Int. J. Heat Mass Transfer. Vol. 9, pp. 597–598. Pergamon Press 1966. Printed in Great Britain

LETTER TO THE EDITORS

COMMENTS ON THE PAPER "SEPARATION OF A GAS MIXTURE IN CURVED SUPERSONIC FLOW"

(Received 27 January 1966)

THE recent paper by A. Kogan [1] reports an impressive attempt to demonstrate diffusive separation of species in a supersonic flow field with highly curved streamlines. This study differs from previous investigations in that a Prandtl–Meyer expansion was used instead of an axisymmetric free jet. I would like to make a few comments for readers to keep in mind as they consider the implications of this very interesting set of experiments.

(1) The design Reynolds number for the "nozzle" was 1600. Under many of the conditions actually used, the Reynolds number was less than 10 per cent of the design value. Thus, one wonders whether the actual flow field was always as conceived. The relatively low operating Reynolds numbers imply the possibility of viscous effects near the corner as well as at the concave wall. Moreover, the relatively high background pressures imply strong viscous effects at the free edge of the jet even quite close to the corner. The relatively low pressure ratios also make one wonder whether the indicated Mach numbers could have been achieved in some of the experiments.

(2) The total pressure traverses seem to be consistent with the flow field as conceived but they should be interpreted with caution. As the author points out, an observed total pressure can be converted to a Mach number only if there has been no viscous dissipation between the plenum chamber and the probe. More important, perhaps, is whether viscous effects at the probe itself may have distorted the results. Without calibration it would be difficult to estimate the magnitude of an appropriate correction for the particular probe shape used. The effective probe Reynolds number for the data of Fig. 5 could have been as low as 30 or less which is well within the region where corrections can become substantial [2].

(3) A questions arises as to whether the composition of the gas "seen" by the pressure gauges is the same as that entering the probe. The author states that the suction through the probe is maintained by an oil diffusion pump capable of producing a high vacuum. The point is that if the flow into the probe is convective but out of the probe line into the vacuum pump is anywhere effusive or free molecular, then the gas in the line could be richer in the heavy species because the light species becomes preferentially pumped. There is not enough information on dimensions and pressure levels in the sampling line to decide whether at least part of the richness of sampled gas in heavy species could be due to this effect.

(4) The problem of whether the gas entering the probe has the same composition as the free stream has been dealt with at some length by the author. In a very ingenious way he has attempted to correct for the substantial separation effect due to the probe itself by assuming that enrichment in heavy species which occurs at large distances from the corner is due only to the probe effect and that this probe effect is constant all along the characteristic line so that it can simply be subtracted from readings obtained near the corner. There are grounds for uneasiness in these assumptions. In the first place, except for the region very close to the wall, the enrichment apparently due to the probe is very much larger than that apparently due to diffusion in the stream. One is inclined to be cautious in the amount of significance he attaches to results which depend upon small differences between big numbers. In the second place the presumption that there should be more diffusive separation near the corner than further out in the stream seems somewhat dubious. To be sure, an elemental volume of gas which follows a streamline near the corner is subjected to much larger gradients than one which follows a streamline far away from the corner. But the elemental volume near the corner is exposed to its gradient for a much shorter time than the one far away. The product of gradient and exposure time, which should be the factor governing the extent of diffusive separation, would seem to be about the same all along the characteristic line. Nor can one assert that the gradients near the edge of the jet (i.e. close to the corner) are enhanced by assuming that the potential for diffusion in the background is negligible, especially at the high background pressures employed in the subject experiments. The possibility of viscous effects in the jet, pointed out above, also casts doubt on the constancy of probe separation along this characteristic. In this connection it is noteworthy that where the alleged diffusive separation is apparently large, in Fig. 7, for example, the probe tip was within 10 or 20 free-stream mean-free paths of the corner. It would be remarkable if diffusive exchange of momentum with the wall at and near the corner did not extend over this distance.

There are fundamental difficulties in any attempt to use material probes to measure the extent of diffusive separation of species in any flow with gradients. These emerge very clearly from the results of Kogan's careful investigation and his closing remarks. Sherman's recent analysis showed that, at Reynolds numbers high enough to inspire confidence in the characterization of the flow field, the probe effect is much larger than any effect due to diffusion in the free stream [3]. At Reynolds numbers low enough to permit extensive separation by diffusion the nature of the flow and its interaction with the probe becomes highly uncertain. This dilemma is an inevitable consequence of the similarity between the diffusion of momentum (viscosity) and species. It seems likely that immaterial probes such as light and electron beams offer the most hope for obtaining quantitative data on species diffusion effects in high gradient flows because they permit *in situ* determination of composition far away from *any* surface.

REFERENCES

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3. F. S. Sherman, *Physics Fluids* **8**, 773 (1965).

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AUTHOR'S REPLY

While I fully agree with Professor Fenn's suggestion that immaterial probes, such as electron beams or light, should be in principle more suitable for obtaining unequivocal quantitative data on diffusion of species in high gradient flows, I must disagree with some of his comments concerning interpretation of the experimental results presented in the paper.

The convergent (subsonic) part of the nozzle was bounded by four plane surfaces at small inclinations to the throat section. With such geometry one would expect, by continuous flow theory, zero boundary-layer thickness at the throat [1].

It should be clear that the measured background pressure represents a stagnation pressure. For a ratio of stagnation pressures p_{02}/p_{01} of the order 0·1, a monoatomic gas should reach Mach numbers in excess of 5·5 before supersonic flow breakdown. Thus the available pressure ratio was adequate for attainment of supersonic flow at $M = 3$ and beyond.

An experimental check on my assertion that, up to the relatively low Mach numbers of this investigation, the boundary-layer displacement thickness is quite small can be inferred from Fig. 5. A build-up of a thick boundary layer should lead to a corresponding lag in Mach number development in the central part of the flow, yet the values of M in Fig. 5 are at most about 5 per cent below their nominal value.

There is no doubt that the composition "seen" by the pressure gauge is not the same as that entering the probe, nor as that in front of it. Moreover, there is no reason to expect that the probe effect should be constant all along the survey line. That is why the process of extrapolation at both ends of the experimental curve of Fig. 7 has been adopted in an effort to evaluate correctly the probe effect. From many data similar to those shown in Fig. 7, it was apparent that appreciable variation of probe effect could have occurred only over a relatively narrow region. The procedure adopted for correcting the data for probe effect should therefore be satisfactory.

REFERENCE

1. L. Rosenhead (editor), *Laminar Boundary Layers*. Clarendon Press, Oxford (1963).

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