Reply by Author to Guile, Naylor, and Hodnett

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GUILE, Naylor, and Hodnett in their criticism of the source model reveal a lack of clarity on my part in explaining a fundamental assumption which I will now set forth: Consider a point source with density ρ_∞ in a uniform flow of the same density (Fig. 3, Ref. 1). Imagine that the density of the source fluid is to be reduced below the value ρ_∞. As this reduction is carried out, the dynamic pressure decreases, altering the balance of forces on the surface AOA' and requiring an adjustment in the flow pattern to regain equilibrium. On the other hand, it is possible to retain the original flow pattern even as the source density is reduced simply by introducing a proper distribution of body force. In the limit of vanishing source density, the total body force in the streamwise direction must equal the negative of the momentum flux originally carried by the wake, that is, $IB = \rho_\infty U_\infty{}^2 b_w$. The fundamental assumption is that the arc can be thought of as producing volume and a distribution of body force that is "just right" in the sense that it accomplishes the aforementioned purposes. This is no answer to the criticism on the validity of the model but is intended to identify the point in question.

Regarding the comparison with experimental data, the source model requires that the arc power be carried away in the wake. In the experiments reported by Roman and Myers,[2] less than 20% of the arc power appears in the wake (inferred from their Fig. 33). They report good agreement between the energy flux in the wake and the increase in power due to blowing which is calculated according to their own description as follows: "Multiplying the increase above the no-blowing average voltage gradient by the arc current gave the additional power input per unit arc length above that required to maintain the arc with no blowing." In view of this, I suggest that a more appropriate electric field to use in the source model equation would be "the increase above the no-blowing average voltage gradient," but the fact remains that these experiments were not convection-dominated as regards the energy transport.

I refrain from comment on the data of Adams and other cited references, some as yet unpublished, which I have been unable to obtain in the limited time available for this reply. Nevertheless, the criticisms are greatly appreciated as is this opportunity to respond.

Concerning my paper,[1] I would like to present two corrections: 1) Statement 2 in Sec IIC, "There is no drag on the point source . . . ," is incorrect. The classical solution yields a negative drag. This requires some revision in the subsequent paragraph to conform to the ideas expressed in the beginning of this present note. 2) It is unnecessary to assume that the velocity is U_∞ at Sec. BB as shown in Fig. 2; the same results are obtained when velocity is treated as a function of position.

Received August 18, 1967.
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References

[1] Otis, D. R., "A source model for predicting the drag force on a moving arc column," AIAA J. 5, 582–584 (1967).
[2] Roman, W. C. and Myers, T. W., "Investigation of electric arc interaction with aerodynamic and magnetic fields," U.S. Air Force Office of Aerospace Research, ARL 66-0191 (1966).

Erratum: "Laminar Convective Heating and Ablation in the Mars Atmosphere"

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[AIAA J. 5, 240–248 (1967)]

EQUATION (9) is incorrect as written. It should read:

$$q_{w_0,B=0}(R/p_0)^{1/2} = (2 \times 10^{-3}/Pr_w)[(M\mu)_{T=280^\circ\mathrm{K}}]^{1/2} \times \{[(\rho\mu)_w - (\rho\mu)_D]/[(\rho\mu)_w(h_D - h_w)]\}^{-0.15} \times (\rho_\infty/\rho_{e_0})^{1/4}(u_\infty)^{2.2}(1 - g_w) \quad (9)$$

Received September 11, 1967.
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