It was erroneously mentioned that the results of Tables 1 and 2 of Ref. 1 are computed by the integrating matrix corresponding to third-order polynomial and differentiating matrix corresponding to fourth order. Actually, the results were computed by using the integrating matrix corresponding to fourth-order polynomial and the differentiating matrix was not used in the computations. The numerical example corresponding to the results of Tables 1 and 2 is a nonrotating blade, in which case the differentiating matrix is not necessary. The data for b_0 (semichord) are not required in the computation of the integrating matrix technique results of Tables 1 and 2.

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Comment on "Some Remarks on the Beck Problem"

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ALTHOUGH El Naschie[1] is researching in the area of the stability of nonconservative systems, he is apparently unware of the eminent text by Bolotin[2] first published in the English translation in 1961. Bolotin's treatise includes (as a first application of the dynamic criterion to a nonconservative-force, elastic-stability problem) the identical configuration, equation of motion, frequency equation, stability criterion, and critical value of the applied load presented by El Naschie in his Note.

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[2]Bolotin, V. V., *Nonconservative Problems of the Theory of Elastic Stability*, Pergamon Press Ltd., Oxford, 1961 (distributed by the Macmillan Company, New York), pp. 11-12.

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Reply by Author to J. Mayers

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I WOULD sincerely like to thank Prof. Mayers for taking the trouble of writing these comments, and his point is very well taken. At the same time, I would like to point out a few things which I feel are not of minor importance. Of course I am familiar with all of the papers published by Prof. Bolotin. However, I have read only the German translation of his book,[1] in which he treats the Beck problem on page 313. His treatment, however, differs completely from my Note, and his result, $P^c = 24.43\ EI/\rho^2$, on page 321 is about 20% larger than my result.[1,2] The most vital point, that the equilibrium method presented in Ref. 2 gives the exact answer for a class of problems including the Beck problem, was not mentioned at all[5]. I doubt that it was ever recognized by anyone except Ingerle. Since I am more fluent in the German language than in English, I did not consider it necessary to look at the English version, also translated in 1961, which Prof. Mayers refers to and which, unfortunately, is not available in our library in Riyadh. (I do know this book exists as I referred to it in Ref. 2.) Nevertheless, I think this is all besides the point because, and I would like to emphasize this, the purpose of my Note was to point out that the T.C.S.S. method[2] can yield the exact answer for a certain class of problem.[3-5] The analysis, which is very elementary, was included merely as a supplement to make the work self-contained and to encourage research in this direction.[3-7]

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Comment on "Shock Penetration and Lateral Pressure Gradient Effects on Transonic Viscous Interactions"

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THE first sentence in the Note in question[1] is as follows: "In existing interaction theories the impinging shock is usually imposed as a boundary-layer edge condition but its subsequent penetration into the layer and the corresponding lateral interaction-pressure gradient is neglected." The remainder of the Note consists of an attempt to adjust Inger's

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model of the interaction to account for the effects of shock penetration and lateral pressure gradient.

It is the purpose of this Comment to bring to the reader's attention the fact that in Refs. 2 and 3 (not referenced in Inger's Note) both of the effects in question are taken into account correctly in the first order as well as higher order approximations, i.e., in a systematic fashion. In addition, several authors (e.g., Refs. 4-6, not referenced in Inger's Note) have performed numerical computations in which these effects are included.

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Reply by Author to Adamson et al.

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DETAILED treatment of the incident shock penetration into a turbulent boundary layer considerably complicates the analysis of the attendant interaction problem[1]; an appraisal of its practical significance is therefore of interest to engineers. The unpublished Refs. 2, 3, and 5 cited in the Comment, while indeed including this penetration, do not give results showing its effect per se on the overall flow properties of physical interest; hence they do not compromise the main objectives of my Note to show explicitly this effect and its parametric dependence in the unseparated case. For transonic flows, the results indicate that the detailed shock penetration has in fact a negligible effect on the interactive pressure and skin friction at the very high Reynolds numbers required by the asymptotic limits used in the theories of Refs. 2 and 3 of the Comment, whereas at Reynolds numbers of practical interest ($10^6 \lesssim Re_L \lesssim 10^8$) the effect becomes significant (especially regarding C_f) and can be estimated conveniently by the approximate method given in the Note.

Regarding the $\partial p/\partial y$ effect across the interacting boundary layer, it is pointed out that the original interaction theory[2] includes this both upstream and downstream of the incident shock position, as clearly indicated by Figs. 2a and 2b of my Note (Fig. 3 also includes this); the *increment* of this effect due to shock penetration per se is also shown explicitly in Fig. 2.

Concerning Refs. 4 and 6 cited in the Comment, the former deals with laminar flow, while the latter is concerned with separated flow, both of which were specifically excluded from this Note. Moreover, it is noted that since the numerical solutions involved do not employ a triple-deck scaling on y and x in the local interaction zone and in the light of Werle and Bertke's experience,[1] it is not clear whether the shock penetration structure down to the sonic line in a turbulent boundary layer is in fact properly resolved by such codes in spite of their otherwise impressive global performance.[3]

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