Letters

Reference Aeronautical Journal, December 1983, 'Vortices and Turbulence' by Professor G. M. Lilley

This paper was not written as a review on researches into vortices and turbulence. Following discussion of the work of Lanchester, the author set out the details of some of the relevant contributions that he and co-workers had undertaken from the early 1950s to the present day. As a personal account it did not include references to many related research works. In omitting those references, the author neither wished to imply he was unaware of such contributions, nor that he was making claims as to the importance of his contributions in relation to those other works in the same field.

However, one important omission was that the author made no reference to the work of Robert Legendre of ONERA in France, on the vortex sheets from the leading edges of slender wings. To help redress the balance, I enclose herewith a brief summary of M Legendre's early work on that subject.

In 1932 M Legendre discovered vortex sheets springing from the leading edges of the blades of a model of a marine propeller. In 1950, working with Maurice Roy at ONERA, on delta wings, M Legendre proposed that leading edge vortex separation was likely and asked M Berert, Head of the wind tunnel at Cannes, to prepare a crucial experiment. It was found by M Berert, M Girerd and M Legendre, that vortex separation did occur. M Legendre asked M Werle to investigate the phenomenon in the water tunnel at ONERA designed by M Roy.

On 9th May 1951, following the completion of the experimental work, Maurice Roy suggested that the vortex sheet had the shape of 'tourbillon en cornet' and encouraged M Legendre to investigate a theory for the phenomenon.

In 1951 at a collaborative panel meeting of RAE and ONERA, under the chairmanship of Mr Ronald Shaw, the results of ONERA were discussed, but their interpretation was not accepted by Dr Dietrich Kuchmann, who argued that the flow was similar to the classical leading edge bubble. Nevertheless, the ONERA work continued and M Legendre presented an account (in French) at the IUTAM Congress in Istanbul in 1952. Later Adams (in USA) uncovered this paper. It was then that international work on this subject commenced, and as a result, Maurice Roy and Dietrich Kuchmann proposed that M Legendre be referred to as 'Ludwig Prandtl Trager'.

G. M. LILLEY

COMMENTS ON 'THE SOLUTION OF LARGE FLUTTER PROBLEMS ON SMALL COMPUTERS' by A. Simpson. Published in the Aeronautical Journal, April 1984, pages 128-140.

No flutter analyst should forget the debt owed to Frazer, Duncan and Collar for the extraordinary range and depth of the papers in the 1920s and 1930s. It was embarrassing to realise from Alan Simpson's work that I had forgotten R&M 1716. A re-reading, however, was reassuring in that the method described was essentially a method for its day — a way of obtaining critical flutter conditions from a large system of equations with limited computing power. Nowhere in R&M 1716 is the claim made that the method gives any physical insight into the problem. The claim that the variation with speed of the forces required to sustain harmonic oscillations does give some physical insight should be made with extreme caution. Two of Frazer's papers R&M 1795[2] and R&M 1872[3] show that the connection between forced response and stability can be very confusing, and the only definite statement that can be made is that a system at finite airspeed undergoing harmonic oscillation with zero excitation is fluttering. It should, therefore, be concluded that the method of R&M 1716 should be regarded as giving with certainty only the critical airspeed and frequency.

Simpson's approach shows elegantly how a surprising amount can be done with a small computer. However, the need for an initial approximation for the process to start and the lack of certainty of converging on to the lowest critical airspeed make the method unlikely to appeal to most flutter analysts with access to a computer similar to a VAX 11/780, for instance. Such a computer is certainly going to be needed for the task of interpreting the initial flutter condition with regard to its accuracy, its sensitivity to improving modifications and the optimising of improvements, which usually follow the initial computation of the critical flutter conditions.

REFERENCES

1. DUNCAN, W. J., COLLAR, A. R., LYON, H. M. Oscillations of elastic blades and wings in an airstream. 1936, ARC R&M 1761.
2. FRAZER, R. A., JONES, W. P. Forced oscillations of aeroplanes with special reference to von Schlippe's Method of predicting critical speeds for flutter. 1936, ARC R&M 1795.
3. FRAZER, R. A. On the power input required to maintain forced oscillations of an aeroplane wing in flight. 1939, ARC R&M 1872.

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Author's Reply

I much appreciated Mr. Jack Baldock's comments on my paper. However, it did surprise me that Jack chose to reply so quickly. Not to press the simile in my Introduction too far, I gather he has washed out a few of the old bottles I suggested (and a few more as well), but has not yet filled them with the new wine!

It is clear that the authors of R&M 1716 were not aware of the fact that the inverse method used by them was the Generalised Rayleigh Quotient (GRQ) procedure, implemented by the Secant Method. This is hardly surprising since such techniques as GRQ were not part of the engineering currency at that time. The GRQ method is known to be applicable to all matrix eigenvalue problems in which the matrix is a regular lambda-matrix or a regular transcendental matrix (*see* Ref. 4 of my paper). Thus, in the flutter problem, it does not fail simply because one ventures away from a flutter

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