Reply by Authors to E. W. Miner and C. H. Lewis

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WE feel that the preceding comments of Messrs. Miner and Lewis on our Note[1] represent an inappropriate comparison of the results of two very different calculations. On the basis of their results for the shock standoff distance and the temperature distributions they claim that our results for the electron-density distributions are questionable. We find their claim difficult to believe because 1) our theoretical formulation and results, when specialized to the appropriate gas model, agree well with previous analyses, especially in terms of the shock standoff distance and the heat-transfer rates; and 2) favorable comparisons were obtained[2] between theory and flight experiment for the electron-density profiles over a significant altitude range. Moreover, we consider their comments to be premature and incomplete because a) they should have performed nonequilibrium-flow calculations including the atomic and diatomic ions (as we have done) before attempting to cast doubts on our results; b) they should have calculated and compared their results for a sphere cone with the results for a hyperboloidal[3,4] body because ideally the hyperboloid may be regarded as a blunted cone[5]; c) some details of their analysis are missing from their comment. For example, the technique that they used to extend Davis' method[3] to integrate smoothly the conservation equations across the sphere-cone junction where the body curvature undergoes a discontinuity is not discussed and they have treated only one cone angle at one altitude. Points 1 and 2 and our objections to their comments are amplified in the succeeding paragraphs.

Concerning point 1, in order to test the validity of our own analysis, which is applicable in the "incipient-merged layer" regime[5,6] and is based on Cheng's work, we did compare our results with previous results which used other methods of solution. These other methods include the finite-difference method which (if correctly applied) should yield more accurate distributions than the integral-method results. Specifically, comparisons were made for the heat-transfer rates and the shock standoff distances in the stagnation region for a perfect gas[7] and for a seven chemical species[8] model. These favorable comparisons were presented in Refs. 9 and 10.

Concerning point 2, we have demonstrated[2] that our theoretical results for the electron-density distributions in the plasma layer at the downstream location (the only location for which profile distributions were measured) agree well over a significant altitude range with those measured in flight using electrostatic probes that protruded into the plasma layer. Specifically, good agreement was obtained for a 9° cone at 233,000 ft alt where the electron-density distribution calculated at the downstream location was relatively insensitive to the inclusion of N^+, O^+, $N_2{}^+$, and $O_2{}^+$ in the chemical model. However, the nose region distributions for the 9° cone as well as those for the 20° cone shown in our Ref. 1 were greatly affected by the inclusion of these ions. Also, favorable comparison was demonstrated[2] between our downstream results (which were sensitive to the inclusion of the O^+, N^+, $N_2{}^+$ and $O_2{}^+$ ions) and the flight data at 275,000 ft alt for a 9° cone, suggesting the importance of these atomic and diatomic ions in considering the nonequilibrium, viscous flow at high altitudes. In performing these nonequilibrium calculations, we have used reaction rate coefficients that have been experimentally determined for the most part (see Ref. 2 for a discussion of the chemical model).

Concerning the work of Miner and Lewis, they present in Fig. 1 the standoff distances along a 9° sphere cone obtained from an inviscid analysis and from a fully viscous shock-layer analysis for a perfect gas and for a binary mixture. They purport to show sizable discrepancy between our results and their own, which they obtained from "extensions of the methods of Davis[3,4] for fully viscous shock-layer flows over nonanalytic bodies such as sphere cones...." We feel that they should have elaborated on the technique that they used to extend Davis' method to integrate smoothly the conservation equations across the sphere-cone junction where the body curvature undergoes a discontinuity. Since the commentators claim to have extended Davis' treatment to the sphere-cone case, we wish to ask: Have they calculated and compared their results for a sphere cone with the results for a hyperboloidal body (since, as Cheng states,[5] "ideally the hyperboloid may be regarded as a blunted cone")? This seems to be a rather important comparison because the results of our own analysis agree with Davis' results, lending credence to the validity of our approach. Specifically, our analysis has been applied, for a perfect gas model, to the flow over a 20° hyperboloid and also to the 20° sphere-cone case. The results for the shock standoff distance at 30 nose radii downstream agree well with each other and with Davis' result[4] (see also Fig. 1 of our Note[1]).

To continue, we refer to Ref. 4 where Davis presents distributions of shock standoff distance along 60°, 45°, and 20° hyperboloidal bodies at various altitudes. We observe from Davis' figure for the 20° case an already sizable magnitude of the shock standoff distance at 30 nose radii downstream and note that the standoff distance will be even greater at 90 nose radii downstream. Also, judging from the results for the 45° and the 60° cases, for the 9°-cone case of present interest the standoff distance is larger than for the 20° case. It seems unlikely that the standoff distances will suddenly suffer such a considerable decrease for the 9° case as the commentators appear to claim, e.g., only 2.5 to 3 nose radii at 90 nose radii downstream, even granting the potential influence of a pressure overexpansion and subsequent recovery in the vicinity of the sphere-cone junction. We feel they should have performed calculations for more than just one cone angle and for only one altitude and until they do we cannot accept their criticism as valid.

In Figs. 2 and 3 of Miner and Lewis, they present, respectively, the distributions of the temperature behind the shock and the temperature profiles at 90 nose radii downstream. They claim to show sizable differences between their results and ours. The temperature profiles in our analysis are expressed in terms of the resultant values for the momentum conservation, the total-enthalpy (energy) and the species conservations, as given in Eq. (18) of Ref. 11, and thus are intimately coupled to the various chemical-species levels. We do not see great inconsistency in our results, especially when the change in the shock inclination angle is already so small that the effect of this change, if any, would not be large. In fact, going along with their argument, we are rather surprised at the seemingly sensitive change in their shock temperature (their Fig. 2) when their shock angle changes so slowly.

In regard to their Fig. 3, where they display the temperature profiles, here again the crux of the matter appears to be tied to the magnitude of the shock standoff distance. One of our main points in our Note was that the maximum temperature point was "moving" toward the body, suggesting viscous-heating effects in the layer close to the body surface. Therefore, a more important result to notice is not so much the "absolute" location of this maximum temperature point in terms of the nose radius, but rather this location in terms of the fraction of the shock-layer thickness. We note that the maximum temperature point from their analyses is located at roughly a quarter of the shock-standoff distance away from the body, while our results show that this point is at about a third of the shock-standoff

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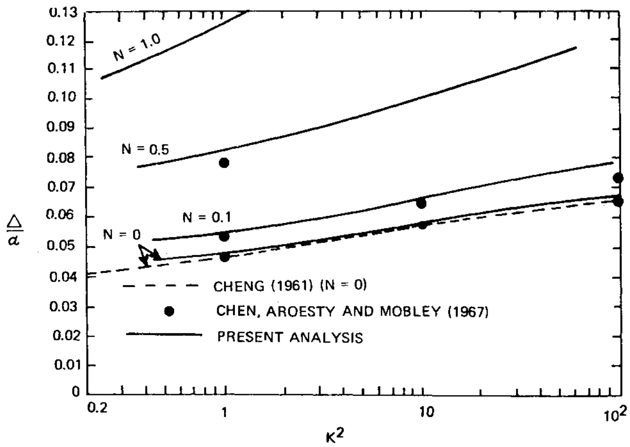

Fig. 1 The shock standoff distance for the axisymmetric stagnation point for various mass-injection rates.

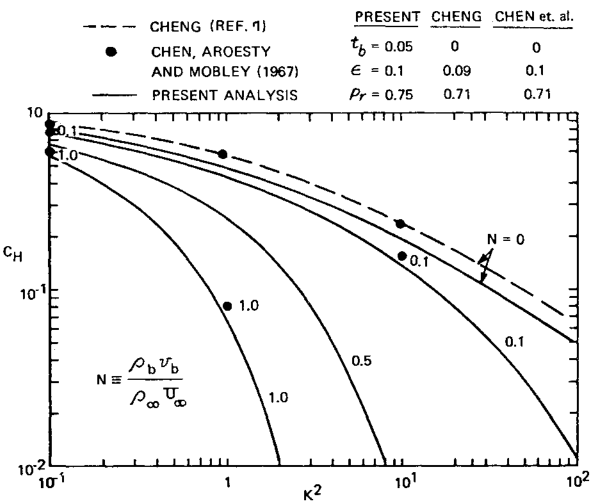

Fig. 2 Heat-transfer rates at an axisymmetric stagnation point at low Reynolds number for various mass-injection rates.

distance from the body surface. We therefore feel that the comparison of their Fig. 3 is basically related to the shock-standoff distances given in their Fig. 1. In addition, we take issue with their sentence (next to the last) in which they state: "The present results strongly indicate that differences as large as those between the present results and the results of Kang and Dunn could not be due to differences in gas chemistry." In their Fig. 3, they show the temperature profiles for a binary gas and a seven-species gas. We observe a rather large temperature difference between their two results, the maximum of the binary gas being about 5200°K while that of the seven-species gas is only about 3700°K. Nevertheless, if they imply (apparently) that these two profiles may be directly compared, despite the additional fact that one is a boundary-layer result and the other is a viscous-layer result, then we do not understand how they can claim that the influence of the gas model on flow properties is small in view of the demonstrated sizable temperature discrepancy among their own results. On the other hand, if they say that their own two profiles are not directly comparable, then we ask why they show these comparisons in the first place?

In Table 1 and Fig. 4, they show heat-transfer rate results from both their analyses and our treatment. With respect to the heat-transfer rates that they present, we can only comment that our results for the heat-transfer rates have compared favorably with other analyses. The figures that we presented in Ref. 7 in connection with the effects of blowing for viscous rarefied flows are included here (as Figs 1 and 2) for the sake of completeness. Comparing our results with those of Cheng[7] and Chen, Aroesty and Mobley,[12] we note that our values are 20% to 30% lower than theirs but we do not observe large differences such as a factor of three, as the commentators show in Table 1.

In summary, we feel that the comments of Miner and Lewis are premature on the basis of the work that they have presented.

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