Technical Comments

Comment on "Aeroelastic Stability Characteristics of an Oblique-Wing Aircraft"

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THE paper by Crittenden, Weisshaar, Johnson, and Rutkowski [J. Aircraft **15**, 429-434 (1978)] concludes with an interesting discussion of the *K-E* (American) and the *p-k* (British) methods of solving the flutter equations and points to "a need for methods that give more correct aerodynamic forces at the imaginary axis." The British method is said to involve multiplying arbitrarily the imaginary part of the aerodynamic matrix by p/w and iteratively solving the equations until a solution is found for which p and k are consistent. It is said to be expensive therefore to use.

While all this is formally true, it does not represent normal British practice nor does it give the right flavor to the background for that practice.

That background is essentially the recognition that knowledge of the unsteady aerodynamic forces for real aircraft configurations in real flows is not precise. The various lifting surface theories which are frequently used have only been verified in simple situations like rigid heave, pitch, or roll. Where comparisons are possible in more complicated situations, e.g. control surface rotation, the differences can be quite marked.

It follows that the aerodynamic maxtrix Q should not be thought of as precise in any pedantic way, and in particular that its dependence on w should not be thought of as being precisely known. In any case, this functional dependence is weak, especially if excursions only from a well-chosen assumed value of w are of interest.

Therefore, for the most part, British practice does not slavishly ensure that the solution k agrees with the w for which the Q was calculated; only that the mismatch is not excessive.

The device of writing $Q(k) = Q_R(k) + iQ_I(k)$ in the form

$$Q(k)/Q_R(k) + ikQ_I'(k)$$

which is the equivalent of "multiplying by p/w", lessens the importance of any mismatch between solution k and assumed k, if over the region of interest in k, $Q_I'(k)$ is less dependent on k.

It follows that the iteration which is mentioned is not normally performed and that the solution of the flutter equation by the *p-k* method is as direct as by the *K-E* method, and therefore no more expensive. There is an iteration required in both procedures, but not mentioned by the authors, to effect a satisfactory match in assumed and solution Mach number; for in the transonic range, the Q is strongly M-dependent.

In very many situations the flutter speed depends hardly at all on the Q_I' (the aerodynamic damping) and indeed on only a few of the Q_R (the aerodynamic stiffness). The art in flutter estimation is to detect those coefficients which are dominant and to make an independent judgement of their likely (range of!) value. Often these coefficients relate to control surfaces, derivatives for which at zero frequency are known from tunnel tests. The question then arises as to how to inject such "outside data" into the formal aerodynamic estimation process.

These same thoughts underlie the British attitude to subcritical response. No British flutter analyst (I hope!) believes that the oscillatory lifting surface theories give, in any way, a formal solution to the aerodynamic forces resulting from general motion as implied in subcritical response calculations. But there is no reason to suppose that the errors in using oscillatory aerodynamic procedures for generalized motion are unacceptable (provided the oscillatory behavior is well enough represented), or that deployment of more generalized aerodynamic procedures, at greater expense, improves the real accuracy at all. This latter encompasses the solution of response problems in the frequency domain using Fast Fourier Transform with oscillatory aerodynamics interpolated from values at various frequencies.

It is therefore normal British practice to interpret the flutter equations as applying equally well to the time domain for subcritical response problems. Solutions in the time domain have direct physical meaning, even if not at the critical point, and may for example be used to predict flight behavior against which flight test results can be monitored. The equations have complex roots which also have physical meaning and are analogous to *p-k* for the critical case.

Overall there is still a need to assess the accuracy of the oscillatory theories other than for simple rigid modes, and particularly for control surfaces. There is also a need to review the accuracy with which the oscillatory aerodynamic theories represent the aerodynamic forces in typical subcritical responses, since this has not been reviewed for a long time. What offers?

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Reply by Authors to H.P.Y. Hitch

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THE authors thank Mr. Hitch both for his interest in our work and for his discussion of the philosophy behind the British practice of flutter analysis. The British approach has not received the acceptance it has deserved in the United States, and it is hoped that Mr. Hitch's remarks will contribute to a better appreciation of a more physically meaningful formulation of the flutter problem.

The *p-k* method employed in NASTRAN, and as used in our paper, follows a version of the British approach which "lines up" the frequency. Although "lining-up" the frequencies may not always be necessary, there are situations (e.g., when the short period ($k \sim 0$), the flutter instability, and a higher frequency autopilot instability are being investigated simultaneously) when NASTRAN's approach and capability are of decided benefit. Furthermore, it is logical that this large-scale general structural analysis procedure should utilize a consistent scheme, since it lessens the "art" required of the not necessarily expert user of the NASTRAN program.

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