Technical Comments

Comment on "Hypersonic Ionizing Air Viscous Shock-Layer Flows over Sphere Cones"

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THIS Comment has been prepared to present a different interpretation of the relative merits of the approximate solutions obtained by Miner and Lewis[1] and those which we published (along with W. L. Jones) in February 1973.[2] In the January 1974 issue of the *AIAA Journal*,[3,4] we exchanged technical comments on Refs. 2 and 5. In that exchange and in this one, Miner and Lewis present much of the same results and figures. Additional results in the present Miner and Lewis paper[7] are a comparison with the laboratory experiments of Pappas and Lee[6] and a comparison with one of the in-flight measured electron-density distributions from the Langley RAM C program. Our comments in this issue will therefore be devoted mainly to the more relevant aspect of the problem; i.e., analysis of ionized flows.

We have three basic reservations concerning the paper of Miner and Lewis and these are: 1) incomplete and inappropriate comparisons with existing flight data and with the results of other analyses, 2) questionable applicability of their formulation to the ionized, viscous shock-layer flows, 3) the data of Pappas and Lee do not seem to support the claims made by Miner and Lewis on the validity of their analysis. In the following paragraphs we will discuss each of these points in detail.

Point 1

As part of the Langley RAM C program, electron-density distributions were measured in the plasma layer using electrostatic probes and these results have been widely reported in the literature. Surface heat-transfer rates and local static pressures were not measured and thus are not available for comparison of calculation technique accuracy. However, the electrostatic probe data were collected at high altitudes where the shock-layer could be considered to be viscous and where ablation products were absent so that the flowfield chemical kinetics could be treated as impurity-free air for which reaction rate coefficients are relatively well known (see Ref. 2 for discussion and other references). It is our opinion that the RAM C flight data described above are quality measurements. As such, these data represent a standard to which the accuracy of various calculation techniques should be compared.

Figure 1 is taken from our 1973 paper[2] and it illustrates the comparison of the calculated electron densities with those measured on the RAM C-II and C-III flight experiments for altitudes from 275,000 ft to 233,000 ft. The uncertainty in the flight data obtained with the constant-voltage probes is indicated by the cross hatching and the vertical bars. The results of our nonequilibrium, viscous shock-layer calculations are indicated on Fig. 1 by the solid lines. Flight measurements were obtained out to 7 cm from the body for RAM C-II and out to 14 cm for RAM C-III. The agreement between the calculated electron densities and the flight data is reasonably good over that portion of the shock-layer for which data were obtained. Our technique predicted an upswing near the shock at this particular location on the body, but flight data were not obtained far enough from the body for us to either confirm or negate this upswing. A possible physical interpretation for this upswing was given in Ref. 2.

In Figs. 6 and 7 of Miner and Lewis they compare their calculated electron densities to ours, those of Evans et al.,[7] and the flight data[2,8] taken at approximately 233,000 ft alt. Evans et al.[7,8] acknowledge that this altitude is the limit of their boundary-layer and outer inviscid-flow analysis. Further, they[7] have argued that the outermost probes at this particular altitude were influenced by probe heating, but they indicate that the influence of this heating on the inferred electron densities is difficult to assess. However, probe heating is not a problem for the flight data obtained for altitudes of 250,000 ft, 265,000 ft, and 275,000 ft, as acknowledged by all concerned. Thus the plasma-layer electron-density profile as measured should be accurate.

In their paper, Miner and Lewis state that "the principal emphasis of their work was not on predicting electron concentrations but rather was on predicting the hypersonic, viscous flowfield over spherically blunted cones with electron concentration as a part of the flowfield predictions." They use this comment to excuse them from comparing their predictions with the high-altitude data noted above, and yet they proceed to use the flight data at the lower altitude (230,000 ft) in an attempt to illustrate some agreement between their results and the boundary-layer results of Evans et al.[7] We object to the incomplete and misleading nature of such a comparison. It is our belief that comparison of predictions with all of the flight data in the viscous-flow regime is the necessary requirement for ascertaining the relative merits of viscous-flow analyses.

Concerning the comparisons of our results to those of Miner and Lewis that are presented in Figs. 2, 3, 5, and 8 of their paper, these are essentially a repetition of those included in the January 1974 Technical Comment.[3] Our response[4] followed their Comment and will not be repeated here with one exception. In Table 1, Miner and Lewis show that our stagnation-point

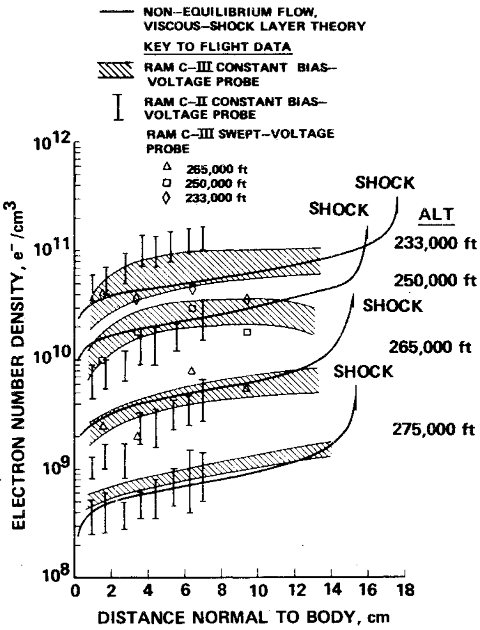

Fig. 1 Comparisons between calculated electron density and in-flight electrostatic probe measurements.

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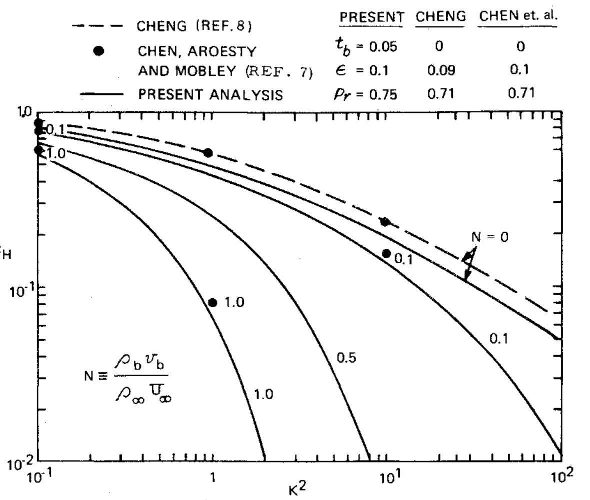

Fig. 2 Heat-transfer rates at an axisymmetric stagnation point at low Reynolds number for various mass-injection rates, and their comparisons with exact (numerical) solutions.

heat-transfer result is a factor of two less than their prediction. In the January 1974 exchange, Kang illustrated that his stagnation-point heat-transfer rate in terms of the Stanton number, shown in Fig. 2 of this Comment, agreed to within 20 or 30 % of the value calculated by Chen, Aroesty, and Mobley[9] and Cheng.[10] To our knowledge no one has challenged the accuracy of the calculation techniques presented in Refs. 9 and 10. Miner and Lewis claim to have compared their stagnation point heat-transfer results favorably with those of Davis[11] who in turn compared favorably with Cheng. We do not understand the contradictory results but we have checked our calculations and we remain convinced that the results presented in Fig. 2 are correct.†

Miner and Lewis comment in their discussion titled *RAM C Re-Entry Case* that, "It was also observed in Ref. 20 that the method of Kang and Dunn overpredicted the N_e values measured by the microwave reflectometers on the RAM C-II at the more forward body stations ($S = 0.8$ and 2.1) by factors as large as 20". They go on to comment that this disagreement raises questions about the results given by Kang. Unfortunately, this comment is only partially accurate. There are three important points that need to be made in order to present a complete statement of the situation. First, we made this observation ourselves in Ref. 2 (p. 147) which was published at least five months prior to the publication of their Ref. 20 and thus there was no attempt by us to hide the disagreement. Secondly, the correct observation concerning this point is taken from page 147 of our paper[2] and reads as follows: "Reflectometer measurements were also performed in the nose region at X/R_N of about 0.9 and just after the hemisphere-cone junction at 2.1. However the agreement between theoretical prediction and flight data was not nearly as good for these locations as it was for the two downstream stations. In the nose region, the calculated peak-number density was about 20 times greater than the reflectometer data, and at X/R_N of 2.1 the calculated peak value was about ten times greater than the reflectometer data." Thirdly, microwave reflectometer data were also obtained at $S/R_N = 5.2$ and $S/R_N = 7.65$, and we compared our results to these measurements in Ref. 2. We have included these comparisons as Figs. 3 and 4 in order to illustrate the agreement between our predicted electron density profile and the peak value of electron density inferred from the reflectometer flight data for altitudes from 233,000 ft to 275,000 ft.

Also in the section titled, *RAM C Re-Entry Case*, Miner and Lewis made the statement: "As mentioned above the emphasis of the present work was on predicting the hypersonic viscous

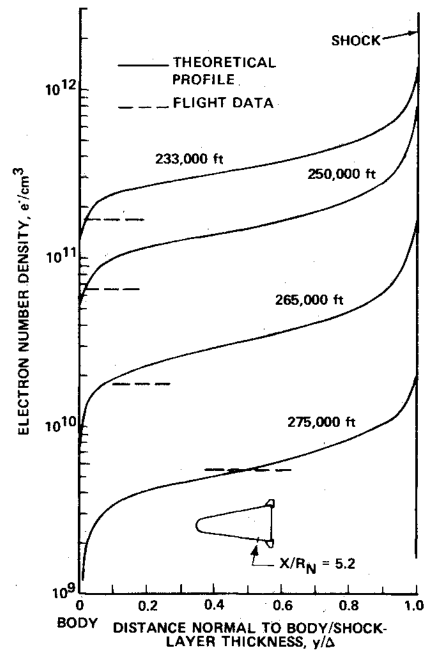

Fig. 3 Comparison of predicted electron density profile with peak value from reflectometer flight data at $X/R_N = 5.2$.

flowfield over spherically blunted cones with electron concentration profiles only a part of the flowfield predictions. Since electron concentration profiles are subject to changes in reaction rate constants as well as changes in temperature profiles, mean flowfield quantities such as heat-transfer distributions would be a more reliable measure of method accuracy." We do not agree with the spirit of this comment for a host of reasons. First, for the problem under discussion in our paper, we were interested in communication problems which of necessity must consider free electron-density levels. Secondly, for the viscous-flow environment of interest we are dealing with a clean air chemistry for which the rate coefficients are relatively well known. Thirdly,

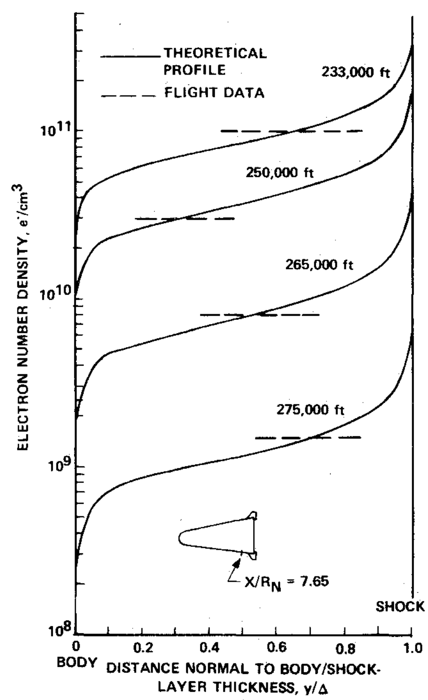

Fig. 4 Comparison of predicted electron-density profile with peak value from reflectometer flight data at $X/R_N = 7.65$.

† Cheng[20] also calculated the chemical nonequilibrium case with much the same results.

quality flight data are available for the flight regime of interest to both us and Miner and Lewis, and these data comprise electron-density measurements and not heat-transfer measurements. Fourthly, there is so little energy involved in the ionization process that any indication of the flowfield ionization would be lost in the noise of a surface heat-transfer measurement. Finally, we do not believe that a surface measurement is the best way to infer properties of a shock layer.

We summarize point 1 by stating that Miner and Lewis have not demonstrated that their results are valid in the ionized, viscous-flow regime. Rather, the manner with which they present the comparisons for only a lower altitude is to us misleading and raises questions as to the appropriateness of their approach for the ionized, viscous flows as specified in the title of their paper.

Point 2

Concerning the applicability of their formulation to ionized, viscous shock-layer flows, we feel that Miner and Lewis have not considered the merits of recent advances on the subject and thus have failed to put their limited analysis in proper perspective. Of the recent advances made in the analysis of ionizing viscous hypersonic flows, two aspects are worthy of specific mention because of their relevance to the formulation used by Miner and Lewis. These are: 1) use of the second-order boundary-layer equations, and 2) use of the two-layer model.

Before entering into a detailed discussion of this comment, we wish to note that at the time when we performed our analysis (1971), we used an approximate integral method because it was the most expedient way to meet our immediate requirement for reasonably good predictions of electron-density distributions in the RAM shock layer, and at that time the assumptions and the flow regime for which our analysis was intended were explicitly stated.[2,12,13] Even though our analysis provides good results, as demonstrated by the comparisons presented in our Ref. 2, and is inexpensive to run on a digital computer, we would not recommend using any simplified formulation to calculate ionizing flowfields in 1974 in view of the significant recent advances, to be discussed below.

As pointed out by Jain and Adimurthy[14,15] the second-order boundary-layer equations fail to predict the detailed structure of the flow and the results are quantitatively far from the exact solution at low Reynolds numbers. Furthermore, the experimental results of Ahouse[16] in the same flow regime appear to bear out the conclusions of Jain and Adimurthy. Thus, we believe extreme caution is required in applying any refined boundary-layer equations to obtain solutions for viscous-flow problems. Miner and Lewis seem to make much of their results at 233 kft altitude but say nothing about their calculations for higher alt. They mention the "good" agreement of their results at that altitude with those of Evans et al.[7,8] based on the boundary-layer theory. Apart from the controversy over the probe-heating effects at 233 kft alt, such a comparison seems to reinforce the claim made by Evans et al.[7] that the boundary-layer theory is applicable at that altitude. Remembering that the equations used by Miner and Lewis are essentially higher-order boundary-layer equations, it is not surprising that their calculations agree with the boundary-layer results. (We wish to add that there is no dispute on our part with respect to the efficacy of the numerical method used by Lewis (Refs. 6, and 12–14 of their paper) when applied to boundary-layer flows). There thus appears to exist a somewhat paradoxical situation: If, as they imply, the boundary-layer theory as applied by Evans et al. is reasonable at 233 kft alt, then they have only demonstrated that the flow regime at that particular altitude may be a boundary-layer regime and not a fully viscous regime as would be the case at higher altitudes. It should be made clear that any "favorable" comparison in the boundary-layer regime does not validate their analysis for viscous, ionized flows, because the fully viscous flow should not be considered to be merely an extrapolation of a boundary-layer flow (see Refs. 14–18). Conversely, if the boundary-layer theory is not applicable at 233 kft alt, then we question the utility of such comparisons with the boundary-layer results. We emphasize that a comparison with the results of a boundary-layer theory does not establish the validity of a viscous-flow formulation.

Because of the limitations of the higher-order boundary-layer equations for viscous hypersonic flows at high altitudes, the more physically accurate Navier-Stokes equations should be used for analysis in these rarefied flow regimes. This in fact has been done in recent years, by Li[17] and by Widhopf and Victoria.[18] For example, Widhopf and Victoria employed the full Navier-Stokes equations along with the nonequilibrium, multicomponent chemical-species conservation equations and quite successfully obtained results for the flow properties surrounding a blunted body. Their results also display the differences in the flow structures between the higher-order boundary-layer equations and the Navier-Stokes equations, with increasing differences with increased rarefaction, i.e., at higher altitudes. Miner and Lewis state in their paper, regarding the results of Jain and Adimurthy, that "the elliptic nature of the (Navier-Stokes) equations, at least in the physical coordinates, increases the complexity of the solution procedure and restricts the application of the methods in the downstream direction." We believe that the crux of the question is whether or not the equations are physically valid in the flow regimes of interest, rather than the secondary question of whether these valid equations are complicated. Concerning the treatment of the elliptic nature of the Navier-Stokes equations, we wish to note that Miner and Lewis are already treating their own equations in a similar way by performing several iterations on the flowfield, resulting in considerably increased computation times and complexity of the solution method.

Although the two-layer model used by Miner and Lewis for distinguishing the shock-transition zone from the shock layer is an attractive assumption, it is basically an artificial, greatly simplified concept. It appears to us that Miner and Lewis are attempting to improve the accuracy with which the ionized, viscous shock-layer flow can be calculated, by employing an implicit, finite-difference method. With this understanding in mind, we question why at this point in time (in (1974) they would apply such a refined, time-consuming numerical-integration scheme with the two-layer approximation as a building block. This seems to be an important question especially when one considers the computation time of 35 min and 45 sec on an IBM 370/158 for a seven-species viscous-layer calculation (Table II of their paper). In summary of Point 2, recent advances raise serious questions concerning the general applicability of the approximations (two-layer model, second-order equations) used by Miner and Lewis in their formulation of the ionized, viscous flow analysis.

Point 3

Aside from the two basic questions discussed above concerning the validity of their formulation and results, we wish to address ourselves to the comparison between the Miner and Lewis results and the test data of Pappas and Lee.[6] Figures 9–11 in their paper purport to display validity of their present theory. We have a somewhat different interpretation of these comparisons. These seem to show, if anything, that the extended boundary-layer theory of Lewis, Adams, and Gilley[19] agrees even better with the experimental results of Pappas and Lee. Miner and Lewis themselves acknowledge that "the present theory did not compare as well with experimental data as did the previous boundary-layer theory ...." As the title of their paper indicates, their analysis is supposed to afford calculation of viscous flows over *sphere-cones* and their treatment of the surface-curvature discontinuity seems to be the only new meaningful feature added to the formulation of Davis.[11] If their analysis is indeed valid over sphere-cones as claimed by Miner and Lewis, it would seem to follow that their results should have compared favorably with the test data of Pappas and Lee, whereas by contrast the comparisons display poor agreement over most of the body. They attempt to explain this discrepancy between their results and the Pappas and Lee data by stating "the sphere-cone considered by Pappas and Lee[6] ended at $S \sim 5$, and almost all of this body was within the length affected by the discontinuity in surface curvature." If this is the case, then it appears to demonstrate more than anything else the shortcoming of their present theoretical treatment of the sphere-cone junction. In fact, we are rather surprised that they did not modify their treatment of the curvature effect in an attempt to ascertain whether or not their present formulation can indeed accommodate realistic calculations of viscous flows over sphere-cones. Also lacking is any discussion on why they believe that their results would be any more accurate and reliable for longer sphere-cones than for the intermediate-sized sphere-cone used by Pappas and Lee. In spite of such poor agreement with the data, Miner and Lewis unjustifiably claim in their abstract that "The predictions of the present method agreed well with the experimental data," which include those of Pappas and Lee.[6] We believe such statements to be misleading.

To recapitulate, the laboratory data of Pappas and Lee do not substantiate the claimed validity of the analysis of Miner and Lewis on the basis of the comparisons that they presented. Rather, it appears to illustrate once again the desirability of a more complete treatment of the viscous-flow problem by application of the full Navier-Stokes equations.

In closing, it is our considered judgment that Miner and Lewis have not demonstrated the correctness of their ionized, viscous-flow analysis. Therefore, we reiterate that comparison of their theoretical results with the RAM C flight data at higher altitudes (250 kft, 265 kft, 275 kft) is one of the compelling prerequisite tasks which will afford the readers an opportunity to assess the validity of the claims made by Miner and Lewis.

References

[1] Miner, E. W. and Lewis, C. H., "Hypersonic Ionizing Air Viscous Shock Layer Flows over Sphere-Cones," *AIAA Journal*, Vol. 13, No. 1, Jan. 1975, pp. 80–87.

[2] Kang, S. W., Jones, W. L., and Dunn, M. G., "Theoretical and Measured Electron-Density Distributions at High Altitudes," *AIAA Journal*, Vol. 11, No. 2, Feb. 1973, pp. 141–149.

[3] Miner, E. W. and Lewis, C. H., "Comment on 'Hypersonic, Viscous Shock Layer Nonequilibrium for Spherically Blunted Cones," *AIAA Journal*, Vol. 12, No. 1, Jan. 1974, pp. 124–126.

[4] Kang, S.-W. and Dunn, M. G., "Reply by Authors to E. W. Miner and C. H. Lewis," *AIAA Journal*, Vol. 12, No. 1, Jan. 1974, pp. 127–128.

[5] Kang, S.-W. and Dunn, M. G., "Hypersonic, Viscous Shock Layer with Chemical Nonequilibrium for Spherically Blunted Cones," *AIAA Journal*, Vol. 10, No. 10, Oct. 1972, pp. 1361–1362.

[6] Pappas, C. C. and Lee, G., "Heat-Transfer and Pressure on a Hypersonic Blunt Cone with Mass Addition," *AIAA Journal*, Vol. 8, No. 5, May 1970, pp. 954–956.

[7] Evans, J. S., Schexnayder, C. J., and Huber, P. W., "Boundary-Layer Electron Profiles for High-Altitude Entry of a Blunt Slender Body," *AIAA Journal*, Vol. 11, No. 10, Oct. 1973, pp. 1371–1372.

[8] Evans, J. S., Schexnayder, C. J., and Huber, P. W., "Boundary-Layer Electron Profiles for Entry of a Blunt Slender Body at High Altitudes," TN D-7332, July 1973, NASA.

[9] Chen, S. Y., Aroesty, J., and Mobley, R., "The Hypersonic Viscous Shock Layer with Mass Transfer," *International Journal of Heat and Mass Transfer*, Vol. 10, 1967, pp. 1143–1158.

[10] Cheng, H. K., "Hypersonic Shock-Layer Theory of the Stagnation Region at Low Reynolds Number," *Proceedings of the Heat Transfer and Fluid Mechanics Institute*, 1961, pp. 161–175.

[11] Davis, R. T., "Hypersonic Flow of a Chemical Reacting Binary Mixture Past a Blunt Body," AIAA Paper 70-805, Los Angeles, Calif., 1970.

[12] Kang, S. W., "Hypersonic Low Reynolds Number Flow over a Blunt Body with Mass Injection," *AIAA Journal*, Vol. 7, No. 8, Aug. 1969, pp. 1546–1552.

[13] Kang, S. W., "Nonequilibrium, Ionized, Hypersonic Flow over a Blunt Body at Low Reynolds Number," *AIAA Journal*, Vol. 8, No. 7, July 1970, pp. 1263–1270.

[14] Jain, A. C. and Adimurthy, V., "Hypersonic Merged Stagnation Shock Layers, Part I; Adiabatic Wall Case," *AIAA Journal*, Vol. 12, No. 3, March 1974, pp. 342–347.

[15] Jain, A. C. and Adimurthy, V., "Hypersonic Merged Stagnation Shock Layers, Part II: Cold Wall Case," *AIAA Journal*, Vol. 12, No. 3, March, 1974, pp. 348–354.

[16] Ahouse, D., "An Experimental Flow Field Study of the Rarefied Blunt Body Problem," *Astronautica Acta*, Vol. 15, 1970, pp. 149–159.

[17] Li, C. P., "Time-Dependent Solutions of Nonequilibrium Airflow Past a Blunt Body," *Journal of Spacecraft and Rockets*, Vol. 9, No. 8, Aug. 1972, pp. 571–572.

[18] Widhopf, G., and Victoria, K., "On the Solution of the Unsteady Navier-Stokes Equations Including Multicomponent Finite-Rate Chemistry," presented at the Symposium on Computers to Fluid Dynamic Analysis and Design, Polytechnic Institute of Brooklyn, Farmingdale, New York, 3–4, Jan. 1973. Also *Computers and Fluids*, Vol. 1, 1973, pp. 159–184.

[19] Lewis, C. H., Adams, J. C., and Gilley, G. E., "Effects of Mass Transfer and Chemical Nonequilibrium on Slender Blunted Cone Pressure and Heat Transfer Distributions at $M = 13.2$," AEDC TR-68-214, Dec. 1968, Arnold Engineering Development Center, Tullahoma, Tenn.

[20] Cheng, H. K., "The Blunt-Body Problem in Hypersonic Flow at Low Reynolds Number," Rept. AF 1285-A-10, June 1963, Cornell Aeronautical Lab., Buffalo, N.Y.

Reply by Authors to S.-W. Kang and M. G. Dunn

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In the preceding Comment, Kang and Dunn consistently avoided the central issues which were raised in our present paper.[1] The first issue is: "Given two viscous shock-layer methods, both of which consider thin viscous shock-layer flows, both of which stem from the same governing conservation equations—one set pseudo first-order accurate and the other set second-order accurate; both using methods of solution which are generally recognized as valid—one an approximate integral method and the second an exact finite-difference method, how could they give such radically different results?" The second issue raised by the comparison of results shown in our present paper can be simply stated as: "Is the method of Kang and Dunn appropriate to viscous-layer flows?"

In the preceding Comment it was acknowledged there were some differences between the predictions of the two methods at the stagnation point, but the far larger differences in the heat-transfer distributions were totally ignored. Also ignored were the differences in the shock-temperature distributions, shock-layer thickness distributions, and temperature profiles. None of the three points made in that Comment addressed these issues.

In the development of the method of Kang and Dunn, it appears that the validation of the accuracy of their method was almost totally dependent upon the comparisons with the RAM C electron concentration profiles. In fact, under their point 1, it is claimed that the electron concentration profile data for the RAM C flights represent a standard to which the

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