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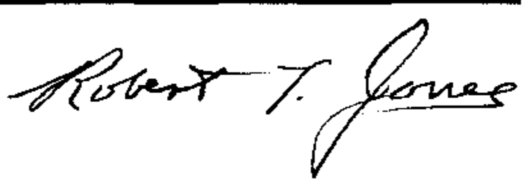

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RECOLLECTIONS FROM AN EARLIER PERIOD IN AMERICAN AERONAUTICS

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Nineteen twenty-nine was an exciting year for American aeronautics. The flood of World War I surplus airplanes was receding, and manufacturers all over the US were bringing out new models to capture an assured market. Aviation was going to have a bright future; its commercial success was expected to follow that of the automobile. The only question was who would be the Henry Ford of the new era.

All during the late twenties the weekly magazine *Aviation* appeared on the local newsstand in my hometown, Macon, Missouri. *Aviation* carried technical articles by eminent aeronautical engineers such as B. V. Korvin-Krovkovsky, Alexander Klemin, and others. Included in both *Aero Digest* and *Aviation* were notices of forthcoming *NACA Technical Reports* and *Notes*. These could be procured from the Government Printing Office usually for ten cents and sometimes even free simply by writing NACA Headquarters in Washington. The contents of these reports seemed much more interesting to me than the regular high school and college curricula, and I suspect that my English teachers may have been quite perplexed by the essays I wrote for them on aeronautical subjects.

Leaving the University of Missouri after one year, I took a job with Charles Fower, who operated a flying circus based at Macon. Fower and his wife, Marie Meyer, had for several years flown Standard J-1 airplanes at county fairs and exhibitions throughout the Middle West. Now, early 1929, only one Standard was left and it was in rather poor shape with tattered fabric and a leaky radiator. I patched the wings using cotton from the local dry goods store and on a calm evening we took off for Marshall, Missouri, to get a new set of wings from the Nicholas-Beazley Airplane Company.

Serving as crew for the Marie Meyer Flying Circus, I was supposed to see that the new wings were properly fitted. After the job was done and all the multitude of wires were tight we took off to test the rigging. On the last bounce before becoming airborne the outboard, forward interplane strut snapped outward in an extreme curve and the leading edge of the wing drooped precariously. Fortunately, the Standard has large powerful ailerons, and these together with the rudder enabled us to circle slowly and make a wheel landing. Though the information may no longer be of service, it seems that in rigging a two-bay biplane one should start at the center and work outward. Tightening the inboard wires last may place an undue strain on the outer bays.

When we arrived at Marshall, Nicholas-Beazley was just ready to start production on an advanced three-passenger airplane designed by Walter H. Barling, a well-known World War I British aeronautical engineer and designer of the famous "Barling Bomber." In spite of my somewhat questionable performance as crew for the Standard, Fower recommended me to Mr. Nicholas, and I started to work on the new production line at Nicholas-Beazley.

The introduction of the Barling NB-3 was accompanied by considerable fanfare. Somehow, Nicholas-Beazley had persuaded all their suppliers to place ads simultaneously announcing the NB-3. Barling's design was a low-wing cantilever monoplane of all-metal construction (except for fabric covering). By what must have been extraordinary skill in structural design Barling had kept the empty weight of the airplane below 700 pounds, with the result that it performed well carrying three passengers with a 60-hp engine.

During the boom that preceded the financial crash, *Aviation* and *Aero Digest* started coming out in color and at one point listed nearly one hundred aircraft manufacturers. Airplanes were being made in Little Rock, Arkansas (*Command-Aire*); Lexington, Kentucky (the *Kentucky Cardinal*); and Colorado Springs

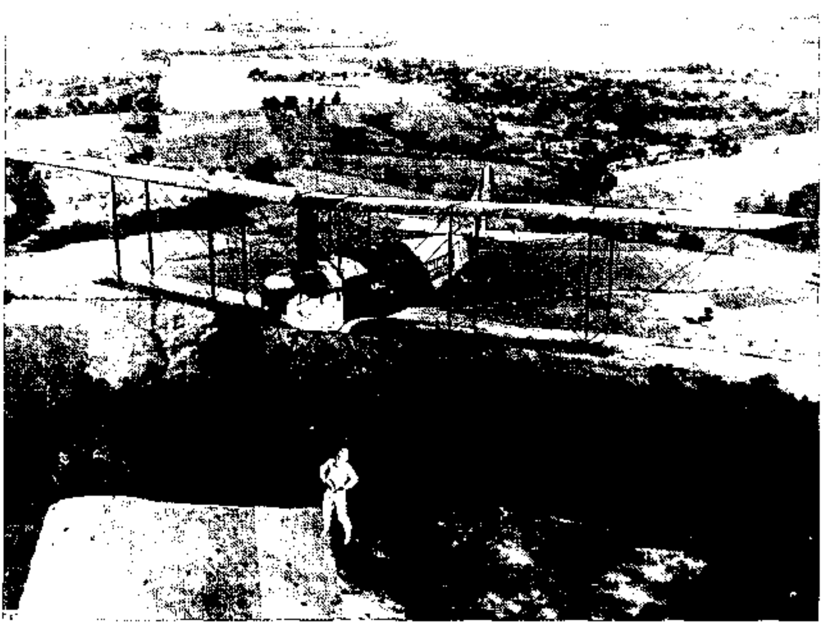


Figure 1 Bertie Brooks hanging by his teeth. OX-5 Standard.

(*Alexander Eaglerock*). Many of the designs were the work of enthusiasts rather than professionally trained engineers, and often a company had to rely on Washington-based consultants to overcome the hazards of the US Department of Commerce "Approved Type Certificate."

The wings of World War I trainers such as the Standard and the JN-4 were rather thin (6–7%) and required lots of bracing. It seems that the early wind-tunnel tests on which these designs were based were made at rather low values of the Reynolds number, a regime in which thin highly cambered sections show favorable properties. Later tests made at higher speeds with larger models revealed, however, that much thicker profiles such as the Clark Y, USA 35, and the G-387 could be used. Many of the aircraft designs of the 1920s simply took advantage of this knowledge and substituted a single-bay biplane with thicker profiles for the older two-bay designs. The cantilever Barling used a slightly modified Goettingen section approximately 18% thick.

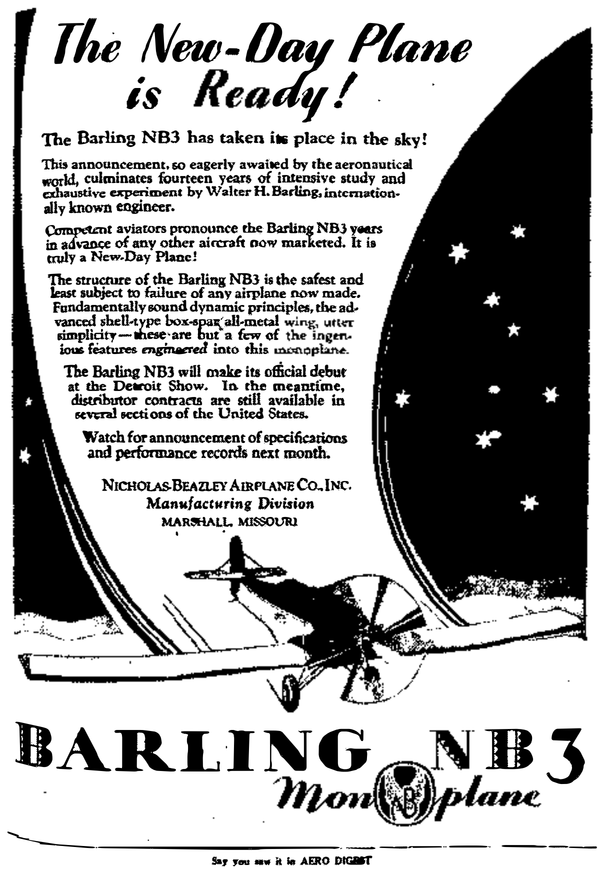
An outstanding design of that period was the *Alexander Eaglerock Bullet*. The *Bullet* was a very clean, low-wing cantilever monoplane with a retractable landing gear and the advertised ability to carry "four people and a dog." Unfortunately,

Figure 2 Charles Fower and Marie Meyer flying down Broadway in St. Louis, ca. 1926.

this remarkable airplane never reached quantity production. Flight tests disclosed a much dreaded phenomenon, the "flat spin" from which recovery was evidently impossible.

Beginning in 1920, NACA began collecting and disseminating in a uniform notation aerodynamic characteristics of airfoils from laboratories around the world. By 1929 NACA had published data on nearly one thousand different airfoil shapes. Each report ended with a series of summary plots intended to show which airfoils were "optimum" with respect to certain performance criteria. Unfortunately, the points on these plots scatter rather widely, no doubt because of varying conditions of the tests. Most of the tests were made at low Reynolds numbers and in tunnels with turbulent streams. In spite of the deficiency in the aeronautical laboratories of its day, the NACA collection was of great service to aircraft designers in providing at least an approximate quantitative idea of the behavior of different shapes.

The invention of the variable-density wind tunnel by Max M. Munk at the



ca. 1929

NACA Langley Laboratory essentially overcame the difficulty inherent in earlier wind-tunnel tests and permitted small models to be tested at full-scale values of the Reynolds number. At about the same time, Munk introduced a significant advance in airfoil theory in the form of a linearization which permitted the calculation of airfoil characteristics directly in terms of easily identified parameters of the shape. Previous airfoil theories of Joukowski, Chaplygin, and von Mises were more accurate (none included viscosity) but were considerably more complex, being based on the artful application of conformal mapping to derive special shapes. Von Mises' theory did indeed encompass all shapes expressible by a series of complex coefficients, but the shape is not explicit. At about the same time Munk introduced his theory for the air forces on an airship. Both the thin-airfoil theory and the airship theory may be thought of as theories based on extreme proportions, the airfoil thin and

Max M. Munk (1947)

slightly cambered and the airship long and slender so that the cross-flow in planes perpendicular to the long axis is approximately two-dimensional. These linearizations proved extremely valuable in later years when airfoil theory was extended to the near-sonic and supersonic speed ranges.

In modern times the importance of simplified theories seems to have been diminished by the prodigious capacity of the electronic computer. Approximate theories appear merely as poor substitutes for accurate calculations which may incorporate a multitude of complex interactions. I hope that the drive toward simplification in theoretical work will not be completely forgotten, however, since only in this way can we arrive at deductions that embrace a wide class of phenomena.

Although many airfoil shapes had been tested, there seems to have been little systematic variation of parameters. One series of Joukowski sections had been tested by Ackeret and Schrenk, but again the Reynolds number had been rather low. The completion of the NACA variable-density wind tunnel together with Munk's newly formulated thin-airfoil theory made possible the testing of a systematic series of wing sections at full-scale Reynolds numbers. Munk's analysis permitted him to derive shapes that in theory would have a stable center of pressure travel. Such airfoils have a slight upward camber near the trailing edge. (The effect of the reflexed trailing edge on airfoil stability had been discovered experimentally by W. R. Turnbull in Canada some twenty years earlier.) The new airfoils were named appropriately "M" sections. M-6 and M-12 had quite good characteristics, and I selected the M-12 for use in a small racer design while at Nicholas-Beazley.

Munk's work during this period received special recognition from Dr. Joseph S. Ames, who was chairman of the NACA, in a report entitled *Resume of the Advances in Theoretical Aerodynamics Made by Max. M. Munk* (*NACA TR No. 213*). Basic work in aerodynamic theory at NACA declined rather abruptly following Munk's departure from the Laboratory in 1926.

One project deserving to be remembered from this period is the Guggenheim Safe Airplane Competition. At that time it was believed that safe flight would depend on the ability to land slowly and take off from restricted areas. The Guggenheim competition required an airplane that could take off and clear a 35-ft obstacle in less than 500 ft. The maximum permissible landing speed was 35 mph, and the landing run in still air could not exceed 100 ft. Moreover, the airplane was required to demonstrate stable flight in gusty air for five minutes with hands off the controls at all throttle settings from 45 to 100 mph. At least two entries, one by Handley Page of England, were able to meet these requirements. The contest was won by the Curtiss Tanager, a cabin biplane having full-span slots and flaps, with lateral control provided by free-floating tip ailerons extending out from the lower wing.

The years just preceding the depression of the thirties have been characterized as an era of wild speculation. To me it seemed more an era of high activity and enthusiasm. At Nicholas-Beazley we were for a short time building and selling a plane every day. The NB-3 set several altitude and distance records. We stayed up late nights in the small cafes in Marshall designing "flivver" airplanes to be stamped out of metal. For a period of several months, I worked from early morning until midnight in the N.-B. engineering office designing a small racing plane for the 1930 air races.

The loss of confidence following the 1929 crash put a stop to all this activity. Only a few of the aircraft companies (e.g. Cessna, Beech) survive from this period.

Following the election of Roosevelt in 1932, concerted efforts were made to revive the failing aircraft industry. Early in 1933 James A. Farley issued the following announcement: "By authority of President Roosevelt's Executive Council, a bulletin is being sent to the head of every executive agency of the Government directing the use of air mail for all but the most urgent Government messages."

Among the imaginative "New Dealers" appointed by Roosevelt was Eugene Vidal, Director of Aeronautics, Department of Commerce. Vidal believed, as many of us did, that the progress of aeronautics would depend on the development of an inexpensive small airplane for individual use. Vidal instituted a design competition for a "\$700 light airplane." Vidal's specifications were remarkably close to our earlier Barling monoplane which cost \$3500. Presumably, real mass production could reduce this figure. The idea created considerable interest in aviation circles—not all favorable—and brought out some ingenious designs. One of these, by the well-known engineer Waldo Waterman, called the *Arrowmobile*, was an all-wing arrangement having considerable sweepback with weathercock stability provided by vertical fins at the wing tips. The winner of the competition was a more conservative Hammond design. It seems that the Government had agreed to buy a certain number of prototypes of the winning design at approximately \$3000.

To many people deeply involved in the problems of the day, the idea of spending money for aeronautical research seemed wasteful. One of the ideas used to sell the virtues of aeronautical development in the period was the concept of "spin-off." It was claimed that the stimulation of aeronautical activity would in the long run lead to more visible practical benefits to the taxpayer. I do not recall a specific claim that a better frying pan would result, but a potential 25–30% reduction in the weight of the automobile was mentioned.

NACA and its Langley Laboratory suffered badly in the depression but managed to survive. I have been told that at one point a bill was introduced in the Congress to abolish the NACA. Mr. John F. Victory, NACA's Executive Secretary, fortunately kept close watch on such legislative happenings and within hours had summoned enough support in the form of telegrams from aircraft manufacturers all over the US to defeat the measure.

In 1932, the NACA budget reached a peak of about one million dollars. By 1934, this had declined to \$690,000, and NACA employees had voluntarily accepted a 15% reduction in salary. By any analysis, the level of activity that was sustained by these limited budgets seems remarkable. There was at the Langley Laboratory a "full scale" 30′ × 60′ wind tunnel, a 20-ft-diameter propeller tunnel, the 20-atmosphere variable-density wind tunnel, a mile-long seaplane towing basin, an active flight research section, and several smaller wind tunnels.

To cope with widespread unemployment, the Public Works Administration under Harold Ickes opened up a number of temporary scientific positions in Government. In late 1934, I was thus enabled to secure a temporary (nine months) appointment as scientific aide at the Langley Laboratory of NACA. My first assignment was in the 7′ × 10′ Atmospheric Wind Tunnel with Carl J. Wenzinger, Thomas A. Harris, Robert Platt, and others under Fred E. Weick, who was Assistant Chief of Aerodynamics.

Our main task in the 7′ × 10′ wind tunnel was the development of high-lift devices and lateral controls to improve the safety of flight at low speeds. Most accidents had resulted from stalls and spins, especially during forced landings. Many of the engineers at Langley were pilots and hence were acquainted with the practical as well as the theoretical aspects of such problems.

Fred Weick had built (with assistance from the wind-tunnel group) a stall-proof airplane called the "W-I." The W-I was a high-wing monoplane with an ungainly looking fixcd slat supported ahead of the leading edge and extending from tip to tip along the wing. On one occasion, the engine failed just after takeoff, and Fred had to bring it down in the NACA tennis court. The W-I never flew again but fortunately Fred was unhurt.

One of Weick's ideas for improving the safety of landing was the tricycle landing gear. The conventional "tail-dragger" with fixed wheels ahead of the center of gravity was, of course, inherently unstable. By placing the main wheels behind the center of gravity and allowing the front wheel to pivot, stability could be achieved. In spite of its simplicity the idea encountered some resistance, and one of my first tasks was to try to show from dynamical calculations that passengers in the rear of an airplane would not be thrown out of their seats when the airplane pitched down onto the nose wheel.

A persistent fault of the tricycle gear was shimmy of the nose wheel. Weick gave this problem to Arthur Kantrowitz, who had recently joined the staff. By experimenting with models, Arthur found that a swiveling nose wheel could actually shimmy at essentially zero forward speed, i.e. in a purely kinematic way due to a

Fred Weick

Henry J. E. Reid

characteristic mode of distortion of the tire. Dynamic shimmy could be prevented by allowing the wheel to slide laterally on a slightly curved axle—a very neat solution, I thought. The designers, however, seemed to prefer a more direct approach, using hydraulic dampers.

In the early days of flying, inherent stability was of the utmost importance, and *NACA Technical Report No. 1* by Jerome C. Hunsaker and E. B. Wilson (1915) was devoted to this subject. Under Fred Weick's guidance, these studies were continued in the thirties at Langley. Following a tendency that persists even to this day, we did not read the earlier work carefully enough, and I am afraid that we repeated some of the mistakes that had been made and corrected years before. Thus, in calculating the stability coefficient N_p, the yawing moment due to rolling, I made what I thought was a very clever use of Munk's theory of the twisted elliptic wing. According to my theory, the downgoing wing developed more lift and, hence, more drag. Charles Zimmerman eventually noticed that my value of N_p had the wrong sign. The downgoing wing pulls forward not back; and when I took account of the correct resolution of velocity vectors, the forward thrust appeared. It seems that E. B. Wilson had, in 1918, found this error in earlier work of J. C. Hunsaker and L. Bairstow and wrote *NACA TR No. 26* to give the correct version of the theory.

In the early thirties, the Langley Laboratory was the acknowledged US center of aeronautical research. Much of what I had learned about aeronautics had been gleaned from *NACA Reports* and *Technical Notes*. I could hardly have wished for a better fortune than to find myself among these engineers who were so involved in the advancement of the art. Because of this, it is perhaps difficult for me to make a purely objective assessment, but others have confirmed my impression of the extraordinary group at Langley. At that time, the inflation of the language had not yet reached the point where we were called "scientists." Even the director of the Laboratory, H. J. E. Reid, was termed "Engineer in Charge." I, of course, did not even qualify as an engineer; and sometime later when it appeared that the lowest professional or engineering grade called for a certain academic preparation, it was necessary for me to take the next higher grade where the academic requirement, though presumed, was not mentioned.

Eastman N. Jacobs, one of the most skillful and innovative American aerodynamicists, had come to Langley in 1925 and his activities were invariably a center of interest there. Jacobs had a wide appreciation of science but did not devote much time to theoretical studies. Rather, he used his theoretical understanding to devise intelligent experiments. Thus, in 1932, he and James M. Shoemaker tested thrust augmentors for jet propulsion. Many years later, after Campini's jet airplane had flown, Jacobs was instrumental in encouraging work on jet propulsion at Langley. Jacobs is, of course, best known for his development of the low-drag laminar-flow airfoil.

As noted earlier, several of the engineers at Langley devoted their spare time to building and flying airplanes. Soon after coming to the lab, Jacobs built a small monoplane powered by an Ace motorcycle engine. Having a one-wheel landing gear, the design was not highly regarded by the professional pilots at the lab. However, Jacobs took off in this machine for his first solo flight. It seems that

Jacobs' flight plan was disclosed prematurely to the laboratory personnel and they gathered around the small grass field to see what they were sure was an impending crash. Evidently, the presence of this audience only brought out Jacobs' best skill, and it is said that he made a perfect landing.

Hurricanes were rather frequent along the Virginia coast, and the one that struck in 1933 nearly destroyed the laboratory. By that time, Jacobs had acquired a Pitcairn biplane (*Mailwing*) and happened to be in Norfolk when the hurricane struck. He tied the airplane down hard with its tail into the wind and waited for the center of the hurricane to arrive. As soon as the wind abated slightly, he took off, following the "eye" of the hurricane until he reached higher ground where he landed and in his words "saved the airplane."

Flying at Langley was often a mixture of fun and aerodynamic experiment. Robert Platt, with whom I worked, maintained a World War I Fokker D-VII which had beautiful flying qualities and two seats. He and I used it frequently to test ideas of stability and control. In one of our experiments we determined that the airplane remained stable and controllable at extreme angles of sideslip.

Following Munk's departure in the late twenties, theoretical work at the laboratory had declined, but was revived somewhat later by Dr. Theodore Theodorsen, Carl Kaplan, and Edward Garrick. In his *Theory of Airfoils of Arbitrary Shape* Theodorsen found a way to determine the von Mises coefficients by successive approximation starting with any two-dimensional or cylindrical shape.

When Eastman Jacobs discovered that the maintenance of laminar flow depended on a prescription of the pressure distribution rather than the shape, he disappeared from the laboratory for several days, and one day he called me over to his house to help him unravel Theodorsen's theory. We decided it could not be used that way

Eastman Jacobs

Theodore Theodorsen

and I devised a simple extension of Munk's theory to serve this purpose. Thin-airfoil theory proved too inaccurate, however, and H. J. Allen developed a more satisfactory theory based on a linearization that started from a Joukowski airfoil having some thickness.

One of the most important contributions of the Theodorsen group was the theory of oscillating airfoils with hinged flaps—related to the problem of flutter. Garrick subsequently extended this theory to cover propulsion of a flapping airfoil. Some time later, I became interested in the extension of this theory to the three-dimensional wing.

Eastman Jacobs represented the laboratory at the 1935 Volta Congress on high-speed aeronautics. Following his return, during lunch-time conversations he and Arthur Kantrowitz tried somewhat unsuccessfully to explain the principles of supersonic flow to me. Being familiar with Laplace's equation and its smooth streamlines, I found it difficult to believe that the streamlines could make sharp bends at Mach waves. Quite a few years later, I found a way to make the supersonic streamlines smooth by sweeping the leading edge of the wing behind the Mach cone.

It was at the 1935 Volta Congress that Busemann had introduced the idea of sweeping the wings to diminish the wave drag at supersonic speeds. Busemann utilized the "independence principle" but kept the wing ahead of the Mach cone so that the cross-flow was still supersonic. Evidently, Busemann had put too many ideas in this one paper, for neither Jacobs nor von Kármán remembered his suggestion when I proposed sweeping the wings some ten years later. In my version, the wing was swept behind the Mach cone to get a purely subsonic type of flow and thus to eliminate the wave drag entirely for infinite aspect ratio. For the independence principle, I had relied on an earlier paper of Munk entitled *The Relative Effects of the Dihedral and the Sweepback of Airplane Wings* (*NACA TN 177*, 1924). Fortunately, before my paper was published, Robert Hess at Langley found Busemann's earlier paper and I was able to refer to it.

The first tests of swept wings at Langley were made by Robert Gilruth, M. C. Ellis, and Clinton Brown. Ellis and Brown tested the independence principle by placing a length of streamlined wire in their supersonic tunnel. Gilruth obtained the first accurate results by attaching wings to a body dropped from a high altitude.

It is not widely known that the first experiments in the US designed to produce power from thermonuclear fusion were initiated at Langley some time before the Manhattan atomic bomb project. The Langley experiments were the idea of Arthur Kantrowitz and Eastman Jacobs, who made use of earlier theoretical work on fusion by Hans Bethe. It is interesting that Kantrowitz and Jacobs attempted to initiate fusion by magnetic confinement of a plasma in a toroidal field—a technique often seen in more recent attempts. By arguing that fusion power could become important for aircraft propulsion they were able to secure an appropriation of $5000 to carry on the work. Both Kantrowitz and Jacobs spent many hours glass blowing and constructing coils, and it is said that during the final test one of them held in the circuit breakers of the variable-density-tunnel power supply to get more current. Unfortunately, the experiment was defeated by the stubborn (and still persistent) tendency of the plasma to become unstable.