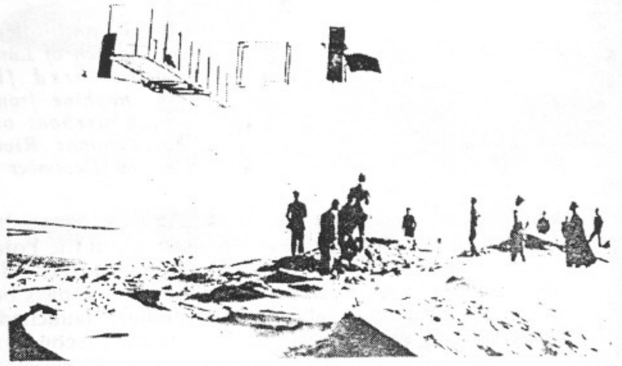
# THE FIRST FIFTY YEARS OF AEROELASTICITY

*A paper of unusual interest and quality was provided for the Historical Group of the Society on 13th December 1977 when Prof A. R. Collar, CBE, MA, DSc, LL.D, FRS, FRAeS, read his lecture on the history of "aeroelasticity over the first five decades of this century". Prof Collar previously gave the Second Lanchester Memorial Lecture in November 1958 (JRAeS January 1959) covering some of the same field, and this is regarded as a classic work on the period. We are particularly pleased to be able to publish this, his current paper, in full in* Aerospace.

*The reference number of this paper is 545*



*Wright brothers' man-carrying glider in flight, 1901.*

Before I speak of the history of aeroelasticity I must first define it. It is the science which treats of the interaction of aerodynamic, elastic, and inertia (including gravitational) forces. In its simplest form, if an increase in aerodynamic load distorts a structure in such a manner that the incidence changes and increases the aerodynamic load further, we have an aeroelastic problem. Since aerodynamic load increases with speed, then as speed rises, we must eventually reach a speed at which, whatever the distortion, the disturbing aerodynamic forces balance the restoring elastic forces. This is a critical condition. At any higher speed, the aerodynamic forces prevail and distortion increases indefinitely — or at least until the force: displacement relation becomes non-linear, or the structure fails. Aeroelasticity is thus concerned with stiffness, not strength; it has much in common with the Euler strut under end load. So long as the load is below the critical, the strut is stable; above the critical load determined by its stiffness, it fails, whatever its strength.

The name 'aeroelasticity' was proposed by Roxbee Cox and Pugsley in the early 1930s. It paralleled that of the photoelasticity phenomenon discussed by Coker and Filon, which was much in vogue at the time. Although it is deficient in that it carries no implication of inertia loads, it is compendious and convenient, and is well established in the literature.

Since aerodynamic loads increase with speed, and since aircraft speeds have steadily increased since flight began, it is not surprising that aeroelastic problems have cropped up regularly over the years, with new ones just round the next corner. It is a complex phenomenon, especially when inertia forces are involved, and requires the deployment of many mathematical and experimental techniques for its solution.

I have chosen to write of the history of aeroelasticity over the first half of this century, and propose to do this chronologically. This was the period when the subject had an aura of black magic about it, with a small number of initiates only — research workers all. But by 1950 the aviation industry had realised that aeroelasticity must be treated as a routine aspect of aircraft design, and recruited the staff and obtained the equipment needed for the purpose. This marked the end of an era, and so seemed a suitable end point for this paper.

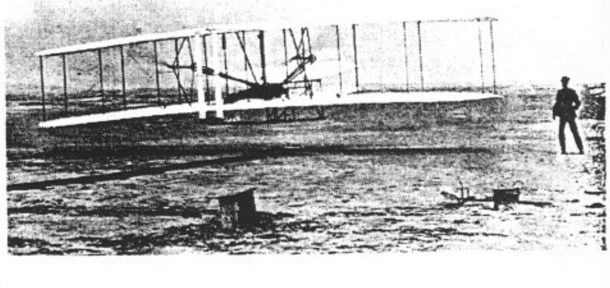
Since I am to cover half a century, it is worth noting here that this year is the centenary of the appearance in 1877 of the Adams Prize essay of E. J. Routh on the stability of motion, in which he enunciated the necessary criteria for stability which have been such powerful tools in the solution of all aircraft stability problems, including those of aeroelasticity.

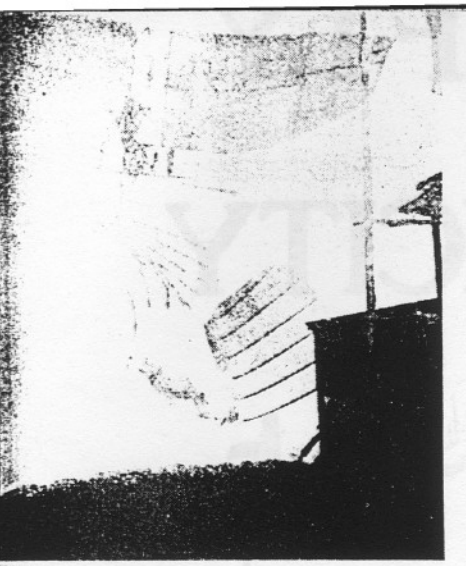
## The first decade

Perhaps surprisingly, aeroelasticity is older than powered flight itself. As is well known, for two or three years before they achieved powered flight in December 1903, the brothers Wright were experimenting with man-carrying gliders. Having observed how birds achieve lateral control in flight, they chose to use wing warping for the same purpose; in other words, to make deliberate use of the aeroelastic properties of a wing. Having achieved success, they incorporated the principle in the Wright Flyer of 1903. It may be remarked in passing that the Flyer, having pronounced anhedral, was basically unstable, and therefore required very efficient lateral control. But the Wrights, having been bicycle manufacturers, were well versed in the control of a basically unstable machine!

There is also a story of aeroelastic failure before powered flight. In the summer of 1903 Samuel Langley, Secretary of

*The Wright Flyer making the world's first powered flight in December 1903.*

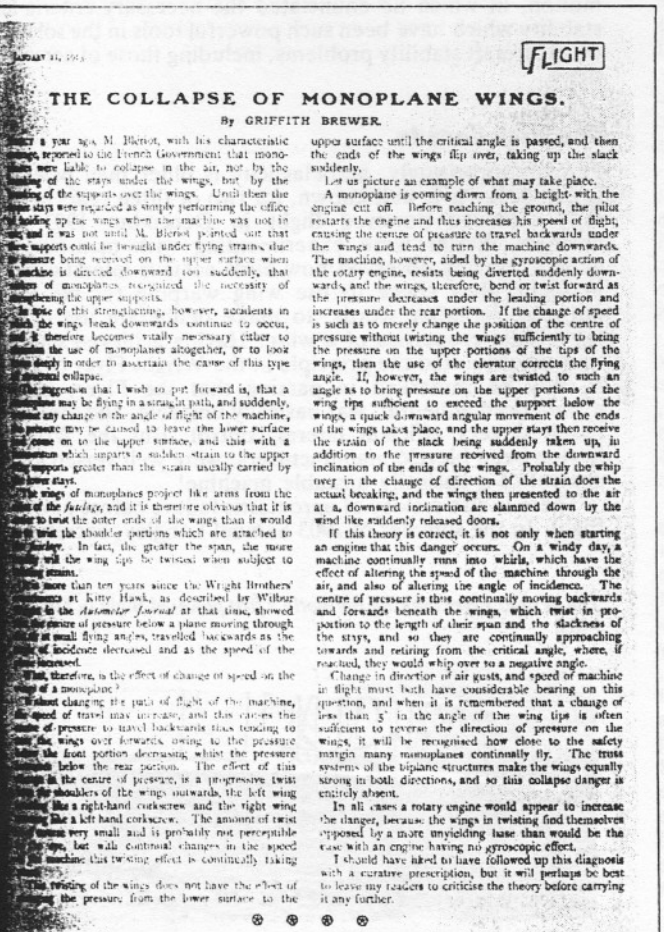




*Second attempted launch of Langley's powered flying machine from the houseboat on the Potomac River on 8th December 1903.*

the Smithsonian Institution in Washington, catapulted a powered flying machine from a houseboat on the Potomac River; it at once ditched. It was recovered and repaired, and a second time, on 8th December 1903 — nine days before the Wrights' historic flight — it was similarly launched; but it immediately broke up in the air. Pritchard recounts how, some years later, and after Langley's death, his successor at the Smithsonian rebuilt the aircraft and flew it successfully at Hammondsport, N.Y. However, in the rebuilding, the

*Griffith Brewer's article reproduced from the pages of a 1913 issue of* Flight.



January 11, 1913. FLIGHT

THE COLLAPSE OF MONOPLANE WINGS.

By GRIFFITH BREWER.

[illegible] a year ago, M. Blériot, with his characteristic [illegible], reported to the French Government that monoplanes were liable to collapse in the air, not by the [illegible] of the stays under the wings, but by the [illegible] of the supports over the wings. Until then the upper stays were regarded as simply performing the office of holding up the wings when the machine was not in use, and it was not until M. Blériot pointed out that these supports could be brought under flying strains due to pressure being received on the upper surface when a machine is directed downward too suddenly, that [illegible] of monoplanes recognized the necessity of strengthening the upper supports.

In spite of this strengthening, however, accidents in which the wings break downwards continue to occur, and it therefore becomes vitally necessary either to [illegible] the use of monoplanes altogether, or to look [illegible] deeply in order to ascertain the cause of this type of [illegible] collapse.

The suggestion that I wish to put forward is, that a monoplane may be flying in a straight path, and suddenly, without any change in the angle of flight of the machine, the pressure may be caused to leave the lower surface and come on to the upper surface, and this with a [illegible] which imparts a sudden strain to the upper [illegible] supports greater than the strain usually carried by the lower stays.

The wings of monoplanes project like arms from the [illegible] of the *fuselage*, and it is therefore obvious that it is easier to twist the outer ends of the wings than it would be to twist the shoulder portions which are attached to the *fuselage*. In fact, the greater the span, the more [illegible] will the wing tips be twisted when subject to [illegible] strains.

It is more than ten years since the Wright Brothers' [illegible] at Kitty Hawk, as described by Wilbur Wright in the *Automotor Journal* at that time, showed that the centre of pressure below a plane moving through the air at small flying angles, travelled backwards as the angle of incidence decreased and as the speed of the plane increased.

What, therefore, is the effect of change of speed on the wings of a monoplane?

Without changing the path of flight of the machine, the speed of travel may increase, and this causes the centre of pressure to travel backwards thus tending to [illegible] the wings over forwards, owing to the pressure below the front portion decreasing whilst the pressure [illegible] below the rear portion. The effect of this change in the centre of pressure, is a progressive twist from the shoulders of the wings outwards, the left wing [illegible] like a right-hand corkscrew and the right wing [illegible] like a left hand corkscrew. The amount of twist is of course very small and is probably not perceptible to the eye, but with continual changes in the speed of the machine this twisting effect is continually taking place.

This twisting of the wings does not have the effect of [illegible] the pressure from the lower surface to the upper surface until the critical angle is passed, and then the ends of the wings flip over, taking up the slack suddenly.

Let us picture an example of what may take place.

A monoplane is coming down from a height with the engine cut off. Before reaching the ground, the pilot restarts the engine and thus increases his speed of flight, causing the centre of pressure to travel backwards under the wings and tend to turn the machine downwards. The machine, however, aided by the gyroscopic action of the rotary engine, resists being diverted suddenly downwards, and the wings, therefore, bend or twist forward as the pressure decreases under the leading portion and increases under the rear portion. If the change of speed is such as to merely change the position of the centre of pressure without twisting the wings sufficiently to bring the pressure on the upper portions of the tips of the wings, then the use of the elevator corrects the flying angle. If, however, the wings are twisted to such an angle as to bring pressure on the upper portions of the wing tips sufficient to exceed the support below the wings, a quick downward angular movement of the ends of the wings takes place, and the upper stays then receive the strain of the slack being suddenly taken up, in addition to the pressure received from the downward inclination of the ends of the wings. Probably the whip over in the change of direction of the strain does the actual breaking, and the wings then presented to the air at a downward inclination are slammed down by the wind like suddenly released doors.

If this theory is correct, it is not only when starting an engine that this danger occurs. On a windy day, a machine continually runs into whirls, which have the effect of altering the speed of the machine through the air, and also of altering the angle of incidence. The centre of pressure is thus continually moving backwards and forwards beneath the wings, which twist in proportion to the length of their span and the slackness of the stays, and so they are continually approaching towards and retiring from the critical angle, where, if reached, they would whip over to a negative angle.

Change in direction of air gusts, and speed of machine in flight must both have considerable bearing on this question, and when it is remembered that a change of less than 5° in the angle of the wing tips is often sufficient to reverse the direction of pressure on the wings, it will be recognised how close to the safety margin many monoplanes continually fly. The truss systems of the biplane structures make the wings equally strong in both directions, and so this collapse danger is entirely absent.

In all cases a rotary engine would appear to increase the danger, because the wings in twisting find themselves opposed by a more unyielding base than would be the case with an engine having no gyroscopic effect.

I should have liked to have followed up this diagnosis with a curative prescription, but it will perhaps be best to leave my readers to criticise the theory before carrying it any further.

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N.P.L. Exhibit at Ghent.

In the British Section at the Ghent International Exhibition the National Physical Laboratory will have an exhibit illustrating the investigation into the science of flight carried on at the Laboratory. There will be large scale models of wind channels and also of the special tank which permits of photographs being taken of the actual currents and eddies which play about and round the wings and framework of an aeroplane.

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wing was greatly stiffened by modified trussing, so that aeroelastic failure was avoided. It seems, therefore, that, but for aeroelasticity, Langley might have displaced the Wright brothers from their place in history.

There must have been many other instances of aeroelastic troubles, unrealised and unrecorded, during the first decade of the century. I shall revert to this shortly. I wish now to refer to one major achievement: Bryan's 1906 theory of the stability of a rigid aeroplane. This may quite properly be regarded as an aeroelastic study in which the aircraft stiffness happens to be infinite, so that only the interplay of aerodynamic and inertia forces is involved. Bryan's studies, based on the use of Routh's criteria, have provided the foundation on which most of the aeroelastic investigations of succeeding years have been based.

## The First World War decade

Many of the accidents of the early years of flying may well

*Several cases of wing failure occurred with the Fokker D-8 which was an unbraced high wing monoplane.*

have been due to unrecognised aeroelastic troubles. Certainly there were a number of occurrences of wing failure under download, and these were considered in a remarkably perceptive short article by Griffith Brewer in the journal *Flight* dated 11th January 1913. Called "The collapse of monoplane wings", it says "accidents in which the wings break downwards continue to occur"; it comments that "the greater the span, the more readily will the wing tips be twisted"; and it points out that "the Wright brothers' experiments . . . showed that the centre of pressure . . . travelled backwards . . . as the speed of the plane increased". Putting these ideas together, Griffith Brewer discussed what will happen to a monoplane wing with bracing stays; he seems to accept that there will inevitably be slack in the bracing. To paraphrase his argument: in normal flight the upper stays will be slack and the centre of pressure will lie between the attachment points of the lower stays; but if the speed is increased so far that the centre of pressure moves behind the attachment point of the rear stay, catastrophe will result. In Griffith Brewer's words "the ends of the wings flip over, taking up the slack suddenly". This does of course pre-suppose small intrinsic wing stiffness, but this was almost always the case. Much of the stiffness was provided by the stays; the intrinsic stiffness in torsion was due only to differential spar bending.

I think it quite likely that the partial eclipse of the monoplane by the biplane which began around this time was due to the much greater stiffness, provided by interplane struts and cross-bracing wires, which could be achieved on the biplane. Certainly the biplane could not claim better drag characteristics.

So we come to the First World War, with its immense impetus to aviation. During the war, there was one recorded story of the occurrence of wing divergence — basically the phenomenon discussed by Griffith Brewer. Fokker, in his book "The Flying Dutchman" relates that there were several cases of wing failure on the Fokker D-8, an unbraced high-wing monoplane. An accident investigation was put in hand, which required check strength tests to be done. These showed the wing to be amply strong enough to

# Aeroelasticity — *continued*

withstand the expected loads. However, during the tests, Fokker himself observed that, as the load was progressively applied, so the wing twisted, and he realised that the load being applied was therefore quite unrepresentative of what would be the airload distribution. Curative measures were then obvious.

Much more spectacular, however, was the appearance of the first recorded and documented case of flutter in an aircraft. During a flight in 1916 the Handley Page O/400 bomber suffered a violent tail oscillation, in which the fuselage twisted through very large angles ($\pm 45°$ was reported), with the elevators flapping antisymmetrically. It was a box tail, with upper and lower elevators connected fairly rigidly, but with no connection from port to starboard except through long separate cable runs to the stick. F. W. Lanchester was called in to advise on this, and his conclusions and recommendations are recorded by the ARC in R&M 276, Part 1. One of his recommendations was that the port and starboard elevators should be connected by a 14g, 2½in dia, steel tube, and such a stiff connection soon became a design feature and ultimately a design requirement. Additionally, the NPL was asked to investigate the occurrence theoretically, and this was done by Bairstow and

*Dr F. W. Lanchester* *Dr R. A. Frazer*

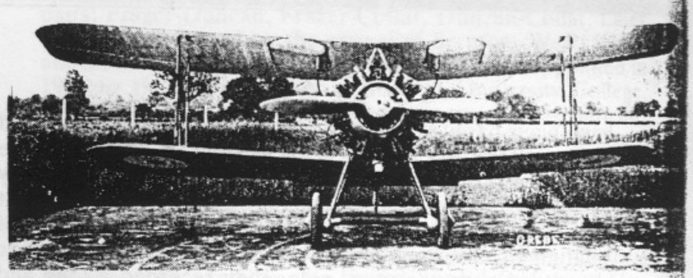
Fage, whose work is recorded in Part II of R&M 276. This first flutter investigation, though erroneous in some respects, indicated that instability was possible, and supported Lanchester's recommendation, which in effect removed the degree of freedom in anti-symmetric elevator movement. Coincidentally, a similar case of flutter occurred in 1917 on the de Havilland DH-9 and was similarly treated.

Undoubtedly, there must have been many more occurrences — often catastrophic — of aeroelastic troubles during this period; but with the state of the art in its earliest stages, they were unrecognised and unrecorded.

### The flutter decade

During the 1920s aeroelastic research began to blossom on an international scale; in Europe, work was done in France, Germany, and Italy as well as in the UK. Almost wholly, the work was directed at the elucidation of the flutter problem, since wing-aileron flutter in particular occurred widely.

Before I describe some of the work which was done, I would like to take a brief look at some of the tools which were available to the research workers faced with the problem. On the theoretical side, there was (1) Bryan's theory of the stability of a rigid aeroplane, extended by Bairstow and Nayler to nine degrees of freedom by the inclusion of the three control surface motions; (2) The idea of aerodynamic derivatives; (3) Aerodynamic strip theory, and, through its validation for propellers at low rates of



*Wing-aileron flutter occurred on both the Gloster Grebe and the Gloster Gamecock, below.*

advance, some understanding of tip effects; (4) Classical theory of elasticity; and (5) Rayleigh's studies of mechanical vibration. On the experimental and practical side, there were (1) A variety of low speed wind tunnels; (2) Workshop staff skilled in model making; and (3) Mechanical desk calculating machines — commonly known as 'rabbits' since they multiplied quickly. But it is one thing to have these tools; it is quite another to know which to use, and how to use them.

As I have said, wing-aileron flutter occurred fairly widely. The first recorded treatment seems to have been that of von Baumhauer and Koning, in 1923. They discussed a rigid rolling wing, elastically restrained, and a flapping aileron. Their work suggested the advantages of mass-balance.

In the United Kingdom the problem was sparked by wing-aileron flutter on both the Grebe and the Gamecock, and was first discussed by the Accident Investigation Sub-Committee of the Aeronautical Research Committee, who recommended that "the vibration of aircraft structures should be thoroughly studied". Studies were put in hand both at the RAE and the NPL in 1925. At the RAE, Gates made a brave attempt to solve the problem of the flexure-torsion oscillations, in a wind, of an elastic wing deriving its stiffnesses from two bending spars. This was really an impossible approach; the analysis resulted in simultaneous integro-differential equations of great complexity. Nevertheless Gates was able to make deductions about possible types of instability and about the relative importance of parameters involved. Shortly afterwards, McKinnon Wood pointed out that flutter experiments might be done in low speed wind tunnels on models of reduced elasticity — a technique frequently used in later years.

At the NPL work was initiated in 1925 by R. A. Frazer; he was joined in the following year by W. J. Duncan. Two years later, in August 1928, they published a monograph "The flutter of aeroplane wings", R&M 1155. This slim volume, of just over 200 pages, has been known ever since as "The Flutter Bible"; and understandably so. I have just reread it: it is quite astonishing in its completeness. Frazer and Duncan solved the flutter problem, in all its essentials, laying down the principles on which flutter investigations have been based ever since. They proposed the semi-rigid device; offered a series of "test determinants" to replace the much more cumbersome Routh criteria; studied the energy balance (including the dissipation function); looked at dimensional questions; and proposed graphical methods of solution which brought out clearly the effects of parametric changes. On the practical side, they recorded a long and painstaking series of experiments, both on flutter in a wind tunnel and on the determination of the aerodynamic derivatives needed for their theoretical study — 18 different derivatives in their ternary problem; they also measured the elastic terms and the moments and products of inertia. Finally, they listed preventive measures, covering almost every possible kind of instability. All this in less than three years.

I must say a word about the semi-rigid principle (incidentally 'quasi-elastic' would have been a better description), since this was probably the most important single step they made. An elastic wing can distort in an infinity of ways, and the varying aerodynamic loads produce corresponding distortions. Strictly, therefore, recourse must be had to differential equations in space variables as well as time. But, as Gates found, even an idealised wing then provides an intractable problem, and any real wing would be quite impossible to treat. A semi-rigid wing, however, has only a limited number of possible modes of distortion — in the simplest case, only one. I like to imagine such a wing as being constrained by some mechanism, similar to lazy tongs, or a gear train. This mechanism constrains the wing always to distort in the same mode; the only unknown then is the amplitude, which can be measured at any convenient spot. All forces can be integrated and related to this single unknown (described as a coordinate) and the equations of motion then relate to the coordinate and its time differentials only.

Frazer and Duncan introduced this concept almost casually: to quote "The problem . . . . . is still too complicated for an exact discussion, and approximate methods must be used . . . . . it will be assumed that the changes of mode with wind speed are of secondary importance. This procedure is virtually equivalent to the substitution for the real wing of a fictitious 'semi-rigid' one . . ." In passing, it may be noted that Duncan, writing some years later (R&M 1904), says the semi-rigid idea was first used by Rayleigh; but I think this does Frazer and Duncan less than justice. Rayleigh, in discussing the free vibrations of a conservative system, first proved the stationary property of the frequency of a normal mode, and deduced that an approximate mode might be used in practice. Frazer's and Duncan's problem was a vastly more complicated one, for which no such theorem could be deduced; nor were they, at that time, concerned with normal modes and natural frequencies. In my view, therefore, their use of the semi-rigid concept was a stroke of genius, which has been amply justified by the work of aeroelasticians over the years which have followed.

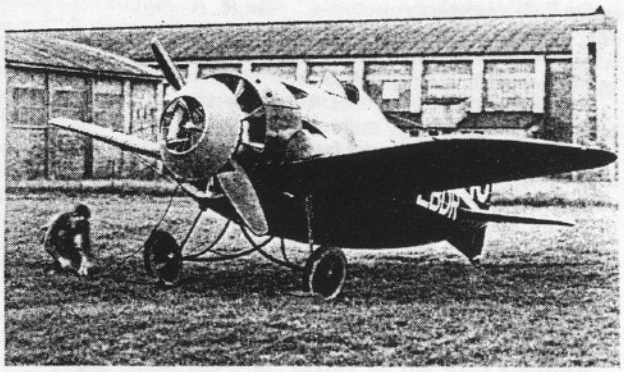
Before leaving the 1920s, I might mention one of the preventive measures advocated by Frazer and Duncan in R&M 1155; it concerns the irreversible aileron control. They record that the suggestion was due to Southwell. We are all familiar with irreversibility; for example, all car windows incorporate an irreversible device, so that turning the handle moves the window, but force applied to the window will not move it. Much work over many years was done on such controls, including the provision of artificial "feel" by springs. Eventually, when it became necessary to introduce powered controls, irreversibility (or at least near irreversibility) was universally adopted.

## Decade of theoretical advance

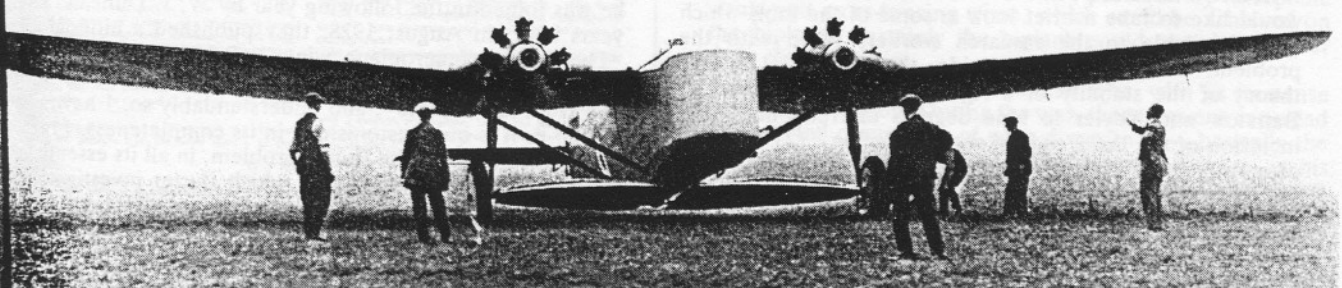
I come now to the 1930s of which I can speak from personal experience; for I joined Frazer and Duncan in January 1930 to help with the work on tail flutter which had been occupying their attention for the previous year. I have called it "the decade of theoretical advance" since it saw the diversification of aeroelastic studies in various directions; but also saw the emergence of studies of frequency-dependent aerodynamic derivatives — indeed, the beginnings of general unsteady motion theory — and the first practical use of the hitherto recondite branch of pure mathematics known as "matrices".

The beginning of the decade marked a major change in aircraft design; the disappearance of the biplane in favour of the monoplane. The device of "stressed skin structures", by which the doped fabrics of the past were replaced by load-bearing skins, made the unbraced cantilever monoplane wing, with its vastly better drag characteristics, a real competitor to the biplane; so much so that it was adopted almost universally for new designs.

We begin with a look at the phenomenon of aileron reversal, or more fully, loss and reversal of aileron control. This was a subject that came to the attention of Roxbee Cox and Pugsley at the RAE. Roxbee Cox had gone from Cardington to Farnborough about the beginning of 1930 and Pugsley followed him shortly afterwards. The reversal story



*Aileron reversal was first noticed during tests on the Bristol Bagshot, below, in 1927, but a somewhat similar problem arose with the earlier Bristol Racer, above.*



that awaited them had begun a few years earlier; tests of the new Bristol Bagshot in 1927 had shown that, as its speed was increased, the aileron power progressively reduced to zero and then became negative. The phenomenon was not catastrophic, but was clearly very dangerous. It has been described graphically to me by the late Cyril Uwins, who flew the Bagshot and by Russell who observed it from the rear gunner's cockpit. There had been a somewhat similar occurrence on an earlier Bristol monoplane, the Bristol Racer, but this was not followed up.

The Air Ministry decided to retain the Bagshot for experimental investigation of its structural stiffness; however, a quantitative explanation of reversal had to await the arrival of Pugsley. He pointed out that the upload generated by a downgoing aileron acted towards the rear of the wing section and so produced nose-down twist and a corresponding download; or alternatively that the down-going aileron effectively increased the section camber, with a corresponding increase in nose-down pitching moment. Roxbee Cox, meanwhile, had recognised that aeroelastic phenomena on a wing all resulted from changes of incidence, which in turn were determined by wing torsional stiffness. Accordingly he began a statistical study of the wing torsional stiffness of aircraft, in an effectively dimensionless form; this study provided the extremely valuable wing stiffness criterion which thereafter played such a valuable part in aircraft design. Together, Roxbee Cox and Pugsley were able to lay down quite precise requirements for the wing stiffness necessary to avoid aileron reversal troubles.

To continue with the RAE story for a while, I must record that the Establishment's contributions were wide-ranging and valuable; they were in large measure masterminded by Pugsley. He was the first to study the effect of wing density on flutter (though its importance was noted in the Flutter Bible); he was the first to point out that the longitudinal stability of aircraft could be affected by wing flexibility; he studied wing divergence as a phenomenon in its own right; he made parametric studies of flutter and also postulated a simplified theory of flutter. Finally, in an investigation of the non-linear characteristics of the extremely popular Frise aileron, he pointed out the importance of control circuit stiffness; all this in addition to the structural studies which were his first and enduring concern.

I must now return to the work of the NPL group. I shall begin by reference to a new phenomenon: tail buffeting. In July 1930 a Junkers F.13 monoplane, flying over Meopham in Kent, entered a cloud and broke up. The accident investigation which followed examined a wide range of possible causes, and concluded that it must have been due to oscillations induced on the tail by eddies shed from the wing at high incidence. Again, Frazer and Duncan led this investigation. Incidentally, such was the variety of phenomena calling for investigation at this period that Frazer, Duncan, and I rarely worked as a group, but in pairs; Frazer-Duncan, Frazer-Collar, Duncan-Collar. Later we were reinforced by the arrival of Scruton, W. P. Jones, and others. On the other hand, Duncan was appointed in 1934 to the Chair of Aeronautics at University College, Hull, and collaboration thereafter was much more difficult.

I have already said that I began flutter work, in collaboration with Duncan, in January 1930. We were investigating rudder flutter on the Parnall Pipit biplane. The investigation required some careful experiments on a model of the aeroplane in a wind tunnel, to find the derivatives to use in a theoretical investigation. The outcome was simple. The firm had mounted a tail lamp on the trailing edge of the rudder, at the location where it was most effective as an anti mass-balance weight, and so had promoted flutter in an otherwise stable system.

Within two years I was again working with Duncan, on three problems which, though apparently very different, were nevertheless related; airscrew flutter, matrix analysis, and frequency-dependent derivatives. Flutter of airscrews was the latest aeroelastic trouble; it followed the introduction of metal blades which played a part in the search for greater efficiency. Metal blades could be made much thinner than wooden ones; but they were naturally more flexible and had almost no internal damping, and were correspondingly prone to flutter. In Germany, Liebers and later Hohenemser had contributed to airscrew flutter studies; but Liebers in particular looked on airscrew vibration as a resonant phenomenon. The work done by Duncan and myself was both experimental and theoretical. For the experiments, we used model blades of reduced elasticity: they consisted of a spine of strip steel carrying wooden ribs, the whole covered in doped fabric. To view the blade oscillations, Duncan devised a most ingenious optical system. At the airscrew hub he placed a step-down gearbox which rotated a mirror at half the airscrew speed; the plane of the mirror contained the axis of rotation. This produced, for nearly half a mirror revolution, an unrotating image of an oscillating blade. This image in turn was viewed through a stroboscope, to make the oscillations apparently very slow. In this way it was possible to make visual observations of high frequency flutter at high speeds of rotation, and so to deduce the nature of the flutter and the modes and relative amplitudes involved, for various conditions and blade parameters.

The theoretical investigations were pursued along the lines of wing flexure-torsion flutter; but there were two stumbling blocks. First, what were the modes of vibration in flexure and torsion of these very un-wing-like blades, with their high aspect ratio and rapid spanwise variation of chord, thickness, and mass? Secondly, these blades fluttered at very high frequency. What effect did this have on the relevant aerodynamics?

We attacked the latter problem first. Following the work of Wagner on the growth of circulation round an aerofoil due to an impulsive change of incidence, Glauert had examined circulation growth on an aerofoil with a constant velocity in pitch, and had then, in 1929, solved the corresponding problem of an aerofoil oscillating with a single degree of freedom in pitch. He showed that the aerodynamic properties depended on what is now called the

*Tail buffeting probably caused an accident to a Junkers F.13 in July 1930.*

frequency parameter (or the "reduced frequency" in America), and tabulated the functions needed for numerical application. For the flutter problem, Duncan and I extended Glauert's work to take account of pitch and vertical translation (heave) simultaneously; we deduced both the direct and cross acceleration, velocity, and displacement derivatives for both motions; 12 derivatives in all. They were expressed as functions of Glauert's frequency parameter. Additionally, we obtained and tabulated the derivatives for a simple divergence, and formally solved the problem of exponentially increasing oscillations. We published this work in October 1932, in R&M 1500. Two months later we had applied the results to the airscrew flutter problem; our results for this are recorded in R&M 1518, dated December 1932. However, we entitled it "The present position of the investigation of airscrew flutter" since we had only been able to deal with blades of uniform section. Real shapes needed the help of matrix analysis, of which I shall speak shortly.

It is, however, convenient to mention here the early work of Theodorsen in America on oscillating air forces. This work was published in 1935 — three years after the period I am discussing. It contained two main advances on the British work; it added a third degree of freedom — an oscillating control surface — and it recognised Glauert's functions of frequency parameter as Hankel functions, fully tabulated in the literature of mathematical functions. Even Glauert had failed to notice this.

## Matrices — applied mathematics

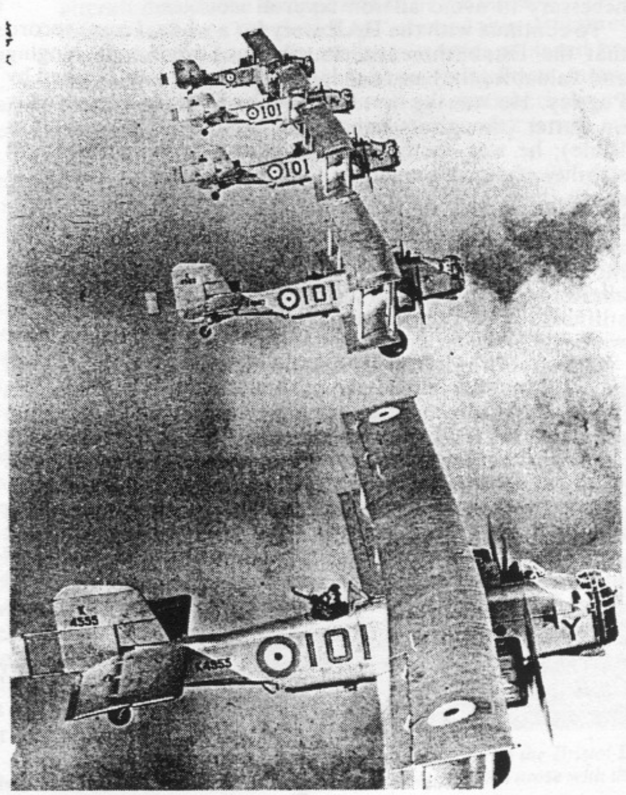
I must return now to the subject of matrices. Frazer had studied matrices as a branch of pure mathematics under Grace at Cambridge; and he recognised that the statement of, for example, a ternary flutter problem in terms of matrices was neat and compendious. He was, however, more concerned with formal manipulation and transformation to other coordinates than with numerical results. On the other hand, Duncan and I were in search of numerical results for the vibration characteristics of airscrew blades; and we recognised that we could only advance by breaking the blade into, say, 10 segments and treating it as having 10 degrees of freedom. This approach also was most conveniently formulated in matrix terms, and readily expressed numerically. Then — perhaps resulting from the notion of semi-rigidity — we found that if we put an approximate mode into one side of our equation, we calculated a better approximation on the other; and the matrix iteration procedure was born. We published our method in two papers in *Phil Mag*, the first, dealing with conservative systems, in 1934 and the second, treating damped systems, in 1935. By the time this had appeared, Duncan had gone to his Chair at Hull.

I will complete the matrix story as briefly as possible. Frazer and I collaborated in the next year or so in producing three or four more papers on matrices, including one on the effect of solid friction on flutter. These were presented to the ARC, which was somewhat perplexed as to how they should be published; vehicles such as the R&M series or *Phil Mag* were not thought suitable for description of these new techniques. Southwell then suggested that the authors of the various papers should be asked to incorporate them in a book, and this was agreed. The result was the appearance in November 1938 of "Elementary Matrices", published by CUP; it was the first book to treat matrices as a branch of applied mathematics. It has been reprinted many times, and translated into several languages and even now, after nearly 40 years, still sells in hundreds of copies a year — mostly paperback. The interesting thing is that the authors did not regard it as particularly good; it was the book we were instructed to write, rather than the one we would have liked to write.

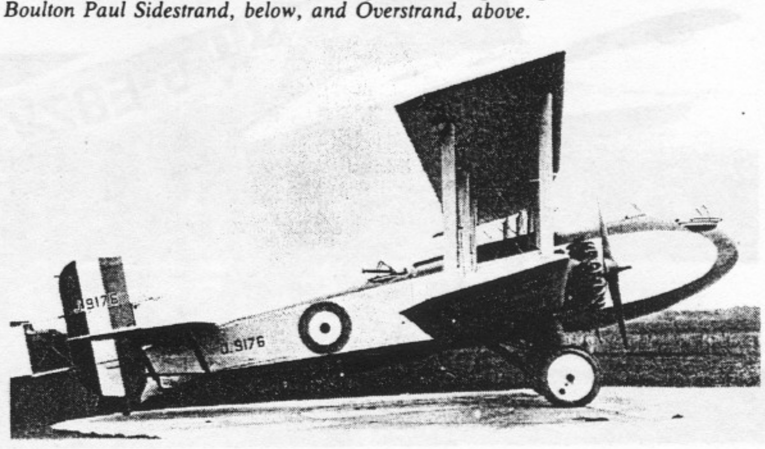
To go back to the NPL work; while Duncan and I were otherwise engaged, Frazer, with Scruton, was dealing with a serious occurrence of flutter on the Puss Moth aeroplane.


*Serious flutter problems arose on the de Haviland Puss Moth. The aircraft in this picture was used by Jim Mollison for his famous North Atlantic flight.*

The ailerons of this aircraft had been mass-balanced, by offset weights under the wing, and flutter should have been impossible. However, the wing was stayed by staggered V-struts; and if a bending load was applied, chordwise movement was induced by the struts, as well as bending. These chordwise movements promoted aileron rotation because of the mass-balance offset, and so produced flutter. But it was a long job to find this out. Later, Frazer and W. P. Jones investigated a practical method of flight flutter testing proposed by von Schlippe in Germany; and W. P. Jones himself began his long and valuable researches into flutter aerodynamics.



*Servo tabs fitted to rudders caused flutter problems, beginning with the Boulton Paul Sidestrand, below, and Overstrand, above.*



# Aeroelasticity — *continued*

Before he went to Hull, Duncan and I collaborated in treating yet another form of flutter; it involved the servo-tabs fitted to rudders. There had been several cases of this trouble, beginning I believe with the Boulton-Paul Sidestrand and Overstrand. We dealt with a case on the Gloster G.33 Goshawk troop carrier which we refer to in our report as Aeroplane X. This was in 1933; tab flutter was to remain a problem for many years.

Finally, a reference to one other development. It was clear that, as aviation expanded, flutter would involve an increasing number of degrees of freedom. But the difficulty of the classical flutter solution increased roughly as the factorial of the number of degrees of freedom. Since, in those days, a binary calculation took about a month and a ternary took three months, six degrees of freedom would require 30 years. The problem would involve the evaluation of 729 determinants, each with 36 elements, the elements being in general functions of the unknown speed and frequency. This done, the determinants would have to be combined in a most complicated way to find the test functions, which in turn would have to be solved for speed and frequency.

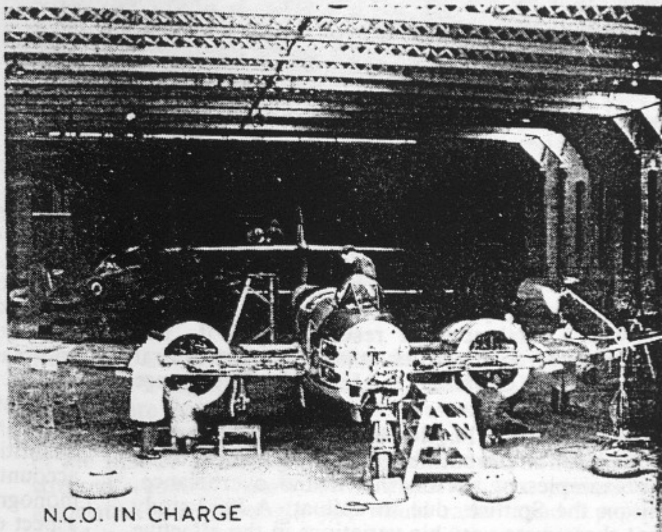
It occurred to me that an inverse method could circumvent this virtually impossible procedure. Instead of working throughout with an unknown speed and frequency, prescribe them from the beginning with some values near the critical; these could be estimated from a simplified binary approach. However, since the values would not be exact, it would be necessary to imagine the imposition of an externally applied force, oscillating at the prescribed frequency, to maintain the sinusoidal oscillations. But, with all the coefficients now fixed numbers, the evaluation of this force was simply a case of solving a set of linear algebraic equations. By repeating the calculations, first with speed varied, then with frequency, it would be possible to determine values for which the externally applied force vanished; i.e. the critical values. (In retrospect, it occurs to me that this idea might have been sparked by von Schlippe's proposals for flight flutter testing, which involved imposing an oscillating force on an aircraft in flight and studying the sinusoidal response at varying speeds and frequencies). Duncan saw at once that by this means we could accomplish various objectives, including checking the validity of the semi-rigid principle, and finishing our study of airscrew flutter. He undertook all the work with the aid of Miss H. M. Lyon, who was then living near Hull so that communication between them was easy. The results of this work were published in 1936 as R&M 1716.

## The fifth decade

At the beginning of the fifth decade the Second World War had been in progress for four months and there resulted major changes in the aeroelastic scene. Frazer was required to undertake other work; Duncan left Hull to go first to Farnborough, then to Exeter, then back to Farnborough, but did no flutter work. Roxbee Cox was in Whitehall and Pugsley was Head of SME Department at Farnborough. I was translated from Teddington to Farnborough to look after its aeroelastic work.

From this time on, the NPL concentrated very largely on its immensely valuable but protracted work on unsteady aerodynamics; however, they did conduct one or two flutter investigations. Frazer (whose main scientific interest, throughout his life, was fluid mechanics) lent a hand whenever he could; in particular, he made an early study of Possio's derivative theory, with compressibility effects at subsonic speeds included. The team which worked on aerodynamic derivatives included, as well as Frazer himself, W. P. Jones, Scruton, Bratt, and Lambourne. In addition, since the RAE had no wind tunnels available for aeroelastic problems, the NPL group undertook model experiments to provide necessary data for some of the RAE investigations.

The Farnborough work was, naturally, concerned mostly with the problems of actual aircraft, with the urgency of a war situation always pressing. By the end of the war I had a powerful team, including Broadbent, Miss Puttick, Miss Victory, F. Grinsted, Molyneux, Fiszdon, Minhinnick, Jahn, Buxton, and Sharpe, while Professor Temple undertook two important specific investigations. Our work, *inter alia*, involved field investigations (including accidents), laboratory work on full scale aircraft and components, theoretical studies, and design requirements. The number of problems calling for trouble-shooting which came our way was legion, so much so that I cannot possibly mention them all; nor would a catalogue be of much interest. But before I describe some of them, I must mention two factors which brought complication with them. The first was the




*In this picture of a Gloster Meteor undergoing inspection, the way in which the engines were buried in the wing is clearly shown.*

advent of the jet engine, which not only offered higher speeds, but also presented a particular problem in that in the Meteor it was buried in the wing, whereas conventional engines were nacelle-mounted well forward of the wing; the second was the increasing importance of compressibility effects.

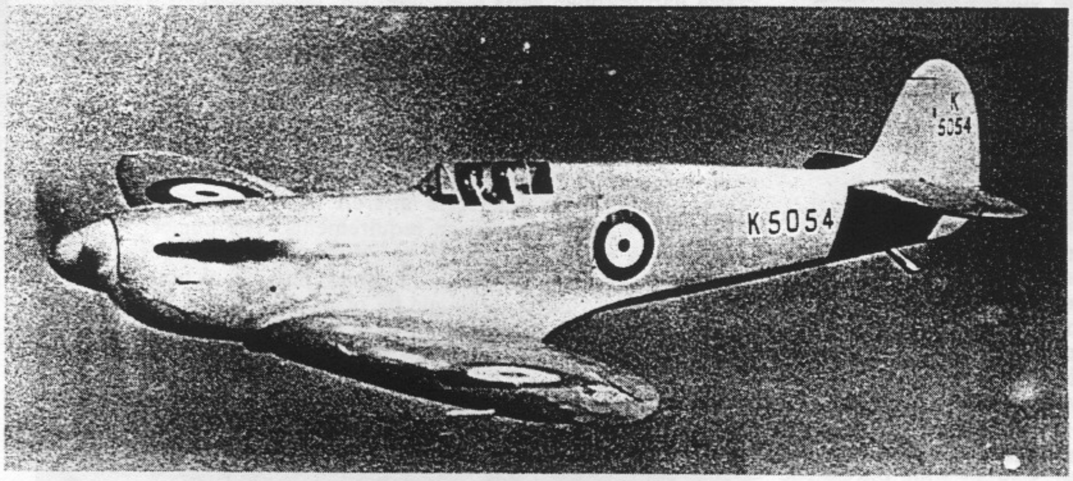
## Four major problems

Now to some of the problems, which I shall arrange loosely under the following headings; vibration, quasi-static problems, flutter and theoretical investigations.

*Vibration.* We were much plagued by cases of airframe vibration, usually induced by the engines or propellers. These involved a lot of work, but our efforts were rarely more than partially successful.

We regularly made resonance tests of aircraft, over a wide frequency range, to determine both vibration characteristics and modes appropriate to flutter; often we made stiffness measurements also, on a multiplicity of components. Originally the techniques, though ingenious, were very crude; and progressively, by devising new sensors and actuators — Fiszdon and Molyneux were much involved in this — we improved our methods and results greatly.

A problem having something in common with flutter was posed by nose-wheel shimmy; a large angle oscillation maintained by tyre frictional forces. This problem was neatly and expeditiously explained by Temple, while a




*Aileron snatch and overbalance, due to upfloat, were particular problems on the Spitfire. The prototype Spitfire is seen in this picture.*

practical solution was offered by the Marstrand twin-tread tyre.

*Quasi-static problems.* One of the first new problems in this field was that of loss and reversal of elevator control due to tailplane and fuselage flexibility. It was prompted by troubles on two wooden aircraft being developed at the time (1942) and was attacked by Grinsted and myself. It was soon apparent that the phenomenon was an aspect of the longitudinal static stability of an aircraft, and an important one; there was a fatal accident resulting from it. The investigation pointed the need for standards of structural stiffness for tailplanes, elevators, and fuselages. These were introduced in due course.

I remarked earlier that Pugsley had shown the need for standards of aileron circuit stiffness. Despite his work, there were many examples of aileron snatch and overbalance, particularly on the Spitfire, due to upfloat. A field study showed that there were very big variations in the effective stiffness, resulting from lack of pretension; we found tensions varying from three to 150 lb in the Spitfire's cables.

A brief reference may be made to a French proposal which we were required to assess; the Rouanet-Rey hinged wing. The proposed wings had hinges with skewed axes, intersecting in the rear fuselage; movement would be constrained by stiff springs. Thus, it was another proposal for the deliberate use of aeroelasticity. Downward bending of a wing produced increased incidence and an upward airload. Rolling was to be achieved by differential elevator — elevon — control. A rolling moment applied to the fuselage at the tail caused the wings to lag and so produce their own rolling moment, without the need for ailerons — a kind of automatic wing warping. Additionally, upgusts would cause the wings to flex and shed load; a heavy landing would induce additional lift. It was an ingenious proposal, but the practical difficulties of the hinge design and spring constraint precluded further work.

Loss and reversal of aileron control assumed increasing importance as speeds rose. Broadbent and I devised an analysis of the rolling power of an elastic (as distinct from a semi-rigid) wing. It was an iterative method of solution of the integral equations; it was also well-suited to the introduction of compressibility effects in the aerodynamic forces involved.

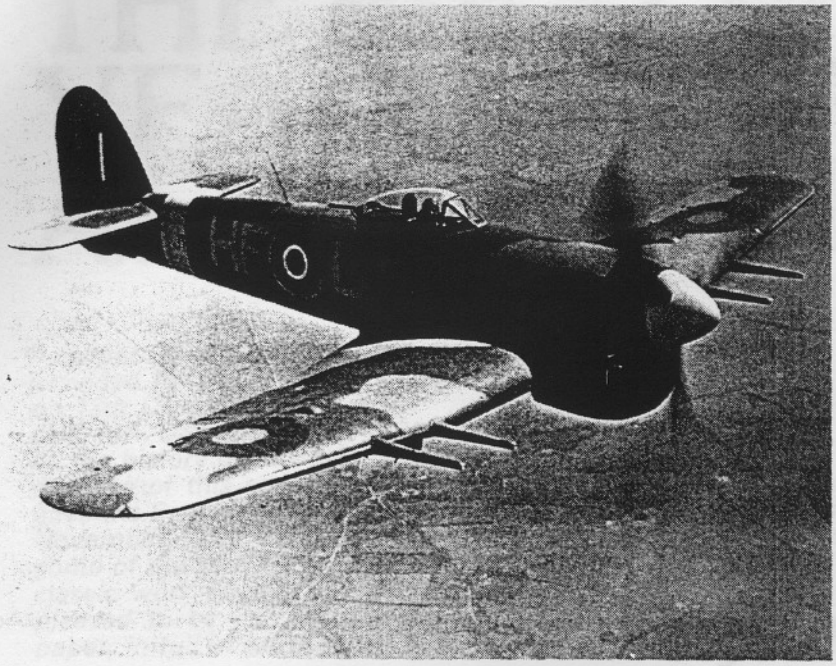
*Flutter.* This continued to occupy much of our time. One perpetual problem was tab flutter, which was something of a plague; trim and geared tabs with backlash were very prone to flutter, while spring tabs with various gearings, shapes, and sizes were growing in popularity. The early work on this topic was done by Frazer and W. P. Jones at the NPL; they showed that the length of a tab mass-balance arm must be strictly limited. Sharpe and I at RAE extended this to show that the balance mass must lie within a limiting circle — a result implicit in the NPL work; it is easy to show geometrically that this is so. Unfortunately, before this work was widely known, there was a fatal accident. A Meteor with an experimental aileron spring tab was being flown from Gloster's aerodrome at Moreton Valence by their chief test pilot. It crashed, and was virtually pulverised. Since a witness had said the wings and ailerons were "waving about" I was called in, and conducted what must have been the shortest accident enquiry on record. I was taken to a hangar, where most of the pieces had been thrown into a heap; but the firm had sorted out a few pieces of aileron. I picked up one piece, a wedge of metal on a steel bar, and asked what it was. "The tab mass-balance weight," I was told. I said at once "no need to look further; it's an anti-mass-balance weight, promoting flutter". The arm was far too long.

It was necessary to give a good deal of attention to mass-balancing, since the factors affecting it vary widely, and quantities such as virtual inertia may have to be taken into account. In after years Templeton wrote a substantial monograph on the subject. I will refer here to one other aspect only; an example of remote mass-balancing. Several aircraft of a new fighter type (the Typhoon — Ed) crashed, after detachment of the whole tail unit in flight, and none of the pilots survived. Tests showed strength to be more than adequate for normal loads; so we had to postulate an abnormal load such as that due to flutter. Resonance tests were revealing; there was a node in the fuselage, near the tail, at the second resonant frequency. And the elevator was statically balanced by a mass attached to the control circuit, two or three feet forward of the elevator hinge and near the node. In this node, all the mass was doing was to increase the elevator inertia. Calculations by Jahn, Buxton, and Minhinnick predicted flutter near the top speed of the aircraft; remedial measures stopped the accidents.

At a fairly early stage in its history, we put in hand calculations, I think in four degrees of freedom, of possible wing flutter on the Meteor. We also asked NPL to conduct some fairly extensive wind tunnel tests, which were made by Lambourne; there is a useful film of his results in existence. Fortunately, both theory and experiment showed that buried engines offered no great hazard.

Finally, as we accumulated information on wing flutter, we realised that the simple wing stiffness criterion originally proposed by Roxbee Cox could be unnecessarily demanding, and that there was a good case for its elaboration to take account of variations in some of the wing parameters affecting flutter. The new criterion was developed by Broadbent, Miss Puttick, and myself.

*Theoretical investigations.* In 1943, the chief technician of Miles Aircraft Ltd appeared in my office to tell me, in the greatest secrecy, that his firm had been asked by the Ministry to build an experimental supersonic aircraft. It was to have straight wings, of lenticular section, with a thickness chord ratio of 4%. He said he must know, within three weeks, how stiff it had to be. Since we had no knowledge whatever of supersonic aero-dynamical derivatives, this was something of a proposition. However, within the specified time limit, I had personally extended Ackeret's theory to



*Flutter near the top speed of the Typhoon was the cause of early accidents in which the whole tail unit became detached. In this picture reinforcing straps can be seen towards the rear of the light coloured band around the rear fuselage.*

# Aeroelasticity *— continued*

provide the derivatives needed for aileron reversal, and by an assumption equivalent to what was subsequently called "piston theory" had evolved a complete set of flexure-torsion flutter derivatives, which were correct for high Mach number, and had specified the required stiffness. Alas, the aircraft was, for other reasons, never built. Since my analysis was rough and ready in the extreme, I also asked Temple and Jahn to do a proper job. They did so, but it occupied two years.

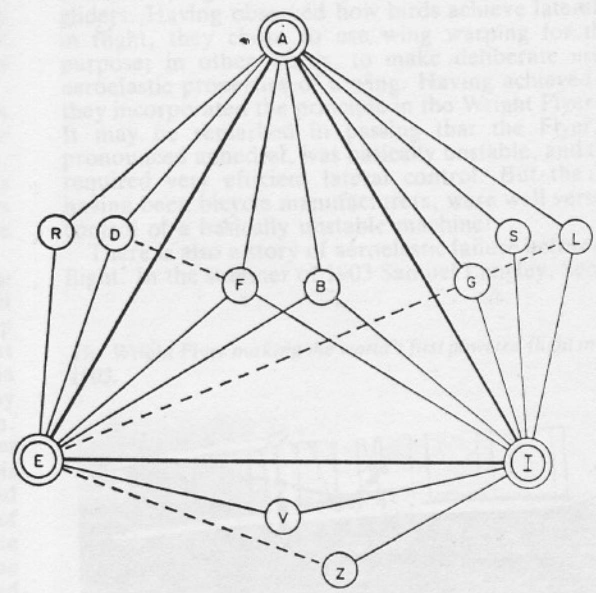
Towards the end of the war, we became aware that experiments were being made in Germany on sweepback as a device for avoiding compressibility effects, and we began studies of the implications of this for aeroelasticians — studies which were to persist for some years. As opportunity offered, and experience grew, we also developed a variety of theorems on matrices, and contributed to thinking about semi-rigidity by establishing the stationary property of critical divergence speed with respect to mode variations.

## Triangle of forces

I left Farnborough in 1946 to take up my Chair at Bristol, where Pugsley was already Professor of Civil Engineering; many other members of the aeroelastic team also went to other work. The NPL team similarly changed direction; Frazer's group was asked to study the aeroelastics of a proposed Severn bridge, and Duncan had new duties at Cranfield. At Farnborough, there was a major job of tying up loose ends left by the war, and of reorientation for a future involving compressibility and sweepback, which occupied much of the rest of the decade. I shall conclude, therefore, with a reference to one other piece of my own work. At Farnborough, I had been privileged to be in charge of work on the widest possible spectrum of aeroelastic phenomena, and it had been borne in on me that these hitherto discrete studies were really all part and parcel of the stability and control of a flexible aeroplane. I put these ideas together in a paper "The expanding domain of aeroelasticity" (JRAeS, August 1946), and by way of simple illustration, included a diagram of the 'triangle of forces' — aerodynamic, elastic, and inertia. Its reception was more than gratifying; clearly the idea of unified treatment was widely acceptable. I will quote only one comment; that of I. E. Garrick of America, who had worked with Theodorsen — "It is both a chart and a compass for aeroelasticians."

## Envoi

I am well aware that history should be objective, and that my paper is highly subjective and personal. I have given hardly more than a mention to overseas work, though this is in part due to the fact that the majority of aeroelastic work in the first 50 years was done in this country. I have also stressed, unduly, work with which I was personally associated. But I thought you would rather read a paper based on personal experience, even if it can at best be described as a biased look at history.




*Professor Collar's Triangle of Forces. A, aerodynamic forces; E, elastic forces; I, inertia forces; F, flutter; B, buffeting; S, stability and control; D, divergence; R, reversal of control; G, gusts; L, loading problems; V, mechanical vibration; Z, impacts.*