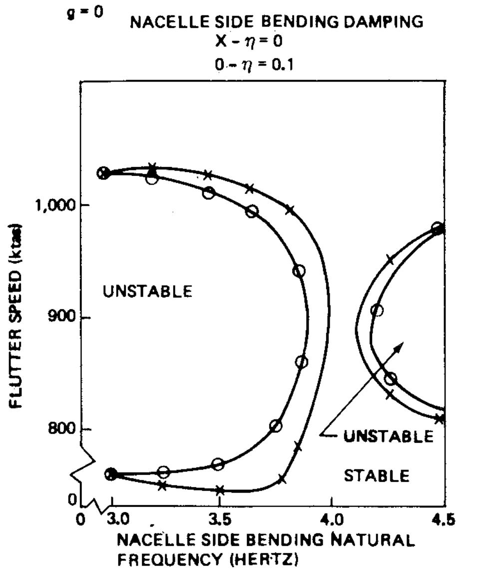

Fig. 1 Nacelle strut damping effect on chimney flutter mode.

flutter boundary is plotted for zero damping and for $\eta = 0.1$ damping added to the nacelle strut in side bending. The addition of damping increases the bandwidth of nacelle side bending frequency in which the nacelle tuning suppresses flutter; the "chimney" becomes wider with the addition of damping. In addition, the flutter speed is increased with the addition of damping at some of the other nacelle side bending natural frequencies studied but not at all of them.

Conclusion

The addition of damping to the nacelle strut in this study improved the airplane flutter characteristics. This addition of damping to nacelle struts has become practical with the recent development of materials such as 3M's ISD 113. An application of these materials is documented on an F-15 application in Ref. 1. Their durability in a harsh environment has been demonstrated on the TF-30 inlet guide vane treatment discussed in Ref. 2. A large selection of damping materials is currently available for the designer to choose from.

Recommendations

1) The addition of structural damping should be considered on designs requiring the suppression of "chimney" type flutter modes.

2) The effects of any damping added to the structure should be included in dynamic gust and fatigue calculations.

3) When damping has been added to a structure for other reasons (e.g., noise control, sonic fatigue), its effect should be included in flutter calculations. This could include transport aircraft aft body damping affecting wing/body coupled flutter modes and affecting empennage flutter.

4) The possibility of adding damping to combat airplanes with external store hump mode flutter problems should be investigated.

References

[1] Share, J.D. and Drake, M.L., "Elimination of a Resonant Fatigue Problem for Major Maintenance Benefits," ASME Paper 77-DET-135.

[2] "Vibration Damping Short Course," Course Notes, University of Dayton, Oct. 1978.

Technical Comments

C 80-095

Comment on "Prediction of Performance of Low-Pressure-Ratio Thrust-Augmentor Ejectors"

~~C 80-04~~

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IN Ref. 1 Kentfield has presented a one-dimensional incompressible flow model of ejector performance which seems to show good agreement with experimental data generated by the U.S. Air Force Aerospace Research Laboratories. In the writer's view, Kentfield's paper is open to criticism on two grounds: the work presented is not original, and the experimental "agreement" is apparently obtained by arbitrarily "adjusting" the diffuser efficiency η_D to get the correct answer. Since η_D is, as his paper shows, the single most powerful coefficient, (it has a large effect on augmentation ratio) such a procedure is hardly meaningful. It is also misleading not to state clearly that such a procedure was employed.

On the first count, the writer thinks he has noticed in recent years a trend toward inadequate citation of prior references†; a trend, incidentally, also suggested by writers in at least one other discipline.[2,3] In the case of Kentfield's paper, a newcomer to the field might think that he was reading the first incompressible flow analysis of this type. In fact, many have preceeded it (one[4] carries the illustrious name of von Karman‡), and Flugel[5] refers to a number of analyses prior to 1930 including one by Lorenz[6] in 1910. [Even earlier, Timothy Hackworth and George Stevenson (circa 1830) employed ejectors to obtain a forced draft in the stacks of their boilers. This led to boiler feed-water ejector pumps, and thence to the extensive employment of ejectors by engineers for a wide variety of applications.]

Perhaps understandably, the writer is most taken with the similarities between Kentfield's analysis and his own of fourteen years ago. (An AIAA paper[7] was presented at a joint CASI/AIAA meeting in Ottawa, and later expanded into Ref. 8.) The writer took the entrained mass flow ratio as the independent variable. Kentfield uses the area ratio—a trivial difference. The writer also used a now outmoded definition for diffuser efficiency which is slightly different from Kentfield's.

Not to be too parochial, it is fair to point out that both Quinn[9] and Salter[10] (both referenced by Kentfield) can make the same complaint and validly ask in what meaningful way Kentfield's theory differs from theirs. As can other, earlier authors, not referenced by any of these three. As a matter of fact, it is arguable that almost no original contributions have been made since Flugel's in 1939, the writer's papers included.

One of the most satisfying things about eductors is that one almost never knows what value to ascribe to the "diffuser

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†The writer is not claiming to be guiltless himself, especially when young. But he *tries* to do better.

‡Who also omitted any reference to prior work.

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.

A)

It would also be nice to have a standardized notation.

78-001

References

Ref 78-145

[1] Kentfield, J.A.C., "Prediction of Performance of Low-Pressure-Ratio Thrust-Augmentor Ejectors," *Journal of Aircraft*, Vol. 15, Dec. 1978, pp. 849-856. 12

[2] Luberoff, B.J., "The Industrial Chymist's Decay of the Literature," *Chemical Technology*, Feb. 1976, p. 73.

0025 [3] Payne, P.R., letter to editor of *Chemical Technology*, May 1976.

0007 [4] von Karman, T., "Theoretical Remarks on Thrust Augmentors," Reissner Anniversary Volume, *Contributions to Applied Mechanics*, J.W. Edwards ed., Ann Arbor, Michigan, 1949.

0001 [5] Flugel, G. "The Design of Jet Pumps," NACA-TM-982, July 1941, ATI 42549.

[6] Lorenz, H., *Technische Hydromechanik*, R. Oldenbourg, Berlin and München, 1910.

[7] Payne, P.R., "Viscous Mixing Phenomena with Particular Reference to Thrust Augmentors," AIAA Paper No. 64-798, Oct. 1964.

[8] Payne, P.R., "Steady State Thrust Augmentors and Jet Pumps," U.S. Army Aviation Material Laboratories, Ft. Eustis, Va., AD 632-126, March 1966.

[9] Quinn, B., "Compact Ejector Thrust Augmentation," *Journal of Aircraft*, Vol. 10, Aug. 1973, pp. 481-486.

[10] Salter, G.R., "Method for Analysis of V/STOL Aircraft Ejectors," *Journal of Aircraft*, Vol. 12, Dec. 1975, pp. 974-978.

§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.

C80-096

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.

References

[1] Kentfield, J.A.C. and Barnes, R.W., "The Prediction of the Optimum Performance of Ejectors," *Proceedings of Institution of Mechanical Engineers*, London, England, Vol. 186, 1972, pp. 671-681.

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