1970 Von Kármán Lecture

Development of Airplane Stability and Control Technology

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1. Preface

THEODORE von Kármán was a giant who dominated the aeronautical and aerospace scene for half a century. He will be remembered for many accomplishments, any one of which would have made him famous. He was a great teacher. His many famous disciples and students attest to his profound influence on the young men who studied and worked with him. Wherever he went throughout the world he was surrounded by his students who obtained inspiration from him throughout his life. He was an amazingly productive scholar. His great output of original works include contributions to the fields of structures, aerodynamics, combustion, and propulsion. He was a productive scholar of great breadth and continued so up to the day of his death at the age of 83. He was also a great organizer. His aeronautical schools at Aachen and at the California Institute of Technology are monuments to his genius and continue today to operate in the environment of excellence that he instilled when he founded them. He will also be remembered for his military-scientific organizations on which he spent most of his time and energy in the last years of his life. The powerful U.S. Air Force Scientific Advisory Board and the NATO, Advisory Group for Aerospace Research and Development (AGARD) continue today to operate with his stamp on them. Only von Kármán could have integrated the SAB at the very top level of the Air Force, and only von Kármán could make AGARD the truly international organization that it remains today.

I consider myself very fortunate to have worked with Dr. von Kármán in several of his organizations, and I always have felt his great power and unique capability to do things impossible to others. One of these experiences was when I was asked to take over as Chairman of AGARD shortly after his death. We put U.S. Air Force Colonel G. Munroe into the Paris office as Acting Director—to size up the situation and find out where we stood. He called me in Princeton within several days saying that our problem could be simply stated. AGARD seemed to be illegal in the NATO structure and was also broke. Only von Kármán could run a successful operation under such handicaps.

Von Kármán was an extraordinary person and all of us who worked with him and were influenced by him consider ourselves lucky indeed. It is a great pleasure and honor for me to participate in this lecture series erected in his memory.

It is my purpose in this paper to look back at man's first struggle to fly and trace the development of the technology of stability and control. In putting this study together it became apparent that the subject is very difficult to compress into a paper of acceptable length. I have therefore chosen to concentrate on a few aspects of the subject that I consider to have been particularly important to the development of the technology.

In my effort to highlight significant developments in the field of stability and control, I have chosen the following as of great importance: 1) the early experimentalists, Lilienthal and the Wright Brothers; 2) the applied mathematicians, Landhester, Bryan, Bairstow, and Jones; 3) the first wind-tunnel experimenters, Bairstow, Jones, and Hunsaker; 4) flight research—flying qualities, Warner to Gilruth; 5) compressibility phenomena, P-47 and the research airplanes; 6) aeroelastic phenomena, B-47 and the supersonic transport.

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2. Introduction

Before embarking on a study of the development of airplane stability and control technology, it might be well to review some elementary facts on the interrelationship between stability and control. Inherent aerodynamic stability of aircraft systems is their tendency to maintain an equilibrium once established. Control is that capability that permits changes in the equilibrium to alter the flight path at the command of the controller. The controls can have a further use, and that is to provide or augment the stability of the system through feedback to the controller, either a human being or an automatic control. The nature of the requirements on inherent stability is inevitably tied in with the dynamics of the system to be controlled and the dynamics of the controller. It took a long time for this interrelationship to be understood.

The first experimenters in aerodynamic design worked with uncontrolled hand launched gliders, and rapidly discovered that they had to have an inherently stable system for success. The first notions of stability came from men such as Zahm in this country, Lanchester in England, and Penaud in France. They developed configurations that could achieve stable free flight. Zahm was one of the first to correctly state the case for static longitudinal stability. He did this in a paper written in 1893.[1] He showed that a configuration with "longitudinal dihedral" or with a tail set at a negative incidence with respect to the wing would give a "stable equilibrium for an aeroplane gliding down an inclined course with uniform velocity." Zahm depicted the situation as shown in Fig. 1.

In today's language, he found that a positive moment at zero lift would require a forward center of gravity location for equilibrium and this equilibrium would be statically stable. We would show this today as a $C_m - C_L$ curve as shown in Fig. 2. Besides the requirement for longitudinal dihedral and static longitudinal stability, these early glider experimenters found that they had to provide rolling moment due to sideslip. Zahm obtained his through wing dihedral angle, and Lanchester[2] obtained this through an interesting double fin arrangement that provided rolling moment because of the sideslip with a net, if small, direc-

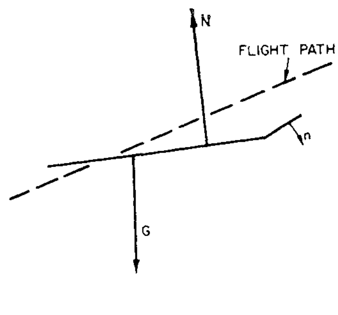


Fig. 1 Zahm's condition for static longitudinal stability.

tional stability (see Fig. 3). These experimenters knew nothing of the basic dynamics of their gliders but found that they could be flown successfully if properly balanced, carefully launched, and if provided with these elements of stability.

As man approached his first experiments in human flight, there were these notions of statically stable equilibriums, no knowledge of the natural frequencies involved and very little experience with control. The early attempts were dominated by two superb experimenters. O. Lilienthal in Germany and the Wright Brothers in the United States. Both started out with glider experiments. Lilienthal with gliders that were stable, but provided with weak control and the Wright Brothers who knew very little about stability but recognized that powerful controls would permit them to maintain the desired equilibrium. Both could have learned from the other, but in the long run the Wright Brothers' approach led straight towards success.

The Lilienthal gliders probably had slight static longitudinal stability and were controlled by the movement of the pilot's body that was suspended from arm holds. One can only feel that Lilienthal was mostly along for the ride and the control that he could provide was small compared to possible upsetting moments introduced by turbulence, or for the control of the natural dynamics of his system. Lilienthal made many successful glides before he was killed in an accident in which his glider was upset by a gust. Although Lilienthal achieved a certain success, he never would have succeeded until he had solved his control problems in a more powerful way. The following Fig. 4 shows a typical Lilienthal glider. Note the longitudinal dihedral or the tail incidences relative to the wing incidence that indicates a longitudinally stable configuration.

The Wright Brothers approached the problem from the outset with the belief that powerful controls were mandatory and that the pilot through use of these controls could establish and maintain equilibrium. They probably knew little of inherent stability but found that under the conditions of their flights, they could learn to fly their configurations, successfully.

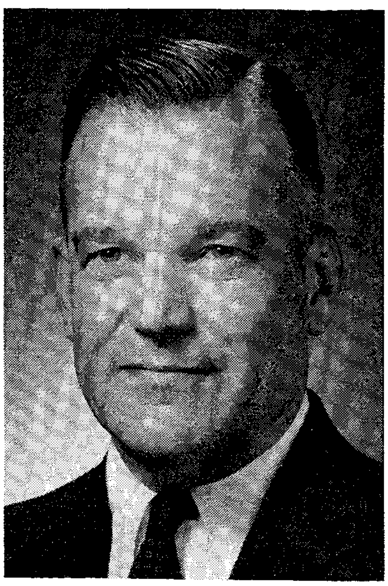
3. Wright Brothers[3,4]

When one studies the early struggles to achieve powered manned flight, one is impatient to get on with the Wright Brothers. This pair of expert mechanics and careful experimenters were able to put together the crucially important elements of manned flight and in spite of many unknown factors were able to work their way around their problems and achieve success. They were confident of what they did know and very realistic and careful about what they did not know. I would like to give the following quote of Wilbur Wright made in a speech in 1901.

> The difficulties which obstruct the pathway to success in flying machine construction are of three general classes: 1) those which relate to the construction of the sustaining wings, 2) those which relate to the generation and the application of power required to drive the machine through the air, 3) those relating to the balancing and steering of the machine after it is actually in flight. Of these difficulties the first two are already to a certain extent solved; but the third, the inability to balance and steer, still confronts students of the flying problem. When this one feature has been worked out the age of flying machines will have arrived, for all other difficulties are of minor importance.

This was probably the first time that an airplane designer had so clearly delineated three of the crucial fields of aeronautical engineering—structures, propulsion, and stability and control. The Wright Brothers soon demonstrated their ability to develop a sound structure, a remarkable internal combustion engine, and effective propellers. They obtained aerodynamic data on lift and drag of wings through careful gliding tests and later from their own wind-tunnel experiments. They were deeply concerned over the problem of equilibrium and steering and recognized this as a crucial and dangerous problem. Some historians of the Wrights claim that they made their configurations unstable on purpose. From my study of their letters I can only conclude that they didn't understand what stability really meant.

That they succeeded is because of their recognition of their shortcomings and their step by step approach to the problem of flying so as to minimize their dangers. They knew they



Courtland D. Perkins

Dean Perkins was born in Philadelphia, Pa. on December 27, 1912. He was graduated from Swarthmore College in 1935 and received his M.S. degree from the Massachusetts Institute of Technology in 1941. During World War II, he headed the Stability and Control Unit of the Aircraft Laboratory, U.S. Army Corps, at Wright Field. He was active during this period improving the sophistication of flight test and flight research operations within the Armed Forces, and had a hand in the creation of the first Air Force Test Pilot School now active at Edwards Air Force Base, Calif. Immediately following the war, Dean Perkins and a Wright Field colleague published a fundamental book entitled *Airplane Performance, Stability and Control.*

After leaving Wright Field in 1945, Perkins joined the Princeton University faculty where he has held the rank of Professor since 1947. He was appointed Chairman of the Aeronautical Engineering Department in 1951, and twelve years later was appointed Chairman of the newly formed Department of Aerospace and Mechanical Sciences. In the fall of 1964, he was named Associate Dean of the School of Engineering and Applied Science.

During his tenure at Princeton, Dean Perkins has twice taken leaves of absence to serve the Department of Defense. In 1956–1957, he served as Chief Scientist of the U.S. Air Force, and in 1960–1961 was Assistant Secretary of the U.S. Air Force for Research and Development. Since returning to Princeton, he continued to serve the Air Force as Vice Chairman of its Scientific Advisory Board and in May 1969 was named Chairman. In 1963, he was elected Chairman of the Advisory Group for Aeronautical Research and Development (NATO) and served in this capacity until 1967; he continued to serve as a U.S. National Delegate to this organization until 1969. In 1964, he became a member of the Space Science Board of the National Academy of Sciences and was also President of the AIAA that year. He was made a member of the Defense Science Board in May 1969. He serves on the Board of Directors of Fairchild-Hiller Corporation, American Airlines Inc., The Mitre Corporation, and Keuffel and Esser Company.

Dean Perkins is a Fellow of AIAA, the Royal Aeronautical Society, and a member of the American Helicopter Society and Sigma Xi. He was elected to the National Academy of Engineering in 1969.

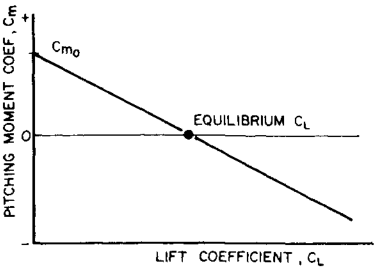


Fig. 2 Statically stable longitudinal equilibrium.

needed long flying times and low ground speeds—so they chose the high steady winds of Kitty Hawk, North Carolina, for their experiments. They knew they would have, and they did have, many accidents, but their rugged structure plus the low true speeds and altitude kept the damage to a minimum, and they were always able to make repairs rapidly. Through tests of unmanned gliders flying as kites and then man controlled in free flight, they slowly solved the problem of equilibrium and steering and ultimately taught themselves to fly. Although they knew little about inherent stability, they found that through practice they, as pilots, could stabilize their system to maintain a steady flight equilibrium.

The Wright Brothers discovered the following fundamental facts: 1) Longitudinal balance could be obtained through a horizontal rudder that changed the glider center of pressure with respect to the center of gravity. This was a better way to do it than to move the c.g. under the c.p. through body motions as did Lilienthal. 2) They realized that lateral balance could best be achieved through changing the spanwise lift distribution through warping the wings. This was the basis for many of their patent fights some years later. 3) They found that the airfoil data available to them were in error and developed their own. They found that cambering the airfoil would increase their lift, but that for low angles the c.p. moved aft as the angle of attack was decreased. They had assumed it would be the reverse. 4) They soon found that the application of rolling control introduced adverse yawing moments and geared a small rudder to the lateral control to offset this. 5) They placed their horizontal rudder (elevator) out front, accepting longitudinal instability and, at first, an overbalanced longitudinal control.

In today's language, the Wrights were flying longitudinally unstable machines with, at first, an overbalanced elevator. They were flying machines that had about neutral directional stability, negative dihedral and with a rolling control that introduced yawing moments. We can only conclude that the low speed and wing loadings, their robust structures, plus careful practice made their success possible. From their own descriptions of their flights they were overcontrolling most of the time, and many flights ended when at the bottom of an oscillation their skids touched the sand. A drawing of the developed Wright "Flyer" is shown in Fig. 5.

In spite of these stability difficulties, the Wrights soon balanced their horizontal rudder correctly, and reduced the airfoil camber which permitted a forward shift of the center of gravity and a reduced instability. The forward horizontal tail was held onto in spite of its shortcomings, perhaps as a design signature even though they realized that eventually they should move it aft. Orville Wright in a letter to Wilbur as late as 1909 states: "The difficulty in handling our machine is due to the rudder (horizontal tail) being in front, which makes it hard to keep on a level course. If you want to climb you must first give the front rudder a larger angle, but immediately the machine begins to rise you must reverse the rudder and give a smaller angle. The machine is always in unstable equilibrium. I do not think it necessary to lengthen the machine but to simply put the rudder behind instead of before." It was only for their latest machines that they capitulated and put the tail in the rear.

The Wright Brothers' experiments were conducted in the remote sand dunes of Kitty Hawk, and as they were not interested in publicity very few people knew of their activities. The news of their success broke upon an unsuspecting world, astounded other experimenters, and left many with the feeling that it must have been a hoax. When the French first heard that the Wrights had achieved powered flight with an airplane with a tail in front they called the whole story a "canard," French for a tall tale. We use "canard" today when referring to tail-first configurations. The Wrights were not completely believed until Wilbur took one of their airplanes to Europe in 1908 and made many spectacular flights. It was obvious then that they were far ahead of their colleagues and were finally given world acclaim. Shortly thereafter they became completely embroiled in patent suits that made enemies of many of their previous friends. Some felt that at this period the Wrights and their suits impeded the development of the airplane and wished they had spent more time pushing forward the technology of manned flight.

These remarkable brothers showed the breadth of their competence when they actually prosecuted their patent suits themselves. They confounded the scientists and engineers that were brought in as expert witnesses to testify against them and won all of these suits. The results of these patent actions made the Wright Brothers wealthy, but while all this was going on the leadership in aeronautical design rapidly passed on to others.

The Wright Brothers then burst upon the scene with a brilliant program that solved many problems directly and

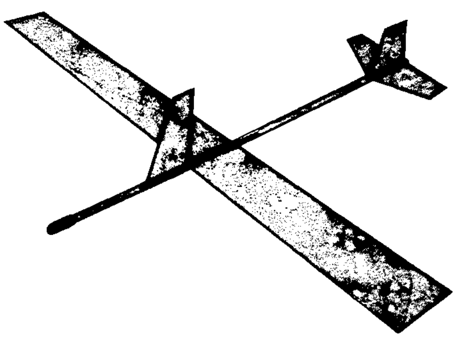


Fig. 3 Lanchester glider.

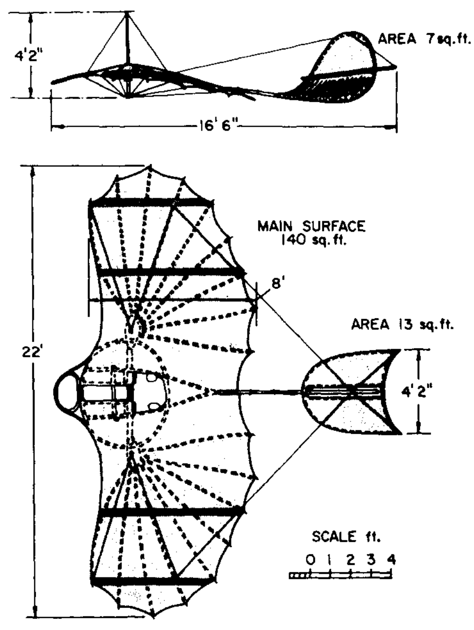


Fig. 4 Lilienthal glider.

permitted them to work around their unknowns. As to stability and control, they were deeply conscious of the problem of "balance" and "steering" and sought powerful controls. They found that the dynamics of their airplanes were such that they, as controllers, could stabilize their system through practice and failed to understand how they could be made easier to fly through design for inherent stability.

In the years following the Wright Brothers' success, many designers became active and a series of new designs and new names came to the front—Bleriot, Santos-Dumont, Farman, Curtiss, and many others. It was the era of "Those Crazy Young Men and Their Flying Machines." Most designers put their horizontal tail behind the wing—provided wing warping or ailerons for lateral control—had little inherent stability, if any—and many were killed as they discovered the stall and spin.

4. Applied Mathematics

The mathematical aspects of the problem of flight was first suggested in a paper by Bryan-Williams in 1903. Lanchester in 1908 published his "Aerodonetics" in which he described his early experiments with hand launched gliders and then with some novel mathematics defined the longitudinal oscillations that he observed. Others made contributions but it was the British applied mathematician, G. H. Bryan, who first set up the problem in a rigorous way and presented the equations of motion in the form that has been familiar to us ever since.

It is difficult to categorize Lanchester as an experimenter or as an applied mathematician—he was something of both, and played an important role in the early development of the airplane. It was mentioned earlier that Lanchester, with others, discovered the requirements for static stability and empirically evolved glider configurations that would fly smoothly and steadily if launched with precision. The first insight into the airplane's natural frequencies was obtained when he observed the uncontrolled motions of his glider when not launched precisely or if disturbed by turbulence.

Lanchester noted that, "if the glider was launched at the exact or natural speed, it would glide steadily as if on rails." If launched at either a higher or lower speed, the flight paths

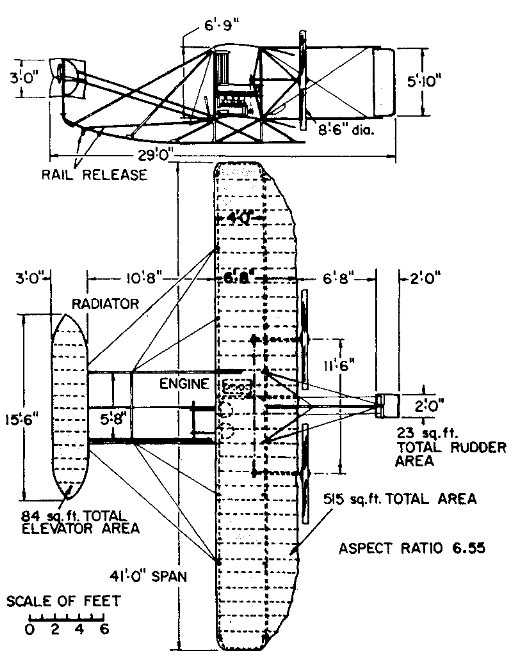


Fig. 5 Wright biplane.

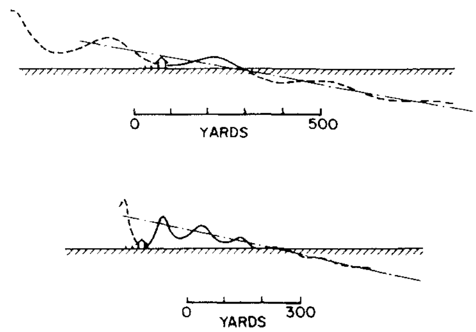


Fig. 6 Lanchester's glider experiments.

would appear oscillatory. This was shown by Lanchester as sketched in Fig. 6.

Lanchester observed that these trajectories were parts of more complete flight paths that were weakly damped rather long-period oscillations. He deduced that this involved an interchange of kinetic and potential energy and with this start, set up his equations to describe the motion. He assumed that the lift was always perpendicular to the flight path and ignored the damping in pitch and inertia about the pitch axis. The solutions gave the long-period oscillation that he termed "phugoids" and we have been stuck with this term ever since.

Lanchester having discovered his phugoids tried to rationalize that these oscillations were important to the design of airplanes and felt that improper attention to them was a major cause of the many accidents befalling the intrepid flyers of that day. He tried to prove that Lilienthal's fatal crash was because of the inadequate phugoid characteristics, but he admitted later on that it was more likely the result of an inadvertent stall.

As mentioned earlier, it was the British applied mathematician, G. H. Bryan,[5] who placed the whole problem of airplane dynamics on a more rigorous mathematical basis in 1911. Bryan introduced the concept of "stability derivates," broke the total airplane motion down into "symmetrical" and "rotative" components, and uncovered the nature of the airplane's natural frequencies.

Bryan made some very cogent points. One, that up to that time, the mathematical approach had received little attention because of 1) the complexity of the mathematics and 2) the airplanes of the day seemed to be able to be flown without such knowledge. He speculated correctly that pilots could fly unstable machines through practice. He writes: "If the design involved instabilities its effect could be overcome by skill and experience, and in particular (for the Wrights) a lateral control obtained by warping the wings." He felt that too much time had been spent getting around the Wright patents, and this directed attention away from the question of inherent lateral stability.

Bryan, the applied mathematician, recommended this field to his colleagues in 1911. He observed: "To the mathematician who is able to devote any time to original work, a wide region of unexplored ground is opened up, of which it has, in many cases, only been possible to exhibit glimpses. There is a freshness about this region which can scarcely be found to the same extent in other fields that involve differential equations that have not been integrated." He recommended "that there is abundant work, too, for the student who wishes to undertake research for educational purposes and airplane stability seems to me to be a very useful alternative for some branches of applied mathematics now taught in our universities."

Bryan's work was the real foundation for subsequent mathematical approaches to the problem of airplane dynamics. Bairstow and M. Jones, of the National Physical Laboratory, finally brought these equations into the dimensional

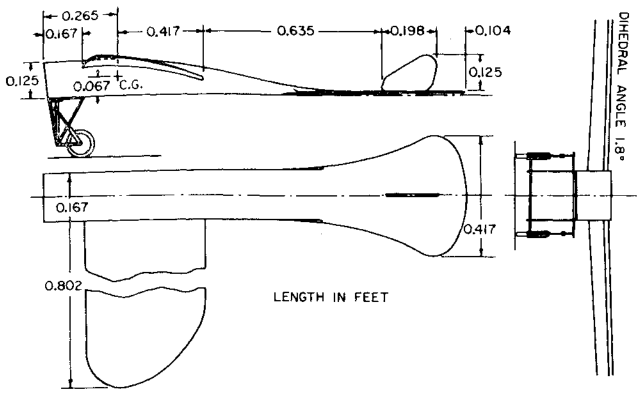


Fig. 7 Model Bleriot monoplane.

form so familiar to us all. Since then many researchers have nondimensionalized these equations in many ways and studied them extensively. Bryan's call for more interest in this general area of applied mathematics did not go unheeded.

Bairstow and Jones set up their equations and showed that there were independent longitudinal and lateral solutions. The solutions to these equations gave the now familiar results:

1) *Longitudinal*: For statically stable airplanes, the motion has two natural frequencies, a heavily damped short-period mode and a weakly damped long-period mode. They ignored the short-period mode and concentrated on the long-period mode which they found to be the famous "phugoid" of Lanchester. They were somewhat surprised to find that it was quite simple to obtain longitudinal stability. They made no attempt to analyze these motions for statically unstable airplanes or for airplanes with neutral stability, the condition under which most airplanes were flying at that time and which they recommend later on.

2) *Lateral*: Their solutions indicated the now famous "Dutch roll" mode, the rolling convergence and the spiral divergence. They found that stability of the spiral mode required reductions in directional stability and an increase in dihedral effect. They found that this weakened the damping of the oscillatory mode and they were unsure of what was to be recommended to the designer.

It is interesting to note that the most sophisticated researchers who answered the call of Bryan became fascinated with the mathematics and the solutions to these equations of motion. Many feel that the real problem was lost in the welter of such solutions, and it was many years before these equations were put to important use.

The basic dynamic motions of airplanes were identified at a very early date, but little was done to correlate them with actual flying experience. It was very difficult for these men to understand how machines like the Wright's that violated all of their requirements for stability flew quite well and didn't seem to know the difference. It took many years to straighten this out, as we shall see.

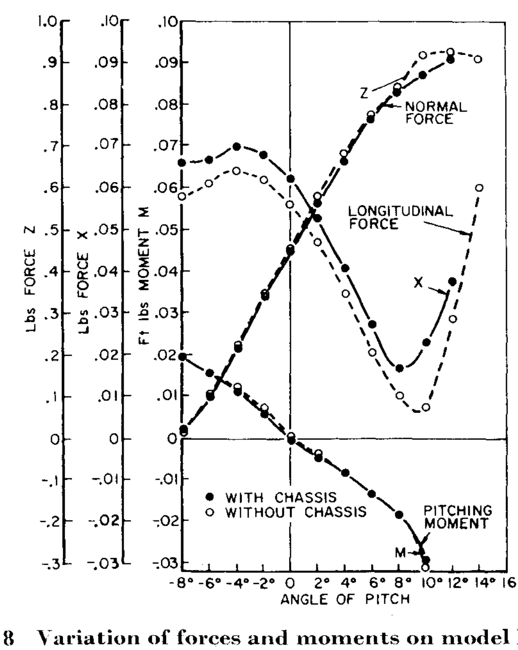


Fig. 8 Variation of forces and moments on model Bleriot type monoplane with angle of pitch.

5. Model Testing

By 1910–1912, a large number of airplane designers were pushing forward the various technologies of airplane design and aviators were learning to fly the various configurations that evolved—many were killed in the process. Researchers of that day were concerned that the design of airplanes for stability was inadequate and that proper levels of stability were not understood. Two very competent groups set about doing something to correct this situation and started testing scale models of actual airplanes in "wind channels." They hoped that through such tests they could obtain rational values for the "stability derivatives" for use in the newly stated equations of motion. Both groups felt that testing full-scale airplanes in flight would be too dangerous and too expensive.

The first group was at the National Physical Laboratory in Great Britain. The NPL had developed a new "wind channel" and Bairstow and Jones decided to test a scale model of a Bleriot monoplane in this facility to obtain stability information on it. Several years later, members of the staff of the newly formed Department of Aeronautical Engineering at Massachusetts Institute of Technology, under J. Hunsaker, duplicated the NPL wind-tunnel and balance and performed tests in their facility on scale models of the Curtiss JN-2 airplane and a variation of it designed by V. E. Clark, then a student of Hunsaker's at M.I.T. From these tests, the M.I.T. group also tried to draw some conclusion regarding the design of airplanes for adequate stability. The NPL experiments were on a $\frac{1}{20}$ scale model of a Bleriot monoplane. A three-view of this model is shown in Fig. 7. The NPL tunnel had a 4-ft^2 test section and a test airspeed of 30 fps. The data taken from these tests are given in Figs. 8 and 9.[6]

The large difference in incidence setting between the wing chord and horizontal tail ($\Delta i_t = -7°$) permitted a positive

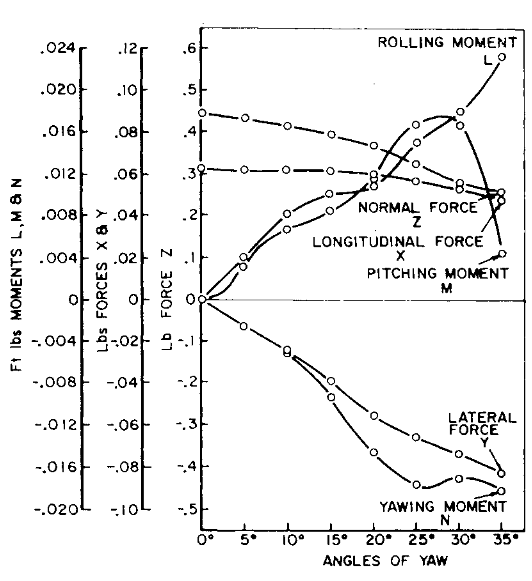


Fig. 9 Variation of forces and moments on model Bleriot type monoplane with angle of yaw.

moment at zero lift (C_{m_0}) and stable trim at useful lift coefficients. It can be seen that the airplane was directionally stable and had positive dihedral effect. It is interesting to note that no tests were run on the controls and no effort in the analysis attempted to relate the control power with the basic stability.

After having obtained these data, the NPL staff seemed hard pressed to know exactly what to do with them and what they meant to the actual airplane. They did the best they could to estimate the stability derivatives and with them solved the equations of motion for the longitudinal and lateral modes.[7] Their principal measures of stability dealt with the damping of the long-period or phugoid longitudinal mode and obtaining a spiral convergence in the lateral case.

It was apparent that the investigators had generated considerable data but were unsure of how to interpret it. They made the following interesting observations: "The degree of stability desirable cannot as yet be exactly specified and this knowledge can only be obtained from the experience of pilots in flight. While marked stability as regards maintenance of speed may be found to be essential, indifference to yawing is probably desirable and a near approach to neutrality as regards rolling may be no disadvantage." They were greatly concerned over the motion of airplanes in wind fluctuations. They felt that stable airplanes under repeated disturbances due to gusts might develop dangerous oscillations. They were surprised that up to that time, there was no evidence of this having occurred. They conclude that the airplane should have a little stability but not much.

Although they stated that the levels of stability required could not be decided upon until there had been correlation with pilot's experience, this correlation was not accomplished in a rigorous way until many years later.

As mentioned before, the M.I.T. group under Hunsaker also embarked on a research program to study airplane stability characteristics and tested models of the Curtiss JN-2 and a variation of it designed by Clark in their new wind tunnel, a close duplicate of the NPL facility. Hunsaker was assisted in these tests by several students and assistants including T. H. Huff, H. K. Chow, V. E. Clark, USA, and one D. W. Douglas. The Curtiss machine was of interest to this group as it was known to be an existing airplane having powerful controls and which "did not pretend to possess any particular degree of stability." Clark's design was an attempt to follow the general lines of the JN-2 but to incorporate changes in its configuration that would give it "inherent stability." A side elevation of the Clark design is shown in Fig. 10. The vertical fin exemplifies the fear they had at that time of more than a very slight directional stability.

The lift and moment curves obtained from tests at a $\frac{1}{26}$ scale model of the Clark design are shown in Fig. 11. These curves come from the original text[8] but have been recalculated to show the results in coefficient form. It may startle one at first to see the slope of the pitching moment curves change with incidence setting of the tail. It appears that they assumed a new c.g. location for each tail incidence setting that would bring the airplane into equilibrium at about the same lift coefficient. As the tail incidence was increased negatively it required a more forward c.g. for trim, and thus an increase in slope.

In analyzing these data, the authors introduced some feeling for a control limitation on the forward center of gravity or on the allowable level of static longitudinal stability. They felt that with the tail incidence at $i_t = -7.0°$ so much elevator would be used up to trim the airplane at high lift coefficients that nothing would be left for control in gusting air. They concluded that the stability would be about neutral when the airplane was balanced with the $i_t = -2.5°$ setting and they felt this level might be too small. They resisted relating static longitudinal stability directly to the c.g. position.

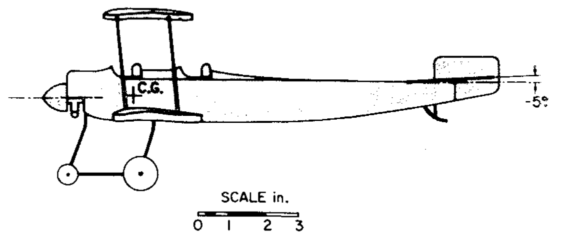


Fig. 10 Model of Clark airplane elevation.

With these data in hand, Hunsaker and his team compared the stability of the two airplanes, the Curtiss and the Clark modification, by comparing their basic dynamics. The stability derivatives of both aircraft were estimated and the stability equations solved. The major conclusion reached on longitudinal stability again dealt mainly with the long-period mode, and they soon found that the damping of this mode was reduced as the speed decreased. At some "critical speed" the damping of this oscillation became neutral and for lower speeds negative. They deduced that the Curtiss airplane's critical speed was higher than that of the Clark design, and they recommended that designers work hard to keep the critical speed low.

The wind-tunnel tests showed the airplane to have had slight directional stability and some positive or stable dihedral effect. They wanted to avoid too much directional stability "as an excessive amount of directional stability, indicated by a steep curve of yawing moments, may cause the airplane to be unmanageable in gusty air. It may take charge and due to excessive weather helm be difficult to keep on any desired course." They also pointed out that directional stability is the determining factor in spiral instability.

It is interesting to note some of their observations on these tests. First of all, they made a strong case for wind-tunnel experiments on complete models. They incorporated no comparison with actual flight. Flight testing was considered too cumbersome for detailed development work. They say: "It is rarely possible in real flying to obtain any idea of the effect of slight changes in design. Weather conditions, motor troubles, personal peculiarities of pilots, etc., tend to add to the complexity of an otherwise very simple problem." They also alluded to the fact that flight testing is dangerous and the results suspect.

The important contribution of this early effort cannot be overstated. They made an important attempt to determine from wind-tunnel tests the performance and stability qualities of an airplane, found that changes in the model were easy to accomplish, despaired of correlating with flight test, were over enthusiastic about the contribution of the dynamic equations, struggled to correlate them with design requirements, and started a tradition of successful wind-tunnel experiments at M.I.T. that has continued until today. The study of airplane stability and control at M.I.T. was first led by J. Hunsaker, then by E. Warner, O. Koppen, and J. Markham. They and their students have played large roles in the U.S. aeronautical scene and these first experiments were excellent first efforts.

The attempts to analyze data from wind-tunnel models by Bairstow and Jones at the National Physical Laboratory in Great Britain and later by Hunsaker and his team at M.I.T. were struggles to correlate the feelings about airplane stability held by the applied mathematicians, Lanchester, Bryan, and Bairstow, who uncovered the fundamental characteristics of airplane motions—with the experimentalists led by Lilienthal, the Wrights, Bleriot, Curtiss, and others. The perplexing fact, for these researchers, was that when airplanes encountered the instabilities directly forecast by the mathematicians, the airplanes got along quite well and flew anyway. It took many years for these complications to be

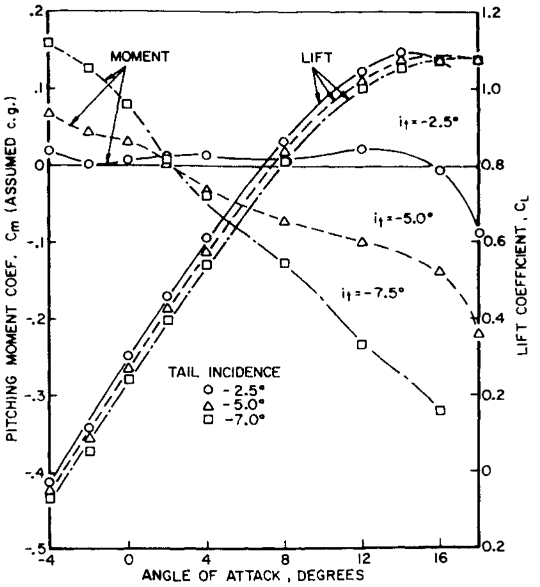


Fig. 11 Lift and pitching moment vs angle of attack Clark airplane—M.I.T. wind tunnel.

better understood and this came about when the human being as a control element was brought more directly into the picture. Stability had to be better related to control and ultimately to the man who was to fly the system. This was brilliantly accomplished during the years of World War II by teams of the NACA, both at the Langley and Ames laboratories and by researchers of the Royal Aircraft Establishment (RAE) in Great Britain.

6. Flight Research—Flying Qualities

Both Bairstow and Jones at the NPL and Hunsaker and his group at M.I.T. tested scale models of their current airplanes in their newly acquired wind tunnels. They both concluded that the data that they generated would not have real meaning until better information was obtained on what was good and what was bad, and this would only come from flight experience and pilots' opinions. They felt that to obtain flight correlation would be difficult as flight testing was expensive, dangerous and pilots' opinions unreliable. It was many years before this all was coordinated with sophistication.

It is very difficult to trace the actual build up of the flight testing experience that led finally to putting this puzzle together. In the United States it was certainly based within the developing NACA, while in Great Britain it was based on the Royal Aircraft Establishment (RAE).

Soon after the establishment of the NACA, flight experiments were undertaken at Langley Field, and a great tradition of flight research was fostered under a remarkable series

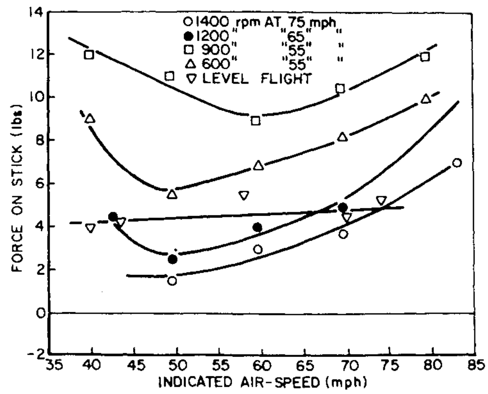


Fig. 13 Control forces on JN4H No. 1.

of men that can be traced directly up to the Apollo program. E. Warner, H. J. E. Reid, F. Thompson, H. Soulè, R. Gilruth, M. Gough, and many others, were powerful contributors to this great capability.

One of the first flight tests for stability and control was performed in the summer of 1919 by E. Warner and F. H. Norton at NACA, Langley Field.[9] This was part of a test on a Curtiss JN4H "Jenny" powered by an Hispano Suiza 150-hp engine and a DH-4 airplane powered by a Liberty engine of some 400 hp. Warner and Norton hoped to correlate the results obtained from flight with the wind-tunnel tests on the JN-2 made by Hunsaker and his group at M.I.T. It is interesting to note that most of the flying was performed by E. Allen, later to become one of our most accomplished test pilots.

The stability and control results from these tests are shown in the following four figures. Figure 12 shows curves of elevator angle vs airspeed. It can be seen that the "Jenny" is unstable at airspeeds above 60 mph, while very stable down to the stall. Figure 13 shows the stick force variations with airspeed, and again the airplane is unstable stick free at the higher airspeeds. The JN4H had no trimming device and these tests were run at one c.g. only. One can only surmise that the weird reputation of the "Jenny" was involved in some way with these characteristics.

The DH-4 was also tested and the results shown in Fig. 14 indicate an airplane with stick-fixed stability throughout its speed range. The last figure of this group (Fig. 15) shows the force-velocity curves. The DH-4 had an adjustable stabilizer and the force curves are given for various stabilizer settings. The airplane appears to have had excellent longitudinal characteristics.

It is interesting in reading the report of these tests to find that the authors come again to the point where although they recognize that the JN4H is unstable and the DH-4 stable they are not sure of the significance. They make a case for being able to trim but feel locking the elevator might be as good as adjusting the stabilizer. The flight determina-

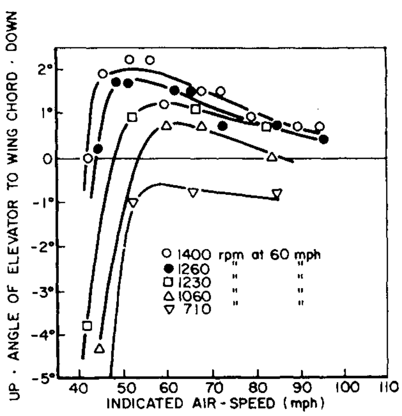


Fig. 12 Elevator angles on JN4H for various speeds and various throttle settings.

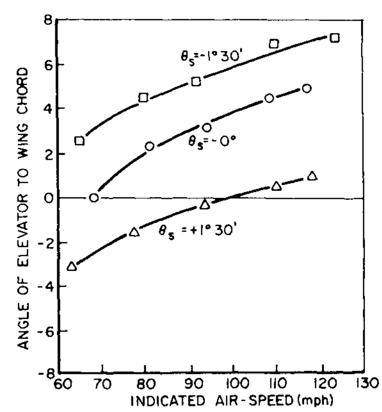


Fig. 14 Elevator angles on DH4 for level flight at various speeds and with various stabilizer settings.

tion of the elevator angle and stick force variations with airspeed was the fundamental test, and these trim curves were related to "stick-fixed" and "stick-free" static longitudinal stability.

The NACA at Langley Field slowly built up their flight research capability that included, from the start, an emphasis on excellent instrumentation and competent engineering test pilots. With this flight tool they set about exploring the relationship between the pilot as a controller and the dynamics of the airplane in flight.

The flight research capability at the NACA developed during the next 10–15 years and by 1940, just prior to World War II, we find this group concentrating on a sophisticated program to correlate airplane stability and control characteristics with pilots' opinions on the airplane's "flying qualities." They determined those factors that could be measured in flight that could be used to define quantitatively the flying qualities of airplanes, they built up an instrumentation capability and test procedures for making the required measurements and finally started accumulating data on the flying qualities of existing airplanes to serve as supporting evidence for the development of design requirements for future airplanes.

Probably the first effort to set down a specification for flying qualities was performed by E. Warner when he was asked to help prepare specifications for the first DC-4 airplane then under development at Douglas. Warner discussed the problem with airline pilots, industrial development engineers, and the NACA staff, and his specification was the result of these studies.

Later the NACA started an exhaustive series of tests of current airplane designs to accumulate data to support the development of a broader specification. The advent of World War II saw this study accelerated and with the cooperation of the U.S. Army Air Corps and the U.S. Navy, information was obtained on military aircraft as well as civilian aircraft. This study was headed up by R. Gilruth and his now famous TR No. 755, published in 1943, was the culmination of all of this work up to that time.[10] During the war both research branches of the Army Air Corps and the Navy became involved with this work, supplied airplanes to the NACA for study, and started to build up their own capabilities to perform flight research. The result of all this was the Air Force-Navy specifications on airplane stability and control that guide designers, wind-tunnel experimenters and flight test operators today. Some of the major conclusions of their tests were as follows:

Longitudinal: 1) The damping of the long-period mode had no correlation with pilots' opinions. 2) Static longitudinal stability stick fixed and free was required within certain speed and configuration regimes. 3) The importance of maneuvering criteria, particularly stick force per g was identified, first, by the way, by Gates of the RAE.

Lateral: 1) The damping of control-free oscillations was important, while controlling the spiral divergence was not. 2) Rolling performance was measured by the helix angle $pb/2v$. Minimum values were specified for maximum forces to be applied. 3) Dihedral effect and directional stability levels were identified indirectly. 4) Directional control was related to yawing disturbance introduced by the lateral control, asymmetric power on multiengined aircraft and cross wind takeoff and landings.

The stability and control specifications continued to be developed during the war and have continued ever since, becoming more elaborate as the performance of aircraft moved into more complicated flight regimes. A great deal of flight research for stability and control at the NACA, and later the NASA, was performed at the Ames Laboratories under H. Goett with L. Clausing and others making up another great flight research team.

During this period, some very important facts about flight research were uncovered. Some examples are: 1) The results from flight research are only as good as the instrumentation involved. 2) That pilots in general are reliable, dedicated, and usually informed people. In spite of this be sure to measure everything. 3) It is easier to make a test pilot out of an engineer than an engineer out of a test pilot. 4) Test flying or flight research is a time consuming, frustrating and expensive business, but we can't do without it.

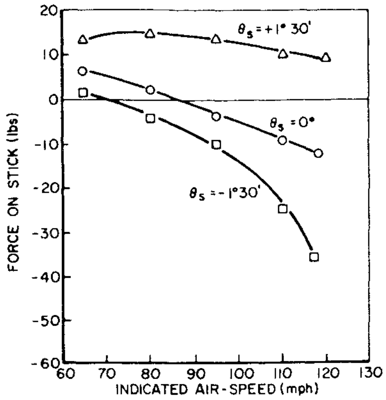


Fig. 15 Control forces on DH4 for level flight at various speeds and with various stabilizer settings.

7. Compressibility

The influence of high speed on the flying characteristics of airplanes before the jet age, was limited to high q effects that involved problems of distortion and elasticity. The World War II fighters of the P-38, P-47, and spitfire class could achieve level flight Mach numbers of only about $M = 0.6$ but they could develop high dynamic pressures, and this gave problems to fabric covered control surfaces and loss in rolling performance because of the wing torsion in response to aileron deflections.

At the start of World War II, the knowledge of compressibility effects on basic aerodynamic parameters was limited to its influence on drag obtained from ballistic experience. At that time compressibility corrections were only applied to propeller efficiency calculations and drag was considered to increase with the square of the velocity. That this led to large error was discovered when that famous triumverate of World War II fighters, the P-54, P-55, and P-56, predicted high speeds of nearly 500 mph—neglecting compressibility of course—and counting heavily on laminar flow airfoils and their pusher propellers. The best of them actually did about 400 mph.

The situation at this time was that the handling of fighter type aircraft was quite predictable with compressibility that phenomena whose effects on propeller efficiency and drag would limit the airplane from going too fast. The maneuver criteria of stick force per g were established and except for possible distortion phenomena were constant with airspeed.

A new serious high-speed stability and control problem developed when two of our fighters, the P-38 and P-47, both equipped with turbo supercharges, started high-speed dives from about 35,000 ft. The pilots reported that upon achieving nearly terminal velocity the airplane that normally would develop about 7–8 g in response to 40–50 lb of pull force, failed to generate noticeable g in response to full pull force plus nose-up tab. This phenomena was soon referred to as the "frozen stick" dive, and the reasons for this were debated heatedly by the aeronautical engineers of the day.

The problem was brought into sharp focus by P-47 diving experiences. Pilots reported that following a split S entry to a vertical dive at 35,000 ft, the nose went down beyond the vertical and no recovery seemed possible for full back stick force and nose-up tab. This situation persisted until the airplane descended to the vicinity of 15,000 ft when very high normal accelerations built up causing blackout. The pilots recovered with the airplane flying at 20,000 ft with bent wings.

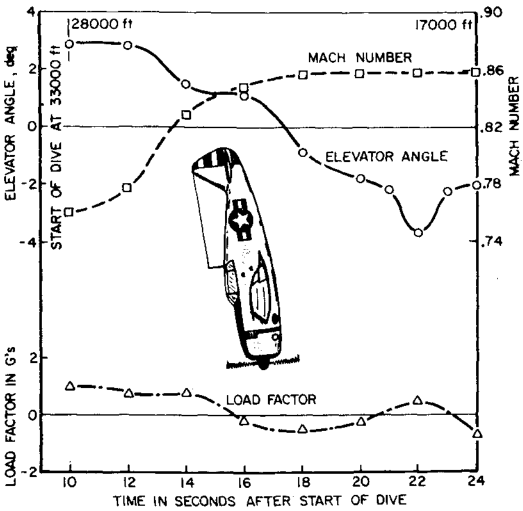


Fig. 16 Time history, dive test P-47C-707, Aug. 1943, Republic Aviation Corporation.

The reasons for this was debated at great length and culminated in a conference at NACA, Langley Field, in 1943,[11] in which three theories were presented. One theory was that at the time of entry into the dive at 35,000 ft the expansion in the area of the elevator hinge permitted condensation and actual freezing with ice on the control surface hinge, thereby preventing deflection and recovery. The second theory was that the loads experienced by the horizontal tail were so high in this maneuver that the resulting deflection actually bound the hinges, preventing control deflection. The third theory was that the aerodynamic parameters of the P-47 were changing at the Mach numbers encountered in the dive in a way to seriously alter the normal stability and control parameters and this could account for the whole thing.

These theories were debated with great heat and each was backed by an NACA team led by top NACA engineers. It became obvious that one simple test would resolve the major difference in the theories. When the pilot pulled on the stick, did the elevator go up or didn't it? If it didn't go up, then one of the first two theories would be correct; but if the elevator did go up and the airplane did not respond as it should, then the third theory would be the most likely answer.

The U.S. Air Corps at Wright Field agreed to run these tests and attempts were made to sign up a test pilot to perform the experiment. None of the contract test pilots were very anxious to do this and would have agreed only at very high fees. The problem was resolved when one of the Air Corps's strongest and ablest test pilots, P. Ritchie said he would perform these tests for nothing. He performed some thirty dive tests on an instrumented P-47 and his reward was an Air Medal.

The P-47 was instrumented to record elevator angle and normal acceleration and P. Ritchie dove the airplane from the vicinity of 35,000 ft to terminal velocity. It was found immediately that the elevator did go up in response to his pull force to an angle that one would have predicted from the elevator hinge moments, yet with this elevator deflection which at this speed normally would have resulted in about 20–30 g or with both wings coming off, the actual response was about $\frac{1}{2}$ g which to the pilot at terminal velocity appeared as no response at all.

The time history of a vertical dive of a P-47 made later by Republic test pilots[12] is shown in Fig. 16. This dive was entered at 33,000 ft and the rapid build up in Mach number can be seen. In spite of the pilot's full back stick force, the nose continued down past the vertical and the airplane experienced heavy buffeting. Note the large up elevator that at these speeds must have taken a tremendous effort by the pilot. Recovery would start shortly after this as the Mach number reduced.

We know now, of course, that these airplanes with thick cambered airfoils and straight wings were encountering serious difficulties in change of lift curve slope, change in downwash, shift in angle of zero lift, loss in elevator effectiveness, and rearward movement of the aerodynamic center at the Mach numbers encountered in the vertical dive. As the airplane descended, the higher air density and increasing speed of sound reduced the Mach number below that for which the divergences occurred. One of the first tests performed in the Ames 16-ft wind tunnel showed the problem immediately (Fig. 17).[13]

The large increase in slope of the pitching moment vs lift coefficient curves at constant Mach number can be seen as is the reduction in trim lift coefficient. I have cross plotted a curve of C_m vs C_L at constant M^2C_L. The airplane flying in level flight at constant altitude will fly at this constant 1-g condition. The World War II fighter aircraft could not achieve level flight at high Mach numbers, but the next generation jet aircraft certainly could. The phenomena of increasing stability as measured by $(dC_m/dC_L)_M$ and the corresponding decrease in stability as measured by $(dC_m/dC_L)_{M^2C_L}$ is clearly seen. It became important then to distinguish between the constant Mach number slope now referred to as angle of attack stability that relates to the short-period mode and stick force per g, and the slope of C_m vs C_L at constant M^2C_L, referred to as speed stability relating to the long-period mode and elevator and stick force variations with speed or Mach number. This airplane becomes very stiff as far as angle of attack response is concerned, but unstable above $M = 0.60$ in the long-period sense. The long-period mode is therefore divergent at high Mach number and this phenomena is referred to as "tuck under." With modern aircraft, these effects have been delayed and reduced in magnitude by using thinner airfoils, removing most of the camber, sweeping the wings and using all moving tail surfaces.

When this problem was properly recognized in 1943, a great deal of thought was given to how to improve these dive characteristics. Small flaps on the under side of the wing and contour changes were suggested to permit a trim lift coefficient that would allow a recovery. These solutions were soon found unnecessary as the basic improvements already alluded to, relieved the problem a great deal. The advent of the jet engine made these high transonic Mach numbers available in level flight and soon the airplane emerged into the supersonic regime.

During this period, experimental evidence was very hard to obtain as the high-speed wind tunnels of the day would choke when testing models at Mach numbers near the speed of sound, making the results of testing above $M = 0.85$ extremely suspect. Data in the late 1940's were accumulated by clever testing strategems such as placing small models in the local wing flowfields of high-speed aircraft, wind-tunnel bumps, free-fall models, rocket model testing, and finally, research airplanes. Later, of course, the solution to transonic testing in wind tunnels was obtained and new aircraft designs could be developed in the usual manner.

The problems of compressibility with respect to design for adequate stability and control characteristics can be summarized briefly as follows:

In the transonic regime: 1) Past critical Mach number the reduction in the slope of the lift curve, reduction in rate of change of downwash and rearward movement of the wing aerodynamic center—tend to induce a speed divergence or "tuck under" that must be minimized by use of very thin, very low cambered airfoils. At the same time, the angle of attack stability becomes very stiff. 2) A rapid deterioration of elevator effectiveness to a point where all moving horizontal tails are required for adequate control. 3) General airplane buffeting and other phenomena brought on through the influence of the shock waves lying on the wings.

In the supersonic regime: 1) Continued loss in lift curve slope with Mach number starts to have a serious impact on directional stability and on the effectiveness of all controls. 2) At supersonic speeds, trim drag becomes an important factor, requiring either configuration control or c.g. control through fuel programing. 3) The secondary influence of design to minimize compressibility effects, such as swept wings, tee tails and others tend to give problems in pitch up, deep stall, and roll coupling.

The fear of aerodynamic unknowns that might be encountered in flight beyond the so-called "sound barrier" and the inability, at that time, to test adequately in wind tunnels led to the research airplane program introduced near the end of World War II. The X-1 was designed in 1945 to achieve supersonic speeds through the use of rocket motors and was built to a load factor of 18 g to survive almost any unexpected incident.

The X-1 achieved world fame when C. Yeager first flew the airplane beyond Mach 1.0. The X-1A achieved Mach numbers in excess of 2.0 and the X-2 later achieved a Mach number of 3.0.

The most costly lesson learned from these research aircraft was their loss in directional stability with increasing Mach number. Both the X-1A and the X-2 went out of control at their highest Mach numbers when responding to the application of lateral control. The resulting wild uncontrolled flight nearly killed Yeager in the X-1A and unfortunately did result in the death of the young Air Force pilot in the X-2.

Later, the very successful X-15 program extended our knowledge up to Mach 8.0. It verified a great deal of aerodynamic theory and showed that provided with adequate directional stability, the airplane could be flown easily up to these Mach numbers. It also verified reaction controls for flight at very high altitudes, stability augmentation systems and pilot control loops such as fly by wire.

8. Aeroelasticity

For the calculation of the aerodynamics of many airplanes, the assumption that it can be considered as a rigid body is a satisfactory approximation. Early analytical studies of airplane stability and control made this assumption, and wind-tunnel testing was performed with solid models that were indeed rigid bodies. The early airplanes were probably far from rigid, but their speeds were low enough that elastic problems were lost in the welter of other difficulties. The Wright Brothers' airplanes obviously were not rigid as their lateral control was obtained through twisting the wings.

The first real problem with the elastic airplane came through the aeroelastic instability known as flutter. By the 1930s, the speeds of airplanes had increased to a point

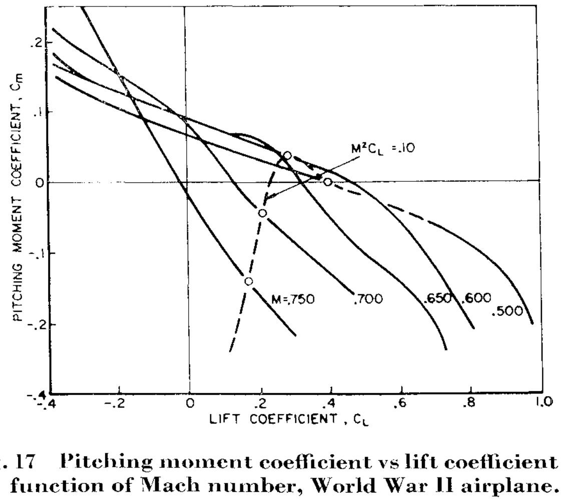
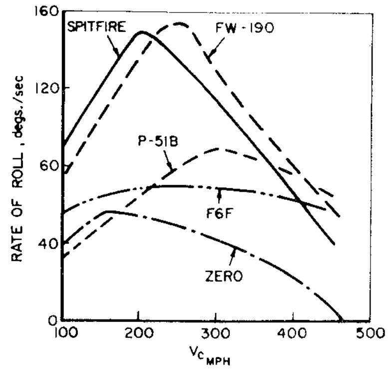


Fig. 17 Pitching moment coefficient vs lift coefficient as a function of Mach number, World War II airplane.

Fig. 18 Rolling performance — several World War II fighters.



where the various modes of wing and control flutter were in danger of losing damping and the design requirements to prevent flutter were established.

In the area of stability and control, the first important aeroelastic problem dealt with the influence of wing elasticity on the rate of roll in response to aileron deflection. Wing torsion in response to control deflection decreased the available rolling moment as a function of dynamic pressure—resulting in an "aileron reversal speed" above which roll response to the lateral control would reverse. This became a crucial design problem for World War II fighters. High rates of roll at high speeds became a prime combat maneuver and any loss in roll response put the fighter so effected at a serious disadvantage. The designers of the spitfire, the FW-190, and the P-51 sought higher and higher roll rates, and this led them to shortening their spans, balancing their ailerons for larger deflection at high speed and stiffening their wings to prevent a large loss in rolling velocity because of the wing elasticity. The rolling performance of several World War II fighters as a function of airspeed is shown in Fig. 18.[14]

The plight of the Japanese Zero, that was designed for lightness and minimum radius of turn, can be seen. The Zero had a very low aileron reversal speed and our Navy pilots were soon able to take advantage of this weakness. They avoided circling combat and established high speed, single pass techniques, where their superior diving speeds and high roll rates could not be followed by the Zero operating close to its aileron reversal speed.

The aeroelastic problems of the airplane were accentuated severely with the advent of the jet propelled airplane. The gas turbine permitted very much higher-speed potentials and the thin, swept wings required by these aircraft introduced new and severe problems to the designers of the first of these aircraft.

The airplane that brought the aeroelastic problem to everyone's attention was the Boeing XB-47, a high subsonic speed strategic bomber with high aspect ratio, swept, thin wings. This airplane was designed in 1945, at which time practically nothing was known about constructing such wings. That it would be very flexible was known. It had a wing tip deflection of some 35 ft between maximum positive and negative loads, and it was recognized by the designers that this would have a serious impact on the stability and control of this airplane, *it did*.

The problem confronting the project engineer who had to design this complex wing structure was very difficult but typifies the problem facing designers emerging into new areas. He had to establish requirements for wing torsional stiffness immediately so that the construction of the airplane could get under way. Yet there was no previous experience to extrapolate from; wind-tunnel techniques had yet to be evolved to handle the elastic problem, and a complete theoretical solution had not been developed. Without a digital computer, there was no hope that solutions to these complicated equations could be solved on a reasonable time scale.

Faced with this situation, the project engineer performed a strip integration solution, computing the wing torsion in re-

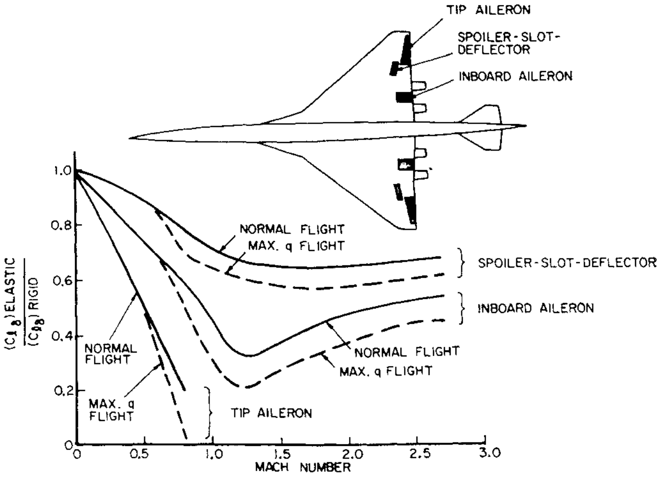


Fig. 19 Supersonic transport configuration.

sponse to aileron deflection and with these results he estimated the wing torsional rigidity requirements to keep the aileron reversal speed above the design limit speed. Unfortunately his solution didn't account for the fact that when ailerons deflected, the wings bent, and when bent, swept wings twist. As a result, torsional rigidity requirements were underestimated and the actual aileron reversal speed was too low and eventually became a problem to the B-47.

A complete theoretical solution to the problem was undertaken at the same time and due to its complexity and the lack of computational help, arrived at the right answer two years after the B-47 first flew. A third approach to this problem was undertaken by a few Boeing experimentalists who put together a crude test involving a makeshift wind tunnel and a steel sparred balsa wood model that was set on a spindle in the tunnel with ailerons deflected and permitted freedom in roll. The tunnel speed was increased until the model's rate of roll started to fall off and then actually reverse. This was the model's aileron reversal speed and came quite close to predicting the full-scale experience. The test was too crude to be taken seriously and results again came too late to influence the design of the B-47.

Since this experience, the availability of the theory with the high-speed digital computers makes this calculation routine. Wind-tunnel techniques for handling the aeroelastic problem have also been developed, and Boeing and other design teams field these problems with great skill.

The stability and control of the B-47 and its successors are all affected by aeroelastic problems other than lateral control. It was found that in longitudinal maneuvers such as pullups, the wings deflected and twisted, moving the wing aerodynamic center and changing the downwash pattern at the tail because of the resulting change in spanwise load distribution. This had a severe influence on the airplane's maneuver points and made it essential to tailor wings and their inertia loads to control these effects.

We are now entering the era of large supersonic airplane configurations and the aeroelastic design problem becomes even more complex. At the dynamic pressures involved in these designs (1500 lb/ft^2) the wings tend to behave more or less like waving flags and the stability and control problem becomes very difficult.

An example of the severity of the aeroelastic problem for the SST is the design of the lateral controls to provide adequate roll rates at the high dynamic pressures and Mach numbers involved. The SST team at Boeing have studied many approaches to the solution of this problem. Several of these possibilities are shown in Fig. 19.[15]

The inadequacy of tip ailerons at high speeds to provide rolling moments is clearly indicated. Even mid chord ailerons have problems and it appears that some form of spoiler slot control will have to be adopted.

The effects of compressibility and elasticity on configurations designed for flight in these speed regimes are complex and serious. The shift of the aerodynamic center to the rear as the airplane accelerates from the subsonic to the supersonic regime must be corrected by either a change in wing configuration, as did the B-70 and F-111, or c.g. control through fuel utilization or pumping in order to keep the time drag low. In maneuvering flight the rearward movement of the aerodynamic center can be balanced by using to advantage the wing twist that can be tailored by working on the mass distribution on the wings. The most powerful element is, of course, the engine location. One of the reasons for the abandonment of Boeing's first SST configuration design was that no practical solution could be found to the aeroelastic problem involved. The present configuration accepts lower aerodynamic efficiency (lower sweep) at cruising Mach number for a greatly simplified structure with its aeroelastic solution at a lighter weight empty.

The anticipated c.g. range for the Boeing SST is shown in Fig. 20, on which is plotted the variation in the airplane's maneuver points with Mach number.[15] It can be seen that in the subsonic regime the maneuver points lie ahead of the most aft c.g. This, up to the present time, would be unacceptable. The problem is that to drive these maneuver points aft might require several thousand pounds of additional empty weight. It is possible that a better solution would be to have this stabilized through the stability augmentation system (SAS) that must be incorporated into the airplane for other reasons. A triply redundant or a so-called hardened SAS may be "as reliable as the structure" and be a better solution to the overall design problem for airplanes of this class. It is interesting to observe that our latest airplane concept, the SST, may achieve its stabilization through the control system, as did the Wright Brothers with the first airplane.

It is becoming apparent that the control system of our modern complex aircraft can be powerful tools to help solve problems other than control of the flight path. Through proper sensors, they can be used for gust alleviation, flutter modal suppression, as well as for augmentation of the basic stability of the airplane. It may well be that through such schemes the structural weights of future complex aircraft can be reduced and the performance potential of the airplane improved.

9. Conclusion

We have come a long way then, from the Wright Brothers' Flyer to the Supersonic Transport. One of the major lessons that we have learned is that the requirements for inherent aerodynamic stability of airplane systems is related to the complexity of the flight regime, the power of the con-

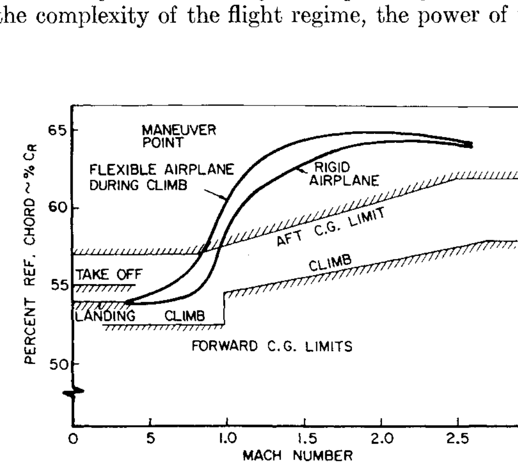


Fig. 20 Effect of aeroelasticity on S.S.T. maneuver points.

trols and the capabilities of the controller, be it a human being or a black box.

The flight regime of the first manned airplane was so rudimentary that control power was the key to success, and aerodynamic instabilities could be stabilized by the pilot after a little practice. As the airplane flew faster and became more heavily loaded, the divergences of the unstable airplane became too rapid for the human pilot to cope with and design for inherent aerodynamic stability became mandatory. As the airplane moved into more and more complex flight regimes, the capability of designing for complete aerodynamic stability throughout the important flight regimes became nearly impossible. The great nonlinearities arising from power effects, compressibility and aeroelasticity have made it important to introduce stabilization elements through the control system to aid the pilot in controlling the airplane. Thus, we have seen downsprings, bobweights, rate dampers, SAS, adaptive autopilots and fly-by wire.

Lilienthal would have been even more successful if he had understood the necessity for adequate controls. The Wright Brothers' airplane would have flown more easily if these able men had taken advantage of the readily available inherent stability. Langley would not have flung poor Manly off a catapult into the Potomac on his first flight if he had known more of the response and learning characteristics of the human pilot. The applied mathematicians would have observed better correlation with flight experience if they had closed the loop with the human controller. The early wind-tunnel experimenters would have made more meaningful tests if they had known that pilots really didn't care whether the phugoid damped or not, and preferred high directional stability to a spiral convergence.

Inherent aerodynamic stability is a great potential asset for airplane design, but it is not an end in itself. We should take advantage of it whenever we can but we must not pay an impossible price for it when there are other solutions to the over-all problems of flight control.

As the airplane becomes larger, faster, and as a result, more complex, the balance between inherent aerodynamic stability and stability provided through automatic systems must be made on an overall aircraft performance judgment. There is much yet to be done with the technology and as Bryan did in 1911, I recommend this field to the young applied mathematicians and experimentalists of today as a technology of fascinating interest and potential engineering satisfaction.

References

[1] Zahm, A. F., "Stability of Airplanes and Flying Machines," *Proceedings of the International Conference on Aerial Navigation*, Chicago, 1893, pp. 143–147; also *New York, American Engineer and Railroad Journal*, 1896, p. 429.

[2] Lanchester, F. W., *Aerodonetics*, A. Constable & Co., London, 1908, p. 433.

[3] *The Papers of Wilbur and Orville Wright*, Vol. I, McGraw-Hill, New York, 1953, pp. 1899–1905.

[4] *The Papers of Wilbur and Orville Wright*, Vol. II, McGraw-Hill, New York, 1953, pp. 1906–1948.

[5] Bryan, G. H., *Stability in Aviation*. MacMillan, London, 1911, p. 192.

[6] Aeronautical Staff, "Method of Experimental Determination of the Forces and Moments on a Model of a Complete Airplane, with the Results of Measurements of a Monoplane of a Bleriot Type," R and M No. 75, 1913, Great Britian Aeronautical Research Committee (in its Annual Rept., 1912–1913, pp. 128–132).

[7] Bairstow, L., Jones, B. M., and Thomson, A. W. H., "Investigation into the Stability of an Aeroplane, with an Examination into the Condition Necessary in Order That the Symmetric and Asymmetric Oscillations Can Be Considered Independently," R and M No. 77, 1913, Great Britain Aeronautical Research Committee (in its Annual Rept., 1912–1913, pp. 135–171).

[8] Hunsaker, J. C., "Dynamical Stability of Aeroplanes," *Smithsonian Miscellaneous Collections*, Vol. 6, No. 5, June 1916, Smithsonian Institution, Washington, D.C., p. 78.

[9] Warner, E. P. and Norton, F. H., "Preliminary Report on Free Flight Tests," TR No. 70, 1919, NACA.

[10] Gilruth, R. R., "Requirements for Satisfactory Flying Qualities of Airplanes," TR No. 755, 1943, NACA.

[11] Siebert, H. W., "Conference on P-47 Type Airplane Dive Difficulties," Memo. Rept. ENG-M-51/4303-10, March 8, 1943, U.S. Army Air Corps.

[12] Brice, P. E. R., "Results of Maximum Mach Number Dive Tests," Interoffice Correspondence, Aug. 31, 1943, Republic Aviation Corp.

[13] Hood, M. J. and Allen, J. H., "The Problem of Longitudinal Stability and Control at High Speeds," TR No. 767, 1943, NACA.

[14] Toll, T. A., "A Summary of Lateral Control Research," TN No. 1245, March 1947, NACA.

[15] Unpublished data, 1969, Boeing Airplane Co.