efficiency" η_D. Even if η_D is measured with the diffuser as a separate component, it will generally change when incorporated into the eductor, because of the changed diffuser inlet velocity profile. But given test data for the eductor one can work backwards and say, "Ah! The diffuser efficiency must have been so and so." This is what Quinn[9] did, so that when Salter[10] put the values of η_D so derived§ into his *own* (basically similar) theory and worked back to thrust, the results were gratifying. In contrast, Kentfield apparently tried different values of η_D until his "theory" curve ran through the data! Having done this he asks us to believe in his Fig. 15, for example, that $\eta_D = 0.92 =$ constant for diffuser area ratios $1.0 < A_3/A_2 < 2.4$. This would be very unusual, to say the least, and in any case is counter to Quinn's[9] measurements of $\Delta q/q_{\mathrm{IDEAL}}$, which increase with increasing A_3/A_2.

Since it seems that nothing can cure aeronautical engineers of their interest in eductors (the writer included) it is suggested that some qualified individual or group write and publish a scholarly history of the technology, with appropriate credits, and that we all then abide by the already established rules of our trade, i.e., 1) Give full credit for prior work. One's stature is not increased by failing to do so. Rather the reverse; 2) Don't publish trivial variations of prior work; 3) If a fudge factor is used to make theory agree with experiment, say so clearly.

It would also be nice to have a standardized notation.

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§Quinn did not actually give values of η_D in his paper, but a more complex parameter ($\Delta q/q_{\mathrm{IDEAL}}$) which contains η_D. Since Quinn does not tell us how to get from the one to the other, Salter must have been privy to unpublished information.

Reply by Author to P.R. Payne

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THE major points raised by Payne will be dealt with in order. Payne claims that the work of his Ref. 1 is not original apparently because of asserted similarities with the work identified by Payne as Refs. 6 and 7. The writer wishes to point out that Payne's Ref. 6 (he has not had an opportunity to examine Ref. 7) is concerned with the analysis of ejectors having a uniform (static) pressure in the mixing zone whereas Payne's Ref. 1 dealt with ejectors in which the mixing zone was of uniform cross-sectional area (constant-area mixing). These two situations can be shown to be identical (i.e. constant-pressure mixing in a mixing zone of uniform cross-sectional area) for ejectors in which the irreversibilities are confined to those inherent in the mixing process.[1] It has also been shown[1] that then additional losses occur, for example diffuser losses, constant-pressure mixing is somewhat less effective than constant-area mixing. Briefly, this appears to be due to the greater burden placed upon the diffuser in a constant-pressure mixing ejector. In an otherwise comparable constant-area mixing ejector there is an increase of static pressure within the mixing zone itself between the exit of the secondary flow inlet nozzle and the entrance to the diffuser.[1]

It was explained, clearly in the writer's view, in Payne's Ref. 1 that all the analytical results were obtained using loss coefficients and values of diffuser effectiveness obtained from, or suggested by, experiment. Indeed there is no mechanism by which the analysis could be used to predict loss coefficients and diffuser effectiveness. Accordingly the "agreement" referred to by Payne was, of course, obtained by selecting a suitable value of η_D to correlate with the maximum performance obtained experimentally. It was, however, shown that the two values of η_D so obtained, one for each of the two cases investigated, were consistent with the results of specialized diffuser experiments quoted in the paper. It was stated, again clearly in the writer's opinion, that the agreement mentioned in the paper relates to the forms of the ϕ versus A_3/A_2 curves obtained (Figs. 14 and 15 of the paper in question).

Incidentally Payne makes a statement to the effect that the constancy of η_D is contrary to a realistic situation. The writer would agree with this if it represented the whole story, but, in his paper, the constancy of η_D was also accompanied by a progressive increase in the effective mixing length, with attendent wall friction, as the diffuser area ratio was reduced. This situation is illustrated in Fig. 13 of the paper and explained in the associated text. The implication is, therefore, that the overall effectiveness, as a diffuser, of the extension to the mixing length plus the reduced area ratio diffuser is less than the value of η_D ($=0.92$ for Fig. 15) assumed for the divergent region only. In other words an analytical simplification was made which appears to represent the real situation fairly closely.

The writer doubts that any "newcomer" reading his paper could think, as suggested by Payne, that the simple analysis given represented a unique attempt to analyze ejector flows. In order to comprehend the paper a reader would, presumably, have at some time attended an undergraduate class in fluid mechanics where, almost certainly, he would have been introduced to the classical analysis of ejectors.

In conclusion, the writer feels that any value his paper may have lies not in the details of the fairly basic analysis, which in his view must be presented at least in outline in order to make the work believable, but rather in the results of the parametric study which quantify, over a wide range of area ratios, the influence of changes in loss coefficients and diffuser effectiveness on the thrust augmentation ratio of single-stage ejectors with constant-area mixing.

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