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Reply by Authors to A. N. Tifford

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IT is often true that qualitative conclusions may be obtained through judicious examination rather than detailed solutions of a mathematical problem. Where a problem involves many interdependent factors and is also nonlinear in character, some caution must be exercised in drawing sweeping conclusions. That such pitfalls are open even to those who are well versed in this art, and who have in addition the benefit of the hindsight offered by existing calculations, is evidenced by the examples treated in the preceding comment.

For instance, the preceding comment states that "since the expansion at the body shoulder increases the F^* levels there, the associated diffusivity is also increased, and a much more energetic dividing streamline is assured," a conclusion drawn from what must be considered a careful appraisal of the mathematical problem. However, the solution of the problem[1] indicates otherwise. The total enthalpy build-up along the dividing streamline is virtually unaffected by profile distortion caused by expansion. On the other hand, the dividing streamline velocity is sensitive to the expansion. It is clear that the increased diffusivity alone is not an adequate basis for prediction, since both the momentum and energy equations have identical diffusivities.

There is another, and perhaps overriding reason, why numerical calculations rather than order-of-magnitude estimates are appropriate for the prediction of observables from hypersonic wakes. In contrast to ordinary applications of heat transfer and fluid mechanics, observables are extremely sensitive to the detailed distribution of fluid properties. A 15% difference in neck enthalpy can produce an order-of-magnitude change in electron density.[2]

Some more or less obvious reasons for carrying out calculations can be added. One is data correlation. For this purpose it is desirable to have quantitative predictions based upon the best theoretical model available. Another use of the numerical results is for purposes of comparison with more approximate methods, such as moment methods. Finally, the experience gained with the numerical techniques has been helpful in further improving the flow model to include such features as the recirculating flow and viscous recompression, which have resisted analysis for many years.

In conclusion, we agree that extensive numerical calculations should be avoided whenever possible. More strongly, however, we feel that technical problems must be treated in a competent manner by whatever means are appropriate. In the present case the nature of the problem and the desired objectives make numerical analysis essential.

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Comment on "A Transient Solution of the Fokker-Planck Equation"

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IN a recent paper, Eaton[1] claimed that he obtained a complete transient solution of the distribution function for the Fokker-Planck equation when the collision frequency is velocity-independent, corresponding to the Maxwell law of interaction. He also mentioned an earlier paper by Osipov.[2] The purpose of this note is to point out the fact that Eaton failed to see that his complete solution can easily be obtained from a corresponding linear equation obtained by Osipov by a single step of integration, and all these complications caused by Eaton were unnecessary. The author also wants to point out that the nonlinear differential equation obtained by Eaton is actually due to his improper choice of variable.

Following Eaton's notation, the corresponding linear differential equation obtained by Osipov is

$$dT_e/dt = -2\nu m/M(T_e - T) \tag{1}$$

where ν is collision frequency, m is electron mass, M is molecular mass, T_e is electron temperature, and T is gas temperature. This equation is valid as long as T and ν are velocity-independent. Since this is a first-order linear equation, the solution can easily be obtained by direct integration by use of an integrating factor.[3] The result is

$$T_e = \exp(-\beta\textstyle\int\nu dt)\{\int\beta T\nu \times [\exp(\beta\int\nu dt')]dt + K\} \tag{2}$$

where K is an integration constant and $\beta = 2m/M$. This is essentially the complete solution obtained by Eaton. Eaton failed to understand this simple extension. In addition, by choosing an improper variable $A = m/2kT_e$, he obtained the following new nonlinear differential equation:

$$(M/m\nu)\ (dA/dt) + 4kT/mA^2 - 2A = 0 \tag{3}$$

He then solved the foregoing equation and claimed that he obtained a solution for the problem. Actually all these complications are not necessary. If one substitutes $A = m/2kT_e$ into Eq. (3), one can see that Eq. (1) obtained by Osipov is easily reproduced.

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