Table 1 Raman cross sections and relative intensities

Gas	$\frac{\sigma_{Rayleigh}}{\sigma_{Raman\ total}}$	Raman rotational cross section cm²/sterad-molecule	
		Peak line	All lines
O_2	22	27×10^{-31}	32×10^{-30}
N_2	64	8.8×10^{-31}	13.2×10^{-30}

case verified the virtually complete absence of any other gas in the scattering region. In addition, there was practically no dust present in the jet to affect the Rayleigh amplitude.

These cross section values were checked by measuring the ratio of the Raman scattering, in a spherical cell, from air and from the benzene rotational line at 992 cm^{-1}. The total air cross section was then calculated using the absolute benzene Raman cross section from Ref. 5. The result was a total cross section for air of approximately 21×10^{-30} cm^2/sterad/molecule, within 25% of the results from the N_2 and O_2 jets. The jet data are considered more reliable, with an estimated error of at most 50%. The magnitude of photomultiplier current observed during the experiments is consistent with these values. More extensive cross-sectional data for several gases by a different group will be published soon.[6]

With these data available it is possible to estimate the power required for time resolution of density in a jet. Use reasonably obtainable values as follows: $\eta_Q = 10\%$, $\eta_S = 20\%$, f:2 collecting lens ($\Omega = \pi$ 16), $l_0 = 0.1$ cm, and a useful air Raman scattering cross section σ of about 9.0×10^{-30} cm^2/sterad/molecule, using all Stokes lines and both polarized and depolarized components of scattered light.

Suppose a minimum time interval of 1 msec is sought. Then for a count rate n of, say, 10^3/msec from the PMT cathode ($n/n^{1/2} \doteq 30$) at 4880 Å and a minimum air density of 1/5 atm in the jet ($N \doteq 0.5 \times 10^{19}$ molecules/cm^3) the power of scattered light collected by the lens would need to be $P = h\nu n \times 10^3 \doteq 200 \times 10^{-6}$ ergs/sec. The required laser power P_L would be of the order of $P_L = P/Nl_0\sigma\Omega \doteq 23$ w.

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Technical Comments

Comment on "Separating Self-Similar Laminar Boundary Layers"

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RECENTLY Wortman and Mills[1] commented on the methods used and the solutions obtained by previous authors for the compressible laminar boundary-layer equations.[2–5] They stated that no adequate justification for the use of highly simplified physical property models such as used in Refs. 2–5 has been given. In response to this criticism, a quotation from the introduction to Ref. 6 appears appropriate: "Even though self-similar boundary-layer solutions correspond to a rather limited class of physical flows with engineering significance, such idealized models of actual flows may be used to gain some insight into the characteristics of more complex boundary layer flows;" and "The self-similar solutions are the first approximation to the complete solution of the flow. In the general case where the self-similarity requirements are not even approximated by the flow field, the self-similar solutions are lower order approximations to the actual flows...;" and finally, "these solutions exhibit most of the characteristics and the influence of the physical, dynamic and thermal parameters of the actual problems. Therefore, self-similar solutions may be used to study the influence of these parameters on the behavior of boundary-layer flows without introducing the unnecessary complications of non-similar solutions...." Although these statements are used to justify self-similar solutions in general, analogous arguments can be advanced to justify the use of simplified physical property models in self-similar solutions. Further, solutions with highly simplified physical properties can be used to determine the accuracy of more complicated solutions. Finally, such solutions may be used to show the physical effects associated with more complicated solutions. References 1 and 6 use the last argument to good effect. In particular, Wortman[6] compares his calculations with those of Dewey and Gross[7] and of Cohen and Reshotko[8] in order to check the accuracy and validity of his algorithm. In the paper under discussion, Wortman and Mills[1] use the results of Rogers[2] to further justify a claim of fourplace accuracy.

Wortman and Mills[1] state that the calculation methods used in Refs. 2–5 are relatively primitive, and could be made to converge only because highly simplifying physical assumptions were made. However, the method used in Refs. 2 and 3 will converge when $\omega \neq 1$, $Pr \neq 1$. Further, the works of Smith and Clutter[9] and Smith and Jaffe[10] show that solutions can also be obtained for real air. In addition, Sparrow, Quack, and Boerner[11] have shown that the method is applicable to nonsimilar flows. The relaxation of the simplifying assumptions only complicates the necessary algorithm. Wortman and Mills also imply that the solution method used in Refs. 1 and 6 does not suffer from the difficulties associated with the methods used in Refs. 2–5. However, a perusal of Ref. 6 reveals that although somewhat more mathematically elegant, it in fact does suffer from similar difficulties—namely, selection of the finite value of $\eta = \eta_{max}$ used to represent the edge of the boundary layer, determination of an acceptable region within which to select

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initial estimates of the unknown surface gradients, the effect of step size on the accuracy of the results, etc.

In referring to the results shown in Fig. 2 of Ref. 1, Wortman and Mills remark that solutions generated with simplified gas properties can exhibit incorrect trends—namely, the separation value of β generally increases with the Mach number parameter E for gases with realistic values of ω, whereas the converse is true for $\omega = 1.0$. However, a careful analysis of the results shown in Fig. 2 of Ref. 1 reveals that these "trends" are more likely attributable to mass transfer at the surface or real gas effects. In particular, Fig. 2 of Ref. 1 reveals that for no mass transfer at the surface, the separation value of β is essentially not a function of E, and that the values for $\omega = 0.5$, $Pr = 0.723$ and $\omega = 1.0$, $Pr = 1.0$ are quite close. In fact, it appears from the results shown in Fig. 2 of Ref. 1 that they may be correlated using an inverse square or cube root of the Prandtl number.

Wortman and Mills[1] also state that the arguments over accuracy entered into by Rogers[3] are quite academic, since the solutions resemble physical reality so poorly. Here Wortman and Mills[1] have misinterpreted the purpose of the accuracy arguments of Refs. 2 and 3. These arguments are directed towards establishing the numerical accuracy of the solutions and not the correspondence between the mathematical model and the physics of the problem. These are two separate questions. In the opinion of the present author, the accuracy of numerical solutions should always be established. Further, numerical solutions should be obtained and published to the highest practical accuracy in order to eliminate the necessity for subsequent investigators to repeat the calculations. Because the limitations of similar solutions in representing physical reality are generally well known, they are not normally discussed by current investigators. However, it should be noted that sufficient justification for investigating similar solutions, even with simplified physical property models, has been presented above.

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Reply by Authors to D. F. Rogers

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IN reply to Professor Rogers we offer the following rebuttal. 1) In Ref. 1 we simply argued that calculations of the separation pressure gradient parameter β using degenerate fluid properties were of doubtful value owing to an absence of a prescription for applying such results to real flows. Rogers' defense of self-similar calculations in general, seems irrelevant.

2) In Fig. 2 of Ref. 1 the zero suction values of β for real air show a variation of about 50% in the range of E considered, and for model air about 10%. Yet Rogers states that the figure reveals β to be "essentially not a function of E," demonstrating a difference in his and our thinking regarding the level of an acceptable approximation.

3) Rogers misrepresents Ref. 6 in asserting that the calculation method developed therein suffers from problems of choice of a finite η_{max}, integration step-size, and initial profile estimation. That the converse is true can be seen in Table 2-1 and Fig. 2.1 which summarizes the results of an extensive study and demonstrated the method to be insensitive to these factors.

4) We agree with Rogers that the accuracy of calculation methods should be carefully established, but find it difficult to be concerned with the 6th decimal place, and are of the opinion that such exercises should not be the main purpose of journal publications.

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