they failed to notice Figs. 4 and 5, where such a comparison is presented. Figure 5 is particularly interesting; it shows that the analysis, despite its so-called drastic assumptions, correctly predicts the entrainment ratio as a function of molecular weight.

The statement in the second paragraph of the Comment that Emanuel compared "...compression ratios without specifying precisely what is to be held fixed" is puzzling. It may well be that the list at the end of Section IIC[1] of the four independent, dimensionless parameters was inadequately emphasized. It should be noted that one of these parameters is the ratio of the mass flow rate of driven to driver gas.

The choice of mass flow ratio vs molar flow ratio is rather arbitrary. What really counts for the applications mentioned in Ref. 1 is the overall size and weight of the ejector system. In this regard, the final remark in the Comment is incorrect. Considerable testing and system analysis at different companies, such as TRW and Rocketdyne, of various hot gas generators shows the desirability of a low molecular weight driver gas. An excellent example is hydrazine,[3] which can decompose into a low molecular weight gas, since one of the major products of decomposition is hydrogen.

References

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Comment on "Transonic Nozzle Flows of Gases with a Rate Process"

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A RECENT synoptic by Ishii[1] makes some concluding statements that require correction and suggest comment.

First, Mr. Ishii states that the solutions obtained in Refs. 2 and 3 "considered the case of Eq. (22)" or constant static pressure on the sonic line ($p_* = \text{const.}$). This is not correct. The transformed Euler equations, as represented by Eqs. (4) and (5) of Ref. 2 and as used in the development of both solutions of Refs. 2 and 3, clearly indicate the consideration given to the radial variation in static pressure. Possibly this misinterpretation stems from the sentence on page 213 of Ref. 3, which reads: "Since the first step is to determine the total pressure gradient across the flowfield, it is convenient to select a station in the combustion chamber where the flow is purely axial ($v = 0$) so that the radial gradient of static pressure will be zero." Note that this merely suggests a procedure for determining the gradient in terms of other known variations in the gas properties; it does not mean a constant static pressure in the transonic region.

Second, Mr. Ishii states, when referring to solutions of Refs. 2 and 3, that "It is obvious that his result is not general in this respect because the condition of Eq. (22) is only one of the necessary conditions for existence of the Hall type of perturbation solution." We contend that the condition of constant static pressure on the sonic line as expressed in Eq. (22) is not a necessary condition, as evidenced by the Hall type solution of Ref. 3 where p_* is variable. Furthermore, it is not apparent what other conditions he refers to when he states that Eq. (22) is "only one of the necessary conditions," since he has already stated, and we agree based upon our results in Refs. 2 and 3, that a stipulation of a constant or variable γ is not necessary.

We are sure Mr. Ishii had valid reasons for expressing the nonuniformities in terms of the flow static properties. However, it is our opinion that expressing them in terms of the total rather than the static flow properties results in the clearer insight into the solution. By doing this in Ref. 3, it was determined that all nonuniformities in the flowfield reduce to a variation in γ only. Since solutions are possible for either a constant or variable γ, one concludes that there really are "no necessary conditions" for the existence of a so-called Hall type of solution other than the basic requirement that the wall contour perturbation parameter be small relative to unity.

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Reply by Author to S. Boraas

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EVEN now, the author believes that the concluding remarks in Ref. 1 are completely correct and valid. Some of the results in Ref. 2 have been based upon erroneous equations. For example, Eq. (5) of Ref. 2 is not correct. The correct one is

$$\frac{\sin\theta \dfrac{\partial V^*}{\partial \xi}}{\left[\left(\dfrac{\gamma+1}{2}\right)\epsilon\right]^{1/2}} + V^*\cos\theta\frac{\partial\phi}{\partial\xi} = \frac{2}{\gamma+1}\,\frac{2\pi r_{tid}{}^2}{\dot m}\,\zeta\frac{\gamma p_t}{a^*}$$

$$\left[1-\left(\frac{\gamma-1}{\gamma+1}\right)V^{*2}\right]^{\frac{1}{\gamma-1}}\Bigg\{\left[V^*\frac{\partial V^*}{\partial\eta} + \frac{V^{*2}}{(\gamma^2-1)}\frac{\partial\gamma}{\partial\eta}\right]$$

$$+\frac{(\gamma+1)}{2\gamma(\gamma-1)^2}\left[1-\left(\frac{\gamma-1}{\gamma+1}\right)V^{*2}\right]\log_e\left[1-\left(\frac{\gamma-1}{\gamma+1}\right)V^{*2}\right]\frac{\partial\gamma}{\partial\eta}$$

$$-\frac{\gamma+1}{2}\,\frac{1}{\gamma p_t}\left[1-\left(\frac{\gamma-1}{\gamma+1}\right)V^{*2}\right]\frac{\partial p_t}{\partial\eta}\Bigg\} \qquad (1)$$

Therefore, the function Γ_0 in Eq. (14) of Ref. 2, which is defined in the Appendix of Ref. 2, must be replaced by

$$\Gamma_0 = \frac{\gamma}{\gamma^2-1} + \frac{1}{(\gamma-1)^2}\log_e\left(\frac{2}{\gamma+1}\right) \qquad (2)$$

It is quite easy to see that the corrected Eq. (14) of Ref. 2 is equivalent to the relation $d(\log_e p^*)/d\beta = 0$ in Ref. 1. Con-

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