Some Speculations on the Aeroelastic Problems of Rotary Wing Systems

By PROFESSOR A R COLLAR,
M A , D SC , F R Ae S

A Lecture presented to The Helicopter Association of Great Britain and The Royal Aeronautical Society, on Saturday, 26th November, 1949, in the Library of the Society, 4 Hamilton Place, London, W 1

J S SHAPIRO, DIPL ING , A F R Ae S ,
(*Member of Council*)
IN THE CHAIR

Introduction by the Chairman

This afternoon we are about to hear a lecture entitled ' Some Speculations on the Aeroelastic Problems of Rotary Wing Systems ' This sounds a terrifying title—rather like the sub-titles of Victorian novels—but I suggest the short title, ' Collar on Blade Flutter ' I have the pleasant task of introducing to you the lecturer In view of Professor COLLAR's reputation, I am really not sure whether I should introduce the lecturer to the Association or the Association to the lecturer I shall therefore do both without, I hope, imposing upon your patience

Professor COLLAR is easily introduced, because he is one of those people who hold their jobs After taking his degree in Cambridge, he served for 12 years on the staff of the N P L , and for 4 years at the R A E , before being invited to become the Sir George White Professor of Aeronautical Engineering in the University of Bristol

Mr COLLAR has created a great reputation for himself through numerous articles, research reports and memoranda published and otherwise Selecting only those with which I personally happen to have a nodding acquaintance I would only mention work on cascade theory and flutter He was also co-author of a book on " elementary matrices " A most inviting title I am assured that there *is* a point of view from which those matrices *are* elementary

I have heard it said by a character in one of Bernard Shaw's plays that Mathematics is a passion I can assure you that this afternoon you have a most passionate lecturer who has practiced great restraint He is going to show a film in which I believe the hero is an engine having a flutter with a wing

Now, Professor COLLAR, the Association is very proud to have you with us this afternoon The Helicopter Association of Great Britain is a society of enthusiasts who agree that Helicopters are a " good thing," but disagree on everything else Indeed, if two members hold the same opinion, we believe that too few opinions are chasing too many members

In fact, we seem to have a terrible reputation with lecturers Somewhere in his lecture Professor COLLAR even talks of shooting He expects to be shot at and even shot down I may assure our distinguished lecturer that nothing like that has ever happened here Nevertheless I am looking forward to a lively and fruitful discussion

I would also like to welcome our guests of the Royal Aeronautical Society, whose presence will contribute to making this occasion a highlight of this season

PROFESSOR A R COLLAR

I think it will be prudent, as well as honest, to begin this lecture by confessing at once that I know remarkably little about rotary wing aircraft , prudent, because it may very well be completely obvious by the end of the lecture, and honest, because I have no wish to pose as an expert in a field of interest to which I am but a very recent newcomer It was for this reason that I asked to be allowed to add " Some speculations on " to the title proposed for this lecture by the Council of the Helicopter Association I felt that very few people, and certainly not I, could speak with authority on this question , on the other hand, having worked for some time in the general field of aeroelasticity, I thought I could speculate on aeroelastic effects in rotary wing systems as well as, for example, a helicopter expert with little knowledge of aeroelasticity

Whatever the outcome of the speculations, I am sure that the most useful thing I can do is to give a description and, as far as possible, an explanation of some of the aeroelastic effects that have been experienced on conventional aircraft partly because it may well point the way to the most profitable form of speculation, but principally in the hope that it may help those of my listeners who are well versed in the problems of rotary wing systems but have no close acquaintance with aeroelastic theory

AEROELASTICITY

We must begin by defining the phenomenon to be discussed So far as I am aware, there is no generally accepted definition , but aeroelastic science, as usually understood, may be described as the study of the dynamics of an aircraft in which elastic deformation plays an essential part

To interpret this, let us examine Fig 1 An aircraft is, in general, subjected to forces of three main kinds external forces (principally aerodynamic in origin), elastic forces (due to deformation) and inertia forces (arising from acceleration) When all three types of force contribute to the motion of the aircraft, we must study the dynamic stability , dynamic instability, in this case, is a form of flutter But

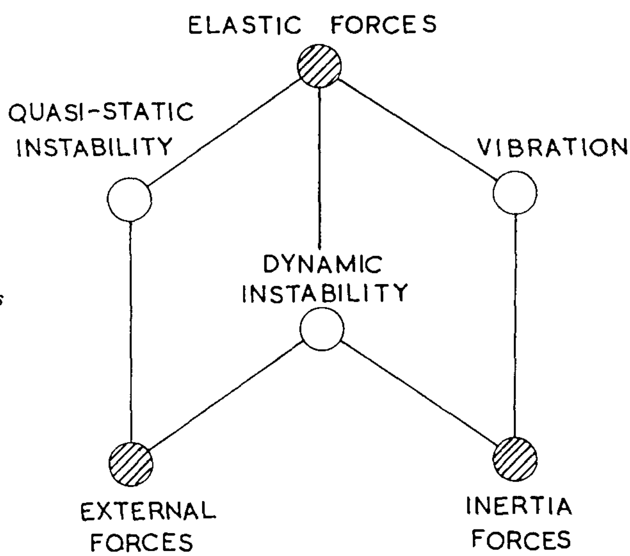


Fig 1
Diagram of forces

the forces may operate together in pairs, with the remaining force inoperative If the accelerations are so slow that inertia forces are negligible, we study the static, or strictly quasi-static, stability Similarly, elastic and inertia forces together govern the vibration† characteristics, while the external aerodynamic forces interact with the inertia forces in the problem of rigid aircraft stability

If, however, we regard aeroelasticity as essentially involving elastic deformation, we shall exclude rigid aircraft dynamics, except as a limiting case corresponding to vanishingly small deformations The rigid aircraft can be used to provide a standard of comparison, and its study is obviously closely allied with aeroelasticity, but we have excluded it from Fig 1

We are left with quasi-static instability, dynamic instability, and vibration as the main constituent phenomena of aeroelasticity Today, I shall restrict myself to the first two This is not to say that vibration is not an important phenomenon in aircraft, the reverse is true, and particularly of rotary wing systems (any massive rotating system—engine, propeller, rotor—always provides vibration problems for obvious reasons) But the study of vibration is much older than aeroelasticity, its principles are better understood and much more widely known than those of aeroelasticity, and its cure is usually a matter of more careful manufacture of matched rotating parts, improved balancing, and insulation of the structure from the forcing impulses (In rotary wing systems, the rotor articulations contribute very greatly to this insulation)

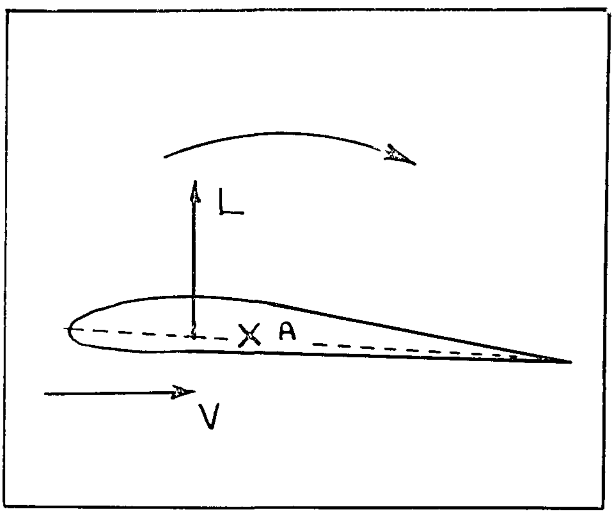
Under the two headings remaining, there are four phenomena which have been noted and studied on conventional aircraft divergence, control reversal, and distortion effects on static stability as quasi-static phenomena, and flutter as a dynamic instability We will examine each of these briefly

Quasi-static Aeroelastic Phenomena in Conventional Aircraft

Divergence

Divergence is a phenomenon analogous to the failure of an Euler strut after some critical load is reached, the deformation increases unidirectionally until failure occurs In the case of aeroelastic divergence, the critical load of the strut is replaced by a critical airspeed Fig 2 illustrates how the phenomenon occurs

Fig 2
Forces on wing section



† Some external (or internal) force is necessary to produce vibration but its magnitude may be insignificant compared with the elastic and inertia actions

A section of a wing or similar component has a flexural centre represented in the diagram by the point A Normal load applied at A produces bending but no twist , a pure couple produces rotation about A but no bending A force L such as that shown can be regarded as a force L at A together with a couple, so that both bending and twist result from it

Now if the wing section shown is given a small displacement in twist when in an airstream, a force L will result, acting approximately at the quarter-chord point , L will be proportional to the displacement, so that a disturbing force proportional to displacement exists This is opposed by the elastic forces, also proportional to displacement Now the restoring elastic force per unit displacement is fixed , but the disturbing air force per unit displacement is proportional to the square of the speed, or rather to the dynamic pressure Thus at low speeds the restoring force is the stronger, and the system is stable , but at some critical speed the forces will be equal, giving neutral stability , at higher speeds instability results

It may be noted that, while bending displacement occurs, it produces no incidence change and consequently no air force , thus, while it is a concomitant effect, it does not affect the stability of the system

To avoid the risk of divergence, the disturbing couple must be reduced or the elastic stiffness in torsion increased Now it is not possible to alter appreciably the magnitude of the load L per unit displacement, nor its point of action , and resort must therefore be had to moving the flexural axis A forward toward the quarter-chord point If the structure is so designed that the flexural axis is at the quarter-chord point then the divergence speed is infinite

Under the heading of divergence we must also consider an allied problem, which is not, however, a stability problem the twist due to section camber The parallel here with the Euler strut is the distortion under end-load of an initially bent strut If an aircraft wing has a cambered section, then as the speed increases there is an increasing torsional couple applied to the wing, which will therefore twist The couple is independent of the twist, so that the latter will not increase indefinitely, as in the case of divergence , but it is possible for the twist to reach a magnitude sufficient to cause structural failure This effect, can of course, be eliminated by the use of symmetrical aerofoil sections

Divergence and the allied problem of wing twist represent the simplest of aero-elastic phenomena they are readily understood and the curative measures required would be relatively straightforward In practice, however, divergence tends to occur at higher speeds than other aeroelastic phenomena, so that, in designing to avoid the latter, divergence is automatically excluded from the flight speed range I know of only two recorded cases One concerned the prototype of a high altitude version of the German Me 109, which had extended wing tips built into the existing wing structure with no additional stiffness the wings twisted off in the first high-speed dive attempted, and the design was subsequently abandoned The second was also a prototype, in this case a glider with a very slender and flexible tailplane This aircraft appeared to suffer from a form of elevator-tailplane divergence , the tail unit twisted off in a dive and the aircraft was lost

Loss and Reversal of Control

This form of aeroelastic trouble has in practice been confined to aileron control, and we shall discuss it in this context

The phenomenon was first noticed very many years ago I believe on the Bristol Racer, one of the earliest unbraced monoplanes The pilot found that as he increased speed, the lateral control, at first normal, fell off, until at one particular speed lateral stick movement produced no response He actually flew to a still higher speed, and found that stick movement then produced roll in the wrong sense

It was not long before aeronautical engineers were designing for sufficient wing torsional stiffness to avoid this trouble , but it has remained with us, and its importance has grown with increasing speeds It is true to say that in many modern high-speed aircraft the wing design is largely dictated by the necessity to avoid aileron reversal

The next figure illustrates how the phenomenon arises The first diagram shows a wing section in which the aileron is displaced downwards This produces, in the first instance, an upward unbalanced force R, so that roll begins, with the section moving upward In the second diagram, we see that the rolling velocity w which results, when combined with the forward speed V, produces an effective

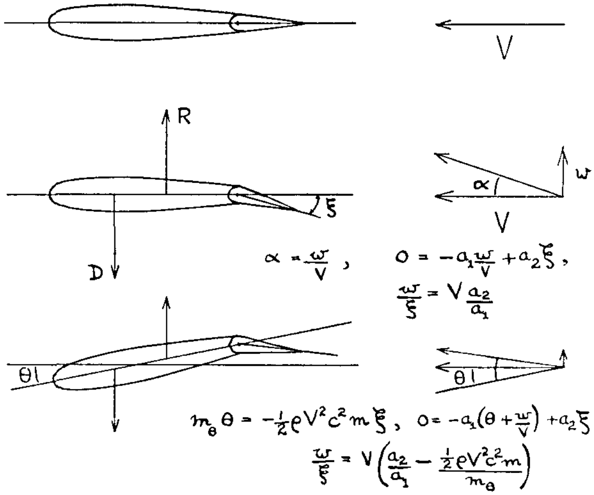


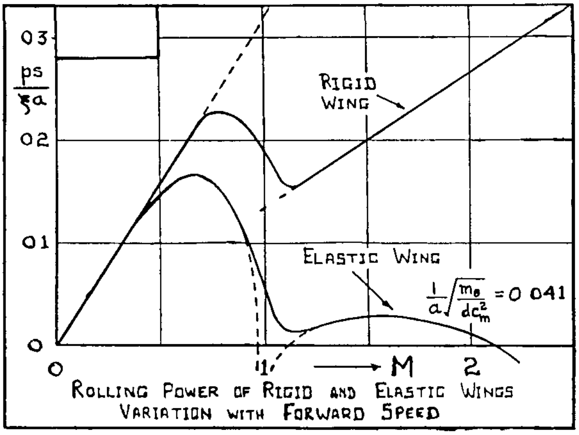
Fig 3 Generation of twist

negative incidence which generates an adverse force D (the damping in roll) balancing the initial force

For a rigid wing, that would be the whole story But the centre of pressure of the initial force R is well aft of the quarter-chord line, while the force due to incidence change D acts approximately at this line The result is a couple which, in the practical case, must twist the wing in a nose-down sense, as in the third diagram This contributes to the force D, and the aircraft does not then require to roll so fast before balance is achieved , and since the couple increases in proportion to the dynamic head, there will be one speed for which the adverse twist due to the couple itself produces a negative force balancing the positive force supplied by the aileron , and no roll results At higher speeds negative roll would occur

Figure 4 shows a theoretical calculation of this effect In place of forward speed, the Mach number is used as abscissa, with the rolling velocity as ordinate Compressibility effects have been included in the calculations, and supersonic as well as subsonic conditions are shown That this is not an unsupported calculation is demonstrated by the next figure, which shows a similar calculation for the Mustang

Fig 4 Rolling Power



https://doi.org/10.1017/S2753447200000688 Published online by Cambridge University Press

aircraft and comparative measurements obtained from flight tests So far as the tests go, the agreement is excellent

Distortion Effects on Longitudinal Static Stability

We now turn to the last of the quasi-static phenomena, and consider the way in which longitudinal static stability is affected by distortion Now by static stability is meant stability in conditions of flight for which normal acceleration is absent, that is, conditions in which attitude and speed are simultaneously varied so that the lift remains constant

The measure of stability usually adopted for this case is elevator angle to trim Stability can be examined in two ways either the aircraft can be disturbed, and the motion due to the resultant unbalanced forces studied, or a balancing force can be applied to create equilibrium in the disturbed position The latter is the more realistic approach, since it represents what the pilot will in fact try to do In the case of longitudinal disturbance, he will move the elevator to maintain equilibrium,

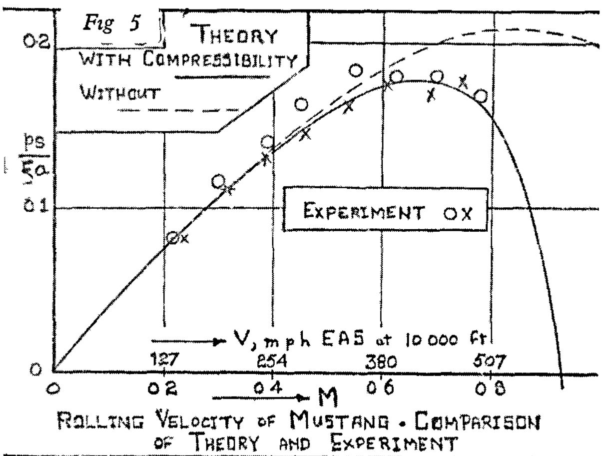
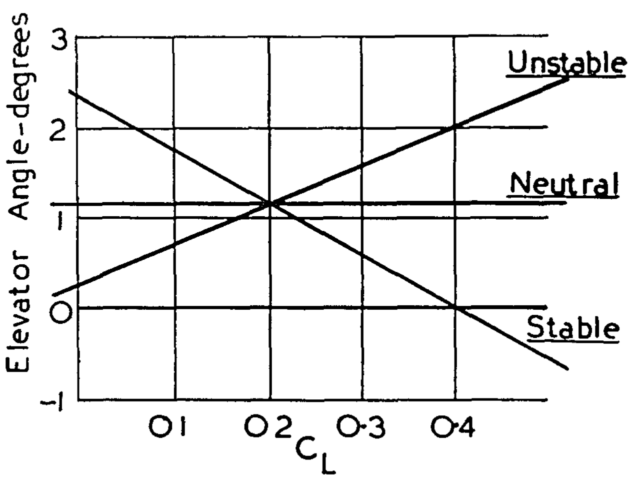


Fig 5
Rolling velocity of Mustang

Fig 6
Elevator angle to trim



that is, to trim the aircraft, and the magnitude of the movement is a measure of the stability

It is readily demonstrated that, for a stable aircraft, increasing speed requires down elevator, that is, a positive movement, further, if the elevator angle is plotted against lift coefficient the curve is a straight line with negative slope Figure 6 shows these trends

We now consider how fuselage flexibility affects the picture The next figure shows diagrammatically the principal loads which operate Suppose now that the speed is increased (C_L reduced) at constant lift The pitching moment due to lift is unchanged, but the wing camber gives an increased nose-down moment For equilibrium, this requires an increased down-load on the tail unit In consequence of this load, the fuselage bends, and the bending gives an increased tailplane incidence Compared with the rigid fuselage, an up elevator angle is evidently needed to give the same load Thus, superposed on the down elevator resulting from decreased C_L, we have up elevator as a consequence of bending, the bending is thus destabilizing

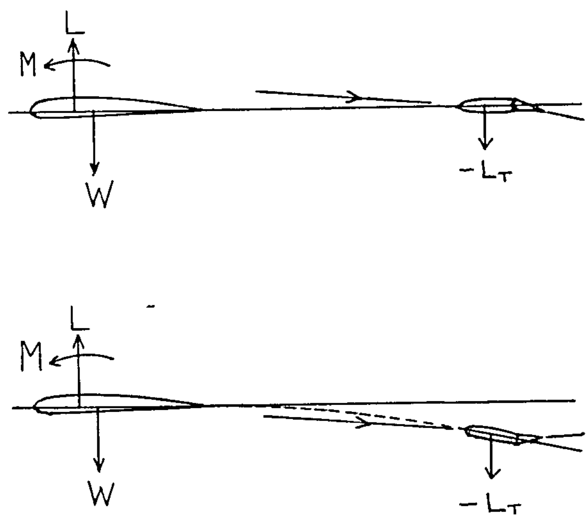


Fig 7
Fuselage bending

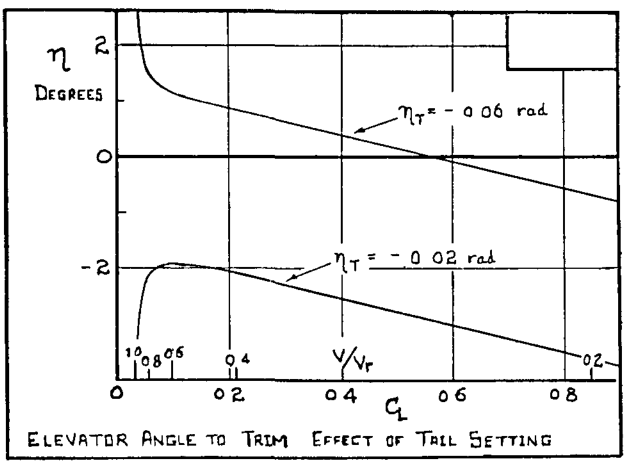


Fig 8
Elevator angle to trim effect of tail setting

https://doi.org/10.1017/S2753447200000688 Published online by Cambridge University Press

The amount of the destabilizing effect depends on the magnitude of the bending, which is clearly greatest at high speeds, and, in fact, as the speed is increased positive stability gives way to neutral stability and then instability

I have no slide showing this effect quantitatively, but similar effects result from tailplane twist In this case, however, the sign of the twist depends on the sign of the elevator movement, as well as on the initial setting of the tailplane with respect to the wing (the longitudinal dihedral) It follows that tailplane twist can either increase or decrease stability, depending on the setting Figure 8 shows the manner in which this occurs Both effects are undesirable If the aircraft becomes increasingly stable, it may involve very large stick forces at high speed, and indeed, since the elevator travel is limited, it may not be possible for the aircraft to reach its top speed The strong destabilizing effect may mean that at high speed there is no stick travel left to pull the aircraft out of a dive

It is not uncommon to find that one distortion effect is set against another to produce good handling characteristics Thus a tailplane setting may be deliberately adjusted in the nose-down direction to give an increasing stability which will offset the loss in stability due to fuselage bending The danger here is that large and unsuspected deformations may take place My next figure shows the calculated

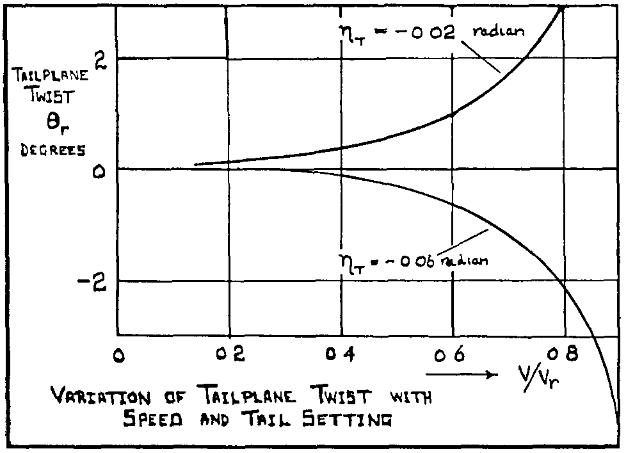


Fig 9

Variation of tailplane twist

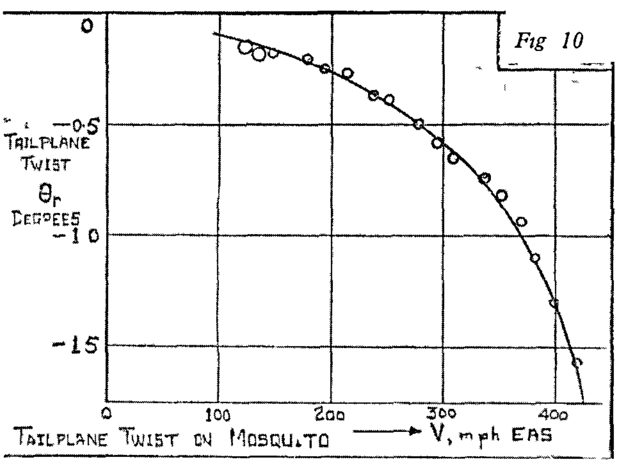


Fig 10

Tailplane twist on Mosquito

https://doi.org/10.1017/S2753447200000688 Published online by Cambridge University Press

tailplane twist corresponding to the last diagram, and Figure 10 some actual flight measurements made on the original Mosquito tailplane The very rapid increase of twist with increasing speed is significant

I do not wish to expand this theme, it will suffice to add that longitudinal stability can be considerably affected by the distortion of several other components—for example, wing twist, elevator twist, panel deformation—and that it has required much intensive study

Dynamic Aeroelastic Phenomena

Flutter

It would be quite impossible, in the short time at my disposal, to give anything but the most elementary introduction to this subject Flutter has been observed and studied in all the various combinations of movements given by the following list

Wing
- Flexure—aileron
- Torsion—aileron
- Chordwise movement—aileron
- Flexure—torsion

Rudder
- Fuselage torsion—rudder
- Fuselage bending—rudder
- Fin bending—rudder

Elevator
- Fuselage torsion—elevator
- Fuselage bending—elevator
- Tailplane bending—elevator

Tab
- Control surface rotation—tab rotation

Propeller
- Flexure—torsion
- Torsion—edgewise bending

All these components may be taken as they stand, with the degrees of freedom in pairs, or in multiple combinations, or combined with any of the six rigid body degrees of freedom of the aircraft The study of flutter began in 1916, and has occupied the attention of scientists and mathematicians in the aircraft industry to an increasing extent ever since

I am therefore going to attempt only a description of the simplest forms of flutter, in order to try to deduce those factors which tend to promote or suppress the phenomenon Regarding the nature of flutter itself, it is an oscillatory instability, usually of an explosive character structural failure of the aircraft can result within a second or two of the onset of flutter While this is not always the case, and flutter can be relatively mild, it must always be taken seriously

We must first of all note that flutter requires more than one degree of freedom for its promotion, unless the component is at or beyond stalling incidences Motion in a single degree of freedom is almost invariably damped Moreover, the two degrees of freedom concerned must have coupling forces between them, that is, motion in one mode tends to promote motion in the other, and conversely Given these conditions, it is possible for the component concerned to act as a sort of engine which extracts energy from the airstream

To see how this can happen, let us examine Figure 11 Each diagram shows one cycle in the oscillation history of a section of a wing carrying an aileron the motion of the section corresponds to wing bending To begin with, let us imagine the aileron to be locked Then it is quite evident that at all points throughout the cycle the wing will be at an angle of incidence which produces a force opposing the motion This motion, in one degree of freedom, is therefore damped the wing is being required to do work against the air forces all the time

Now suppose the aileron is allowed to move, but only either in phase or 180° out of phase with the wing We now have to superpose the forces due to aileron movement on the forces due to wing movement But since in any half-cycle the displacement rises from zero to a maximum and back to zero, the net work done in any half-cycle vanishes Since the wing has still to do work against the forces induced by its own motion this system is also stable In fact, with aileron movement in phase or 180° out of phase, the system has in effect only one degree of freedom if the wing displacement is specified, so is that of the aileron, without ambiguity

Let us now examine the situation if a phase angle intermediate between 0° and 180° is permitted The next figure, in the top diagram, shows a 90° phase angle In this case it is possible for the direction of the force due to aileron to be uniformly in the direction of motion of the wing, that is, throughout the complete cycle this component of air force is doing work on the wing, and if this amount of work exceeds that done against the wing damping forces, instability—that is, flutter—will result

https://doi.org/10.1017/S2753447200000688 Published online by Cambridge University Press

We may deduce that, in order that flutter may occur, we require at least two degrees of freedom and that these must not move in phase (though in fact they need not be exactly in quadrature either) The last diagram illustrates the constituent motions in flexure-torsion flutter, and shows how the net incidence of the wing is always favourable to the extraction of energy from the air

We must next consider what factors tend to promote or suppress conditions favourable to flutter , these factors must, for the most part†, produce their changes by effecting alterations in the aerodynamic, elastic, or inertia forces But there is virtually nothing that can be done to alter the aerodynamic forces , for one thing, the aerodynamic characteristics of different aerofoils, from the flutter viewpoint, are all very much the same for another, the aerodynamic characteristics are in any case

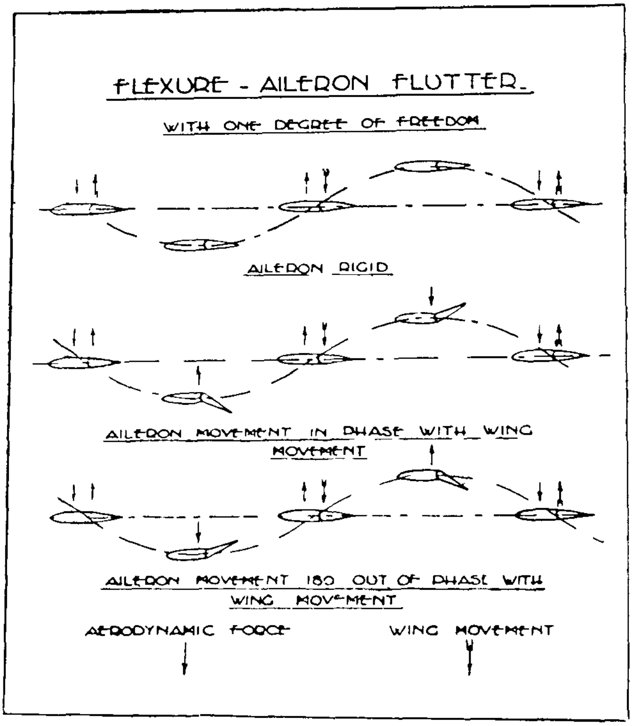


Fig 11
Flexure-aileron flutter

dictated by the requirements of performance, control and stability for the aircraft Thus the aerodynamic approach is not open to us

We next consider the elastic approach , and here it is evident at once that high elastic stiffness, in an overall sense, is beneficial, in that it tends to delay the onset of flutter to high airspeeds For at low airspeeds the air forces are very slight in comparison with the elastic forces, and the system approximates, in consequence, to an undamped inertia-elastic system As is well known, the constituent motions of such a system are all in phase , flutter will therefore not occur If, therefore, we increase all the elastic forces so that they dominate the air forces at higher speeds, the system will still be stable However, it is necessary to enter a *caveat* here while it is true that if *all* the elastic stiffnesses are increased in proportion, the critical dynamic pressure is increased in the same proportion, it is not true that increase in one stiffness alone is necessarily beneficial An increase in one stiffness may simply tend to curb a constituent motion which contributes damping, and so tend to reduce the overall damping For example, an increase in the bending stiffness of a wing without a corresponding increase in torsional stiffness usually reduces the critical flutter speed

† We are here excluding such devices as artificial damping which have never proved efficacious

https://doi.org/10.1017/S2753447200000688 Published online by Cambridge University Press

Lastly, let us consider the approach *via* changes in the inertia characteristics Clearly, major increases or decreases in mass, aimed at changing the inertia characteristics as a whole, are out of the question* , we can attempt no more than local adjustments Fortunately, however, these local changes offer a powerful method of affecting the flutter characteristics for they can effect profound changes in the phase relationships between constituent motions

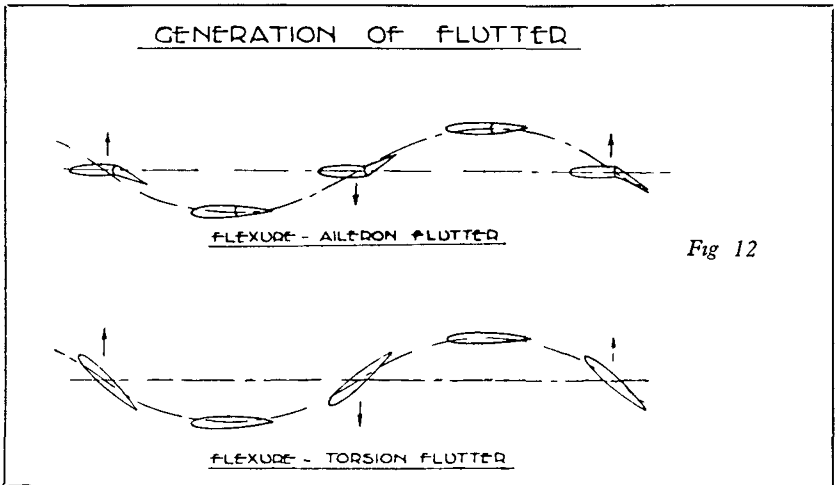


Fig 12

Physical reasoning gives a good clue as to how this occurs If we think of a wing carrying an aileron (Figure 13), it is evident that when an upward (bending) acceleration is imposed on the wing, the aileron will tend to rotate in the nose-down or tail-down sense according as its centre of gravity is forward or aft of the hinge line If we consider the latter case (aileron tail-heavy) we see also that, reciprocally, if the aileron is given an angular acceleration, in the nose-up sense, about its hinge-line by the action of a pure couple, this will impose an upward force on the wing through the hinges Thus we have the required coupling acceleration of the wing gives a hinge moment, and angular acceleration of the aileron gives a wing force Moreover, since upward acceleration of the wing gives a tail-down aileron rotation, the air force generated by the aileron movement is in the direction of movement of the wing Thus everything favours the possibility of flutter

If we now consider the nose-heavy aileron, we see that while we still have coupling inertia forces, the air force generated by the aileron motion is in the opposite sense to the wing motion , hence the system will be stable

The process of adding balancing masses to the structure of a control surface to reduce to zero the tendency to rotate under accelerations applied *via* the hinges—mass balancing, in fact—is so well-known that I need not dwell on it in general There is, however, one aspect which is less well-known In the early days of mass-balance it was often convenient to place the balancing mass on an external arm, usually underslung In this way the centre of gravity was brought, not to the hinge-line, but vertically below it For vertical bending of the wing this is adequate‡ , but if chordwise acceleration is experienced then there is an inertia coupling which may cause trouble The classical instance of this is the Puss Moth aeroplane The wing bracing struts, in the form of a V, were so arranged that downward bending of the wings was accompanied by a forward (chordwise) motion, so that the mass-balance of the ailerons was rendered inadequate There have subsequently been several occurrences of a similar nature even one of our most modern fighters suffered from an allied phenomenon

* They would in any case produce no major change in the flutter characteristics

‡ In the neutral position If the control surface is put up the centre of gravity moves aft and flutter may occur at least one occurrence of this kind is known

What is true of an aileron is true of a wing, also and underslung masses need careful watching from the chordwise flutter viewpoint In the case of a wing the axis which replaces the hinge of a control surface is the flexural axis, it is simplest to use this as a datum since elastic coupling, which in general is present, is then (by definition) avoided To avoid wing flexure-torsion flutter, we need adequate torsional stiffness and a mass distribution which locates the chordwise centre of gravity as near to the flexural axis as possible Even this leaves aerodynamic coupling, and the optimum arrangement is for the inertia, elastic, and aerodynamic ("quarter-chord") axes to be coincident However, apart from some early gliders, we have (so far as is known) managed to avoid flexure-torsion flutter for many years

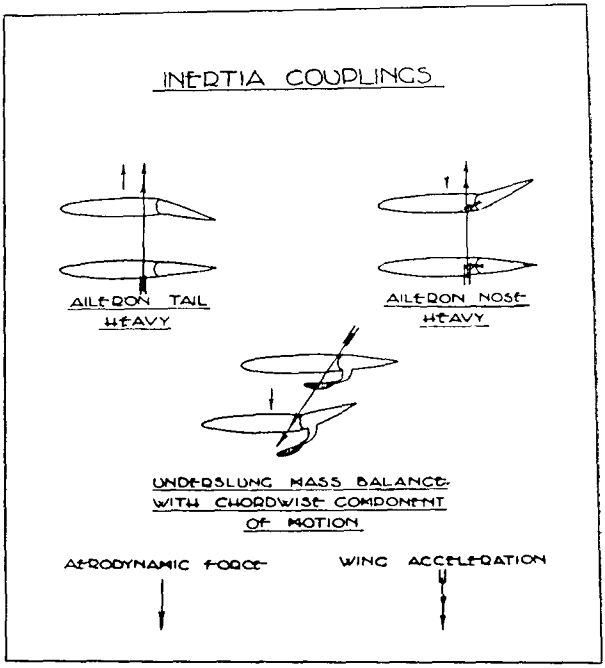


Fig 13
Inertia couplings

I have elaborated the chordwise motion story a little because of possible application to rotary wing systems, for the same reason I should like to refer to propeller flutter There have been a good many occurrences of propeller flutter, and it has never been easy to elucidate what was happening Developments on the R A E spinning tower have permitted study under static (zero rate of advance) conditions, but even so the picture is very incomplete Many occurrences are at stalling incidences and involve blade torsion only, in others coupled bending and torsion have been observed However, there has been a fair body of theoretical investigation, and one major research on flutter involving inertia couplings showed that, with the usual aerofoil sections at least, chordwise bending could be strongly destabilizing It appears that there may be two reasons for this first, the inertia and flexural axes are at different distances from the chord line, so that the blade is not mass-balanced, secondly, the frequencies of chordwise bending and torsion can be closer, in a slender blade, than those of normal bending and torsion, chordwise movement will thus generate larger torsional displacements than normal bending

So much for flutter as it is known at present Before we proceed, however, I should like to show a short film of wing flutter This film was made at the National Physical Laboratory, and demonstrates the way in which heavy masses, such as engine nacelles, can influence flutter. While there is a moral in this, namely that concentrated balance masses have only a limited effect on flutter, I show the film principally as a spectacle for those who may not have had the opportunity of witnessing flutter before. The bending and torsional motions, and the phase difference between them, should particularly be remarked.

(The meeting was then shown the film on flutter, after which the Lecturer resumed)

Aeroelastic Effects of Sweepback

In what has gone before, we have been considering the aeroelastic problems of conventional aircraft with unswept wings. It is desirable here, for a reason which will presently appear, to devote a short time to the problems introduced by sweeping the wings backwards.

In general, the effects may be summarized as follows. The divergence problem is much eased, but control reversal troubles are accentuated. Wing distortion has a serious adverse effect on dynamic longitudinal stability (manoeuvrability), while new flutter possibilities are introduced. Thus all the phenomena we have discussed are affected in one way or another by sweepback.

The explanation for this is as follows. With an unswept wing, bending displacement does not alter the local wing incidence, and only wing torsion can produce

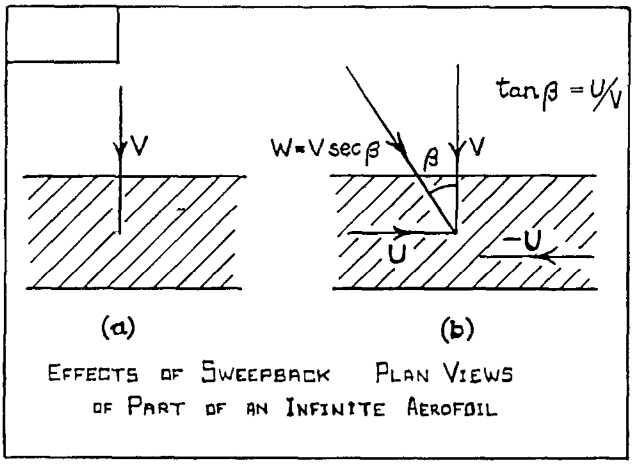


Fig 14 Sweepback plan views

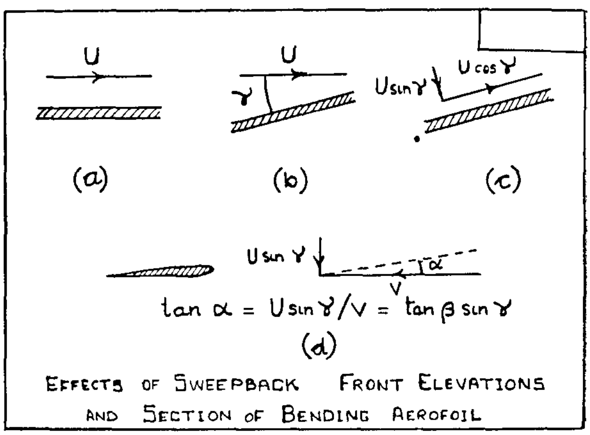


Fig 15 Sweepback Front Elevations

https://doi.org/10.1017/S2753447200000688 Published online by Cambridge University Press

redistribution of aerodynamic load With a swept wing, however, the change in wing slope due to bending has a component along the wind direction (if the angle of sweep is β, the component of slope along the wind direction is $\sin\beta$ times the slope along the wing) Figures 14 and 15 illustrate this effect Thus bending produces incidence changes, and hence redistribution of load, just as torsion does Moreover, if the wing bends up under a normal lift load (*e g*, when pulling out from a dive) the sense of the induced incidence change is such that the wing tips tend to shed their load The effects on the various phenomena follow at once the relative loss of tip load eases the tendency to divergence, application of elevon produces an upload which bends the wing and consequently reduces the upload, the shedding of tip load moves the overall aerodynamic centre forward and so tends to instability—a really serious degree of instability may easily occur, and finally, and in particular, a totally new wing flutter phenomenon can result, in which wing torsion plays no part, so that wing bending stiffness dictates the critical flutter speed, and not wing torsion This flutter phenomenon has been demonstrated in a wind tunnel at the National Physical Laboratory, with wing bending coupled to pitching and vertical translation of the aircraft as the constituent motions

So far as is known at present—and since sweepback is a relatively new notion, we have not progressed very far in our investigations in this field—the aeroelastic problems due to sweepback do not become very noticeable until the angle of sweep reaches about 20°

General Speculations on Possible Aeroelastic Effects in Rotary Wing Systems

At this point I leave the relatively firm ground of experience, theory, and experiment which supports the aeroelastic conceptions we have been discussing for conventional aircraft, and I now venture (with some trepidation) into the quicksands of speculation on possible parallel effects for rotary wing systems I am going to restrict these speculations to single lifting rotors, I do not think there is a sufficient body of experience and theory on the stability and control of multi-rotor systems in the absence of aeroelastic effects for anyone to venture on the further complication of introducing these effects and as regards flutter and divergence I think we can probably regard the rotors as independent, and so restrict ourselves to one rotor

From what has already been said, however, we may make some preliminary deductions which are not too speculative Since aeroelasticity is always associated with high speeds, it is unlikely that (for some years at least) there will be any effects on any part of a rotary wing aircraft other than the rotor itself Moreover, the helicopter as we know it has no empennage of a kind which would be subjected to strong air forces varying with speed At first sight, it might appear that a tail rotor (torque-balancing) might bend the fuselage, but since its function is to offset the power plant torque, its load will not be directly dependent on airspeed and the fuselage bending may well be sensibly constant Thus we cannot anticipate any fuselage distortion which will appreciably affect the distribution of airload on the aircraft, and we may dismiss aeroelastic effects on aircraft stability as being very unlikely

Again, since a rotary wing aircraft has no control surfaces of the kind employed by conventional aircraft, it is difficult to envisage any form of loss or reversal of control It is not entirely impossible for this to happen, for control implies a redistribution of aerodynamic force to produce the required aircraft movement When this redistribution is accomplished *via* a control surface, elastic distortion results which produces a further redistribution, and there is no *a priori* reason why the redistribution caused by tilting a rotor should not also produce some elastic deformation within the rotor However, with rotor blades as conventionally built, it is extremely difficult to envisage any deformation arising from air forces, of sufficient magnitude to create a balancing air force In any case, such a phenomenon, in a rotary wing aircraft, would presumably not be a function of forward speed, and would mean that the aircraft would be unacceptable at all forward speeds

We may conclude, therefore, that—so far as can be seen at present—it is unlikely that there can be any adverse aeroelastic effects on the stability and control of a rotary wing aircraft Moreover, this immunity appears to derive from the fact that the rotor itself provides all the stability and control, any troubles of the kind we are envisaging must therefore be looked for within the rotor

We may therefore proceed to discuss the possibilities of divergence and flutter of rotors, and, in general, we need consider only a single blade

I believe that almost all modern rotary wing aircraft have rotor blades with both drag hinges and flapping hinges, one exception is the two bladed "see-saw" type rotor (*i e*, with fixed coning angle) the stability of which has been recently discussed by Coleman and Stempin of the N A C A, and which is mentioned briefly below

If we consider the articulated blade as typical, we must note first of all that it has, in the power-on condition at least, some sweepback about the drag hinge In view of its circular path, the effective aerodynamic sweepback varies along the radius, and becomes very slight at the tips, but it is quite possible that for a large radius rotor driven by a powerful motor, and with the drag hinges not too remote from the hub, sweepback effects might be appreciable Certainly it seems that the possibility of effects from this cause should not be overlooked, as we shall see later

The next point of note is that the rotor differs from the wing or propeller in respect of its two articulations it has no elastic bending stiffness and no elastic chordwise stiffness, except for overtone modes of motion Moreover, it is not held rigidly at the root in respect of twist, the pitch changing mechanism gives it a certain freedom

With these preliminary considerations in mind, let us proceed to the discussion of divergence and flutter

Divergence of a Rotor Blade

I believe it is modern practice to design a rotor blade in such a manner that the flexural axis lies at the quarter-chord point all the way along the blade This means that the aerodynamic forces due, for example, to a gust would produce no tendency to twist the blade if it were rigidly held at the root, so that its divergence speed would be infinite On the whole, to place the flexural axis at the quarter-chord is probably a very good thing, but in the power-on case at least it may not really be necessary For there exists then a small degree of sweepback—probably only of the order of 10° at the drag hinge, and while the aerodynamic sweep varies, the geometrical sweep is constant It follows that a disturbing load applied near the blade tip is well behind the line of the pitch axis inboard of the drag hinge, and will produce rotation about this axis in the sense which reduces incidence In brief, the effective flexural axis lies well ahead of the blade section, so that the position of the local flexural centre is immaterial

Not only is this rotation significant there is an additional effect from the aerodynamic sweep, such as it is Whether the added load produces bending or flapping, the induced blade slope has a component along the local wind direction which again tends to suppress divergence We may conclude that, in the power-on case, divergence is avoided by the designed position of the flexural axis, and assurance is made doubly sure by the further effects of pitch rotation, bending, and flapping And here we may therefore speculate since there is in fact some advantage to be gained on the flutter side by moving the sectional position of the elastic axis back, is there a case for reconsideration of the design of rotor blades in this respect? It may well be that it would be necessary to add a stop in the drag hinge mechanism to ensure that a slight sweepback persisted in the auto-rotation condition, but is that a serious objection?

While we have successfully disposed of divergence, there remains the allied phenomenon of blade twist due to camber Rotor blades are designed with symmetrical sections not, I imagine, to avoid aeroelastic effects, but to avoid the torsional load which would have to be carried, through the pitch change mechanism, by the pilot But in fact, even with careful manufacture, it is not possible to achieve zero C_{mo}, and it is the practice to provide small fixed tabs which are adjusted to give an overall zero C_{mo}, so that no load is transmitted to the pilot However, I believe that such an adjustment is not always successful in catering for all conditions of rotor speed and forward speed, indeed, it would be surprising if it were For if we have, say, a negative C_{mo} built into a blade along its length, and if adjustment of the tab gives a positive C_{mo} over the tab span, balance will only be achieved so long as the distribution of velocity along the blade does not vary In forward flight at various speeds this cannot hold However, I am not so concerned with the load transmitted to the pilot as with the torsional actions suffered by such a blade With each revolution, in forward flight at least, it is subjected to a cycle of twisting actions, maximum in the advancing condition and least in the retreating condition The magnitude of the twist is probably slight, even though a long and slender blade can twist appreciably under small loads But the accumulated effect of such twisting actions must be bad for the blade structure and is likely to produce loose rivets and cracked skin

Fortunately, the remedy is fairly clear continuous tabs should be provided (or at least two or three tabs along the span) But how the adjustment is to be made so that zero local C_{mo} rather than zero overall C_{mo} is obtained, I would prefer to leave to the ingenuity of others

Flutter of a Rotor Blade

Our discussion so far leads to the conclusion that if any serious aeroelastic effect is to be found in rotor blades, it will probably be some form of flutter The flutter possibilities are, however, very considerable

For one thing, we have, again in forward flight, a regular forcing mechanism arising from blade rotation , and it is known that such forcing can promote and sustain flutter The Tempest aircraft was a good illustration of this the precautions that were necessary to prevent incipient elevator-tab flutter on the Sabre and Centaurus versions were quite different In one case, a quite normal amount of backlash was permissible , in the other, regular daily inspection of the tabs was necessary in order to ensure that the backlash was virtually non-existent

Again, the number of possible modes of motion entering into flutter is legion compared with the simple flexure-torsion of the average wing We discuss some of the possibilities under the following sub-headings

Flexure-torsion flutter Since the blade is not rigidly held at the root, its effective torsional stiffness must be less than the actual blade stiffness, and indeed must depend acutely on the control circuit stiffness Vibrations will occur with a node in the circuit and the stick 180° out of phase It is always difficult to make really stiff circuits , so that we may expect flutter, if it occurs, to appear at relatively low tip speeds compared with the speed which would obtain if the blade were *encastre*

But even though the effective stiffness is low, how are we to explain the occurrence of flutter on a blade for which the inertia and flexural axes coincide with the quarter-chord line—an arrangement which would undoubtedly secure complete immunity on a wing ? We may offer several tentative answers In a wing the modes in torsion and flexure would approximate to the fundamental modes of vibration The rotor modes are quite different in form, owing to the remoteness of the node for the torsional motion and to the flexural articulation Moreover, the relative frequencies are quite different from the wing case, the flapping frequency of the rotor being determined largely by centrifugal stiffness It is therefore more than likely that overtone motion in flexure may have to be considered

We may remark that some actual occurrences of flutter were not found to be predictable by simple wing flutter theory , and again that the addition of local masses at the tip to alter the flutter characteristics did not produce the expected results In view of the features demonstrated by the flutter film, which showed how localized masses induced flutter in new modes, this is not altogether surprising

But perhaps the most likely solution of this flutter question is to be found in the slight sweepback of the blades in the powered condition (I do not know if blade flutter has been experienced in the auto-rotation case) We have a double aerodynamic coupling—bending or flapping inducing incidence change, and conversely—while the inertia characteristics are certainly no longer free from coupling , or alternatively (depending on the choice of co-ordinates) there is coupling between the motions through the centrifugal stiffness Moreover, the effective moment of inertia in torsion of the blade, about the pitch-change axis, may be of a different order of magnitude from the actual moment of inertia

This is all speculative in the extreme I have made no attempt at any formal analysis, even with simple assumptions But let us proceed to examine another possible type of flutter

Torsion-edgewise flutter We have indicated that in the case of propeller flutter, edgewise motion could be strongly destabilizing In a rotor, the drag hinge permits relatively unrestricted edgewise movement, so we may guess that it is quite likely that such motion enters into flutter Indeed, since rotation about the drag hinge occurs cyclically in any event during forward flight—I understand the amplitude is of the order of one degree—it must be a constituent factor

It remains to consider how such motion is coupled to, say, torsional motion It is obvious that aerodynamic coupling exists incidence changes are always accompanied by drag changes We may, however, envisage possible inertia coupling also Under the average lift load on the blade, it will bend only very slightly perhaps, but nevertheless perceptibly If we draw an average straight line along the blade, some sections will be in effect underslung with respect to this line, and others overslung Though the amounts are small they are probably comparable with the separation of the inertia and flexural axes in the case of a propeller blade section Blade rotation and twist, therefore, may well induce inertia forces tending to produce fore-and-aft movement I do not pretend to have seen other than dimly the possibilities here probably some simple research on the vibration characteristics of slightly bent blades would be very helpful

I believe motion about the drag hinges is often constrained by the fitting of powerful hydraulic (or other) dampers such devices would undoubtedly help to suppress flutter of the kind envisaged, unless a mode of motion were possible in which there was a node at the point of attachment of the damper

Weaving of a see-saw rotor I believe that a considerable research into this question has been conducted in the U S A , though I have not had a full report on it In the case of a see-saw rotor with a fixed built-in coning angle, it is obvious that a pitch change is inevitably accompanied by fore-and-aft movement of some, at least, of the blade sections Whether this is a major factor in producing instability I can only guess but I understand that it was concluded that instability (called in this case " weaving," though, since it depends on the circuit stiffness, it is a form of flutter) can occur even for mass over-balanced blades, and that increase in the coning angle produces more pronounced instability The unusual inertia characteristics for this type of system also have a marked effect on the instability

Stalling flutter Beyond the stall, the slope of the curve of life coefficient against incidence is negative—often markedly so It follows that the damping due to flapping is negative , similarly the pitching damping is usually negative We may thus find oscillations in one degree of freedom generated—though, unlike flutter, their amplitude is strictly limited Such oscillations have frequently been recorded on propeller blades, and may produce quite severe stresses

In certain conditions of forward flight, appreciable areas of the rotor disc may be marked as " stalled " the blade sections passing through them are beyond stalling incidence It is therefore possible that, in this condition, they may contribute to flutter But, on the whole, I rather doubt if this effect is serious Unlike the stalled propeller blade, only limited regions are stalled part of the time, as against the whole propeller disc† all the time Moreover, in the rotor case the incidence may proceed so far beyond the stall (reversed flow may occur) that the negative slope of the lift curve may again change sign, or give place to an effectively zero slope The point may need watching, but I suspect it will not be necessary to introduce it quantitatively in flutter analysis

Concluding Remarks

I think I have said quite enough—perhaps too much—on the various flutter possibilities, without introducing further complications But two general matters deserve mention

First, everything that has been discussed must be to some extent affected by compressibility effects tip Mach numbers, though not high, are sufficient to produce noticeable variations in aerodynamic force But while this is a complication in the analysis, I think that any curative measures for flutter, which analysis indicates for the incompressible case, will be effective in the compressible case also

The other important point concerns virtual inertia Experience has taught us that it is necessary to include an allowance for virtual inertia in flutter calculations involving control surfaces—even for a metal covered aileron the virtual inertia may contribute 20 per cent to the structural product of inertia It follows that allowance should be made for this effect in rotor flutter calculations (as was done in the American

† Or most of it, at least

research referred to earlier) I suspect that the virtual inertia always moves the effective inertia axis backwards

My previous remarks indicate clearly that, if one gives one's imagination full rein, one can produce in a short time a surprisingly large number of quite complicated phenomena which are at least not improbable But to base an analysis on these suppositions would be an immensely longer and more laborious business How then are we to tackle this problem ? The answer is, I think, through the medium of controlled experiment, such as is possible on the rotor tower at Bristol It is not so difficult to devise tests which will confirm or deny the existence of certain of the modes of motion which we have been suggesting , and while experimenting in the dark may well miss certain aspects, deliberate searching usually produces most useful results Most of the experimenter's ideas have to be discarded , one or two may be confirmed , but—most important—the work opens up new avenues of thought and indicates possibilities which the earlier speculations have overlooked I think it is only in this way—by the acid test of controlled experiment—that the study of aeroelastic effects in rotary wing systems will be put on a rational basis

Here I bring my speculations to a close In concluding, I can only say that I fully expect some of my more speculative offerings to be shot at, and indeed shot down I can only hope that my example will induce a speculative mood in the discussion to follow, so that I may have at least an opportunity of a shot or so in my turn

Discussion

J S Shapiro, Dipl , Ing , A F R Ae S *(Founder Member)*

We may derive great encouragement from the first section of Professor COLLAR's lecture, showing that close agreement has been achieved between aeroelastic theory and practice in fixed wing aircraft This gives us some indication that we may expect advances in the treatment of flutter before we get any accidents, and we should not expect flutter, or any other aeroelastic phenomenon, to be too dangerous or troublesome a feature of rotary wing development

I do think we ought to make it quite clear that very few flutter troubles have actually been observed in rotary wing systems In fact, I should say, speaking in very general terms, that I always suspected that nine-tenths of all vibration troubles in rotary wing systems are due to resonant response to periodic impulses, which in themselves are small Since I had the opportunity of gaining further experience on fairly complicated systems, I have come to the conclusion that 99% of all such troubles are due to resonant response That, I think, is the impression gained by most designers

Dealing with individual topics, the lecturer states that we need consider only a single blade in the case of divergence and flutter It would seem to me that one could do so easily if there were only the collective interconnection between the blades, but the cyclic interconnection would probably require the consideration of all three blades at once I agree that modes of motion can be found which can be represented as imaginary single blades but I think there will be two, and not one

Leaving aside the question of aerodynamic sweepback, there are two reasons why the geometric sweep back is mostly absent, in spite of appearances to the contrary

In the first place, geometric sweepback expresses mainly the angle between the longitudinal axis of the wing and the main axis of bending In the helicopter blade the main axis of bending is the flapping pin and this is usually at right angles to the blade axis when the blade is lagging back to take a predetermined average torque There is, therefore, more likely to be a sweep forward in autorotation than a sweep back in the power driven condition

Second, part of the torsional elasticity of a blade is due to the elasticity of the root constraint, this part, of course, is unaffected by the ' portion ' of the blade axis I wonder therefore, if the apparent geometric sweep back of helicopter blades is, in fact, significant

The last observation brings us to another point, the position of the node in torsional deformation Perhaps one should not assume generally that the node is always in the control systems and ought to visualise the possibility of a node along the blade This could happen with a very light control system in terms of inertia about the blade torsion axis

Coming now to divergence, this is determined in a fixed wing by the distance between the aerodynamic and flexural axes but in the fundamental, rigid mode of blade ' bending ' the inertia axis takes the place of the flexural axis, because that is where the centrifugal force which takes the place of the elastic force is located Usually, the aerodynamic and inertia axes are very close in rotating wings and the torsional moment due to change in incidence is very small In the second flexural mode where the flexural axis of the blade is involved a certain distribution of blade masses can produce resultant shear forces opposite to the aerodynamic forces and divergence can be suppressed On the other hand the flexural axis of the blade structure can be forward of the aerodynamic axis and divergence may not arise for that reason alone Both cases are entirely practical and when acting together will again give rise to divergence

As regards twist due to camber, we have used cambered section for years, for very good reasons The effective twist is not very serious, even in wooden blades From the point of view of carrying the torsional load a slight movement of the inertia axis backwards is desirable Whether that is advisable or not is another matter, but compensation of aerodynamic moments due to camber can be done that way There are other means available to achieve light stick forces with cambered sections, such as springs To equalize the pitching moments we use trimming tabs but on a tab of a length of about one-sixth of the blade, the adjustment to balance the non-uniformity between blades is about a quarter of a degree On full length tabs it would be somewhere around one-sixteenth The adjustment would be quite impossibly small

Perhaps the lecturer has dismissed stalling flutter too lightly It does not require a stall, at least not always, and in some ranges of variables does not require a stall at all I am sure Professor Collar will agree that all we require is a negative damping coefficient which at very low frequency parameters is highly probable According to some estimates negative damping is expected in this range

I would like to hear the lecturer's opinion on the question of flexural axis position In simple torsion flexure flutter, is it actually necessary to have the flexural axis anywhere in particular if the inertia and aerodynamic axes strictly coincide?

Finally, in special cases we have encountered vibrations which should really be classified as flutter because they are produced by instability arising out of the coupling of two normally stable modes of displacement

The coupling between the rigid and the first elastic mode of an articulated blade can produce flutter in the presence of a strongly stable interconnection between flapping angle and pitch It is an interesting example where ' stabilizing ' one mode promotes instability of the coupled motion and has been demonstrated in model form

Perhaps it is a good general rule for designers that whenever automatic features are introduced, such as an automatic reduction of pitch with increase in flapping, we have to look out for some form of flutter

H B Squire, M A (Oxon) (*Member*)

It seems to me that the most hopeful line of approach to this study is to assume that all elastic stiffnesses present in a rotor are vanishingly small and to determine what measures should then be taken to prevent the occurrence of any aero-elastic instability One such measure is to arrange for the flexural axis and the inertia axis of the blade section to be located at the quarter-chord point, as is usually done This approach leads to a valuable simplification and the effect of the small but finite elastic stiffnesses can then be treated as a correction, if necessary I should not, at the same time, assume that the stiffnesses associated with centrifugal force are also small, without additional justification

W Tye, O B E, B Sc, F R Ae S (*Member*)

Towards the end of the lecture Professor Collar suggests that the most fruitful method of investigation of the many aeroelastic problems lies in the medium of controlled experiment I would not dispute this conclusion, but I believe we must also consider the possibility of short-term work I have in mind that there are several helicopters of new types close to introduction into commercial use We shall have to be tolerably certain of the safety of these helicopters Supposing that each new type is test flown in various combinations of forward speed, rotor speed, and rotor pitch to cover all practicable combinations which may later arise in service, will such test flying be proof that aeroelastic troubles will not later develop, or could the helicopter rotor system be on the edge of an aeroelastic precipice? If this were so, is it possible that apparently small differences between the helicopter in service and the one tested would produce catastrophic results?

Each of the contributory characteristics—mass distribution, structural stiffness, back-lash at joints and gearing, and aerodynamic shape—are bound to vary slightly from standard, and I wonder whether the probable variation of any of these characteristics could be critical. If there is real cause for worry in these directions, would it be practicable on test flying to vary, by artificial means, the important characteristics to establish what is the worst that can happen in service.

May I conclude by thanking the lecturer for presenting a difficult subject in delightfully clear terms. His approach to the matter is one which is usually (and erroneously) described as the "common-sense" outlook. In point of fact, the ability to make this approach is a rare virtue and should be accorded the honour of being called the "uncommon-sense" outlook.

R A Frazer, D Sc, F R S, F R Ae S (*Dept of Scientific and Industrial Research, National Physical Laboratory*) (*Contributed*)

I feel that very few people could discourse intelligently about rotary wing systems, and at the same time profess to know very little about them. But Professor COLLAR is exceptional. He has a flair (which I greatly envy) for exposing the skeleton of a problem. He looks at it, and decides how the body will behave. And this leads me to add that if Professor COLLAR knows very little about rotary wing systems, I certainly know very much less.

There is one point which seems to require a little expansion. Professor COLLAR remarks that flutter requires more than one degree of freedom for its promotion, unless the component concerned is stalled, and he adds that motion in a single freedom is almost invariably damped. The point here is that when two or more freedoms participate, flutter may occur even though oscillations in each freedom separately (with the others rendered inoperative) would be positively damped. With stalled wings, or aerodynamically inefficient sections, it is of course sometimes possible for single freedom oscillations to occur. Some suspension bridges, for example, when exposed to wind, provide striking examples of oscillations which are in effect purely torsional. Bridges stiffened by plate girders can also show flexural oscillations (or galloping).

And this leads me to the vexed question of whether oscillations involving one degree of freedom only should be classed as "flutter"? Some people reserve the term "flutter" for unstable *coupled* oscillations, but this veto on a single freedom seems to me to be illogical. In practice an unstable single freedom oscillation is bound to induce some movements in other freedoms, and it is then "flutter" according to any definition. But if the main instability is sufficiently active, the induced movements will be quite insignificant. What virtually remains, then, is flutter in a single freedom.

Another debatable question is whether the term "flutter" should be restricted to oscillations which are unstable in the classical sense—that is to say which increase when a system is disturbed from equilibrium. Such oscillations often appear "explosively" and cause failure, particularly when friction is present. But oscillations also sometimes occur which become choked to a steady amplitude by structural damping and other non-linearities which are not fully understood. Examples are provided by stalled wings and bridge sections, which under some conditions can generate alternations of large eddies and so—strictly speaking—possess no equilibrium position at all. The behaviour of the section here is connected with the frequency and phasing of the eddies, and is not strictly attributable to classical instability. Perhaps Professor COLLAR will decide for me whether such oscillations shall be called "flutter"?

In conclusion I would like to offer my congratulations to him for a paper which, I am sure, will prove valuable not only to designers of rotary wing aircraft but also to all who are interested in aero-elasticity.

Raoul Hafner (*Member*)

Aero-elasticity in rotary wing aircraft is quite a new subject and the lecturer therefore—very appropriately—has used the word "Speculation" in the title of his paper. Before we may begin to speculate on a new subject we must make a survey on the relevant ground already explored and I would congratulate Professor COLLAR on the masterly fashion in which he has done this in the first part of his paper

Here I would beg to comment on a matter of definition of aeroelasticity In its most narrow sense it comprises only phenomena involving structural elasticity I would consider it profitable, however, if the definition included the centrifugal force acting, as it does in a rotor, as a quasi-elastic restoring force, not necessarily in conjunction with structural elasticity

Coming now to the speculative part of Professor COLLAR's paper, I would at first comment on the reference to multiple trimming tabs on rotor blades I fully agree with him on the need for such tabs We have been experimenting for some time with twin tabs and have learnt to appreciate their value, especially if a direct and reversible form of rotor control is employed, where already small out-of-balance forces in the rotor can be felt at the controls The full benefits of twin tabs, however, are only obtained, if the blade is correctly balanced, which, with two tabs, involves a more complex technique and special equipment

In order to prevent inertia coupling between edge-wise movement and torsion of the blade, bending in the flapping plane must be avoided It is therefore necessary, to balance aerodynamic and inertia loads all along the blade by suitable radial mass grading

The rotating wing may produce quite novel aero-elastic phenomena In this respect I have been speculating myself a little and would quote as an example the following —

During a control action, as well as in translational flight, the rotor blade is subject to cyclic changes of incidence with respect to the rotor orbit The variation is substantially a harmonic one, the maximum incidence being diametrically opposite the locus of minimum incidence This blade feathering is controlled from the root end of the blade and if the blade structure is sufficiently rigid, the aerodynamically effective tip portion of the blade will precisely and instantaneously repeat this movement If, however, the blade is not rigid, but there is an elastic link interposed between the root and the tip portion of the blade, then the movement of the latter portion can be expressed by the equation

$$I\alpha + A\alpha + C(\alpha - \alpha_r) = 0$$

Where α = angular displacement of tip portion
I = Polar moment of inertia of tip portion
A = Aerodynamic damping in pitch of tip portion
C = Stiffness of the elastic link between root and tip portion
α_r = Displacement of blade root due to feathering control

If we assume the feathering movement to be $\alpha_r = \alpha_0 \sin \omega t$ where α_0 is the amplitude of control movement and ω the frequency of rotation, then the above equation can be written

$$\left(D^2 + \frac{A}{I}D + \frac{C}{I}\right)\alpha = \frac{C}{I}\alpha_0 \sin \omega t$$

This is, of course, the equation for the forced oscillation of a body restrained by elastic and damping forces and if $\frac{A}{I} = \zeta\omega$ and $\frac{C}{I} = \nu\omega^2$ then the natural frequency in the fundamental torsional mode of the blade (with the node at the blade root) will be $\sqrt{\frac{C}{I}}$ or $\omega\sqrt{\nu}$

The particular integral of the above equation will express the steady feathering movement of the tip portion of the blade

This integral is $\alpha = \alpha_0 \nu \dfrac{(\nu - 1)\sin \omega t - \zeta \cos \omega t}{(\nu - 1)^2 + \zeta^2}$

In present blade design $\sqrt{\frac{C}{I}}$ in flight is of the order of 3 5Ω (depending to some extent on flexural deformation, especially in the flapping plane), so that ν is about 12 I cannot give a figure for aerodynamic damping of this movement but I know it is well below the critical damping and small enough to permit neglecting ζ^2 in the above equation , and with the value assumed for ν we obtain thus

$$\alpha = \alpha_0 (1\ 09 \sin \omega t - 1\ \zeta \cos \omega t)$$

Comparing this expression with that applying to the blade root, *i e* ,

$$\alpha_r = \alpha_0 \sin \omega t$$

it will be noted, there is an increase in amplitude by about 9* together with a phase displacement depending on ζ

If ν becomes unity, owing to insufficient torsional stiffness or excessive rotor speed, α becomes very large and the phase displacement reaches 90°, *i e* , there will be a complete loss of control as a result of resonance

In terms of blade flapping in translational flight the above equation means, an elastic blade, compared with a rigid one, will show a reduction in flapping amplitude together with a displacement in azimuth—an aero-elastic effect peculiar to the rotating wing

THE AUTHOR'S REPLY TO THE DISCUSSION

In reply to MR J S SHAPIRO While the close agreement between aeroelastic theory and practice is in many ways very encouraging, it is also true that often a flutter phenomenon has come first and its theoretical explanation has followed It is always difficult to predict what modes of motion will enter into flutter, and the labour of trying every possibility (even if they could all be envisaged) would be prohibitive Thus, while we have been able to formulate design rules which have ensured that flutter is a fairly rare occurrence, we can never be certain that in some new system there are no new possibilities of flutter

However, I would agree that, so far at least, troubles due to vibration have heavily outnumbered those due to flutter in rotary wing systems The same is probably true of conventional aircraft, but there the vibration troubles have been less pronounced and so have not outweighed as well as outnumbered the flutter problems Vibration is clearly much more important in rotary wing systems than in conventional aircraft , but that does not mean that flutter can be ignored In view of its potentially destructive nature, it must always be regarded seriously

When I said that flutter could be studied by considering a single blade, I had in mind that, in all probability, the flutter motion of any blade, which may be superposed on any steady or constrained motion, will repeat that of its predecessor with the appropriate phase lag, and that, in this sense, any one blade can be regarded as typical Appropriate distribution of the actions in the hub mechanism would, of course, be necessary

I do not think I have completely followed Mr Shapiro's arguments on sweepback However, taking the hovering condition for simplicity, if one sketches a helicopter blade, rotated backward about its drag hinge, then whether the flapping hinge is inboard or outboard of the drag hinge, it is clear that a line drawn on the blade at the tip in the direction of motion (at right angles to the radius joining the tip to the hub) will suffer a change of incidence when flapping displacement occurs I had not observed that the sense of the incidence change is different in the two cases , but in either case the rotation about the drag hinges produces a change in aerodynamic load with flapping displacement

Regarding the position of the node in a torsional oscillation of the system, Mr Shapiro is, of course, quite right that one should not always assume that its location is in the control system , though I had always understood that the gearing in control systems is such that their effective inertia is high However, the point, of course, is that with rotary wing systems the node may be in positions which would be unusual for conventional aircraft, so that the flutter characteristics may be expected to differ

My suggestion of multiple or full-length tabs was aimed not so much at equalizing the overall pitching moments between blades as at providing means for achieving the correct distribution of pitching moment along the blades

On the question of flutter in a single degree of freedom, it has been pointed out by MR MINHINNICK that it is not sufficient to have only a low frequency parameter , there is another and more stringent condition to be satisfied also

Turning to Mr Shapiro's question on the location of the flexural axis, it is true that, in conventional aircraft, movement of the flexural axis has only about one-fifth of the significance of a similar movement of the inertia axis, and that if the inertia axis coincides with the aerodynamic axis the flexural axis may be anywhere within practical limits (say 0 15 to 0 45 chord from the leading edge) without flutter appearing Accordingly, I should not regard the position of the flexural axis as very important in conventional aircraft indeed, in the recent elaboration of the torsional stiffness criterion for wings (in which I had a hand) we decided to ignore the influence of this parameter But it does not follow that the same is necessarily true of unconventional wings (*e g* , cranked wings with swept-back outer sections) or rotary wing aircraft, when the effective flexural axis position—at least with certain designs of the articulations—may be outside the section entirely There is, however, a more fundamental difficulty in talking of the position of the flexural axis For a straight unswept wing the flexural axis has been a useful conception , but when we come to swept and particularly cranked wings, the expression begins to lose its meaning A load applied at the local flexural centre will produce no local twist, but an overall rotation due to twist elsewhere may easily result Thus we require to use other parameters than the simple deflections in flexure and torsion , and the couplings then become much more involved For example, we may eliminate both elastic and inertia couplings simultaneously by choosing normal co-ordinates that is, the modes of vibration in vacuo , but then the aerodynamic couplings become more powerful As one answer to Mr Shapiro's specific query as to whether it is permissible to ignore the position of the flexural axis, I might cite the classical case of the spring tab Here, if the ordinary conceptions of inertia and aerodynamic couplings are employed, and the system is mass-balanced in the light of these conceptions, violent flutter may occur (and did occur in several cases) because there exists an elastic coupling It was only when this was recognized that the solution of the problem of spring tab flutter was in sight

Mr Shapiro's last point is borne out by experience on conventional aircraft It has long been realized that measures designed to improve the overall handling and stability of an aircraft may have unhappy effects on the flutter characteristics, and conversely A simple illustration is provided by the question of mass-balance of elevators for obvious reasons a tail-heavy elevator helps to avoid the pheneomen known as " tightening up the pull-out "—*i e* , dynamic instability , but to load the trailing edge of the elevator to give a good manoeuvre margin would clearly invite elevator flutter, and conversely, mass-balancing the elevator to avoid flutter reduces the manoeuvre margin

In reply to MR H B SQUIRE At first sight Mr Squire's proposal is very promising, and it might, in fact, serve for some phenomena Certainly it would lead to great simplification in the analysis , for with centrifugal forces and air forces both proportional to the square of the rotational speed, the stability of the system would become independent of speed But, on second thoughts, I am doubtful whether the introduction of small but finite elastic stiffnesses could be regarded as a correction Particularly in torsion, modes of motion could be introduced which were previously absent Moreover, the system envisaged could not always represent the basic conditions For example, incidence changes imposed at the root would have no effect along the blade in the absence of torsional stiffness , again, the control changes envisaged by Mr HAFNER would not be revealed by an analysis based on Mr Squire's proposal I think it must be concluded, therefore, that the elastic stiffnesses are an essential feature, and cannot be ignored even in a preliminary survey

In reply to MR W TYE It is true that I believe controlled experiment to be the most profitable avenue of exploration in the aeroelastic field , but I did not intend to suggest that ground experiments were the only possible ones to make The proof of any pudding is in the eating and flight experiments, provided adequate safeguards are employed, are doubly profitable, for obvious reasons Any of the quasi-static phenomena would be shown up by flight tests, since they are all progressive in nature (divergence, in practice, would always be reached by an asymptotic increase in twist) The one trouble which might provide Mr Tye's "aeroelastic precipice" is flutter, which can be explosive in character However, when all mass-balancing and other precautions have been taken, and flutter occurs despite them, it usually happens in practice that it is a mild form of flutter—enough to produce a very unpleasant shaking, but not enough to cause structural failure Thus I would not expect any catastrophic occurrences from the small differences which must exist between the aircraft which has been tested and the production model Nevertheless, it seems to me to be wise to carry out preliminary flutter tests of any new rotor system on the rotor tower or on the ground , then if the flight tests are undertaken carefully and with the data from the ground tests in mind, there should be no great hazard

I certainly think, of course, that a flight test programme should envisage making variations in the parameters mentioned by Mr Tye, in order to establish as far as possible the safety of the rotary wing aircraft for any possible combination of the parameters which might occur in practice

In reply to Dr R A Frazer I am grateful to Dr Frazer for his elaboration of my description of the nature of flutter in two degrees of freedom , he is, of course, quite right to bring out explicitly the fact that each degree of freedom by itself is stable, although flutter may occur if the two interact

Dr Frazer also draws attention to an inconsistency on my part Under the heading, "Stalling Flutter," I said "we may thus find oscillations in one degree of freedom generated—though, unlike flutter, " The implication is that, despite the title of the paragraph, "flutter" should be restricted to coupled oscillations in a multiplicity of freedoms This was, I think, due simply to the main emphasis of my remarks I agree with Dr Frazer that any unstable oscillation of aerodynamic origin involving elastic deformation of the structure should be called "flutter" I said also that the oscillations constituting stalling flutter are strictly limited in amplitude , but so, in fact, are those of the coupled oscillations in many freedoms—the one by the limited incidence range for which the damping is negative, the other by the incidence range for which the force-displacement relationship is linear The latter range is usually larger, so that the oscillations can be more dangerous , but clearly there is no case for restricting the term "flutter" to the coupled oscillations

Dr Frazer's last point has unusual interest he refers to systems which possess no position of equilibrium, since they are continually shedding an alternation of eddies Perhaps a clarification of this might be obtained by consideration of conditions at vanishingly small wind speeds , however, in answer to Dr Frazer's specific query, I can see no reason why such oscillations should not also be described as "flutter"

In reply to Mr R Hafner Mr Hafner refers to the quasi-elastic nature of centrifugal forces, and suggests that phenomena dependent on centrifugal actions to supply a restoring force should also be classed as aeroelasticity I have no strong views against this , the domain of aeroelasticity is continually expanding, and might well include rigid aircraft dynamics as well as phenomena of the type envisaged by Mr Hafner Perhaps a new name for it would be desirable, however In connection with Mr Hafner's point, it may be worth recalling that gravitational stiffness has been included in flutter studies in R & M 1247, I think, there is described an investigation into rudder-fuselage flutter in which the fin and rudder behave like an inverted pendulum when the fuselage twists, so that both direct gravitational stiffness and gravitational coupling result There is an important difference in the case of centrifugal forces, however both the centrifugal stiffnesses and (approximately at least) the aerodynamic forces are proportional to the square of the rotational speed Thus, if no elastic forces are involved, one would in general expect the system to be either stable or unstable at all speeds , the critical speeds we have been discussing would not exist

I am a little disturbed to find Mr Hafner saying that, to avoid bending in the flapping plane, it is necessary to balance aerodynamic and inertia loading along the blade by suitable mass grading It was my object to look for possible aeroelastic phenomena , but I would not advocate meeting half way a trouble which, at this stage, is purely speculative !

Mr Hafner's last point is very interesting It had not occurred to me that a serious effect on control could result from dynamic movements of the rotor system Loss of control on conventional aircraft, though it has transient dynamic aspects, has always been in essence a quasi-static problem But Mr Hafner is obviously right in his deduction that torsional oscillations of rotor blades, if resonating with the rotational motion, can produce serious control troubles It would be very interesting to obtain an experimental check on his conclusion that in present-day designs, the pitch amplitude at the blade tip is nearly ten per cent greater than that at the root But I can see no reason to doubt his conclusion that as resonance is approached the blade tip moves into quadrature with the root , and since the amplitude ratio α/α_0 is then $1/\zeta$, it is evident that the blade tip will dominate the control As the resonant condition is approached, therefore, there will be, not a loss of control, but a steady increase in a misdirected control This is a new phenomenon in aero-elasticity, and I am grateful to Mr Hafner for drawing attention to it

Vote of Thanks by Mr H B Squire

One of my problems in the helicopter field is that I have the greatest difficulty in understanding those people who work all the time in it After a very long struggle I can dimly follow what they mean There seems to be modes of thought in it which are not really shared in the outside world Professor Collar comes from the " outside," and everything he says is perfectly clear—you either agree or disagree—at least you understand it For that reason alone I thank Professor Collar for his lecture, which I really understood, and I would ask you to join with me and express our appreciation in the usual way

---

THE THIRD ANNUAL DINNER
OF
THE HELICOPTER ASSOCIATION OF GREAT BRITAIN

The Association's Third Annual Dinner was held on the 24th September, 1949, at 6 Stanhope Gate, Park Lane, London, and was presided over by the President, JAMES G WEIR, C M G , C B E , F R Ae S The Guest of Honour was SIR GEORGE CRIBBETT, K B E , C M G

https://doi.org/10.1017/S2753447200000688 Published online by Cambridge University Press