Technical Comments

Comment on "Optimum Performance for a Single-Stage Gaseous Ejector"

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EMANUEL[1] has developed a convenient method of calculating the compression ratio of an ejector, based on simple, if somewhat drastic, assumptions. The method was checked by comparing the results with those of a more elaborate computer code believed to be quite accurate. No comparisons of calculated compression ratios with experimental data were given.

In his discussion, Emanuel suggests using hydrogen as a driving gas as a way to improve performance. Since the experimental data of Eichacker and Hoge[2] showed that efficiency increases when the molecular weight of the driving gas increases, his suggestion runs counter to our experience. We used a constant-area mixing region, whereas Emanuel's mixing region is assumed to be so designed as to maintain constant pressure. However, it hardly seemed that this difference could completely reverse the dependence on molecular weight. We used the conventional definition of efficiency based on isentropic enthalpy changes, whereas Emanuel compares compression ratios without specifying precisely what is to be held constant. We decided to calculate compression ratios from Emanuel's equations both at constant mass entrainment ratio and at constant molar entrainment ratio to see if the apparent discrepancy between our results and his predictions could be explained.

The systems chosen were helium-hydrogen and helium-nitrogen. Both were calculated for a mass entrainment ratio $\omega = 0.1$ grams of driven gas per gram of driving gas and for a molar entrainment ratio $\Omega = 0.05035$ mole of driven gas per mole of driving gas. For helium the molecular weight $W = 4.004$ and the isentropic exponent $k = 1.667$; for hydrogen $W = 2.016$ and $k = 1.4$; for nitrogen $W = 28.016$ and $k = 1.4$. The results of the calculations are given in Table 1. Quantities referring to mixtures formed by the driving and driven gas are designated by the subscript m. The Mach number after mixing is M_m. In all cases the Mach number of the driving gas at the beginning of mixing has been taken as 3.5 and the Mach number of the driven gas at the same point is by assumption zero.

In the tabulated pressure ratio, P_{02} is the stagnation pressure of the mixture after compression and P_l is the stagnation pressure of the driven gas as mixing begins (and also the static pressure of the driving gas at the same point).

Since we do not question the desirability of heating the driving gas and cooling the driven gas, the effects of heating and cooling have been eliminated from the comparison by assuming all stagnation temperatures to be equal, thus excluding irrelevant changes from the comparison.

The systems He: H_2 and He:N_2, compared at $\omega = 0.1$, show a higher compression ratio when hydrogen is the driving gas than when nitrogen is the driving gas (10.96 vs 4.728), as predicted by Emanuel. However, when the comparison is made at $\Omega = 0.05035$, the compression ratio when hydrogen is the driving gas is lower than when nitrogen is the driving gas (10.96 vs 13.98). Since the two driving gases have equal values of k, their molar specific heats are equal and the isentropic work of compression between given limits is the same for both gases, to a good approximation. Hence, equal expenditures of work will compress a given amount of driven gas to a higher pressure when the driving gas is N_2 than when it is H_2. This result, obtained by applying Emanuel's equations at constant molar entrainment ratio, shows that, at least in the situation considered, there is no basic conflict between the calculated results and our published data. However, the recommendation to use hydrogen as a driving fluid appears to be incorrect, except perhaps in unusual circumstances where the work required to compress the driving gas need not be considered.

Table 1 Compression-ratio comparisons

Driven gas	He	He	He
Driving gas	H_2	N_2	N_2
ω	0.1	0.1	0.00720
Ω	0.05035	0.6997	0.05035
W_m	2.111	18.13	26.86
k_m	1.408	1.479	1.408
M_m	2.848	1.777	3.233
P_{02}/P_l	10.96	4.728	13.98

References

[1] Emanuel, G., "Optimum Performance for a Single-Stage Gaseous Ejector," *AIAA Journal*, Vol. 14, Sept. 1976, pp. 1292-1296.

[2] Eichacker, S. S. and Hoge, H.J., "Jet-Compressor Efficiencies as Influenced by the Nature of the Driving and Driven Gases," *Journal of the Aerospace Sciences*, Vol. 27, Aug. 1960, pp. 636-637.

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Reply by Author to S.S. Eichacker and H.J. Hoge

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THE foregoing Comment is useful in pointing out that different engineering criteria result in different conclusions. In Ref. 1 the author chose to optimize the compression ratio, since this is of paramount importance for high performance ejectors. Eichacker and Hoge[2] instead use an efficiency, which is based on an enthalpy ratio, as a measure of performance.

However, the statement in the second paragraph of the Comment, that the analysis in Ref. 1 runs counter to their experimental data, is refuted by their own discussion[2]: "The goals of high efficiency and high compression ratio desired in practice are not completely compatible. It was found that the highest efficiencies were always associated with low compression ratios." In fact, no discrepancy ever existed.

In the first paragraph of the Comment, it is stated that no comparison is made in Ref. 1 with experimental data. Perhaps

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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

[1] Emanuel, G., "Optimum Performance for a Single-Stage Gaseous Ejector," *AIAA Journal*, Vol. 14, Sept. 1976, pp. 1292-1296.

[2] Eichacker, S.S. and Hoge, H.J., "Jet-Compressor Efficiencies as Influenced by the Nature of the Driving and Driven Gases," *Journal of the Aerospace Sciences*, Vol. 27, Aug. 1960, pp. 636-637.

[3] Teper, R., "Chemical Laser Ejector Demonstration," Rocketdyne Rept. No. AFWL-TR-76-92, Dec. 1976.

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.

References

[1] Ishii, R., "Transonic Nozzle Flows of Gases with a Rate Process," *AIAA Journal*, Vol. 15, March 1977, pp. 299-300.

[2] Taulbee, D.B. and Boraas, S., "Transonic Nozzle Flow with Nonuniform Total Energy," *AIAA Journal*, Vol. 9, October 1971, pp. 2102-2104.

[3] Boraas, S., "Transonic Nozzle Flow with Nonuniform Gas Properties," *AIAA Journal*, Vol. 11, Feb. 1973, pp. 210-215.

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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

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

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

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

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

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

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

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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