Table 1 Lift coefficients and drag coefficients obtained with various force calculation techniques

Force analysis technique	Medium grid $\alpha = 2$ deg C_L	Medium grid $\alpha = 2$ deg C_D	Medium grid $\alpha = 4$ deg C_L	Medium grid $\alpha = 4$ deg C_D	Fine grid $\alpha = 4$ deg C_L	Fine grid $\alpha = 4$ deg C_D	Coarse grid $\alpha = 4$ deg C_L	Coarse grid $\alpha = 4$ deg C_D
Surface pressure integ.	0.1751	0.0032	0.3496	0.0081	0.3482	0.0080	0.3496	0.0086
Far-field integration	0.1750	0.0026	0.3496	0.0079	0.3481	0.0075	0.3493	0.0079
Wake integration								
$\Delta x/c_r = 0.1$	0.1754	0.0014	0.3498	0.0057	0.3486	0.0056	0.3491	0.0056
$\Delta x/c_r = 0.5$	0.1760	0.0014	0.3510	0.0056	0.3485	0.0054	0.3487	0.0053
$\Delta x/c_r = 1.0$	0.1763	0.0014	0.3509	0.0054	0.3493	0.0053	0.3485	0.0052

Conclusions

Several techniques for the calculation of drag using an Euler-equation formulation are discussed and compared. The technique based on the integration of the surface pressures (near-field) and two far-field techniques, one based on the integration of the momentum flux along a closed contour enclosing the configuration and the second based on the evaluation of a wake integral, are described and applied to three-dimensional flowfield solutions. The present calculations are limited to steady, low-Mach-number flows in the absence of active systems such as surface blowing/suction and propulsion.

The far-field technique based on the wake of the wing appears to provide the most consistent and accurate drag predictions. Both the surface pressure integration technique and the momentum-integration technique give erroneous drag values, mainly as a result of the inherent numerical viscosity in the Euler solutions and the inadequate grid resolution in the flowfield.

Acknowledgment

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Technical Comments

Comment on "Transition Effects on Airfoil Dynamics and Implications for Subscale Tests"

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In Ref. 1, L. E. Ericsson repeats flawed explanations[2] for the results of our previous observations,[3] which we have already rebutted.[4] However, we regret that a further rebuttal is necessary because in Ref. 1 Ericsson has misinterpreted an unpublished report that is not widely available.[5]

Reference 5 provided small sketches of p/pt (where p and pt are the static and total pressure, respectively) that were intended merely to represent the general character of the wing pressure distributions. Ericsson has reproduced these, labeled as curves of $-Cp$, and made some unrealistic inferences. We offer the following brief remarks:

1) With regard to the measurements at $M = 0.80$, $R = 6.5 \times 10^6$ (Ref. 1, Fig. 11), the milder adverse pressure gradient with the RAE 5237 profile compared to the RAE 5238 ensures that the growth of the boundary layer over the rear of the aerofoil is less rapid for the former than for the latter. Hence, the response for the RAE 5237 profile is smaller, as explained in Ref. 3.

2) With regard to the comparison of measurements at $M = 0.5$ and 0.8 on RAE profile 5238 (Ref. 1, Fig. 12 and Fig. 1 here based on the measurements of Ref. 5), the larger response at $M = 0.5$ is due, primarily, to the larger induced incidence (due to the decreased freestream velocity) compared to $M = 0.80$. It is likely that the differences in the C_p distribution, Reynolds number, and aerodynamic damping do not contribute significantly to the widely differing responses.

3) With regard to the comparison of measurements at $M = 0.8$ on RAE profile 5238 at two Reynolds number (Ref. 1, Fig. 13), the response is higher at $R = 6.5 \times 10^6$ than at 3.0×10^6 primarily because of the increased magnitude of the negative aerodynamic damping (proportional to the product of density $\times$ velocity). Ericsson cannot justify his remark

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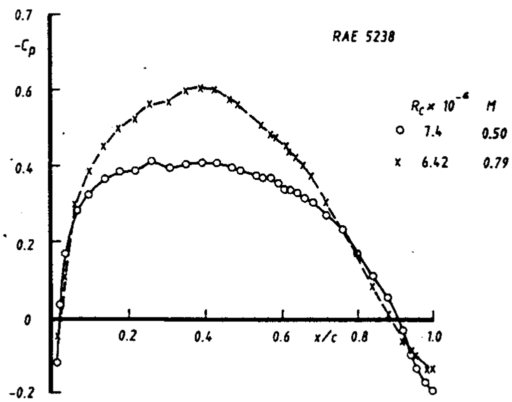

Fig. 1 Typical pressure distributions on RAE swept panel model.

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.

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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.

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