perfectly true, it is evident that such a construction is quite justified.

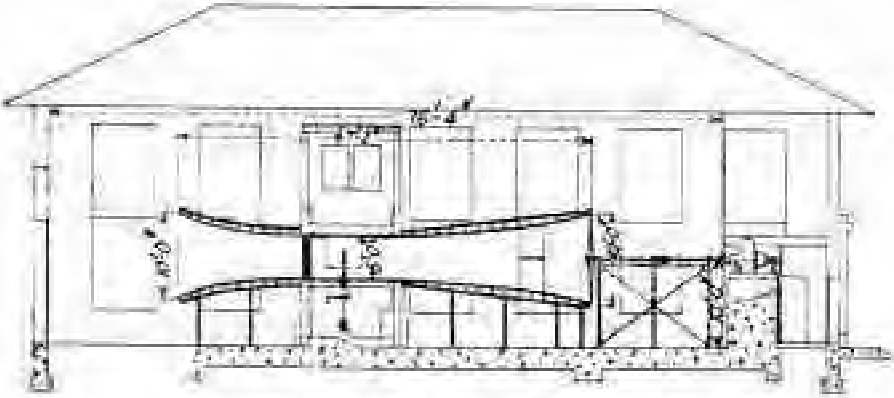
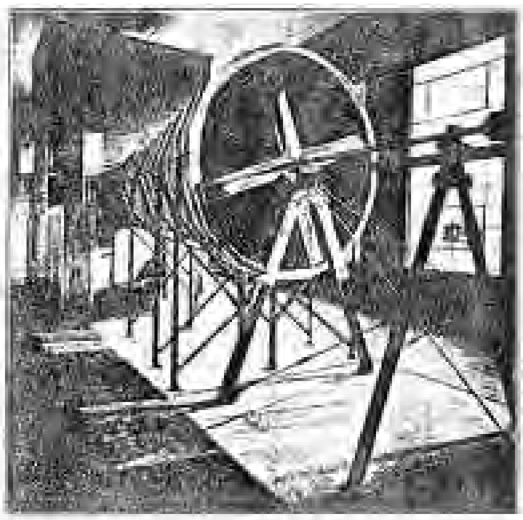


Fig. 1—Longitudinal Section of the Wind-Tunnel Building and the Tunnel

The experimental chamber, which is about 10 ft. long, 14 ft. wide, and 23 ft. high, is built around four concrete columns of very massive construction to withstand the heavy pressures, in some cases as much as 80 lb. per sq. ft., that arise during the high-speed runs. The lower story of the experimental chamber contains the National Physical Laboratory balance and the controlling devices for the air speed, while the upper story contains the propeller dynamometer and wire type of balance. The chamber is entered either by a large door when the tunnel is not running, or, when it is necessary to enter with a difference in pressure, an air lock is provided which consists of two small doors with air valves. Adjustments are made on the model through large doors which can be opened in the throat of the tunnel, these doors being curved to fit



[illegible] View of the Tunnel

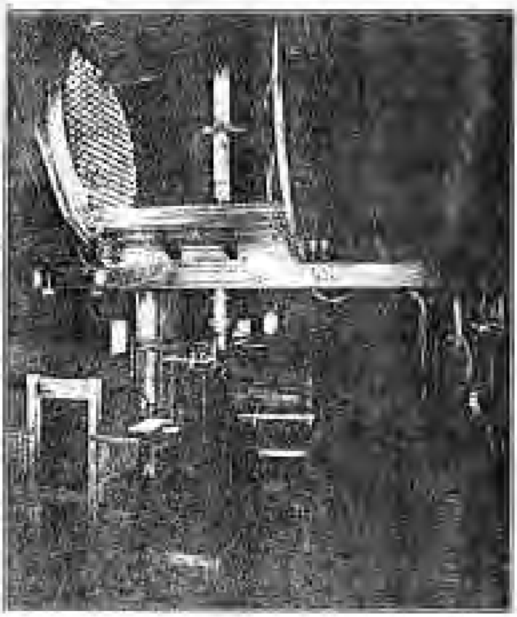


Fig. 3—Interior of the Experimental Chamber Showing a Model Set Up for Pressure Distribution Tests of the Tail Plane

the section of the throat. In order that the model may be inspected during a test, there are three curved glass windows set flush with the inner surface of the tunnel, two in the floor of the tunnel and one in the top. Besides these inspection windows there are also four illuminating windows with electric bulbs, providing a powerful light for photographic purposes. A general view of the lower story of the experimental chamber is shown in Fig. 3, with one of the curved tunnel doors open, indicating the ease with which the model may be reached for adjustment.

POWERPLANT.

The driving motor consists of a 200-hp. direct-current, adjustable-speed motor, with a speed range of from 1 to 1000 r.p.m. At present, power is supplied to this motor at 250 volts from a dynamo driven by a Liberty engine in an adjacent building, but as this powerplant is expensive and inconvenient it is hoped that a more suitable source of supply will soon be available. The propeller is directly connected to the motor by a 3-in. shaft supported on a steel framework and mounted in three ball bearings, while the propeller itself is 10 ft. in diameter and has four blades. As this propeller was designed to be driven by a Liberty engine at 1400 r.p.m. it does not at present absorb all the power which the electric motor can deliver and a new propeller with a larger number of blades and a higher pitch is soon to be used which will increase the air speed obtainable in the tunnel to a considerable extent. The main switchboard is at the entrance of the building, and to obviate the necessity of running heavy wires down to the experimental chamber, the control is by automatic push buttons, one for starting and one for stopping, while a field rheostat is used for speed control above the normal rate of 250 r.p.m. and a series rheostat or potentiometer for speeds below this. The rheostats are placed outside of the experimental chamber to eliminate the heat which would raise the temperature in the small chamber to an uncomfortable degree.

In Fig. 4 is shown a curve of power input to the driving motor plotted against the air speed in the throat of the tunnel. It should be noted that the efficiency of the motor with the very small field excitation which occurs at the higher speeds is exceedingly low and the actual power supplied to the tunnel is much less than that supplied to the motor. An approximate energy factor for the tunnel and propeller of 1.90 is obtained, showing an efficiency considerably higher than that for either the National Physical Laboratory or the straight Eiffel type of tunnel. The reason for the power curve showing a sudden increase at the basic speed is because the potentiometer rheostat is connected in at this speed and absorbs a constant amount of power which is much more than the motor itself absorbs. In Fig. 4 is also shown a curve of the propeller speed plotted against the air speed, that is, the effective propeller slip; and, as would be expected, the slip is constant for all air speeds.

The steadiness of velocity in this wind tunnel compares favorably with that of other tunnels, the maximum variation of the velocity from the mean at any air

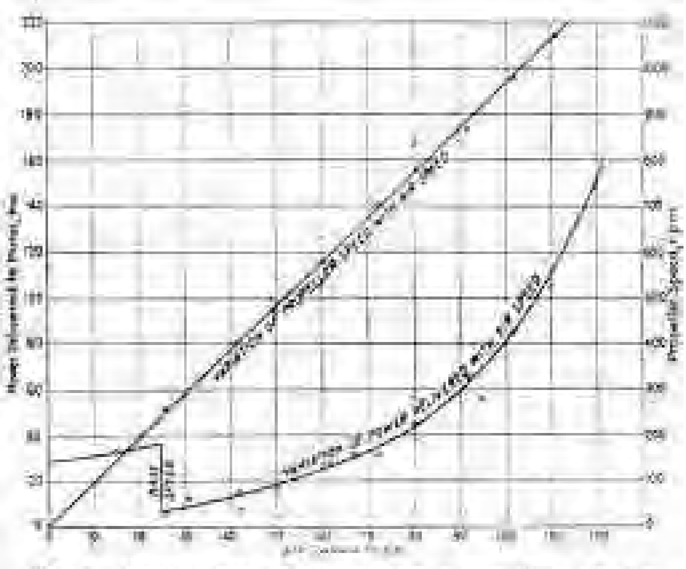


FIG. 4—AERODYNAMIC CHARACTERISTICS OF THE WIND TUNNEL

speed not being greater than 1 per cent at a given point in the tunnel. No manual operation of the controls is necessary during a test, except at long intervals, to compensate for the changing resistance of the motor due to a temperature rise. It was at first planned to construct a speed regulator similar to the one used at Göttingen, but with the proper adjustment of the governor on the generating set, such satisfactory results were obtained that the regulator was found unnecessary. The maximum variation of wind direction in the throat as determined by a recording yawmeter is ±0.5 deg., and this variation is of such a high period that it does not appreciably affect the readings of the balance.

BALANCES.

The balance mainly used in this wind tunnel is of the modified National Physical Laboratory type, designed and constructed at the Committee's laboratory, a cross-section of which is shown in Fig. 5. The distance from the center of the model to the pivot point on this balance is 54 in., while the distance from the pivot to the end of the weighing arm is 27 in., so that the weights are actually twice as heavy as they are marked. The balance was designed to measure forces on the model up to 50 lb., while the weight of the moving parts was kept down to 46 lb. by the use of aluminum alloys and high-tensile steel. While the National Physical Laboratory type of balance is convenient and satisfactory for small tunnels and low wind speeds, it is felt that an entirely different type of balance must be designed if it is desired to measure forces any larger than 30 or 40 lb., as it is found when the maximum forces are used on this balance that a great amount of trouble is introduced by deflection of the various members and especially by the vibration which is set up when a model is turned to an angle near its burble point. Another objection to the National Physical Laboratory balance for large forces is the heavy weights that must be used; that is, for balancing a weight of 50 lb. on the model, weights to the amount of 100 lb. must be lifted onto the

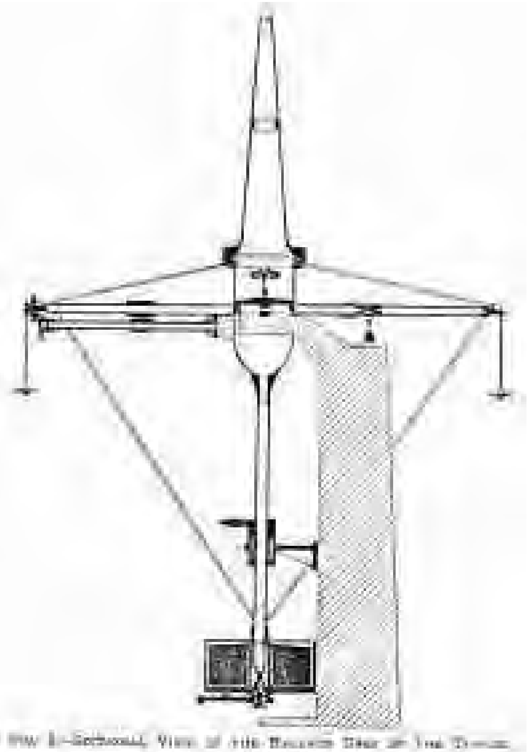


FIG 5.—SECTIONAL VIEW OF THE BALANCE USED IN THE TUNNEL

arms, which is very inconvenient when rapid tests are being made. The principal changes besides weights and dimensions which have been made in the original National Physical Laboratory balance are as follows:

1. A ball-bearing pivot is used in place of the usual conical pivot as the latter gave considerable friction under large loads and also gave trouble through a shifting of its position so that the zero reading was changed during a run.
2. A weighing arm is used for measuring moments instead of the former torsion wire, and in this way lift, drag and pitching moments can be measured simultaneously.
3. The weighing arms are made of light steel tubing, and to prevent deflection they are trussed up with tie-rods, thus greatly diminishing the weight of the arm, while at the same time increasing its stiffness.
4. A pinion is used for turning the head of the balance to make small adjustments more accurately and a prism is used for reflecting the horizontal graduations in a convenient direction.
5. An improved locking device is used on the lower balance tube.
6. The lift is measured very satisfactorily and quickly by a Toledo weightless scale, which allows direct readings to be made.

It was found necessary to use a mercury seal to prevent air from passing around the balance spindle into the tunnel, even though the doors in the experimental chamber were closed. This is due to the fact that even with the tightest possible construction there are a number of small leaks about the experimental chamber, the air from which accumulating at the crack around the balance spindle produces an air flow large enough to introduce a considerable error into the readings of the balance. This seal is made of cast iron and allows a maximum head of mercury of 2 in., the height of the mercury being at all times observable by a glass tube on the outside of the seal. Models are usually supported from the top of the balance by a tapered steel spindle, which is 1 in. in diameter at the base and tapers to 5/16 in. at the top, and this is enclosed up to within 2 in. of the model with a thin brass fair-water to reduce the spindle correction to a minimum. As the bending moment in this spindle is very large at high speed and as there is considerable trouble with the model vibrating, a method has been devised by D. L. Bacon for supporting the top of the model rigidly and at the same time reading the forces with accuracy and great rapidity on the balance.

This method is shown in Fig. 6 and consists of a set of small wires, one extending from the top of the model, one from the lifting arm of the balance and a diagonal to the top of the experimental chamber, the direction of this diagonal being such that if it were projected downward it would pass exactly through the center of rotation of the moving parts of the balance. It is also necessary that the plane of these wires shall be exactly in the plane of the spindle and the lift arm so that there will be no component of lift transferred to the drag arm; and this is done by carefully adjusting the top of the diagonal wire until a force on the model in the direction of the lift plane will have no effect upon the reading of the drag arm. The same method could be used on the drag arm but as the forces are not so great it

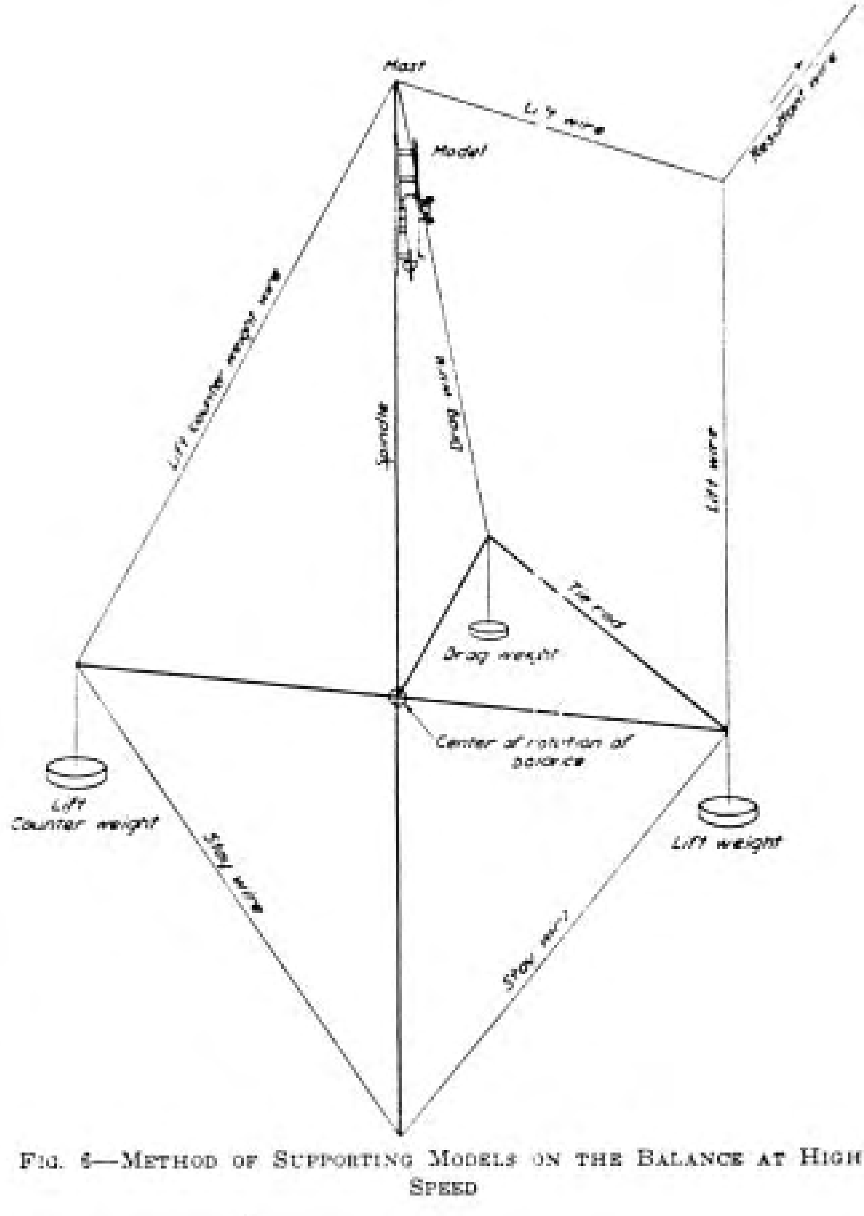


FIG. 6—METHOD OF SUPPORTING MODELS ON THE BALANCE AT HIGH SPEED

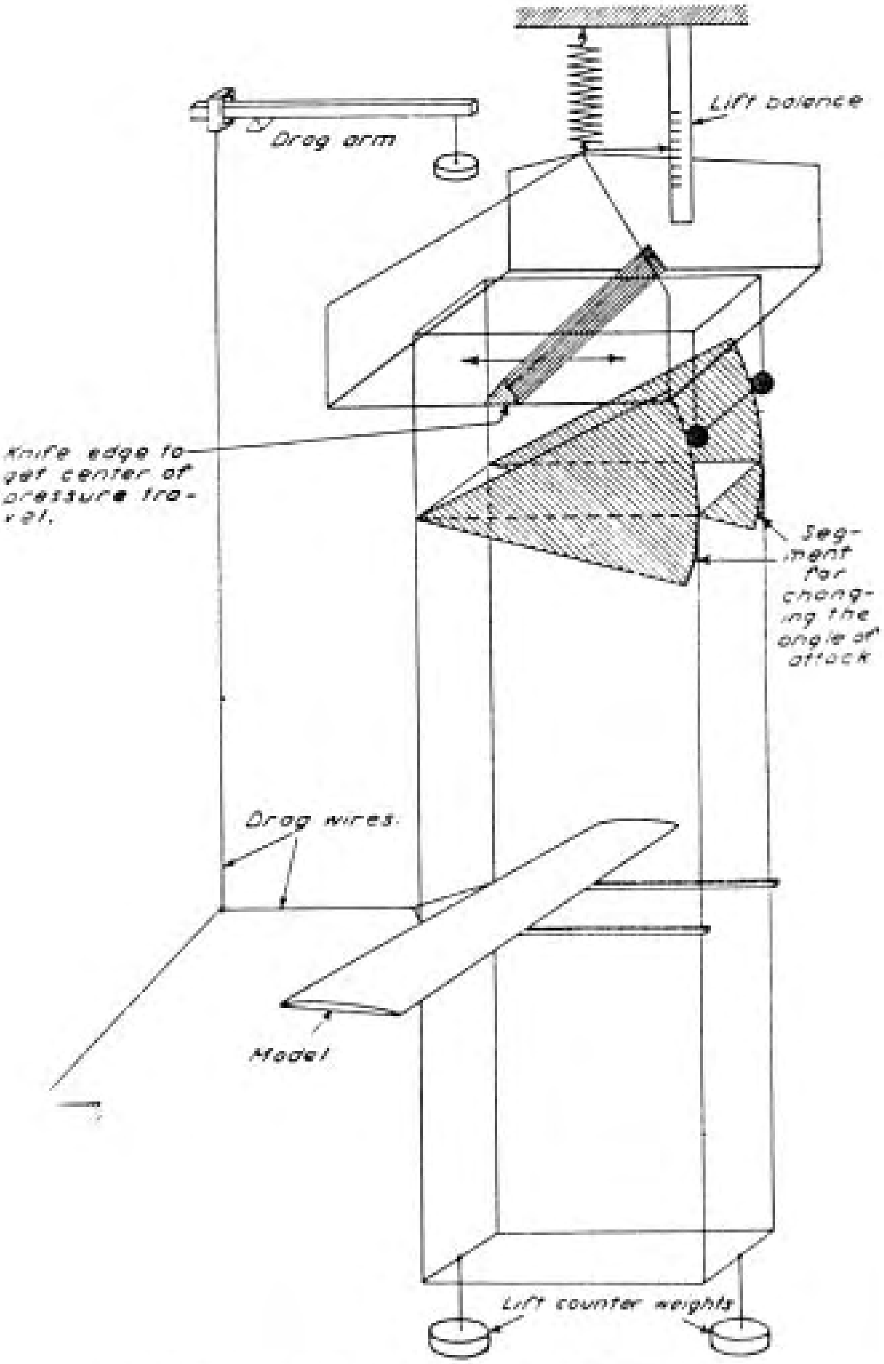
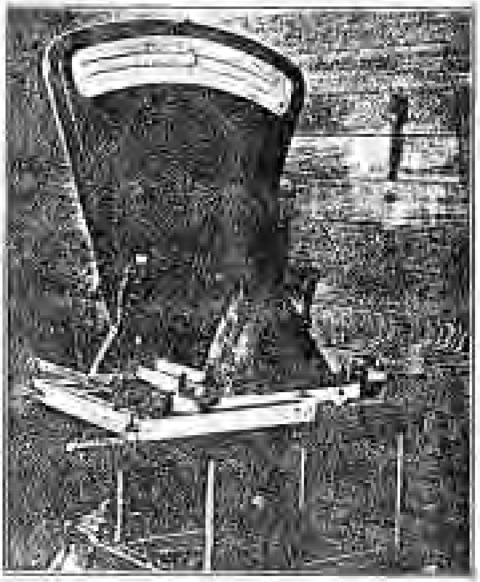


FIG. 7—DIAGRAMMATIC VIEW OF THE WIRE BALANCE



[illegible]

was found sufficient to run a diagonal wire directly down to the end of the arm. These wires pass through small holes in the tunnel walls which are large enough to allow the free play of the tunnel wires and yet due to the air-tight experimental chamber there is very little air passing through them. By this method it has been possible to test a 1⁄24 size model of the Curtiss JN4 up to 100 m.p.h. without excessive vibration or deflection and this speed was the limit only because the model itself, even though made of metal, was not sufficiently rigid to stand a heavier load.

As it is believed that the important development in aeronautics of the future centers about the internally braced wing, the Committee's policy is to conduct extensive researches on this type of airfoil. The National Physical Laboratory type of balance is unfortunately unsuited to tests of this nature, especially where the wings are tapered down to a thin section at the tip, as it is practically impossible to support such a model by a spindle attached to the end of the wing. For this reason it has been necessary to design and construct another type of balance, which supports the wing nearly at its center by wires, as shown in Fig. 7. The lift and drag can be measured on this balance directly, the lift on a Toledo scale and the drag on a small balance connected to the wing by a parallelogram of wires. The center of pressure is determined directly by finding the point about which the wing is in equilibrium when balanced on knife edges, as shown in Fig. 8. This method is very convenient and accurate and eliminates the large amount of computation which was necessary in finding the center of pressure travel by the usual methods. While this balance does not take the place of the National Physical Laboratory balance for the majority of the tests, still it is a necessity when it is required to support the model by its center and for wings of high aspect ratio where it would be impossible to support them steadily by an end spindle.

SPECIAL APPARATUS.

The air speed in the tunnel is originally determined by a pitot tube and the micro-manometer shown in Fig. 9. This manometer can measure a head of water to 0.001 in., which is sufficiently accurate for any work required in the wind tunnel, and is very much more convenient than the Chattock gage generally used in wind tunnels, as its sensitivity can be changed by altering the slope of the glass tube, and its range can be extended to a head of 18 in. without difficulty.

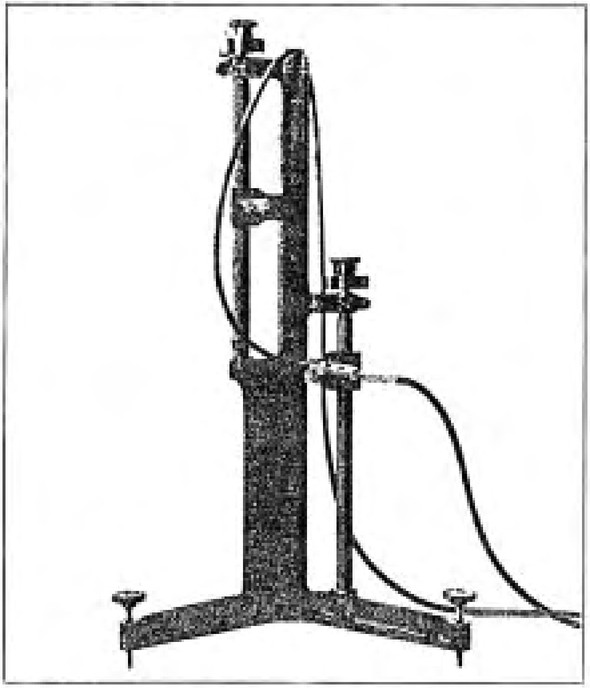


FIG. 9—THE MICRO MANOMETER

For usual running, the air speed in the tunnel is determined by the difference between the static pressure in the side of the tunnel and the outside air in the building, this difference in pressure being measured by the manometer shown in Fig. 10. This manometer is arranged so that its sensitivity will be inversely proportional to the head measured so that at the higher speeds where the fluctuations are naturally greater than at the lower speeds, the variations shown by the liquid will be proportionately the same as at the low speed, which is a necessity when running at the highest velocities. This gage also obviates the necessity of having a very long inclined tube, which would mean that the meniscus must change its position by several feet, so that the operator would have to stand in different positions for various velocities in the tunnel. This gage is, of course, used as a secondary instrument and is calibrated from the readings of the pitot tube, but actual heads can be easily read on it by measuring the angle of the tube and knowing the distance from the center of the reservoir to the meniscus of the liquid.

A multiple manometer is used to a large extent in determining the distribution of pressure over models, and one containing 20 tubes is shown in Fig. 11. The two outside tubes are connected directly to the top of the reservoir so that they will read the height of the liquid at all times, while the other 18 tubes are connected to the pressure holes on the model. The height of the liquid can be adjusted by raising or lowering the reservoir, while the sensitivity can be changed by varying the inclination of the glass tubes.

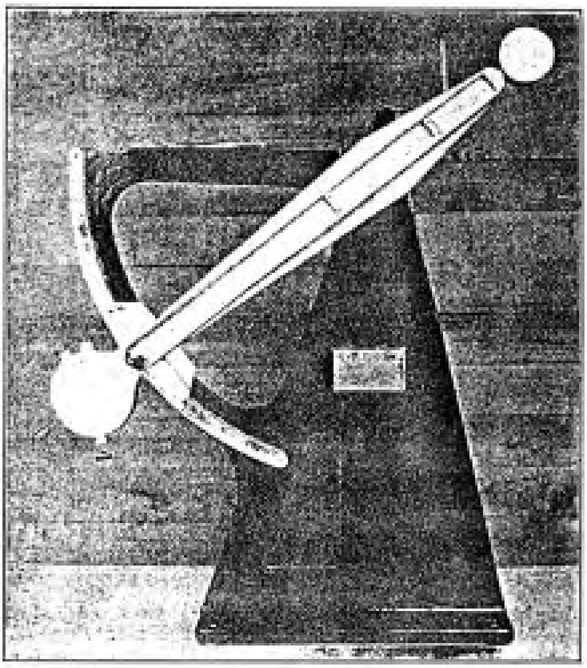


FIG. 10—THE TILTING MANOMETER

For the study of fluctuations in velocity and direction in both the model and full-sized tunnel, a number of high-period recording air-speed meters have been constructed, the most recent one being shown in Fig. 12. This instrument was designed to be portable, requiring only a small battery for the light and the motor, and the film is carried in light-tight drums which are used like plate holders. The sensitivity and position of the zero of this gage can be changed easily, making it available for a large number of uses, while its natural period is so high and the friction is so small that it can easily record the highest period fluctuations that will occur in any wind-tunnel work.

Because of the inconvenience and inaccuracy of the old method of aligning wings by attaching a batten to them and then aligning this batten with a parallel line on the floor of the tunnel, a new method is used consisting essentially of a projector which throws a parallel beam of light upon a small plane mirror temporarily attached parallel to the chord of the wing, and this beam of light is reflected back to the cross line of a white target on the wall of the experimental chamber, Fig. 13. To align the model all the operator has to do is turn the head of the balance until the light spot falls on this cross line, which he can see from any part of the experimental chamber, so that one man can line up a wing to within 0.02 deg. in a few seconds, thus greatly reducing the time and increasing the accuracy compared with the older methods.

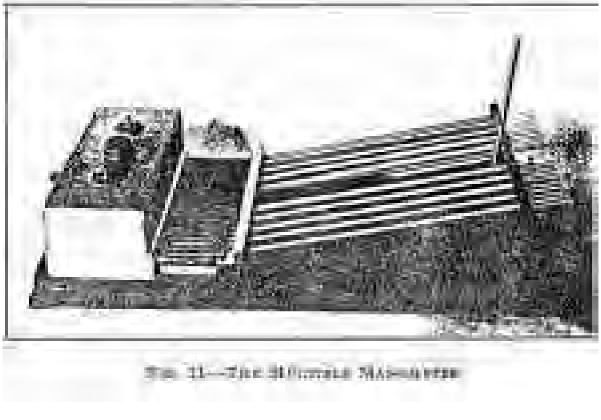


FIG. 11—THE MULTIPLE MANOMETER

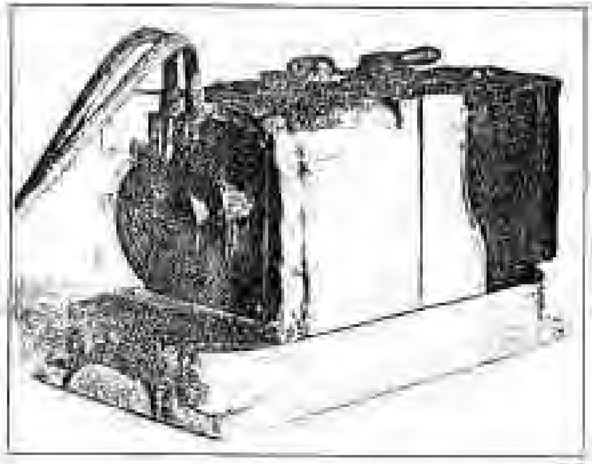


FIG. 12—THE [illegible]

This method is also applicable for determining the angular deflection of a wing or model during actual test. The optical method of aligning has been in use for a considerable length of time and has proved very satisfactory, entirely eliminating those errors in alignment which are bound to creep in with the older method, due to unskilled operators or curvature in the batten.

One of the chief causes of dissimilarity between a model test and a free-flight test is the lack of a slipstream in the model, and to produce this effect in the wind tunnel a small propeller is driven before the model by a belt from a high-speed electric motor above the tunnel, the propeller being supported by steel wires from the walls of the tunnel as shown in Fig. 14. The wires, the belt, and the propeller mounting undoubtedly cause a somewhat different air flow from that occurring in full flight, but this interference is probably very small and at least gives us a much closer approximation of actual conditions than has been obtained before. While it has not been possible as yet to drive a model propeller at a proportional speed to the full-size propeller, it has been possible by using a model with a pitch slightly

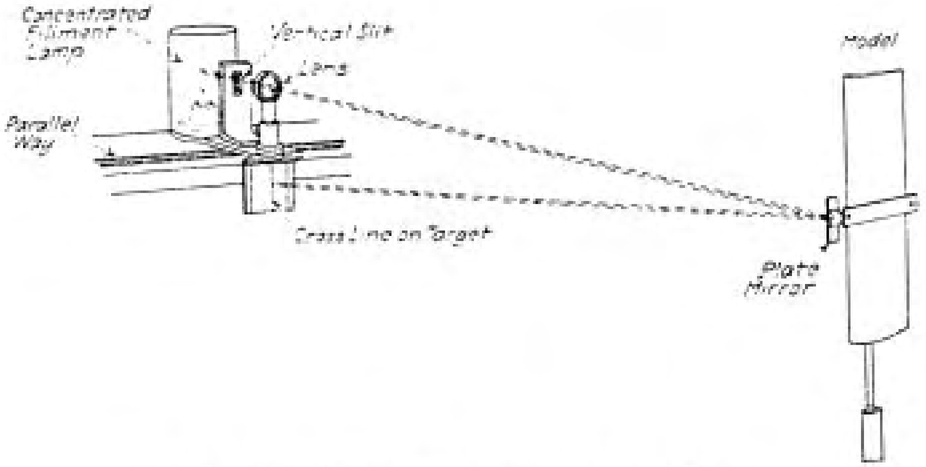


FIG. 13—METHOD OF ALIGNING THE WIRES

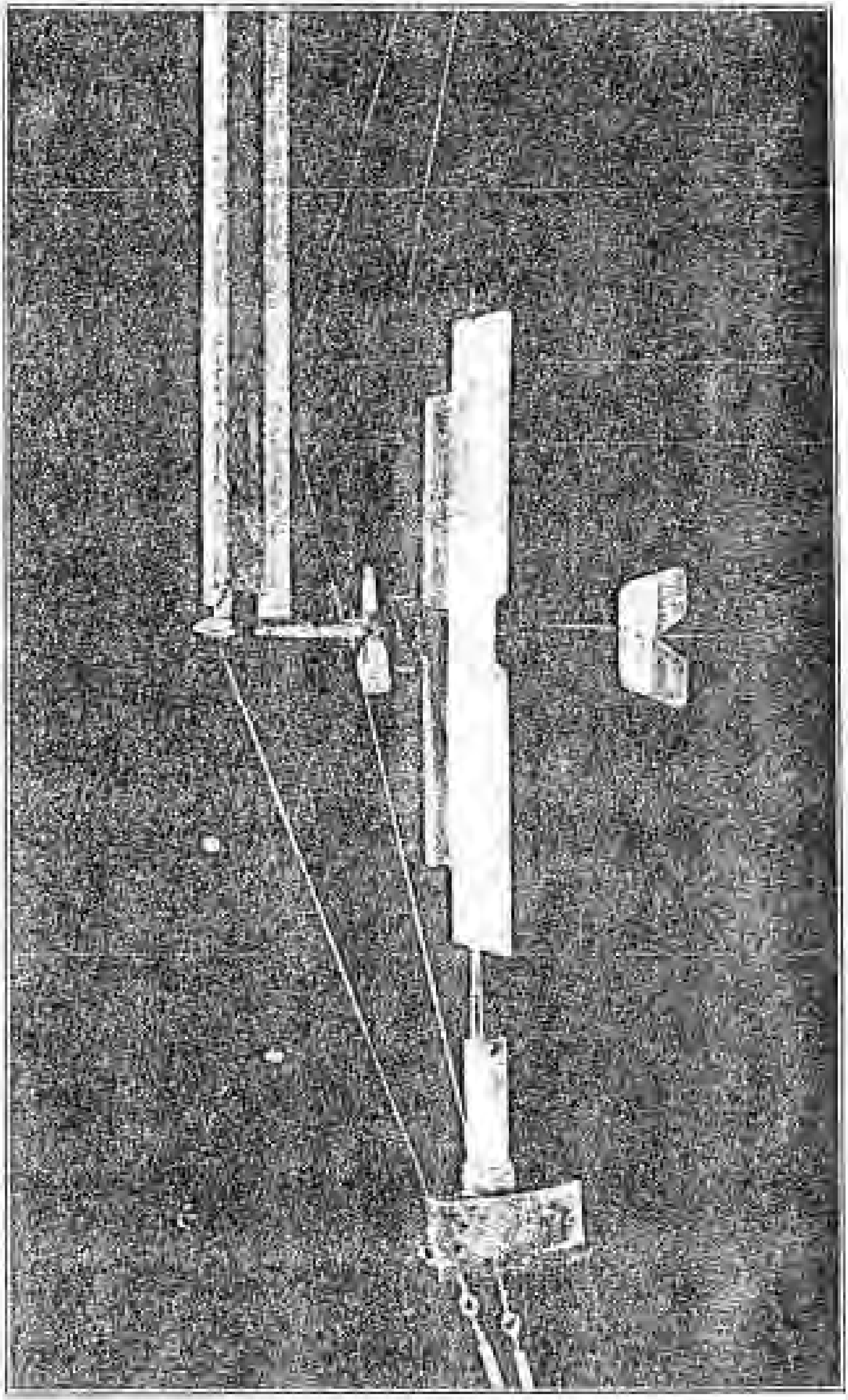


FIG. 14—METHOD OF DETERMINING THE SLIP STREAM EFFECT

larger than the full-scale propeller to get a slipstream of the same characteristics as the slipstream in the full-sized machine, which should give identical results as far as the interference of the model is concerned. Work is being carried out on the design of a very small high-pressure turbine to drive the propeller at a higher speed and so that a smaller mounting can be used through reducing the interference.

MODELS.

While the models tested in the wind tunnel are, strictly speaking, not a part of the equipment, still those models of such standard form that they are used repeatedly in tests can be considered as such. The Committee has constructed two models of the JN4H airplane, one of them being the $\frac{1}{24}$ scale model illustrated in Fig. 15, which is constructed with the greatest accuracy to reproduce all parts that might affect the air flow, that is, the engine, radiator, and wind shields, the only omission being the wires and fittings which can be more accurately calculated than tested. The other model of the same machine has been constructed to $\frac{1}{15}$ model size for pressure distribution tests on the tail surfaces.

All of the medium or thick wings that are tested in the tunnel are constructed of maple, as this material can be worked with proper precautions to within an error of 0.002 in. The wings are cut upon a special machine shown in Fig. 16, which not only will cut the usual constant-section type, but will also cut wings tapering in plan form and thickness, as, for example, a wing with a depth-to-chord ratio falling off toward the tip as a parabolic function, and at the same time with an elliptical plan form. A much heavier and more precise machine of the same type has been designed for cutting and grinding aluminum or steel wings. Without a machine of this type the Committee's extensive program on thick tapered wings would be impossible because of the great expense of making the models by hand.

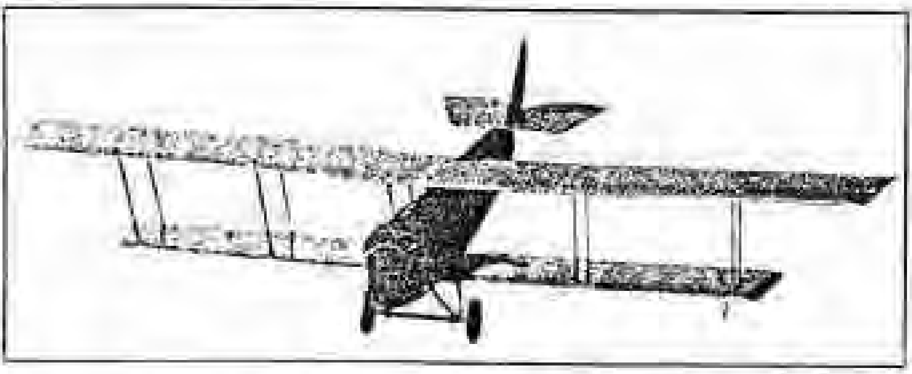


FIG. 15—A WIND TUNNEL MODEL OF AN AIRPLANE

The wind-tunnel work of the National Advisory Committee at Langley Field, in contrast to the work of most of the tunnels in this country, is entirely research on the fundamental problems of aeronautics, such as the systematic design of airfoils, the scale effect on models and the relation of the stability on models to that in free flight. Fortunately, it is possible to carry this work on most efficiently because of the close cooperation that can be had with the free flight investigations which are being conducted by the Committee at the same time. It is, however, realized that with only one wind tunnel, the various model investigations can not be carried on simultaneously, and so the work can not be conducted as rapidly as desired, but it is expected that a second and larger tunnel will be constructed by the Committee in the near future, which will greatly extend the range and amount of investigation possible.

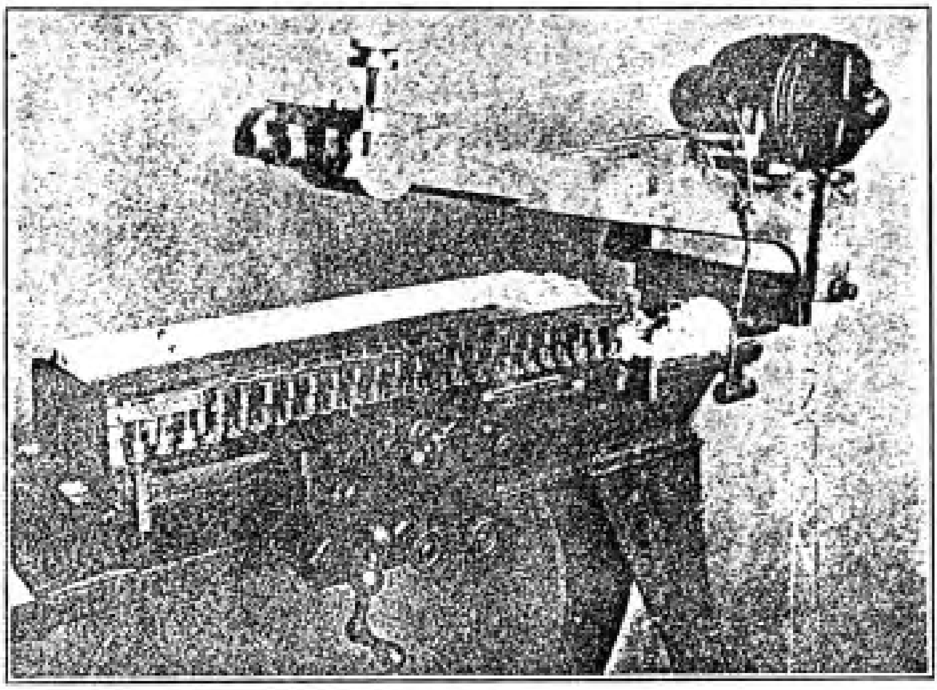


FIG. 16—A SPECIAL MACHINE FOR CUTTING AIRFOILS

Document 2-18(a–j)

(a) Max M. Munk, letter to J. C. Hunsaker, 7 October 1920, Hunsaker Collection, Box 4, File M.

(b) J. C. Hunsaker, letter to Lester D. Gardner, 3 May 1921, Hunsaker Collection, Box 2, Folder 4, File G.

(c) F. H. Norton, Chief Physicist, NACA Langley, memorandum to NACA Executive Officer (G. W. Lewis), 5 August 1921, RA file 44, Historical Archives, NASA Langley.

(d) W. Margoulis, excerpts from "A New Method of Testing Models in Wind Tunnels," ***Aeronautics*** **[Britain] XIX, No. 373 (New Series), (9 December 1920): 412–413.**

(e) Edward P. Warner, Massachusetts Institute of Technology, letter to G. W. Lewis, Director of Aeronautical Research, NACA, 5 March 1921, NASA Record Group 255, General Correspondence File, Box 12, National Archives, Washington.

(f) W. Margoulis, excerpts from "A New Method of Testing Models in Wind Tunnels," NACA Technical Note No. 52, [1921].

(g) Max M. Munk, "On a New Type of Wind Tunnel," NACA Technical Note No. 60, May 1921.

(h) F. H. Norton, memorandum to G. W. Lewis, "Design of compressed air wind tunnel," 6 October 1921, NASA Record Group 255, General Correspondence Numeric File 21-5, Box 80, National Archives, Washington.

(i) "Compressed Air Wind Tunnel," ***NACA Annual Report for 1921*** **(Washington, DC), pp. 29–30.**

(j) Max M. Munk and Elton W. Miller, *The Variable Density Wind Tunnel of the National Advisory Committee for Aeronautics*, NACA Technical Report No. 227 (Washington, DC, 1925).

Langley's study comparing a Curtiss JN4H in flight to models of the same airplane in the Atmospheric Wind Tunnel confirmed that wind tunnel testing was a valid predictor of aircraft performance and behavior, but it also pointed the NACA researchers toward another well-known, but poorly understood, problem: scale. It had long been known that the forces generated by a model were not proportional to the model's scale. A 1⁄20-scale model of a plane in an air stream 1⁄20 as fast as the actual plane flies generated considerably less than 1⁄20 of the actual lift and drag forces. Early aerodynamicists had developed empirical coefficients to "scale up" the data, but the comparison tests showed just how unreliable these coefficients were. The problem was that the air itself could not be "scaled-down" to model size, and its properties, such as density and temperature, were almost the same in the wind tunnel as they were around a full-size airplane. To gain the maximum value from wind tunnel testing, an answer to the scaling problem had to be found.

The answer to the scaling problem had its roots in the work of a nineteenth-century British scientist, Osborne Reynolds. Reynolds showed that the forces a moving fluid exerts on a body, or vice versa, depended on the fluid's velocity, density, and viscosity, and on a key dimension of the body, such as length or diameter. He combined these parameters into a mathematical expression where all of the dimensions cancelled one another out. The dimensionless result, known as the Reynolds number, was a key to understanding scale factor. Because it was dimensionless, the Reynolds number could be used to compare fluid-flow forces around similarly shaped, but differently sized objects. By varying different parameters, such as increasing the velocity or decreasing the density, to obtain the same Reynolds number for different tests—a condition known as dynamical similarity—an excellent correlation between model tests and aircraft performance was possible. But this was easier said than done. When a 1⁄20-scale model was tested in the AWT, the Reynolds number was about 1⁄10 that of the corresponding actual flight. Larger models could theoretically be used, but wingspans greater than about 3 ½ feet were not useful in the five-foot-diameter tunnel due to interference by tunnel walls. The AWT lacked the power to run at the high speeds necessary to generate the required Reynolds numbers; furthermore, such speeds were not practical in open wind tunnels anyway.

Two possible solutions to the problem emerged almost simultaneously in the early 1920s. In both cases, the intent was to increase the density/viscosity term in the Reynolds number calculation. Wladimir Margoulis, a Russian-born aerodynamicist and former director of the Eiffel laboratory in France, proposed using carbon dioxide instead of air in a sealed wind tunnel, because carbon dioxide's density was over one-and-one-half times that of air. Max Munk, a brilliant and quixotic German student of Prandtl, suggested that a wind tunnel be built inside a pressure vessel so that tests could be run under pressure, thus increasing the density of the air as much as twentyfold. The NACA employed both experts, bringing Munk to the United States and retaining Margoulis as an agent in Paris. Debate continues over which man was first to suggest using higher densities, but the NACA chose to pursue Munk's concept.

The result was the Variable Density Tunnel (VDT), Langley's second wind tunnel, which went into service in 1922. Externally, it appeared to be little more than a large cylindrical tank with spherical ends. A closed five-foot-diameter wind tunnel was mounted inside, such that air flowed through the central test section, past the fan, and returned via an annular passage. The entire tank could be pressurized to 300 pounds per square inch (twenty atmospheres), sufficient to produce Reynolds numbers for tests of $\frac{1}{20}$-scale models that were equivalent to full-scale flight. A wide variety of tests were run in the VDT, including studies of several model airplanes to validate the high-pressure concept, but the most significant investigations involved airfoils. Through extensive use of the VDT, Munk and other NACA researchers drew accurate performance curves for the commonly used airfoils of the era, and then extended the investigation to develop families of airfoils with similar characteristics. The VDT provided unique capabilities, and the airfoil data produced with it put the NACA and its Langley Laboratory "on the map" of first-class aeronautical laboratories in the mid-1920s. (Document 2-15(i) in the preceding section on international standardization of wind tunnels is a 1925 report from Britain's National Physical Laboratory comparing results from their Duplex wind tunnel and the NACA VDT. It notes that the favorable comparison "tends to establish a considerable amount of confidence in the compressed air tunnel results.")

The documents included herein illustrate how Munk's idea of a compressed air wind tunnel moved from a concept to a reality. In a 7 October 1920 letter to Jerome Hunsaker, Munk mentioned that he had "finally found a perfectly new manner for increasing Reynold's [sic] number," but he declined to provide any details prior to employment. Once on the NACA's payroll, he readily furnished the theoretical basis for his proposal, which the committee published as Technical Note No. 60, "On a New Type of Wind Tunnel." Once the VDT was in operation, the NACA published a thorough description of the tunnel in Technical Report No. 227, *The Variable Density Wind Tunnel of the National Advisory Committee for Aeronautics*.

A secondary theme in this section concerns some of the problems surrounding Max Munk himself. From the start he was embroiled in controversy, both national (because he was German), and personal (because he was unbearable to work with). Documents in this section hint at the difficulties Langley personnel had with Munk. As shown in the 1921 letter from Hunsaker regarding reactions to employing German experts at the time, Munk faced an uphill battle from the start. Readers should note that just to bring Munk into the United States and employ him in federal service took no less than two special Presidential orders.

Frederick Norton's 5 August 1921 letter regarding the use of German airfoils is another case in point; Norton was incensed that the NACA would even consider allowing the use of a foreign—especially a German—airfoil when a number of good American designs were available. While such national parochialism is perhaps understandable so soon after the end of World War I, these documents suggest that acceptance at Langley during this period would have been very difficult for any German immigrant, not just Munk.

Norton's letter of 6 October 1921, dealing this time with VDT issues, shows that the situation had continued to deteriorate. In his case, however, Munk managed to retain George Lewis's support, leading Norton to resign two years later. Max Munk's triumph was not long-lived, however. The recollections of Fred Weick, included as Document 2-20(c) in the section on the success of the Propeller Wind Tunnel (PRT), recount some of the continuing difficulties encountered in working with the brilliant but temperamental German aerodynamicist. This challenge culminated in a 1927 "revolt" by Langley personnel against Munk that resulted in his departure from the NACA.

Document 2-18(a), letter from Max M. Munk in Germany to Jerome Hunsaker, 1920.

Oct. 7th. 1920

To: Commander J. C. Hunsaker, U.S.N.
Assistant for Aeronautics to the Chief of the Bureau of Constr. and Repair of the U.S. Navy Department
Washington, Navy-Building

Sir,

hoping [sic] to support my request of July 1920 at Göttingen, I beg leave to inform you, that I made an important invention concerning wind-tunnel tests. You told me then to be greatly discouraged by the present model-tests, on account of the small value of their Reynold's number and of the want of security in applying them on large bodies, following from it. I quite agree with you in this matter: the law of the lift of wings being found, there remain only investigations about details in the present wind-tunnels.

Meditating on these things, I finally found a perfectly new manner for increasing Reynold's number to about eightfold or even the elevenfold (but then with a little complication surmountable however) without increasing the expenses of the construction or the tests. The new method is important for such investigations where Reynold's number is to be considered. But then, you either get the eightfold value of this number or otherwise you can save seven eights of the costs.—

Not even knowing, whether your address is correct, I dont [sic] like to explain the new method in this letter. Indeed I do not like to do this at all, as long as I have not yet any answer to my request. I prefer reserving my idea to my next employer. I shall however thank you very much for passing on the matter to the Committee and for helping my next employer to be in the U.S.

Trusting you to excuse my once more troubling you in this matter, I shall be very much obliged for the favor of an early reply.

Believe me Sir to be Yours most respectfully
[signed] M. Munk.

Document 2-18(b), letter from J. C. Hunsaker to Lester D. Gardner, 1921.

May 3, 1921.

Mr. Lester D. Gardner,
The Gardner, Moffat Co., Inc.
225 Fourth Avenue
New York, N.Y.
Dear Gardner:

I note with interest the row you appear to have got into over the report to the President on the aeronautical situation, and so long as you are convinced you are doing good I suppose your policy is correct from your point of view. However, you ought to be more careful about using the term "minority report" since there wasn't any, and since the gentleman who wrote a special letter to the President had already agreed in committee that [there] would be no minority report.

The information you wanted about Dr. Munk is that he was got from the University of Göttingen by the National Advisory Committee for Aeronautics on my recommendation as knowing more about German aerodynamics and theoretical questions generally than [anybody] else they could get, and that the best way to bring ourselves up to date with the research work at the Göttingen laboratory, and especially the unpublished portions of it, was to import the man who had done most of it himself. He has not been turned over to the Navy or anybody else but is employed in Lewis' office in translating German data and preparing dope [intelligence information]. I don't think you can get anywhere by attacking the Committee for having employed a German in order to make available in this country what the Germans know.

With kind regards,
Very truly yours,
[signed] J. C. Hunsaker

Document 2-18(c), F. H. Norton, memorandum to George W. Lewis, 1921.

August 5, 1921.

From: Langley Memorial Aeronautical Laboratory
To: National Advisory Committee for Aeronautics
(Attention Executive Officer.)
Subject: Tests of Gottingen wing section.
Reference: (a) NACA Let. 54-6(44/21)—18648, Aug. 4, 1921.

1. Under no circumstances would I consider it advisable to use a German wing section on a Committee helicopter as I am sure our own wing sections are fully as good as Germany's and if Dr. Munk can not find one which we have already tested which has the properties he desires I will gladly undertake to design and test one if he will furnish me with more particulars.

2. I think you will see that we should use every means to advertise the Committee and the Committee's work and the use of a German wing section in its own helicopter should certainly not be advisable from this point of view.

F. H. Norton
Chief Physicist

Document 2-18(d), W. Margoulis, excerpts from "New Method of Testing Models in Wind Tunnels," **Aeronautics** *[Britain], 1921.*

In forecasting the conditions of flight of aeroplanes by the results of model tests made in existing laboratories, serious errors are inevitably made, owing to the fact that in the laboratory it is impossible to observe the laws of similitude requiring the equality of Reynolds numbers and the equality of the ratios of the velocities to the velocity of sound (Law of Bairstow and Booth). The first of these conditions is due to viscosity, and is of special importance at low speeds; the second is due to the consideration of compressibility, and should be observed at high speeds.

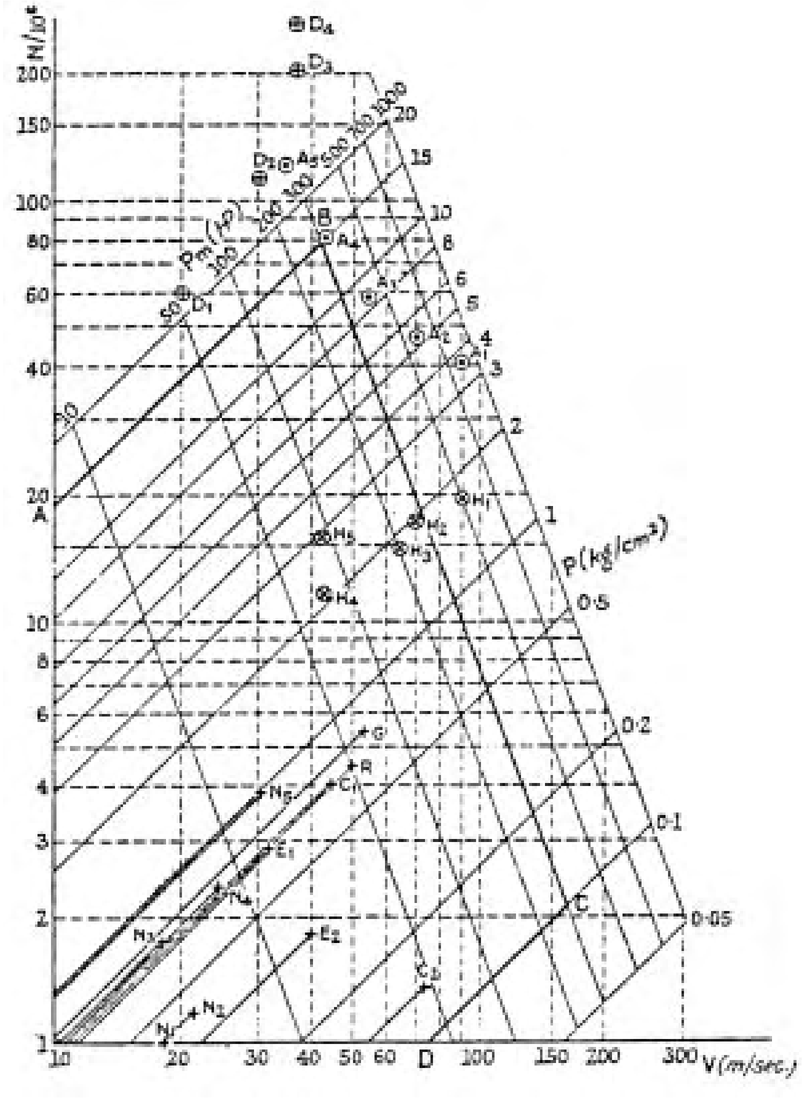
Now, the Reynolds numbers attained in existing laboratories are from 15 to 25 less than those reached by machines in flight, whilst the velocities of the airstream remain from two to three times lower than the speed in free flight. Thus, when a model aeroplane is tested in the laboratory, the streamline wires resist relatively twice as much, and the struts of the rigging and landing chassis five times as much, whilst the wings carry up to 30 per cent less on the model than on the aeroplane.

It will be the same in the large laboratories (1,000 to 1,500 h.p.) now being planned, which, though realizing higher velocities, will not attain the seventh part of the true value of Reynolds number.

WE WILL SHOW, HOWEVER, THAT IT IS POSSIBLE TO HAVE WIND TUNNELS GIVING HIGHER VALUES OF REYNOLDS'S NUMBER AND OF THE RATIO OF THE VELOCITY TO THE velocity of sound than those reached by full scale machines, and that with less outlay for installation and upkeep than is required for the laboratories now being planned.

This result will be attained by employing some gas other than air, and especially carbonic acid, at pressures and temperatures which are suitable and generally very different from those of the surrounding atmosphere.

Let v and d be respectively the velocity and diameter in the working section of a wind tunnel; μ—the coefficient of viscosity; ρ^{o}—the density at $1\ \text{kg.}/\text{cm}^2$ and 273°; p—the pressure, and T the absolute temperature of the fluid circulating in the flue. The units employed are the kg., the metre, and the second.



The motive power *Pm* required for working the fan of a closed circuit wind tunnel of the Crocco type is:

$$Pm = 0.47 v^{2.75} d^{1.75} \mu^{1.25} \rho_0^{0.75} p^{1.75} T^{-0.75} \qquad (1)$$

If the span of the model aeroplane is equal to 6/10 of the diameter of the flue the Reynolds number will be:

$$N = 0.6 \frac{vd}{\nu} = 0.0164 v d \mu^{-1} \rho_0 p T^{-1} \qquad (2)$$

Where ν is the kinematic coefficient of viscosity; $\nu = \mu/\rho$.

The velocity of sound in a fluid being equal to $\sqrt{\gamma \frac{p}{\rho}}$, γ being the ratio of the specific heats, the condition of equality of the ratios of the velocity to the velocity of sound requires that:

$$v = 1.1086 \rho_0^{-0.5} \gamma^{0.5} T^{0.5} V \qquad (3)$$

V being the speed of the full scale machine.

We will examine three cases:

1st Case.—Tests of model aeroplanes and dirigibles; *N* is given.

From formulas (1) and (2) we deduce:

[illegible] (4)

and

[illegible] (5)

The use in a wind tunnel of carbonic acid at 15 kg./cm² and 253° reduces the power required for realizing a given Reynolds' number with a given diameter of flue in the ratio of 1088 to 1.

2nd Case.—Tests of model propellers; *V* is given.

We have:

[illegible] (6)

the true speed (*V*) in the tunnel being determined by formula (3).

The use of CO_2 at 0.5 kg./cm² and 253° reduces the power required by 70 per cent. and if we reduce the pressure to 0.1 kg./cm², the power required will be reduced by 90 per cent.

3rd Case.—Any model whatever: *V*, *N*, and *d* are given.

We have:

[illegible] (7)

and

[illegible] (8)

the value of v being given by formula (3).

The use of CO_2 at 253° and at a pressure determined by formula (8), reduces the power required by 55 per cent.

We would point out that the terms characterizing the nature of the fluid and the conditions of temperature and pressure in formulas (4) and (7) are independent of the experimental value of n the expression giving the power drop in tunnels:

$$\Delta = a \cdot \mu^n \rho_0^{1-n} \cdot p^{1-n} T^{n-1} \cdot v^{2-n} d^{-1-n}$$

It is the same for all the terms of formulas (3), (5), and (8).

As an application of the method, we will consider a closed circuit tunnel 2 m. in diameter, 300 horse-power, utilizing carbonic acid. Model aeroplanes will be tested at a pressure of 15 kg./cm² and at 253°; we shall thus realize at 30 m./sec. a Reynolds number of 81.10^6, corresponding to that of an aeroplane with a span of 26 m. at 150 km./h. or of a racing plane at 650 km./h.

If the propeller tests are made at 0.5 kg./cm², the speed attained will be 76 m./sec., equivalent to a speed in free flight of 103 m./sec., that is 370 km./h, whilst at 0.1 kg./cm2 the speed will be 570 km./h.

For realizing these conditions an ordinary wind tunnel 2 metres in diameter would require 326,000 h.p. in the fist case, and 3,000 h.p. in the second case.

During the manipulations of the model, the working section can be isolated by means of two doors, the carbonic acid contained in the working section having been previously collected in a special tank. The measuring devices, registering the stresses automatically, will be placed in an airtight cabin fixed on the wall of the working section. . . .

[*The author here discusses a graph and calculations, omitted here, that "represents the functioning of a tunnel of our system (diameter 2 m, carbonic acid at 253°)."*]

We thus see that for all aeroplanes, except giant planes (42 m. span), we realize the condition of equality of Reynolds's number, that for dirigibles (24 m. in diameter) we attain the half of Reynolds's number which is sufficient to give a good approximation, and that for all propellers we realize the conditions imposed by the consideration of compressibility and viscosity. . . .

Remark.—In the application just given of our system to a tunnel of 2m., we assumed, in order to treat a general case, that the carbonic acid was cooled to –20°. Practically this cooling leads to complications of installation and functioning which are not justified by the slight gain of power (see formulas 4, 6 and 7) which results.

We therefore consider it preferable to work at the temperature of the surrounding atmosphere. If we assume a mean temperature of +10°, formulas (7) and (8) show that for the same values of V and N the power is increased by 11 per cent and the pressure by 6 per cent, with respect to those corresponding to a temperature of -20°.

Document 2-18(e), letter from Edward P. Warner, Massachusetts Institute of Technology, to George W. Lewis, 1921.

March 5, 1921.

Mr. G.W. Lewis,
National Advisory Committee for Aeronautics
Washington, D.C.

Dear Mr. Lewis:

I have recently received a letter from Mr. Margoulis relative to his new system of high-pressure windtunnel employing carbon dioxide, and I am really quite favorably impressed with such a tunnel for some classes of work. For determining the speed and density effects on air speed meters, for example, such a tunnel would be favorably used and also in connection with the test of streamlined bodies.

Margoulis is anxious to be retained as consulting engineer to construct such a tunnel here, and while I do not see the necessity of this, as it would be easy to secure the necessary talent for securing a high-pressure tunnel in America (incidentally I think that it would be best to use air at high pressure in preference to carbon dioxide sacrificing some of the theoretical advantages but considerably simplifying the construction and obviating the necessity of absolute leak-proofness), I do recommend that Mr. Margoulis be retained by contract for not over 2500 francs to prepare a 12,000-work report on "European Windtunnel Practice" and on suggestions for windtunnel design. He has shown himself possessed of many excellent schemes and we would avail ourselves of his knowledge at least to that extent. I further recommend that Margoulis be retained to prepare a general report on the use of nomograms and alignment charts in aeronautics. It is a subject which we have too much neglected and which he is better qualified by experience to treat than any one else in the world, as he is the originator of almost all the very ingenious nomograms used at the Eiffel laboratory.

Yours sincerely,

[signed] Edward P. Warner

Document 2-18(f), W. Margoulis, excerpts from "A New Method of Testing Models in Wind Tunnels," NACA Technical Note No. 52, 1921.

We know that the two essential conditions of the application of the law of proportionality of pressure to the product of density, the square of the linear dimensions and the square of the speed to the results of model tests are:

1) THE EQUALITY OF REYNOLDS' NUMBER.

$$N = \frac{vl}{\nu} = \frac{VL}{\nu_1}$$

v being the velocity of the airstream in the tunnel,
V being the speed of the machine in free flight,
l and L respectively one of the principal linear dimensions of the model and of the full-scale airplane.

ν and ν_1 respectively the kinematic coefficients of viscosity of the fluid circulating in the tunnel and of the air in which the machine flies.

2) THE EQUALITY OF THE RATIOS OF THE SPEED TO THE VELOCITY OF SOUND (Law of Bairstow and Booth).

$$M = \frac{v}{w} = \frac{V}{W}$$

w and W being respectively the velocities of sound in the tunnel and in the air.

The first of these conditions is due to viscosity and is important especially at low speeds (tests of model airplanes); the second condition is due to the consideration of compressibility and must be observed at high speeds (tests of model propellers).

Now, in existing laboratories utilizing a horsepower of 100 to 300, the models are generally made to a $\frac{1}{10}$ scale and the speed is appreciably lower than the speeds currently attained by airplanes; the Reynolds' Number realized in the laboratories is thus from 15 to 25 times smaller than that reached by airplanes in free flight, while the ratio M varies between the third and three-fourths of the true ratio.

Thus, when a model airplane, for instance, is tested in such a laboratory, the streamline wires resist relatively twice as much, the struts of the rigging and undercarriage five times as much, while the wings carry 30% less on the model than on the airplane, so that RESULTS OBTAINED IN EXISTING LABORATORIES CAN NOT BE PRACTICALLY UTILIZED.

We cannot appreciably increase Reynolds' Number by increasing either the diameter (d) of the tunnel, or the velocity (v) of the airstream, for the motive power required for working the fan producing the airstream is proportional to d1.75 v2.75 and such increase would therefore lead to installations much too costly both as to establishment and upkeep.

Thus wind tunnels are now being planned having a diameter of 3 to 5 m., speeds of 60 to 75 m/sec., and horsepower of 1000 to 1500; but the Reynolds' Numbers attained in such tunnels will still be 8 times less than those of existing large airplanes.

We will show, however, that it is possible to have wind tunnels in which the Reynolds' Number will be greater than that now attained by airplanes, and in which the ratio of the velocity to the velocity of sound will also be greater than that realized in practice, and we will show that this can be done with an outlay for installation and upkeep much below that required by the laboratories now being planned.

In order to attain this result we have only to employ a gas other than air, at a pressure and temperature different from those of the surrounding atmosphere.

[. . .]

2.—lst CASE—REYNOLDS' NUMBER IS GIVEN; TESTS OF MODEL AIRPLANES AND AIRSHIPS.

Eliminating *v* from equations (1) and (2) [*same as (1) and (2) in Document 2-18d*] we have:

[illegible] (4)

and

[illegible] (5)

Formula (4) shows that the power P_m is proportional to the term μ^3/ρ_0^2 characterizing the fluid, and to the term T^2/p^2 characterizing the conditions of temperature and pressure.

THE GAS WHICH IS PRACTICALLY MOST SUITABLE TO THE DIFFERENT CONDITIONS LAID DOWN BY THE ABOVE FORMULAS, AND BY THOSE WHICH FOLLOW, IS CARBONIC ACID (CO_2), AS A GAS HAVING A LOW COEFFICIENT OF VISCOSITY, HIGH DENSITY, AND A LOW RATIO OF SPECIFIC HEATS.

For air, $\mu^3/\rho_0^2 = 301/10^{18}$ for CO_2, $\mu^3/\rho_0^2 = 72.4/10^{18}$,

[illegible]

[*Footnote:*] (Other gases, such as chloride of methyl (CH3Cl) and Xenon would give better results but could not be practically employed. Thus μ^3/ρ_0^2 is equal to $571/10^{18}$ for water, to $15.6/10^{18}$ for CH_3Cl and to $33/10^{18}$ for Xe. It is evident that in the formulas we must always assume for water p = 1 kg/cm² and T = 273°.)

WE THUS SEE THAT FOR A GIVEN REYNOLDS' NUMBER AND WITH EQUAL DIAMETER OF FLUES, THE USE OF CARBONIC ACID AT 273° AND 1 KG/CM^2 WILL ECONOMIZE 3/4 OF THE POWER REQUIRED WITH ATMOSPHERIC AIR AND THAT COMPRESSION TO 15 KG/CM^2 AND COOLING TO 253° WILL REDUCE THIS POWER IN THE RATIO OF 1088 TO 1, WHICH CERTAINLY CONSTITUTES A REMARKABLE RESULT.

THE SPEED WILL BE REDUCED IN THE RATIO OF 1.85 TO 1 IN THE FIRST CASE AND IN THE RATIO OF 30 TO 1 IN THE SECOND CASE.

[*Margoulis here includes two additional cases that are essentially the same as the 2nd and 3rd case in Document 2-18(d).*]

WE THUS SEE THAT IF WE WISH TO ESTABLISH A LABORATORY FOR WELL DETERMINED VALUES OF V AND N, THE USE OF CARBONIC ACID AT 15 KG/CM^2 AND 253° REDUCES THE POWER REQUIRED IN THE RATIO OF 48.5 TO 1 AND REDUCES THE DIAMETER IN THE RATIO OF 22 TO 1.

IN A LABORATORY ALREADY BUILT, THE USE OF CARBONIC ACID AT 253° REDUCES THE POWER REQUIRED IN TEE RATIO OF 2.2 TO 1; THE PRESSURE SHOULD THEN BE EQUAL TO 68/100 OF THE PRESSURE REQUIRED WITH AIR.

[. . .]

REMARK I.—When, in the formulas giving the value of the power in the three cases considered, we compare the values of the terms characterizing the nature of the fluid, we find that water forms the least advantageous fluid for use in a laboratory, the more so as, being incompressible, its density can not be varied.

We may remark, however, that heating water to 100° reduces its coefficient of viscosity in the ratio of 6 to 1; in the lst case it then becomes more advantageous than air at atmospheric temperature and pressure. On this subject we may say that we may consider the use in wind tunnels not only of gas (that is, of fluids for which the temperature of saturation at 1 kg/cm2 is below 273° absolute) but also of vapors, which must be heated so that their temperature is above the temperature of saturation at the pressure at which they are utilized. Thus we may consider using water vapor, although its characteristics ($\mu = 0.89 \times 10^6$, $\rho_0 = 0.079$) are not favorable.

REMARK II.—In our theory of wind tunnels, we have called the coefficient of utilization of a tunnel (rs) the ratio of the kinetic energy of the fluid stream in the working section to the power of the engine running the fan.

For the closed circuit tunnels which we are studying, this coefficient takes the very simple form:

$$r = \frac{\text{[illegible]}}{\text{[illegible]}} \quad \text{[illegible]}$$

$N_s = vd/\nu = N/0.6$.

5.—PRELIMINARY PROJECT OF A WIND TUNNEL.

As a practical application let us consider a closed circuit tunnel, 2 m. in diameter, utilizing carbonic acid.

For tests of MODEL AIRPLANES we use carbonic acid compressed to 15 kg/cm² and cooled to 253° and we fix a speed of 30 m/sec. (corresponding, according to formula (3) to a speed in the air of 41 m/sec.).

The power required given by formula (1) will be 300 HP and the Reynolds' number realized will be 81.10^6 and equal to that realized by the largest existing airplanes (N is greater for large airplanes going slowly than for small planes flying at a high speed). This Reynolds' number will correspond to that of a racing plane flying at 650 km/hr.

The large laboratories now being planned will realize Reynolds numbers 8 times smaller with powers about 4 times greater. If in one of these laboratories having a diameter of 3 m. we wished to attain $N = 81.10^6$, we should require a power of 300 x 1088 x 2/3 = 217,000 HP. [*Footnote:*] (This figure must be considered rather as a proof of the impossibility of realizing this Reynolds' number in an ordinary tunnel than as an exact value of the required power. As a matter of fact, the speed in this case should reach 900 m/sec. and we have not the right to apply our formulas to such speeds, for which moreover the phenomena of compressibility would completely distort the results.)

For high speed tests, and especially for PROPELLER tests, the pressure must be below 1 kg/cm². Thus formula (1) shows that with carbonic acid at 0.5 kg/cm² and 253°, the speed realized with the same power of 300 h.p. would be:

$$30 \times \left[\frac{15}{0.5}\right]^{\text{[illegible]}} = 30 \times 2.53 = 76 \text{ m/sec.}$$

equivalent to a speed in the air of 76/0.74 = 103 m/sec., that is 370 km/hr.

If the pressure is reduced to 0.1 kg/cm², the equivalent speed in the air would be 570 km/hr., with an economy in power of 90%; to attain such a speed an ordinary tunnel of 3 m. would thus require

$$300 \times \left[\frac{3}{2}\right]^{2} \times 100 = 67{,}500 \text{ h.p.}$$

Lastly, we would say a few words on the realization of this wind tunnel.

There should be a closed circuit flue with continuous wall formed of thick sheet metal and protected from over-heating. Two doors sliding perpendicularly to the axis of the flue will isolate the working section while the model is being handled. The measuring devices will be placed in an airtight cabin on the wall of the working section, so that the rods of the model supports can traverse the wall of the flue by joints which should not be airtight. The measurements will be registered automatically by apparatus installed either in the cabin and visible from outside, or actually installed outside the cabin. In the latter case the apparatus will consist of manometers connected with dynamometric capsules placed in the cabin. The propeller-fan will have adjustable blades so that it can be adapted to the density of the fluid used in the tunnel.

[. . .]

Document 2-18(g), Max M. Munk, "On a New Type of Wind Tunnel," NACA Technical Note No. 60, 1921.

Introduction.

The difficulties involved in conducting tests on airplanes and airships in actual flight, difficulties greater in the early years of aviation than now, and the matter of expense also, induced investigators to seek information through tests upon models. The first of such tests was made by moving the model through stationary air either by means of a whirling arm or in a straight line. Later the method adopted was to suspend the model in a current of air flowing in a large tube. Wind tunnels of this type have become of increasingly great importance. At first the tunnels were only small pieces of physical apparatus in a laboratory, but at last they require an entire building. The latest wind tunnel of the Zeppelin Company in Germany provides a current of air ten feet in diameter, which has a velocity of 110 mi/hr. and absorbs 500 H.P.

The results obtained with this type of wind tunnel are of very great value and at the present time they are the chief source of information for the aircraft designer. However, there are certain critics who declare that the results of wind tunnel tests are valueless for purposes of design. Indeed, justification for such opinions is not wholly lacking. There is, in fact, no necessary and exact connection between the motion of air around a small airplane model and that around the full-sized airplane. Sometimes the results of the tests on models agree well with those observed with the airplane itself, but important cases are known where the two do not agree. Further, there are questions the answers to which it is most important for the designer to have, and yet the answer deduced from tests of models in wind tunnels would be absolutely wrong. There is always an uncertainty connected with such tests, because one is never quite sure whether or not the results thus obtained may be applied to full-sized bodies.

In spite of this uncertainty, wind tunnels have been of the greatest use in the development of aeronautics. Tests upon models led to the construction of streamlined bodies having small resistance, and of aerofoils of good section. Experiments in wind tunnels led to the discovery of the theorems referring to the lift of aerofoils and to the effect of combining several aerofoils. A wind tunnel is still the most important means available for scientific tests. It can not be denied, however, that it is becoming more and more difficult to find a problem suitable for study by a wind tunnel, which can be immediately applied in aeronautics. Many tests of a theoretical character can be suggested, but it is difficult to interpret them. There are many important and urgent tests with respect to the design of aircraft which should be performed, but the results would be worthless if they were carried out in a wind tunnel of the present type. The theory of non-viscous motion is almost complete, the tests referring to it have been made, and the field of investigation lying between non-viscous motion and actual motion in the air is cultivated so intensely that it is difficult to find a new problem.

For all these reasons, the author believes that his proposition to make use of compressed air in a new type of wind tunnel comes at the right moment. Tests in such a tunnel will give information concerning those questions which could not be investigated with the present tunnels because of the exaggerated effect of viscosity. The new type of tunnel is free of the uncertainty characteristic of the older type, and will indicate clearly what problems may be undertaken with the latter. It will make unnecessary many full-flight tests, and will mark a step in advance in aeronautics.

Let us then consider this new type of wind tunnel; its advantages, the difficulties attendant upon its use, and the special methods required.

I. PRINCIPLE OF THE PROPOSED WIND TUNNEL.

The main difference between the new type of wind tunnel and the ones now in operation is the use of a different fluid. The idea is to diminish the effect of viscosity. It would not be surprising if any other fluid were better than air in this respect. However, there does not seem to be such a fluid. Water, the liquid most easily obtained, has, indeed, a comparatively small viscosity; that is, the ratio of its viscosity to its density is only the 13th part of the similar ratio for air. The density of water, however, is so great that it is hardly possible to afford the horsepower required to force water through a large tunnel. But, even supposing that such a current of water could be obtained, e.g. by using a natural waterfall, it would be quite impossible to make tests in it. A model could not be made sufficiently strong to withstand the enormous forces acting on it, nor would it be possible to hold the model stationary. The same difficulty would be met in using any other liquid. As for gases other than air, carbonic acid is the only one which has a ratio of viscosity to density less than that of air, but the difference is so small that it would not pay to use it. It is less expensive to build a larger wind tunnel than to construct one for using carbonic acid gas, which has to be sealed and requires gasometers and other contrivances for holding the gas; and, further, the difficulties of operation would all be increased.

The fact that there is still another way of changing the fluid did not occur to any one for many years. Air may be used, but, if it is compressed, it becomes a fluid with new properties,—a fluid which is the best suited for reliable and exact tests on models. When air is compressed, its density increases but its viscosity does not. The increased pressure, it is true, requires strong walls for the tunnel to withstand the pressure and to prevent the air from expanding, but the increase of effectiveness secured for the tests is so great that it will pay to make the necessary changes and to replace the light walls of existing tunnels by heavy steel ones.

Before discussing this point we must first convince ourselves that the increase of pressure greatly increases the range and value of wind tunnel tests.

II. THE REYNOLDS NUMBER.

We are inclined naturally to compare small objects with large ones, with the assumption that all the qualities are independent of the size of the object, and that therefore the effects will be correspondingly smaller or larger. Coming at once to our problem, we are disposed to think that useful information for the designer of a flying machine may be obtained by observing the shapes of a butterfly or of various insects. In fact, this is the idea underlying tests on models. The absolute size of bodies is, it must be noted, a concept devoid of exact meaning. There is no absolute length; the length of any object can only be compared with that of another. Imagine all scales to have been destroyed, and let us not be conscious of the dimensions of our own bodies. Then we would not be able to decide whether our physical world should be called a dwarf one or a giant one—we would have no basis of comparison. We may therefore reasonably expect that a world on a different scale than ours would not differ essentially from ours if the same physical laws are valid in both.

This does not mean that all numerical ratios would be the same in both. It is not necessary that the same physical laws produce the same motion of a fluid, i.e. a geometrically similar motion, around two similar bodies. For the streamlines of a fluid around an immersed solid are not related to its shape by geometrical relations but by those derived from the laws of mechanics. It is possible, however, to derive the condition for obtaining such similar motions by extending our general considerations, without using mathematical processes.

We picture two phenomena, independent of each other; in particular we presuppose that no scale is carried from the seat of one phenomenon to that of the other. We consider separately two geometrically similar solids, each immersed in its own fluid, and endeavor, under these conditions, to see if we can detect any difference between them. If we can not, it would be absurd to expect two different motions, for one of the absolute truths, of which everyone is convinced, is that equal causes have equal effects. Further, where we can not find a difference, we believe, there is equality.

The two solids being supposed to be geometrically similar, no difference can be found between them, since we do not have a scale. By selecting any particular length of the body, its dimensions can provide us only with a standard length for the investigation of the relation between the body and the space qualities of the fluid.

For the same reason we can not detect any difference between the densities of the two fluids. Instead of considering density as the second standard unit—length being the first—we will obtain a more useful one and one to which we are more accustomed if we combine the concepts of volume and of density, and consider, for instance, the mass of a cube of unit volume filled by the fluid as our standard unit of mass.

The velocity of the fluid relative to the immersed body and at a great distance from it may be considered as a third standard unit.

It is essential to realize that it is not possible to find any relation between these three quantities. Neither do any two of them mean the same physical thing, nor can any two of them be combined in such a way that the third appears. If, therefore, the qualities mentioned were sufficient to determine all the features of the phenomenon, the flow around similar bodies would always be similar also; we would not be able to detect any difference. This is the actual case if the fluid is non-viscous, and therefore motions around similar bodies immersed in perfect fluids are similar.

The viscosity of a fluid is characterized as follows: consider a unit cube of the fluid, so chosen that in any plane parallel to one of its faces the fluid has a constant velocity; let the velocity of the fluid increase uniformly as one passes from this face across to the opposite one; then, if this change in velocity equals the unit of velocity, the force of friction on the face of the cube is called the coefficient of viscosity of the fluid. This appears to be a complicated concept, so we shall try to combine it with the two standard units of length and of mass, so that we obtain a velocity characteristic of the viscosity of the fluid, in combination with the other two qualities. Let us imagine now a unit cube of the fluid and any difference of velocity on the two opposite sides. There is a force of friction on each such face. If this force were to act on a unit cube of the fluid, i.e., on a unit mass, it would produce an acceleration, and in the course of being moved through a unit distance this cube would have its velocity increased from 0 to a definite value.

We may imagine the conditions of velocity on the two opposite faces of the unit cube varied until the force of friction is such that the resulting velocity of the second cube equals the difference in velocity at the two faces of the first cube. Half this velocity may be called the “Reynolds velocity.” It is characteristic of the viscosity of a fluid whose density is known, the dimensions of a solid body immersed in it being known, so as to furnish a unit of length. It can be determined for one of the two phenomena considered without reference to the other.

Therefore the ratio of the velocity of a fluid to this Reynolds velocity can be determined without reference to another phenomenon; it is an absolute number, called the Reynolds number. It may be the same in the case of two phenomena, or it may be different. If it is not the same, here is an essential difference between the phenomena, which may be observed and stated; and it would be most remarkable if, in spite of this difference, the fluids should have the same motions; it would in fact be impossible. But if, on the other hand, the two numbers are equal, we describe the motions of the two fluids as identical, taking viscosity into account, too. We may seek other differences; if there are none, it would be absurd to expect different motions.

Before extending our general considerations, we shall express the Reynolds number in terms of the quantities ordinarily used. Let r be the density of the fluid; m be the coefficient of viscosity; B be the characteristic length of the immersed solid. The mass of a cube of the fluid of length B on each edge is B3r; the force of friction on a face of area B2 is mBV1, when V1 is the difference of velocity at the two opposite faces; the work performed by this force if acting through a distance B is mB2V1, which equals the kinetic energy gained by the (second) cube of mass rB3 —i.e. $\frac{1}{2}\rho B^3 V_1^2$. Hence, if $V_1 = V_2$

$$\mu B V_1 = \frac{1}{2}\rho B^3 V_1^2 \quad \text{or} \quad V_1 = 2\frac{\mu}{\rho}\frac{1}{B}$$

the Reynolds velocity is one-half of this, i.e.

$$V_R = \frac{\mu}{\rho}\frac{1}{B}.$$

Writing V for the velocity of the fluid at a great distance from the solid, we have, by definition, the Reynolds number

$$\frac{V}{V_R} = \frac{VB}{\frac{\mu}{\rho}}.$$

If this has the same value in two phenomena of flow, they are alike in all respects. This may be called the Reynolds Law.

III. DEDUCTIONS FROM THE REYNOLDS LAW.

In the preceding section an attempt has been made to derive the expression for the Reynolds Law in as elementary a manner as possible. Only by knowing the basis of the law can one grasp its complete meaning and obtain the absolute confidence in it which is required for one to apply it safely. A mathematical proof was not given although it would have been shorter, for it would at the same time have been poorer of content.

We considered only the viscosity of the air, and did not discuss the other differences which exist between the tests on models and those on full-sized objects. The next stop is to investigate whether these differences do not introduce such errors that it would not be worthwhile simply to get rid of a possible error due to viscosity. Before doing this we must consider the deductions from the Reynolds Law so far as wind tunnel tests are concerned.

Let the span of the wing of a model be 3 ft., and the air velocity be 60 mi/hr. (= 88 ft./sec.). The kinematical viscosity of air at 0°C and normal pressure is 0.001433, i.e., about $^{1\ \text{ft.}^2}/_{700\ \text{sec.}}$. Hence the Reynolds number, regarding the span as the characteristic length is

$$\frac{3\ \text{ft.} \times 88\ \text{ft./sec.}}{\frac{1\ \text{ft.}^2}{700\ \text{sec.}}} \quad 185{,}000$$

That is, the velocity of the air in the tunnel would be almost <u>two hundred thousand times</u> the velocity called the Reynolds velocity. The full-sized airplane may have a span ten times as great, and the velocity of flight may be 1 ½ times as great; so that its Reynolds number is

$$10 \times 1.5 \times 185{,}000 = 2{,}775{,}000.$$

The magnitude of these numbers is surprising. The viscosity of the air is so small that in the neighborhood of the wings of an airplane the velocities produced by the forces of friction are only about <u>three millionth</u> of the velocity of flight. Equation (1) shows that the kinetic energy is proportional to the square of the velocity, while the work performed by the frictional force is proportional to the velocity. Hence the work performed by the frictional force is a minute fraction of the kinetic energy, $^{10}/_{185{,}000}$ in the model test referred to and $^{10}/_{2{,}775{,}000}$ in the case of the airplane. It seems surprising that any effect of friction can be detected, since it increases or decreases the kinetic energy by such a small fraction.

However, in the calculation of the Reynolds number one quantity is chosen arbitrarily. An arbitrary length occurs in the formula, and the magnitude of the number depends upon the choice of this length. Indeed, within a range of a dimension like the span of wings, the viscosity has almost no influence, but the smaller the range considered, the greater is the effect of viscosity, provided there are in this range the same differences of velocities as in the other. It must be noted that great differences of velocity occur within very small ranges. Near the surface of the wing velocities almost zero occur close to velocities of the magnitude of the velocity of flight. The character of the motion depends upon the stability of flow near the surfaces, and therefore, upon phenomena within small ranges. Within these the Reynolds number and the ratio of the acceleration to the viscosity is less than the number commonly used for comparison.

In any case, tests show that there are considerable differences of motion at the Reynolds numbers of the test and the flight. There is even instability, changing the character of the motion near the largest airships, on increasing its velocity, when flying at normal velocities.

These facts are not contradictions of the Reynolds Law, but, on the contrary, are in agreement with it. The surprising fact that, even when the Reynolds number is large, its influence is considerable, does not furnish the least reason-for doubting the correctness of a law based upon such elementary considerations.

Doubts about the Reynolds Law are based upon a different fact. In spite of the convincing proof, it happens that model tests at the same Reynolds number sometimes give quite different results. Now the Reynolds Law does not mean that at the same Reynolds number only one particular motion of the air is possible. It states that there is no difference between two phenomena with the same number. It may be that two or more motions are possible, but then they are possible in any case of the same Reynolds number.

There must be some reason, however, why the one or the other motion occurs. The reasons may be different. Sometimes there is a kind of hysteresis, the fluid remembered, as it were, what happened before this particular motion began; and the motion is different, for instance, if the angle of attack was larger or smaller immediately before. If such a phenomenon occurs with the full-sized body, it can be investigated by a model test at the same Reynolds number. Sometimes there is no such hysteresis, but the motion is very sensitive and is changed by the least change of the shape of the body, or of the smoothness of its surface, or with a change of the turbulence of the air. In such cases the motion around the full-sized body will be sensitive at the same Reynolds number as in the model tests. In this case it will be difficult to obtain the exact shape of the model and the right smoothness of its surface in order to have the same motion. At the same time other differences between the model test and the actual flight will produce differences in the results; but in such cases it is very doubtful whether two airplanes which are apparently identical have the same qualities. There does not exist a definite motion around the body at that particular Reynolds number. The careful investigator will observe this fact. Then the model test has shown all there is to be shown, and the method is not to be blamed for revealing phenomena which are surprising to the designer but true nevertheless.

IV. ERRORS DUE TO OTHER CAUSES.

There are still other differences between the tests on models and in actual flight, which will cause errors. It is necessary to realize that these, other than the one due to viscosity, do not affect seriously the value of the results of the tests. The new type of wind tunnel may, then, be expected to give reliable results.

The best evidence of the insignificance of these errors due to other causes is obtained by comparing tests made in different wind tunnels. It may be stated that there is found a certain agreement, but only with the same value of the Reynolds number. Reynolds himself deduced the law called by his name from experiments upon water flowing through pipes. In the two wind tunnels at Göttingen very careful investigations were made on aerofoils, over a large range of Reynolds numbers, and under very different conditions. Most results at the same Reynolds number agree well; even the results which can not be plotted on a curve against the Reynolds number appear much more regular when so plotted than when plotted in any other way. The results of these tests show that full-sized tests are much better than model ones, and provide the designer with clear, reliable and useful information. It is not sufficient, however, to compare the results of several tests in a perfunctory manner; care must be taken.

The Göttingen tests were not made under conditions geometrically similar; the two tunnels are not equally good. There are many tunnels which have more turbulence than is necessary, the designer having only taken care to obtain a uniform velocity. The older wind tunnel at Göttingen was exceedingly turbulent. The surfaces of the models were different purposely. Only the results obtained in good wind tunnels should be compared, the model having a proper surface, and the test being thoroughly laid out with reference to its influence. Then the differences would be smaller, and the reliability and usefulness of tests at the full-sized Reynolds number would appear more distinctly.

The matter may be considered also from another point of view. The tests show that under particular conditions the results of different tests agree very well; in certain cases only is good agreement lacking. Now it is not evident that the results may be expected to agree. It may be and is very probable that the motions which do not agree with each other are such sensitive motions as were described in the previous section. Of course this sensitiveness appears exaggerated if the differences in the test conditions are.

Theoretical reasons are not wanting, however, as to why the character of the motion depends almost exclusively on the ratio of the velocity of air to the Reynolds velocity, and not upon other ratios, e.g. the ratio of the Reynolds velocity to the velocity of sound in the medium, the latter being characteristic of its compressibility. It is not at all sufficient to state that this ratio is small, the Reynolds number (or its inverse) being small too. But the ratio of velocity to the velocity of sound has only one meaning; there is no arbitrary quantity used in forming it—such as B in the-Reynolds number. It does not matter whether this ratio is calculated for a wide range or for a small one. There is no discontinuity if the range or the compressibility passes to zero. In this case the fluid acts, with respect to its compressibility, like a perfect fluid. If the ratio of the velocity of flight to the velocity of sound is small, there is no physical reason for expecting a large influence. So much the less is the influence of a difference of compressibility in the tests on the model and in flight. Stated mathematically, any coefficient is a function of the two ratios; but, when both are small, the function is continuous with respect to the one and irregular with respect to the Reynolds number.

The same deduction is valid for the other errors; whether the cause be the contrivance used for supporting the model, the turbulence of the air, the variation of pressure or of velocity, or the finite distance of the walls of the tunnel or the boundaries of the current of air, the error is small provided the cause is. Their influence can be made as small as is necessary and customary in any technical test. Not only is the error small, it is regular, it can be compensated for, and it does not impair the comparison of different tests, as would the error due to viscosity.

V. THE DIMENSIONS OF A COMPRESSED AIR WIND TUNNEL.

In a tunnel filled with compressed air it is possible to obtain a Reynolds number much larger than in the tunnels now in use. But the range is limited in several respects, and its features must harmonize with each other in order to secure good results and also a low cost of operation.

The size of the tunnel is limited by the size of the models. It is not possible to make correctly shaped models if they are too small. The velocity of flow, on the other hand, must not be too great, lest the contrivances for supporting the model become so large that they disturb the motion. The stresses in the model must also be considered. This condition is duly respected if the dynamical pressure of the air does not exceed a particular value.

Hence the velocity must be the smaller the greater the density. This is desirable also with respect to the power required, to the increase of temperature produced, and to the dimensions of the fan and its shaft. The designer must also consider the time required to fill the tunnel with a compressor of proper dimensions. The pressure is limited only by questions of construction.

Let D be the diameter of the section where the model is placed, V be the velocity of the air and P be the maximum pressure. Then

Reynolds number	$R \alpha DVP$
Power required	$P \alpha D^2V^3P$
Heat produced per unit of surface	αV^3P
Dynamical pressure	$q \alpha V^2P$
Weight of tunnel walls	αD^3P

Energy required to fill tunnel	$\alpha D^3P^{1.25}$
Shaft diameter/diameter of tunnel	$\alpha VP^{1/3}$
(velocity of circumference of fan constant)	

The designer, in the first place, must choose the dynamical pressure he can permit without the supports of the model introducing too great an error. Then he may calculate the pressure needed for the Reynolds number desired, and the smallest diameter he considers proper. If he selects too high a pressure, the diameter must be made greater. Generally this will increase both the cost of operation and other difficulties. The Reynolds number and the dynamical pressure being given, the diameter and the velocity may be expressed as functions of the pressure.

If $R=aDVP$ and $q=bV^2P$ then $V=AP^{-1/2}$ [and] $D=BP^{-1/2}$ where a, b, A and B are constant coefficients. Substitutions may then be made in the expressions for the different quantities. It appears:

Power absorbed	$\alpha\ P^{-3/2}$
Heat produced per unit of surface	$\alpha\ P^{-1/2}$
Weight of tunnel walls	$\alpha\ P^{-1/2}$
Energy required to fill tunnel	$\alpha\ P^{-1/4}$
Shaft diameter/diameter of tunnel	$\alpha\ P^{-1/6}$

That is to say, all the quantities mentioned are more favorable the higher the pressure. This advantage must be compared with the difficulty of construction in consequence of high pressure, and the disadvantage of a smaller diameter. A theoretical limit for the pressure is the critical point where the air ceases to be a "perfect gas." In the neighborhood of this point the viscosity increases and therefore it is of no advantage to increase the pressure; but reason of construction would prevent this point being reached. The critical point of carbonic acid gas is, however, much lower, especially if it is cooled.

We can not close this chapter without considering the most interesting question, whether it would be possible to build a wind tunnel for tests of models of airships, having a Reynolds number equal to flight conditions. Let the length of the actual ship be 655 ft., and its velocity be 95 mi/hr. In a tunnel designed for tests of ship models only, the dynamical pressure could be increased to 2000 lbs./ft.^{2} The pressure could be 100 atmospheres (200,000 lb./ft.^{2}). Then the velocity would have to be just 95 mi/hr., and happens to be "full sized." The scale would be 1:100; the diameter could be 2 ft., and the power about 1000 HP. We think this tunnel could be made. It would give the designer information long desired.

The results of tests in a compressed air wind tunnel would be applied in the same way as is the practice with existing tunnels. The tunnel would give the ordinary coefficients, and the right ones. The Reynolds number could be calculated from the observed temperature and pressure.

The results would be, first of all, for the information of the designer of aircraft, giving him the true values of the coefficient required for any problem. The tunnel could also be used with advantage for scientific investigations. The differences in the Reynolds numbers which could be realized in such a tunnel are much greater than can be obtained in existing tunnels. At the same time, the pressures and the forces on the model vary only as the Reynolds number, if the same model is used, whereas in existing tunnels they vary as the square of this number.

Document 2-18(h), F. H. Norton, memorandum to G. W. Lewis, 1921.

October 6, 1921

From: Langley Memorial Aeronautical Laboratory
To: National Advisory Committee for Aeronautics
(Attention Executive Officer)
Subject: Design of compressed air wind tunnel

1. There are at present three Draftsmen spending almost all of their time on this work as it takes most of Mr. Morgan's time to supervise Mr. Pratt and Mr. McAvoy. Mr. Morgan feels that they are working very inefficiently and are getting very few results, as Dr. Munk does not seem to have any clear idea as to what he wishes in the engineering design excepting that he is sure that he does not want anything that Mr. Griffith or myself suggest. At the end of this week I must take Mr. Morgan and Mr. Pratt entirely away from this work and let Mr. McAvoy struggle along as best he can. For this reason it is requested that Mr. McAvoy be ordered to Washington so that he can be directly under Dr. Munk's supervision. I am getting so disgusted with the way the whole thing is being carried out that I would like to keep as much of it in Washington as possible.

[signed] F. H. Norton
F. H. Norton
Chief Physicist.

Document 2-18(i), "Compressed Air Wind Tunnel," NACA Annual Report for 1921.

THE LANGLEY MEMORIAL AERONAUTICAL LABORATORY.

In previous annual reports the committee described the progress made in the development of its field station at Langley Field, Va., for the prosecution of scientific research in aeronautics. The station now comprises three principal units, namely, an aerodynamical laboratory or wind tunnel, an engine dynamometer laboratory, and a research laboratory building, the latter including administrative and drafting offices, machine and woodworking shops, and photographic and instrument laboratories. The research laboratory and the wind tunnel building are of permanent brick construction; the engine dynamometer laboratory is housed in a temporary four-section steel airplane hangar.

The committee has recently completed the construction of a factory type building of brick and steel, designed to house the new compressed-air wind tunnel. It is expected that the new wind tunnel will be in operation about July, 1922.

The Langley Memorial Aeronautical Laboratory occupies a plot of ground known as plot 16, Langley Field, Va., the plot having been set aside for the committee's use by the Chief Signal Officer of the Army in 1916, at the time the site was selected as a proposed joint experimental station and proving ground for the Army and Navy air services and the advisory committee. The use of that plot of ground was officially approved by the Acting Secretary of War on April 24, 1919. The four buildings at present constituting the Langley Memorial Aeronautical Laboratory have been erected by the committee pursuant to authority granted by Congress.

COMPRESSED AIR WIND TUNNEL.

On June 9, 1921, the executive committee of the National Advisory Committee for Aeronautics authorized the construction at the Langley Memorial Aeronautical Laboratory of a compressed air type of wind tunnel designed by Dr. Max Munk, technical assistant of the committee.

The utility of the present type of wind tunnel is limited by the fact that owing to a "scale effect" the results of tests on the small models, which are usually about $\frac{1}{20}$ scale, are not immediately applicable to the full-size machine. Obviously it is very desirable to obtain, if possible, test results which are strictly proportional to those obtained in free flight. This condition may be realized by the use of a wind tunnel in which the air is compressed to about 20 atmospheres or more in order to compensate for the difference in the "scale" or Reynolds number for the model and for the full-size airplane.

The wind tunnel under construction has a diameter of 5 feet, the wind tunnel proper being placed within a steel cylinder 15 feet in diameter and 34 feet long. The steel cylinder has been tested for an internal pressure of 450 pounds per square inch and is designed for an average working pressure of 300 pounds per square inch.

The design of the cylinder further provides for a large door at one end and means for observing and operating the balance and setting wing angles from without the cylinder. The design of the balance has been carefully considered and due provision is made for the large forces to be measured.

The wind tunnel motor is 300 horsepower and the Reynolds number will be controlled by changing the air density rather than by changing the air speed. The air compressing units consist of two 300-horsepower compound compressors which compress the air to 115 pounds per square inch. The air is compressed into a receiving chamber and is then compressed by a 175-horsepower duplex booster compressor to the desired pressure in the test chamber. With the compressor units selected it will require approximately one hour to fill the chamber with air at a pressure at 300 pounds per square inch and every provision is being made in the design to make it unnecessary to open the chamber until the model is completely tested. Provision is also being made to maintain constant density so as to take care of temperature variations.

This tunnel when in operation will test models with a span of about 2 feet, but the results will be strictly comparable to similar data for a full scale machine, with a span of 30 feet, flying at 100 miles per hour. The construction of the models will therefore require special study and care.

Document 2-18(j), Max M. Munk and Elton W. Miller, The Variable Density Wind Tunnel of the National Advisory Committee for Aeronautics, *NACA Technical Report No. 227, 1925.*

SUMMARY

This report contains a discussion of the novel features of this tunnel and a general description thereof.

PART I

FUNDAMENTAL PRINCIPLES

By Max M. Munk

All the novel features of the new variable density wind tunnel of the National Advisory Committee for Aeronautics were adopted in order to eliminate the scale effect. The leading feature adopted was the use, as the working fluid, of highly compressed air rather than air under normal conditions.

It is not at once obvious that the substitution of compressed air eliminates the scale effect with aerodynamic model tests, although the necessary theoretical discussion has been available for some years. The idea of using compressed air must have occurred, in all probability, to many. It was not, however, till early in 1920 that the thought came to the writer; and in what follows is given his own line of reasoning, expressed in as simple language as possible.

In a paper entitled "Similarity of Motion in Relation to the Surface Friction of Fluids," by T. E. Stanton and J. R. Pannell, Philosophical Transactions A, volume 214, pages 199–224, 1914, will be found an excellent treatment of the subject, with references to the earlier discussions by Newton, Helmholtz, and Rayleigh.

Proceeding at once to the motion of a rigid body immersed in a fluid, the aim of the investigation is to obtain information concerning the fluid forces on such a body. Everything in connection with the problem has to be studied to that end, and has to be included in the investigation, whether this latter be analytical or, as we suppose now, experimental. There are the properties of the immersed body, its shape, its direction of motion, eventually the character of its surface. Even more important is the action of the fluid brought into play by these properties. Every detail of the motion of the fluid, together with the physical properties of the fluid, is immediately connected with the kind and magnitude of the forces created. We can only attain to a full knowledge of the forces created by regarding their cause, the fluid motion. All velocity components at all points of the flow are important and characteristic details of the cause of the forces on the body immersed in the fluid.

Then, why do investigators think that they can learn about what will occur on a large scale by observing what occurs on a small scale? Not from any intuitive feeling, inexpressible in words because devoid of thought; not from any vague metaphysical argument difficult to explain. There is a definite, extremely sound and simple reason why we expect to obtain reliable information from model tests. It is because we expect the two cases when compared with each other will perfectly, at all points, conform to each other, point by point. We do not mentally confine the geometrical similarity to the bodies immersed and to the dimensions of the entire arrangement, leaving as an unsolved and uninteresting question what the fluid does in the two cases. We do not expect that, for some mysterious reason, the fluid forces will correspond to each other in accordance with some simple rule. On the contrary, we include the flow patterns in our conception of "model." Any two corresponding portions of the flow, however small, are supposed to be similar with respect to shape and direction of the streamlines and with respect to the magnitude of velocities. The ratio of the lengths of a pair of corresponding portions of a streamline is supposed to be constant throughout the flow, and so is the ratio of two velocities corresponding to each other. We are under the impression that with respect to every detail the entire small-scale experiment is an exact replica of what occurs on a large scale, and we believe that the smallest quantity, whatever it is, occurs in a numerically corresponding way with the same conversion factor throughout the entire flow. In such a case, and only then, are we entitled to expect a simple relation between the fluid forces of the model test and those on the large-scale experiment. Such forces are the integrals of the elementary forces, and hence they stand in a constant ratio if the elementary forces do. This constant ratio can furthermore be expected to be a simple algebraic expression of the ratios between the characteristic quantities of the two arrangements.

Not only the model but the entire flow is the replica. There is a good illustration. It sometimes occurs in aerodynamics that the same body moved in the same way in the same fluid gives rise to different configurations of flow. The air forces are then also different.

The question, “Can we learn from aerodynamic model tests?” is thus reduced to the equivalent question, “Can flow patterns be geometrically similar?” If so the boundaries of the flow in general, and the immersed bodies in particular, have to be similar, but this alone is no sufficient reason why the similarity should extend to every streamline. The question whether a test is really a model test in the strict meaning, the question whether the small-scale flow is similar to the large-scale flow, requires a special examination. This examination will decide whether we can obtain reliable information from the test. If the flows are not exactly similar, but only approximately, the information also will only be approximately correct and not wholly reliable. There will exist a “scale effect.”

Two configurations of aerodynamic flow are created in different fluids under conditions geometrically similar. We wish to know whether the flow patterns are geometrically similar. We imagine a small-scale flow to exist exactly similar to the large-scale flow really existing, and we ask whether this imagined small-scale flow is compatible with the general laws of mechanics and hence identical with the actual small-scale flow. More particularly, we examine whether each particle of the imagined small-scale flow is in equilibrium, remembering that the corresponding particle of the large-scale flow is.

We assume first that no physical properties of the fluids, nor differences of such properties, have any influence on the shape of the flow pattern or on the fluid forces, except the density of the fluids. We dismiss also any external influence, like that of gravity. Then the only type of force brought into action by the motion of the fluid is the mass force of all the particles, and they are equalized by means of a variable pressure. The pressure distribution is only the natural reaction against changes of mutual positions of all the fluid particles, which changes must be compatible with the continuity conditions of the fluid. Each particle has the natural tendency to move straight ahead with constant velocity. This tendency is in conflict with the other tendency of each fluid particle to claim its own space, not to share its space with any other particle. These two conflicting tendencies lead to a distribution of varying pressure and to mass forces on the particles due to their motion along curved paths and with varying velocities. The pressure distribution gives rise to an elementary force on each particle, and the flow arranges itself in such a configuration that this pressure force is in equilibrium with the mass force.

Let us consider now the case when the linear dimensions are diminished in the ratio l_2/l_1 , all velocities diminished in the ratio V_2/V_1, and the density ρ_2, bears the ratio ρ_2/ρ_1 to the original density.

The mass forces are expressed mathematically by a type of term occurring in Euler's or Bernouilli's equation. Per unit volume, they are of the type

$$\frac{\text{Density x Velocity}^2}{\text{Length}}$$

and hence resultant mass forces of corresponding portions of the flow are of the type

$$\text{(1) Density x Length}^2\text{ x Velocity}^3$$

Such forces are in equilibrium with the pressure forces, and this determines the latter. Hence a change of density, scale, and velocity gives rise to a change of all elementary forces and hence of all resultant forces in the ratio

$$\frac{\rho_2 l_2^2 V_2^2}{\rho_1 l_1^2 V_1^2}.$$

The equilibrium of the particles remains unimpaired by the change of scale, and we conclude that corresponding flow patterns are necessarily similar. Hence, if the density of the fluid were the only property influencing the fluid paths and hence the fluid forces, all aerodynamic model tests would be interpreted correctly by the application of the so-called "square law." Corresponding fluid forces would be proportional to the fluid density, to the square of the velocity, and to the square of the linear scale. Accordingly, the absolute coefficients generally in use for expressing the magnitude of fluid forces would not only be absolute, but also constant for similar shapes and arrangements.

Experience has shown that the "square law" does not strictly hold, but that the air-force coefficients vary, sometimes slightly and sometimes in a very pronounced way. This is due to the influence of other properties of fluid, neglected before. There arises the question which other property of air is the principal cause of variations of flow patterns under conditions otherwise geometrically similar. All men who have devoted much thought to this problem agree that viscosity has such an effect, greatly in excess of that of other properties. The point is that the forces taken care of by the introduction of such properties of the fluid are very small when compared with the mass forces, which latter alone are governed by the "square law." This holds true at all points of the flow and with respect to all fluid properties, except with viscosity, where it only holds at most points. Viscous forces are proportional to the rate of sliding of adjacent layers of fluid, and are expressed by terms of the type,

$$(2)\quad \mu \frac{\partial u}{\partial y}\, dx\, dz$$

Here the constant quantity m is called the modulus of viscosity. *u*, a velocity, is at right angles to *y*, a Cartesian coordinate, together with *x* and *z*. Hence $^{\delta u}/_{\delta y}$ has the physical dimension of an angular velocity, $^{1}/_{\text{Time}}$. Now, this rate of sliding is small throughout an aerodynamic flow except near the boundary. There it may assume a very large magnitude. So, in spite of the small value of the modulus of friction of air, μ, the friction μ $^{\delta u}/_{\delta y}$ can assume a very large value and can become dominating at certain points of the flow. It can then produce essential changes of the entire flow pattern. Very little in detail is known about these things, and it seems useless to carry the discussion on at this point. Experience has shown that proper attention to the viscosity brings system and regularity into results of tests otherwise obscure and contradictory. It is for this reason that the elimination of the effect of viscosity for many years was thought desirable in the first place as a fundamental improvement of aerodynamic model tests, resulting in the elimination of the scale effect.

There has been some controversy as to whether these arguments are sufficient for the final decision that viscosity is the all-important fluid property. No arguments whatsoever will definitely decide that, but only final success. The separation of the physical effects to be taken into consideration for any practical purpose from those which may be neglected is a mental step which can not be accomplished by mere logics.

Granted, now, that viscosity is of practical importance, the question arises, Are similar flows possible in viscous fluids; and if so, under what conditions will the flows be similar? It is understood now that the arrangements are geometrically similar, that only the density r and viscosity m of the fluid have to be considered in addition to the linear scales of the arrangement and the ratio of the velocities.

The answer to the last question depends again upon the result of the examination whether each particle of an imagined small-scale flow, similar to an actual large-scale flow, is in equilibrium or not. Now, in viscous fluids the mass forces are not in equilibrium with the pressure forces, but in equilibrium with the combination of both the pressure forces and the viscosity forces. We have now three types of forces in equilibrium with each other, and that gives rise to a variety of possibilities. Two forces in equilibrium are, of necessity, numerically equal, hence if one of them be changed in a given ratio the other will too. With three forces, all three may be changed in a different ratio and still the equilibrium maintained.

The criterion for the similarity of flows is, therefore, that two of the three forces be changed in the same ratio. Then the third, in equilibrium with the two, will be changed in this same ratio and needs no special examination.

We compare the ratio of chance of the mass forces and of the viscosity forces with each other. We have seen already (1) that the mass forces are changed in the ratio [illegible].

The viscous forces being of the type [illegible]

are seen to be changed in the ratio [illegible].

Now, the two flow patterns will be similar and the test will be a strict model test only if the mass forces and the viscosity forces are changed in the same ratio. Hence we obtain, as the condition of an exact model test,

[illegible]

or, written in a different way,

(3) [illegible]

The expressions on either side of equation (3) are generally called “Reynolds numbers,” from Osborne Reynolds, who was the first to emphasize their importance. Since V and l are certain velocities and lengths in the two flows, corresponding to each other, but otherwise arbitrarily chosen as “characteristic” velocity or length, the value of one special Reynolds number in one single case has as little meaning as the scale of one single object. The equality of the Reynolds numbers of two arrangements, different but geometrically similar, expresses the dynamic equivalence of the two flows compared.

If the ratio of the two Reynolds numbers is different from unity the value of this ratio can be considered as a kind of relative scale between these two tests, not of the geometric scale but one which may be called dynamic scale. The ratio of the Reynolds numbers indicates differences in the relative importance of the mass forces and of the viscosity forces. A single Reynolds number, together with the definition of the characteristic velocity and length, is only an identification number, not much more than the street number of a house. Comparison of Reynolds numbers of flows where the conditions are not geometrically similar have hardly any meaning.

The preceding discussion has led us to the condition under which a wind tunnel will have no scale effect due to viscosity, and probably not any scale effect of practical importance. This condition is not equal velocity in model test and in flight. Full velocity is only of value for investigating certain original airplane parts and original flight instruments. The test with a model of diminished scale but at the velocity of flight is by no way distinguished from tests at other wind-tunnel velocities. On the other hand, if there is no scale effect expected, the Reynolds number being equal in both model test and free flight, the dynamic scale being 1, and if there are still arguments raised doubting the validity of such tests, such arguments hold with equal right or wrong against all other model tests, more particularly against such tests in ordinary atmospheric wind tunnels. For the principal difference between the variable density tunnel and atmospheric tunnels is the elimination of one source of error, of the one moreover, which is believed by most experts to be the most serious.

The fact is, then, that in general model tests in atmospheric wind tunnels are made at a Reynolds number smaller than in free flight. The linear dimensions of the model are largely diminished, and nothing is done to make up for this; the velocity is at best the same as in flight and the ratio μ/ρ is the same, the same fluid being used in test and in flight.

It is neither practical nor sound to make up for the diminution of the model by correspondingly increasing the velocity so as to obtain the original value of the product $V l$ as required in equation (3). It is not practical because such a wind tunnel would consume an excessively high horsepower, and because the air forces on the model would become excessive to such an extent as to make the test practically impossible. Such a method would also be unsound. For the differences in air pressure, which amount only to little more than 1 per cent in flight and in ordinary wind tunnels, would increase rapidly with velocities approaching the velocity of sound. Thereby the influence of the compressibility would be rapidly increased, and thus another error, now negligible, would make the results unsuitable for the desired purpose.

There remains then only the diminution of the ratio μ/ρ often denoted by v, in order to make up for the diminution of l in equation (3). This means the choice of another fluid. The use of water instead of air has been seriously proposed. With water $v = \mu/\rho$ is indeed seven times as small as with air. The problem of the large power consumption could eventually be solved, either by using a natural stream or by towing the model. However, water is about 800 times as dense as air, and hence the forces produced at the same velocity are 800 times as large, giving rise to stresses 800 times enlarged. It is practically impossible to make ordinary model tests with forces on the model 800 times as large as they are now.

What we need is a fluid which may be denser than atmospheric air at sea level, but only so to a moderate degree. Its dynamic viscosity modulus $v = \mu/\rho$ should be distinctly smaller than that of air, in order to make up for the scale of the model and eventually for the diminished velocity necessary for bringing down the pressure on the model and the absorbed horsepower. No such fluid is known under ordinary atmospheric conditions. Further consideration showed that a high pressure transforms air (or another gas) into a fluid suitable for wind-tunnel work giving results without scale effect. This fact depends on the physical property of air of keeping the same viscosity modulus m under all variations of pressure. This has been confirmed by experiments and is mentioned in treatises on physics. It is in keeping with the molecular theory, with denser air the average free paths are proportionally shorter. The viscosity modulus μ remains the same, but the density increases when the pressure increases. Hence the ratio $v = \mu/\rho$ varies inversely with the pressure (the temperature remaining unchanged). Hence we have

Kinematic viscosity ~ Pressure^{-1}
Model pressure ~ $\text{Pressure} \times \text{Velocity}^2$
Absorbed horsepower ~ $\text{Pressure} \times \text{Velocity}^3$

Assuming a model scale of say 10, we want a kinematic viscosity at least 10 times as small as with air. With pressure of 20 atmospheres we could get

Test velocity = ½ flight velocity.

Resultant model pressure = $20(\frac{1}{2})^2$, 5 times actual pressure.

Horsepower consumption of the tunnel = $20(\frac{1}{2})^3$, 2.5 that of an atmospheric tunnel of the same size and operating at full scale velocity.

Reynolds number = Reynolds number in free flight. These figures seemed practical. On them the design of the variable density wind tunnel of the National Advisory Committee for Aeronautics has been based.

More generally it can be seen that the principle of compressing the air allows any Reynolds number, even with a small model, if only the pressure can be produced and maintained. For keeping the Reynolds number constant and increasing the pressure in the ratio A, decreases the resultant pressure on the model as A^{-1} and the required horsepower as A^{-2}.

The throat diameter of 5 feet was chosen in order to be able to use the same models as in the atmospheric wind tunnel of the National Advisory Committee for Aeronautics. A small diameter would require smaller models, and it becomes increasingly difficult to construct such models accurate enough.

Furthermore, 5 feet is the smallest diameter for a closed tunnel where a man can walk and work without exceeding discomfort. The choice of the smallest diameter suitable was necessary in view of the large costs and difficulties for procuring a large enough housing strong enough to withstand an internal pressure of 25 atmospheres.

The same restriction of space decided the choice of a closed (not free jet) type of tunnel.

All other novel features can be traced back to the particular features of this tunnel, the large inside pressure and the larger resultant force on the model. They are described in the second part of this paper.

PART II

DESCRIPTION OF TUNNEL

By Elton W. Miller.

In the pages which follow a description is given in some detail of the tunnel and the methods of operation. The purpose in preparing this report is to make clear the testing methods employed, in order that the technical reports now in preparation may be better understood. The building of this tunnel was first suggested by Dr. Max M. Munk in 1921 (Reference 1). The writer has assisted Doctor Munk and Mr. David L. Bacon in the design and development of the mechanical features of the tunnel.

The tunnel is shown in sectional elevation in Figure 1, and consists briefly in an experiment section, E, 5 feet (1.52 meters) in diameter, with entrance and exit cones housed within a steel tank 15 feet (4.57 meters) in diameter and 34 feet 6 inches (10.52 meters) long. The air is circulated by a two-blade propeller, returning from the propeller to the entrance cone through the annular space between the walls of the tank and an outer cone, C_o. The balance, which is of novel construction, is mounted in the dead, or noncirculating, air space between the walls of the experiment section and the outer cone. The balance is operated electrically, and readings are taken through peepholes in the shell of the tank. Figures 2 and 3 are general views of the tunnel. Figure 4 is a plan of the building showing the tunnel and compressors.

The tank, which was built by the Newport News Shipbuilding & Dry Dock Co., of Newport News, Va., is capable of withstanding a working pressure of 21 atmospheres. It is built of steel plates lapped and riveted according to the usual practice in steam boiler construction, although, because of the size of the tank and the high working pressure, the construction is unusually heavy. There is a cylindrical body portion of 2 ⅛-inch (53.98 millimeters) steel plate with hemispherical ends 1 ¼ inches (31.75 millimeters) in thickness. Entrance to the tank is gained by an elliptical door K 36 inches (914 millimeters) wide by 42 inches (1,066 millimeters) high. The tank, which with its contents weighs about 100 tons (90.7 metric tons), is supported by a foundation of reinforced concrete.

The walls of the experiment section and cones are of wood; those of the experiment section consist of a series of doors which may be unbolted and removed to gain access to the balance. The cross-sectional area at the large end of the exit cone is substantially twice that of the experiment section, and the cross-sectional area of the return passage at its largest part is about five times that of the experiment section. Two honeycombs, H_p and H_s, are provided for straightening the air flow. Honeycomb H_P is of 2-inch (50.8 millimeters) round cells, while honeycomb H_S is of 1 ¼-inch (31.75 millimeters) square cells. The latter honeycomb is made removable to permit access to the experiment section; it is suspended from a removable trolley track by which it may be rolled to one side of the entrance cone. In order that the honeycomb may be returned to exactly the same place each time, it is made to seat on three conical points where it may be securely locked. Arrangements have also been made for adjusting the position of the honeycomb, as shown in Figure 5.

The propeller is driven directly by a synchronous motor of 250 horsepower (253.5 metric horsepower), which runs at a speed of 900 revolutions per minute. The synchronous motor has an advantage over the usual direct-current motor in that no complicated devices are necessary for maintaining a constant speed of revolution. Such variations in dynamic pressure as are made in the ordinary atmospheric tunnel by changing the air velocity are here made by changing the density of the air. It is therefore not necessary to vary the air velocity. Fluctuations of a fraction of a per cent occur, due to variations in the frequency of the electric current supplied to the motor; otherwise the velocity is constant for a given tank pressure. There is a slight increase in air velocity with an increase in tank pressure, as shown in Figure 16, but this is not objectionable.

The propeller, which is 7 feet (2.14 meters) in diameter, is mounted on a ball-bearing shaft which passes through one end of the tank. The stuffing box through which this shaft passes is only loosely packed, and air leakage is reduced to a minimum by means of oil which is fed by gravity from a reservoir above. The oil which is carried through the stuffing box is returned to the reservoir by a motor-driven pump.

Air compressors for filling the tank with air are shown in Figure 4. The air is compressed in two or three stages, according to the terminal pressure in the tank. A two-stage primary compressor is used up to a terminal pressure of about seven atmospheres. For pressures above this a booster compressor is used in conjunction with the primary compressor. The booster compressor may be used also as an exhauster when it is desired to operate the tunnel at pressures below that of the atmosphere. The primary compressors are driven by 250-horsepower synchronous motors and the booster compressor by a 150-horsepower squirrel-cage induction motor.

A diagrammatic drawing of the balance is shown in Figure 6. It consists essentially in a structural aluminum ring (1) which encircles the experiment section, two lever balances (2) and (3) for measuring lift, and a third lever balance (4) for measuring drag. The ring as it looked before assembly in the tunnel is shown in Figure 7. An assembly view in the tunnel is seen in Figure 8. The doors which surround the experiment section have here been removed, exposing the balance to view. The model is attached to the ring, by wires or other means, and all forces are transmitted to the ring and thence to the lever balances. The ring is suspended from lever balances (2) and (3), Figure 6, by the vertical members (9), of which there are four, two on each side. Cross shafts and levers are employed in order to carry the full weight of the ring to the two lever balances. The drag forces are transmitted by horizontal members (10) to bell cranks and thence by vertical members (11) to lever balance (4). Hanging from the ring are bridges which carry coarse weights (5) and (6). Any desired number of coarse weights may be added or removed by means of motor-driven cam shafts. A similar bridge carrying coarse weights (7) is hung from lever balance (4).

The sliding weights are moved by motor-driven screws to which are geared revolution counters; these may be read through peepholes in the shell of the tank. At the end of each beam is a pair of electrical contact points by which the beam may be made to balance automatically. The sliding weights may also be controlled by a manually operated switch. The lift balances are sensitive to plus or minus 10 grams and the drag balance to plus or minus 1 gram.

It is possible with this balance to measure any three components; for instance, lift, drag, and pitching moments. The lift is first approximately counterbalanced by increasing or decreasing the number of coarse weights hanging from the two weight bridges. The remainder is then counterbalanced by moving the sliding weights on the two lever balances. The drag is measured similarly. The total lift is the sum of the readings of the two lift balances; the pitching moment is the algebraic sum of the three balance readings multiplied by their respective lever arms.

The model may be supported in the tunnel by wires only, or by a combination of wires or struts and a spindle. In the latter case the spindle is attached to a vertical bar (12) which may be raised or lowered by appropriate gearing thus changing the angle of attack of the model. The angle of attack is indicated by an electrically controlled dial on the outside of the tank. The vertical bar (12) is protected from the air flow by a fairing (13).

Round wires of about 0.040 inch (1 millimeter) diameter have been used for supporting models, this much larger diameter being necessary because of the large forces, but streamlined wires of much larger section have been found preferable. These wires are attached to the balance ring below and to the model above, thus serving as struts or free columns to support the weight of the model when the air stream is not on. The struts may be attached to the wheels of the model as shown in Figure 9 or to threaded plugs screwed into the wings as in Figures 10 and 11. The advantage of the streamline wires over the round wires is illustrated in Figure 12. The wire and spindle drag for two airfoils and one airplane model have been reduced to a per centage of the gross minimum drag of the model with wires and plotted against Reynolds number.

All the various operations required within the tunnel while running, such as the shifting of balance weights and the setting of the manometers, are performed by small electric motors. It has been necessary, therefore, to carry a large number of electric wires through the shell of the tank. These wires pass through a suitable packing gland and are attached to terminal boards inside and out. The outside terminal board may be seen in Figure 3.

The airspeed is measured by static plates, one of which is located in the wall of the experiment section and the other in the wall of the other cone. The static plates are calibrated against Pilot tubes placed in the experiment section. A micromanometer designed especially for use in this tunnel is shown in Figure 13. Alcohol is the liquid used, and a head up to 1 meter may be measured. This manometer is similar in principle to that described in National Advisory Committee for Aeronautics Technical Note No. 81, but is different in that the index tube is stationary and the reservoir is raised or lowered by a motor-driven screw. A revolution counter geared to the motor indicates the head to 0.1 millimeter. It is possible to determine the dynamic pressure to an accuracy of plus or minus 0.2 per cent.

The dynamic-pressure distribution in the experiment section is represented by contour lines in Figure 14. This survey was made by using a number of Pitot tubes mounted on a bar which could be revolved in the tunnel. Observations were thus made at a large number of points. The dynamic pressure will be seen to vary in the region occupied by the model within a range of plus or minus 2 per cent. This survey was made at one and two atmospheres only. We know from check runs that the same flow condition holds for other pressures. The horizontal static pressure gradient in the tunnel at various pressures is shown in Figure 15. Pressures are given with reference to a static plate located in the wall of the experiment section. It will be noted that the curves which are plotted on semilog paper are parallel, indicating that the pressure gradient is proportional to the density. Operating data of general interest, as the time required for raising pressure in the tank, the time required to exhaust the tank, the power consumption of the compressors and drive motor, are shown in Figure 16. The velocity change with change of tank pressure is also shown. The energy ratio of the tunnel for various tank pressures is shown in Figure 17.

The building of this tunnel and the development of its various mechanical devices to a point where routine testing may be done has required the solution of a number of mechanical problems. This development period has passed, and the results now being obtained in the tunnel are believed to be as consistent and reliable as those obtained in any other wind tunnel. Two airplane models and thirty-seven airfoils have so far been tested. Tests of a Sperry Messenger airplane model provided with eight different sets of wings are now in progress.

The variation of the aerodynamic characteristics of an airplane model with change of scale is shown in Figure 18. This figure gives the polar curves of the

Fokker D-7 airplane model tested at various tank pressures. The minimum drag and the lift/drag ratio for this model, and also for a Sperry Messenger model, are plotted against the Reynolds number in Figure 19.

CONCLUSIONS

The underlying theory of the variable density tunnel has been discussed, the mechanical construction of the tunnel has been described, and some typical results obtained on an airplane model have been given. The tunnel is in continuous operation, and there is every reason to believe that the results obtained at the higher densities are truly representative of full-scale conditions.

REFERENCES

Reference 1.—”On a New Type of Wind Tunnel,” by Dr. Max M. Munk. N. A. C. A. Technical Note No. 60.

“Abriss der Lehre von der Flüssigkeits und Gasbewegung,” by Dr. L. Prandtl. Handwörterbuch der Naturwissenschaften, vol. 4.

“Experimental Investigations,” by O. Reynolds, Phil. Trans. 174 (1883), and “On the Dynamic Theory of Viscous Fluids,” Phil. Trans. A. 186 (1894).

“Similarity of Motion in Relation to the Surface Friction of Fluids,” by T. E. Stanton and J. R. Pannell, Phil. Trans. A, vol. 214, pp. 199-224, 1914.

Document 2-19(a–c)

(a) Ludwig Prandtl, letter to J. C. Hunsaker (translation), 30 March 1916, Hunsaker Collection, Box 4, File 4.

(b) Joseph S. Ames, NACA Headquarters, letter to J. C. Hunsaker, Navy Department, 15 October 1920, Hunsaker Collection, Box 1, File A.

(c) Ludwig Prandtl, *Applications of Modern Hydrodynamics to Aeronautics, Part 1*, NACA Technical Report No. 116 (Washington, DC, 1921).

It is difficult to overstate the degree of difference between the American and German approaches to understanding aerodynamics. While the American researchers opted for a practical, problem-solving approach, the Germans chose to pursue a rigorous, mathematically based understanding of the supporting theory. Ludwig Prandtl was the leader of the German school of thought, and he influenced many of the leading aerodynamicists of the twentieth century, including Max Munk and Theodore von Kármán. Most of Prandtl's publications were in German, but the NACA commissioned and published a seminal paper, *Applications of Modern Hydrodynamics to Aeronautics*, in 1921. Much of this extensive paper involved mathematical derivations, but portions are included herein to illustrate what Prandtl called "the leading ideas" and their experimental confirmation.

Not all Americans disdained the theoretical approach, however. Jerome Hunsaker and Joseph Ames, both of whom held graduate degrees in science or engineering, understood what a sound theoretical footing could add to the experimental work of the NACA, and both men were well acquainted with the German developments in this area. Hunsaker had known Ludwig Prandtl since his 1913 European inspection trip, and the latter's 30 March 1916 letter to Hunsaker, noting that "it has pleased me very much to see that you have agreed with my ideas regarding the boundary lamina of viscous fluids," indicates they shared a belief that the complex nature of fluid flow could be understood and predicted. Ames, too, realized the importance of the German theoretical work. Ames's enthusiasm for having obtained several copies of the "Technische Berichte," a collection of German wartime research reports, is readily apparent in his 15 October 1920 letter to Hunsaker, where he states, "The importance of the information . . . can not be over-estimated."

Such an attitude provides a stark contrast to the parochial views of Frederick Norton noted elsewhere in this chapter, and the NACA was fortunate to have the services of such strong and well-respected personalities to argue the merits of theoretical methods and press for the employment of Prandtl protégés. Through the immigration of Prandtl's students, especially von Kármán, theoretical aerodynamics came to the United States, and it gradually claimed a leading role in the saga of aeronautical progress.

Document 2-19(a), letter from Ludwig Prandtl to J. C. Hunsaker (translation), 1916.

[Göttingen, Germany, 30 March 1916]

TRANSLATION

Mr. J.C. Hunsaker
Instructor in Aeronautics, Massachusetts Institute of Technology
Boston, Mass.

Dear Sir:

Please accept my heartiest thanks for the gift of your survey of the Hydrodynamical Theory in its application to experimental aerodynamics as well as for the interesting collection of the results of the experiments of the aerodynamical laboratory at the Smithsonian Institute. I am forwarding under separate cover a small acknowledgement and hope that you will receive it as well as I received yours. (This may be translated, "hope that you will value it as highly as I valued yours").

With reference to the survey of the "Hydronamical Theory" it has pleased me very much to see that you have agreed with my ideas regarding the boundary lamina of viscous fluids. This is the first publication in the English language which has taken notice of it. The exceptional collection of original references has been a great pleasure as it has also been of great value to me. It contains some things with which I was not yet acquainted.

With greatest respect, I am, Yours very truly,

[Prof. Dr. L. Prandtl]

Document 2-19(b), letter from Joseph S. Ames to J. C. Hunsaker from NACA Headquarters, 1920.

[October 15, 1920]

Commander J. C. Hunsaker, U.S.N.
Navy Department
Washington, D.C.

Dear Commander Hunsaker:

It is with pleasure that I am informing you that the National Advisory

Committee for Aeronautics has been successful in obtaining a number of sets of the "Technische Berichte" and we are mailing you under separate cover volumes No. 1, 2, and 3. The Committee is also forwarding a carefully prepared translation of the index of the first three volumes together with a list of symbols used.

The "Technische Berichte" consists of separate memoranda issued as confidential material during the course of the war by the Aeronautical Supply Department of the German Air Force.

These reports contain practically all of the official German aeronautical information resulting from research conducted during the period of the war. The research work carried on in German covers a wide range of subjects, including the study of propellers, the pressure distribution on control surfaces, resistance of struts, methods of calculating performances, analysis of the performance and design of airplane engines, and systematic tests of approximately 350 wing sections.

The importance of the information contained in the "Technische Berichte" can not be over-estimated and it is the desire of this Committee that all research laboratories and individuals interested in aeronautical research should become familiar with the results of the aeronautical research carried on in Germany during the War.

Respectfully,
National Advisory Committee for Aeronautics
[signed] Joseph S. Ames
Chairman, Committee on Publications and Intelligence.

Document 2-19(c), Ludwig Prandtl, Applications of Modern Hydrodynamics to Aeronautics, Part 1*, NACA Technical Report No. 116, 1921.*

PREFACE.

I have been requested by the United States National Advisory Committee for Aeronautics to prepare for the reports of the committee a detailed treatise on the present condition of those applications of hydrodynamics which lead to the calculation of the forces acting on airplane wings and airship bodies. I have acceded to the request of the National Advisory Committee all the more willingly because the theories in question have at this time reached a certain conclusion where it is worthwhile to show in a comprehensive manner the leading ideas and the results of these theories and to indicate what confirmation the theoretical results have received by tests.

The report will give, in a rather brief Part I, an introduction to hydrodynamics which is designed to give those who have not yet been actively concerned with this science such a grasp of the theoretical underlying principles that they can follow the subsequent developments. In Part II follow then separate discussions of the different questions to be considered, in which the theory of aerofoils claims the greatest portion of the space. The last part is devoted to the application of the aerofoil theory to screw propellers. [*Note: Only Part I is included in this chapter.*]

At the express wish of the National Advisory Committee for Aeronautics I have used the same symbols in my formulae as in my papers written in German. These are already for the most part known by readers of the Technische Berichte. A table giving the most important quantities is at the end of the report. A short reference list of the literature on the subject and also a table of contents are added.

PART I.

FUNDAMENTAL CONCEPTS AND THE MOST IMPORTANT THEOREMS.

1. All actual fluids show internal friction (viscosity), yet the forces due to viscosity, with the dimensions and velocities ordinarily occurring in practice, are so very small in comparison with the forces due to inertia, for water as well as for air, that we seem justified, as a first approximation, in entirely neglecting viscosity. Since the consideration of viscosity in the mathematical treatment of the problem introduces difficulties which have so far been overcome only in a few specially simple cases, we are forced to neglect entirely internal friction unless we wish to do without the mathematical treatment.

We must now ask how far this is allowable for actual fluids, and how far not. A closer examination shows us that for the interior of the fluid we can immediately apply our knowledge of the motion of a nonviscous fluid, but that care must be taken in considering the layers of the fluid in the immediate neighborhood of solid bodies. Friction between fluid and solid body never comes into consideration in the fields of application to be treated here, because it is established by reliable experiments that fluids like water and air never slide on the surface of the body; what happens is, the final fluid layer immediately in contact with the body is attached to it (is at rest relative to it), and all the friction of fluids with solid bodies is therefore an internal friction of the fluid. Theory and experiment agree in indicating that the transition from the velocity of the body to that of the stream in such a case takes place in a thin layer of the fluid, which is so much the thinner, the less the viscosity. In this layer, which we call the boundary layer, the forces due to viscosity are of the same order of magnitude as the forces due to inertia, as may be seen without difficulty. [*Footnote*] (From this consideration one can calculate the approximate thickness of the boundary layer for each special case.) It is therefore important to prove that, however small the viscosity is, there are always in a boundary layer on the surface of the body forces due to viscosity (reckoned per unit volume) which are of the same order of magnitude as those due to inertia. Closer investigation concerning this shows that under certain conditions there may occur a reversal of flow in the boundary layer, and as a consequence a stopping of the fluid in the layer which is set in rotation by the viscous forces, so that, further on, the whole flow is changed owing to the formation of vortices. The analysis of the phenomena which lead to the formation of vortices shows that it takes place where the fluid experiences a retardation of flow along the body. The retardation in some cases must reach a certain finite amount so that a reverse flow arises. Such retardation of flow occurs regularly in the rear of blunt bodies; therefore vortices are formed there very soon after the flow begins, and consequently the results which are furnished by the theory of nonviscous flow can not be applied. On the other hand, in the rear of very tapering bodies the retardations are often so small that there is no noticeable formation of vortices. The principal successful results of hydrodynamics apply to this case. Since it is these tapering bodies which offer specially small resistance and which, therefore, have found special consideration in aeronautics under similar applications, the theory can be made useful exactly for those bodies which are of most technical interest.

For the considerations which follow we obtain from what has gone before the result that in the interior of the fluid its viscosity, if it is small, has no essential influence, but that for layers of the fluid in immediate contact with solid bodies exceptions to the laws of a nonviscous fluid must be allowable. We shall try to formulate these exceptions so as to be, as far as possible, in agreement with the facts of experiment.

2. A further remark must be made concerning the effect of the compressibility of the fluid upon the character of the flow in the case of the motion of solid bodies in the fluid. All actual fluids are compressible. In order to compress a volume of air by 1 per cent, a pressure of about one one-hundredth of an atmosphere is needed. In the case of water, to produce an equal change in volume, a pressure of 200 atmospheres is required; the difference therefore is very great. With water it is nearly always allowable to neglect the changes in volume arising from the pressure differences due to the motions, and therefore to treat it as absolutely incompressible. But also in the case of motions in air we can ignore the compressibility so long as the pressure differences caused by the motion are sufficiently small. Consideration of compressibility in the mathematical treatment of flow phenomena introduces such great difficulties that we will quietly neglect volume changes of several per cent, and in the calculations air will be looked upon as incompressible. A compression of 3 per cent, for instance, occurs in front of a body which is being moved with a velocity of about 80 m./sec. It is seen, then, that it appears allowable to neglect the compressibility in the ordinary applications to technical aeronautics. Only with the blades of the air screw do essentially greater velocities occur, and in this case the influence of the compressibility is to be expected and has already been observed. The motion of a body with great velocity has been investigated up to the present, only along general lines. It appears that if the velocity of motion exceeds that of sound for the fluid, the phenomena are changed entirely, but that up close to this velocity the flow is approximately of the same character as in an incompressible fluid.

3. We shall concern ourselves in what follows only with a nonviscous and incompressible fluid, about which we have learned that it will furnish an approximation sufficient for our applications, with the reservations made. Such a fluid is also called "the ideal fluid."

What are the properties of such an ideal fluid? I do not consider it here my task to develop and to prove all of them, since the theorems of classical hydrodynamics are contained in all textbooks on the subject and may be studied there. I propose to state in what follows, for the benefit of those readers who have not yet studied hydrodynamics, the most important principles and theorems which will be needed for further developments, in such a manner that these developments may be grasped. I ask these readers, therefore, simply to believe the theorems which I shall state until they have the time to study the subject in some textbook on hydrodynamics.

The principal method of description of problems in hydrodynamics consists in expressing in formulas as functions of space and time the velocity of flow, given by its three rectangular components, u, v, w, and in addition the fluid pressure p. The condition of flow is evidently completely known if u, v, w, and p are given as functions of x, y, z, and t, since then u, v, w, and p can be calculated for any arbitrarily selected point and for every instant of time. The direction of flow is defined by the ratios of u, v, and w; the magnitude of the velocity is $\sqrt{u^2 + v^2 + w^2}$. The "streamlines" will be obtained if lines are drawn which coincide with the direction of flow at all points where they touch, which can be accomplished mathematically by an integration. If the flow described by the formulas is to be that caused by a definite body, then at those points in space, which at any instant form the surface of the body, the components of the fluid velocity normal to this surface must coincide with the corresponding components of the velocity of the body. In this way the condition is expressed that neither does the fluid penetrate into the body nor is there any gap between it and the fluid. If the body is at rest in a stream, the normal components of the velocity at its surface must be zero; that is, the flow must be tangential to the surface, which in this case therefore is formed of stream lines.

4. In a stationary flow—that is, in a flow which does not change with the time, in which then every new fluid particle, when it replaces another particle in front of it, assumes its velocity, both in magnitude and in direction and also the same pressure—there is, for the fluid particles lying on the same stream line, a very remarkable relation between the magnitude of the velocity, designated here by V, and the pressure, the so-called Bernouilli equation—

$$p + \frac{\rho}{2} V^2 = \text{const.} \qquad (1)$$

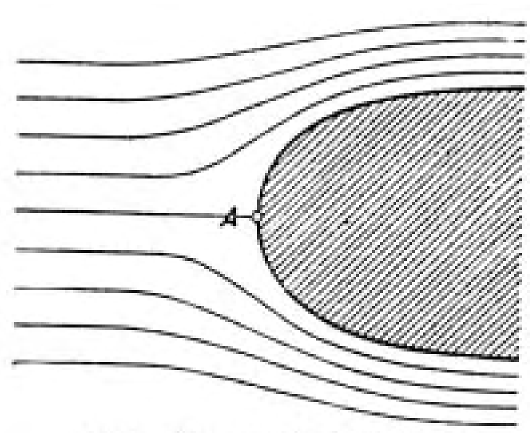


FIG. 1.—Flow around a blunt body.

(ρ is the density of the fluid, i.e., the mass of a unit volume). This relation is at once applicable to the case of a body moving uniformly and in a straight line in a fluid at rest, for we are always at liberty to use for our discussions any reference system having a uniform motion in a straight line. If we make the velocity of the reference system coincide with that of the body, then the body is at rest with reference to it, and the flow around it is stationary. If now V is the velocity of the body relative to the stationary air, the latter will have in the new reference system the velocity V upon the body (a man on an airplane in flight makes observations in terms of such a reference system, and feels the motion of flight as "wind").

The flow of incident air is divided at a blunt body, as shown in figure 1. At the point A the flow comes completely to rest, and then is again set in motion in opposite directions, tangential to the surface of the body. We learn from equation (1) that at such a point, which we shall call a "rest-point," the pressure must be greater by $\frac{\rho}{2}V^2$ than in the undisturbed fluid. We shall call the magnitude of this pressure, of which we shall make frequent use, the "dynamical pressure," and shall designate it by q. An open end of a tube facing the stream produces a rest point of a similar kind, and there arises in the interior of the tube, as very careful experiments have shown, the exact dynamical pressure, so that this principle can be used for the measurement of the velocity, and is in fact much used. The dynamical pressure is also well suited to express the laws of air resistance. It is known that this resistance is proportional to the square of the velocity and to the density of the medium; but $q=\frac{\rho}{2}V^2$; so the law of air resistance may also be expressed by the formula

$$W = c \cdot F \cdot q \qquad (2)$$

where F is the area of the surface and c is a pure number. With this mode of expression it appears very clearly that the force called the "drag" is equal to surface times pressure difference (the formula has the same form as the one for the piston force in a steam engine). This mode of stating the relation has been introduced in Germany and Austria and has proved useful. The air-resistance coefficients then become twice as large as the "absolute" coefficients previously used.

Since V^2 can not become less than zero, an increase of pressure greater than q can not, by equation (1), occur. For diminution of pressure, however, no definite limit can be set. In the case of flow past convex surfaces marked increases of velocity of flow occur and in connection with them diminutions of pressure which frequently amount to $3q$ and more.

5. A series of typical properties of motion of nonviscous fluids may be deduced in a useful manner from the following theorem, which is due to Lord Kelvin. Before the theorem itself is stated, two concepts must be defined. 1. The circulation: Consider the line integral of the velocity $\int V \cos (V, ds) \cdot ds$, which is formed exactly like the line integral of a force, which is called "the work of the force." The amount of this line integral, taken over a path which returns on itself is called the circulation of the flow. 2. The fluid line: By this is meant a line which is always formed of the same fluid particles, which therefore shares in the motion of the fluid. The theorem of Lord Kelvin is: In a nonviscous fluid the circulation along every fluid line remains unchanged as time goes on. But the following must be added:

(1) The case may arise that a fluid line is intersected by a solid body moving in the fluid. If this occurs, the theorem ceases to apply. As an example I mention the case in which one pushes a flat plate into a fluid at rest, and then by means of the plate exerts a pressure on the fluid. By this a circulation arises which will remain if afterwards the plate is quickly withdrawn in its own plane. See figure 2.

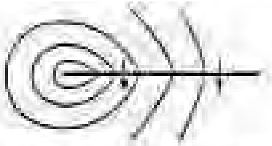


Fig. 2.—Production of circulation by introduction and withdrawal of flat plate.

(2) In order that the theorem may apply, we must exclude mass forces of such a character that work is furnished by them along a path which returns on itself. Such forces do not ordinarily arise and need not be taken into account here, where we are concerned regularly only with gravity.

(3) The fluid must be homogenous, i. e., of the same density at all points. We can easily see that in the case of nonuniform density circulation can arise of itself in the course of time if we think of the natural ascent of heated air in the midst of cold air. The circulation increases continuously along a line which passes upward in the warm air and returns downward in the cold air.

Frequently the case arises that the fluid at the beginning is at rest or in absolutely uniform motion, so that the circulation for every imaginable closed line in the fluid is zero. Our theorem then says that for every closed line that can arise from one of the originally closed lines the circulation remains zero, in which we must make exception, as mentioned above, of those lines which are cut by bodies. If the line integral along every closed line is zero, the line integral for an open curve from a definite point O to an arbitrary point P is independent of the selection of the line along which the integral is taken (if this were not so, and if the integrals along two lines from O to P were different, it is evident that the line integral along the closed curve OPO would not be zero, which contradicts our premise). The line integral along the line OP depends, therefore, since we will consider once for all the point O as a fixed one, only on the coordinates of the point P, or, expressed differently, it is a function of these coordinates. From analogy with corresponding considerations in the case of fields of force, this line integral is called the "velocity potential," and the particular kind of motion in which such a potential exists is called a "potential motion." As follows immediately from the meaning of line integrals, the component of the velocity in a definite direction is the derivative of the potential in this direction. If the line-element is perpendicular to the resultant velocity, the increase of the potential equals zero, i. e., the surfaces of constant potential are everywhere normal to the velocity of flow. The velocity itself is called the gradient of the potential. The velocity components *u*, *v*, *w* are connected with the potential Φ by the following equations:

$$u = \frac{\partial \Phi}{\partial x}; \quad v = \frac{\partial \Phi}{\partial y}; \quad w = \frac{\partial \Phi}{\partial z}. \tag{3}$$

The fact that the flow takes place without any change in volume is expressed by stating that as much flows out of every element of volume as flows in. This leads to the equation

$$\frac{\partial u}{\partial x} + \frac{\partial v}{\partial y} + \frac{\partial w}{\partial z} = 0 \tag{4}$$

In the case of potential flow we therefore have

$$\frac{\partial^2 \Phi}{\partial x^2} + \frac{\partial^2 \Phi}{\partial y^2} + \frac{\partial^2 \Phi}{\partial z^2} = 0 \tag{4a}$$

as the condition for flow without change in volume. All functions Φ (*x*, *y*, *z*, *t*), which satisfy this last equation, represent possible forms of flow. This representation of a flow is specially convenient for calculations, since by it the entire flow is given by means of the one function Φ. The most valuable property of the representations is, though, that the sum of two, or of as many as one desires, functions Φ, each of which satisfies equation (4a), also satisfies this equation, and therefore represents a possible type of flow ("superposition of flows").

6. Another concept can be derived from the circulation, which is convenient for many considerations, viz, that of rotation. The component of the rotation with

reference to any axis is obtained if the circulation is taken around an elementary surface of unit area in a plane perpendicular to the axis. Expressed more exactly, such a rotation component is the ratio of the circulation around the edge of any such infinitesimal surface to the area of the surface. The total rotation is a vector and is obtained from the rotation components for three mutually perpendicular axes. In the case that the fluid rotates like a rigid body, the rotation thus defined comes out as twice the angular velocity of the rigid body. If we take a rectangular system of axes and consider the rotations with reference to the separate axes, we find that the rotation can also be expressed as the geometrical sum of the angular velocities with reference to the three axes.

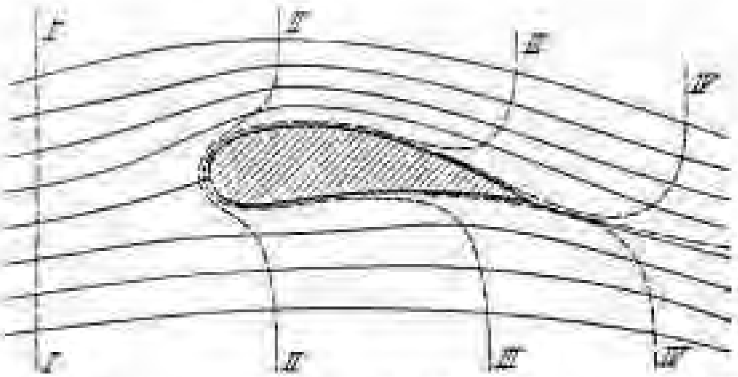


FIG. 3.—Successive positions of a fluid line in flow around a solid body.

The statement that in the case of a potential motion the circulation is zero for every closed fluid line can now be expressed by saying the rotation in it is always zero. The theorem that the circulation, if it is zero, remains zero under the conditions mentioned, can also now be expressed by saying that, if these conditions are satisfied in a fluid in which there is no rotation, rotation can never arise. An irrotational fluid motion, therefore, always remains irrotational. In this, however, the following exceptions are to be noted: If the fluid is divided owing to bodies being present in it, the theorem under consideration does not apply to the fluid layer in which the divided flow reunites, not only in the case of figure 2 but also in the case of stationary phenomena as in figure 3, since in this case a closed fluid line drawn in front of the body can not be transformed into a fluid line that intersects the region where the fluid streams come together. Figure 3 shows four successive shapes of such a fluid line. This region is, besides, filled with fluid particles which have come very close to the body. We are therefore led to the conclusion from the standpoint of a fluid with very small but not entirely vanishing viscosity that the appearance of vortices at the points of reunion of the flow in the rear of the body does not contradict the laws of hydrodynamics. The three components of the rotation ξ, η, ζ are expressed as follows by means of the velocity components *u*, *v*, *w*.

$$\xi = \frac{\partial w}{\partial y} - \frac{\partial v}{\partial z}; \quad \eta = \frac{\partial u}{\partial z} - \frac{\partial w}{\partial x}; \quad \zeta = \frac{\partial v}{\partial x} - \frac{\partial u}{\partial y} \tag{5}$$

If the velocity components are derived from a potential, as shown in equation (2), the rotation components, according to equation (5) vanish identically, since

$$\frac{\partial^2 \Phi}{\partial y \partial z} \quad \frac{\partial^2 \Phi}{\partial z \partial y}$$

7. Very remarkable theorems hold for the rotation, which were discovered by V. Helmholtz and stated in his famous work on vortex motions. Concerning the geometrical properties of the rotation the following must be said:

At all points of the fluid where rotation exists the direction of the resultant rotation axes can be indicated, and lines can also be drawn whose directions coincide everywhere with these axes, just as the stream lines are drawn so as to coincide with the directions of the velocity. These lines will be called, following Helmholtz, "vortex lines." The vortex lines through the points of a small closed curve form a tube called a "vortex tube." It is an immediate consequence of the geometrical idea of rotation as deduced above that through the entire extent of a vortex tube its strength—i. e., the circulation around the boundary of the tube—is constant. It is seen, in fact, that on geometrical grounds the space distribution of rotation quite independently of the special properties of the velocity field from which it is deduced is of the same nature as the space distribution of the velocities in an incompressible fluid. Consequently a vortex tube, just like a stream line in an incompressible fluid, can not end anywhere in the interior of the fluid and the strength of the vortex, exactly like the quantity of fluid passing per second through the tube of stream lines, has at one and the same instant the same value throughout the vortex tube. If Lord Kelvin's theorem is now applied to the closed fluid line which forms the edge of a small element of the surface of a vortex tube, the circulation along it is zero, since the surface inclosed is parallel to the rotation axis at that point. Since the circulation can not change with the time, it follows that the element of surface at all later times will also be part of the surface of a vortex tube. If we picture the entire bounding surface of a vortex tube as made up of such elementary surfaces, it is evident that, since as the motion continues this relation remains unchanged, the particles of the fluid which at any one time have formed the boundary of a vortex tube will continue to form its boundary. From the consideration of the circulation along a closed line enclosing the vortex tube, we see that this circulation—i.e., the strength of our vortex tube—has the same value at all times. Thus we have obtained the theorems of Helmholtz, which now can be expressed as follows, calling the contents of a vortex tube a "vortex filament": "The particles of a fluid which at any instant belong to a vortex filament always remain in it; the strength of a vortex filament throughout its extent and for all time has the same value." From this follows, among other things, that if a portion of the filament is stretched, say, to double its length, and thereby its cross section made one-half as great, then the rotation is doubled, because the strength of the vortex, the product of the rotation and the cross section, must remain the same. We arrive, therefore, at the result that the vector expressing the rotation is changed in magnitude and direction exactly as the distance between two neighboring particles on the axis of the filament is changed.

8. From the way the strengths of vortices have been defined it follows for a space filled with any arbitrary vortex filaments, as a consequence of a known theorem of Stokes, that the circulation around any closed line is equal to the algebraic sum of the vortex strengths of all the filaments which cross a surface having the closed line as its boundary. If this closed line is in any way continuously changed so that filaments are thereby cut, then evidently the circulation is changed according to the extent of the strengths of the vortices which are cut. Conversely we may conclude from the circumstance that the circulation around a closed line (which naturally can not be a fluid line) is changed by a definite amount by a certain displacement, that by the displacement vortex strength of this amount will be cut, or expressed differently, that the surface passed over by the closed line in its displacement is traversed by vortex filaments whose strengths add up algebraically to the amount of the change in the circulation.

The theorems concerning vortex motion are specially important because in many cases it is easier to make a statement as to the shape of the vortex filaments than as to the shape of the stream lines, and because there is a mode of calculation by means of which the velocity at any point of the space may be determined from a knowledge of the distribution of the rotation. This formula, so important for us, must now be discussed. If Γ is the strength of a thin vortex filament and ds an element of its medial line, and if, further, *r* is the distance from the vortex element to a point *P* at which the velocity is to be calculated, finally if α is the angle between *ds* and *r*, then the amount of the velocity due to the vortex element is

$$dw = \frac{\Gamma\, ds \cdot \sin\alpha}{4\pi r^2}. \tag{6}$$

the direction of this contribution to the velocity is perpendicular to the plane of *ds* and *r*. The total velocity at the point *P* is obtained if the contributions of all the vortex elements present in the space are added. The law for this calculation agrees then exactly with that of Biot-Savart, by the help of which the magnetic field due to an electric current is calculated. Vortex filaments correspond in it to the electric currents, and the vector of the velocity to the vector of the magnetic field.

As an example we may take an infinitely long straight vortex filament. The contributions to the velocity at a point *P* are all in the same direction, and the total velocity can be determined by a simple integration of equation (6). Therefore this velocity is

$$v=\frac{\Gamma}{4\pi}\int_{-\infty}^{+\infty}\frac{ds\,\sin\alpha}{r^2}.$$

As seen by figure 4, $s = h$ ctg α, and by differentiation,

$$ds=-\frac{h}{\sin^2\alpha}\,d\alpha \quad \text{Further } r=\frac{h}{\sin\alpha}\text{; so that}$$

$$v=\frac{\Gamma}{4\pi h}\int_0^\pi \sin\alpha\,d\alpha=\frac{\Gamma}{4\pi h}\Big[-\cos\alpha\Big]_0^\pi=\frac{\Gamma}{2\pi h}. \qquad (6a)$$

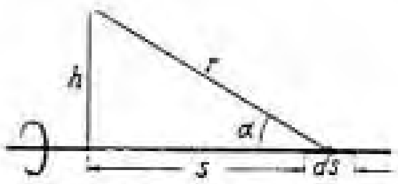


FIG. 4.—Velocity-field due to infinite rectilinear vortex.

This result could be deduced in a simpler manner from the concept of circulation if we were to use the theorem, already proved, that the circulation for any closed line coincides with the vortex strength of the filaments which are inclosed by it. The circulation for every closed line which goes once around a single filament must therefore coincide with its strength. If the velocity at a point of a circle of radius *h* around our straight filament equals *v* then this circulation equals "path times velocity" = 2 $\pi h \cdot v$, whence immediately follows $v=\Gamma/2\pi h$. The more exact investigation of this velocity field shows that for every point outside the filament (and the formula applies only to such points) the rotation is zero, so that in fact we are treating the case of a velocity distribution in which only along the axis does rotation prevail, at all other points rotation is not present.

For a finite portion of a straight vortex filament the preceding calculation gives the value

$$v = \frac{\Gamma}{4\pi a}(\cos\alpha_1 - \cos\alpha_2) . \qquad (6b)$$

This formula may be applied only for a series of portions of vortices which together give an infinite or a closed line. The velocity field of a single portion of a filament would require rotation also outside the filament, in the sense that from the end of the portion of the filament vortex lines spread out in all the space and then all return together at the beginning of the portion. In the case of a line that has no ends this external rotation is removed, since one end always coincides with the beginning of another portion of equal strength, and rotation is present only where it is predicated in the calculation.

9. If one wishes to represent the flow around solid bodies in a fluid, one can in many cases proceed by imagining the place of the solid bodies taken by the fluid, in the interior of which disturbances of flow (singularities) are introduced, by which the flow is so altered that the boundaries of the bodies become streamline surfaces. For such hypothetical constructions in the interior of the space actually occupied by the body, one can assume, for instance, any suitably selected vortices, which, however, since they are only imaginary, need not obey the laws of Helmholtz. As we shall see later, such imaginary vortices can be the seat of lifting forces. Sources and sinks also, i.e., points where fluid continuously appears, or disappears, offer a useful method for constructions of this kind. While vortex filaments can actually occur in the fluid, such sources and sinks may be assumed only in that part of the space which actually is occupied by the body, since they represent a phenomenon which can not be realized. A contradiction of the law of the conservation of matter is avoided, however, if there are assumed to be inside the body both sources and sinks, of equal strengths, so that the fluid produced by the sources is taken back again by the sinks.

The method of sources and sinks will be described in greater detail when certain practical problems are discussed; but at this point, to make the matter clearer, the distribution of velocities in the case of a source may be described. It is very simple, the flow takes place out from the source uniformly on all sides in the direction of the radii. Let us describe around the point source a concentric spherical surface, then, if the fluid output per second is *Q*, the velocity at the surface is

$$v = \frac{Q}{4\pi r^2} ; \qquad (7)$$

the velocity therefore decreases inversely proportional to the square of the distance. The flow is a potential one, the potential comes out (as line-integral along the radius)

$$\Phi = \text{const.} - \frac{Q}{4\pi r}. \tag{7a}$$

If a uniform velocity toward the right of the whole fluid mass is superimposed on this velocity distribution while the point source remains stationary—then a flow is obtained which, at a considerable distance from the source, is in straight lines from left to right. The fluid coming out of the source is therefore pressed toward the right (see fig. 5); it fills, at some distance from the source, a cylinder whose diameter may be determined easily. If V is the velocity of the uniform flow, the radius r of the cylinder is given by the condition $Q = \pi r^2 \cdot V$. All that is necessary now is to assume on the axis of the source further to the right a sink of the same strength as the source for the whole mass of fluid from the source to vanish in this, and the flow closes up behind the sink again exactly as it opened out in front of the source. In this way we obtain the flow around an elongated body with blunt ends.

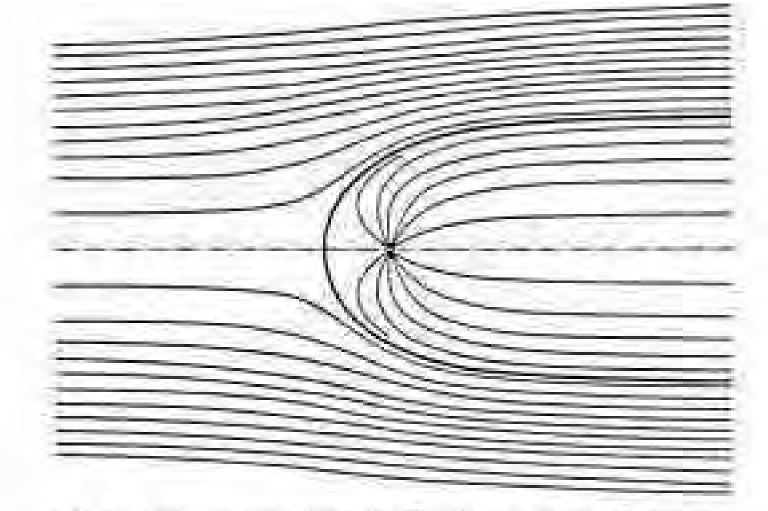


FIG. 5.—Superposition of uniform flow and that caused by a source.

10. The special case when in a fluid flow the phenomena in all planes which are parallel to a given plane coincide absolutely plays an important role both practically and theoretically. If the lines which connect the corresponding points of the different planes are perpendicular to the planes, and all the streamlines are plane curves which lie entirely in one of those planes, we speak of a uniplanar

flow. The flow around a strut whose axis is perpendicular to the direction of the wind is an example of such a motion.

The mathematical treatment of plane potential flow of the ideal fluid has been worked out specially completely more than any other problem in hydrodynamics. This is due to the fact that with the help of the complex quantities ($x + iy$, where $i=\sqrt{-1}$, is called the imaginary unit) there can be deduced from every analytic function a case of flow of this type which is incompressible and irrotational. Every real function, $\Phi\ (x, y)$ and $\Psi\ (x, y)$, which satisfies the relation

$$\Phi + i\,\Psi = f\,(\,x + iy\,), \tag{8}$$

where f is any analytic function, is the potential of such a flow. This can be seen from these considerations: Let $x + iy$ be put $= z$, where z is now a "complex number." Differentiate equation (8) first with reference to x and then with reference to y, thus giving

$$\frac{\partial\Phi}{\partial x} + i\frac{\partial\Psi}{\partial x} = \frac{df}{dz}\,\frac{\partial z}{\partial x} = \frac{df}{dz}$$

$$\frac{\partial\Phi}{\partial y} + i\frac{\partial\Psi}{\partial y} = \frac{df}{dz}\,\frac{\partial z}{\partial y} = i\frac{df}{dz} = i\frac{\partial\Phi}{\partial x} - \frac{\partial\Psi}{\partial x}.$$

In these the real parts on the two sides of the equations must be equal and the imaginary parts also. If Φ is selected as the potential, the velocity components u and v are given by

$$u = \frac{\partial\Phi}{\partial x} = \frac{\partial\Psi}{\partial y};\quad v = \frac{\partial\Phi}{\partial y} = -\frac{\partial\Psi}{\partial x}. \tag{9}$$

If now we write the expressions $^{\delta\Phi}/_{\delta x} + {}^{\delta v}/_{\delta y}$ (continuity) and $^{\delta\Phi}/_{\delta x} - {}^{\delta v}/_{\delta y}$ (rotation) first in terms of Φ and then of Ψ, they become

$$\left.\begin{aligned} \frac{\partial u}{\partial x} + \frac{\partial v}{\partial y} &= \frac{\partial^2\Phi}{\partial x^2} + \frac{\partial^2\Phi}{\partial y^2} = \underbrace{\frac{\partial^2\Psi}{\partial x\,\partial y} - \frac{\partial^2\Psi}{\partial y\,\partial x}}_{0} \\ \frac{\partial v}{\partial x} - \frac{\partial u}{\partial y} &= \frac{\partial^2\Phi}{\partial y\,\partial x} - \frac{\partial^2\Phi}{\partial x\,\partial y} = \frac{\partial^2\Psi}{\partial x^2} + \frac{\partial^2\Psi}{\partial y^2} \end{aligned}\right\} \tag{10}$$

It is seen therefore that not only is the motion irrotational (as is self-evident since there is a potential), but it is also continuous. The relation $\delta^2\Phi/\delta x^2 + \delta^2\Phi/\delta y^2 = o$ besides corresponds exactly to our equation (4a). Since it is satisfied also by Ψ, this can also be used as potential.

The function Ψ, however, has, with reference to the flow deduced by using Φ, as potential, a special individual meaning. From equation (8) we can easily deduce that the lines Ψ = const. are parallel to the velocity; therefore, in other words, they are streamlines. In fact if we put

$$d\Psi = \frac{\partial\Psi}{\partial x}dx + \frac{\partial\Psi}{\partial y}dy = 0, \text{ then } \frac{dy}{dx} = -\frac{\frac{\partial\Psi}{\partial x}}{\frac{\partial\Psi}{\partial y}} = \frac{v}{u}$$

which expresses the fact of parallelism. The lines Ψ = const. are therefore perpendicular to the lines Φ = const. If we draw families of lines, Φ = const. and Ψ = const. for values of Φ and Ψ which differ from each other by the same small amount, it follows from the easily derived equation $d\Phi + id\Psi = df/dz\,(dx + idy)$ that the two bundles form a square network; from which follows that the diagonal curves of the network again form an orthogonal and in fact a square network. This fact can be used practically in drawing such families of curves, because an error in the drawing can be recognized by the eye in the wrong shape of the network of diagonal curves and so can be improved. With a little practice fairly good accuracy may be obtained by simply using the eye. Naturally there are also mathematical methods for further improvement of such networks of curves. The function Ψ, which is called the "stream function," has another special meaning. If we consider two streamlines $\Psi = \Psi_1$ and $\Psi = \Psi_2$, the quantity of fluid which flows between the two streamlines in a unit of time in a region of uniplanar flow of thickness 1 equals Ψ_2-Ψ_1. In fact if we consider the flow through a plane perpendicular to the *X*-axis, this quantity is

$$Q = \int_1^2 u\,dy = \int_1^2 \frac{\partial\Psi}{\partial y}dy = \int_1^2 d\Psi = \Psi_2 - \Psi_1.$$

The numerical value of the stream function coincides therefore with the quantity of fluid which flows between the point *x*, *y* and the streamline Ψ = *o*.

As an example let the function

$$\Phi + i\Psi = A\,(x + iy)^n$$

be discussed briefly. It is simplest in general to ask first about the streamline Ψ = *o*. As is well known, if a transformation is made from rectangular coordinates to polar ones r, φ, $(x + iy)^n = r^n (\cos n\varphi + i \sin n\varphi)$. The imaginary part of this expression is $ir^n \sin n\varphi$. This is to be put equal to $i\Psi$. $\Psi = o$ therefore gives $\sin n\varphi = o$, i.e., $n\varphi = o, \pi, 2\pi$, etc. The streamlines $\Psi = o$ are therefore straight lines through the origin of coordinates, which make an angle $\alpha = \pi/n$ with each other, the flow is therefore the potential flow between two plane walls making the angle α with each other. The other streamlines satisfy the equation $r^n \sin n\varphi =$ const. The velocities can be obtained by differentiation, e.g., with reference to x:

$$\delta\Phi/\delta x = i\,\delta\Psi/\delta x = u - iv = An\,(x + iy)^{n-1} = Anr^{n-1}\,\{\cos\,(n-1)\,\varphi + i \sin\,(n-1)\,\varphi\}.$$

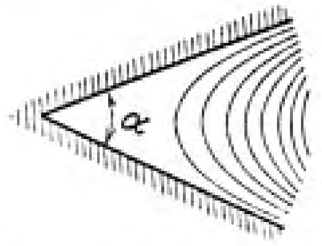


FIG. 6.—Uniplanar flow between plane walls making an angle α=45° with each other.

For $r = o$ this expression becomes zero or infinite, according as n is greater or less than 11, i.e., according as the angle α is less or greater than π (= 180°). Figures 6 and 7 give the streamlines for $= \pi/4 = 45°$ and $3/2\pi = 270°$, corresponding to $n = 4$ and $2/3$. In the case of figure 7, the velocity, as just explained, becomes infinite at the corner. It would be expected that in the case of the actual flow some effect due to friction would enter. In fact there are observed at such corners, at the beginning of the motion, great velocities, and immediately thereafter the formation of vortices, by which the motion is so changed that the velocity at the comer becomes finite.

It must also be noted that with an equation

$$p + iq = \varphi\,(x + iy) \tag{11}$$

the x-y plane can be mapped upon the p-q plane, since to every pair of values x,y a pair of values p,q corresponds, to every point of the x-y plane corresponds a point of the p-q plane, and therefore also to every element of a line or to every curve in the former plane a linear element and a curve in the latter plane. The transformation keeps all angles unchanged, i. e., corresponding lines intersect in both figures at the same angle.

By inverting the function φ of equation (11) we can write

$$x+iy = x(p+iq)$$

and therefore deduce from equation (8) that

$$\Phi+i\Psi = f\,[x(p+iq)] = \Phi(p+iq) \qquad (12)$$

Φ and Ψ are connected therefore with π and θ by an equation of the type of equation (8), and hence, in the p-q plane, are potential and stream functions of a flow, and further of that flow which arises from the transformation of the Φ, Ψ network in the x-y plane into the p-q plane.

This is a powerful method used to obtain by transformation from a known simple flow new types of flow for other given boundaries. Applications of this will be given in section 14.

11. The discussion of the principles of the hydrodynamics of nonviscous fluids to be applied by us may be stopped here. I add but one consideration, which has reference to a very useful theorem for obtaining the forces in fluid motion, namely the so-called "momentum theorem for stationary motions."

We have to apply to fluid motion the theorem of general mechanics, which states that the rate of change with the time of the linear momentum is equal to the resultant of all the external forces. To do this, consider a definite portion of the fluid separated from the rest of the fluid by a closed surface. This surface may, in accordance with the spirit of the theorem, be considered as a "fluid surface," i. e., made up always of the same fluid particles. We must now state in a formula the change of the momentum of the fluid within the surface. If, as we shall assume, the flow is stationary, then after a time dt every fluid particle in the interior will be replaced by another, which has the same velocity as had the former. On the boundary, however, owing to its displacement, mass will pass out at the side where the fluid is approaching and a corresponding mass will enter on the side away from which the flow takes place. If dS is the area of an element of surface, and v_n the component of the velocity in the direction of the outward drawn normal at this element, then at this point $dm = \rho dS \cdot v_n\, dt$. If we wish to derive the component of the "impulse"—defined as the time rate of the change of momentum—for any direction s, the contribution to it of the element of surface is

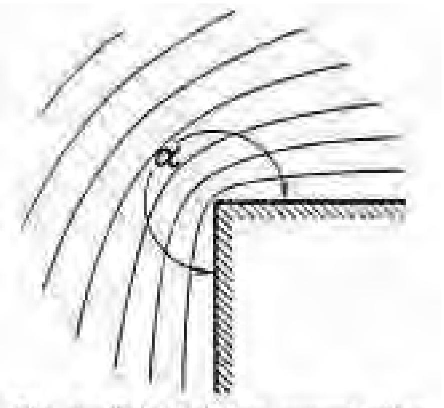


FIG. 7.—Uniplanar flow around plane walls making an angle 270° with each other.

$$dJ_s = v_s\,(dm\,/\,dt) = \rho dS \cdot v_n v_s. \qquad (13)$$

With this formula we have made the transition from the fluid surface to a corresponding solid “control surface.”

The external forces are compounded of the fluid pressures on the control surface and the forces which are exercised on the fluid by any solid bodies which may be inside of the control surface. If we call the latter P, we obtain the equation

$$\sum P_s = \int\int p \cdot \cos(n,s) \cdot dS + \rho\int\int v_n v_s dS \qquad (14)$$

for the s component of the momentum theorem. The surface integrals are to be taken over the entire closed control surface. The impulse integral can be limited to the exit side, if for every velocity v_s on that side the velocity v_s' is known with which the same particle arrives at the approach side. Then in equation (13) dJ is to be replaced by

$$dJ - dJ' = (v_s - v_s')\,(dm\,/\,dt) = \rho dS\, v_n\, (v_s - v_s'). \qquad (13a)$$

The applications given in Part II will furnish illustrations of the theorem.

Document 2-20(a–c)

(a) Max M. Munk, memorandum to George W. Lewis, 10 March 1925, RA file 44, Historical Archives, NASA Langley.

(b) Fred E. Weick and Donald H. Wood, *The Twenty-Foot Propeller Research Tunnel of the National Advisory Committee for Aeronautics*, NACA Technical Report No. 300 (Washington, DC, 1928).

(c) Fred E. Weick, excerpts from *From the Ground Up* (Washington, DC: Smithsonian Institution Press, 1988), pp. 46–67.

While the VDT provided unprecedented capabilities for airfoil research at high Reynolds numbers, it was not well suited for certain other investigations, such as propeller testing. No practical means to utilize model propellers existed, because they did not deflect during operation the same way full-size ones did; this introduced significant error. After a promising propeller design had been tested for structural integrity and to determine its basic characteristics, an evaluation of its actual performance on an airplane required expensive and sometimes dangerous flight tests. However, accurate measurements in flight were difficult at best. Langley had a dynamometer to get reliable data on engine performance, and the NACA had sponsored propeller research in Stanford University's Eiffel-type tunnel since 1917, but no valid laboratory method of studying the entire propulsion system and its interaction with the aircraft fuselage existed in the mid-1920s.

The NACA's response was to construct the first very-large wind tunnel, the Propeller Research Tunnel (PRT). The PRT, completed in 1927, was a closed tunnel with an open test section, often referred to as an open-jet type. The nozzle supplied a twenty-foot-diameter air stream with velocities up to 110 miles per hour. A tunnel this size required more electrical power than was available at LMAL, so the PRT used two 1,000 horsepower diesel engines that had been removed from a navy submarine. The balance could support an airplane or a substitute "test fuselage" that featured an onboard dynamometer to directly measure engine torque. Technicians developed optical methods to measure blade deflection during operation. The PRT quickly proved its worth for propeller testing in conditions that were very close to those in actual flight, but since it was the first tunnel large enough to accommodate full-size airplanes (except for their full wing spans), its role soon expanded to include drag studies of aircraft components such as landing gears, radiators, tail planes, and—perhaps most significantly—radial engine cowlings. The PRT also served as the model for even larger wind tunnels that the NACA would build over the next two decades.

As the 10 March 1925 letter from Max Munk to George Lewis shows, it was Munk who conceived this large tunnel, and it was he who convinced Lewis that such a tunnel would be not only feasible, but also tremendously valuable to the NACA. As he had with the VDT, Munk correctly assessed a current research problem—propeller performance tests that were not possible in the VDT—and devised a solution that was at once bold and theoretically sound. Unfortunately, he ran into serious non-technical problems when Lewis put him in charge of the PRT's construction.

While Max Munk brought a keen theoretical mind to America in 1920, he also brought a large ego and an unremitting belief in the German organizational model, in which a leader's ideas were unquestioningly accepted and practiced. Munk was clearly brilliant, but he did not—perhaps could not—adapt to the more egalitarian mode of operation at Langley. The other Langley researchers and engineers considered his style overbearing and excessively autocratic, and they finally revolted against him. No single document fully portrays the situation, but Fred Weick recalled several incidents in his autobiography, *From the Ground Up*, which gives a good picture of the mismatch between Munk and other Langley personnel. In addition, a letter from Fred Norton to NACA Director of Aeronautical Research George Lewis, one of many written by Langley managers between 1921 and 1927, illustrates his frustration with the situation. However, these letters met blind eyes, as Lewis was Munk's biggest supporter. Even after the mass resignation of LMAL section heads in 1927, Lewis tried, unsuccessfully, to find some way to retain Munk in the Washington office. Munk, however, would not accept this loss of "his" laboratory and chose to resign from the NACA. Although Munk maintained some visibility in aeronautics for a while, notably as the author of a series of articles on basic aerodynamics in *Aero Digest* during the 1930s, his resignation from the NACA effectively marked the end of his serious contributions to the field.

Document 2-20(a), Max M. Munk, memorandum to George W. Lewis, 1925.

March 10, 1925.

MEMORANDUM for Mr. Lewis.

Subject: Laboratory for Testing Full Size Propellers.

1. At the present, our aerodynamic free flight research has not yet been brought forward enough to separate the performance of an airplane into that of

the engine, that of the propeller, and that of the remaining airplane. Nor has it been possible to obtain any definite comparisons between the results of model tests and free flight tests.

The first step taken to logically and systematically adapt our research work to the requirements of the practice was the construction of the variable density tunnel in order to get rid of the scale effect. This step was successful as far as could be expected. The model results in the new tunnel agree better with free flight experiments than with results from ordinary tunnels. The influence of the propeller slipstream will be included in the variable density tunnel tests by providing apparatus for driving and observing a model propeller.

The model propeller in the variable density tunnel will be free from the scale effect proper. It will, however, experience pressures and hence elastic (and permanent) deformation different from those of the full size propeller. Further, the influence of the compressibility of the air will not properly be taken care of. It will be very difficult and expensive to make an exact model of the propeller and of the portion of the airplane in its vicinity. At last, the variable density tunnel is fully occupied for the next years by tests not directly referring to the propeller.

2. I wish, therefore, to recommend that the tests referring to propeller performance and to the influence of the other portions of the airplane on the performance of the propellers be excluded from the research assigned to the variable density tunnel. I wish further, to recommend that the mentioned part of our research program be turned over and assigned to a special laboratory to be constructed for this purpose.

A similar laboratory was built during the last war at Fishamend (near Vienna, Austria) by Prof. Th. v. Karman. No published description has ever come to my attention. As far as I know, this laboratory was destroyed under the terms of the Peace Treaty of Versailles. It may be possible to obtain some information about it from Dr. v. Karman, now at Aachen.

3. The tests to be made in the laboratory would comprise:

(a) Determination of Torque and Thrust of actual propellers (up to 10 ft. diameter, say) at different ratios U/V of the propeller tip velocity U to the velocity of motion and at actual speeds.

The tests would be run in air of sea level pressure and would be correct as to deformation of the propeller, compressibility of the air, and would not involve any scale effect.

(b) The same tests with the propeller in front of any airplane fuselage, and in the presence of the wings and of other parts of the airplane.

The results will then include the forces on the several parts of the airplane and will give the actual merits of the propeller under these conditions,

(c) Propeller tests with the axis of the propeller slightly tilted. This will correspond to the use of propellers under different angles of attack.

(d) Determination of the engine performance. The propeller can be driven by any airplane engine and the tests can then be made to include a test of that engine. In particular, the merits of the cooling system can be examined.

(e) The tests will further give information of the merits of different shapes of fuselages or engine cars taking into account the slipstream produced by the propeller under different conditions.

(f) In special cases, the laboratory would be available as the largest wind tunnel in the world.

4. The laboratory is contemplated to consist of a 20-ft. wind tunnel with open jet test chamber and with the return channels being under compression. At first glance, the cost of a 20-ft. tunnel would seem to be prohibitive, but a closer examination will show that such is not the case. Our present atmospheric 5-ft. tunnel could be built much cheaper and must not be taken as a basis for the costs of the new laboratory. Our variable density tunnel is costly on account of the high compression of the air used. This new 20-ft. tunnel is supposed to be built especially for the purpose indicated and will be comparatively cheap on account of the following items:

1. The power plant will consist of [two] 400 H.P. combustion engines, say Liberties, and not of electric motors. A third engine will be required to drive the propeller.
2. The building will be made chiefly of wood frame construction, and internal steel rod bracing used where required.
3. No high priced special balances will be built but only ordinary balances will be used as can be purchased on the market.

No estimate of the costs has been made yet, but I believe that the entire cost will not be in excess of the cost of an ordinary good wind tunnel. The results will be of much greater value, being free from any important error and giving plenty of information needed and wanted by the practice for many years. The research program will not be exhausted for many years, but on the contrary will probably grow larger and larger as the development of aeronautics goes on.

5. The need for a laboratory as the one in question is demonstrated by the many unsuccessful attempts to require the information to be obtained in other ways.

There have been constructed giant whirling arms, towers on railroad cars, dynamometer hubs on airplanes, airplanes gliding along cables in an airship shed, and the Fishamend Laboratory mentioned above. (The latter was possible with open return.)

[signed] Max M. Munk.

Document 2-20(b), Fred E. Weick and Donald H. Wood, The Twenty-Foot Propeller Research Tunnel of the National Advisory Committee for Aeronautics, *NACA Technical Report No. 300, 1928.*

SUMMARY

This report describes in detail the new propeller research tunnel of the National Advisory Committee for Aeronautics at Langley Field, Va. This tunnel has an open jet air stream 20 feet in diameter in which velocities up to 110 M.P.H. are obtained. Although the tunnel was built primarily to make possible accurate full-scale tests on aircraft propellers, it may also be used for making aerodynamic tests on full-size fuselages, landing gears, tail surfaces, and other aircraft parts, and on model wings of large size. [Italics appeared in original.]

INTRODUCTION

The need of an accurate means for making aerodynamic measurements on full-size aircraft propellers has been realized for some time. Tests on model propellers in wind tunnels are not entirely satisfactory because the deflection of the model is different from that of a similar full-scale propeller, which introduces a rather large error in some cases. The difference in scale and tip speed between the model and full-scale propeller is also a cause of error. Full-scale flight tests on propellers are made, of course, under the correct conditions, but at the present time they can not be made with sufficient accuracy.

In the spring of 1925 the design and construction of a propeller research wind tunnel to fill this need for full-scale tests was started by the National Advisory Committee for Aeronautics. It was completed during the summer of 1927 and testing has been carried on since that time. The tunnel is of the open-jet type with an air stream 20 feet in diameter. This is large enough to permit the mounting of a full-sized airplane fuselage with its engine and propeller. The open-jet type is particularly suitable for testing propellers because no corrections are required for tunnel-wall interference. (References 4 and 5.) Also, since with the open-jet type the inside of the experiment chamber is free from restricting walls, the installation of the objects to be tested is relatively simple.

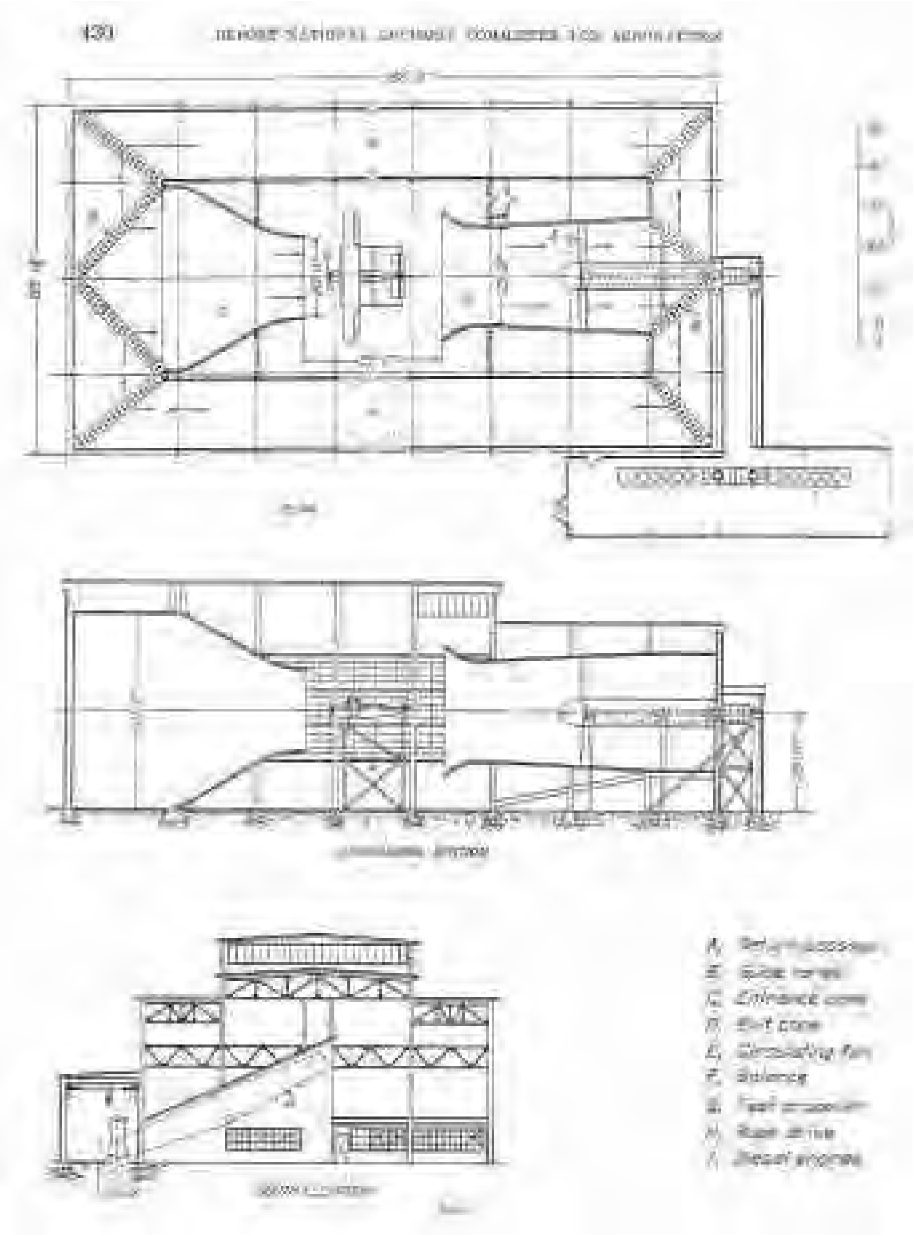
This wind tunnel makes it possible for the first time to make aerodynamic tests with laboratory accuracy on full-scale aircraft propellers and also on full-scale fuselages, engine cowlings, cooling systems, landing gears, tail surfaces and other airplane parts. Full-scale tests of wings are not, of course, possible in a 20-foot air stream, but large model wings (12 feet in span) can be tested at comparatively high values of the Reynolds number.

Dr. Max M. Munk is responsible for the general arrangement of the propeller research tunnel, and the detail design and construction were carried out under the direction of Mr. E. W. Miller of the laboratory staff.

DESCRIPTION OF THE TUNNEL

GENERAL

The propeller research tunnel of the National Advisory Committee for Aeronautics is located at Langley Field, Va., on a plot adjacent to the committee's other research equipment. Figure 1 is a diagrammatic sketch indicating the general arrangement of the tunnel and Figure 2 illustrates the exterior appearance.



The tunnel proper is a wood walled steel-framed structure 166 feet long and 89 feet wide, having a maximum height of 56 feet. The walls are of 2 inch by 6 inch tongued and grooved pine sheathing attached to steel columns with wooden nailers. Except for the fact that the walls are on the inside of the framing only and that the heights vary from point to point, standard structural practice is followed.

The tunnel (fig. 1) is of the open-throat, closed test chamber, return passage type. The direction of the air flow is indicated by arrows. The air is drawn across the test chamber into the exit cone by a propeller fan. After passing through the fan the air column divides, passes through successive sets of guide vanes at the corners, and returns through the side passages to the entrance cone. The areas of the passages are varied in the case of the exit cone by varying the diameter, and of the return passages by sloping the roof and floor, so that the velocity of the moving air is gradually decreased at the large end of the entrance cone to about one-eighth that through the test chamber. It is then rapidly accelerated in passing through the entrance cone.

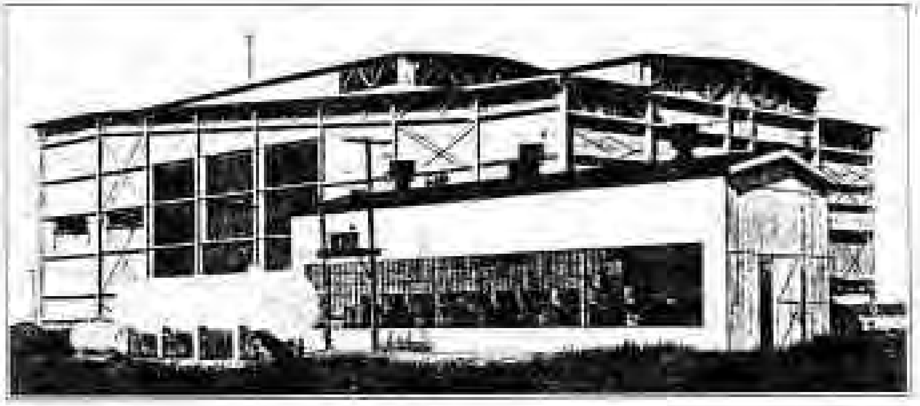


Fig. 2

TEST CHAMBER

The test chamber is about 50 by 60 by 55 ft., located, as shown in Figure 1, near the center of the tunnel structure. Large windows in the east and west walls afford ample light. Doors open out of the west wall to permit the movement of material to and from the test chamber. An electric crane traveling along a roof truss is useful in lifting loads about the chamber and onto the balance. Electrical outlets for light and power are provided at convenient points.

ENTRANCE CONE

The entrance cone (fig. 3) is of 50 ft. square section at the large end, changing to 20 ft. diameter in its length of 36 ft. It is constructed of a double layer of ¾ in.

by 2 in. sheathing bent, fitted, and nailed to wood forming rings. These, in turn, are bolted to angle clips riveted to I-beams bent to proper shape. A built-up wood ring forms the end of the cone. At the large end the cone runs into the return passage on a gradual curve.

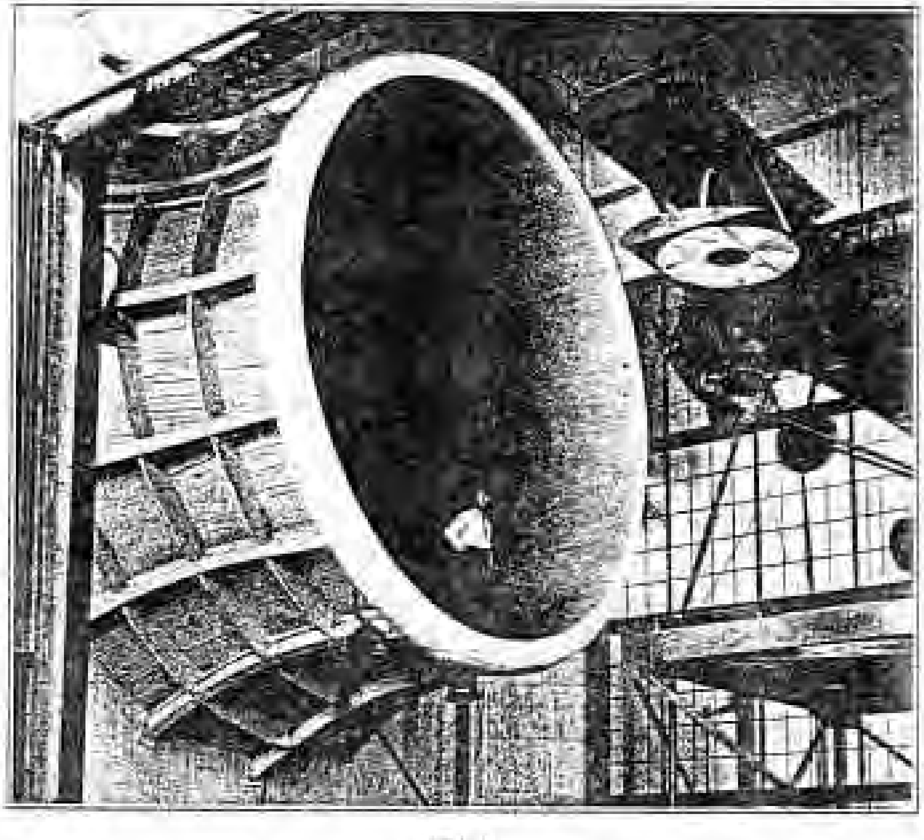


Fig. 3

EXIT CONE

The exit cone (fig. 4) is similar in construction to the entrance cone. It is circular in section from the mouth of the bell in the test chamber to the fan. The cone has a diameter of 33 ft. at the mouth of the bell, reducing to 25 ft. at the test chamber wall and then increasing with a 7∞ included angle to 28 ft. in diameter at the fan. From the fan a gradual change is made to 30 ft. square at the return passage. The total length of the exit cone is 52 ft.

GUIDE VANES

Guide vanes (fig. 5) are located, as shown in Figure 1, at each point of change of direction of the air stream. These consist of metal covered wood framed curved

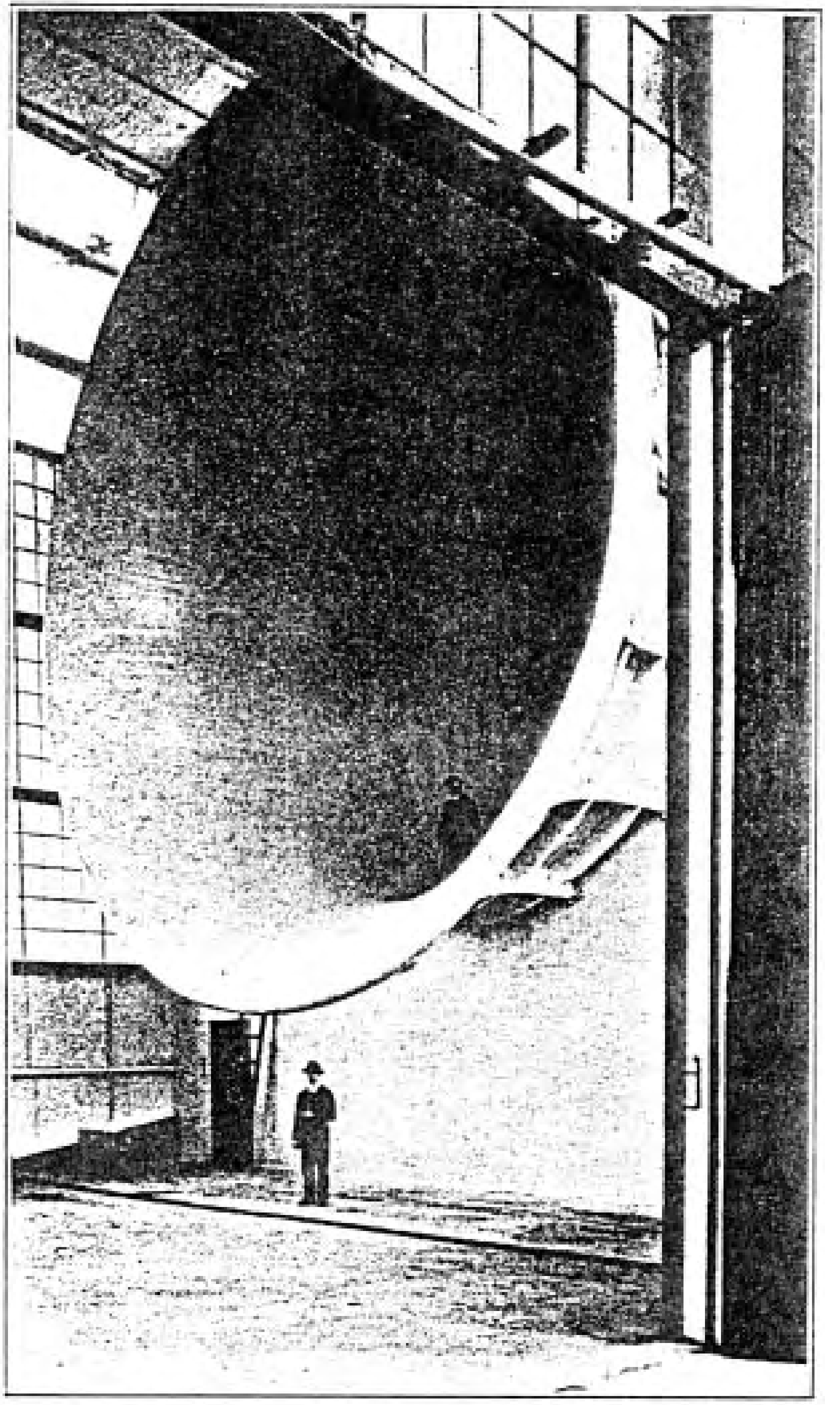


Fig. 4

shapes built up in sections 5 ft. long. Rounded leading edges and pointed trailing edges are of wood. The vanes are so proportioned that the free area between them is about a mean of the passage areas before and behind them. Streamlined wood separators run diagonally across the corners and act as stiffeners and supports for each tier of vane sections. It may also be noted in Figure 5 that cross bracing in the return passages is streamlined in the direction of flow.

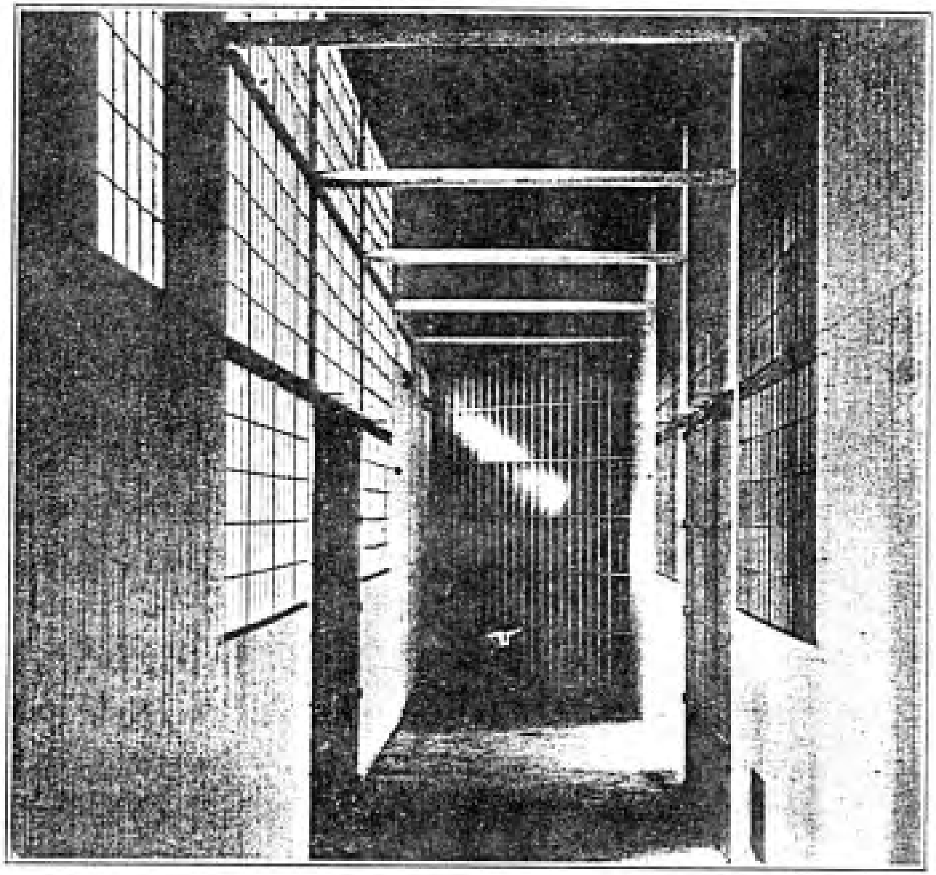


FIG. 5

FAN

Circulation of air is accomplished with a 28 ft. diameter propeller type fan. (Fig. 6 and fig. 1.) It consists of eight cast, heat-treated, aluminum-alloy blades screwed into a cast steel hub and locked in place by means of wedge rings which are forced between the blade shanks and the hub. This makes it possible to change the pitch to adapt the fan to the driving engine characteristics or to secure different air speeds with the same engine speed. At present 100 M. P. H. is obtained with 330 R. P. M. of the engines and fan. The weight of each blade is 600 pounds and the total weight of the fan is about 3 ½ tons.

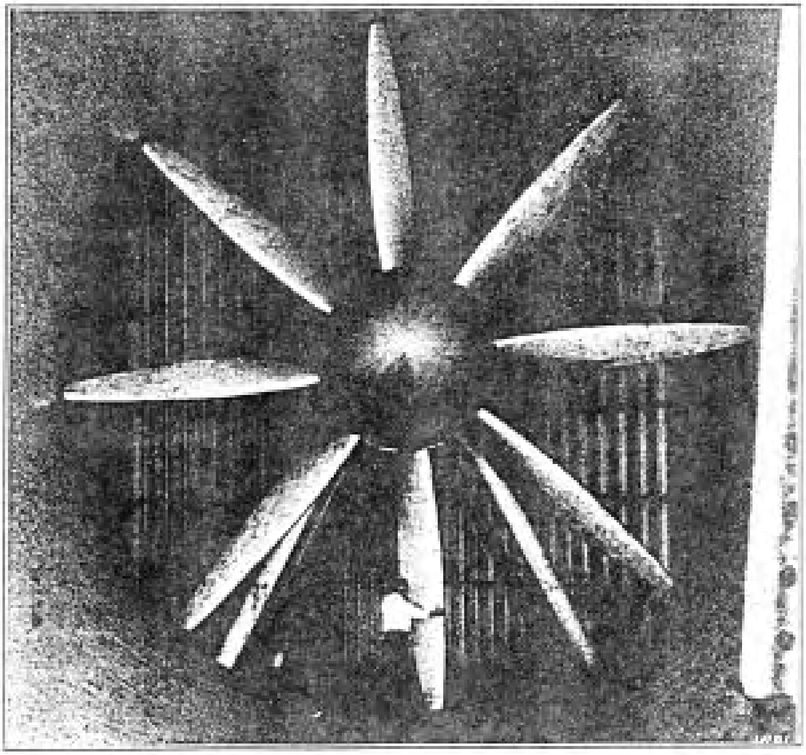


Fig. 6

A steel framed sheet aluminum spinner 7 ft. in diameter is attached to the hub. This fairs into the cylindrical propeller shaft housing.

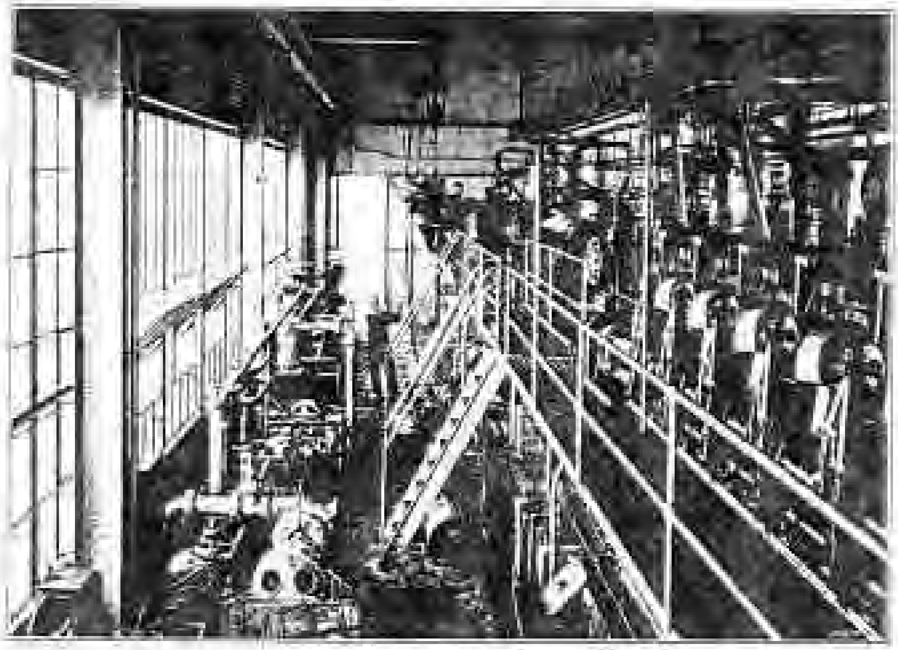
DRIVE SHAFT

The fan hub is keyed on to the tapered end of an 8-in. solid steel shaft running back through the exit cone and return passage. This shaft is supported on four plain, collar oiled, bearings and one combination plain radial bearing and deep groove ball thrust bearing. The latter is located at the end of the shaft opposite the propeller. The bearings are supported, in turn, on steel I-beam A frames resting on spread footings in the ground below the exit cone.

The shaft and bearing bracing are surrounded by a cylindrical sheet steel fairing on wood formers of the same diameter as the fan spinner. The legs of the A frames are also suitably faired.

POWER PLANT AND TRANSMISSION

Because of local conditions it was found advisable to use Diesel engines rather than electric motors to furnish power for circulating the air through the tunnel. Two Diesel engines, which had been removed from a submarine, were furnished by the Navy Department.



These engines are full Diesel M.A.N. type, 6 cylinder, 4 cycle, single acting, rated at 1,000 HP. each at 375 R.P.M. After due consideration, it was decided to install these end to end as they had been in the submarine, using the existing flywheels and clutches, spacing them far enough apart to allow the installation of a driving sheave between. The location of the engine room is shown in Figure 1, with the engine and sheave position indicated. The auxiliary machinery is arranged on the opposite side of the room from the engines. Figure 7 is a general view of the engine room.

Power is transmitted from the driving sheave to a similar sheave located forward of the thrust bearing on a part of the fan shaft extending through the main tunnel wall. Forty-four "Texrope" V-belts are used with two adjustable grooved idler pulleys located as shown in the end view, Figure 1. The transmission ratio is 1 to 1. The belt pull is carried on a suitable steel structure and the whole framing, is roofed over and sided with a protected corrugated metal. This same material is a covering for the engine room proper, rendering this part of the installation practically fireproof.

BALANCE

The testing of full size airplane fuselages necessitated the design of a new type balance. This, as shown in Figures 8 and 9, consists essentially of a triangular frame **A** of steel channels and gussets resting on tubular steel posts **B**, which in turn bear on the platforms of ordinary beam scales **C**. Double knife edges are provided at both ends of these posts. The rear post of the frame is on the longitudinal center line of the balance and the forward posts are at equal distances (5 ft.) on either side. The sum of the net readings on all three balances is the lift. The pitching moment is computed from the sum of the front balance readings and from the rear balance reading. Since the rear balance is on the longitudinal axis, the rolling moment is computed from the net readings on the front balances.

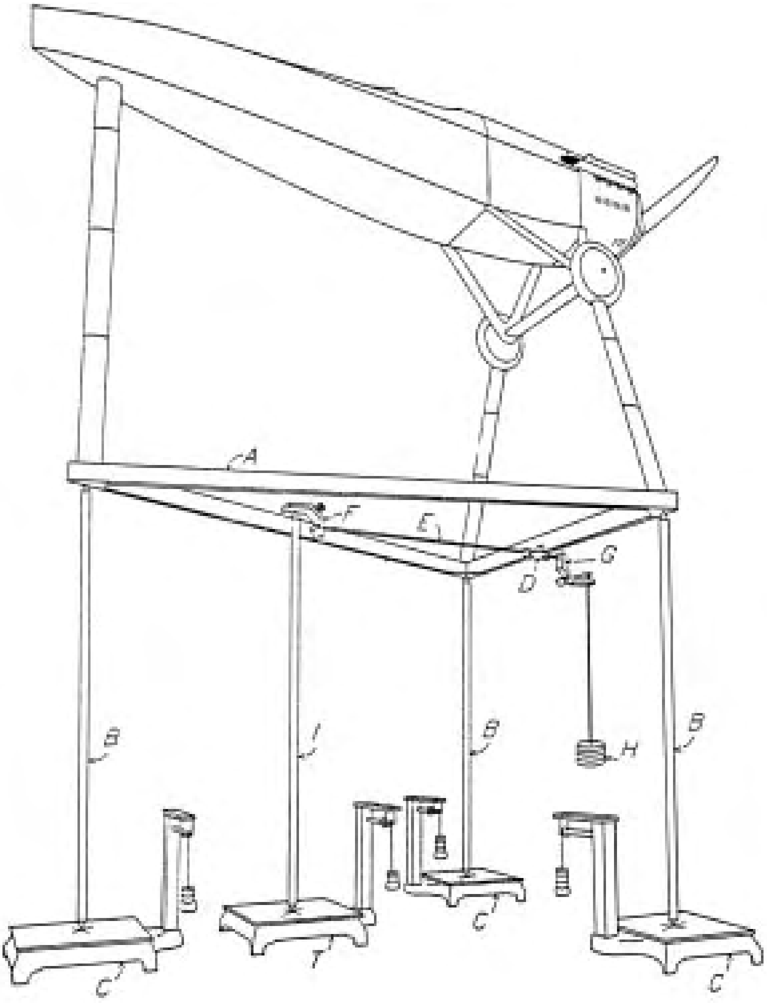


FIG. 8

At **D** are located knife edges connected to tie rods **E** running forward to a bell crank **G** and a counterweight **H**, and aft to a bell crank **F** and a post **I** resting on the scale **T**. A forward pull or thrust on the frame produces a down force on the post or an increase in load on the scale T. The counterweight H produces an initial load on the scale T and consequently a drag or backward force is measured as a diminution of load on the scale. The counterweight consists of several 50-lb. units and can be easily adapted to the range of thrusts and drags expected during any one test.

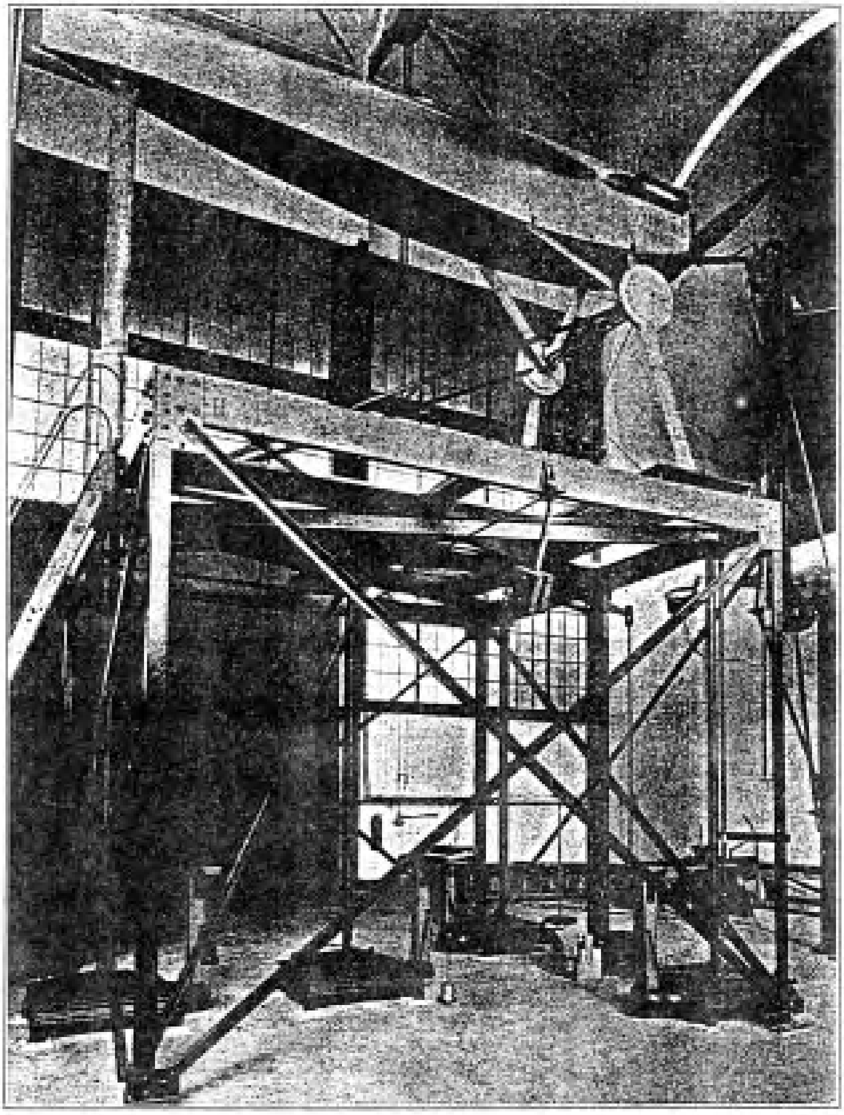


Fig. 9

TORQUE DYNAMOMETER

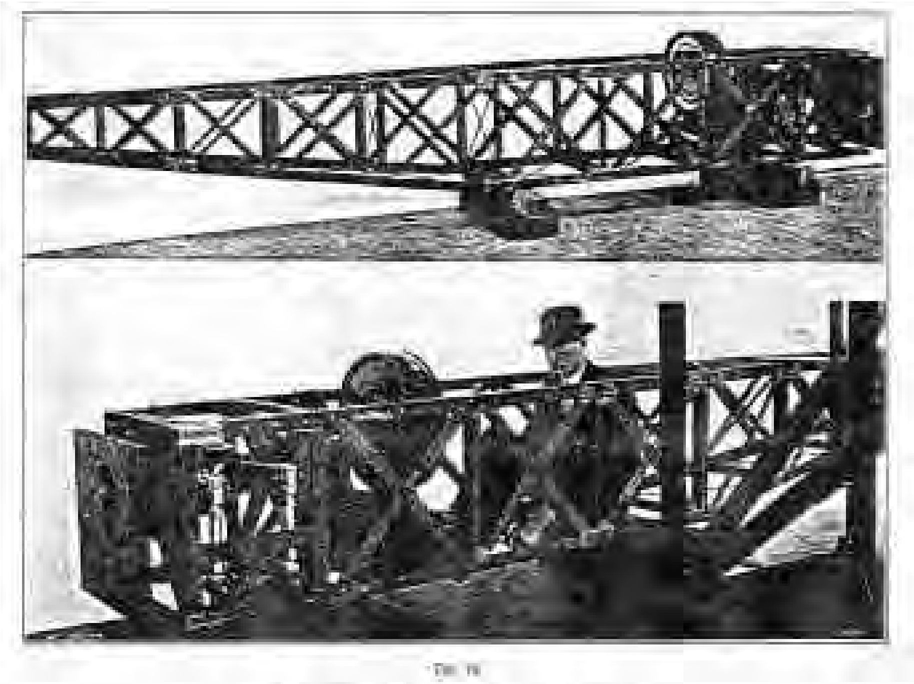
The fixed knife edges on the bell cranks are seated on blocks bolted to a rectangular steel frame rigidly fastened to the floor, as shown in Figure 9. In addition, this frame is provided with knife edges, links, and counterweights which hold the triangular frame in a fixed lateral position. Screws are also provided for raising the triangular frame from the knife edges while working on the attached apparatus. A stairway at the rear and a grating floor facilitate work on the supports and apparatus mounted on the balance.

At each corner of the triangular frame are ball ended steel tubes, adjustable in length and angle, which support the body under test. The forward tubes, in the case of a fuselage with landing gear, have a fitting at the upper end which clamps the axle of the landing gear. The rear post has a ball-and-socket attachment to the fuselage. The drag of these supports is reduced by streamline fairings which also serve to cover wires and fuel and water lines running to the fuselage.

TORQUE DYNAMOMETER

As the engine power is one of the major variables determining the propeller characteristics, a test fuselage has been developed which allows the engine driving the propeller to be mounted on a dynamometer and the torque to be measured directly.

As shown in Figure 10, this is a heavy angle and strap steel frame so shaped that it can be slipped inside a standard airplane fuselage and supported by suitable



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PROPELLER BLADE DEFLECTION

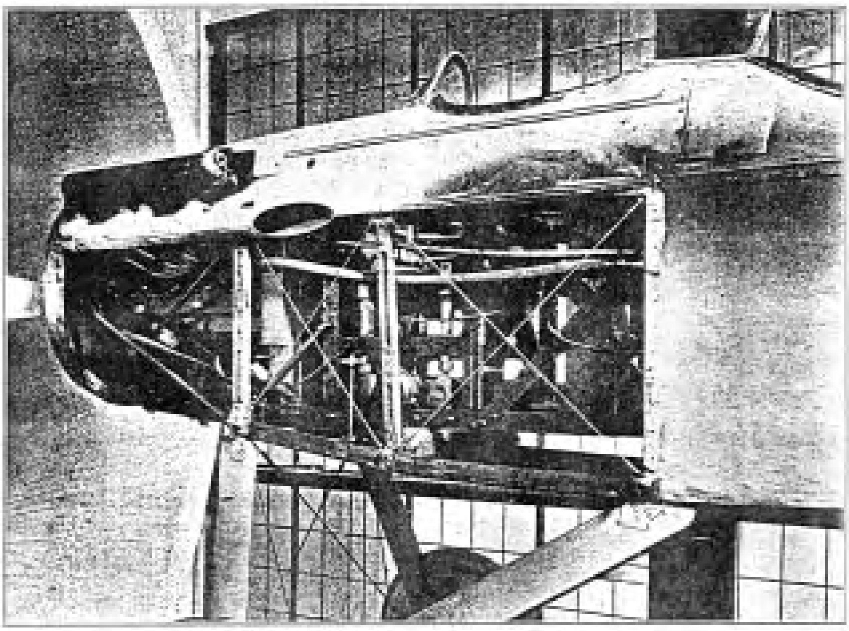


FIG. 11

ENGINE STARTER

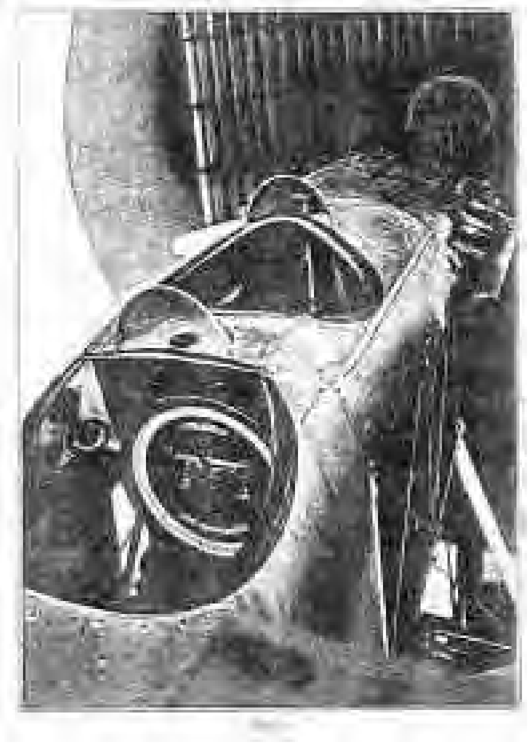
blocking. At its forward end a steel casting is fixed carrying two large ball bearings and an extension shaft and plate. An airplane engine can be mounted on this plate. Its torque, which is carried through the plate and shaft and a special linkage, is read on a dial scale mounted farther back in the fuselage. This dial reads directly in lb. ft. up to 2,000 lb. ft. and a total of 4,000 lb. ft. may be obtained with a counterweight. A double link system renders the operation independent of the direction of engine rotation.

Figure 11 shows a VE-7 airplane mounted on the test fuselage with an E-2 engine on the plate. The radiator is mounted independently of the engine and is not used for cooling. Cooling water is supplied and returned through rubber hose running back through the fuselage and down the rear post to the floor. Figure 12 shows the dial in the rear cockpit.

To reduce the fire hazard and to simplify installation, fuel is supplied from a small tank located on the outer wall of the tunnel, feeding by gravity to the engine carburetor. The gravity tank is filled from a large storage tank by an electric gear pump which is started and stopped by an automatic float switch in the gravity tank.

PROPELLER BLADE DEFLECTION

Propeller blade deflections are measured as follows. A telescope with cross hairs, in conjunction with a prism, is mounted on a lathe bed beneath the propeller



being tested. One blade of the propeller at a time is painted black and a black background is painted on the ceiling. Two lights are arranged so that their beams strike the propeller blade. On sighting through the telescope no image will be seen when the black blade passes the black background; but when the white or bright metal blade passes, a line of the leading or trailing edge will appear. By locating, the cross hairs successively on these lines and reading, the distance moved it is possible to compute the angular deflection of the propeller blade at any given radius. Further development of this apparatus is in process.

MANOMETER

For routine testing, velocities are calculated from the readings of an N.A.C.A. micro-manometer, one side of which is connected to plates set in the walls of the return passage and entrance cone, and the other side open to the air in the test chamber.

SPEED REGULATOR

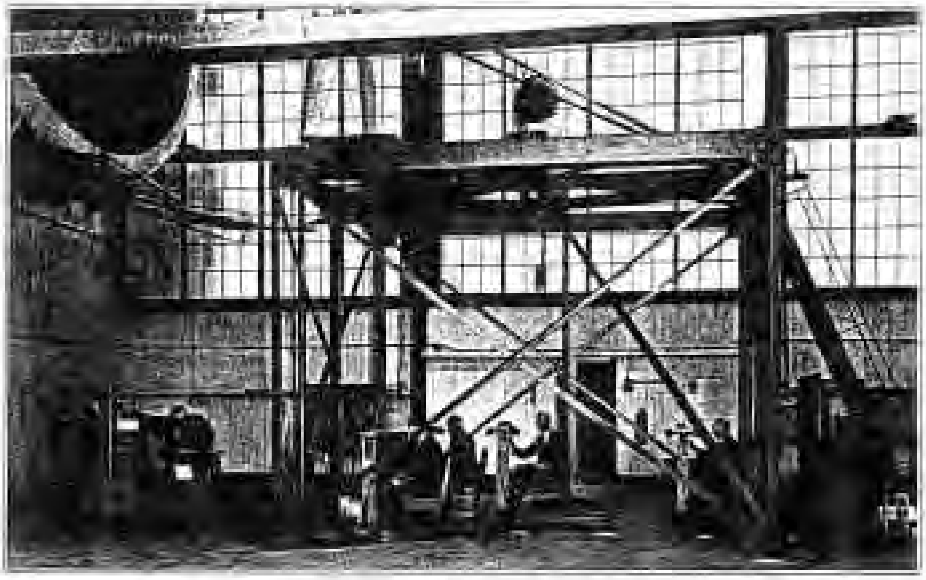
An air-speed regulator has been developed to insure a uniform dynamic pressure, but to date its use has not been found necessary.

ENGINE STARTER

For starting an airplane engine mounted on the balance, an electric starter is secured to the entrance cone shown in Figures 12 and 13. A hollow shaft with a pin meshing with a dog on the propeller shaft is driven by means of a chain from an electric motor. The whole unit is arranged to swing down clear of the air stream during a test.

CALIBRATIONS

A velocity survey has been made over the entire cross section of the air stream at a point about 6 ft. back of the entrance cone edge. Seventy-nine points were taken at 2 ft. intervals. The velocity without a honeycomb or air straightener was found to be constant within 1 per cent over the test area. This is attributed to the large reduction of area in the entrance cone. Large variations of velocity at the



entrance to the cone are greatly reduced by the rapid acceleration through it. In consequence, while provision was made in the structure for the installation of a honeycomb, none has been deemed necessary.

The wall plates and manometer are calibrated from time to time against a group of Pitot tubes set in the air stream. These are attached to a movable frame to which one or more Pitot tubes may be attached and the velocity at any point in the air stream determined without a special installation. In particular, this apparatus is used to measure the velocities in the plane of the airplane propeller.

The tunnel was designed to give a velocity of 100 M.P.H. with an energy ratio of 1.2 based on the power input to the fan. A velocity of 110 M.P.H. has been obtained indicating an energy ratio higher than that assumed.

Figure 13 is a view in the test chamber during a standard propeller test. Balances, manometer and deflection apparatus are shown in operation. An observer stationed in the fuselage to control the engine and read the torque scale does not appear in this view.

SOME RESULTS

A considerable amount of testing has already been accomplished since operation began in July, 1927. Figure 14, taken from Technical Note No. 271 (Reference 1), indicates the proportional drag of various parts of the Sperry Messenger airplane fuselage. The propeller research tunnel is particularly adapted to full-scale tests of this nature. Figure 15 shows the characteristics of Propeller I, previously tested in model form at Stanford University, and in two separate flight tests. (References

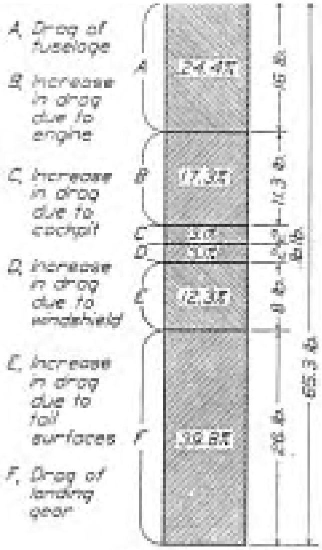


Fig. 14

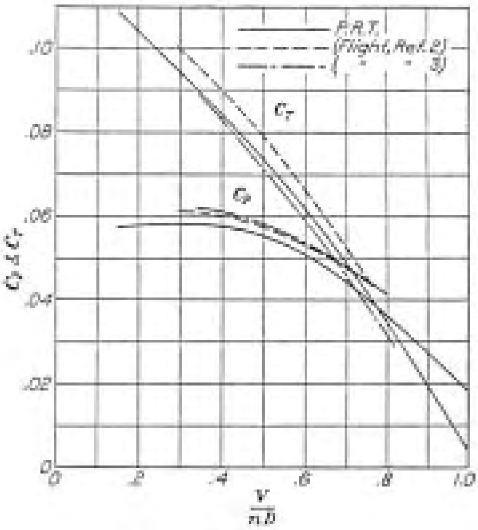


Fig. 15

2 and 3.) Curves from these tests are given for comparison. Attention is called to the inaccuracy of flight data mentioned in the introduction to this paper. Tests of wings of 12 ft. span have also been made at speeds up to 100 M. P. H. A comparatively high Reynolds number is thus attained. Figure 16 is a view of a wing set up for test. A comprehensive program of tests to determine the effect of propellers on air-cooled engines operating in front of various types of fuselages with several shapes of cowling is now in progress. The effect of these bodies on the propeller is also being determined.

ACCURACY

Dynamic pressure, thrust, torque, and R.P.M. are measured with an accuracy of from 1 to 2 per cent. Computed data are, therefore, correct to approximately plus or minus 2 per cent and final faired curves through computed points to about plus or minus 1 per cent. This compares favorably with other engineering measurements. The beam thrust balance is to be replaced with a dial scale which will increase the accuracy and will enable the observers to read more quickly and more nearly simultaneously. A change in the linkage of the torque scale is contemplated which will increase the accuracy of that reading. When these changes are in effect it is hoped that computed points will be correct to plus or minus 1 per cent.

CONCLUSION

The propeller research tunnel fulfills a long-felt want in aerodynamic research. Propellers can be tested full scale, and with actual engines and bodies in place, with an accuracy not attained in flight tests. The components of the airplane, fuselage, landing gear, and tail surfaces can be tested full scale. While full size

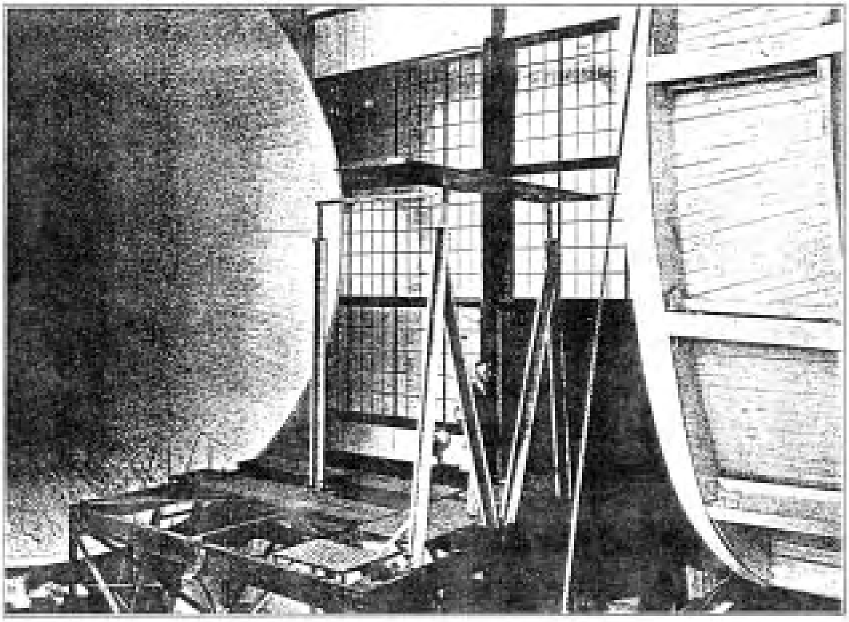


Fig. 16

ACCURACY

wings can not be accommodated, a stub wing can be installed which is sufficient to study the effects of all parts of the airplane on the propulsive system, and vice versa. Tests thus far made are consistent and reliable and it is increasingly evident that the propeller research tunnel is a useful addition to the extensive research facilities of the National Advisory Committee for Aeronautics.

LANGLEY MEMORIAL AERONAUTICAL LABORATORY,
NATIONAL ADVISORY COMMITTEE FOR AERONAUTICS,
LANGLEY FIELD, VA., June 2, 1928.

REFERENCES

Reference 1. Weick, Fred E.: Full Scale Drag on Various Parts of Sperry Messenger Airplane. N.A.C.A. Technical Note No. 271, 1928.

Reference 2. Durand, W. F., and Lesley, E. P.: Comparison of Tests on Air Propellers in Flight with Wind Tunnel Model Tests on Similar Forms. N.A.C.A. Technical Report No. 220, 1926.

Reference 3. Crowley, J. W., Jr., and Mixson, R. E.: Characteristics of Five Propellers in Flight. N.A.C.A. Technical Report No. 292, 1928.

Reference 4. Glauert, H., and Locke, C. N. H.: On the Advantages of an Open Jet Wind Tunnel for Airscrew Tests. B.A.C.A. R.& M. 1033, 1926.

Reference 5. Durand, W. F.: Experimental Research on Air Propellers. N.A.C.A. Technical Report No. 14, 1917.

Document 2-20(c), Fred E. Weick, excerpts from **From the Ground Up**, *1988.*

In response to the requests of Dr. Lewis about how NACA might help the Bureau of Aeronautics, I kept mentioning the need for fullscale propeller tests at high tip speed, where compressibility losses became evident. Compressibility is the physical property by which the volume of matter decreases to some extent as pressure is brought to bear on it. The British had made high-tip-speed tests on two-foot models in a wind tunnel, but the Reynolds number was so low that the results were questionable when applied to full scale. Their compressibility losses were much greater than those indicated by our meager flight tests in this country. At that time, however, I could not see any practical way of making full-scale tests other than in flight.

One day in early 1925 Dr. Lewis called me into his office and asked me how I would like to see a wind tunnel capable of making the full-scale propeller tests. But, I said, in order to make full-scale tests on a 10-foot propeller, the diameter of the tunnel's throat would have to be at least 20 feet, or four times the size of NACA's largest wind tunnel at Langley up to that time. "Yes, you're right," Dr. Lewis said. "But," I said, "in order for it to be practical, the tunnel's airflow would have to reach at least 100 miles an hour, and to achieve that, you'd have to have an immense amount of power—probably a couple of thousand horsepower." "Yes," Dr. Lewis said again. "I've been talking it over with Dr. Munk and we think that such an arrangement might be practical." I was astonished: a 20-foot tunnel would require a structure of 43 or sixty-four times the volume! Neither the Hampton nor Newport News power plant was large enough to supply electric power, but NACA, Dr. Lewis informed me, had arranged to get two navy surplus diesel engines of 1,000 horsepower each, taken from a T-2 submarine. "If we can get this tunnel built," Dr. Lewis asked me, "would you like to come down to Langley Field and run it for us?" Without hesitation, I said that I would indeed.

When Dr. Lewis made the suggestion that I be transferred from the navy to NACA, the assistant chief of the Bureau of Aeronautics, Capt. Emory S. Land, absolutely refused. The captain could not see losing me to another government organization when I had just become useful in his propeller department. Some time passed without approval for my transfer, until the line officer in charge of the propeller department, Lt. Stanton Wooster, who had replaced my old boss Lieutenant Shoemaker, advised Captain Land that I would probably do the Navy Department at least as much good doing research in NACA's new propeller tunnel at Langley. Moreover, Wooster told him he didn't exactly like standing in the way of a possible improvement for me. So after this polite badgering, Captain Land finally said, "All right, all right," and the transfer was made.

NACA at that time had an annual appropriation of roughly $250,000 with which to pay a staff of one hundred twenty-five, buy buildings and equipment, and take care of operating expenses. As I remember it, the total cost of the bare structure for the propeller research tunnel was about $70,000. In order to get it started as soon as possible, NACA had skimped and saved $35,000 out of its FY appropriation, which ended on June 30, until the early 1970s the end of the government's fiscal year. On June 30 NACA had entered into a contract with the Austin Company to construct the tunnel's outer shell for $35,000. Then on the next day, July 1, it had entered into another contract to construct the internal structure, entrance and exit cones, and return passages. Construction started at once. This all happened before my transfer and was the main reason I was itching to move from Washington down to Langley.

[. . .]

The distance by air from Washington, D.C., to Hampton, Virginia, the town nearest Langley Field, is only about 120 miles, but by road through Richmond it is about 175 miles. In the 1920s these roads were surfaced with gravel and often badly rutted; smooth ribbons of concrete were not to be found in rural Virginia. In our Model T roadster, packed to overflowing, it took us all day to make the trip.

The engineer in charge of the Langley Memorial Aeronautical Laboratory at that time, a Californian by the name of Leigh M. Griffith, appeared unhappy with the idea that I had been placed under him from above; in fact, Griffith must have been generally unhappy with his situation at Langley, for he left within the month. He was replaced by Henry J.E. Reid, an electrical engineer who had been in charge of the laboratory's instrumentation. Reid remained the engineer-in-charge until he retired from the National Aeronautics and Space Administration (NASA) in 1960. The lab had a flight research division headed by test pilot Thomas Carroll, a power plant division headed by Carlton Kemper, and two wind tunnel sections, one, the 5-foot or atmospheric wind tunnel (AWT) section headed by Elliott G. Reid, and, the other, the variable-density tunnel (VDT) section headed by George J. Higgins. There was also an instrument shop, model shop, technical service department, and a clerical and property office headed by Edward R. ("Ray") Sharp. The new 20-foot propeller research tunnel (PRT) was being constructed under the supervision of Elton W. Miller, a mechanical engineer who had previously been in charge of the construction of the variable-density tunnel. I was placed under Miller until the tunnel was ready for operation.

By the time I started at Langley, the outer shell of the new tunnel had been completed but work on the entrance and exit cones and guide vanes was still going on. The tunnel, which had been laid out by Dr. Max Munk in Washington, was of the open-throat type then most suitable for testing propellers. My first job was to design and get constructed a balance arrangement that measured the aerodynamic forces on the model and the model's reaction to them. This balance had to support an airplane fuselage, complete with engine and propeller, 25 feet above the floor in the center of the tunnel's 20-foot-in-diameter airstream. All of the pertinent forces, such as drag, thrust, and moments, were to be measured down below by four small and simple beam scales.

Since 1921 Dr. Munk had been holed up in a little office at NACA headquarters in Washington, where he had been turning out excellent theoretical work. Munk had studied under Ludwig Prandtl at the University of Göttingen in Germany and had been brought to this country by the NACA in 1921. His entry into this country required two presidential orders: one to get a former enemy into the country, and another to get him a job in the government. And I guessed this helped him to appreciate his importance.

Without question, Munk was a genius, and, without question, he was a difficult person to work with. In early 1926 he decided on his own that, since Langley laboratory was where all the real action was taking place, that was where he should be. NACA headquarters must have agreed, because it made him the lab's chief of aerodynamics; this put him in charge of the flight research division and the two wind tunnel sections. My boss, Elton Miller, now reported to Munk, and all of my work ultimately had to be approved by him. I had known Munk in Washington and had great respect for his abilities. On the other hand, I did not want my balance design turned down at the last minute; so I had taken the pain to take each detail of design, mostly on cross-section paper, up to Munk to get his approval, and I got his initials on every single one of them. This, I thought, would certainly assure his final approval.

The movable parts of the balance supporting the airplane were supported by a structural steel framework about 12-feet high, 12-feet wide, and 16-feet long. In place of adjustable cables, steel angles ¼ by 2 ½ by 2 ½ inches provided the diagonal bracing.

A couple of days before we expected to try out the balance using a little Sperry Messenger airplane with its 60-horsepower engine running, Munk made an unannounced visit to the PRT building. Just as he walked into the bare-walled 50-foot cubicle that housed the test section, a loud horn squawked, calling someone to the telephone. This sent Dr. Munk into a tantrum, and I immediately had one of my mechanics disconnect the horn. Before he had entirely calmed down, he walked over toward the balance structure and put his hands on the long diagonal braces. These were fairly flexible, and he found he could move them back and forth a bit. Visualizing the entire structure vibrating to the point of failure and the whole airplane and balance crashing to the ground, the perturbed Munk ordered me to tear down the balance entirely and to design a new foundation and framework for it. He then turned and went back to his office a couple of blocks away.

Naturally I, too, was perturbed. Munk, after all, had approved every detail of my balance design. Not knowing what to do, I waited for some time to give him an opportunity to cool down. Then I went to his office and, as calmly as I could manage, mentioned that I thought the natural frequencies of the long diagonal members would be so low that vibrations would not be incited by the more rapid impulses from the engine and propeller. But mainly I suggested that, inasmuch as all the parts were made and ready to be put up, why not wait a couple of days before tearing it down and make a careful trial using the Sperry Messenger, starting at low speed, gradually increasing it, before dismantling the apparatus. Munk finally agreed, but demanded to be present when the test was made.

I did not like the idea of his presence one iota. To start the engine, the Messenger's propeller had to be cranked by hand from a balloon ladder that was put up in front of the propeller 25-feet above the floor. (A balloon ladder was like a fireman's ladder but its base was attached to a pair of weighted wheels, which permitted it to be "leaned" out into space. At its base there was a "protractor" that told you how far it could be angled without tipping over.) This sweaty business often took some time. It was not the kind of operation I wanted the excitable Munk to watch. Moreover, since no one else in the PRT section had ever started an airplane engine by turning the propeller, I was the one who was going to have to do it.

I brought my problem to Elton Miller, my boss, and to Henry Reid, the engineer-in-charge. Together, we decided that the only thing to do was to make an end-run around Munk and check out the tunnel balance system in his absence. This was easily done, as Munk worked on theoretical problems in his room at a Hampton boarding house every afternoon. We set up the test run and after a bit got the engine started without any difficulty. We then experimented with it until we could start it easily and felt ready for the final trial.

The problem of convincing Munk remained. We could not simply tell him about the successful test, so we agreed to arrange another "first test" for Munk to witness. Engineer-in-charge Reid escorted Munk to the tunnel the next morning. I casually said, "Good morning," clambered up the ladder, and pulled through the Messenger's prop. Luckily, the engine started on the first try. We then moved the ladder away, ran the engine through its entire range with no vibration difficulty, and then shut it down. Now, I wondered, what sort of explosion will we have? I needn't have worried. Munk walked toward me with his hand outstretched and congratulated me on the success of the operation. Everything had turned out all right. The balance system of the PRT operated satisfactorily with engines of up to 400 horsepower into the late 1930s, when it was replaced by a new and better one.

In 1926 Dr. Munk gave a number of lectures on theoretical aerodynamics to a select group of young Langley engineers. I was very happy to learn these things from him. Ever since graduation from the University of Illinois, I had thought about taking some graduate courses in aeronautical engineering. While working for Tony Yackey, I had read in a magazine article about the graduate courses in aeronautics offered at Massachusetts Institute of Technology. I had written MIT for information and had received a letter back from Professor Edward P. Warner, who had been Langley's first chief physicist in 1919 and would later become assistant secretary of the navy for aeronautics, editor of the magazine Aviation, and finally president of the International Civil Aviation Organization (ICAO), which continues to coordinate the rules and regulations for aeronautical activities throughout the nations of the world. I had hoped still to find a way to work in some graduate courses even after reporting to work at the Bureau of Aeronautics. But Dr. Lewis had talked me out of the idea on the basis that formal aeronautical engineering education was inferior to what I could learn if I went to work for the NACA at Langley. I guess he was probably right in regard to the aeronautical courses per se, but on occasion, in later years, I sorely missed the extra mathematics and physics that would have been obtained in school.

As mentioned earlier, the power plant for the new PRT consisted of two 1,000-horsepower, six-cylinder in-line diesel engines taken from a T-2 submarine. These engines were located end-to-end with crankshafts connected to a large sheave or pulley between them. This sheave carried forty-four Tex-rope V-belts to a similar sheave on the shaft of the propeller fan that drove the air through the tunnel. The shaft of the propeller fan was 25 feet above the ground, and the two sheaves were 55 feet apart center to center. Because we were concerned that some destructive vibrations might occur in the crankshaft-sheave assembly, we decided that a theoretical analysis of the torsional oscillations should be made, with Dr. Munk outlining the problem and a new man, Dr. Paul Hemke, to work out the solution. As a junior engineer, my assignment was to give the measurements and sizes that I would get from the drawings of the engines and sheaves.

I had no difficulty giving them the measurements, but Dr. Hemke was never able to get the gist of the torsional pendulum problem as described by Munk. This went on for some time with no results being obtained. Finally, I looked into my mechanical engineers' handbook and into a couple of textbooks and found that considerable work had been done on the problem and that the solution was not too difficult. I made the computation myself, coming out with a natural frequency of 312 RPMs. Later on, after the tunnel was in operation, some men came down from the navy shipyard in Brooklyn with equipment to measure the torsional oscillations; they found exactly the same natural frequency as I had computed. Hitting it exactly, of course, was a matter of luck, but it helped give me a good reputation, whether I deserved it or not. The success put me in good with Munk, but unfortunately Dr. Hemke was never able to work satisfactorily with him. A short time later he left the NACA. Hemke later joined the faculty of the U.S. Naval Academy, after holding a prestigious Guggenheim Fellowship for research under B. Melville Jones at Cambridge.

Another problem I helped to solve was the design of the 28-foot propeller fan that was to circulate the air in the propeller research tunnel. This fan needed to have eight blades of normal width. The exact energy ratio of the tunnel was not known in advance, so I desired to have blades that could be adjusted so that the pitch could be set exactly right after trial runs. Aluminum-alloy blades therefore seemed the best choice, but the blades we wanted were too large to be forged in the manner of the aluminum-alloy propeller blades then being manufactured. Fortunately, the propeller was to turn at only 375 revolutions per minute, which meant that the stresses would be very low in comparison even with airplane propellers having large diameters. This gave me the idea that a cast aluminum alloy might be used successfully, which it was.

I arranged with the Aluminum Company of America to cast the blades in their plant at Cleveland, Ohio. Before the large blades were cast, however, the company made two blades for a small ten-foot model that I then took to McCook Field in Dayton, where they were tested by Army Air Service engineers on their propeller whirl rig. This test showed the blades to be sufficiently strong.

[. . .]

We had gotten the diesel engines to run satisfactorily in a short time, but the long Tex-rope drive, with its forty-four different V-belts, were always getting so tangled up that they could not stay on the sheaves satisfactorily. It took several months of experimenting with idling sheaves in various locations before the operation became satisfactory. The entire drive, with Tex-ropes and sheaves, had been purchased from the Allis Chalmers Company and had been guaranteed to operate satisfactorily. After spending much money and time on this problem, the NACA sued Allis Chalmers for a sizable rebate. My daily log of operations was used as part of the testimony, and after one futile operation I had put down in disgust, “No soap.” Although this was a generally used term in the Midwest, where I came from, indicating failure, no one in the room, all of whom were from the East, knew what I had meant. So I had to explain it in detail, an amusing interlude in an otherwise dull trial.

[. . .]

Finally, in 1927, the Tex-rope problem was solved and the PRT was ready for actual testing. The tunnel personnel included Donald H. Wood, a mechanical engineer from Renssalaer Polytechnic Institute who was about my age and who had been with the PRT section from the start; Melvin N. Gough, a young engineer who had come to Langley directly from Johns Hopkins; William H. Herrnstein, Jr., an engineer who had come directly from the University of Michigan; and John L. Crigler and Ray Windier, also engineers. The power plant and shop work under Ted Myers, who was a little older than the rest of us, included George Poe and Marvin Forrest. There were two or three others whose names I have forgotten. At any rate, we had a good team and we all worked together very well.

Just before testing actually started in the PRT, Langley experienced a rather sad affair: a revolt against Dr. Munk, the head of our aerodynamics division. Munk was a wonderful theoretical aerodynamicist, but, as the story of my design of the PRT's balance structure illustrates, he was also an extremely difficult supervisor, not just for me but for all the section heads working directly under him. Eventually all the section heads, including Elton Miller, decided they couldn't work with Munk any longer and handed in their resignations. Munk was then relieved of his job, which I feel was a great loss. If Dr. Lewis could only have kept him holed up in his little office in Washington, Munk could have produced a great deal more of his useful theoretical work.

With Munk's departure, Elton Miller became the chief of the aerodynamics division, and I became head of the PRT section.

In the first months of PRT operation, just to get experience, we merely tested the Sperry Messenger and obtained drag data with various parts of the airplane removed. This was then written up and published as an NACA technical note. Before we could actually make propeller tests, though, we had to design and build a dynamometer that would support the engine and propeller up in the airstream and measure the torque of the engine while it was operating. We mounted the dynamometer in a long structural steel frame of small cross-section. Any engine up to about 500 horsepower could be mounted ahead of it, and any fuselage form could be put on over it. The engine torque we measured directly in foot pounds on a dial-type Toledo scale.

For the first propeller tests, we slipped this dynamometer inside the fuselage of a Vought VE-7 airplane. The engine was a 180-horsepower Wright E-2 liquid-cooled unit, similar to the old Hissos. These tests were made primarily to compare with the propeller data we had gotten from our flight testing and with the small-model data acquired previously from wind-tunnel testing at Stanford University. Three full-size wooden propellers of the same type previously used on the same VE-7 airplane, and models of which had also been investigated in the Stanford tunnel, were tested. The results agreed as well as could have been expected, considering the difference in Reynolds number (the nondimensional coefficient used since Osborne Reynolds's pioneering experiments at the University of Manchester in the 1880s as a measure of the dynamic scale of a flow) between flight and tunnel testing.

An interesting sidelight about the accuracy of aerodynamic testing appeared soon after we began PRT testing. In our final plots, the points of our wind-tunnel data for the full-scale tests had substantial scatter about the curves, whereas the small-model tests had the points right on the curves. I worried about this for a while, until I found the answer. Our results at NACA were plotted with fine points and fine lines at a large scale, and the model tests at Stanford were in printed form with heavy lines and large points. When we plotted our results in the same manner as the model test results, our accuracy was at least as good as Stanford's. I reported these results in NACA Technical Report (TR) 301, "Full-Scale Tests of Wood Propellers on a VE-7 Airplane in the Propeller Research Tunnel," in 1928. TR 300, describing the tunnel and testing equipment, had been prepared just previously by Donald H. Wood and me.

After the technique of making satisfactory propeller tests in the propeller research tunnel had been worked out satisfactorily, one of the first things that I wanted to investigate was the effect of high propeller tip speeds; after all, this had been one of the main reasons for having the tunnel built in the first place. The original propeller-testing setup with a 180-horsepower engine was not powerful enough to cover the range desired in high tip speeds, but we did what we could with it. The tests had to be made with a very low pitch setting that corresponded to angles of attack of the cruising or level range of flight but not of climb or take-off conditions. The range of the tests was from 600 to 1,000 feet per second, about 0.5 to 0.9 times the velocity of sound in air, or in modern terminology, from Mach 0.5 to Mach 0.9. Within the range of these tests, the effect of tip speed on the propulsive efficiency was negligible, and there was no loss due to the higher tip speeds. The results are given in my NACA Technical Report 302 (June 20,1928). Later on, with a more powerful engine, we were able to test a whole series of propellers at tip speeds of up to 1,300 feet per second, which is well above the speed of sound. The results of these tests are recorded in NACA TR 375 (November 1930) by Donald H. Wood.

Document 2-21(a–c)

(a) Elliot G. Reid, "Memorandum on Proposed Giant Wind Tunnel," 3 April 1925, National Archives, 171.1, Washington, D.C.

(b) Arthur W. Gardiner, "Memorandum on Proposed Giant Wind Tunnel," 22 April 1925, National Archives, 171.1, Washington, D.C.

(c) Smith J. DeFrance, *The NACA Full-Scale Wind Tunnel*, NACA Technical Report 459, Washington, D.C., 1933.

By 1930, the VDT had proven that wind tunnel testing of models at high Reynolds numbers produced data that accurately predicted the performance of airfoils, and the PRT had shown the value of full-scale testing in determining the drag characteristics and synergistic effects of integrating the various components of an airplane. Yet, neither tunnel totally reproduced the conditions experienced by a complete airplane in flight, so the NACA decided to build a still larger wind tunnel, one capable of testing full-size aircraft. This was not a brand new idea. Shortly after Max Munk proposed the twenty-foot-diameter PRT in March 1925, others at Langley began to call for a ten-foot increase in its diameter to enable the tunnel to test large- and full-scale aircraft components. Arthur W. Gardiner and Elliot G. Reid, assistant aeronautical engineers at Langley, each prepared a "Memorandum on Proposed Giant Wind tunnel" that applauded the idea of a facility suitable for propeller research and suggested how such a "giant wind tunnel" could do much more than test propellers. Their ideas had little effect on the design of the PRT, which was built with a twenty-foot-diameter nozzle, but good ideas rarely die.

The Full-Scale Tunnel (FST), completed in 1931, was gigantic in all respects, and it loomed over every other structure at the LMAL. Like the PRT, the FST was a closed-circuit tunnel with an open test section and return ducts whose outer walls formed the building walls. With its larger size, the FST design included dual return ducts. The elliptical nozzle—a brilliant solution to the problems associated with very large circular or rectangular designs—delivered an air stream that measured thirty by sixty feet and attained velocities of up to 118 miles per hour. Two thirty-five-foot-diameter propellers, each driven by a 4,000 horsepower electric motor, circulated almost 160 tons of air through the 838-foot-long circuits. The FST could handle airplanes or large-scale models with wingspans of up to forty-five feet. While the FST's speeds were not extraordinary, they were sufficient to enable measurements that could be confidently extrapolated to cover the aircraft's entire speed range because no scale factor was involved. In addition to lift and drag studies, the FST supported stability analysis and research into the interferences between the various parts of an airplane. With the FST joining the VDT and PRT in service, the NACA's Langley Laboratory possessed the most extensive and versatile research facilities in the world. As with its earlier facilities, the NACA published a technical report discussing the FST and its capabilities.

Document 2-21(a), Elliot G. Reid, "Memorandum on Proposed Giant Wind Tunnel," 1925.

MEMORANDUM ON PROPOSED GIANT WIND TUNNEL.

I have been thinking over the possibilities of a tunnel of the size and type suggested and have become very enthusiastic over the project. As requested, I am outlining my ideas of the value of such an apparatus.

Below are listed a number of problems which I consider particularly suitable for investigation in the large tunnel:

(A) Propeller research:
  (a) Tests of full size propellers, isolated, for determination of characteristics under the conditions of deformation imposed by air loads and centrifugal forces.
  (b) Investigation of the propeller slipstream, including confirmation or rejection of the inflow theory and investigation of the phenomena accompanying tip speeds greater than that of sound.
  (c) Tests of propellers with axes inclined to the wind.
  (d) Tests of propellers on actual airplane fuselages for determination of full scale interference characteristics.
  (e) Pressure distribution tests on propellers.
  (f) Investigation of the magnitude and causes of full size propeller deflections.
  (g) Tests of adjustable and reversible propellers and their operating mechanisms under flight conditions.
(B) Testing of full size airplane parts which cannot be accurately reproduced to the scale of present models.
  (a) Tests of radiators, air-cooled engine installations, etc.
  (b) Tests of fuselages with and without slipstream for development of better forms.
  (c) Pressure distribution over full size wings under conditions either very difficult or dangerous to obtain in flight. (Conventional airplane practice, i.e., spars, ribs, and usual covering to be used in construction).

(C) Strength and stiffness of wing cellules.

  (a) Tests of large models or full size half cellules for the purpose of measuring air load deflections and, if it is found possible to determine aerodynamic characteristics under these conditions, improve the structures in such ways as to eliminate dangerous or inefficient conditions.

(D) Power plant testing.

  (a) Tests of air-cooled engines under flight conditions. To be made by installing engine in fuselage and running in tunnel under propeller load.
  (b) Development of cowling and cooling controls for air-cooled engines.
  (c) Heat dissipation tests on full-scale radiators.

(E) Testing of very large models of wings and complete airplanes.

Probably the one line of research to which the large tunnel would be the greatest boon is propeller testing. It is known, of course, that models could be tested in the Dense Air Tunnel [VDT] at Reynolds numbers equal to those existing in the flight operation of full size propellers. This, however, is not the criterion for complete dynamic similarity. Dr. Durand's excellent analysis of the necessary relations for the establishment of complete dynamic similarity, as given in N.A.C.A. Technical Report No. 14, Part II, shows that dimensional homogeneity can be fulfilled only by making the model of the same size as the original. While he does not take up the possibility of testing in a different medium—as compressed air—the fulfillment is still incomplete because the factors involving medium density are different.

It has been the feeling for some time that the deflections of full-scale propellers were considerably different from those of their smaller prototypes and that numerous failures to predict full-scale propeller performance arose from this fact. Mr. Lesley was convinced that such a condition did exist and supported his belief with the statement that model and full scale results were in better agreement in the case of stiff bladed propellers than for limber ones.

Therefore it would seem not only desirable, but almost imperative to have some means of testing full-size propellers and the large wind tunnel seems the only possible solution.

With regard to tests of propellers with axes inclined to the wind, it would seem that only small angles could be explored if the closed throat type of tunnel be used. This is more particularly the case for propellers mounted on full size fuselages. The wall effect would probably so modify these results as to make them misleading.

Interference and slipstream studies could be carried out with an accuracy never before possible. The possibility of using a full size radiator directly behind the propeller solves one of the most troublesome problems of such tests.

Fuselage forms have received very little study up to the present. The Variable Density Tunnel provides a means of eliminating the scale effect factor but the difficulty of producing accurate replicas of fuselages is great and to study their characteristics under the action of a slipstream is almost out of the question with this tunnel.

Static testing of wing cellules will give very good and reliable information on the ultimate strength but only very approximate data concerning deflections. It is of course known that wing characteristics may undergo large changes if any deflections are introduced and yet very little attention has been paid to this matter. By the use of a very large tunnel, it would be possible to test complete semi-span cellules of the smaller machines and very large scale models of large ones. By the use of proper strength relations, the deflections in full size cellules could be very accurately predicted from tests of models sufficiently large to permit use of the same type of construction. It would thus be possible to investigate the conditions which have caused inefficient flight in some cases, failure of controls in others and accidents in some few. The failure of monoplane wings in service during a dive is one of the latter.

The testing of aero engines under actual flight conditions is a thing which can not be accurately done at present. It is known that the ideal conditions of the electric dynamometer test do not prevail in actual operation and just how much the performance suffers is a matter of conjecture. As it would be possible to put an entire plane, minus the outer portions of the wings, into the proposed tunnel, the conditions of flight could be exactly simulated and the propeller testing equipment utilized to measure the engine output.

Likewise the cooling of the air-cooled engine could be studied with an accuracy and completeness quite out of the question in flight and it should be possible, as a result of such testing, to devise much more efficient (from the aerodynamic standpoint) cowling for such engines.

The testing of large-scale models of wings and airplanes is particularly desirable. We feel quite sure that the much feared scale effect becomes almost vanishingly small as the VL product rises to the range covered by actual airplanes and that in this range the effects of turbulence are also practically negligible. If the maximum value of this product could be brought to three or four times as large as that obtainable in present tunnels, it should be quite safe to predict full-scale performance directly from such test data. While the Variable Density Tunnel can, theoretically, accomplish this very thing, there always exists the difficulty of reproducing the airplane with sufficient accuracy in the small size necessary. Then, too, any extensive pressure distribution investigation can be carried out only with an enormous expenditure of time, apparatus, and labor.

The large tunnel would eliminate a number of these outstanding difficulties. Models could easily be built to the required accuracy with only a little more pains than is usually used in normal airplane construction. Their cost would be relatively small and thru their use it would be possible to study a large number of problems which are important but can not be undertaken for one reason or another at the present.

The questions of size and general arrangement will naturally involve much thought and planning. A few remarks on these subjects are appended.

The suggested 20 ft. throat diameter seems ample except for one possibility which was not mentioned in the list above. It would be a marvellous asset to be able to test scaled-down models of large airplanes and having the models capable of actual flight. This could be done in many cases if the tunnel were to have a throat of about 30 ft. diameter, but any model capable of carrying a pilot would be too large for a 20 ft. tunnel. The comparison of flight and tunnel tests on the same model would be just about the ne plus ultra of aerodynamic investigation.

The use of return ducts would effect such a great saving of power and make possible a maximum speed so much higher, with any given power, that the open circuit type of tunnel is completely out of the question.

The outstanding advantages of the Eiffel chamber would seem to overbalance any possible aerodynamic advantage inherent to the closed throat tunnel. Installation and operation of measuring apparatus have few terrors in an Eiffel chamber but, as a result of my experience in the atmospheric tunnel, I would expect many troublesome and expensive complications if the throat were to be closed.

With the use of an open balance room, the shape of throat is pretty well limited to a polygon or circle, as a free jet of air having a square or rectangular cross section would be rather difficult to handle, particularly in the matter of collecting after having traversed the balance room. The circular jet would eliminate several difficulties attendant upon the propeller installation.

A single large propeller might be difficult to build but its operation would doubtless be more satisfactory than any other system that could be devised. It is thought that a multiblade fan of the inserted blade type, somewhat similar to the one in use in the 5 ft. tunnel at McCook Field, offers the best possibilities for adaptation to very large installations.

[signed] Elliott G. Reid
Elliott G. Reid,
Assistant Aeronautical Engineer
Langley Field, Va.
April 3, 1925.

Document 2-21(b), Arthur Gardiner, "Memorandum on Proposed Giant Wind Tunnel," 1925.

MEMORANDUM ON PROPOSED GIANT WIND TUNNEL.

As requested, I submit herewith a brief memorandum on the proposed giant wind tunnel indicating certain investigations for which such a wind tunnel would seem to be peculiarly well adapted.

To my mind, a wind tunnel of the proposed proportions would fulfill a very definite need in aeronautic research, and, if built, would have a threefold purpose: (1) to aid in correlating the wind tunnel testing of models with the flight testing of full-size airplanes, i.e., to duplicate, after a fashion, the function of our variable density tunnel, but attacking the problem from the standpoint of increased linear dimension, approaching or equalling full-scale, rather than from the standpoint of increased air density; (2) to take over certain phases of flight research, thereby substituting more accurate methods for the somewhat unreliable flight test methods, and, at the same time, making it possible to exercise control of the operating conditions, and (3) to enable some very pertinent research problems to be investigated which are not being undertaken at present due to the lack of suitable equipment.

To utilize a giant wind tunnel to fulfill the purpose mentioned under (1) above might appear, at first thought, to be an unnecessary duplication of effort. However, full-size, or nearly full-size, model testing would seem to have many advantages over small model testing in a variable density tunnel, as full-size models could be made in our own shops according to standard airplane construction practice, thus effecting a great saving in time and enabling a greater variety of model building. Also, it would seem that considerable time could be saved in the actual testing of the model. The advantages accruing from either the direct application of wind tunnel test data from a giant tunnel or the application of the test data with but a small correction for scale effect would be highly desirable in predicting flight performance.

The purpose mentioned under (2) above might also appear to be a duplication of effort, but here again it would appear that certain phases of flight research could be conducted in a giant wind tunnel much more expeditiously and with greater accuracy than possible with flight testing methods. In addition, there are cases where a greater range of investigation is possible in wind tunnel testing than is permissible in flight testing. The fact that wind tunnel tests can be conducted independent of weather conditions would also make for a saving in time.

The purpose to be fulfilled under (3) above is by far the most important. The fact that a giant wind tunnel would enable certain investigations to be made, which are not being undertaken at present due to the lack of suitable equipment, might very well, in itself, be a sufficient reason for taking the pioneer step in constructing a wind tunnel of the proposed type. Some pertinent investigations that could be conducted might include tests to determine: full-scale propeller characteristics, either with a free mounting or when mounted adjacent to full-size slipstream obstructions, the data obtained including all scale and installation factors; performance of engine-propeller units mounted in full-size fuselages; the cooling capacity of, and the heat distribution throughout the cylinders of air-cooled engines when mounted with cowling in full-size fuselages; full-scale radiator performance under all conditions of mounting in or near a full-size fuselage (nose, wing, and retractable mountings; drag characteristics of full-size landing gears assembled to fuselage, the investigation to include a study of the retractable type looking toward its general adoption; comparative performance of direct-driven and geared propellers, gear efficiency included in results; the efficiency and general operating characteristics of adjustable and variable pitch propellers; drag characteristics of full-size fuselages with a view to increasing the efficiency of design; parasite resistance of full-size airplane parts either independently or in assemblies; effect on airplane characteristics of such equipment as spoiler gears, variable camber wings, etc., and the behavior of empenage assemblies (reference is made to recent tests with the MO-1 tail unit).

One of the greatest benefits to be derived from the construction of a giant wind tunnel would be its utilization in connection with the investigation of certain power plant problems.

In connection with our analysis of airplane performance, the outstanding need at present is a means for determining the power output during flight of normal, over-compressed and throttled, supercharged and geared engines. All of these types of engines either are being investigated at the present time or are included in our immediate future program of tests. As the purpose of the present and proposed tests is to arrive at a definite answer as to the most efficient power plant unit, it is extremely essential that means be provided for determining power output during flight. In lieu of a suitable torque-meter, propellers, calibrated as installed in the airplane, would serve the purpose. A giant wind tunnel would provide a means for supplying the required propeller calibrations.

The determination is a giant wind tunnel of the performance characteristics of engine-propeller units mounted in full-size fuselages, complete with radiator installation, et al, would aid materially in arriving at the efficiency of a given power plant installation. There is some question as to whether or not accurate torque and thrust measurements could be made with the vibrations of the engine imposed on the measuring devices, but if the effect of engine vibrations could be reduced to a negligible amount, the proposed method of engine-propeller calibration would seem to have many possibilities. It is thought that the problem of handling the engine exhaust would not be serious.

A giant wind tunnel would serve directly as a means for studying air-cooled engines. If the required accuracy of measurement could be secured, a great advantage would accrue from the direct testing of air-cooled engine-propeller units mounted with cowling in full-size fuselages. We have felt the need for investigating such problems as heat distribution throughout air-cooled engine cylinders, effect of fuel mixture on cylinder temperature, effect of cylinder temperature on power, etc., in connection with our tests with the supercharged Lawrance J-1 engine, but have been unable to make any such tests due to lack of equipment. A giant wind tunnel might serve to supplant the immediate necessity for securing special equipment for testing air-cooled engines.

The study of adjustable and variable pitch propellers is tied up directly with the power plant problem, especially with the supercharged engine problem. In light of present knowledge, it would seem that variable pitch propellers must be developed into safe and efficient units if the ultimate advantages of the supercharged engine are to be realized. In our tests with the Roots supercharger we have felt a definite need for the variable pitch propeller, especially for high altitude operation. These two types of propellers could be carefully studied in a giant wind tunnel, whereas their characteristics could not be investigated efficiently, if at all, in a small model due to the difficulty of reproducing them to a small scale.

In conclusion, I concur with the general opinion that the tentative size (20 ft.) of the proposed tunnel be increased to such a size as to enable certain of the smaller size airplanes, or specially built small size airplanes, being tested therein in toto, and to permit accurate testing of large propellers (we are at present using a propeller having a diameter of 13 ft.) such as are used with geared engines.

[signed] Arthur W. Gardiner
Arthur W. Gardiner,
Assistant Aeronautical Engineer
Langley Field, Va.
April 23, 1925.

Document 2-21(c), Smith J. DeFrance, The NACA Full-Scale Wind Tunnel, *NACA Technical Report No. 459, 1933.*

SUMMARY

This report gives a complete description of the full-scale wind tunnel of the National Advisory Committee for Aeronautics. The tunnel is of the double-return flow type with a 30 by 60 foot open jet at the test section. The air is circulated by two propellers 35 feet 5 inches in diameter, located side by side, and each directly connected to a 4,000-horsepower slip-ring induction motor. The motor control equipment permits varying the speed in 24 steps between 25 and 118 miles per hour. The tunnel is equipped with a 6-component balance for obtaining the forces in 3 directions and the moments about the 3 axes of an airplane. All seven dial scales of the balance system are of the recording type, which permits simultaneous records to be made of all forces.

The tunnel has been calibrated and surveys have shown that the dynamic-pressure distribution over that portion of the jet which would be occupied by an airplane having a wing span of 45 feet is within ± 1 1/2 per cent of a mean value. Based on the mean velocity of 118 miles per hour at the jet, the ratio of the kinetic energy per second to the energy input

to the propellers per second is 2.84. Since it is generally recognized that a long open jet is a source of energy loss, the above figure is considered very satisfactory.

Comparative tests on several airplanes have given results which are in good agreement with those obtained on the same airplanes in flight. This fact, together with information obtained in the tunnel on Clark Y airfoils, indicates that the flow in the tunnel is satisfactory and that the air stream has a very small amount of turbulence.

INTRODUCTION

It is a generally accepted fact that the aerodynamic characteristics of a small model can not be directly applied to a full-sized airplane without using an empirical correction factor to compensate for the lack of dynamic similarity. Two methods have been used to overcome this difficulty. One is to compress the working fluid and vary the kinematic viscosity to compensate for the reduction in the size of the model. This method is used in the variable-density wind tunnel where tests can be conducted at the same Reynolds number as would be experienced in flight. The other method is to conduct tests on the full-scale airplane.

The variable-density wind tunnel offers a satisfactory means for testing the component parts of an airplane and is particularly suitable for conducting fundamental research on airfoil sections and streamline bodies. However, this equipment has its limitations when the aerodynamic characteristics of a complete airplane are desired, especially if the effect of the slipstream is to be considered. It is practically impossible to build a model of the required size that is a true reproduction of a complete airplane. This difficulty is increased by the requirement that the model withstand large forces.

It is apparent that the most satisfactory method of obtaining aerodynamic characteristics of a complete airplane is to conduct a full-scale investigation. Heretofore such investigations have been conducted only in flight. Because of the variation in atmospheric conditions, it has been necessary to make a large number of check flights to obtain enough data to average out the discrepancies. Furthermore, in flight testing the scope of experiments is often limited by the fact that the possible alterations that can be made are restricted to those that do not seriously affect the weight or airworthiness of the airplane. In order to provide a means of full-scale investigation by which the conditions can be controlled and alterations made without serious limitations, the full-scale wind tunnel has been erected. Of course, only the steady-flight conditions can be readily investigated in the wind tunnel, but the execution of this work in the tunnel will facilitate full-scale testing and allow the flight-research personnel of the laboratory to concentrate on those problems possible of solution only in flight.

The full-scale wind tunnel may be used to determine the lift and drag characteristics of a complete airplane, to study the control and stability characteristics both with and without the slipstream, and to study body interference. In addition, equipment has been installed to determine the direction and velocity of the flow at any point around an airplane. Aircraft engine cooling and cowling problems can also be investigated under conditions similar to those in flight.

The design of the full-scale wind tunnel was started in 1929. Since this was to be the first wind tunnel constructed with an elliptic throat and with two propellers mounted side by side, a 1/15-scale model was constructed to study the flow problems. Very satisfactory flow conditions were obtained in the model tunnel. This piece of equipment is now being used for small-scale testing. Construction of the full-scale wind tunnel was started in the spring of 1930; it was completed and operated for the first time in the spring of 1931.

DESCRIPTION OF TUNNEL

The general arrangement of the tunnel is shown in figure 1 and an external view of the building is given in figure 2. The tunnel is of the double-return flow type with an open throat having a horizontal dimension of 60 feet and a vertical dimension of 30 feet. On either side of the test chamber is a return passage 50 feet wide, with the, height varying from 46 to 72 feet. The entire equipment is housed in a structure, the outside walls of which serve as the outer walls of the return passages. The overall length of the tunnel is 434 feet 6 inches, the width 222 feet, and the maximum height 97 feet. The framework is of structural steel and the walls and roof are of 5/16-inch corrugated cement asbestos sheets. The entrance and exit cones are constructed of 2-inch wood planking, attached to a steel frame and covered on the inside with galvanized sheet metal as a protection against fire.

Entrance cone.—The entrance cone is 75 feet in length and in this distance the cross section changes from a rectangle 72 by 110 feet to a 30 by 60 foot elliptic section. The area reduction in the entrance cone is slightly less than 5:1. The shape of the entrance cone was chosen to give as far as possible a constant acceleration to the air stream and to retain a 9-foot length of nozzle for directing the flow.

Test chamber.—The test chamber, in which is located the working section of the jet, is 80 by 122 feet. The length of the jet, or the distance between the end of the entrance cone and the smallest cross section of the exit-cone collector, is 71 feet. Doors 20 by 40 feet located in the walls of the return passage on one side provide access for airplanes. In the roof of the test chamber are two skylights, each approximately 30 by 40 feet, which provide excellent lighting conditions for daytime operation; eight 1,000-watt flood lights provide adequate artificial illumination for night operation. Attached to the roof trusses and running across the test chamber at right angles to the air stream and also in the direction of the air stream are tracks for an electric crane which lifts the airplanes onto the balance.

Exit cone.—Forward of the propellers and located on the center line of the tunnel is a smooth fairing which transforms the somewhat elliptic section of the

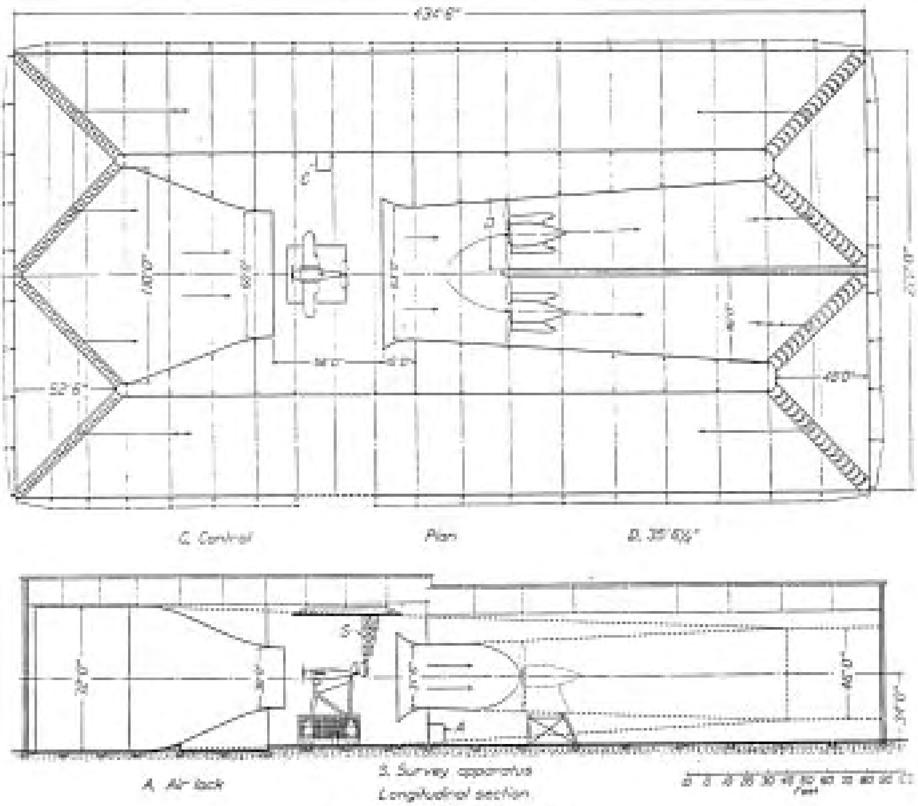


FIGURE 1.—Plan and elevation of the tunnel.

[illegible]

single passage into two circular ones at the propellers. From the propellers aft, the exit cone is divided into two passages and each transforms in the length of 132 feet from a 35-foot 6 1/2-inch circular section to a 46-foot square. The included angle between the sides of each passage is 6°.

Propellers.—The propellers are located side by side and 48 feet aft of the throat of the exit-cone bell. The propellers are 35 feet 5 inches in diameter and each consists of four cast aluminum alloy blades screwed into a cast-steel hub.

Motors.—The most commonly used power plant for operating a wind tunnel is a direct-current motor and motor-generator set with the Ward Leonard control system. For the full-scale wind tunnel it was found that alternating current slip-ring induction motors, together with satisfactory control equipment, could be purchased for approximately 30 per cent less than the direct-current equipment. Two 4,000-horsepower slip-ring induction motors with 24 steps of speed between 75 and 300 r.p.m. were therefore installed. In order to obtain the range of speed one pole change was provided and the other variations are obtained by the introduction of resistance in the rotor circuit. This control permits a variation in air speed from 25 to 118 miles per hour. The two motors are connected through an automatic switchboard to one drum-type controller located in the test chamber. All the control equipment is interlocked and connected through time-limit relays, so that regardless of how fast the controller handle is moved the motors will increase in speed at regular intervals.

The motors are provided with ball and roller bearings, which reduce the friction losses to a minimum. Roller bearings of 8.5- and 11.8-inch bores are provided at the slip-ring and propeller ends respectively, while the thrust of the propellers is taken on a ball bearing at the rear end of each motor shaft. The motors are mounted with the rotor shafts centered in the exit-cone passages. The motors and supporting structure are enclosed in fairings so that they offer a minimum resistance to the air flow.

Guide vanes.—The air is turned at the four corners of each return passage by guide vanes. The vanes are of the curved-airfoil type formed by two intersecting arcs with a rounded nose. The arcs were so chosen as to give a practically constant area through the vanes.

The vanes at the first two corners on back of the propellers have chords of 7 feet and are spaced at 0.45 and 0.47 of a chord length, respectively. Those at the opposite end of the tunnel have chords of 3 feet 6 inches and are spaced at 0.41 of a chord length. By a proper adjustment of the angular setting of the vanes, a satisfactory velocity distribution has been obtained and no honeycomb has been found necessary.

Balance.—The balance, which is of the 6-component type, is shown diagrammatically in figure 3. Ball and socket fittings at the top of each of the struts **A** hold

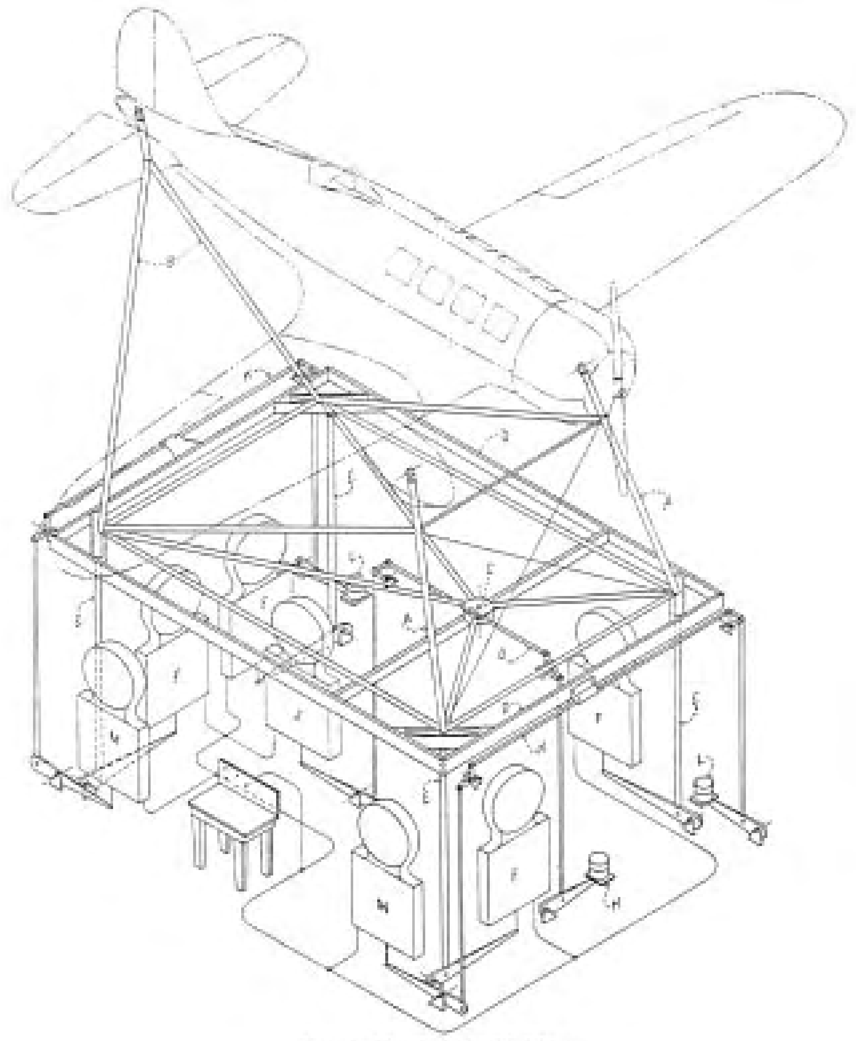
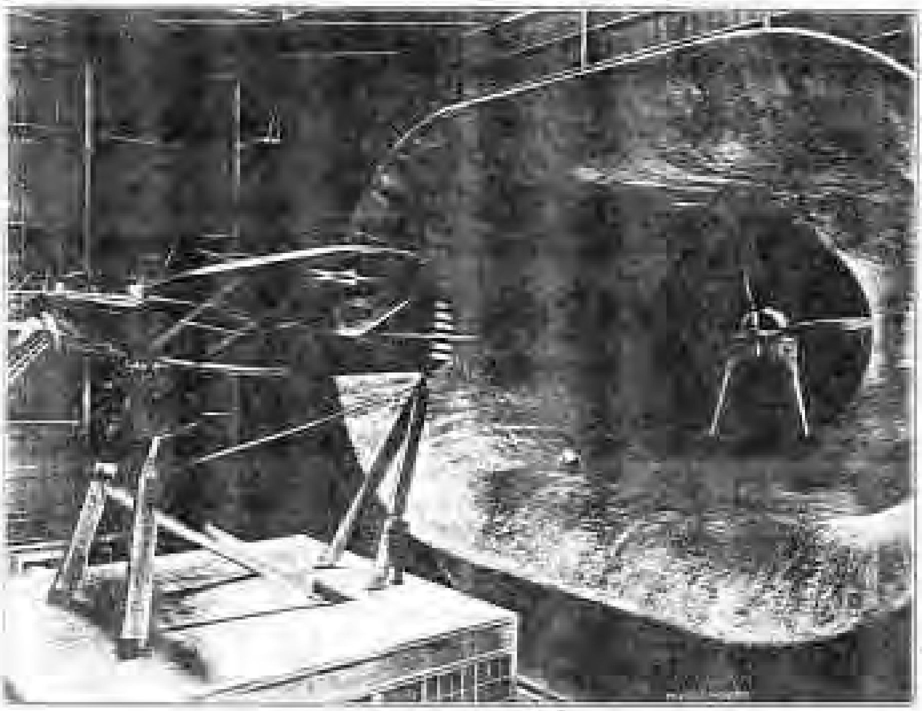


FIGURE 3—Schematic drawing of the balance.

the axles of the airplane to be tested; the tail is attached to the triangular frame **B**. These struts are secured to the turntable **C**, which is attached to the floating frame **D**. This frame rests on the struts **E**, which transmit the lift forces to the scales **F**. The drag linkage **G** is attached to the floating frame on the center line and, working against a known counterweight **H**, transmits the drag force to a scale **J**. The cross-wind force linkages **K** are attached to the floating frame on the front and rear sides at the center line. These linkages, working against known counterweights **L**, transmit the cross-wind force to scales **M**. In this manner forces in three directions are measured and by combining the forces and the proper lever arms, the pitching, rolling, and yawing moments can be computed.

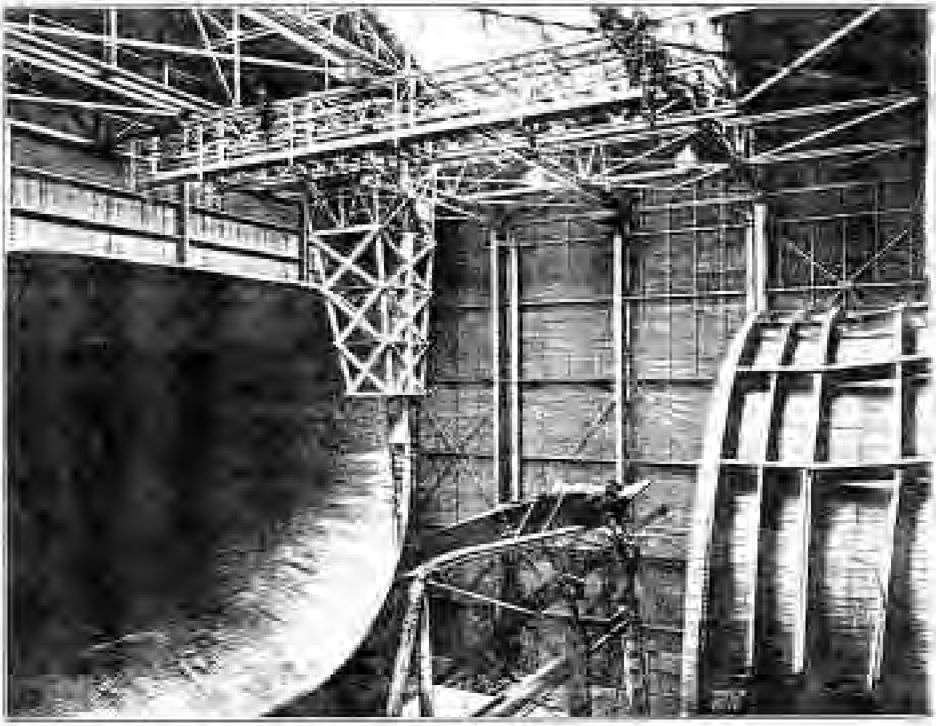


The scales are of the dial type and are provided with solenoid-operated printing devices. When the proper test condition is obtained, a push-button switch is momentarily closed and the readings on all seven scales are recorded simultaneously, eliminating the possibility of personal errors.

The triangular frame **B** is caused to telescope by electrically operated screws which raises and lowers the tail of the airplane and thereby varies the angle of attack. By a similar mechanism the turntable **C** can be moved so as to yaw the airplane from 20° left to 20° right.

The entire floating frame and scale assembly is enclosed in a room for protection from air currents and the supporting struts are shielded by streamlined fairings which are secured to the roof of the balance room and free from the balance. In figure 4 it can be seen that very limited amount of the supporting structure is exposed to the air stream. The tare-drag measurements are therefore reduced to a minimum.

Survey equipment.—Attached to the bottom of the roof trusses is a 55-foot structural steel bridge (fig. 5), which can be rolled across the full width of the test chamber; mounted on this bridge is a car which can be rolled along the entire length. Suspended below the car is a combined pitot, pitch, and yaw tube which can be raised or lowered and pitched or yawed by gearing with electrical control



[illegible]

on the car. This arrangement permits the alinement of the tube with the air flow at any point around an airplane. The alinement of the tube is indicated by null readings on the alcohol manometers connected to the pitch and yaw openings in the head and the angle of pitch or yaw is read from calibrated Veeder counters connected to the electric operating motors. This equipment is very valuable for studying the downwash behind wings and the flow around the tail surfaces of an airplane.

CALIBRATIONS AND TESTS

The velocity distribution has been measured over several planes at right angles to the jet, but the plane representing approximately the location of the wings of an airplane during tests was most completely explored. The dynamic-pressure distribution over the area that would be occupied during tests by an airplane with a wing span of 45 feet is within ±1? per cent of a mean value. It is possible to improve the distribution by further adjustment of the guide vanes. However, tests already conducted in the tunnel indicate that the present distribution does not detrimentally affect the results. This fact has been shown by the excellent agreement which has been obtained between the tunnel and flight results.

A survey of the static pressure along the axis of the tunnel showed that the longitudinal pressure gradient is small, as evidenced by the fact that between 11 and

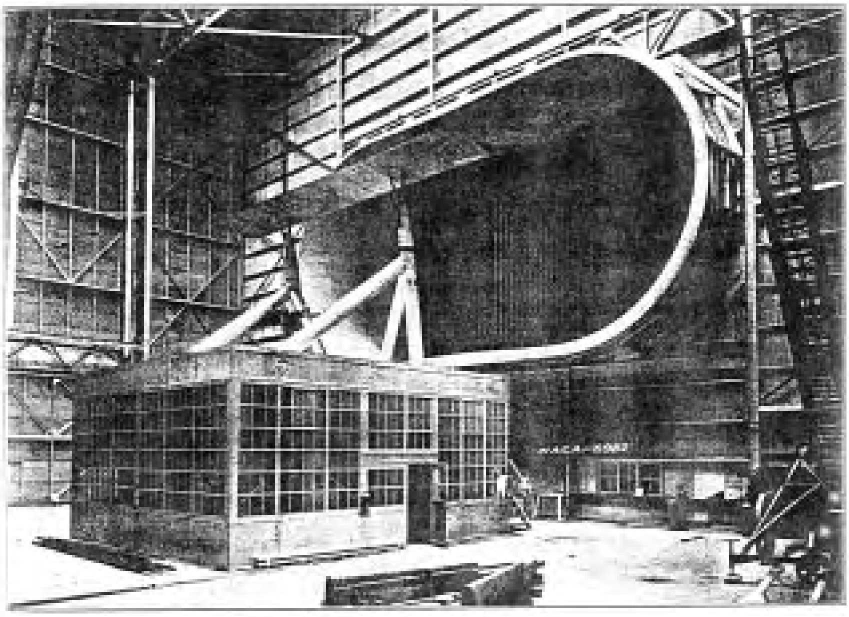


FIGURE 4.—A [illegible] Clark Y airfoil mounted in the tunnel.

36 feet from the entrance cone the variation of the static pressure is within ± 1 per cent of the mean dynamic pressure at the test section.

Two wall plates with static orifices are located in each return passage just ahead of the guide vanes at the entrance-cone end of the tunnel. The orifices are connected by a common pressure line, which is led to a micromanometer on the control desk in the test chamber. The other side of the manometer is left open to the test-chamber pressure. This installation has been calibrated against the average dynamic pressure determined by pitot surveys of the jet at the test location and it is used to determine the dynamic head during tests.

A series of Clark Y airfoils of the same aspect ratio, but with spans of 12, 24, 36, and 48 feet, have been tested at the same Reynolds number to determine the jet-boundary correction. Tests have also been made to determine the blocking effect of an airplane in the jet. The results of the complete investigation will be presented in a separate report.

Using the mean velocity across the jet of 118 miles per hour for computing the kinetic energy per second at the working section and dividing this by the energy input to the propellers per second gives an energy ratio for the tunnel of 2.84. This ratio, considering the length of the open jet, compares very favorably with the most efficient open-throat tunnels now in operation and exceeds the efficiency expected when the tunnel was designed.

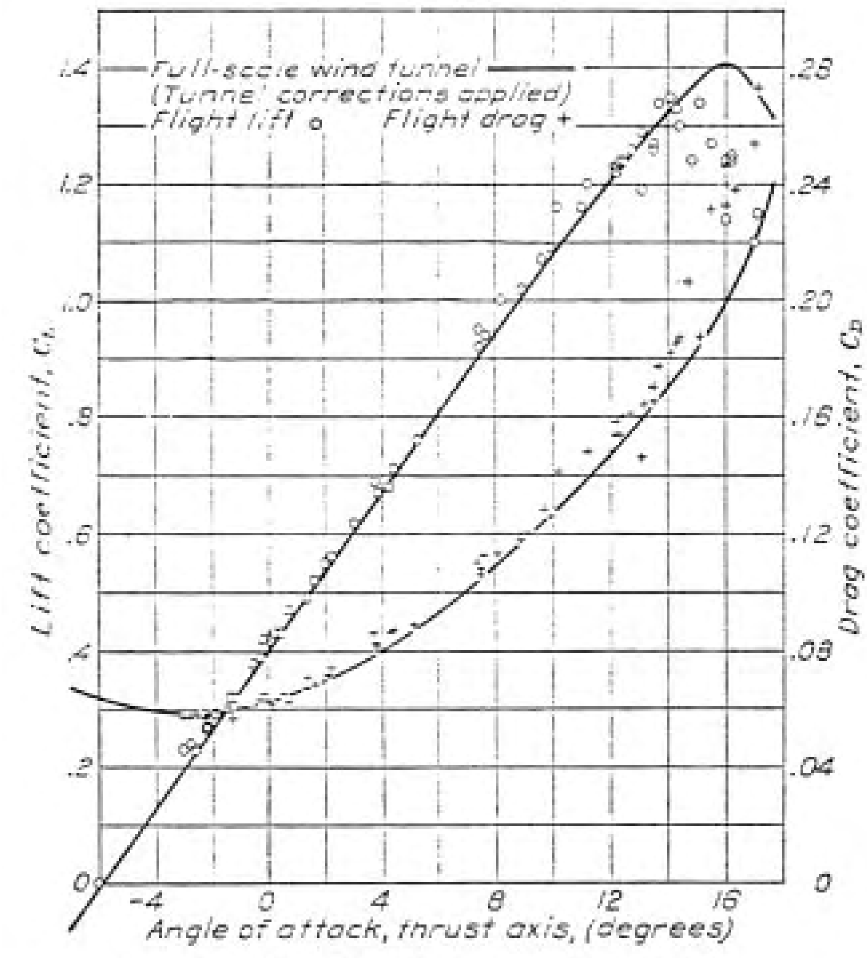


FIGURE 7.—Lift and drag characteristics of the F-22 airplane.

Before force measurements are made on an airplane, the airplane is suspended from the roof trusses by cables and held within one half inch from the balance supports while the tare forces are measured. The tare-drag coefficient determined in the above manner has been of the order of 25 per cent of the minimum drag coefficient of the airplanes tested.

When testing airfoils the airplane supports are replaced by those shown in figure 6. The angle of attack is changed by displacing the rear support arms and rotating the airfoil about pins in the top of the main supports. The rear support arms are moved by linkages, which are connected to long screws on the back of the main supports, and the screws are operated by hand cranks inside the balance house. The tare drag of this support system is exceptionally small and amounts to only 3 per cent of the minimum drag of a 6 by 36 foot Clark Y airfoil.

The lift and drag characteristics have been measured in the tunnel on several airplanes which had been previously tested in flight and their polars determined. These tests were conducted to obtain a check between the tunnel results and those from flight tests. A comparison of the results from the two methods of testing for one of the airplanes, the Fairchild F-22, is shown in figure 7. The wind-tunnel results are shown by the solid lines and the flight results are presented by the experimental points. These curves are representative of the results obtained with the different airplanes.

The agreement that has been obtained between the flight and full-scale tunnel results, together with the consistent manner in which measurements can be repeated when check tests are made, has demonstrated the accuracy and value of the equipment for aeronautical research.

LANGLEY MEMORIAL AERONAUTICAL LABORATORY,
NATIONAL ADVISORY COMMITTEE FOR AERONAUTICS,
LANGLEY FIELD, VA., March 13, 1933.

Document 2-22

Max M. Munk, memorandum on "Recommendations for New Research," 16 November 1926, AV400-1, LaRC Correspondence Files, NARA, Philadelphia, PA.

This letter from Max Munk, chief of the Aerodynamics Division at Langley, to George Lewis, the NACA's director for research in Washington, furnishes an intriguing glimpse into Munk's vision for himself and the Langley Laboratory. Five days earlier, Lewis had issued instructions that all recommendations for new research projects from the Langley technical staff be forwarded to his office, via the engineer-in-charge, for consideration by the appropriate subcommittee. While this appeared to be an innocuous request to streamline the process, Munk took decided issue with a process that could bypass him. Noting that Langley was "at present pretty well filled up with problems; we are really overstocked," Munk downplayed the need for the Langley staff to make such recommendations. He expressed concern over his staff's need to instead focus "the fullest amount of thought and interest" on current projects, and suggested that a staff member should only propose projects "derived directly from the problem he is engaged in at the time." Such suggestions from subordinates would, of course, be reviewed by Munk. Given Munk's uncompromising faith in himself and his commanding methods, one senses that this may have been more of an effort to retain control over the research program at Langley than an expression of real concern over workload and a possible compromise of research quality.

Document 2-22, Max M. Munk, memorandum on "Recommendations for New Research," 1926.

November 16, 1926

Comment of Dr. Munk.
Subject: Recommendation for new research.
Reference: NACA Let. Nov. 11, 1926. 21-1.

1. The type of wings of reference has been discussed at the last meeting of the Subcommittee on Aerodynamics.

2. As a comment to the memorandum of Mr. E. G. Reid, and to Mr. Lewis' letter, I wish to call to Mr. Reid's and to Mr. Lewis' attention, that we are at present pretty well filled up with problems, we are really overstocked. Each problem should receive the fullest amount of thought and interest and should be carried through as far as can be. Otherwise, we might degenerate into a mere test factory. From

this point of view it is desirable to have only as many problems being turned over from outside as absolutely necessary. It is further desirable that each staff member propose chiefly such new problems as are derived directly from the problem he is engaged in at the time. Otherwise, the conclusion can not be avoided that he does not concentrate his entire mind on his problem; and furthermore, he is less prepared to know about the desirability of his proposed problem, if it does not belong to his present work in investigating.

3. To sum up, we need on the side of our staff members the serious will and the intense interest necessary to solve problems, rather than reflecting on new problems to be solved by somebody else.

[signed] Max M. Munk
Technical Assistant

Document 2-23(a–b)

(a) Daniel Guggenheim, letter to Secretary of Commerce Herbert Hoover, 16 January 1926, reprinted by Reginald M. Cleveland in *America Fledges Wings* (Chicago: Pittman Publishing Co., 1942), pp. 3–7.

(b) California Institute of Technology, "Development of Aeronautics," August 1926, Historical Files, Folder A1.1, California Institute of Technology Archives, Pasadena, CA.

While the Federal government's investment in aeronautical research facilities grew throughout the 1920s, other wind tunnels were built with private funding. While "airmindedness" and a general interest in the possibilities of commercial aviation were on the rise during this period, no one could be sure of its future. Flying remained a risky business, and aircraft manufacturers found it difficult to attract investment capital until some of the safety issues could be resolved. There was a pressing need for trained aeronautical engineers in 1925, but only five American schools offered aeronautical-engineering courses, and among them only MIT and the University of Michigan awarded degrees in aeronautical engineering. Such programs required wind tunnel laboratories for instruction and research, but the colleges were either private or state institutions and Federal funding was generally unavailable in the mid-1920s.

Into this void stepped Harry Guggenheim and his father, Daniel. Daniel Guggenheim was an American businessman who, along with his brothers, had acquired a large fortune managing mining operations started by his father. Harry, who had served as a pilot during World War I and maintained a keen interest in aviation after the war, determined that a committee of wealthy men should underwrite research that would lead to safer airplanes. Over lunch one day, the younger Guggenheim heard of a plan by New York University to raise $50,000 to endow a new aeronautical engineering program. Rather than a public appeal, he proposed that New York University Chancellor Elmer E. Brown write his father—Harry would personally deliver the letter—and request a gift from the Guggenheim family. Rather than involve his brothers, the elder Guggenheim decided to make the donation himself, and NYU opened its Guggenheim School of Aeronautics in 1925.

Daniel Guggenheim soon began to think beyond NYU, however, and in a letter to Commerce Secretary Herbert Hoover he offered to underwrite a larger, nationwide program, the Daniel Guggenheim Fund for the Promotion of Aeronautics, and endow it with $2,500,000, a substantial sum for the day. Established in 1926, the

Guggenheim Fund relied on a board of distinguished aviation personalities to determine the fund's distributions. Between 1926 and 1930, the Guggenheim Fund awarded major grants totaling $1,693,000 to seven engineering schools: the California Institute of Technology (Caltech), Leland Stanford University, the University of Michigan, MIT, the University of Washington, the Georgia Institute of Technology (Georgia Tech), and the University of Akron. A large portion of these grants went into construction of wind tunnel laboratories, but, significantly, some funds were earmarked for the hiring of exceptional professors and researchers. A Guggenheim grant allowed Clark and Robert Millikan to woo Theodore von Kármán, Germany's outstanding theoretical aerodynamicist (and Prandtl protégé), into moving to California and assuming the directorship of Caltech's Guggenheim Aeronautical Laboratory (GALCIT), where he would play a dominant role in shaping the growth of theoretical aeronautics in the United States. While the Guggenheim wind tunnels were generally unremarkable, exhibiting no significant technical innovations, they were a vital part of programs that began to supply the professional engineering talent that American aviation so badly needed. By midcentury, over 90 percent of the nation's leading aeronautical engineers were graduates of Guggenheim-funded colleges.

Document 2-23(a), letter from Daniel Guggenheim to Herbert Hoover, 1926.

Honorable Herbert Hoover
Secretary of Commerce
Washington, D. C.
My Dear Mr. Secretary:

Under your general direction the United States Government has made substantial progress in the promotion of civil aviation. I am venturing to advise you, therefore, by this letter, of my purpose to establish a Fund which will cooperate with you and with all agencies of the Government and the public generally in advancing the art and science of aeronautics and aviation.

This action is taken particularly in view of the very wise endorsement by the President of the United States of the recommendation by the National Advisory Committee for Aeronautics that a Bureau of Air Navigation be established in the Department of Commerce. President Coolidge stated that:

"The outstanding weakness in the industrial situation as it affects national defense is the inadequacy of facilities to supply air service needs. The airplane industry in this country at the present time is dependent almost entirely upon Governmental business."

The Department of Commerce, in studying the need for commercial aviation, reported:

"There are indications of a great change in the last few months which has given an impetus to plans for developing American civil aeronautics that is bound to produce permanent results. The extent of our country, its physical characteristics and its intimate contact with Canada, Mexico, and the West Indies are such as to make air service highly desirable. The success of the transcontinental air mail service operated by the Post Office Department and the general approval which has greeted its operations indicate that the choice of a method for developing civil aeronautics in the United States is the question demanding immediate solution. There is every reason to hope that before the end of the present year, civil aviation in the United States will have taken a long step forward toward a position of permanent security."

You, yourself, have pointed out that a Government public service must be provided to cooperate with aviation in a manner comparable to the cooperation our Government now gives to shipping, and you have very properly pointed out that without such service "aviation can only develop in a primitive way." There is undoubtedly a function in this situation which the Government alone can perform.

I have also been much impressed by the question raised and the answer given, in the report of the President's Aircraft Board which, as of November 30, 1925, said:

"How can the civilian use of aircraft be promoted? This . . . may well be the most important question which aviation presents in its far-reaching consequences to our people. A great opportunity lies before the United States. We have natural resources, industrial organization, and long distances free from customs barriers. We may, if we will, take the lead in the world, in extending civil aviation."

Such considerations as the foregoing have convinced me that there is a function which can only be performed by private enterprise aside from the proper function of the Government. So much remains to be done before civil aviation can realize the possibilities before it, that everyone must recognize that there intervenes a period of necessary study and experimentation.

In these circumstances I have decided to establish the Daniel Guggenheim Fund for the Promotion of Aeronautics and to place at its disposal the sum of $2,500,000. The Fund will be administered by a Board of Trustees composed of men of eminence and competence.

I shall place the sum of $500,000 immediately in the hands of the trustees to defray the expense of their studies and any work they may decide immediately to undertake. In addition, I will hold myself in readiness to supply any additional sum, up to a total of a further $2,000,000 as and when the judgment of the trustees may indicate that the money can be used wisely to promote the aims of the Fund. The trustees will have unrestricted power to do anything which in their judgment may develop aeronautics, the only condition being that the Fund shall not be a profit-making enterprise. Any earnings the Fund may realize from its efforts will go back into the Fund to carry on the work for which it was created.

The trustees are to have the power to spend the principal sum thus contributed and there is no purpose to establish a permanent foundation. The thought is, rather, that, the whole art and science of aeronautics and aviation being now in its infancy, it will be possible with the sums thus contributed, to bring about such an advance in the art that private enterprise will find it practicable and profitable to "carry on" and thus render a continuous and permanent endowment for this purpose unnecessary.

You will perhaps recall that last year I established a School of Aeronautics at New York University, my desire in making that gift being more quickly to realize for humanity the ultimate possibilities of aerial navigation, and to give America the place in the air to which her inventive genius entitles her. This school is already making gratifying progress, and the studies that have been made in connection with it have indicated clearly the enormous field of opportunity which unfolds itself to the pending development of air transportation. The establishment of additional schools such as that at New York University may well be warranted in the future.

Among the most important objects which I would now like to see accomplished at the earliest possible moment is the development of opportunities for new fields of employment for American young men. My family, as you know, has long been identified with exploration beneath the earth. We have tried to assist in development which would make mining more safe as well as more profitable, and therefore, of the greatest economic and value. Not the least desirable results which have followed from this effort have been the opportunities for the profitable employment of able engineers and workmen generally. My hopes, therefore, are that, through the impetus which the Daniel Guggenheim Fund for the Promotion of Aeronautics will give, attractive opportunities for men to work and serve in the air may develop far more rapidly than would otherwise be the case.

The general purposes to which I trust the new Fund will devote itself may be broadly defined as follows:

1. To promote aeronautical education both in institutions of learning and among the general public.

2. To assist in the extension of fundamental aeronautical science.

3. To assist in the development of commercial aircraft and aircraft equipment.

4. To further the application of aircraft in business, industry, and other economic and social activities of the nation.

I am hopeful without desiring in any sense to restrict their own freedom of judgment, that the Trustees of the Fund will govern themselves as far as possible by the following principles:

1. Restrict the work to civil activities.
2. Avoid duplication of effort with other aeronautical organizations.
3. Avoid work which is properly a Government function.
4. Plan carefully to concentrate effort and to carry an investigation or project through to definite conclusions.
5. Maintain a simple, inexpensive directing organization depending on outside established agencies wherever possible, to carry out the aims of the Fund.

I have confidence that the Fund can serve an important purpose. Recent events in the United States have stimulated much discussion of aviation. The time is ripe for action. There is urgent need in our country for immediate, practical, and substantial assistance to aviation in its commercial, industrial, and scientific aspects. No less urgent is the need to awaken the American public, especially our business men, to the advantages and possibilities of commercial aircraft—in a word, to make the American public in a very real sense, "air-wise."

In closing, Sir, may I express my delight at the intelligent and constructive interest you yourself are manifesting in the development of aeronautics as one of the most important new agencies of civilization. In making my deed of gift to the trustees of the Fund, I shall accordingly request that the trustees cooperate with your Department in every possible manner.

With best wishes, I am, Mr. Secretary,
Yours very truly,
Daniel Guggenheim
January 16, 1926

Document 2-23(b), California Institute of Technology, "Development of Aeronautics," 1926.

DEVELOPMENT OF AERONAUTICS

It has been announced that the Daniel Guggenheim Fund for the Promotion of Aeronautics has made a gift of $300,000 to the California Institute of Technology, and an equivalent gift to Stanford University. News of this gift was received at the California Institute in the following telegram from Harry F. Guggenheim, President of the Daniel Guggenheim Fund:

"It gives me great pleasure as President of the Daniel Guggenheim Fund for the Promotion of Aeronautics to advise you that the Trustees of the Fund have authorized a grant amounting altogether to approximately $300,000 for the erection of a permanent building at the California Institute of Technology to be devoted to the study of aeronautics and including a provision of fifteen thousand dollars a year for a term of years for the conduct of study and experiments in this rapidly developing science and art. May I remark that this gift is made in recognition not merely of the opportunities for study and research which the climatic and other conditions in California make possible, but also as a tribute to the distinguished work in science and education of yourself and associates, and because of our belief that you are developing in Southern California an institution which is destined to make very great contributions to the progress not only of our own country but of the whole world."

On the basis of this gift the California Institute has established the Daniel Guggenheim Graduate School of Aeronautics at the California Institute of Technology, and there will be constructed immediately a new Aeronautics Building containing a new ten-foot High-speed wind tunnel, the total construction involving an expense of approximately $200,000.

The California Institute has just announced the programs:

1. The extension of the Institute's theoretical courses in aerodynamics and hydrodynamics, with the underlying mathematics and mechanics, taught by Professors Harry Bateman, Edward T. Bell, and Paul S. Epstein.

2. The initiation of a group of practical courses conducted by the Institute's experimental staff in cooperation with the engineering staff of the Douglas Airplane Company, with the aid of the facilities now being provided at the Institute combined with those of the Douglas plant.

3. The initiation of a comprehensive research program on airplane and motor design, as well as on the theoretical bases of aeronautics.

4. The immediate perfection of the new stagger-decalage, tailless airplane recently developed at the Institute, primarily by one of its instructors in aeronautics, A. A. Merrill, a radical departure from standard aeronautical design, which in recent tests has shown promise of adding greatly to the safety of flying.

5. The establishment of a number of research fellowships in aeronautics at the California Institute.

6. The building and testing, not only of models for wind tunnel work, but also of full-size experimental gliders and power planes for free flight work.

It is considered of especial importance that the facilities of the Douglas Airplane Company in Santa Monica, with its large corps of engineers, will be added to those of the Institute for both instructional and research purposes in the effort to make a center of the first importance in Southern California for the development of both the theoretical and practical phases of aeronautics.

The Daniel Guggenheim Fund for the Promotion of Aeronautics was established by Mr. Daniel Guggenheim last February with provision for supplying $2,500,000 as needed by the Fund, the Fund being unique among the great foundations of the country in that its founder did not contemplate a permanent foundation but merely provision for sums which would make possible experimentation and development in the field of aeronautics and aviation during the infancy of this art and science as a civil enterprise.

Last year Mr. Guggenheim established the School of Aeronautics at New York University, with a principal fund of $500,000; and now one of the first acts of the new Guggenheim Fund is to make financial provision of equal amount in order that the scientific experience and the equable climatic conditions of California may be utilized to supplement the work of eastern and European institutions in the study of this important subject.

A further announcement was made today that Dr. von Karman of Aachen, Germany, one of the foremost mathematical physicists and one of the most outstanding aeronautical engineers of Europe, has accepted the invitation of the Daniel Guggenheim Fund to visit the aeronautical centers of this country in the near future. He will go immediately to Pasadena for the sake of advising with the aeronautical staff of the Institute, both as to the best type of aeronautical installation and as to the design of new planes already being perfected there.

Document 2-24(a–b)

(a) C. G. Grey, excerpts from "On Research," *Aeroplane* 35, No. 21 (21 November 1928): 837–840.

(b) C. G. Grey, excerpts continued from "On Research," *Aeroplane* 35, No. 22, (28 November 1928).

Before the end of the 1920s, the American model for aerodynamics research had become the standard by which much of the industrialized world judged its own efforts. While the empirical and theoretical contributions of other nations, particularly Great Britain and Germany, were substantial, critics condemned their governmental agencies and longed for research organizations and facilities like those in the United States. The criticism in England is especially interesting since that nation's Advisory Committee for Aeronautics served as the model for the American NACA. In 1928, *Aeroplane* Editor C. G. Grey penned a two-part editorial in response to comments by C. W. Brett, managing director of Barimar Ltd. Brett had called for an aeronautical research laboratory independent from the government agencies, which he felt could not address the needs of independent inventors. He proposed that it be funded and managed by a consortium of three existing British aviation organizations. Grey agreed that such a facility was needed, but he doubted that Brett's organization plan would work and instead called for a wealthy benefactor to underwrite the facility, much like Daniel Guggenheim had done in America. Such a laboratory never came to pass in England, but this editorial called attention to the fact that "ad hoc" research—research focused on solving practical problems, such as that practiced in American universities and the NACA—had produced some outstanding results.

Document 2-24(a), C. G. Grey, excerpts from "On Research," 1928.

AS THINGS ARE.

The official reply to Mr. Brett's suggestion, if a Question were asked about it in the House of Commons, would be that any inventor who has an idea, whether patented or not, for the improvement of anything aeronautical, has only to take it to the Air Ministry, and that if the official experts judge it to be of any value it will be tested impartially on behalf of the Air Ministry either at the Royal Aircraft Establishment at Farnborough or at the National Physical Laboratory at Teddington.

But the trouble is, as Mr. Brett suggests, that the Air Ministry grant for purely experimental work is in fact too small,—in spite of the half million or so pounds per annum spent by the R.A.E. at Farnborough. And so an undue amount of time passes before experiments can be made.

The N.P.L. at Teddington is always far behind in its experimental work, owing to pressure of work and likewise lack of money. Moreover it is not primarily interested in aeronautics. And so aeronautical experiments have to take their turn with all other branches of engineering.

As for the R.A.E., there is always the suspicion that if an inventor submits a really good idea his invention may be held up for months, and even for years, while the bright brains at Farnborough, who are in fact paid for producing new ideas themselves, try to invent some better way of doing the same thing as the inventor has done. Even when there is no suspicion of that sort the R.A.E. is so busy experimenting with its own inventions that it does take a long time to test anybody else's. For example, the Pobjoy engine, which recently passed its type-test successfully, was actually sent to Farnborough last March, and was only put through its tests a few weeks ago.

The really wily inventor can, certainly, get his invention tested in other ways. If he happens to have a friend on the spot he may have tests made at South Kensington or at Cambridge, provided that his invention comes within the scope of the experts at those places. Or if he is a good enough salesman (which few inventors are) he may induce one or two manufacturers to test his invention in their own experimental shops, or on their machines.

All these methods have their objections, and at the [finish] the inventor who manages to get his invention properly tested within a reasonable time must be something of a specialist in the gentle art of wangling,—without which in peace as in war, achievement of a desired end is difficult.

Therefore there is a great deal to be said for Mr. Brett's suggestion that a central and completely equipped laboratory and experimental station which exists entirely by, with and from aviation is desirable. The only question is, who is going to pay for it?

The Aircraft Industry certainly has not the money. And if it had the money, individual members of the industry would certainly not cooperate in anything which might help general progress and so help their rivals. The British Taxpayer obviously could not be asked to fork out money for the purpose so long as there are doles and pensions to be paid.

There remains then only the dim hope that some British millionaire, possibly of extraneous origin, such as Sir Basil Zaharoff, who has already founded the Chair of Aviation of the London University, might be induced to put his hand in his pocket for the purpose much in the same way as that good hundred-per-cent American, Mr. Daniel Guggenheim, has done in establishing the Daniel Guggenheim Fund for Aeronautics.

[. . .]

WHAT RESEARCH MEANS.

Research is of two kinds, that which is known as basic research, and that which is known as ad hoc research. In the former kind seekers after truth dig around blindly, working on more or less established, scientific facts, in the hope that something may turn up. In the latter kind deliberate research is made to discover a certain thing,—*ad hoc* meaning "*to this*," the word "*object*" or "*end*" being understood.

Occasionally in *ad hoc* research for one thing quite a different thing, or a different application of the same thing, may be discovered accidentally,—as, for example, the usefulness of the Handley Page slot in assuring control below stalling point, when in fact search was being made for a method of improving lift and slowing landing speeds. Basic research may, by sheer luck, discover something which revolutionizes the whole industry, or science, with which it is concerned. But, on the whole, the cheapest and quickest way of making progress is by means of *ad hoc* research.

That is just where the Americans have scored over us so far. Over and over again their practical aeronautical engineers have started out with one definite end in view, and they have pursued that end until they have got it.

The present improved performances of aircraft practically all over the World is due primarily to the search for the proper stream-line shape of fuselages and engine housings for high-speed racing machines in the Curtiss Company's wind tunnel. The Curtiss designs were brought to this country by Mr. C. R. Fairey five years ago, and those lines dominate the design of the machines with the highest performances all over the World.

Further search for improved performance, and consequent economy of running, caused the invention of the high-pressure wind tunnel (300 lbs. to the square inch) by a German engineer who was imported to the States just after the War. And the high-speed wind tunnel with its air-stream of 100 miles an hour, and 20 feet diameter, which the American National Advisory Committee for Aeronautics have at Langley Field was built because the N.A.C.A. wanted to experiment with engines and airscrews on fuselages under flying conditions.

The N.A.C.A. has had its high-pressure wind tunnel for five years or so, and it has discovered all kinds of things about interference effects as between fuselages and planes and struts and undercarriages and tail units, of which we in this country know but little. We are just beginning to build a high-pressure wind tunnel.

If only we had a completely equipped laboratory and full-scale experimental station, such as Mr. Brett has suggested, and if we only had the money to run it, regardless of expense, and if we only had the man at the top and a staff to spend that money efficiently and to get results from it, we certainly could make as much progress in the next five years as we are likely to make in the next twenty-five.

This is said without belittling in any way the splendid work which is being done by the Department of Supply and Research at the Air Ministry. There everything possible is done, even to the point of being unconstitutional, to hasten experiments and to urge research. But there are limits to what any Government Official can do. And private enterprise has no limits except those imposed by finance.

Nor are there any limits to the things to be discovered, if Aviation is to progress as it should. Next week we will consider some of these.—C.G.G.

(To Be Continued.)

Document 2-24(b), C. G. Grey, excerpt continued from "On Research," 1928.

THINGS TO BE DISCOVERED

One of the interesting facts which has been discovered in the N.A.C.A.'s high-speed tunnel is that by totally enclosing a radial engine in cowling the speed of a machine can be increased beyond anything which mere calculation would show to be possible, and yet the engine can be kept cool. A machine of the popular cabin-monoplane type which, with a 200 h.p.Whirlwind engine and normal cowling covering about three-quarters of it, has a speed of 125 m.p.h., has had its speed up to up to 133 m.p.h. with complete cowling. And, incidentally, as photographs show, the appearance of the machine is improved.

Figures quoted in THE AEROPLANE recently from an American paper gave the improvement as being from 118 m.p.h. to 137 m.p.h., and a scientific critic proved that the higher speed was impossibly high. The true figures, as herein quoted on reliable authority, are startling enough. No doubt they also could be proved by pure or applied mathematics to be impossible. And quite possibly the higher figure may be reached by further improvements.

Experiments have gone that far, but there is still more to be done in the way of discovering what shape of cowling best suits particular forms of fuselages. Also the shape of spinner which best suits particular forms of cowling has to be discovered,—and whether, with certain airscrews and cowling, any kind of spinner is worthwhile has to be proved.

Then there are experiments to be made as to the best shapes of fuselage which can be made big enough to include a cabin. And there is the question of the best way of fixing a wing in relation to a cabin fuselage.

Naturally all our own experimentalists have been and still are engaged on these problems. Some of them try to get there with slide rules and the Prandtl Theory. Some of them try to do it in wind-tunnels with such low speeds that the results may be quite misleading. And some of them try to do it on actual aeroplanes, in which all sorts of errors are likely to occur because a combination of undercarriage, wing, fuselage, engine, and cowling which gives a certain performance may have that performance entirely upset by altering any one of the components, though altering other components to fit in with the first alteration might give a vastly improved performance.

Document 2-25(a–c)

(a) Frank A. Tichenor, "Why the N.A.C.A.?" *Aero Digest* 17, No. 6 (December 1930): 40, 124–134.

(b) Frank A. Tichenor, "The N.A.C.A. Counters," *Aero Digest* 18, No. 2 (February 1931): 50, 122–126.

(c) Edward P. Warner, "Speaking of Research," *Aviation* 30, No. 1 (January 1931): 3–4.

While the NACA's continued expansion of its research programs and facilities was generally applauded, both at home and abroad, it was not without controversy and criticism. The NACA's founding legislation had charged the committee to "direct the scientific study of the problems of flight, with a view to their practical solution," but this mandate was interpreted differently by different people. By 1931, the NACA tended to emphasize the last phrase, and its efforts were largely directed toward finding practical solutions to well-known problems. Critics, including *Aero Digest* Editor Frank Tichenor, argued that Congress intended something else entirely. In two of his "Air—Hot and Otherwise" columns, Tichenor blasted the NACA for its failure to continually generate new, purely scientific knowledge, something he claimed its charter demanded. The NACA's chief of aerodynamics, Elton Miller, prepared a response for both his boss, Henry Reid, and *Aviation* Editor (and NACA Committee on Aeronautics member) Edward Warner, who based a rebuttal editorial on it. Although glossing over the fundamental science issue, they countered that the NACA saw its development work as science with a purpose, but legitimate science nonetheless. In truth, the argument was really over the difference between science and engineering, a distinction frequently missed in the era, even within ranks of the NACA. Tichenor's criticism had little long-term effect on the NACA, and Warner received numerous compliments for his stand, but the exchange did serve to illustrate that aeronautical research had come to play a visible, important, and growing role in aviation development, even if it was not purely, or even primarily, one of fundamental science.

Document 2-25(a), Frank A. Tichenor, "Why the N.A.C.A.?" 1930.

AIR—HOT AND OTHERWISE

Why the N.A.C.A.?

By Frank A. Tichenor

HERE is a matter of such vital importance to the industry that we can not write of it save with plain words of considerable solemnity. It is a matter to which we respectfully would call the attention of the President. Indeed, we do so explicitly and respectfully, refraining from anything except such a statement as will make facts clear.

In this period of industrial readjustment, particularly in the aviation industry, our thoughts turn to a very important basis of technical enterprise, experimental aeronautical research. A young industry is more dependent on research, and at the same time less able to provide for it, than older and better established industries. Because the Government has been well aware of this situation, nearly all aeronautic research in this country has been financed and carried on by the Federal Government. Foremost in this activity has been the National Advisory Committee for Aeronautics, for which Congress has provided funds. The N.A.C.A. has obtained from Congress funds for the largest, the most splendidly equipped and the most modern laboratories, and facilities for aeronautic research. To all practical purposes aeronautic research in America means N.A.C.A. research. Our thoughts turn in this hour to this research activity, and with full concern for conditions in the aeronautic industry, we ask ourselves whether the N.A.C.A. has discharged its duty well, whether it has given to the industry the full return to which it is entitled for these appropriations.

How greatly aeronautic progress depends upon research has indeed been fully realized by those in charge of N.A.C.A. work, as is indicated in the annual report of the N.A.C.A. for 1921 (page 5) :

"Substantial progress in aeronautical development must be based upon the application to the problems of flight of scientific principles and the results of research."

Research activity of the N.A.C.A. has been going on for more than ten years. The first appropriation for a wind tunnel having been made in 1917, this tunnel was reported to have been completed in 1918. Experts tell us that a year is ample time to build an ordinary small wind tunnel. Nevertheless, although the wind tunnel was completed, it was not then put into operation. In 1919, the tunnel was again reported not yet in operation. Finally, in 1920, the same tunnel originally reported as finished in 1918, was once more reported as finished. The year 1920, therefore, we are entitled to consider as the beginning of research activity, particularly inasmuch as an engine laboratory and free flight test facilities had been announced as completed in 1919.

This fact is important because the results of research can not be judged from the activity of one day, or one month or even one year. After ten years of uninterrupted activity, however, with continuous liberal financial support, the N.A.C.A. can be judged according to the results derived from its research work and an estimate can be made of what we have a right to expect in the future. Let us, therefore, review these results and ascertain what the N.A.C.A. has achieved.

The standard by which the results of research should be appraised is defined by the N.A.C.A. itself. Repeatedly, its annual reports have stressed scientific research as of paramount importance. For instance, almost all reports close like that of 1927 (page 76) : "Further substantial progress is dependent largely upon the continuous prosecution of scientific research," and farther below on the same page, "its (the N.A.C.A.'s) work in the fields of pure and applied research on the *fundamental* problems of flight." The latest report, that for 1929, states (page 87) : "The most important active influence upon aeronautics has been the farsighted and constructive policy of the Federal Government, liberally supported by Congress and the President, in providing for the continuous prosecution of organized scientific research." In the 1926 report we find (page 69), "The more *fundamental* investigations are undertaken by the Committee in its own laboratory," and (page 68), "to conduct investigations of a truly *scientific* character." (The italics are mine.)

We could easily quote other passages from N.A.C.A. publications to the same effect. The N.A.C.A. is not an aircraft factory; it is not interested in the properties or the development of any particular airplane. More general scientific investigations are its domain. It is charged with the responsibility of furnishing information concerning aeronautics as a science.

Nor do the annual reports of the N.A.C.A. leave any doubt about what is meant by "scientific research." That of 1922 (page 48), defines the term clearly:

"By scientific research is meant the investigation by trained men in a properly equipped laboratory of the *fundamental phenomena of nature*. . . . All progress depends upon the acquisition of knowledge, of new knowledge. This can be obtained only by long continued investigations *directed by men who know the problems and the methods used for their solutions*."

Perhaps the best standard by which to judge the results of ten years of N.A.C.A. research is in terms of returns for the funds spent. Even with a small appropriation there is no upper limit to what can be obtained in the way of research if that research is directed "by men who know. . . . " There is, however, a lower limit to what ought to be obtained for a given amount of money. It stands to reason that we can expect more for an expenditure of $2,500 than for one of $250, and more for one of $25,000 than for one of $2,500.

The N.A.C.A. has spent on each of its research items undertaken more than $100,000, and we have a right to count on important results from $100,000 researches. This average expenditure for each problem investigated is computed by dividing the sum of the money spent by the number of problems undertaken. Thus far the N.A.C.A. has received $4,936,370 in appropriations. Approximately $4,800.000 has been spent (presuming the expenditure of the whole sum of $1,508,000 appropriated for 1930). The results of its research are laid down in eighty-eight Technical Reports. All other N.A.C.A. Technical Reports contain information obtained from outside sources, the N.A.C.A. acting only as publisher. This means that more than $30,000 has been spent for each report on a research project. It means much more per research, for at least four reports are always issued on the same research. This would give $200,000 per research item. Allowing for those research projects not yet completed for which no reports have yet been published and allowing also deductions for other expenses of the N.A.C.A., we are certainly justified in estimating that more than $100,000 has been spent for each research undertaken. Since 1925, and until 1930, the annual appropriation for the National Advisory Committee for Aeronautics has been approximately $500,000. This year it was increased to $1,508,000. No one can claim that during any one of the last four years more than five research problems have been finished and the results made available to the public. One hundred thousand dollars per research is perhaps too moderate an estimate.

It is pertinent to ask whether really useful scientific results have been obtained, and if not, to inquire about the reasons why research so liberally supported failed to furnish an adequate return. This sum can not be considered exorbitant if valuable results have been obtained from it.

If we make a more detailed analysis of the N.A.C.A.'s research of the past ten years, we find that it can be classified into wind tunnel research, free flight research on actual airplanes, and engine laboratory research.

In the engine laboratory, tests have been conducted with a view to improving the efficiency of gasoline aircraft engines by the choice of the best compression ratios, richness, and mixtures, and the like. That work would be valuable if important results had been obtained, but we doubt whether, lacking this research, any one existing engine would be worse. To say the least, this study and experiment has not been of a scientific nature. In addition, the Diesel engine was studied, likewise not a scientific or new phenomenon, and no tangible results were achieved, except possibly in the case of the spray research with solid injection.

The free flight researches gave valuable information concerning the maximum accelerations and maximum pressures occurring in maneuvers. Also some practical information regarding the ice hazard and similar subjects was obtained. Apparently the only fact demonstrated in the study of the supercharger was that such a device increases the available horsepower, and that was known before. This can hardly be considered an outstanding success. On the whole it can, nevertheless, be said that the free flight research has been the most beneficial conducted by the N.A.C.A. At the same time it can be said that no free flight test has been a scientific test nor dealt with investigation of fundamental phenomena of nature. Test flights conducted over a period of ten years, with the aid of good instruments, can not but yield some valuable information, especially at a time when flying is new, but they are not likely to advance fundamental science.

The class of wind tunnel research should correspond most to the description “scientific.” Therefore, we ought to consider it in more detail in order to find there at least some of the promised scientific work. In this category the pressure distribution work of the N.A.C.A. showed only that wings should be rounded at the tips, which was known before, and which could be and was demonstrated in the course of natural industrial development. Merely to make pressure distribution measurements is not scientific. We are sometimes inclined to believe that it would be better for wind tunnel research if it were more difficult to do this kind of work; an abundance of patience is necessary but not much creative mental effort. The results are not of great practical value, because they are made under steady wind tunnel conditions, whereas the largest pressures occur under unsteady flight conditions. For this reason, the pressure measurements made in flight tests are much more valuable.

In addition there have been wind tunnel tests on complete airplane models, and drag measurements on airplanes and airplane parts. This research can not yield new results of general value, and is therefore outside the scientific research the N.A.C.A. is charged to undertake.

During all of the ten years, much time and effort has been spent on a series of tests undertaken to standardize wind tunnels throughout the world. This work showed merely that different wind tunnels give slightly different results and that these differences can not be predictedæwhich facts we knew before. Tests referring to wind tunnel technique are secondary anyhow. Someone has claimed that all wind tunnels could continue to do research even if no airplanes existed. They could, but we would not accept such work as useful unless science had been advanced.

Propellers have been investigated and found to possess a certain thrust and torque. Interesting, but again not scientific progress, not even technical progress.

We come at last to the research having most of the scientific element in itæthat dealing with the rotating cylinder. This stirred the imagination when the first tests were made and showed undreamed-of lifts. Right now, a very prominent manufacturer is making experiments along that principle. Unfortunately, the first tests along this line were not made by the N.A.C.A. On the contrary, the N.A.C.A. refused a suggestion in 1921 to measure this phenomenon. Several years later, it did repeat measurements made abroad without adding one new thought or result.

The Autogiro is the most painful subject in connection with the N.A.C.A. research. The N.A.C.A. had the priority in this new and perhaps most important invention of recent years. Autogiro models were investigated in 1922. It is hard to believe, but nevertheless true, that these tests were never published in a Technical Report. Five years later, after the practical value of the Autogiro had been demonstrated abroad, the results were published in mimeographed form, giving evidence of an opportunity to contribute to scientific progress which was woefully neglected.

In the investigation of auto-rotation of wings, it was demonstrated that, in a wind tunnel, wings can be made to rotate like windmills. This has hardly any bearing on or connection with the spinning of airplanes. It can hardly be called a research, but rather only making pretense of research. No airplane designer gives any attention to such tests, and science rejects them entirely.

A study of boundary layer control is on the program of the N.A.C.A., according to its statement, but no report has appeared in print on the results and we have not been apprised of any progress. This should be the most important subject of the work, but in fact hardly anything seems to have been done except the repetition of some work abroad.

Finally there is the wing section research. This is the only line in which the N.A.C.A. has contributed to aeronautics by way of its own experimental research. The M wing sections were developed by the N.A.C.A., in its wind tunnel, and at least two of them have been adopted in practice, being considered superior to older ones. Accordingly, the N.A.C.A. report for 1924 (page 50) says: "satisfactory progress has been made in the science of aerodynamics during the past year. . . . One important result of wind tunnel investigations has been the development of a number of remarkably efficient wing sections of adequate thickness for economical structures. *It is desirable that this development continue substantially along the present course*."

This was indeed desirable for the investigation was intended only as the first and preliminary step of a more systematic research. Much better wing sections were expected from the next series of tests, as the report indicates (page 59), "It is believed that a fruitful field for research lies in the determination of these sections which have a stable flow with good aerodynamic properties." In the interim, however, there has been no evidence of further work and the M section research, so admirably begun, has never been, continued.

We do not believe that we have overlooked a major research item of the N.A.C.A.: we are certain we have not overlooked a successful one. The N.A.C.A. was officially awarded the congressional medal for its low drag cowling. Apparently, even the friends of the N.A.C.A. consider this the most outstanding of the research projects completed. Yet, in the true sense, this cowling work was a development rather than an original work. Moreover, because it had reference to special airplanes and engines, it can not be regarded as having general value. Therefore, it can not be considered scientific work. It does not involve the study of new and fundamental phenomena of nature. Its doubtful value in this connection is clearly contrasted with the research of similar aimthough along entirely different linescarried on at the same time in England. The Townend Ring is definitely superior to the N.A.C.A. cowling. It is the outcome of strictly scientific research carried on with scientific spirit, involving the systematic exploration of new and fundamental phenomena, and incurring relatively little expense. It represents more brain and less expenditure than for the N.A.C.A. cowling research.

The results of the N.A.C.A. experimental research are not, in our opinion, an adequate return for the money spent. There is hardly one research project of scientific value, and only a few of technical value. There is an enormous gap between the principles of research laid down and those applied.

It can not be denied that there is keen feeling of disappointment throughout the industry about the outcome of the N.A.C.A. research. Every year the industry gathers at Langley Field to acquaint itself with the latest results of the research going on, but every year it is presented with stone rather than with bread. New laboratories and instruments are exhibited but no new results worth speaking of.

Responsibility for the N.A.C.A.'s failure to make substantial contributions to aeronautic science does not rest entirely on the organization itself. General supervision of the research undertaken is in the hands of committees, which are composed of members serving without compensation. Under these circumstances, they can not give much time to this research; and after all, they are not to be blamed for its shortcomings. Scientific knowledge can not he amassed by a committee any more than an opera can be written by a committee. The capable and patriotic members of the several research committees feel that they can give best service by keeping their hands off, by assisting with advice and suggestion only, without showing too much initiative.

The real responsibility would seem to rest, therefore, upon the director of research. Is he one who knows "the problems and the methods used for their solution"? We fear not. But then it must be remembered that this director exercises the direction of the research from a distance of 200 miles, and as an auxiliary duty only. His primary duty, is that of an executive. In the first place he must practice diplomacy and exercise organizing talent: only secondarily need he exhibit any scientific spirit. Most of his direction of the research is done over the long-distance wire, or on occasional visits. These facts, together with his normal duties which stand in distinct contrast to the duty of research supervision, and require entirely different capabilities, make it plausible to believe that the director of research is not in a position properly to discharge his duty. As one important reform that will improve the present conditions, we suggest that the Langley Field laboratory be separated entirely from the Washington political office of the N.A.C.A. and be put in charge of a capable research engineer who would be fully responsible for the research and for it only.

As it is, the true initiative must come from the local head of the laboratory, and from the heads of the single divisions. We expect most from the aerodynamic sections. It is now a fact that both positions, the head of the L.M.A.L. and of the aerodynamics division, have been occupied in recent years by men who are decidedly not research engineers at all. Neither of them has ever contributed anything to science, and neither of them expects to do so. They are mere routine engineers, and hardly that; they are mere bureaucrats, signing letters and unwrapping red tape.

This brings us to the question of the N.A.C.A. staff. Friends of the N.A.C.A. have claimed that the staff has suffered great losses because the industry has induced its best men to leave by offering them lucrative positions. This does not sound probable. In the first place, a capable research engineer does not leave his work if he has found favorable working conditions, and is progressing satisfactorily in his work. The fact that nearly all good research engineers have left the N.A.C.A. constitutes in itself a reproach to the management. From inside information we know that most engineers left of their own initiative, because they were dissatisfied with the management. They are now employed in industry, and most of them did not leave as friends of the Committee. During these ten years, the head of the laboratory at Langley Field has changed four times, and two and a half years is about the average time the engineers used to stay. There must be a reason for this state of flux in the personnel. Most of the research engineers are young graduates, and the few older men who have stayed with the organization are for the greatest part less capable than those who left. Jealousy and petty politics have always played too great a part in the activities at Langley Field. The spirit of research and scientific work was never really encouraged by the management. Nobody can carry on research work successfully if he is compelled to devote a great part of his time to fighting for the cooperation of others to which he has a right, and fighting off the aggressiveness of his colleagues. The failure of the National Advisory Committee for Aeronautics is the failure typical of so many public organizations. There is no effective check on what is accomplished. If the results of the N.A.C.A. could be computed according to their worth in dollars and cents, the Committee would long ago have been bankrupt. But it is not a money-making organization; it is a money-spending organization. That leaves much energy free, and unfortunately the conditions in such a case are favorable to the survival of those most unsuitable for carrying on scientific research.

The activity of the N.A.C.A. has become a mere building of new laboratories without distinct ideas of what to do with them after they are built, and it has become a mere weighing and measuring of less value than the weighing of a grocery clerk. No concerted efforts are made to advance science; no efforts are made to apply the results of the tests to any logical system, to digest them, and to interpret their significance in the sum of general knowledge. The truth is that the tests can not be interpreted that way because the program has not been guided by scientific reasoning. Weighing for weighing's sake is not scientific research, but at the best a kind of indoor golf.

We urge that radical changes in the management be made with the view to improving the conditions to the end that real and honest talent be attracted to the N.A.C.A. Only then will there be some prospect of an intelligent use of the research equipment and a reasonable return on the money spent.

Let's devote a period of thought to wondering if these large appropriations devoted to the N.A.C.A. have served, are serving, or will serve the industry.

Let's hope that Congress, yes, and even the President of the United States, will give consideration to the self-same subject.

Let us spend money, certainly—no detail of aviation should be stinted but let us have men in charge of its expenditure who will see to it that the money which we spend shall count.

Document 2-25(b), Frank A. Tichenor, "The N.A.C.A. Counters," 1931.

AIR—HOT AND OTHERWISE

The N.A.C.A. Counters

Frank A. Tichenor

IN these columns in December, I reviewed the conditions prevailing in the National Advisory Committee for Aeronautics which prevent it from functioning in a manner useful to the best interests of the industry it purports to serve. The discussion has disclosed evidence of widespread interest in this question. It becomes ever more apparent that the points touched on have for some time been a subject of concern to many. The importance of a wise and honest expenditure of public funds appropriated specifically for scientific research and not for a cheap substitute for it, is generally recognized. The conditions which urged us to stress this vital phase of aeronautical development have found sympathetic response among all who have at heart the good of aviation and the country.

The comments of those who concur in our contentions contain little that has not already been said in these columns. As a matter of fact, little more can be said concerning these conditions which have lasted so long and about which informed public opinion fairly agrees. It was with interest and curiosity, however, that we awaited replies of defenders of the present N.A.C.A. management. We had hoped that some bright spots might be brought to shine on the otherwise dark picture, that perhaps things are not quite as bad as they appeared to us. Unfortunately we were disappointed in this hope. Although the management of the N.A.C.A. has an advocate who apparently tries to defend its policy, actually he only concurs in the broad picture which we painted. Indeed, we fear the picture is even darker after his defense than it would have been without it. We are disposed to believe now that conditions are even worse than we at first suspected, and that the trouble is not only lack of ability, but also absence of an honest attempt to accomplish the laudable task which the N.A.C.A., through its presiding body, has assigned itself.

This seems to be another case in which a feeble attempt to defend a weak cause serves only to render its defects more vulnerable. It almost looks as though the defender of the N.A.C.A. management in his own heart agrees with us; and although he finds it expedient to depreciate our criticism, he writes as though he himself would like to see reform effected. He does not call attention to one successful research, nor one scientific advancement which can be credited to the N.A.C.A., nor even one technical advance which we may have overlooked. Nor does he suggest that such advances can be expected in the near future. Indeed, there is not even the assurance that they will eventually be forthcoming or that anything is being done to hasten that day. Our principal criticism, the absence of scientific research, is tacitly admitted. Such research, he contends, is the proper sphere of universities, not of the N.A.C.A.

Now, we have not, merely as the result of our own judgment, specified scientific research as the task of the N.A.C.A.; we quoted this as the N.A.C.A.'s task from the Committee's own annual reports. The defender of the N.A.C.A. can not logically ignore this point altogether, as he does, for it is the most important consideration, the keynote of the N.A.C.A.'s shortcomings. This is not a question of opinion only: rather, it is far more a question of keeping faith, of loyalty to duties defined by the supervising body of the N.A.C.A. The policy of conducting scientific research was adopted ten years ago by the presiding Nominee Committee, made up of the foremost experts of the country. In all annual reports since then, it has been recorded as the accepted policy of this body. It has been pleaded for in hearings before congressional committees. It has formed the basis for public appropriations. Does the defender of the N.A.C.A. mean to imply that there is one policy for obtaining appropriations and for general advertising and publicity purposes and quite another one for the actual service and activity within the walls of the N.A.C.A.?

We are referring chiefly to a reply to our article expounded under the sponsorship (and probably under the personal authorship) of one who is himself a member of the supervising body of the N.A.C.A. Having been appointed by the President to serve as a member of the group prescribing the policy of the N.A.C.A., he certainly can not plead ignorance of that policy. His words would indicate that he is opposed to the policy of scientific research, that he prefers something easier and cheaper. If that is so, he can well argue that everyone has a right to an opinion of his own. It is, however, one thing to advocate a change of policy and quite another to advocate disobedience to a policy established by authority, laid before Congress and the nation, for the execution of which policy appropriations have been made. No ordinary citizen should advocate disobedience to rules established by authority;

he ought rather to satisfy himself by urging and pleading for reforms. How much more shocking is it that a member of the Committee should defend the management's activities when they are in direct contrast to the policies promulgated by the presiding body! Our opponent's failure to acknowledge the authorized policy of the Committee has greatly intensified our conviction that something is wrong with the N.A.C.A.

We expected the defender of the N.A.C.A. to give us evidence (or at least to try to give us evidence) that we had erred, and that the N.A.C.A. after all is true to its avowed ideals. Instead he denounces these ideals, admitting thereby the justification of our criticism. He joins us so far as the facts are concerned; he concedes the point that the N.A.C.A. has made no "contribution to pure science" and merely tries to make that failure appear insignificant.

This thing is not insignificant, however. If money is appropriated for scientific research, can we consider it of no consequence that those funds are spent for something else? The presiding body of the N.A.C.A., Congress, the industry—all of us expect the N.A.C.A. to be a bright torch of science; we have a right to expect that. We know that a great national organization is needed as a guiding light for a prosperous aviation industry, not as a satellite which merely reflects faintly the illumination of more brilliant bodies. This point is not one for which compromise can suffice. Either there is scientific research or there is not, and authority has decreed that the N.A.C.A. should conduct scientific research.

Such is the defense the management of the N.A.C.A. summons, a defense worse than none. It begins with mild mocking on the degree of courage necessary for attacking scientists—as if any scientists were attacked. The absence of scientists on the staff of the N.A.C.A. was attacked, and the politicians who occupy positions rightfully belonging to scientists were attacked. Certainly it takes more courage to defend their presence there than to criticize the absence of scientists. It takes more courage to disclaim the policy laid down by authority of office and public consent than to plead for it. It takes more courage to advocate a use of public funds other than that for which they were appropriated. It takes more courage to sacrifice the interest of the industry and to advocate administrative measures foreign to American spirit than to ask for a clean policy. Pleading weakness and inability to defend oneself in a case like this, where the defense would be easy if the criticism were unjust, is inadvertently an admission of guilt. Where are the advances of science made by the N.A.C.A. at Langley Field? Why not enumerate them, if there are any, instead of lapsing into disputes about personal courage?

Let us review, in short, what results the champion of the N.A.C.A. is able to point out. As the most conspicuous achievement, he mentions the Townend Ring and the fuselage cowling, admitting that the N.A.C.A. did not invent the latter. With the evolution of the cowling, the N.A.C.A. had nothing whatsoever to do. It is nevertheless seriously suggested that neither cowling nor ring would have been adopted by the industry had it not been for the N.A.C.A. The industry is alleged to be so timid that the information about improvements available is not sufficient to induce it to adopt them; the industry needs the guiding hand of the N.A.C.A.; the industry does not trust and has no confidence in its own speed tests made by its own pilots. The implication is that, instead, it waits until the N.A.C.A. measures in pounds and ounces the diminishment of the drag in consequence of some improvement and then computes the increase in the speed. The industry, it is seriously alleged, has more confidence in such computed speed gain than in a speed gain directly observed. How grotesque! We really have cause to admire the courage of one who advances such opinions. He dignifies this business of measuring by calling it "determination of relative merit," which must have something to do with the theory of relativity. At least we must confess that we fail to understand it fully.

As further useful research of the N.A.C.A., our attention is called to the pressure measurements of floats. There is certainly some use in such measuring. A tailor taking down the measurements of his customer also does something useful. Measuring pressures is but a secondary duty of the N.A.C.A.; its chief duty is to measure pressures (if any) in such a connection that scientific theory is advanced thereby. This was not done in the case mentioned.

As a third citation of successful activity of the N.A.C.A., another pressure measurement is mentioned, one which likewise failed to advance science—the pressure measurement over the dirigible *Los Angeles*. Although genuine contribution to science is in fact not claimed for this work, it is contended that these measurements have proved indispensible for practical purposes, and that the new Goodyear Navy airships will be stronger and lighter in consequence of them. We doubt whether the design staff or the Goodyear-Zeppelin Company will agree. The strength and weight of a dirigible depend upon structural improvements and aerodynamic progress. The air force loadings assumed for the structure of a dirigible are based chiefly on theoretical aerodynamic developments (to which no research at Langley Field has ever contributed), together with one numerical factor, based on operation experience. It is a curious fact that the Goodyear-Zeppelin Company, after its experience with more than 120 airships, arrived at a different factor than the U.S. Navy with its experience with two airships. A commendable research project would have been an attempt to clear up that discrepancy. Instead, the N.A.C.A. made some pressure measurements, leaving the whole question as it was before. No harm done; dirigibles will continue to be improved without the assistance of the N.A.C.A. But, after all, why all the measurements?

It is obvious, therefore, that our arguments have been strengthened by those of the advocate of the N.A.C.A. We stand in line with the supervising body of the N.A.C.A. and insist with it that scientific research is the proper and chief domain of N.A.C.A. activity. We advocate that capable scientists be encouraged to join, not to leave, the N.A.C.A. staff. We heartily agree with the able and patriotic chairman of the N.A.C.A. that the N.A.C.A., to borrow his own words, "undertake investigations directed by men who know the problems and the means of their solution" and that such changes be effected as are necessary to bring this about. We insist that is not the function of the N.A.C.A. to gape and squint at what others are doing, like a loafer standing on the street, and do at best a little measuring to see whether the work was done right, but that it should be the purpose of the N.A.C.A. to do things itself. Aeronautics has not yet reached its goal. The final shape of airplanes will eventually be quite different from what we have now. We want that development hastened. We want a critical and scientific survey, an exploration of all known possibilities. It may be possible (it probably is possible) to increase the specific lift to ten times what we have now, and we want a central institution of research to give us light on that. It may be possible to reduce the specific drag to one-tenth what we have now; the theory of air motion producing drag is still entirely in the dark. Friction of air, as such, does not account for more than one-twentieth of actual drag. We want to have some light on that too. We want knowledge concerning boundary control, concerning the effect of rotating cylinders, of vibrating surfaces, of lubrication, of autogiros, of Flettner cylinder, of jet action, of shooting action, of sound wave action, and of chemical action. Indeed the possibilities are without limit. We want a national agency to explore these unexplored regions, and to do so with scientific spirit, systematic thought, and honest endeavor. We are not satisfied with useless pressure measurements and with the building of wind tunnels which will never be really usefully employed. Build small laboratories and do big things in them; not the other way. Only then will the nation attain high rank in world aviation.

Our conviction as to the failure of the N.A.C.A. is not an original idea. The conditions existing have been recognized by none other than General John J. Pershing, who states in the third chapter of the story of his war experiences, appearing in the New York Times, Wednesday, January 14, "…we had some fifty-five training planes in various conditions of usefulness—all entirely without war equipment. Of these planes it is amusing now to recall that the National Advisory Committee for Aeronautics, which had been conducting an alleged scientific study of the problem of flight. . . . " General Pershing was in a position to know and no one can question his sincerity.

Document 2-25(c), Edward P. Warner, "Speaking of Research," 1931.

Nothing is easier, and nothing demands less courage, than attacking scientific work for the benefit of a non-scientific audience. The general public, and even most of us who are engaged in applying science to industry, have little understanding of scientific theories or of laboratory work, and it is immensely consoling to feel that people who pretended to be so much wiser than ourselves, and claimed to "understand all that stuff," were wrong all the time.

To assail the scientist is the safest of pursuits, for he has neither the inclination nor the equipment for rebuttal. Reasoned and orderly discussion in reply to a vindictive assault on the caliber of work done in a laboratory is quite impossible before an audience that has no background or experience of its own to give it an understanding of the nature of the controversy.

The National Advisory Committee for Aeronautics has had its share of vilification. Perhaps no answer, and not even passing comment, is necessary. The steady growth of interest in the industry's pilgrimages to Langley Field, annual since 1926, and the lively discussions that take place there, give evidence of the esteem in which manufacturers and operators hold the N.A.C.A. Nevertheless it is worth recalling how much influence that body's activities have already had on American aeronautical development. Let us take an example.

The venturi and ring cowlings are the most conspicuous of recent contributions to aerodynamic efficiency. They are the exclusive invention of no individual or group. Their genesis can be traced back at least to 1920. They have appeared in various forms here and abroad,—but the most important single step in practical application was the direct quantitative proof of the reduction in resistance they permitted. Without the tests that were made at Langley Field, the new forms of cowl might have been suggested and argued and perhaps tried in a small way, but their general acceptance would have been a matter of many years. Even in England, the original home of the ring, its adoption as a standard feature of airplane design has been enormously accelerated by the American laboratory work, which has given generalized in place of specialized results, and definite measurements of resistance in place of speculations based on a measurement of maximum speed. The most important function of the Committee, in short, was not to invent a new type of cowling, but to determine the relative merits of all available types and to make the determination on a scale which no other laboratory in the world was prepared to duplicate.

It is a good general rule that there are three types of research work, and they are adapted to three different types of organization. First, and in the very long run most important, is contribution to pure science and underlying theory, perhaps most generally the product of members of the staffs of educational institutions or of laboratories endowed by private capital or by especially farsighted industries. At the other extreme is the study directed to solve a particular and specialized problem of particular design, or to lead directly to the invention of a new proprietary device, and that is the proper sphere of the research department of a corporation.

It is a sphere, be it said in passing, not as yet sufficiently exploited by American airplane manufacturers, for in the face of the great affluence of the industry in 1928 and 1929 the absence of research departments in most airplane factories remained a proper ground for surprised comment by foreign visitors and for shame-faced acknowledgment by American engineers.

Between the two extremes there is a third class of work, the conduct of "practical" studies, general but immediate in their application. That is the particularly fitting task for a government laboratory.

To produce a theory of heat flow which will make it possible to calculate the cooling characteristics of an air-cooled cylinder is the function of an individual mathematical physicist, and it is on a college faculty that he will most often be found. To determine the cause of repeated spark-plug failure in the XYZ engine is the responsibility of the XYZ Engine Company. But to find out by actual measurement on a group of typical engines how temperatures are typically distributed in cylinders and how they are affected by changing conditions of flight, information that can be applied to the XYZ engine or any other, is work most profitably to be undertaken for the general good, and for general dissemination of the results, by such a body as the Advisory Committee.

The determination of design data may not be inspiring or spectacular. It does not appeal to the imagination as does an invention, but it is extraordinarily important. Without it, design does not progress.

The structural design of every airplane built in the United States today is dependent to some degree upon the N.A.C.A. work on pressure distribution and air loads in flight. The structure of seaplanes has gained a rational foundation for the first time through the studies made on pressure distribution on float bottoms. The airship being built at Akron will be a stronger and a lighter craft than would be possible without the N.A.C.A. measurements on the Los Angeles. Examples can be multiplied without number.

Science is a term that covers a multitude of widely different things. Not the least important among them is the skillful devising of means for accumulating data upon which the designers of engineering material may lean.

The Advisory Committee has acquired the material and the personnel to do that work for American aviation, and has been doing it. Aeronautical engineers in Europe are quick to express their envy of their American colleagues' good fortune in having at their disposal an institution of such resources.

Document 2-26

Joseph S. Ames and Smith J. DeFrance, remarks at the dedication of the NACA Full-Scale Wind Tunnel in "Report of Proceedings of Sixth Annual Aircraft Engineering Research Conference," Langley Field, Virginia, 27 May 1931, NACA, pp. 20–26, Historical Archives, NASA Langley.

As the NACA's research capabilities, physical plant, and budget grew, the committee clearly understood that continued public, and especially industry, support would be crucial. The committee also knew that it needed a way for the emerging civil aviation industry to bring its problems to the attention of the NACA. In a brilliant stroke, Director of Research George Lewis and Executive Secretary John Victory devised a conference where civilian and military leaders would be brought to Langley for a first-hand look at the facility and an opportunity to talk directly with NACA people. The first conference, with 38 invited guests in 1926, proved popular, and it quickly grew into the Annual Aircraft Engineering Conference. By 1931, the conference had become a well-staged and -attended event hosting over a hundred guests—including a number of prominent aviation journalists—to ensure publicity, and the NACA used the event to show off its latest technology. In 1931, the latest technology was the Full-Scale Tunnel, a thirty- by sixty-foot monster of a wind tunnel, and the committee made the dedication of the FST the highlight of that year's conference. This excerpt from the conference report outlines the carefully scripted dedication program. The dedication opened with a brief review of the history of aeronautical research by Joseph Ames, followed by a report by tunnel chief engineer Smith J. DeFrance, and culminated with a live demonstration of the new tunnel.

While the actual number of research projects that resulted from industry inputs at these conferences was small—less than two dozen in all—the conferences succeeded in projecting the NACA's best image to the aviation industry and the public, and they helped ensure essential congressional support.

Document 2-26, Joseph S. Ames and Smith J. DeFrance, remarks at the dedication of the NACA Full-Scale Wind Tunnel in "Report of Proceedings of Sixth Annual Aircraft Engineering Research Conference," 1931.

DEDICATION OF FULL-SCALE WIND TUNNEL.

The members of the conference then proceeded to the new full-scale wind tunnel, where a Vought Corsair airplane was mounted on the balance in position for test.

DR. JOSEPH S. AMES.

The tunnel was dedicated by Dr. Ames, who presented the following statement:

Before putting this new wind tunnel into operation I would like to say a few words with regard to the history of wind tunnels and something with regard to the design and study of equipment of this type. The problem is to find the resistance offered to the passage of a solid body through the air. Early experiments along this line were attempts to study the effect of wind on the human body by determining how far a man could jump with the wind and how far against the wind.

The beginning of the science of aerodynamics probably dates back to the year 1661, when Hooke read a paper before the Royal Society of London, on the resistance of the air. The material in this paper was based on experiments Hooke had conducted by throwing different shaped bodies horizontally from the top of a tower and observing the time they remained in the air before striking the ground. Similar experiments were made by Sir Isaac Newton in 1710, with spherical bodies. In 1746, Benjamin Robins, an Englishman and a distinguished mathematician, developed an early form of whirling table and accumulated considerable data on air resistance and on the motion of bodies projected into the air.

Although these investigations were concerned chiefly with ballistics and had little bearing on the aerodynamics of flight, they led to the statement of the pressure velocity law by Charles Huttin about 1790, that pressure varies as the square of the velocity, and to the importance of aspect ratio.

The whirling table, or whirling arm, and straight-away towing arrangements, were used for the investigation of problems in aerodynamics until the beginning of the twentieth century. These devices were supplemented by the actual flight of models, man-carrying gliders, and in some cases engine-driven airplanes. The era of gliding, from the time of Lilienthal, did much to lay the foundation of flight and the principles of stability.

Langley, early in his career, conducted research on the sustentation of bodies by inclined planes with a view to determining the fundamental data as to lift and drag of airfoils and the probable efficiency of air propellers.

About 1890, Sir Hiram Maxim constructed a wind tunnel with a three-foot square throat.

In 1901, Orville and Wilbur Wright set themselves to solve various problems of flight and started a lengthy series of experiments to check previous data on wind resistance and lift of curved surfaces, besides problems on lateral control. They built a wind tunnel at their home in Dayton, which had a 16-inch square throat and was 6 feet long. In this tunnel they measured the lift and drag of over 200 miniature wings. In the course of these tests they produced comparative results on the lift of square and oblong surfaces, with the result that they rediscovered the importance of aspect ratio.

It was not until 1909 and 1910 that organized research was undertaken, and wind tunnels were constructed at the National Physical Laboratory in England, at the Eiffel Laboratory in France, and at the Aeronautical Research Institute at Göttingen.

In the United States, wind tunnels were constructed at the Washington Navy Yard, at the Massachusetts Institute of Technology, and at the Bureau of Standards, and the first wind tunnel constructed by the National Advisory Committee for Aeronautics was completed in 1919.

In 1927 the Committee placed in operation the present propeller research wind tunnel. This wind tunnel was designed and constructed largely for the purpose of investigating the characteristics of full-sized airplane propellers. A large number of propeller investigations have been completed in this wind tunnel, but the major portion of the operating time has been taken up in the study of other than propeller problems. The investigation of large wing models, the study of the cowling and cooling of engines, the study of engine-nacelle-wing arrangements, the investigation of different forms of airship models, and many other projects have so filled this program that in 1928 the Committee submitted to the Director of the Bureau of the Budget a request for authority to expend $5,000 for the development and procurement of a design of a wind tunnel suitable for research on full-sized airplanes. With the approval of the Bureau of the Budget, authority was obtained from Congress for the expenditure of $5,000 for the study and design of a full-scale wind tunnel on May 16, 1928.

Preliminary designs and estimates were prepared and submitted to the Bureau of the Budget, and by act approved February 20, 1929, the Congress provided an appropriation to extend over a period of two years the construction of a full-scale wind tunnel.

The contract was awarded for the construction of this wind tunnel on February 12, 1930, and we are here today to place in operation this important equipment. With the completion of this equipment we now have available a means for actually studying a full-sized airplane under flight conditions.

The ideal method of investigating the stability and control characteristics of an airplane would be to place on the airplane in flight means of measuring the lift, drag, and moment characteristics. This, of course, would be very difficult to accomplish so as to obtain accurate measurements. In this wind tunnel we have practically done the same thing by placing the airplane on a balance capable of measuring all the changes, and providing the air stream flowing past the airplane.

The completion of this wind tunnel opens up a new vista of important problems, the solution of which I am confident will mean much toward increasing the safety and efficiency of aircraft. The Committee has received many suggestions for research problems from the military services and from aeronautical engineers, which will provide a research program that will keep this piece of equipment in continuous operation for a long period of time.

The Executive Committee in 1928 authorized the preparation of a preliminary design, and Mr. Smith J. DeFrance, of our technical staff, was placed in charge of this project. Mr. DeFrance, with his assistants and other members of our technical staff, has been responsible for the design and construction of the completed equipment. I wish to add at this point the appreciation of the members of the Committee to the staff of the Committee and to Mr. DeFrance for the excellent manner in which they have carried through this enterprise.

Mr. SMITH J. DeFRANCE.

The Chairman then called on Mr. DeFrance, who described the operation of the full-scale wind tunnel, referring, as he spoke, to diagrams of the plan of the tunnel and balance. His remarks were as follows:

After what Dr. Ames has said I am sure that you are all familiar with the principle of the wind tunnel. You have seen small wind tunnels in operation. This tunnel is in many respects similar to a small tunnel but of course much larger. The cross-sectional area of the throat is five times that of the next largest wind tunnel in the world, the propeller research tunnel.

This is the first wind tunnel ever constructed for the purpose of testing complete full-sized airplanes, and as such it will fill a very important place in the field of aeronautics. Its principal use will be in the determination of the lift and drag characteristics of an airplane. Previously it has been necessary to do this from glide tests in flight, and sometimes the tests have been very lengthy because of inability to control test conditions. Here we will be able to control the test conditions, and to obtain the polar of an airplane in approximately one hour whereas it might take a month in flight.

In this tunnel we will be able to study control, especially control at low speeds and at high angles of attack; and the drag of air-cooled engines, and of water-cooled engines with radiators, under practically the same as flight conditions.

The dimensions of the tunnel are 30 by 60 feet at the throat, and, as may be seen from the chart, the tunnel is of the double-return flow type. The velocity of the air stream may be varied up to 115 miles per hour. This stream is produced by two 35 1/2-foot propellers, each directly connected to a 4,000-horsepower motor of the slip ring induction type. The speed of the motors may be varied between 75 and 300 r.p.m. in twenty-four equal steps.

The airplane is mounted on a six-component balance through tubular struts. The chart shows the arrangement of the balance and struts, which are at present streamlined by fairings. Eventually the fairings will be separately supported and merely serve as shields for the tubes, thereby reducing the support drag to a minimum. The tubes in turn are secured to a floating structural steel framework, which is connected by linkages to six recording scale heads. The lift is taken on two scales forward and one in the rear, the drag on one scale, and the side-wind force on two scales. From the lift readings it will be possible to compute the pitching and rolling moments, and from the side-wind readings the yawing moments.

Mounted on top of the floating frame is a turntable by means of which it will be possible to turn the airplane from 15 degrees left to 15 degrees right while the tunnel is in operation. We will also be able to vary the angle of attack from 5 degrees to +20 degrees, thereby taking the airplane through the stalled condition and making it possible to obtain data so difficult to obtain in flight.

The scales are equipped with electrical recording devices which are operated from a single control stand. The amounts of the forces are printed on cards from all scales simultaneously, thereby eliminating errors which may arise from readings. The cards are moved from time to time as readings are made.

Because of the amount of power required to operate the tunnel and the small capacity of the local power plant, we are compelled to take the power on off-peak load, or between midnight and 6:00 a.m. The amount of power permitted during the day is 750 kilowatts, which will give an air speed of 55 miles an hour. This afternoon we are operating at that speed. Before the tunnel is started, the pilot will climb aboard the airplane and after the air stream has been started he will start the airplane engine. Readings will be taken on the scales, and you will be notified by placards when the cards are moved and when the angle of attack and the angle of yaw of the airplane are changed.

The pilot will now go aboard the airplane. Dr. Ames, I ask you, as Chairman of the National Advisory Committee for Aeronautics, to dedicate this full-scale wind tunnel.

Dr. Ames.

This Committee started its work in 1915. I regard this moment as probably the most important moment in the history of the Committee, because it is to set in operation a piece of apparatus which promises to give in the shortest time the most important information desired in the development of aerodynamics; an instrument which is unique in the world, and which we owe to the ingenuity of our engineers and to a Congress and a Budget Committee who understood our problem and were willing to cooperate with us.

The pilot having climbed aboard the airplane, Dr. Ames pressed the button and propellers were rotated, starting the air stream. The pilot started the engine of the airplane and readings were taken on the balances, the members of the conference being notified by placards as to the variation of the attitude of the airplane with respect to the air stream.

Document 2-27

Minutes of the Second Technical Committee Meeting, United Aircraft and Transport Corporation, 5 December 1929, pp. 522–531, Boeing Company Archives, Seattle, Wash.

Once the merits of wind tunnel testing had become apparent, as they had by the late 1920s, American aircraft builders began to think seriously about constructing major new tunnels for their own research and development work. The Curtiss Aeroplane & Motor Company had been operating a private wind tunnel for two decades with some notable results, including designs for Schneider Cup racers, but most manufacturers were content with the data coming from the NACA and university laboratories. Aircraft building was a risky endeavor in 1920, and the fledgling firms engaged in it did well to merely survive. As the decade drew to a close, however, the future of aviation seemed much more assured, thanks to a combination of federal regulation, greater public acceptance and enthusiasm, and technological progress. The aircraft industry, especially the larger companies formed through consolidation, could also support greater investments in research facilities.

The United Aircraft and Transport Corporation established a technical committee made up of prominent individuals in aviation to identify technical trends and recommend actions for the company to take. At the committee's second meeting in December 1929, the minutes recorded a discussion about wind tunnels. These minutes provide unique insight into the thoughts of several influential people in the aeronautical community concerning the role of private versus government laboratories, the state of intercompany cooperation, and the overall understanding of wind tunnels. While no two situations can be exactly alike, these minutes, and especially the debates over the desired size and costs, reveal much about the decision process for governing and managing boards.

Document 2-27, minutes of the Second Technical Committee Meeting, United Aircraft and Transport Corporation, 1929.

CHAIRMAN MEAD: Another question which has been brought up a number of times was that in the group we have no aerodynamic facilities in which to deposit all the information we have gotten so far, and the next decision is whether with as much money as we have in aviation we can rely on the outside sources of information whenever we need such data.

I have no brief either way, but it just seems peculiar not to have some facilities of that kind in our own outfit, and I would like to hear what you think about that.

I have incidentally heard a good deal of criticism of having some work done outside because of the leaks which occur; everybody knows about it before you do.

MR. MONTEITH: Before you go into the aerodynamic research, I think that everybody in the group who is doing flight testing of production ought to be equipped with decent barograph and calibration instruments.

CHAIRMAN MEAD: Tomorrow, we want to have you, if you will, get your gang together on this flight testing and see if you can not come to some conclusions as to both how to do it and what equipment is required. Then we could get the various units so equipped, because that certainly is most important.

MR. SIKORSKY: I think that is very important, and I would even add the suggestion that maybe we can order some entirely identical instruments, because it will help if we can know the instruments; it will simplify the comparison of the test.

MR. McCARTHY: We bought a barograph recently which is the same as the Navy uses at Anta Costa. It was quite expensive, of course.

MR. SIKORSKY: As a central unit, it seems to me a simple method to install a method of correcting these instruments.

MR. CHATFIELD: The operation for calibrating an ordinary barograph is not very elaborate, ten or fifteen dollars will buy one. We use mercuro-chrome with a bell jar. Apparently the temperature areas are not important enough to worry about.

MR. McCARTHY: I think we ought to talk a little about wind tunnels before we go away. It seems to me that the United can well afford to buy one wind tunnel somewhere in the East for the use of our outfit.

MR. MONTEITH: Why the East?

MR. McCARTHY: Well, it would certainly have to be in the East or West to be of any use to anybody.

—You will have to get your own facilities out there. I don't see how we could have one wind tunnel to serve everyone.

But, for a fairly modest sum we can put up about an eight-foot tunnel that would serve all the Eastern units and probably excluding Stearman.

MR. SIKORSKY: I am very much in favor of it. I think it is simply a necessity for us to arrange our own research laboratory of such size as would give reasonable service. As was stated here, the very important fact of secrecy,—and we know that it is almost impossible to get it,—is sufficient.

Besides this, simply the service is sometimes very hard to get. The data is not so reliable and not comparable with each other, and I believe the wind tunnel will simply pay back its cost in one year or so to everyone of us, besides the special work for every unit the wind tunnel would do, and the general work everyone would be interested in.

Again, it is interesting both so to speak in a positive and a negative way. Today, for example, we asked the question about these new wheels; who knows accurately what the data is? With our own wind tunnel, we could test it correctly and have reliable information available.

The same thing holds true with the new shape of stresses which come out, the results would be the actual tests of such new refinements, ideas and so on which may come out. Because, probably one of the strongest things which United may have is to keep leading the industry, and to do it accurately, I believe a research laboratory would be of considerable value, it is certainly worth spending twenty or thirty thousand dollars on, or whatever it would cost.

CHAIRMAN MEAD: There is quite a variance in opinion as far as I can gather as to what kind of a wind tunnel it should be, and how large.

MR. McCARTHY: I don't think you want to go below eight feet.

MR. CHATFIELD: I think the central tunnel should be,—I favor going a little larger than eight feet, with the idea that possibly some of the individual plants may have smaller tunnels and would like a larger one in which to take those problems which can not very well be handled in a four or five foot tunnel.

CHAIRMAN MEAD: I think we want to interrupt Chat just a second—I realize that this gang around here is being looked upon to provide the equipment which will return a good profit on the investment in United, and therefore it is a much different picture than those of us individually faced before when we have all pinched the pennies here because we have had to. Now, by pooling,—everyone giving a bit to this project, we certainly can afford better equipment than we could ever think of having ourselves, and I don't think we ought to look at this thing in too niggardly fashion.

What we really should have is desirable equipment, or at least ask for it, and if the money is not forthcoming that is too bad and we will have to trim our sales accordingly.

I feel that if you want a wind tunnel, I certainly want it to be a good one whatever size that might be. I don't know anything about wind tunnels so I can't advise as to size.

MR. CHATFIELD: I would like to have Mr. Weick's opinion on that point.

MR. WEICK: Well, I think that size is more important than velocity for instance. You can put money into a wind tunnel in two ways, one is in size and the other is in power to obtain velocity, and while of course, you want to get as near to full scale results as you can in both cases, the size when you are dealing with all sorts of models is more important than just velocity, and the power goes up very quickly with increased velocity. I think that Mr. Sikorsky's point is worth emphasizing that one wind tunnel, if you are going to rely on wind tunnel work which apparently you are to some extent, one wind tunnel in which all of these various models can be tested in by uniform methods under uniform conditions would be greatly valuable, because as it is with some testing in one tunnel and some testing in another you can not prepare those models with any degree of exactness, you can't expect to be able to compare them.

CHAIRMAN MEAD: We can take for definite example, Boeing Company developed something here in the wind tunnel that might exist at the University of Washington. The result of that test might go readily to Hartford to be checked, or wherever this other tunnel would be, so that our data would all be alike.

MR. WEICK: That would be very well.

MR. MONTEITH: I am pessimistic about shipping wind tunnel models. We used to ship them to Atlantic, or M I T, and the Railroad Company used them like they were cord wood.

MR. CHATFIELD: We have had fairly good luck recently at M I T with models coming in.

CHAIRMAN MEAD: We have not answered the question of how big or how fast here.

MR. McCARTHY: You would not want to answer it in one gulp, would you?

MR. MONTEITH: It depends on how much money you have.

CHAIRMAN MEAD: Well, let's put down two or three operations here, and let's see then how much money we can get; we will go out and canvas the crowd.

MR. MONTEITH: I think you have only two operations. You have either the full sized wind tunnel, or a reasonably sized tunnel like eight or ten feet; eight feet preferably. I think the four foot tunnel is absolutely out of the question except for very minor tests.

CHAIRMAN MEAD: Your optional one is how big then? Would you say this big fellow?

MR. MONTEITH: It is twenty feet like NACA.

MR. WEICK: That is very good because you can put all full scale bodies into it, and propellers and so forth, and the only thing you can't get in full scale is the wings.

CHAIRMAN MEAD: How low speed in a twenty-foot tunnel is going to be any earthly good?

MR. MONTEITH: Sixty miles an hour anyhow.

MR. WEICK: You ought to have at least I would say eighty. You see the tunnel there has only one hundred ten as an absolute maximum at the present time, and one hundred is what we call top speed for testing, and that was quite satisfactory for almost all conditions.

CHAIRMAN MEAD: Can I tell again what is the top limit on this thing?

MR. MONTEITH: I don't think you should go above one hundred.

MR. McCARTHY: What did it cost to put in that unit down there?

MR. WEICK: It is hard to say exactly, because the power units were put in by the Navy with a couple old submarines.

MR. McCARTHY: I think the eight or ten foot tunnel is about the limit.

MR. WEICK: The only thing I can say about the ten-foot tunnel is what someone here said the other day about the cost of the Berliner-Joyce tunnel. They have one which cost $37,000.00.

MR. CHATFIELD: California Tech's actually cost $60,000.00.

CHAIRMAN MEAD: And what velocity in a ten-foot tunnel?

MR. WEICK: I would say you want one hundred miles an hour.

CHAIRMAN MEAD: Well, now, that is the way to go at it and we can ask them for what we feel we need. Is there any use of having an intermediate size here? That is, the cost of these things seem to go up as the sixth power.

MR. MONTEITH: No use going beyond ten feet if you can't go to the full size.

CHAIRMAN MEAD: Well, isn't NACA apt to go at things in rather an expensive manner? As long as you are out of it, Weick, you can perhaps feel this is not criticizing, and we could not perhaps build a big tunnel for much less expense. As an example, Cline's tunnel down here, he admits for certain reasons that it is very expensive and apparently could be well cut down to $30,000.00 or $35,000.00.

Do you think there is any chance of the NACA's being very elaborate,—very much more than would be necessary?

MR. WEICK: I don't think the NACA tunnel could be classed elaborate in any sense of the word. It is built of only a single thickness of board on a steel framework for the walls, and it was sort of a factory job in construction all the way through with no trimmings whatever.

CHAIRMAN MEAD: I would think from what little I could see that the cost is in the machinery and not in the tunnel itself.

MR. McCARTHY: They got the machinery for nothing.

MR. MONTEITH: How much power have they there?

MR. WEICK: 2000 horsepower is all.

CHAIRMAN MEAD: Now, wouldn't it be worthwhile, we seem to be interested in wind tunnels, to make Chat the dog again and have him go around again and see or get information on what it really might cost to build these tunnels based on actual cost of other places and examination of NACA's, and discussion with them as to production and costs, and so on, and then we can put up a figure which looks reasonable and let the Executive Committee decide what they want to do about it.

MR. WEICK: I think that is a good idea.

Incidentally, they are now building another tunnel at NACA which would give better information than our tunnel, but you see when you double the linear dimension of a tunnel, which is done in that one, then you go up as a cube in volume and the price of your building goes up to beat the band.

MR. McCARTHY: That type of tunnel is not usable for a lot of things. The small tunnel is too.

CHAIRMAN MEAD: We might find a grandpa to give us these things, who knows?

MR. SIKORSKY: I believe a ten-foot tunnel would do all right, because in a big tunnel simply the speed with which you can make big tests and actually put models in,—I think it may be a little too big.

CHAIRMAN MEAD: Of course, we could do this, we could start off in a modest fashion and take the ten-foot, then if we kept growing and there was any need for a bigger one and we found it was desirable we could build it perhaps.

MR. WEICK: I would certainly recommend starting with a ten-foot tunnel.

CHAIRMAN MEAD: What else in the way of equipment do we have to have,—or is the desirable equipment to have, is the better way perhaps to put it?

Here is one wind tunnel—

MR. CHATFIELD: (Interrupting) There is a point in the operation, I think a great many wind tunnels are limited in their usefulness, not in their ability to conduct the test, but in their ability to get them written up afterwards.

To get the full use of a tunnel, I think it ought to have a larger personnel than many wind tunnels have so as not to have the delay in the making of the tests and the reporting of the results.

MR. McCARTHY: I think that is a secondary question. If you get the Executive Committee to approve of the construction of a wind tunnel, you merely go out and hire people to run it, that follows naturally.

Document 2-28(a–b)

(a) A. L. Klein, "The Wind-Tunnel as an Engineering Instrument," *S. A. E. Journal* 27, No. 1 (July 1930): 8–90.

(b) A. L. Klein, letter to V. E. Clark, 14 August 1934, Klein Collection, Folder 3.2, California Institute of Technology Archives, Pasadena, California.

At a Los Angeles meeting of the Society of Automotive Engineers (a leading professional society for aeronautical as well as automotive engineers) in 1930, A. L. Klein, an aerodynamics professor at Caltech, presented "The Wind-Tunnel as an Engineering Instrument," which briefly discussed some of the ways wind tunnels could be used, with particular emphasis on research to enable high-speed planes to land on short fields. The paper itself is mildly interesting, but the discussion that followed—moderated by former Langley Laboratory Engineer-in-Charge Leigh M. Griffith—ventured into several contemporary issues concerning wind tunnel design and operation. The discussion concerning the problems with small, high-speed electric motors with model propellers explained part of the rationale for building the PRT and FST at Langley, and other comments, primarily by Griffith, noted both problems and successes with the VDT. Considering his background with the NACA, two of his remarks are especially interesting. Griffith opened the discussion with, "The wind-tunnel is more or less of a mystery to many who are otherwise well versed in aeronautics. I have personally had some experience with it, and it is still a mystery to me." His ensuing comments belie that self-effacing remark, and his final comment at the end of the discussion sums up his belief that "It is only a question of time, I think, when we shall be able to design aircraft upon the basis of tunnel tests and not miss the computed performance on the full-scale machine by more than 2 or 3 per cent." Time would prove him right.

While Klein's 1930 S. A. E. paper shows his interest and mature understanding of wind tunnels and research techniques, a letter he wrote to Virginius E. Clark in 1934 went into greater detail concerning many of the practical considerations for wind tunnel work. Klein's letter, the result of years of experience with Caltech's tunnel, outlined a careful procedure for testing new aircraft designs and included an itemized estimate of the costs associated with wind tunnel testing. The dollar amounts appear miniscule today, but at a time when an engineer might earn $15 per week, spending $800 to $1,200 on a test series was not something to be taken lightly, and the planning of test programs grew increasingly complex to ensure the greatest possible benefits.

Document 2-28(a), A. L. Klein, "The Wind-Tunnel as an Engineering Instrument," 1930.

Of the many problems that arise in the design of an airplane, those in connection with the wings can be most easily investigated in the wind-tunnel. The determination of the mean aerodynamic chord of an unorthodox wing cellule is one of the most obvious types of wind-tunnel problems. It is highly desirable that the wing cellule of the airplane be investigated independently, as only by determining the polars of the wing cellule alone and then repeating the measurements with the fuselage, nacelles and other parts in place, can the interference between them be measured. By following this procedure and then trying different types of filleting, marked improvements in the characteristics of the complete airplane can be obtained. Muttray [Assistant Professor of Aerodynamics, California Institute of Technology] has shown that an improperly filleted fuselage can have a marked effect upon the wing-fuselage interference. [See National Advisory Committee for Aeronautics Technical Memorandum No. 517.] A badly designed fuselage and fillets cause a great decrease in the equivalent span of the airplane. The interference drag can be almost completely eliminated by correct design. It is well known that anything attached to the upper surface of a wing has a very detrimental effect. If protuberances can not be avoided, a model of sufficient scale should be constructed and their design worked over so that they will have the least effect. All re-entrant angles and small gaps, especially up above the wing, should be avoided. The recent work of the National Advisory Committee for Aeronautics has shown that a properly mounted wing-engine has only one-sixth the drag of the present normal type of engine nacelle. [See National Advisory Committee for Aeronautics Technical Note No. 320.]

Miscellaneous Drag Problems

The present type of landing-gear has very large drag, principally because of acute angles between the struts. Landing-gears can be tested at full-scale in a large wind tunnel or at half scale in a smaller wind-tunnel. The N.A.C.A., in its brilliant development of the Venturi cowling, has pointed the way for a more scientific attack upon drag. The British townend ring, though different in principle from the N.A.C.A. cowl, produces similar results.

A newer type of drag problem has arisen in connection with very high-speed airplanes. This type of plane is necessarily so clean that its gliding angle is very flat. This characteristic, combined with some of the ways in which the tendency toward low-wing monoplanes, has produced airplanes having great floating tendencies. These planes are very difficult to land in small fields over obstructions. There are three possible methods of landing them, all unsatisfactory: (a) The pilot may glide into a field steeply, picking up speed all the time and then floating a long distance before making contact; (b) he may glide in at his minimum gliding-speed and touch the ground at approximately the same distance from the obstruction as before; or (c) use the last method which is to squash into the field and pull out just before contact, thus making a short landing.

The first two methods are impossible in small fields, while the last method requires great skill and is very dangerous in bumpy air. Side-slipping a high-speed airplane is not very effective, as the fuselage used in this type is a very good streamline body at any ordinary angle of yaw. A few calculations will show that enough flat-plate area to decrease the lift-drag ratio to a reasonable value will be almost impossible to obtain in a safe and controllable manner. The usual form of spoiler is likewise inadvisable as it decreases the lift markedly, thus increasing the sinking speed. The only reasonably safe way to decrease the lift-drag ratio is to use some form of interference-drag device that will not spoil the lift and yet will produce a large increase in drag. This is an ideal wind-tunnel problem, and the polar and pitching-moment curves of any contemplated device can be easily determined.

High-lift devices should always be investigated to determine their effectiveness. Enough tests have been made with models and with the corresponding full-scale airplane to prove the reliability of the wind-tunnel methods.

Dynamical Stability Difficult To Test

All of the foregoing tests can be made with the ordinary three-component wind-tunnel balance. To investigate the complete airplane with its six degrees of freedom, a six-cylinder balance is necessary. The model of the complete airplane can be tested for stability and control, the effectiveness of the controls measured and the statical stability investigated. The problem of stability, power on, is more difficult. The statical stability of the airplane can be determined without the slipstream, and after the coefficients have been determined the moments of the gravity forces and the propeller thrust can be added in. To work with the slipstream a small high-speed motor is necessary. To date no satisfactory power unit has been developed, although a number of laboratories have designed or purchased apparatus for this purpose.

The study of dynamical stablity is very difficult, as it requires an entirely different type of set up than any of the foregoing tests. The model must be free to oscillate and it must be dynamically as well as geometrically similar to the full-scale airplane to be investigated.

The problem of wing or tail flutter is very difficult to investigate, as the model must be constructed so as to have the same geometrical shape, structural rigidities and mass distribution as the airplane. Work of this nature has been done and more will be done in the future. The surface texture of airfoils is now being worked on in the world's laboratories, and definite data on the effect of corrugations, rivet heads and the like will be available in the near future.

It is well to mention that propellers are being constantly tested in the laboratories that specialize in this work and our present remarkably high propeller efficiency has been achieved as a result of their efforts.

The writer does not believe that all of the tests mentioned are necessary for the design of a conventional airplane, but every plane should have its polars, moment curves and static stability determined. The rules now extant for dynamic stability give satisfactory results and those for the prevention of dangerous spinning characteristics are sufficient for the designer in most cases.

THE DISCUSSION

CHAIRMAN L. M. GRIFFITH [M.S.A.E.—Vice-president, general manager, Emsco Aero Engine Co., Los Angeles]:— The wind-tunnel is more or less of a mystery to many who are otherwise well versed in aeronautics. I have personally had some experience with it, and it is still a mystery to me. One thinks the building of a tunnel is a simple sort of job. He sees a tunnel running, notes its character, gets the dimensions and drawings and builds one like it. If he has had no experience, he says, "In two months we will have the tunnel finished and start making tests." But after the tunnel is completed in the two months, usually a year or two years is required to find out whether it is a good tunnel or not. There seem to be many things to contend with when one deals with air at high velocity through a wind-tunnel; the air does not follow the nice, smooth lines that were laid down on the drawing-board. Information resulting from wind-tunnel tests of all kinds, however, forms the real basis of our aerodynamic advance. We discover many things with full-size machines but can not very conveniently measure them. The quantities involved can not be determined readily, as we found at Langley Field; therefore we are dependent for much of our information upon the results of tests in wind-tunnels on models and parts of airplanes.

DR. A. L. KLEIN:— I can echo what Mr. Griffith has said, since, after the tunnel at the Institute was built, we found that one place inside of its perfectly conical body the air was moving upstream. We were much astounded; then we did a few things and got the air to go in the same direction over all of the tunnel.

Full-Scale Application of Tunnel Results

CHAIRMAN GRIFFITH:— Many airplane designers who have had a little experience are prone to think that the wind-tunnel is suitable only for the use of research men working on problems that have no bearing on the actual airplane. On the other hand, there may be one or two designers who have implicit faith in any result that comes from a wind-tunnel. Somewhere between these two views is a happy medium where the work of the designer is guided, not controlled, by wind-tunnel results. All such results are subject to interpretation and modification as necessary to suit the actual full-scale design, taking into consideration the difference in operating conditions between the flight of the full-size airplane and the passage of air around the small model in the wind-tunnel.

STANLEY H. EVANS [Aeronautic Engineer, design staff, the Douglas Co., Santa Monica, Calif.]:— What model airscrew speed can you get in the tunnel, Dr. Klein?

DR. KLEIN:— Our largest models will be of 6-ft. span, and an airscrew of the same proportional size as that used in an airplane would be approximately 18 in. in diameter. To run that propeller at the same *V*/*D* ratio as the actual propeller would require a speed of 10,000 to 15,000 r.p.m. Great difficulties have been experienced with the small electric motors at such speeds because of overheating. We hope soon to have a high-frequency generator to drive a three-phase motor at any speed up to 20,000 or 25,000 r.p.m. and to be able to control its speed by controlling the speed of the motor generator. Such apparatus is very expensive and the sets built to date have not been very satisfactory.

MR. EVANS:— I assume you could use a much larger propeller and only a small portion of the airplane model.

DR. KLEIN:— That could be done, but we were thinking of running the propeller in the stability tests of the airplane as a whole. If you were developing nacelles, you could make a model of just the parts of the structure adjacent and use a larger propeller; this would require more horsepower. The only successful work of this type has been done in England and Germany, and one of the aerodynamical laboratories in this Country received a duplicate of one of these motors and found that it ran red hot.

Trouble with High-Speed Electric Motors

WELLWOOD E. BEALL [Jun. S.A.E.—Assistant chief engineer, Walter M. Murphy Co., Pasadena, Calif.]:— The motor to which Dr. Klein refers was imported from Germany by Prof. Alexander Klemin of New York University. It was about 2 ½ in. in diameter and about 9 in. long. It operated on 500 cycles and required a special converter, also of German manufacture. This small motor developed, as I recall, about 1 ¾ hp. and was similar to one the Navy experimented with some time ago.

This motor was intended to be mounted in a wind-tunnel model and drive a propeller so that conditions approximating actual powered flight could be simulated. It was designed for operation at 40,000 r.p.m. with the propeller geared down to a suitable speed. However, operation at this speed was found to be impracticable, due to the motor overheating. It was then adjusted to turn at 36,000 r.p.m. With the motor mounted by itself in the laboratory and when it was turning a small propeller which threw considerable air upon it, this speed proved to be practicable. However, as soon as it was mounted inside a model for test, where no air current could strike it, it immediately became hot and after 45 sec. of running became too hot to operate.

The propeller reduction-gears were mounted on the motor in such a way that the torque reaction, and consequently the power delivered to the propeller, could be measured. This reduction-gear train was carried by a frame that pivoted in such a way that the torque reaction tended to rotate it. This rotation was restrained by a calibrated spring and the torque was indicated by a long, thin arm. This torque indicator operated satisfactorily in still air but, when placed in the slipstream of the propeller or in the wind-tunnel, it became inoperative due to the impact of the wind on it. This prevented the indication of the torque and consequently the calculation of the power.

This motor was also equipped with a revolution counter consisting of a worm-gear train and a small disc about an inch in diameter with one mark on its circumference. To obtain the speed of the motor, it was necessary to watch this disc, count its revolutions and calculate the result. This method is suitable for obtaining the speed of the motor before it is mounted in the model but very inconvenient when mounted in the model and in the tunnel. The reasons for this are obvious.

Although this motor was rather disappointing, it did arouse considerable interest and at least has provided a start in obtaining data for predicting the effect of the propeller slipstream and wash by means of wind-tunnel tests. The motor, I believe, has been sent back to its manufacturers to be rewound and rebuilt to operate at lower temperatures under load. A new system of distance-type indicating devices for the speed and torque is also being devised. With this rebuilt motor it is hoped that many valuable data may be obtained.

CHAIRMAN GRIFFITH:— The difficulty of running small-motor tests in the wind-tunnel is one of the factors that led to the present large tunnel at Langley Field and is leading the National Advisory Committee for Aeronautics to plan the construction of a much larger tunnel. I understood that the size of this was to be in the neighborhood of 30 ft. high and 40 ft. wide, but Dr. Klein tells me it has been increased to 30 x 60 ft. It is interesting to note that the 20-ft. tunnel takes about 2000 hp. to drive it.

With reference to Dr. Klein's comment about the detrimental effect of protuberances on top of the wing, I have been curious to know how much the Dornier-X speed might be below that of a similar airplane having the engines mounted within the wing itself.

DR. KLEIN:— German engineers have made some tests but the results have not been completely published. They showed that locating the propeller completely above the wing does not interfere with the wing. Before he built the flying-ship, Dr. Dornier expected to get a considerable increase in lift at take-off, because the slipstream would be entirely above the wing and increase the circulation about the wing.

QUESTION:— Has any work been done on an airplane which has some variable-drag device to increase the drag on landing so as to reduce the landing speed?

DR. KLEIN:— I do not know of any that has been done. We expect to try several devices of our own and of other people for this purpose. I think personally that the only feasible means is to use some interference-drag device; any other way is open to objections on the ground of reduction of controllability.

Variable-Density Wind Tunnels

Question:— What is the situation in regard to increasing the air density, using a closed pressure-system?

Dr. Klein:— That is one way of achieving a large Reynolds Number, which is our criterion of scale effect. A tunnel 5 ft. in diameter that can be pumped up to 20 atmospheres has been built at Langley Field and has been very successful. The British are contemplating building a similar tunnel. The Langley Field tunnel was exceedingly expensive. I imagine a high-pressure tunnel would cost about five times as much as an open tunnel of the same size. Our tunnel has a 10-ft. diameter, and we get an increased scale-factor by running at air-speeds up to 200 m.p.h. I think that our tunnel, without the building, cost approximately $75,000. We did not expect to get such high speeds but are pleased that we can get them. We build wind-tunnels and get astonishing results; nobody has very clear-cut ideas as to what the ideal wind-tunnel is.

Chairman Griffith:— An interesting item about the variable-density wind-tunnel at Langley Field is the tank in which the tunnel was placed. It was a very good piece of ship-plate work. The shell is 15 ft. in diameter and about 30 ft. long, with hemispherical ends, and weighs 43 tons. The side plates are 1 ¼ in. thick. This tank was tested to a pressure of 450 lb. per sq. in. and showed very little leakage. It cost $24,000, and $1,200 more was spent to get it from the place where it was built to the site of the tunnel. When we got it there, we began to figure how much more money we would have to spend. We had enough to put up a building and, through the cooperation of the Navy Department, used Navy equipment that originally cost about $80,000 and had been used in the helium plant at Fort Worth, Texas. Consequently, we were able to do a relatively big job for a small sum of money.

Plans for Huge Tunnel at Langley Field

A Member:— Is that very large tunnel at Langley Field actually being constructed and is it possible to make a guess as to the power that will be required?

Dr. Klein:— I believe that the National Advisory Committee for Aeronautics obtained from the Congress an appropriation of $900,000 for it and expects to use about 8000 hp. The Committee was considering the larger tunnel very seriously and was debating how to build it.

Chairman Griffith:— This wind-tunnel problem is really very interesting. When we built the 5-ft. variable-density tunnel at Langley Field we thought we would be in an excellent position to investigate all kinds of aerodynamic problems in the tunnel. Shortly after that tunnel was finished, we started the 20-ft. tunnel with the idea that we would then be able to make tests at the same Reynolds number but with different air densities and model scales. The interior of the high-pressure tunnel burned out several times and we found that wood was not a suitable material at 20 atmospheres, or a pressure of about 300 lb. per in., as combustion is extremely energetic at that pressure and air velocity.

All this time the Committee was carrying on full-scale work with airplanes with about 17 different varieties of recording instrument and found the limitations of that method. Having completed the 20-ft. tunnel, it is now building one 30 x 60 ft. To the industry the world over it looks as if it were going to be worth a lot of money. I really believe that, with proper coordination between full-flight tests with all the instruments that can be crowded into the cockpit, tests of the model airplane in a variable-density tunnel and tests in an ordinary tunnel, we can produce a mass of coordinated data that will tie up rather closely the various testing means.

In any case we can look forward to the increasing use of the wind-tunnel and to its influence being reflected in greater aerodynamic efficiency of aircraft. That is very definite. We know that the wind-tunnel has given us the basis on which we have built most of our aerodynamic progress and is going to be the main instrument for further development. It never will take the place of free-flight development, but it is coming closer to it. It is only a question of time, I think, when we shall be able to design aircraft upon the basis of tunnel tests and not miss the computed performance on the full-scale machine by more than 2 or 3 per cent.

Gerald Vultee [Chief engineer, Lockheed Aircraft Co., Burbank, Calif.; now with Detroit Aircraft Corp., Detroit]:— The result of my experience in flying airplanes is that, if one sometimes could be sure of hitting within 25 per cent of calculations, he would feel much better.

Present Landing-Speeds Seem Safe

JOHN K. NORTHROP:—Will the new landing rules of the Department of Commerce necessitate the use of variablelift devices, or will it be possible to bring a full-scale machine to a landing much slower than the theoretical figures would indicate?

MR. VULTEE:—As I remember the rule, unless the theoretical landing-speed was below 60 m.p.h. the Department required special flight-tests to prove the practicability of the design. If those were passed, and they were not particularly stringent tests, the design was approved. However, I believe from the experience we have had that the plane can be brought in at a considerably lower speed than would appear from theoretical considerations.

It is natural to assume that most manufacturers' performance figures on landing-speeds are somewhat optimistic; checking the maximum lift–coefficients of existing commercial planes against the performance that is claimed for them, the maximum lift–coefficients are found to run in the neighborhood of 0.0040 or higher. These are rather high figures. We encountered something similar to that in trying to reduce the landing-speed we had. We found we could not get any assurance of being able to reduce the landing-speed appreciably with any normal wing-section, as by actual flight-tests we already had a maximum lift-coefficient of about 0.0040. However, I believe that a good pilot can bring a plane in rather more slowly than the theoretical figures indicate.

Perhaps we have become used to seeing planes coming in at 60 and 65 m.p.h. and it looks like 45. The landing-speeds we are using now seem to be satisfactory as regards safety. The planes get in and out of fairly small fields and do not average a large per centage of crack-ups on landing; therefore, as an increase in landing-speed will make possible a greater increase in high speed, it seems that we should go a little slowly in drawing conclusions regarding specifying slower landing-speeds. Planes designed for a landing-speed of about 40 m.p.h. would look rather queer compared with the planes that are being built at present. All the builders, I believe, are making a little increase in the allowable landing-speed in their designs except for airplanes that are built for special purposes, such as training, for which a lower landing-speed is absolutely necessary.

Document 2-28(b), letter from A. L. Klein to V. E. Clark, 1934.

August 14, 1934

Mr. V. E. Clark
30 Rockefeller Plaza
New York City
Dear Mr. Clark:

You will please find enclosed a copy of our wind tunnel specifications together with some additional blueprints and addenda to bring them up to date.

In answer to the first point in your letter, I would state that the maximum span of models which we have tested in our tunnel is 7 ¾ ft.; any size up to this will be satisfactory. We usually try to pick a model span of some simple ratio to the full scale airplane, in order to simplify the computation of the model dimensions. The wind tunnel corrections are now in such good order that we have found that this size of model, 77 ½%, gives perfect satisfaction. The only reason we do not try to go to a larger model is because we are afraid that our velocity distribution will not be satisfactory very much nearer the walls. This size of model results in approximately a ⅐th scale model of a modern single engine transport.

Your question 2 is covered, I think, in the wind tunnel model specifications.

Our normal procedure in testing airplane models is to test the wing alone first, in order to find its characteristics, then to build up the model part by part, putting on the fuselage first and filleting it with wax in order to get the minimum detrimental effect, then adding the engine and landing gear, etc. Finally, having made a complete test of the model less horizontal tail surfaces, we add the horizontal tail surfaces and take measurements with three positions of the stabilizer in order to determine the optimum stabilizer setting. After making the stabilizer tests with the elevator fixed, we work with the elevator set at various angles for its effectiveness, and similarly with the ailerons and rudder. If control tabs are used on the elevator, we make elevator free tests, in which case the elevator must be mounted on ball bearings and counterweighted so that it is in static balance. These tests are made at several tab settings in order to get the tab effectiveness.

We have found from experience that the following gives a fairly accurate estimate for experiments on normal airplane models:

a) A fixed cost of about $120 for preparing the model for the tunnel, making the preliminary calculations for reducing the data, etc.

b) A running cost of approximately $28 per Run, a normal Run consisting of a series of three or six component measurements at about 15 angles of attack and at one air speed.

Our costs are based on the following items:

a) 1.5 times our labor cost.

b) A wind tunnel charge of $6.00 per hour for all time in which the tunnel is tied up for the investigation.

c) Electrical power used at 1.2 cents per K.W.H. (about $2 or $3 per Run).

We prefer to base our charges directly on the above costs, but if you prefer, we will make a definite bid based on a detailed list of exactly the tests you desire.

In order to quote on a single set of lift, drag and pitching moment curves corrected for Reynolds' Number and turbulence, it will be necessary for us to make at least three runs at various speeds in the wind tunnel and then to extrapolate to full scale. This will cost in the neighborhood of $200 to $225 on account of the large overhead of one test. The unit cost of a small number of runs is rather large. Our costs have been as small as $20 per run for investigations that were of some length, i.e. from 60 to 100 runs on the model. We have found that the normal cost of a complete test on a model as mentioned is from $800 to $1200.

I hope that the foregoing will give you all of the necessary information which you desire.

Sincerely yours,
A. L. Klein

Document 2-29

Starr Truscott, Aeronautical Engineer, memorandum to Engineer-in-Charge [Henry J. E. Reid], "Work in connection with special aerodynamic tests for Bureau of Aeronautics which has been requested by Mr. Lougheed," 5 April 1932, RA file 210, Historical Archives, NASA Langley.

As noted elsewhere in this chapter, there is a close link between hydrodynamics and aerodynamics, and the latter discipline frequently benefited from theoretical and experimental work in the former. The NACA constructed two model-towing tanks at Langley for the express purpose of investigating the behavior of seaplanes during takeoff and landing, but some rather unusual tests were performed in the tanks on occasion. One such test involved a study of seagull flight characteristics for the U.S. Navy's Bureau of Aeronautics (BuAer). BuAer's Victor Lougheed devised a program to capture several seagulls, freeze them with outstretched wings, and test their aerodynamic characteristics by towing them submerged through one of the seaplane tanks. Always cooperative, the NACA approved the project but, as this memo shows, the Langley staff wanted to be sure that the navy assumed most of the responsibility and risk for such an unorthodox program. The tests were run, but there is no evidence that they contributed anything of significance to aeronautical engineering.

Document 2-29, Starr Truscott, "Work in connection with special aerodynamic tests for Bureau of Aeronautics which has been requested by Mr. Lougheed," 1932.

April 5, 1932

MEMORANDUM For Engineer-in-Charge.

Subject: Work in connection with special aerodynamic tests for Bureau of Aeronautics which has been requested by Mr. Lougheed.

1. On the morning of Wednesday, March 30, Mr. Lougheed appeared at the tank. The special dynamometer for use in making the tests in which he is interested had been received the day before. Under Mr. Lougheed's direction the dynamometer was unpacked and a rack made especially to suit it. He also explained the operation in detail.

2. In view of the general delicacy of the device and the ease with which the small spring hinge "knife edges" could be put out of adjustment, it was decided to leave the balance exactly as it was received, with all motions locked, until the work of assembling it to its support and on the carriage had been completed. The

support has not yet been received from the Norfolk Navy Yard, although it is understood that it is completed.

3. After discussing the installation and the precautions to be observed in this work and in the operation of the balance, Mr. Lougheed gave an outline of the further work which would be required to prepare for and carry out the tests.

4. A working platform across the fan end of the tank is required. On this is to be carried the freezing box for freezing the birds, two protractors for measuring the angle of attack of the wings, a projecting walk for reaching the balance on the spur projecting from the carriage, a receptacle for liquid air, two scales, two scale pans, and fine shot for calibrations of the balance.

5. The freezing box must be about 8 or 10 feet long and 4 feet wide. In plan form it should taper from the center to the ends so as to reduce the volume to be filled with liquid air. The depth of the box should be as little as will accommodate the birds, for the same reason. This box can be made of insulite or some other insulating board with wood batten stiffening.

6. The two protractors can be made or bought. They can be relatively crude in construction but must include blades 12 inches to 18 inches long to extend under the wings to measure the slope of the wing chord relative to some level line. Two are required so that one can be held while the other is being used to set the other wing.

7. The projecting walk is required to provide access to the calibrating screw which lies under the balance and some 4 feet back from the tip of the spike on which the models are supported—or on which the birds are impaled.

8. The receptacle for the liquid air will probably be the one in which it is received, but it must be supported in such a manner as to make access easy and replacing simple.

9. The two scales are required for weighing the birds, or models, and for weighing the calibrating loads. This balance has no calibrated springs or lever balances. On each test run the device is brought to a null point and left there. After the run the model is removed and the forces required to restore the null are measured by dead loading with shot.

10. One scale should read up to 5 pounds by ¼ ounce; the other should have a capacity of ½ to 1 pound and should read to 5 grains.

11. With the balance there have been supplied stirrups for supporting scale pans in which the shot may be placed, but the scale pans must be supplied. Mr. Lougheed suggests paper cups as easy and light.

12. In addition to the material to be used in connection with the tests, there will also be required a supply of birds for testing. Mr. Lougheed suggested 5 gulls and 5 turkey buzzards. These could be obtained by local trapping he thought and probably boys might be interested in getting them.

13. For keeping these birds a small menagerie will be required. The cages must be large enough to permit the birds to spread their wings to preen them. If the birds do not have sufficient space they will disarrange and break feathers by pushing them against the walls of the cages. Mr. Lougheed estimated that a cage about 12 feet long, 5 feet high, and 5 feet deep would suffice. This should be divided by a solid partition into a section 7 feet long and one 5 feet long. The larger section is for the buzzards, the smaller for gulls. Perches should be provided.

14. The birds will require to be fed to keep them in condition. The gulls get fish and the buzzards meat in the form of spoiled meat or carrion.

15. Mr. Lougheed now has one gull and one buzzard in the zoo at Boston which will be shipped down here.

16. I called his attention to the protection against killing or taking which is given these birds by State and Federal laws. He said he would get the necessary permits issued to himself and supply copies of the original to anyone who undertook to catch birds for this job.

17. Discussing the balance and its method of operation, Mr. Lougheed referred to the adjusting of speed, while the carriage was in motion, to suit the model or bird. I called his attention that we could not do it from the carriage. He replied that he understood that, but that by fitting two colored lights which could be seen by the operator at the desk and which would indicate plus or minus speed, it ought to be possible to do it. These lights would be operated by contacts on the balance. I recalled to him that we had told him in the beginning that it was not possible to change speed while running. He said the speed wasn't critical and if it didn't work out it would simply mean more runs!

18. He requested that we install the balance on the balance support which is still at the Navy Yard. This would require installing the wiring from the contacts, the fitting of lamps, with condensers (if required) and batteries, for signalling the operator on the carriage, and the fitting of signal lamps for the operator at the desks.

19. After we had the balance installed we should play with it, mounting discs or other objects on the spike and determining their resistance, etc. A box of wing models which could be fitted was also on its way to the laboratory.

20. Mr. Lougheed inquired about a local supply of liquid air. I told him I had no information but doubted that it could be obtained locally or even in Norfolk. He said it could be obtained in Washington and how it could be transported. I suggested the Ludington Line airplanes if it was urgent. He replied that probably the boat would be all right.

21. When the recital of this list of things to be done began I, of course, acquiesced in the items of the freezing box and the access platform. The first was relatively simple, requiring only a few sheets of insulite and a little carpenter work, while the second we have in the form of our small portable bridge.

22. However, as the list kept increasing in length—and obviously in cost—I thought it might be better to let Mr. Lougheed complete his tale before commenting. When this involved the provision of the menagerie and keepers, to say nothing of trappers, I decided that it would be better to make no objections but simply to get the picture well in mind, study it, and then make proposals as to what we should do and what the bureau should provide.

23. Accordingly, I suggest the following:

A. The Bureau of Aeronautics should be informed that Mr. Lougheed has visited the laboratory in connection with this work and has discussed the equipment required in addition to the balance and also the method of taking the birds. Certain items of this material can be supplied by the laboratory but others should be supplied by the bureau because they are of types which are not used at or easily available to the laboratory. These items are:

(1) The birds required for experiment

(2) the liquid air for freezing them

(3) the protractors for measuring angle of attack of wings

(4) two scales, one reading 5 pounds to ¼ ounce, the other reading 1 pound to 5 grains

The bureau should also be reminded that in the course of the conferences before these tests were authorized, the representatives of the bureau were informed that the speed of the carriage was not controllable from the carriage and hence could not be varied during the run. From the recent discussion with Mr. Lougheed it has been learned that the operation of the balance which has been provided requires that the speed of the carriage shall be adjusted while running. This will require the fitting of some small items of equipment not contemplated in the original plan and the adopting of a method of operation which will make heavy demands on the control operator. The laboratory is willing to attempt this method of operation, but of course can not promise certain success.

B. The laboratory will assemble the balance to its support, provide all incidental fastenings, wiring, lights, extensions of control rods, batteries, and other items connected with the operation of the balance, and will install the balance on the carriage in the manner described by Mr. Lougheed on his recent visit. It will also provide and have ready the means of access to the balance, the freezing box, and one or more cages for the birds. When the birds are received it will provide for their feeding. (It is assumed that requisitions covering the necessary meat and fish will get by the Comptroller.)

24. It will be noted that this calls for the bureau to supply the birds. It seems to me they should do this because it will require quite a bit of arranging. Mr. Lougheed spoke of putting an advertisement in the papers. Such a thing would only start trouble. Some group or person either well intentioned or seeking notoriety would protest and the resulting troubles would keep us busy.

25. A further thought is that Mr. Lougheed warned me that the gulls might easily put out an eye for anyone who handled them, as they always strike for it, while the buzzards can bite off a finger without trouble, thanks to their "tinsnip" jaws. I would just as soon others should handle such birds while catching them. Feeding them will be bad enough.

26. A further thought is that if this work is to be kept at all confidential the purpose of the birds must not be advertised. This would surely occur if the birds ere taken locally. It would be much better to take them around Washington and ship them here. It might even be feasible to handle the taking at the Naval Air Station.

Starr Truscott,
Aeronautical Engineer

Document 2-30(a–b)

(a) Edward P. Warner, "Research to the Fore," *Aviation* 33, No. 6 (June 1934): 186.

(b) "Research Symphony: The Langley Philharmonic in Opus No. 10," *Aviation* 34, No. 6 (June 1935): 15–18.

Aviation Editor Edward P. Warner was clearly a big fan of the NACA. He extolled the virtues of the committee, its research methods, and the products of that research in numerous columns, but Warner, who served on the NACA's Committee on Aeronautics, knew what he was talking about. Thus, his comments and conclusions are worthy of consideration as more than simply those of an apologist.

The final documents presented in this chapter both come from the pages of *Aviation*; the first a Warner editorial, and the second an article likely written—and certainly approved—by him as well. They were published about the time of the NACA's 1934 and 1935 Annual Aircraft Engineering Research Conferences at the Langley Laboratory, and both dealt with the rise of aeronautical research to a leading role in aircraft design. In 1934's "Research to the Fore," Warner observed that "every group in the aircraft industry" had come to "a new appreciation of the vital importance of the scientific fundamentals of aircraft design. Research has ceased to be the servant of aeronautical development, and has become its guide." Journalistic hyperbole aside, Warner's observation had some solid evidence, in the form of recently built wind tunnels and new aircraft designs, to back it up.

Aviation took a whimsical tone in its report on the tenth (1935) conference, titling it "Research Symphony: The Langley Philharmonic in Opus No. 10" and using musical analogies to categorize the research work being done under the baton of "Conductor Joseph S. Ames" and his able "Concert Master George W. Lewis." Nevertheless, this "brief recapitulation of some of the principal movements" included a wealth of information on the current state of aeronautical research and its impact on the aviation industry. Reading it, one will quickly sense how far aviation technology had come in the two decades since the NACA's founding, thanks in no small part to that organization's increasingly sophisticated research program.

Document 2-30(a), Edward P. Warner, "Research to the Fore," 1934.

Research to the Fore

About this time each year the aircraft industry prepares to move in force on Langley Field and the laboratories of the N.A.C.A. This springtime visit has become an annual habit, and out of the annual attendance of 200 or 300 at the N.A.C.A. field day the majority are veterans who have acquired the habit so thoroughly that they never think of missing the trip. In the last two or three years those old timers have been sensing a change in the atmosphere of the meeting, for an interest in the detail of research equipment, method, and results that was once concentrated among scientists has spread to every group in the aircraft industry. The representatives of the builders of aircraft for the military market attend in steadily growing numbers, and the manufacturers of light commercial craft and the operators of airlines, though they came comparatively late to the roster of the meeting, are finally beginning to play a part.

All this is quite a compliment to the National Advisory Committee, but it is much more than that,—it is evidence of a new appreciation of the vital importance of the scientific fundamentals of aircraft design. Research has ceased to be the servant of aeronautical development, and has become its guide. One need not go to the very remote past to find that research, like God and the doctor in the ancient jungle, was valued by the man of strictly practical interests principally in time of trouble, when he had run into an unexpected obstacle and needed to have it removed in a hurry. Now he has learned not only to avoid the obstacle by a sufficiently intense preliminary study, but to make certain that he is really getting the best possible result from his product, and not merely a passably good one, by trying out the whole range of possible alternatives under laboratory conditions. On a modern high-speed transport the difference between a wing-fuselage fillet casually faired in to look about right and one determined as the ideal through a long series of studies in the wind tunnel may be 3 m.p.h. in maximum speed. On a 12-passenger twin-engined transport that means a saving of about $2,500 a year in operating cost on a single plane. On an order for twenty such planes, the saving in a single year would be enough to pay the cost of building and equipping a first-class wind tunnel in which to do the work. That fact has made itself felt, and whereas no more than four or five years ago it was rather an extraordinary thing to have any extensive wind-tunnel testing done before building a new ship it has now become the general rule. Not only the wind tunnel, but the seaplane channel as well, has become an accepted and an almost necessary instrument of the designer in his preliminary planning of a new type.

Aviation has suffered at all times from a delusive belief, which one may still encounter here and there, that research and analysis in airplane design are futile frills and that what is needed is to have a good practical man with an extended experience as a pilot and a good eye for line draw a picture of the new airplane and build it accordingly. There was a time when that went so far that the very making of engineering drawings for an experimental machine was considered to fall under the head of "frills," and one pioneer builder used to boast "he could start in the middle and work outwards and not decide what anything on the plane was going to be like until he came to it." That certainly is one way of doing it, but not the best way. How far it is from being the best becomes apparent when the practical man scornful of theory and of research has done his best and when another designer unhampered by any such scorn, but possessed of wind tunnels and believing their results, takes the same set of specifications and produces a machine to be put into the competition. The airplane built by inspiration and by seasoned judgment is prone to look extremely foolish, under those conditions, as against the ship in which judgment is backed by careful application of science and of laboratory technique.

Already it is true that a majority of America's foremost airplane builders either have wind tunnels of their own or have access to the tunnels of neighboring universities. We predict that within another three or four years the company that fails to own and operate its own tunnel will be quite out of the running, and that an aerodynamic section attached to every engineering staff, with its personnel concerning themselves exclusively with aerodynamic research and with the analysis of aerodynamic problems handed over to them by the designers, will be no questionable extravagance but quite as much of a necessity as the stress analysis group is today. The compass points that way, and the wise management will make its plans accordingly.

Document 2-30(b), "Research Symphony: The Langley Philharmonic in Opus No. 10," 1935.

Research Symphony

The Langley Philharmonic in Opus No. 10

With conductor Joseph S. Ames insisting on inflexible adherence to tempo and Concert Master George W. Lewis getting brilliant performance from his individual group and solo performers, the Tenth Annual Engineering Research Conference of the National Advisory Committee for Aeronautics proceeded with all the smoothness of a major symphonic ensemble in action. The audience was the aviation industry in numbers that strained the facilities of the isolated Tidewater Peninsular at Old Point Comfort almost to the limit. It is impossible within our space limitations to reproduce each nuance in the detailed development of every research theme. That we leave to the extensive literature of the committee. All we can do here is to give a brief recapitulation of some of the principal movements.

Spinning Song

Most fascinating number of this year's performance was the demonstration of the brand new tunnel where model airplanes may spin freely in a vertically rising air jet of controllable velocity. Most remarkable are the models themselves, miniature airplanes that are not only geometrically similar to their full scale counterparts, but also must have identical mass distribution and similar dynamic characteristics. To add wonder to wonders, into each model has been built delicate timing machinery connected to controls to reset rudder, elevators, ailerons at predetermined intervals during spinning to promote recovery from the spin, or to change its characteristics. To watch these intricate models swimming in the air stream like goldfish in a bowl, reproducing well-known spinning maneuvers as though under the control of a miniature human Pilot, was an experience that few of this year's visitors will soon forget.

Thanks to Public Works Administration funds which made possible the installation of the new tunnel and its accessory equipment, the 800 factors which contribute to spins outlined some five years ago by Fred E. Weick, may now be subjected to exacting laboratory tests, reducing by many times the expense and the danger involved in full-scale spinning research.

Wings; Crescendo

Long before the Department of Commerce undertook a program of private flying encouragement through equipment purchases, the NACA had been at work on the fundamentals of what makes flying simpler. Five years of work on variable lift devices for lowering landing speed and on surer lateral controls had gradually split the boundaries of knowledge, and the conference revealed for the first time a new series of results.

Particularly important was the report of flight trials on designs so far tried only in the wind tunnel. Flaps and variable area wings, generally considered only as a means of reducing minimum speed, are established on a new footing as proven aids to performance in getting off and climbing. On a standard Fairchild monoplane, the installation of a Fowler wing (sliding a flap out of the lower surface of the wings to rear and at the same time pulling it down so that both the area and the camber are increased) reduced take-off distance from 500 ft. to 330 ft. in still air. The total distance to clear a 50-ft obstacle came down from 910 ft. to 720, the calculated distance to accomplish the same take-off and climb with a heavily loaded twin engine ship, from 1,500 ft. to 840. On the twin engine take-off, the use of a simple split flap without area increasing features would reduce the space needed for take-off only from 1,500 ft. to 1,100 ft.

A trend toward variable lift devices of higher efficiency than the plain hinged flap, a readiness to accept as necessary whatever mechanical complications their virtues might involve, were plainly indicated in the report and their discussions by the engineers in attendance. Area variation, for example, is clearly a matter of practical interest for the near future.

Most elaborate and most effective of devices so far known is boundary-layer control, sucking off the air from the surface of the wing into its interior through slots parallel to the span along the upper surface. The committee's studies show that with a fixed wing with a single slot halfway back on the cord, burbling can be eliminated, a steady flow and a steadily increasing lift be maintained up to an angle of attack of over 50 deg., a maximum lift coefficient of 3.0 be attained with an application to the blower of less than 3 per cent of the engine power. That would make it possible to land at 55 m.p.h. with a wing loading of 23 lb. per sq. ft. Even with a thin tapered wing well suited for high speed use the same lift could be secured with expenditure of about 6 per cent of the total power.

Interesting experiences with the handling of flap-equipped airplanes came out in the course of discussion. The committee's pilots have found that if the flaps are pulled down suddenly to steepen tile glide path in coming in over an obstacle, the immediate effect is exactly the opposite of what is wanted. The increase of lift by the flaps sets the ship to climbing above its original course, and with a light airplane fully 850 ft. had to be covered before the flight-path dropped below the level that would have been reached if the flaps had not been used at all. To overcome any such reversal of effect it was suggested that the flaps be pulled down to very large angles, as much as 80 deg., where there will be a pure air-brake action with no further increase of lift. One experimental machine of private-owner type has been fitted with such a control for trial.

Another way of increasing the lift, well known to test pilots trying to meet a minimum speed specification but seldom made the subject of research, is by pulling back into a full stall and then opening the throttles wide to blow the slip-stream across the wings and to carry a part of the weight directly on the propeller thrust. Measurement on a typical plane in the propeller research laboratory at Langley Field showed an effective increase of 0.3 in the lift coefficient from such a maneuver, a possible reduction of about 10 per cent in minimum speed.

The effect of large flap angles on stability has proved to be bad throughout, even when the flap extends over only a part of the span. The Fairchild with the Fowler flap became longitudinally unstable both with free and with fixed controls at all speeds above 70 m.p.h. with the flap clear down. Fortunately the stability characteristics prove to be best in the part of the speed range where the flap is most likely to be wanted, but at low speeds there is an extreme sloppiness of rudder control that requires as much as 11 deg. of rudder to hold a straight course. In some cases, in fact, the machine could not be flown at all at minimum speed with full flap effect because of the impossibility of keeping it straight even with full rudder. Mr. McAvoy of the Committee's technical staff and Temple N. Joyce debated flap landing technique and agreed that it differed from normal practice in that the machine need never be brought anywhere near a stalled attitude. The drag being so large that the nose call be put down sharply without picking up much speed, the angle of attack can be kept small until the very last instant of flattening out.

For that reason, the abrupt collapse of the lift coefficient at angles beyond the burble point that characterizes all flap arrangements makes no trouble in practice.

The NACA slot lip control is a new and most promising addition to the long list of lateral controls developed especially for use in conjunction with flaps and to be effective beyond the stall. A combination of slot and spoiler, it is a small flat plate lying flush with the upper surface and hinged at its forward edge. So placed as normally to block very largely but not entirely the exit of air from a slot through the wing just forward of mid-chord, its raising cuts down the lift with none of the time lag that marks the action of an ordinary spoiler.

Drag; Diminuendo

Few of the committee's programs have been as extensive or as fruitful of practical results as the relentless pursuit of drag. First notable contribution to aerodynamic efficiency was the familiar NACA radial engine cowling, then a long series of investigations on nacelle position, interference, correlated with more highly theoretical studies of air flow, scale effect and turbulence.

In the course of recent research the laboratories have dipped deeply into fundamentals without losing sight of visual manifestations of flow phenomena. For example, the smoke streamer studies of flow separation from airfoil bodies have revealed that a turning propeller in optimum position (tractor or pusher installation) improves the airflow in normal flight and at high angles of attack.

Optimum position for engine installation has been found to be within the structure or in leading edge nacelles. An ideal installation would have the engine completely enclosed in the structure fitted with extension shaft. Rear extension of the shaft gives best net efficiency at low speeds but, at 200-300 m.p.h. is worse than one with propeller ahead of the leading edge.

Tests on in-line engine installation in wing-nacelle combinations have yielded slightly higher drag figures than for radials, although it appears probable that with higher power concentration in a given nacelle, conditions may be reversed.

Bombshell for retractable landing gear advocates was the news that clean fixed landing gears had only slightly lower drag than fully retracting types. In terms of top speed, the difference was approximately 3 per cent. Airline engineers pointed out in conference that 6 m.p.h. at the upper end of the range was the equivalent to 10 per cent power, asked for study of fixed landing gear effect on stability and spinning qualities. Investigation of the take-off characteristics of airplanes with normal retracting gear extended as compared with fixed faired gears was suggested by T. P. Wright.

To run down the influence of various fuselage-wing combinations on drag and therefore on speed, a series of tests was run in the variable density tunnel to determine optimum wing position with respect to fuselage. Beginning with a bare fuselage with an uncowled radial engine in the nose and a rectangular wing in the best low-wing position, refinements were added successively. Engine cowling, wing root fillets, complete housing of engine in the wing, adaptation of a symmetrical airfoil (rectangular then tapered), and finally a shift of the wing up to a high mid-wing position, boosted the potential high-speed of the combination (for constant engine power) from 145 up to 205 m.p.h. The addition of a trailing edge flap naturally did not add to the top speed but gave, as would be expected, a greatly increased speed range.

New sources of efficiency were promised by Eastman N. Jacobs, whose airfoil family increases yearly. Ideal offspring, NACA 23012, is symmetrical, has maximum camber relatively far forward. In such airfoils, maximum camber may be moved forward beneficially to 15 per cent of chord length, harmlessly to 5 per cent.

Stresses, Giros, Boats; Miscellany

Heretofore the study of gust loads has been confined to effects on wings. Recent flight research has indicated, however, that gust loads on tail surfaces are much greater than has been June, suspected. Accelerometer readings give an average for the action of a gust over the entire wing span but since the wave length of the gust may be much shorter than the wing span, the peak loads imposed by the gust may be much higher than the average recorded for the entire wing. Where the wing may extend beyond the boundaries of a single gust wave, the shorter span tail surfaces may take the full gust impact. On an O2H machine, for example, where an average wing gust of 13 ft. per second was recorded, simultaneous readings on the horizontal and vertical tail surfaces showed 33 and 45 ft. per second gusts respectively. An extensive investigation of tail loading to supplement the results so far obtained from acceleration readings on transport planes in actual service is now under way.

Work with rotating wing systems, although apparently not as active as a year ago, still has a place in the research program. Recent investigations have covered the selection of airfoil sections for rotor use, also plan form modifications for maximum values of L/D. Thin cambered sections show higher efficiency than the symmetrical types; for example, NACA 4412 was found to be some 13 per cent more efficient than NACA 0018. Although it was suspected that greater rotor efficiencies might be obtained by cutting out portions of the effective blade area near the hub, experiment soon indicated not only that maximum L/D's were obtained with a full span blade, but also that greater efficiencies might be expected from tapered plan forms where the chord at the blade root was considerably greater than at the tip.

The ability of an autogiro to take off vertically on energy stored in an over speeded rotor was demonstrated with a 10 ft. electrically driven model. During initial rotation the blades were held at zero lift position then suddenly released to a high angle of attack position. The model rose vertically to a height of 20-25 ft. It was shown that the vertical distance attained during the initial jump is a function of disk loading. Tests on the model showed that, with a 392 ft. per second tip speed and a disk loading of 2 lb. per sq. ft., the initial rise is only 1 ft. Cutting the disk loading in half, however, the initial jump goes up to 15 ft.

The research program which led to the discovery of the beneficial effects of pointed main steps for flying boat bottoms (announced in 1934) was extended to cover steps of varying depths, and to study the effects of changes in the angle of dead rise and after-body keel. Shallow steps were shown to perform better at low speeds, deeper steps at higher speeds. Relatively flat after-body angles (the range between 0 and 8 deg. has so far been investigated) appear more advantageous at low speeds, low loadings. Dead rise angles did not seem critical, however, for the characteristics of similar hulls with bottom angles of 15, 20 and 25 deg, were essentially alike.

Most important result announced concerned the effect of various shapes of rivet heads in bottom plating. By towing metal planing surfaces with a standard pattern of dimpled, brazier and button headed rivets, the advantage of keeping bottoms as smooth as possible became evident. The resistance of dimpled rivets was 5 per cent over the smooth plate, brazier heads 12 per cent, button heads 17 per cent.

Power plants; Energico

Some twenty forms of NACA cowl with varying ratios of nose opening to overall diameter and rear gap area are being studied at full scale in the 20 ft. tunnel. Charts will shortly be available from which designers will be able to select cowl characteristics for all desired engine and flight conditions. As suggested by airline operators who experience cooling troubles at normal angles and speeds of climb, the program will be extended to include rang of angles of thrust in line to flight path of from 0 to 10 deg.

Preliminary test results indicate that cylinder cooling at constant power output is independent of altitude as long as the mass movement of air over the cylinder is constant. The cooling effect per unit of cooling surface seems to vary approximately as the square root of the airflow in terms of pounds per second per square feet of area covered. Studies of variation of cylinder temperatures with changes of cooling air temperatures indicate that the variation is linear, independent of brake mean effective pressure, mass flow.

Cooling fins should run at least eight to the inch, should be closely jacketed to force airflow to follow the fin and the wall closely. The shape of the jacket and the size of the intake and outlet openings are being investigated not only for free air flow cooling but also for blower cooling.

Interest in completely housed-in engines for drag reduction prompts research in blower cooling. Of chief interest is the cost of blower operation in horsepower. Some 30 to 35 per cent of total horsepower goes into the cooling of a bare engine, 13 to 16 per cent to cool the same engine with a properly designed

NACA cowl. Calculations based on skin friction of the average radial engine indicate that the absolute minimum of power required is about 1? per cent of the total horsepower. Therefore, the range in which blower designs must work is between 1? and 13 per cent of the total horsepower. On the basis of 65 per cent blower efficiency, the cooling loss should not be over 5 per cent.

Where long range performance is required, the low specific fuel consumption of the compression ignition engine put is independent of altitude as long as is very attractive since 40 to 45 per cent of initial useful load of an airplane may be required for fuel alone. Work has progressed far enough to indicate that it is possible to obtain the same power for the same displacement and r.p.m. for carburetor and for compression ignition engines with reduction of specific fuel consumption of some 20 per cent.

Shape of combustion chamber affects the efficiency of compression ignition engines. Spherical or disk type mixture chambers were found not as effective as a new displacer type recently developed by the laboratories. A solid boss cast on top of the piston projects into the mixture chamber at top dead center, causing a high velocity air flow in the narrow clearance between boss and sidewall and great turbulence in the chamber at the instant of fuel injection.

Among questions submitted for consideration of the committee: Best cooling arrangement for six-cylinder in-line engines? How can airflow inside an NACA cowl be restored to the outside flow most efficiently?—through an open slot or through louvers?—is it permissible to support the skirt of the cowl on the fire wall? Are internal guide vanes permissible? How may the accessories on an in-line engine best be cooled? What is the effect of a propeller spinner on the cooling of an in-line engine? How best to cool a flat engine—where to take in the air, where to discharge it? What is the drag of openings such as the ends of exhaust stacks, facing aft,

Propellers; Vibrato

Resonance with frequencies originating in the engine rather than aerodynamic flutter is now recognized as the cause of vibrations frequently leading to fatigue failures in propellers. It is demonstrable that true flutter can occur only at speeds far in excess of the speed of sound. The natural frequency of a rotating propeller is quite different from one at rest, due to the blade tension induced by centrifugal forces. For full scale propellers, therefore, vibration analysis by accoustical [sic] methods becomes the only practical method.

A method of scaling down propeller vibration effects for study was shown. A propeller of full-scale diameter was prepared with the aluminum alloy blades reduced to one-tenth normal width and one-tenth normal thickness. It is mathematically demonstrable that the vibration characteristics of such a propeller when rotated at one tenth normal revolutions per minute are exactly similar to those of the normal propeller at normal speeds. It is therefore possible by rotating the propeller (with blades completely enclosed in steamline tubes to eliminate all air effects) and imposing at the same time axial vibrations of known frequencies on the hub, to determine the resonant frequencies of the modified blades. Results scale up to full size by simply multiplying all data by ten.

It was stated that undesirable resonances had been eliminated in some cases by changing the mounting of a propeller from a position parallel to the engine crank throw to one 90 deg. away, but the desirability of such a method of correction was seriously questioned by propeller and engine manufacturers.

A new light on the composition of propeller noise was obtained by filtering out certain frequencies (or combinations of frequencies) from the sound picked up by microphone 50 ft. away from an electrically driven full scale propeller. It was evident that the troublesome ranges were (1) the higher frequency harmonics and (2) the vortex noises which produce the characteristic tearing sounds emitted by a propeller at high speed. It was shown that the basic frequency filtered out from all the super-imposed harmonics and vortex noises was a musical note of low pitch. This is the note normally heard by an observer at a relatively great distance. The unpleasant frequencies are filtered out by the intervening atmosphere. This effect was produced electrically to conclude the demonstration.