agreement with this data. In Ref. 1, however, the discrepancy is empirically corrected by inserting a square root of the Prandtl number in R. If this dependence is correct, it can be verified for a monotonic gas (which is free of rotational and vibrational relaxation effects) by using, for instance, a mixture of He and Ar. (Reference 1 shows data for both He and Ar but, unfortunately, not for a binary mixture.) Table 2 shows the Prandtl number for such a mixture computed by formulas contained in Ref. 6. The change in Prandtl number with mole fraction appears to be adequate for the proposed verification.

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Reply by Authors to G. Emanuel

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WE appreciate Emanuel's favorable remarks but we are puzzled at his attempt to compare directly the result of Coles for a two-dimensional planar nozzle with that of Tang for the axisymmetric case. Surely it would be even more remarkable if there were no discrepancy between the A values of his Eqs, (4) and (5). Nor should it be surprising that Eq. (5), which relates to the axisymmetric nozzle, agrees better with our experimental data than does Eq. (4), which relates to the two-dimensional planar nozzle. Our data were obtained with axisymmetric nozzles.

With respect to the derivation of Kuluva and Hosack, we note that the pressure-gradient parameter β was not incorporated in their analysis. Thus, they did not assume any particular value. Our choice of the $\beta=\infty$ curve for comparison with the data stems from the fact that only for this case is a solution possible in closed form. Even more important, as pointed out in the paper,[1] the actual nozzle contours relate much more closely to $\beta=\infty$ than to other values. As we also pointed out, the theoretical C_D for $\beta=1$ is in much better agreement with the data than the C_D for $\beta=\infty$. Indeed, the agreement is even better than with the C_D values of Kuluva and Hosack, which, as Emanuel points out, do match the data better than our values of C_D for $\beta=\infty$. The point is that sheer consistency with experiment is not in itself a sufficient criterion for evaluating a theory. One can always force a better fit by empirical means of the kind used by Kuluva and Hosack in improving the fit to some of their data, or as we could in our case by arbitrarily picking the β value which gives the best fit. We felt that an empirical adjustment based on a modified Prandtl number was more defensible than choosing a β which was neither physically realistic nor analytically convenient.

In this connection, we find Emanuel's suggestion that measurements be made with mixtures of helium and argon in order to probe the true effect of Prandtl number interesting and constructive.

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Comment on "Unconstrained Variational Statements for Initial and Boundary-Value Problems"

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THE first observation is a question of semantics directed to Simkins (and others—he is probably in good company) concerning the use of the word "variational." If a functional exists for which the vanishing of the first variation leads to a physical law, then there exists a variational principle. If a functional does not exist, then it is meaningless to speak of a variational statement. In reality, the formulation presented in Ref. 1 is nothing more than the method of weighted residuals with weighting functions expressed in terms of the trial functions of the approximation—that is, a generalized Galerkin formulation. For this procedure, if the approximation function is denoted by $\bar{y}(t)=\Sigma a_n\phi_n(t)$, then the weighting function is selected to be $\psi(t)=\Sigma b_n\phi_n(t)$. Next, if b_n is denoted by δa_n, it is possible to write $\psi(t)=\delta\bar{y}(t)$; and the appearance of the symbol "δ" would appear to be the only justification for the term "variational statement." It is the contention of this writer that such a term is confusing, nonprecise, and really unnecessary. When there is no functional, and therefore nothing to vary, then be precise and identify the formulation as one of the methods of weighted residuals.

Much of Ref. 1 is involved with identification of the Lagrange multipliers through integration by parts. When doing this, there is an implication (certainly there is no explicit statement to the contrary) that the Lagrange multipliers so obtained are the only possible values. In other words, there appears to be an implication that it is necessary to eliminate the so-called redundant boundary terms through suitable definitions of the $\delta\lambda$. However, after, the procedure is identified as a method of weighted residuals, then it becomes

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