was found directly, without first finding the normal modes of vibration. Therefore, the commentators' criticism of a "frequency" equation is not applicable. It is possible that Forrestal et al. are referring to some specific equation in the paper, but they did not indicate the equation or the missing terms to which they refer. Since the commentators do not give sufficient information to identify the object of their criticism in our solution, it is not possible to produce a specific reply. Nevertheless, this writer has re-examined the derivation of the differential equation governing the unknown time-dependent terms of the series solution, Eq. (17) in the paper, assuming that Forrestal et al. may be referring to it. This equation was found to be correct for the linear analysis of the problem examined.

The paper examined the broad problem of a nonaxisymmetric time-varying pressure pulse of finite duration applied to an infinitely long cylindrical shell using the linearized Flügge shell equations. Solutions were presented for several thickness-to-radius ratios, and for several types of loadings. One of these loadings was the limiting case of the "pure" impulse (zero duration time), with which Forrestal et al. have compared their modal solution. It must be noted that they show only the bending stresses in their figure, for an unspecified thickness-to-radius ratio. The peak bending stresses are always less than the peak membrane stresses for this problem, for thin shells, as indicated in the original paper, and thus the figure magnifies the importance of the differences between the two sets of results. They show an increase of 33% in the peak bending stress over our values, from $\sigma h/Ic = 0.3$ to 0.4. However, the peak membrane stress is about 0.7, as shown in our paper. If the total stress was plotted, it would show that peak values would increase from 1.0 to 1.1, or only 10%, using their results.

Furthermore, we stated in our paper that the purely elastic solution for the response would be useful only during early time periods, because the influence of damping or dissipative effects would become important soon after the load was cut off. Therefore, we did not consider that the response at large times, say $\tau > 20$, would be meaningful for a pure impulse load, since the peak stresses and deformations were surely to be attenuated. In this connection, it should be noted that Forrestal et al. show different results than ours only for those later times, when the usefullness of the solution is in doubt.

Comment on "Skin Friction on Porous Surfaces Calculated by a Simple Integral Method"

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Zien has recently described the application of a "refined" integral method[1] (essentially a moment-integral approach of the type originated by Tani[2] and subsequently developed by Lees and Reeves[3]) to obtain approximate laminar boundary-layer skin-friction solutions with surface blowing. The present Note is a commentary upon this work with a two-fold purpose: 1) comparison with the findings of a previous investigation[4] not cited by Zien which further illuminates the relative accuracy and utility of various approximate solution methods; 2) presentation of additional results for the blowing problem regarding the behavior of the displacement thickness.

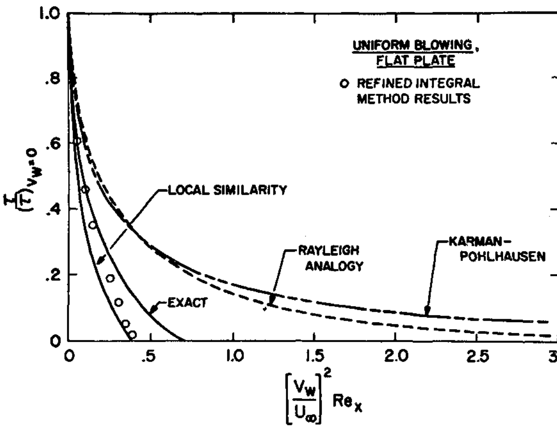

Fig. 1 Wall shear distribution.

In Ref. 4, skin-friction predictions were presented for non-similar flow along a flat plate with uniform blowing using a variety of approximate methods, including the Kârmân-Pohlhausen integral method with fourth-degree polynomial velocity profile, local similarity and the Rayleigh analogy. Figure 1 compares these findings with the newer results of Ref. 1 and the exact numerical solution of the problem.[5] Although this comparison further illustrates the general superiority of the refined integral method over the other approximations as regards skin friction, it is noteworthy that its accuracy does degrade considerably as the skin friction approaches zero (blow-off). Moreover, it is interesting to note that the well-known local similarity approximation also yields fairly good results in this example.

Whereas Zien presents results for skin friction only, it is also important to examine the corresponding blowing effect on displacement thickness growth, since this property is of great physical interest in the study of viscous-inviscid interaction[6] and boundary-layer separation[7] phenomena connected with the effects of surface mass transfer. This is particularly so in view of our previous findings[4] that approximations yielding satisfactory skin friction results do not necessarily do the same for the boundary-layer thickness parameters in the presence of blowing. Accordingly, we have calculated the $\delta^*(x)$ distribution pertaining to Zien's integral solution as well as to the various other aforementioned approximate solutions for uniform blowing; the results are compared with the exact behavior in

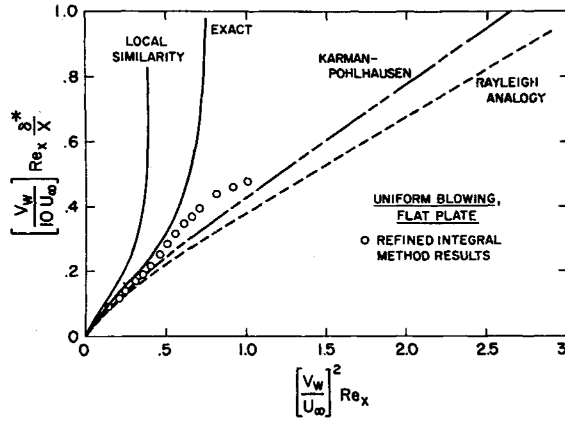

Fig. 2 Displacement thickness growth.

Received January 11, 1973.
Index category: Boundary Layers and Convection Heat Transfer—Laminar.

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Fig. 2. In marked contrast to the uniformly good results obtained for skin friction, the refined integral method predicts a displacement thickness distribution that diverges drastically from the rapid rise of the exact solution when the skin friction becomes small (incipient blow-off), tending in fact toward the linear behavior given by the ordinary Kármán-Pohlhausen and Rayleigh analogy approximations. Evidently, δ^* is quite sensitive to the skin-friction behavior near blow-off. It is also noteworthy from Fig. 2 that the local similarity approximation once again yields qualitatively correct results. Clearly, then, in spite of its generally good accuracy in predicting skin friction, the refined moment integral approach still possesses some deficiencies that would limit its usefulness in treating strong blowing and incipient separation problems where an accurate account of the displacement effects is essential.

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Reply by Author to G. R. Inger

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THE author wishes to thank Professor Inger for taking interest in the author's work.[1] His enthusiastic efforts in exploring the potential of the approximate method, currently being investigated by the author, are particularly appreciated. Stimulated by Inger's comments, the author feels that some further remarks about Ref. 1 are in order.

The nature and basic ideas of this approximate method along with its principal merits have been elucidated on several occasions in the *AIAA Journal* [see Ref. (2) in particular], and, therefore, require no further elaboration.

The merits of the present method are highlighted in Fig. 1 of the Comment which illustrates an interesting comparison on the results produced by a wide variety of approximate methods of different nature. In response to Inger's remarks on the obvious inadequacy of the method near blow-off, it is perhaps appropriate to reiterate here that the present method is only meant to be a refinement of the usual Kârmân-Pohlhausen (K-P) method. Being aware of the approximate nature of the method the present author certainly makes no claim as to its perfection. Obviously, Ref. 1 presented the method as a simple, practical tool for studying usual boundary-layer flows with surface mass transfer. In this form, it was never intended for studying the delicate and difficult problem of blow-off, separation or separated flows.

In light of the aforementioned discussion, Fig. 2 of the Comment provides further evidence to the accuracy of the present method. It is emphasized that due care must be exercised in observing the obvious region of its intended applications, i.e., $\varepsilon^2 Re_x < (\varepsilon^2 Re_x)_{\tau_w=0}$ (≈ 0.4 for the particular profile under discussion; $\varepsilon \equiv v_w/u_\infty$). Accordingly, Inger's calculation of δ^*, based on the solutions of Ref. (1), should have been limited to this region where the refined integral method [Ref. (1)] predicts positive skin friction. The importance of the effective displacement thickness in studying the problem of viscous-inviscid interaction in the presence of surface mass transfer has received considerable attention from most fluid dynamicists, including the present author[3] (unsteady, weak interactions). Interested readers are referred to Fannelop[4] and Li[5] among others, for a more thorough discussion on the subject and pertinent references. We only note that the conventional displacement thickness, δ^*, alone generally does not represent the total displacement effect. Modifications should be made to account for the effects of surface mass transfer.

In closing, the author is of the opinion that the merits of the refined K-P method, as illustrated and reiterated in Refs. 1 and 2, are best exploited when applications are made to the calculations of surface properties. The extension of the method to study problems of massive blowing, viscous interaction, or separated flows must proceed with caution.

Finally, the author would like to take this opportunity to correct a minor typographical error in Ref. 1. The suction strength in the caption of Fig. 4 should read $\varepsilon A^{1/2} = -1/(2)^{1/2}$.

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Received April 9, 1973. This research project is supported by NOL Independent Research Program.

Index category: Boundary Layers and Convective Heat Transfer—Laminar.

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Comment on "Finite Elements for Axisymmetric Solids under Arbitrary Loadings with Nodes on Origin"

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IN a Technical Note, Belytschko[1] outlines how a finite element analysis of axisymmetric solids using linear displacement triangular ring elements may correctly include nodes lying on the

Received February 20, 1973.

Index categories: Structural Static Analysis; Structural Dynamic Analysis.

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