chordline and is

$$\frac{q_2}{V_\infty} = 1 + \frac{u_{1T}}{V_\infty} \pm \frac{u_{1L}}{V_\infty} + \frac{u_{2T}}{V_\infty} \pm \frac{u_{2L}}{V_\infty} + (C \pm T)(C'' \pm T'') + \frac{1}{2}(C' \pm T')^2 - \frac{\alpha^2}{2} \tag{14}$$

and the surface pressure coefficient is

$$C_{p2} = -2\left(\frac{q_2}{V_\infty} - 1\right) - \left(\frac{u_{1T}}{V_\infty} \pm \frac{u_{1L}}{V_\infty}\right)^2 \tag{15}$$

The lift coefficient (correct to second order) is obtained from the substitution of Eq. (15) into Eq. (8) and is

$$C_L = \frac{4}{c}\int_{-c/2}^{c/2}\left\{\frac{u_{1L}}{V_\infty} + \frac{u_{1T}u_{1L}}{V_\infty^2} + \frac{u_{2L}}{V_\infty} + TC'' + CT'' + C'T'\right\} dx \tag{16}$$

Using the terminology of this Comment, the lift coefficient for the improved thin-airfoil theory of Ref. 1 is

$$C_L = \frac{4}{c}\int_{-c/2}^{c/2}\left(\frac{u_{1L}}{V_\infty} + \frac{u_{1T}u_{1L}}{V_\infty^2}\right) dx \tag{17}$$

and it can be seen that it is missing the last four terms in the integrand of the correct result in Eq. (16). The lift coefficient in Eq. (17) was obtained by keeping the quadratic (second-order) term in Bernoulli's equation for the pressure coefficient [for use in Eq. (8)] even though terms comparable in magnitude had been neglected in the determination of the velocity components.

In addition to the above incorrect use of thin-airfoil theory to include the effect of thickness in the lift and moment coefficients, the authors present a technique to evaluate the Fourier coefficients for the velocity using the airfoil ordinates rather than the airfoil slopes. For the integral equation version of thin-airfoil theory presented here, a similar technique is given in Van Dyke[2] that is based on results published by Riegels and Wittich in the 1940s.

Also, the results of the analysis of Ref. 1 for a series of Kármán-Trefftz airfoils are compared with exact solutions using complex variables as well as the results of a panel code identified as the Hess-Smith method adopted by Moran.[3] It is claimed that the improved thin-airfoil theory (with a leading-edge correction for the pressure) outperforms the panel method for a certain range of the airfoil parameters.

According to Hess,[4] the code used in Ref. 1 is the 1967 version. The Kármán-Trefftz airfoils for which the panel method lift are most in error are limiting cases of the airfoil shape having sharp leading edges (for example, $\beta = 9$ deg, $\tau = 0.04$, $\epsilon = 0$ and $\beta = 13.5$ deg, $\tau = 0.06$, $\epsilon = 0.04$) and therefore not appropriate for comparison. Hess states that "the current higher-order panel method has no trouble with such shapes." He also states that "the difficulty the 1967 code has at cusped trailing edges is well-known" and was resolved in Hess.[5] "To remove the pressure crossing near the trailing edge, recourse to the higher-order method is not necessary, but it suffices to use the lower-order methods with a surface vorticity weighted parabolically (instead of constant) around the airfoil in such a way that it falls to zero at the upper and lower trailing edge."

Finally, a word needs to be said about the pressure distribution results presented by the authors. The results of thin-airfoil theory are invalid in the neighborhood of stagnation points that occur near round leading edges or at finite angle trailing edges. The authors introduce a correction due to Riegels to alleviate the "unrealistically high velocities near the leading edge" but then claim that the results for a very thin airfoil (shown in Fig. 4) are "almost identical" with or without the correction. The extent of the neighborhood of the leading edge where the thin-airfoil results are invalid is of the order of the thickness ratio squared and an expanded plot of this region in Fig. 4 would clearly show the same behavior seen in Fig. 5. An excellent discussion of the edge singularities of thin-airfoil theory and a unified theory of corresponding leading-edge corrections can be found in Van Dyke.[2]

In conclusion, it appears that the "improvements" to thin-airfoil theory that the authors present have been provided correctly in 1956 by Van Dyke[2] and that accurate airfoil solutions can be obtained from the current versions of many available panel codes.

References

[1]Zedan, M. F., and Abu-Abdou, K., "Improved Thin-Airfoil Theory," *Journal of Aircraft,* Vol. 25, No. 12, 1988, pp. 1122–1128.

[2]Van Dyke, M. D., "Second-Order Subsonic Airfoil Theory Including Edge Effects," NACA Rept. 1274, 1956, pp. 541–560.

[3]Moran, J., *An Introduction to Theoretical and Computational Aerodynamics,* Wiley, New York, 1984.

[4]Hess, J. L., Private communication, letter dated Feb. 14, 1989.

[5]Hess, J. L., "The Use of Higher-Order Surface Singularity Distributions to Obtain Improved Potential Flow Solutions for Two-Dimensional Lifting Airfoils," *Computer Methods in Applied Mechanics and Engineering,* Vol. 5, No. 1, 1975, pp. 11–35.

Reply by Authors to A. Plotkin

M. F. Zedan* and K. Abu-Abdou*
King Saud University, Riyadh, Saudi Arabia

THE authors wish to thank A. Plotkin for his interest in their work. The technical comment addresses three points: 1) the order of accuracy of the method, 2) comparison with the panel method, and 3) the results of the parametric study. In response to the first point, we would like to emphasize that the method presented in the paper was not intended or claimed to be a second-order, thin-airfoil theory at all; in fact, nothing was mentioned about the order of the method. Although we are glad to see that our expression for C_L shares some common terms with a more rigorous second-order theory, we believe that this part of the technical comment has missed the main objective of the work. It was repeatedly stressed that the power of the improved thin-airfoil theory lies in the fact that enhanced accuracy is achieved while fully maintaining the main popular characteristics of the classical theory; namely, closed-form solution, simplicity, and ease of programming. The presented method may be considered as an improved first-order, thin-airfoil theory if one wishes to do so. The improvement is brought about by making no approximations in evaluating the pressure distribution beyond those already made in obtaining the velocity expression in Eq. (6) of the comment. There is no obvious reason, at least to us, to apply the linearized Eq. (7) to obtain the pressure coefficient; instead we have directly applied the Bernoulli equation. We do not see how the use of an exact relation like the Bernoulli equation could be disqualified as an "incor-

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*Associate Professor, Mechanical Engineering Department, College of Engineering.

rect use of the thin-airfoil theory" and how the use of an approximation of this equation would be more correct. Common sense indicates that avoiding further approximations—when feasible—would mean improved overall accuracy. The results of the numerous test cases presented in the paper showed substantial and consistent improvements in the prediction of C_L and C_M over the existing classical method.

The second-order analysis of Van Dyke as presented in Plotkin's comment is based on a perturbation technique and appears to be quite rigorous. However the utility of this analysis is not obvious; we presume that the integration of the last four terms in the integrand of Eq. (16), in a closed form, may not be a simple task. Correct evaluation of these integrals would definitely make the method so cumbersome that it loses the main advantage of the thin-airfoil theory, namely simplicity. This could be the reason why Van Dyke's method is not discussed frequently in aerodynamic texts. We are currently trying to get a copy of the report in which this method is discussed to see if (and how) the author evaluated these integrals. It would be interesting to see some of the results for this second-order theory, especially a breakdown of the contributions of the last four terms in Eq. (16) and an appraisal of the labor involved in evaluating them.

As for the second point in the comment, we would like to emphasize that the comparison with the panel method was not intended to be a central issue in the paper as the comparison between the improved and classical thin-airfoil theories. It is acknowledged that modern panel codes are capable of handling arbitrary profiles without difficulty. The Hess-Smith (HS) panel method we used was chosen simply because its source code is listed in Ref. 1. It is not mentioned in this 1984 reference that the program is based on a 1967 code. We believe it was justifiable to compare the accuracy of our simple easy-to-apply method with that of a well established computational method such as the panel method. The conclusions drawn were not claimed to be valid beyond the range of the parametric study. The profiles and parameters used in this study covered a wide range and therefore were not planned in any way to amplify the disadvantages of the Panel method as implied in the comment.

Concerning Plotkin's last point, it should be clear from our paper that the improved theory's main outcome lies in the added corrections to the expressions for the overall aerodynamic coefficients C_L and C_M, not to the detailed velocity or pressure distributions. The shortcomings of the thin-airfoil theory in predicting velocities near stagnation points are well known and are not removed by improving on C_L and C_M. The Riegel correction was applied as usual to reduce the extent of the region of odd velocity behavior near the leading edge of the airfoil. This is again a well accepted practice in the aerodynamic analysis of airfoils using thin-airfoil theory.

In closing this response, we agree with Plotkin that the present method cannot compete with the modern versions of panel codes beyond a certain range of applications. However, the major part of his comment is related to a higher order, thin-airfoil method that is different from what we presented. We feel that some of what he quoted or rephrased from our paper has been taken out of context.

Reference

[1]Moran, J., *An Introduction to Theoretical and Computational Aerodynamics*, Wiley, New York, 1984.

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