slope to a small but still positive value. In order to obtain quasisteady negative damping, the total aerodynamic derivative, $(C_{L\alpha})_{FT} - (\Delta^i C_{L\alpha})_{TR}$, must be negative. Actually, before divergent oscillations will result, the negative aerodynamic damping must be of a magnitude sufficient to overcome the structural damping present in the test, being 0.5% or more of critical.

Thus, the classic quasisteady flow concept cannot explain the observed divergent oscillations, and the distorted quasisteady flow concept offered in conjunction with Fig. 8 in Ref. 1 should never have been allowed to be published. Even more incredible is the claim made on p. 467, that "...the flow sketches of Fig. 8 could give a physical explanation for losses in pitch damping observed with transitional boundary layers." For pitching oscillations at $\alpha_0 = 0$, the generalized quasisteady angle of attack is $\tilde{\alpha} = \theta$, and the pitching moment coefficient is

$$C_m = [(C_{m\alpha})_{FT} - (\Delta^i C_{m\alpha})_{TR}]\theta \tag{3}$$

That is, the classical quasisteady aerodynamics affect only the aerodynamic stiffness and, thereby, the frequency of the oscillation. They have no effect on the damping and, therefore, cannot influence the amplitude of the oscillation.

The experimentally observed effect of free transition on the pitch damping[2] is caused by convective flow time lag, through the associated phase lag, as is explained in detail in Ref. 3. Adding the time lag effect, Eq. (3) can be written as

$$C_m(t) = (C_{m\alpha})_{FT}\, \theta(t - \Delta t_i) - (\Delta^i C_{m\alpha})_{TR}\, \theta(t - \Delta t_i - \Delta t_v) \tag{4}$$

where Δt_i is the inviscid flow circulation lag and Δt_v is the convective viscous flow time lag. For the low reduced frequencies of interest here, Eq. (4) can be expanded in a Taylor series to yield

$$C_m(t) = (C_{m\alpha})_{FT}\, [\theta(t) - \Delta t_i\, \dot{\theta}(t)\ldots] - (\Delta^i C_{m\alpha})_{TR}\, [\theta(t) - (\Delta t_i + \Delta t_v)\, \dot{\theta}(t)\ldots] \tag{5}$$

That is, if the quasisteady effect is to decrease the aerodynamic stiffness (and thereby the oscillation frequency), the effect of the phase lag is to increase the aerodynamic damping, thereby decreasing the oscillation amplitude. This is exactly the effect predicted[4] and observed experimentally[5] on slender bodies of revolution; i.e., the aerodynamic damping is increased – not decreased, as the authors of Ref. 1 erroneously claim – by the effect of free transition.

References

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[2]Ericsson, L. E. and Reding, J. P., "Dynamic Stall of Helicopter Blades," *Journal of the American Helicopter Society*, Vol. 7, Jan. 1972, pp. 10–19.

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Reply by Authors to L. E. Ericsson

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We would like to thank Dr. Ericsson for his comment on our paper.[1]

We accept that the quasisteady concept cannot yield a change in pitch damping for which some consideration of phase is essential. Our point is that the sketches regarding the relation between steady incidence and transition position could, with knowledge of or an assumption about phasing, give an explanation for the loss in pitch damping. We regret that we omitted to make the need for phase information explicit.

We referred (Ref. 1, p. 463) "to a loss of pitch damping on full-scale re-entry bodies at high supersonic speeds." This is an appropriate warning, because the summary of Ref. 2 also refers to a serious decrease in damping on bluff bodies at hypersonic speeds due to transition movements. Reference 2 also cites an increase in pitch damping on slender bodies due to transition movements, but this seemed inappropriate for the warning we wanted to give.

With regard to the bending vibrations observed, we see no conflict between ourselves and Dr. Ericsson. We agree that, on a quasisteady basis, the aerodynamic damping for bending oscillations is related to the slope of the lift curve. In Fig. 8c of Ref. 1, transition case B is related to a reduced, but still positive, slope (the dashed line), while transition case C *is* related to a negative slope (the other dashed line).

The validity of the quasisteady theory would be tested by local static lift measurements over outboard positions close to $C_L = 0$, $\alpha = 0$. Such local lift measurements were not possible during our tests, but the *overall lift curve* for the complete wing with natural transition is close to zero at $R_{\bar{c}} = 6 \times 10^6$, whereas with transition complete it is positive, as would be expected.

We regard our explanation [Ref. 1, Eq. (2)] of an effective lift/curve slope, which is the product of a constant lift/curve slope and a term expressing the effective camber, as being physically sound. As shown in Fig. 9 of Ref. 1, the loss of effective camber with forward movement of transition becomes more marked with increasing adverse pressure gradients over the rear of the section. This is consistent with our observation that the oscillations were more pronounced for section RAE 5238 than for RAE 5237, the former being closer to separation at the trailing edge than the latter. Any explanation ignoring this physical feature of the flow is incomplete. The camber effect for dynamic motion might be somewhat different from that for quasistatic motion. Nevertheless, we think that the remark on p. 466 on Ref. 1 pertaining to the locally reduced value of the quasisteady lift/curve slope is relevant to any explanation of the phenomenon. We both agree that the phenomenon has potentially serious implications whenever transitional boundary layers occur, because it can modify aerodynamic damping in both rigid-body and structural modes.

References

[1]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, July 1987, pp. 463–469.

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