and so, if

$$b_{rr} \gg b_{ss}, \qquad \omega \doteq \omega_s \tag{13}$$

Thus, if the direct dampings are not balanced, the damping line is close to the conic at $\nu^2 = 0$, and its intersection with the conic probably occurs at a low value of ν^2. Decreasing the larger damping coefficient or increasing the smaller coefficient, while not altering the conic significantly because of the high density ratio, moves the intersection of the damping line and the axis away from the conic and, providing the change in the slope of the damping line is not excessive, either of these changes moves the intersection of damping line and conic to a higher value of ν^2 and, hence, increases the flutter speed. Qualitatively, it matters not whether the direct damping is aerodynamic or structural.

Concluding Remarks

It has been shown that the effect of density ratio on flutter can be clarified by a particular form of solution of the flutter equation that leads to the visualization of the solution as the intersection of a conic and a straight line. Part of the effect is surffered only by the conic. Strictly, the aerodynamic coefficients should always be those appropriate to the reduced frequency at the intersection, but experience shows that insights into the equtions can be obtained even when the coefficients are not exact. This has been demonstrated by the clarification of the conditions under which "anomalous" behaviors with increase of damping occur.

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Technical Comments

Comment on "The Role of Structural and Aerodynamic Damping on the Aeroelastic Behavior of Wings"

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AFTER Ref. 1 was presented at the Symposium on Structural Dynamics and Aeroelasticity in 1965, this commentator offered some remarks and showed two slides at the meeting that were related to the inaccuracies associated with the aerodynamic approximations employed. Since it is not an American custom to append remarks in meeting proceedings, the remarks and slides were not published. However, they have been included in this commentator's notes for teaching aeroelasticity.[2] Reference 3 now appears, 21 years later, and draws its conclusions based on similar aerodynamic approximations, again with no regard for the magnitude of the associated errors or the current state-of-the-art of unsteady aerodynamic theory. This suggests that a more formal publication of the 1965 remarks and slides is long overdue.

The first slide (Fig. 1) compared Pines' quasisteady aerodynamic method[4] with Theodorsen's exact aerodynamic solution[5] in a flutter analysis of a two-degree-of-freedom airfoil at sea level. The example airfoil was mounted at its 40% chord elastic axis on bending and torsion springs, having uncoupled bending and torsion frequencies of $\omega_h = 10$ rad/s and $\omega_\theta = 25$ rad/s, respectively. The airfoil mass m was such that its mass ratio was $\mu = m/\pi\rho b^2 = 20.0$, where ρ was the density at sea level and the semichord was $b = 3.0$ ft. A range of centroids was considered from 35 to 85% chord, assuming a constant dimensionless centroidal radius of gyration $r_c = 0.49$, or a dimensional radius of gyration of $r_c b = 1.47$ ft. Also shown in Fig. 1 is a curve based on the variation suggested by Pines in which the airfoil lift curve slope c_{ℓ_α} was refined (iteratively) by $c_{\ell_\alpha} = 2\pi|C(k)|$ in which $C(k) = F(k) + iG(k)$ is the Theodorsen function, $|C(k)|$ its magnitude, and $k = \omega b/V$ the reduced frequency. This refinement is seen to improve the agreement with the exact solution for a limited range of centroid (from 40 to 50% chord). More significantly, however, Pines' method predicts *no* flutter for centroids forward of the elastic axis (note that no coalescence can occur with a centroid forward of the elastic axis) and much higher flutter speeds than the exact solution for aft centroid locations (behind the 50% chord).

The second slide (Fig. 2) compared Dugundji's low-frequency aerodynamic method[6] with the Theodorsen solu-

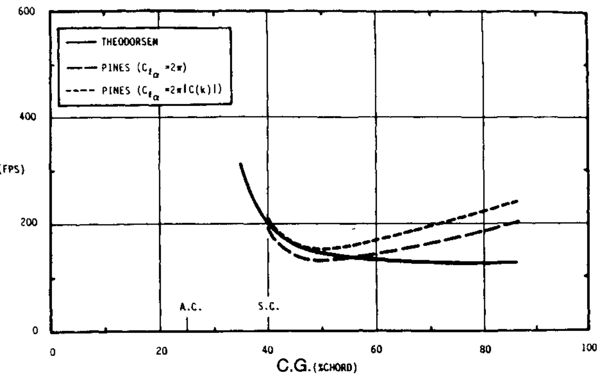

Fig. 1 Comparison of general unsteady and quasisteady flutter analyses.

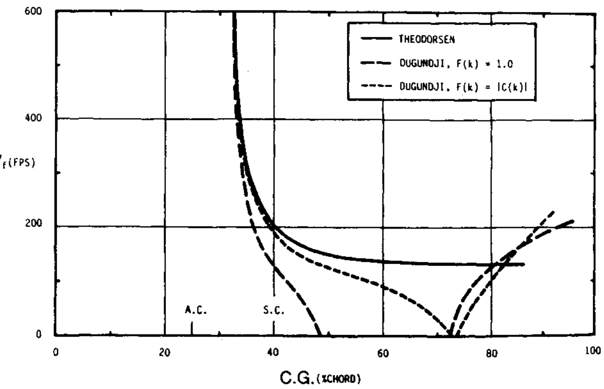

Fig. 2 Comparison of general unsteady and first-order unsteady flutter analyses.

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tion for the same airfoil and range of centroid. Dugundji's method is seen to be conservative or overly conservative for forward centroids but totally unreliable for aft centroids. Pines' refinement using $F(k) = |C(k)|$ decreases the conservatism somewhat for forward centroids, but the solution remains unacceptable for far aft centroids. The difficulty with Dugundji's method is that his low-frequency assumption of $F(k) \approx 1.0$ and $G(k) \approx 0.0$ overlooks the fact that, as k goes to zero, *the limit of* $G(k)/k$ *is minus infinity rather than zero*, which results in an estimate of low-frequency damping coefficient $C_{L_{\dot{\alpha}}}$ that is incorrect, *not only in magnitude but also in sign*, as is evident from Etkin's Eq. (7.10,6)[7]:

$$C_{L_{\dot{\alpha}}} = \pi[1 + 2G(k)/k] \tag{1}$$

When these approximate methods are applied to lifting surfaces with the additional approximation of strip theory,[8] as they were in Ref. 3, the errors are compounded. Whatever successes strip theory has enjoyed over the years since its recommendation by Smilg and Wasserman in 1942 have been the result of a *conservatism in aerodynamic stiffness* (the outboard loading not decreasing to zero at the tip) *offsetting an unconservatism in aerodynamic damping*. The problems of two-dimensional aerodynamic damping have an extensive literature, e.g., Refs. 7, 9–12, so that one would assume these problems to be well known. Unfortunately, Lottati's Note indicates that this is not the case. More unfortunately, however, a publication[13] of the Federal Aviation Administration that describes strip theory as an acceptable method for determining the flutter safety of light airplanes also indicates an unfamiliarity with the problems of two-dimensional aerodynamic damping.

A standard textbook example illustrates the unconservatism of strip theory compared to lifting surface theory. The configuration is the BAH wing, i.e., the jet transport wing considered throughout Ref. 14, and again in Ref. 15, at sea level. Three stability curves, shown in Fig. 3, for the critical first torsion mode were obtained by applying the MSC/NASTRAN version of the British (PK) method[15–17] of flutter analysis. The British method is preferred for obtaining realistic estimates of the system damping away from the neutral stability point, in contrast to the American (K) method,[4,8,15] which obtains only an artificial mathematical system damping. The lifting surface solution used the doublet-lattice method (DLM)[18,19] at a Mach number $M = 0.0$. The strip theory solution used the exact Theodorsen function. The modified strip theory used Diederich's formula for the total lift-curve slope[20] and the Schrenk approximation[21] for the spanwise distribution of the local lift-curve slope for the modification. [This modification considers only spanwise variations in the local lift-curve slope. Yates' modified strip theory[22] also considers spanwise variations of the aerodynamic center (assumed at the 1/4 chord here) and the downwash collocation point (assumed at the 3/4 chord here).] No structural damping (g) was included a priori. The following results are taken from Fig. 3. First, with no damping ($g = 0.0$), the DLM predicts flutter at a velocity $V = 1060$ ft/s, whereas strip theory predicts $V = 1157$ ft/s (9.2% higher), and the modified strip theory predicts 1276 ft/s (20.4% higher). Secondly, with the actual structural damping recommended in Ref. 13 of $g = 0.03$ (which may be an unreliable value to assume throughout the service life of an airplane and may not exist in all modes after assembly), the DLM predicts $V = 1170$ ft/s, strip theory predicts *no flutter*, and the modified strip theory gives $V = 1428$ ft/s (22.1% higher than the DLM prediction).

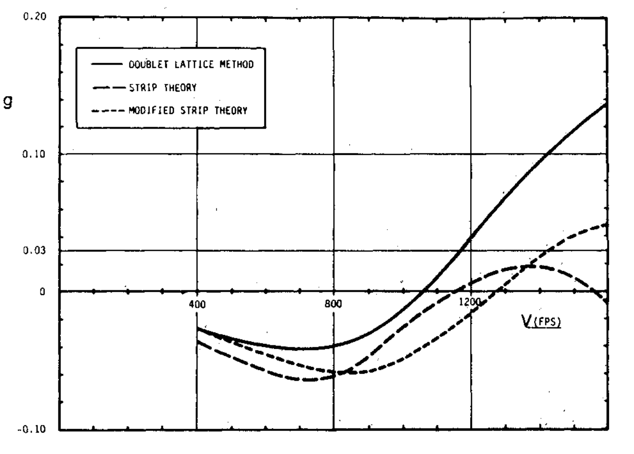

Fig. 3 Stability curves for critical torsion mode of BAH wing by three aerodynamic theories.

In this example, the more accurate analysis of aerodynamic damping from lifting surface theory results in a flutter prediction that has a lower speed than the strip theories, is more violent (by virtue of the much higher slope of the V-g curve at $g = 0.0$), and is relatively insensitive to the magnitude of the actual structural damping (because of the much higher maximum value of g).

One can only wonder why Lottati did not utilize his own lifting surface theory[23–26] in Ref. 3. Certainly, the generality of his conclusions is unwarranted since they are based on so many approximations. It is hoped that these comments have served to illuminate the relative values of strip theory (its historical and *qualitative* values *in the classroom*) and lifting surface theory (its *quantitative* value in design, analysis, research and *in flight*) and that they have clarified the role of aerodynamic damping on the aeroelastic behavior of wings in its proper context of three dimensions.

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Reply by Author to W. P. Rodden

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I WOULD like to acknowledge the interest that W.P. Rodden has shown in my Note.[1] The main point of Rodden's lengthy Comment is the accuracy of the aerodynamic strip theory compared to the lifting surface theory. It should be mentioned that the accuracy of the lifting surface theory is unquestionable and should be applied whenever possible. The strip theory is applied mainly for its simplicity and its ability to predict the mechanism of flutter and to get trends while conducting parametric studies. It should be noted that the strip theory is still used in research whenever aeroelastic characteristics of high-aspect-ratio wing are to be investigated. The lifting surface theory is more complicated in application and is therefore used in design and for low-aspect-ratio aerodynamic wing calculations.

It should be emphasized that the many recently published papers dealing with aeroelastic subjects apply the strip theory, proving that the situation described in Rodden's Comment is not accurate.

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Notice to Subscribers

We apologize that this issue was mailed to you late. As you may know, AIAA recently relocated its headquarters staff from New York, N.Y. to Washington, D.C., and this has caused some unavoidable disruption of staff operations. We will be able to make up some of the lost time each month and should be back to our normal schedule, with larger issues, in just a few months. In the meanwhile, we appreciate your patience.