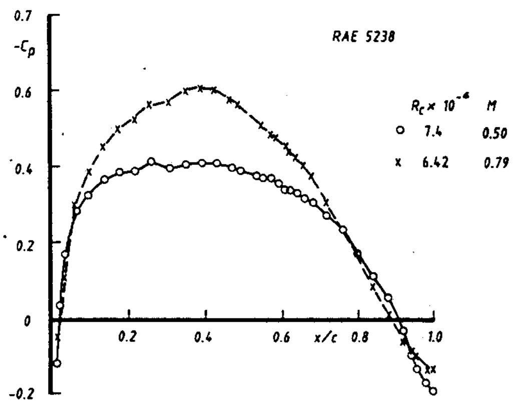


**Fig. 1 Typical pressure distributions on RAE swept panel model.**

that the $m/m_b$ ratios for peak responses are different from our sparse measurements.

Many other critical comments could be made on Ref. 1 but we prefer to close on a positive note. A recent paper[6] stresses the importance of the outboard wing sections when determining the conditions for limit cycle oscillations (LCO). The crucial importance of the tip region (where the amplitude is largest) was stated explicitly in Ref. 3. We believe that quasisteady theory still offers an adequate explanation of the LCOs caused by transition described in Refs. 3 and 5.

## References

[1]Ericsson, L. E., "Transition Effects on Airfoil Dynamics and the Implications for Subscale Tests," *Journal of Aircraft*, Vol. 26, No. 12, 1989, pp. 1051–1058.

[2]Ericsson, L. E., "Comment on Aeroelastic Oscillations Caused by Transitional Boundary Layers and Their Attenuation," *Journal of Aircraft*, Vol. 25, No. 10, 1988, pp. 975–976.

[3]Mabey, D. G., Ashill, P. R., and Welsh, B. L., "Aeroelastic Oscillations Caused by Transitional Boundary Layers and Their Attenuation," *Journal of Aircraft*, Vol. 24, No. 7, 1987, pp. 463–469.

[4]Mabey, D. G., Ashill, P. R., and Welsh, B. L., "Reply by Authors to L. E. Ericsson," *Journal of Aircraft*, Vol. 25, No. 10, 1988, p. 976.

[5]Mabey, D. G., and Ashill, P. R., "On Aeroelastic Oscillations Associated with Transitional Boundary Layers," Royal Aerospace Establishment, England, UK, TM Aero 1995, Feb. 1984.

[6]Mayer, J. J., and Zwaan, R. J., "Investigation of a Semi-Empirical Method to Predict Limit Cycle Oscillations on a Modern Fighter Aircraft," "*Aircraft Dynamic Loads due to Flow Separation*," AGARD CP 483, 1990.

# Reply by Author to D. G. Mabey and P. R. Ashill

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As *Journal of Aircraft* readers know by now, D. G. Mabey and I have a difference of opinion[1,2] about the flow physics causing the divergent bending oscillations observed in a test of a slightly swept wing.[3] This Comment[4] indicates that it is possible to miss the point made in Ref. 5 about why the divergent bending oscillations occurred only in a Reynolds number range for which boundary-layer transition took place in a region near midchord. This is the only region on the airfoil where the static pressure gradient is mild enough to allow the moving wall effects, generated by the bending oscillations, to dominate over the static pressure gradient and provide the coupling between wing bending and transition location, which could generate the divergent bending oscillations. Thus, what was needed of the pressure distribution in the insets of Figs.[11–13] in Ref. 5 was "merely to represent the general character of the wing pressure distributions,"[4] which they, according to this comment, apparently did.

In regard to the specific points made in Ref. 4, my response is as follows.

1) The pressure distributions support the physical flow picture presented both in Refs. 3 and 5. The reader is referred to Ref. 1 to find out what the fundamental difference between the two flow assumptions is.

2) Remembering that it is the pressure gradient rather than the pressure level that counts, the inset pressure distributions in Fig. 12 of Ref. 5 tell the same story as those in Fig. 1 of Ref. 4. In regard to the quasisteady effect of a difference in the induced incidence, the reader is again referred to Ref. 1.

3) The difference in the product of density and velocity for the two Reynolds numbers should provide the same relative difference at $m/m_b > 0.3$ as at $m/m_b < 0.2$. The reason for the lack of difference at $m/m_b > 0.3$ is that the large blowing rate has moved transition far forward toward the leading edge, possible fixing it at the 5% chord location of the blowing orifice. When transition was moved that far forward by increasing the Reynolds number to $Re > 8 \times 10^6$, there was an insignificant difference between the bending responses for free and fixed transition (see Fig. 1 of Ref. 3 or Fig. 1 of Ref. 5).

It is unfortunate that the authors of Ref. 4 restrained themselves from making the other critical comments they say they had. It is my firm belief that airing differences of opinion in this manner helps to clarify the technical issues involved.

## References

[1]Ericsson, L. E., "Comment on Aeroelastic Oscillations Caused by Transitional Boundary Layers and their Attenuation," *Journal of Aircraft*, Vol. 25, No. 10, 1988, pp. 975–976.

[2]Mabey, D. G., Ashill, P. R., and Welsh, B. L., "Reply by Authors to L. E. Ericsson," *Journal of Aircraft*, Vol. 25, No. 10, 1988, p. 976.

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[4]Mabey, D. G., and Ashill, P. R., "Comment on Transition Effects on Airfoil Dynamics and Implications for Subscale Tests," *Journal of Aircraft* Vol. 29, No. 3, p. 505.

[5]Ericsson, L. E., "Transition Effects on Airfoil Dynamics and the Implication for Subscale Tests," *Journal of Aircraft*, Vol. 26, No. 12, 1989, pp. 1051–1058.