Technical Comments

Comment on: "A Unified Analysis of Gaseous Jet Penetration"

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A RECENT discussion of the Zukoski-Spaid model [1-4] for the flowfield created by injection of gas into a supersonic primary flow by Billig, et al.[5] is a gross misinterpretation of that model. The aim of the Spaid and Zukoski model was to develop a characteristic length which could be used to predict the influence of a various parameters on the scale of the disturbance produced by secondary injection, the absolute value of which is of no consequence. This aim is clearly stated. For example, in Refs. 1–4 (which were quoted in Ref. 5 as Refs. 1–4), the following paragraphs appear: "It is proposed that the radius h can be used as a measure of the scale of the disturbance produced by injection. Note that, although the expression given in Eq. (3) contains no adjustable constants, the exact correspondance between values calculated from Eq. (3) and any measured feature of the flow, such as the penetration height, is purely fortuitous. However, it is to be expected that changes in scale of flow features will be proportional to changes in h."

In addition, when discussing the concentration profiles, Refs. 3 and 4 contain the following, with similar statements in Refs. 1 and 2: "... It is obvious that the observed and calculated penetration height corresponds much more closely to the line of maximum concentration than to the outer edge of the injectant stream. ... Farther downstream, mixing is slower. From this result, it is obvious that the no-mixing approximation made in the model can only be useful close to the injector. However, h is a measure of the scale of the injectant flow, and hence it is reasonable to expect that it would be the characteristic dimension for the mixing process too." Despite these statements, Billig et al. chose to interpret the characteristic scale as a physical length (e.g., Figs. 14 and 15 of Ref. 5), which is certainly unwarranted, and as expected gives invalid results.

The model of Spaid and Zukoski is based on the assumption that the injectant expands isentropically to the ambient static pressure at the downstream face of the control volume. This fixes the momentum flux from the control volume, and combination of this result with a Newtonian drag assumption allows the characteristic height to be found.

A region of strong recirculation exists immediately downstream of the injectant jet, and therefore an unknown fraction of the control volume exit area A_c is occupied by this stagnant flow and is not available for injectant flow. Billig et al. have ignored the existence of this region. Hence, contrary to the statement made in Ref. 5, a continuity argument connecting mass flow and control volume cross sectional area A_c could not be and was not used. Specific attention was drawn to this fact. For example, in Refs. 1 and 2, the statement is made, "Note that the injectant is allowed to expand to the static pressure of the undisturbed flow, but is not assumed to completely fill the semicircular cross section of the downstream face of the quarter sphere which forms the nose of the equivalent solid body." Similar statements were made in Refs. 3 and 4, and a figure in Ref. 4 was used to illustrate this point. The model leads to a limiting value of freestream Mach number, depending upon the particular injectant conditions, above which the computed area available for flow is insufficient for the passage of the injectant. This limitation is normally not encountered in practical applications. It is primarily a result of the crude assumption of isentropic expansion and turning of the jet. It is well known that the trends predicted by such a model are quite insensitive to the of the assumed turning process, as long as the no-mixing assumption is retained (see Fig. 3, Ref. 5). As a result, this difficulty can easily be overcome without changing the basic concept or the resulting scaling relationships.

Finally, the relatively simple model developed by Spaid and Zukoski accurately scales a wide range of experimental results concerning sonic injection normal to a wall. The model taken in its proper sense is in good agreement with the data presented in Refs. 1–4, with data presented by a number of other authors such as Chrans and Collins,[6] and even with the data presented by Billig et al. in their Figs. 14, 15, and 17.

Received August 13, 1971; revision received March 27, 1972.
Index category: Jets, Wakes, and Viscid-Inviscid Flow Interactions.
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Reply by Authors to E. E. Zukoski and F. W. Spaid

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IN the discussion in Ref. 1 of the mathematical model used by Zukoski and Spaid,[2] the point was not that they employed a continuity agreement (which they did not) but rather that a model which does not violate continuity is to be preferred. The new unified model presented in Ref. 1 not only takes into account the presence of a Mach disk in the stucture of an underexpanded

Received March 27, 1972.
Index category: Jets, Wakes, and Viscid-Inviscid Flow Interactions.
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jet, (which the Zukoski and Spaid model does not) but also holds to the conservation equations. This model is more complex than that of Zukoski and Spaid, and it does require empirical knowledge of the location of the mean height of the Mach disk (y_1), but it is pointed out[1] that y_1 for transverse jets into supersonic streams appears to be well correlated with y_1 for jets discharging into quiescent atmospheres by means of an "effective back pressure" concept, so that the necessary empirical knowledge is, in fact, available for the pressure ratio range of interest. Thus, a model that provides a much more appropriate physical picture of the jet structure and shape, and does not violate continuity, is available and should be preferred by anyone studying jet trajectories.

It is also pointed out in Ref. 1 that anyone wanting a simpler model for predicting either the jet area ratio A_c/A_j (Fig. 3 in Ref. 1) or the location of the outer edge of the Mach disk $\bar{y}$ (discussion of Fig. 17 in Ref. 10 of our article) would do better to use the Newtonian drag model above than the hybrid model of Zukoski and Spaid, and no violation of continuity would arise.

In any case, the authors deemed it important to disclose the fact that the model of Zukoski and Spaid does not preserve continuity, (and therefore, is not consistent with respect to approaches usually taken in fluid mechanics), so that others wanting a model for prediction of jet penetration would be aware of that fact. As the authors noted,[1] the model of Zukoski and Spaid[2] consists of a flow which is initially defined as one-dimensional with all properties given, expanding isentropically, with no mixing, to a downstream station which is again one-dimensional, with flow direction defined and one other property defined, viz. pressure. With these assumptions all properties at the downstream station are also defined by use of the conservation equations, which, of course, include an expression for the momentum flux, which in itself requires the conservation of mass. Moreover, this momentum flux is unique to isentropic flow and for the case under consideration requires a turning force that is not, in general, equal to that which would result from a Newtonian pressure distribution. The cross-sectional area of the flow at the downstream station is also defined; however, the cross-sectional shape and location with respect to the primary stream are not constrained. In Ref. 2, however, this complete definition of flow provided by the isentropic assumption is disregarded and instead, the momentum flux required to turn the isentropically expanding jet is equated to the force of a Newtonian pressure distribution on another body, thus eliminating the possibility of defining a unique control volume.

In their Comment, Zukoski and Spaid also fault the authors for comparing their model with experimental results ("... Billig, et al. chose to interpret a characteristic scale as a physical length e.g., Figs. 14 and 15 of Ref. 5, which is certainly unwarranted, and as expected, gives invalid results"). However, after disclaiming any exact correspondence between features of the flow and their scaling parameter "... the exact correspondence between values calculated from Eq. (3) and any measured feature of the flow, such as penetration height, is purely fortuitous,"[2] they then contradict this disclaimer by concluding that "The agreement between experiment and theory is good over the whole pressure ratio studied, and the dependence on specific heat ratio, molecular weight, and Mach number is correctly predicted."[2] Of course, the whole point of developing analytical models is to be able to correlate experimental results based on some insight into the physical phenomena involved. The authors assert that the model of Ref. 1 provides not only a better representation of the physical phenomena but also better correlation in terms of jet shape (trajectory and representative downstream crossections).

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Comments on "Symptomatic Behavior of an Electric Arc with Superimposed Flow"

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THE interest paper by Wutzke, Pfender, and Eckert[1] deals with the experimental and theoretical study on a transition of a d.c. longitudinally blown arc from a stable regime with a fixed anode spot to a regime of an arc end moving in a gas flow when continuous discharge is maintained by repeated breakdowns of a near-electrode gas layer. Theoretically the stability of an arc anode spot with stable discharge is explained by the balance in the near-electrode region between the electromagnetic force which is caused by current interaction with a self-magnetic field and aerodynamic force. The experimental data are correlated by the Reynolds number, in which the length of a radial arc section from an arc column surface to an electrode wall is taken as a scale dimension.

To our point of view, there is some contradiction in such approach. The Reynolds number shows the interaction between the inertia forces of a gas flow and friction forces. The electromagnetic force which is the basis of theoretical consideration was not at all taken into account when correlating experimental data.

The interaction of electromagnetic and aerodynamic forces for the case under investigation may be described in Ref. 2

$$\Pi = (\rho v^2 d^2/4\mu_0 I^2)^{1/2}$$

where ρ is the gas density at $T = 300°K$ and appropriate pressure, v is the velocity, d is the characteristic dimension, I is the current, μ_0 is the magnetic constant.

This number is proportional to the inverse value of the aerodynamic drag coefficient. Probably, when correlating the experimental data it is advisable to consider the number Π as a function of the Reynolds number or of any other numbers which play an essential role in the phenomenon considered. The value of the number Π for the data presented in Ref. 1 varies from 0.6 to 4.0, that is already not bad. The best correlation is given by the dependence of Π upon the Peclet number, in which the blowing arc velocity is expressed through Π

$$Pe = \rho_0 c_{p_0}{}^2 \mu_0 I^2/\lambda_0{}^2$$

Here the scale values of the density ρ_0, specific heat capacity c_{p_0} and thermal conductivity λ_0 are taken according to data in Ref. 3. These are typical for the conditions at the surface of a high-current arc column.

The Peclet number takes into account the dependence of dimensions of a radial arc section upon convective heat transfer at the arc column surface. Heat transfer by conduction inside an arc also lays a certain role, but its influence at the small Peclet numbers is probably less essential. Since the Prandtl number variation is small, then in the correlation $\Pi = f(\mathrm{Pe})$ the influence of friction processes which is represented by the Reynolds number is also taken into account. The absolute values of the Peclet numbers show that stable discharge of an arc is realized mainly in a laminar gas flow near an arc surface.

Figure 1 shows the relation $\Pi = f(\mathrm{Pe})$. It is seen that the correlation appears to be sufficiently good for all gases studied,[1] besides nitrogen. Probably when an arc is blown by nitrogen the electrode jets are more essential than when an arc is blown by other gases, preventing from deflecting near-electrode arc sections

Received December 21, 1971.

Index category: Plasma Dynamics and MHD.

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