DISCUSSION

R. C. Ahlert

The calculations reported in this paper appear to be consistent and accurately presented. However, I fail to see the purpose behind Mr. Potter's paper. There is no apparent contribution to Thermodynamics, Engineering Science, or Process Design. The scope is that of a term paper for a graduate thermodynamics course and demonstrates the author's ability to apply an equation of state to calculation of a secondary thermodynamic function.

The author's conclusion that Joule-Thomson coefficients "can be computed from each of the principle sources of equations with equal confidence," is not new. The literature has reported that equations of state, representing one thermodynamic or equilibrium parameter well, can be expected to describe others with nearly equal accuracy. Considerable effort has gone into the development of the equations of state for steam and water reported in Mr. Potter's references 6, 7, and 9. I am surprised that any doubts would persist that these relationships are not thoroughly satisfactory bases for the calculation of thermodynamic properties. Further, reporting Joule-Thomson coefficients to four significant figures goes beyond reasonable need or interest. The percent deviations reported in Table 8, carried to three decimal places, are irrelevant.

The author's conclusion that measurements of Joule-Thomson coefficients of compressed liquids are yet to be made is quite valid. He has had to go to some length to find an experimental basis for comparison with his calculations. I am not sure that the error in conversion of data from the experiment cited to Joule-Thomson coefficients is equal to or greater than the disagreement reported in Table 8. I note that there is no reference to the possible use of the information generated. Is it perhaps that the Joule-Thomson coefficient of compressed liquid is of no practical value in design? This may have limited generation of laboratory data and literature.

I would delight in seeing a comprehensive paper on the Joule-Thomson coefficient of the condensed state. This could be a real contribution to the thermodynamic literature. The present paper appears to me to be a fragment without a purpose.

P. G. Hill

Professor Potter has presented an interesting selection of Joule-Thomson Coefficients for compressed liquid water as calculated from two general equations of state in relation to particular experimental data. He has shown in Table 7 that there is remarkably little difference in the values of the Joule-Thomson coefficient obtained from the Zaworski and Keenan formulation and those obtained from either the Keyses, Keenan, Hill, and Moore equation or the Meyer, McClintock and Silvestri equation, considering reasonable experimental uncertainties, particularly in regions where the Joule-Thomson coefficient is small.

Considering that formulation of the single equation of state was not based on any direct experimental input of the Joule-Thomson data, it is indeed gratifying that a degree of agreement between the Joule-Thomson calculated in this way and that produced by the four-part equation of the state of Meyer, et al. An interesting comparison of data from the Keenan et al. equation with the experimental data of Harper shown in Table 8. Here the variations do not seem to show a simple pattern and underline the need for further experimental data on the Joule-Thomson coefficient.

It would be of value if, from his studies of the Joule-Thomson coefficient in the superheat region as well as the compressed liquid region, Professor Potter would provide some advice on where most attention is needed experimentally in the determination of new values of the Joule-Thomson coefficient.

Authors' Closure

The authors wish to thank Professors Bhaduri and Smetana for their comments on this paper. It should have been noted in the paper that temperature measurements have been made both upstream and downstream of the test section. In all cases no measurable temperature gradient existed. Also, all data are at Reynolds numbers, $\rho u d / \mu$, less than 35.0. The minimum pressure ratio across the throttling valve was 100. Since this is far in excess of that needed to choke the flow it is apparent that the flow rate was indeed constant. A more detailed discussion of the experimental procedure has been given by Wilkerson.

The authors believe that the questions raised in the first four items of Professor Smetana discussion have been adequately answered in previous publications [2, 3, 4, 5, 6]. The last item is worthy of note and the authors agree that pressure ratio is a parameter of importance. In this paper the experimental data represents pressure ratios across the test section between 66 and 373. No attempt was made to document pressure ratio effects beyond this limited range where no significant effect was noted. Future investigations will consider in depth the pressure ratio aspect.

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DISCUSSION

S. Bhaduri

The authors are to be congratulated for the extension of Sparrow and Ebert's solution for the slip flow in a rectangular duct and inclusion of the "self-diffusion" effect due to concentration gradient in their analysis. The theoretical result for the total specific molecular flow rate has been derived in a straightforward manner. The agreement of the experimental results with the theoretical prediction for the mass flow rate, as shown in Fig. 3, is surprisingly excellent in view of the complications usually encountered in experimental studies of rarefied gas flows.

The analysis considers isothermal steady flow, whereas the brief description of the experimental procedures given in the paper indicates a strong possibility that the flow was not steady nor isothermal. In rarefied gas flows where density and pressure change in time and space, the associated change in temperature is unavoidable. The authors did not indicate whether there was any discernible temperature gradient along the length of the tube. If, however, there was an appreciable change of temperature in the flow direction, then the noncontinuum slip boundary conditions, equations (2a) and (2b), are no longer valid for experimental setup. The effect of temperature gradient in the direction of flow plays an important role in modifying the slip velocities $w(x, h)$ and $w(y, y)$ at the walls. The additional "thermal creep" term, containing the temperature gradient which needs to be considered along with the usual velocity gradient term for isothermal cases, is of considerable importance particularly for many vacuum instrumentation problems since it can induce a pressure variation along the tube even when there is negligible net flow. A quantitative evaluation of this term, if possible, from the collected data would certainly strengthen experimental findings.

The description of the experimental procedure is too brief. It would have been helpful if the authors would have indicated a little more elaborate details of the techniques used in collecting and processing the data.

The authors have used Weber's solution for slip flow in a circular tube and replaced the radius of the circular tube, $r$, by the hydraulic radius $r_p$. The hydraulic radius, defined by equation (12), leads to the equivalence between the side of a square and the radius of a circle equal to the side of the square. This simplification of the flow geometry drastically and implies that the corners of the square duct will have negligible effects on the flow. This type of approach could give some "trend" only. Instead of providing a simple "ball park" model on the basis of hydraulic radius, the authors could have developed an approximate dimensionless empirical equation from the experimental results. I believe that the inclusion of the hydraulic radius technique has weakened the quality of an excellent paper.

F. O. Smetana

Several cautions should be mentioned regarding the work presented in this paper:
1. There is really no such thing as gas flow described by equation (1) of the paper. If, however, the pressure ratio across the test section is approximately unity, and if the system is essentially isothermal, then the solution of equation (1) is a fairly accurate representation of the actual gas velocity.
2. Equation (1) is also incorrect for flows with very large Reynolds numbers unless $L/D \rightarrow \infty$.

Author's Closure

Professor Ahlert's remarks appear to be intemperate and jejune. Also he misspelled "Joule-Thomson" six times in the course of his review.

His objection that the Joule-Thomson values were reported to too many significant figures is valid in terms of the general usage for such data. On the other hand, as a machine computation based upon an equation of state having constants with at least twice as many significant figures, little damage appears to have been done. There is less justification for reporting percentages beyond three significant figures.

The author agrees on the desirability for a study of Joule-Thomson data in the condensed state.

Professor Hill's comments are especially valuable, as he spent several years on the team that developed the single equation of state. As the instrumentation becomes available it is to be hoped that new values will be determined in the liquid region, and in the superheat region beyond the Kleinschmidt temperature and pressure ranges.