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DISCUSSION

I. E. Beckwith[2]

In general, the subject paper presents a good review of theory and previous experiments, however, the Voytovich[3] paper referred to was not included in the original review. Since this reference paper also presented data on the effect of nozzle shape, as well as different contraction ratios, the authors of the subject paper should comment on any similarities or differences in the two sets of results. In particular, further discussion would be useful on the effects of initial turbulence levels and/or high noise levels, which were apparently the cause of large increases in the u^2 component of turbulence in the Klein and Ramjee [8] experiments and which may have also caused similar increases in the Voytovich paper. These increases are not in agreement with predictions from linear theory or with the data in the subject paper.

With regard to the effect of contraction on mean flow nonuniformities, it should be noted that with the one additional assumption of $V_1/V_2 = A_2/A_1$, which is consistent with the assumptions already used for equation (1), the right-hand side of this equation may be written as $(1 + V_1/U_1)/(c^2 + V_1/U_1)$. This result then shows directly the effect of $V_1/U_1 \neq 1.0$ on mean flow deviations.

Some of the discussion in the subject paper regarding upstream effects of nozzle shape as shown in Figs. 8, 9, and 13 could be clarified if x was normalized with D_e rather than L_c, since L_c is different for different nozzles.

This reviewer cannot agree with statements in the subject report (see page 62, on *a*) *Initial flow condition* and page 67, the last two sentences under DISCUSSION) to the effect that the boundary layer data included in the paper would be useful to predictors for calibrating their methods. The reason these data are *not* useful for this purpose is that large effects of upstream local separation and reattachment are present in the data at the exit (see Fig. 15 and the author's discussion), particularlyfor the HB and LB nozzles. Such statements may apply to pr diction methods based on the Navier-Stokes equations but certainly not to boundary layer methods.

One final question concerning the effect on turbulence of an extension pipe placed at the exit of the nozzle CE (see item 2, on page 66 of subject paper). Do the authors have any data on the ratio of v to u components at the end of the extension? The possible effects of an extension (analogous to a wind tunnel test section), on the return of the turbulence to isotropy are perhaps of more importance to wind tunnel engineers than the effect on turbulence level.

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[3]Voytovich, L. N., "The Influence of Nozzle Contraction on the Damping of Turbulent Pulsations," NASA TT F-15, 724, July 1974.

V. W. Goldschmidt[4]

The contribution of Professor Hussain and Mr. Ramjee is indeed welcome. In their work they present the detailed measurement of the turbulent velocity characteristics of flow through different contractions, all with the same aspect ratio. Their results are primarily experimental although they do compare them with existing and published theoreies. The comparison is not satisfactory and as the authors correctly indicate a nonlinear theory is now needed. It would have been interesting to have asked the authors to extend on that suggestion and see what alternate paths they might propose for the derivation of a satisfactory theory.

Inasmuch as their results are primarily experimental by nature, the reader would benefit from receiving an estimate of how accurate and how dependable their results are. As an example, as we turn to Fig. 15, we would find that $u'_e/U_{ce} = 0.0058$. Similarly, from Fig. 6 we find that $u'_i/U_i = 0.0764$. Now as we turn to Fig. 10 we find that at the exit of the nozzle where $c = 11.1$, $u_{c_e}{}^2/u_{c_i}{}^2 = 0.458$, hence, $u'_{c_e}/u'_{c_i} = 0.677$. Now for flow that is essentially uniform, $U_{c_e}/U_{c_i} = 11.1$. Hence, if we take the results of Fig. 10 we would find that $(U'_{c_e}/U_{c_e})/(U'_i/U_{c_i})$ should be equal to 0.0609. However, combining the results of Figs. 5 and 6 we find that ratio to be 0.076. A difference in the order of 20 percent. This may not be too bad but it might best be quantified for the benefit of the reader. The authors might want to clarify what the outer diameter of the horizontal face of the nozzle is at the exit. The size of that horizontal face-plate will influence the entrainment pattern into the jet. This, in turn, can influence the conditions in the mouth of the jet.

The authors' conclusion Number 10 that the nozzle shape has a noticeable influence on the entrainment rate in the near field of jets, should be better quantified. The authors' referred to preliminary measurements were apparently taken at only 10 diameters from the exit. It is conceivable that the apparent increase in the entrainment rate might be instead due to a relocation of the virtual origin of the jet. I have no basis for suggesting this other than similar measurements that were tkaen in a plane turbulent jet. These results, measured by Flora, were published as a technical note in the *AIAA Journal*, Vol. 7, No. 12, Dec. 1969, pp. 2343–2346. In his work 10 different nozzles with different configurations but the same length gave no effective change in the spreading rate, in the decay rate, or in the virtual origins. In the experiment now reported by Hussain and Ramjee, the effective nozzle length is changed at the same time as its geometry. This could lead to an apparent change of the virtual origin which could be erroneously interpreted as a change in the entrainment rate.

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From this discusser's point of view he finds the data very interesting and worthy of further study, attempting to derive a theory to explain it.

As a final observation, the presented results further confirm to this duscusser that while designing contraction sections for wind tunnels and nozzles for jets, it really doesn't make much difference how we bend the nozzle in order to get the flow that we want at the end. We are probably just as well off with the pragmatic approach of reference [6], when trying to select an ultimate design.

P. Bradshaw[5]

The authors are to be congratulated on exposing the deficiencies of the popular formulae for contraction effects. It would be helpful to have more details of the weakness of the inequality (6*b*) in the present experiment. Fig. 19 indicates $l \simeq 2$ cm, so $l/L \sim 0.15$ while the initial u'/U (Fig. 6) is at least half this. It is therefore not surprising that rapid-distortion theory fails. Fig. 2 shows that the configuration is representative of medium-quality test rigs but the integral scale deduced from Fig. 19 seems rather small. In high-performance wind tunnels (where reduction of turbulence is most important) the intensity upstream of the contraction will be smaller, and the small-scale turbulence may already have been eliminated by screens. In this case, rapid-distortion theory may be more useful in predicting the effect of the contraction on the remaining (large-scale) turbulence.

Authors' Closure

The authors are grateful to Professors Bradshaw and Goldschmidt and Mr. Beckwith for submitting their elegant discussions; their competence in this subject is well established.

It is interesting to note from Professor Goldschmidt's discussion that one can read three digit values from the scattered data in our figures. Turbulence intensity data in the precontraction region have large uncertainty due to low mean velocity. The uncertainty of the data within the nozzles, which constitute the concern of the paper, is much smaller. Our conclusion on the effect of nozzle shape on jets was based on, as indicated, detailed measurements over the region $0 \leq x/D_e \leq 10$. Our near field circular jet observations do not appear to be related to the far-field pitot data of Flora and Goldschmidt in a plane jet.

The effects of initial boundary layer mean and turbulence characteristics on the near fields of both plane and circular jets have been found to be significant, as to be expected, in a number of hot-wire based investigations in our laboratory. From the results of the current paper it is clear that the nozzle shape can affect the initial boundary layer characteristics and thus, obviously, the near field jet flow mean and turbulence characteristics.

The authors especially appreciate Mr. Peter Bradshaw's penetrating comments. Subsequent data show that $(u'/U)_i$ and $(l/L)_i$ are typically of the same order of magnitude so that the inequality (6*b*) is not satisfied. We deliberately did not endeavor to suppress the precontraction turbulence intensity so that the effect of contraction on turbulence could be measured clearly when the turbulence intensities are large. In a high performance tunnel, the precontraction turbulence scales can be large enough to satisfy condition (6*b*). However, as indicated in the text, the longitudinal stretching in a large contraction may produce small enough scales that viscous effects cannot be ignored. Consideration of viscous effects would increase the discrepancy between the theoretical prediction of the longitudinal turbulence intensity and the data.

Regarding the comments by Mr. Beckwith, the paper by Vytovitch was not available in open literature until recently. Detailed comparison with our data has been included in a later manuscript by us on the effect of contraction *ratio* on turbulence (accepted by the Journal of Fluids Engineering). For brevity, we will not include the comparisons here. Figs. 8, 9, and 13 were initially plotted with the abscissa as x/D_e but the abscissa was changed to x/L on the demand of a referee. Attachment of a $3D_e$ pipe at the end of the nozzles produced no noticeable return to isotropy. We agree with Mr. Beckwith that these data will be useful to only those predictors who calim that their methods permit prediction through a separated and reattached boundary layer.

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