they are efficiently entrained by the thruster exhaust the background neutrals, on the average, are stationary. The measurement will, therefore, underestimate the average velocity of the beam neutrals. Furthermore, at a given neutral flow rate, the density of the beam neutrals decreases with increasing axial velocity. The higher the true velocity, the more this measurement would underestimate it.

The magnitude of this effect is clearly appreciable in the experiments of Malliaris and Libby (configuration *a*). In the typical MPD conditions considered by the authors [after Eq. (14)], the beam density is taken as $2\ (10)^{14}\ \text{cm}^{-3}$ (ions plus neutrals). This corresponds to the lowest mass flow rate considered (10 mg/sec), so presumably the lowest tank pressure (10 $\mu$) would be appropriate. The temperature of the background gas would be about the same as that of the tank wall or room temperature. Thus, for $10\ \mu \approx 10^{-5}$ atm, the background neutral density would be:

$$N \approx 10^{-5}N_o \approx 2.6(10)^{14}\text{cm}^{-3} \qquad (1)$$

where $N_o$ is the density at standard conditions. By the author's own arguments, the background gas would diffuse freely into the beam region. Since the estimated background density exceeds the estimated density of neutrals in the beam, a doppler shift measurement would significantly underestimate the velocity in this case.

The above effect may account for some of the qualitative difference between the results of Malliaris and Libby and those reported earlier by Sovie and Connolly.[2,3] Sovie and Connolly reported much higher neutral velocities than those observed by Malliaris and Libby. The Sovie and Connolly experiments, however, were performed at two orders of magnitude lower background pressure where the effect described above would be negligible.

### References

[1] Malliaris, A. C. and Libby, D. R., "Spectroscopic Study of Ion-Neutral Coupling in Plasma Acceleration," *AIAA Journal*, Vol. 9, No. 1, Jan. 1971, pp. 160–167.

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# Reply by Author to D. J. Connolly and R. J. Sovie

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CONNOLLY and Sovie[1] argue that the background particle density in our experiments[2] is comparable to the particle density in the beam. This might be so but, in an ammonia MPD flow, the beam fluid is much richer in atomic species (such as H and N), than the background fluid. The environmental background fluid (mainly $NH_3$, $H_2$ and $N_2$) diffusing into the beam, well downstream of the accelerator, has a much smaller chance of being dissociated because of the much milder conditions prevailing at these downstream stations. Thus, the presence of atomic species of background origin is fractionally negligible in the beam.

More important are the following experimental facts: in our experiments[2] we have used two configurations, a and b. The first is unfavorable to a strong ion-neutral coupling, while the opposite is true for the second. These configurations have been tried under experimental conditions which overlap over a certain range (see Table 1 of Ref. 2). In this range, whatever background effects are present in the expanded beam of configuration a should also be present in b. However, under otherwise identical conditions, the neutrals in a were found to be much slower than those in b. It follows that the background effect mentioned by Connolly and Sovie is not sufficiently strong to affect our measurements.

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# Comments on "Flow with $M_\infty = 1$ Past Thin Airfoils"

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IN a recent paper[1] a comparison of theoretical[2] and experimental[3] sonic pressure distributions is presented for airfoils having ordinates $Z$ and chordwise location $(x/c)_{Z\ \max}$ of the maximum thickness given by

$$(Z/c) = A[(x/c) - (x/c)^n], \quad (x/c)_{Z\ \max} = (1/n)^{1/(n-1)} \ldots \qquad (1)$$

and

$$(Z/c) = A[(1 - x/c) - (1 - x/c)^n], \quad (x/c)_{Z\ \max} = 1 - (1/n)^{1/(n-1)} \ldots \qquad (2)$$

in which $A[\tau n^{n/(n-1)}]/[2(n - 1)]$, $c$ is the chord length and $\tau$ is the thickness–chord ratio. For maximum thickness locations rearward of $(x/c)_{Z\ \max} = 0.50$, the discrepancy between the theoretical and experimental distributions ceases to be small.

The validity of the experimental data quoted has been questioned by Thompson,[4] since they derive from wind-tunnel "bump" tests using a ventilated working section that may not have been adjusted precisely for interference free conditions. Also the chord length (3 cm) was relatively large in relation to the working section height (7 cm) and span (5 cm). These suspicions have been substantiated by measurements on symmetrical airfoils made in the 81 cm high, 53 cm span, Transonic Wind Tunnel at A.R.L., Melbourne.[5] The tunnel wall open area ratio and the airfoil chord-tunnel height ratio were varied in order to assess the extent of the interference. Airfoil chord length varied from 6.27 cm–20.32 cm, while thickness-chord ratio was held at 0.12 throughout. The maximum thickness location was $(x/c)_{Z\ \max} = 0.3$ (strictly 0.3011) corresponding to $n = 6$ in Eq. (2) above. Data for maximum thickness location $(x/c)_{Z\ \max} = 0.7$ corresponding to $n = 6$ in Eq. (1) above were obtained by reversing the airfoils.

Results estimated to be reasonably free of interference are presented in Fig. 1 as chordwise distributions of the transonic

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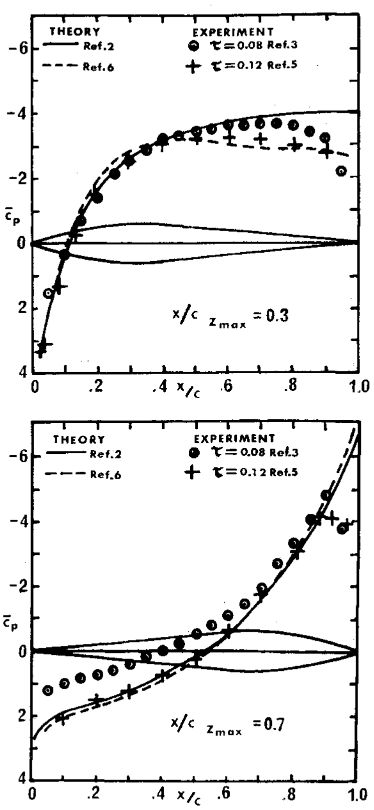


**Fig. 1 Theoretical[2,6] and experimental[3,5] pressure distributions at sonic speed for airfoils having various locations $(x/c)z_{\max}$ of the point of maximum thickness.**

similarity pressure coefficient

$$\bar{C}_p = [M_\infty^2(\gamma + 1)/\tau^2]^{1/3}C_p$$

Use of this parameter should account for the effect of variation in thickness-chord ratio $\tau$, at least for values up to 0.12. The theoretical and experimental results shown in Ref. 1 are included, along with theoretical results obtained by the parametric differentiation method.[6]

For the airfoil with maximum thickness at 0.3 of chord (Fig. 1) the new results show the zero and slightly positive pressure gradient beyond 0.5 of chord predicted by the parametric differentiation method. Upstream of 0.4 of chord the results from all sources are in reasonable agreement. For the airfoil with maximum thickness at 0.7 of the chord the new results agree closely with values calculated by both[2,6] theoretical methods.

The magnitude and chordwise distribution of the wall interference found in the recent investigation[5] suggest that this was not the main reason for the discrepancy in the earlier experimental data. The main reason appears to have been the "half-model" technique used. Evidently the approaching wall boundary layer distorted the displacement shape, giving in effect a forward elongation of the chord. This effect would be expected to less apparent with increase of leading edge angle, i.e. with forward movement of the maximum thickness location, at constant thickness-chord ratio, or with increase of thickness-chord ratio at constant maximum thickness location. Both these tendencies appear in the results from Ref. 3 presented in Fig. 1 of Ref. 1.

The new results thus further reinforce the conclusion of Ref. 1 that inviscid theory is capable of providing a good approximation to the sonic pressure distribution on airfoils of this shape. However discrepancies near the trailing edge are apparent in all the results, indicating that viscous effects are far from negligible.

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## Comment on "A Direct Numerical Analysis Method for Cylindrical and Spherical Elastic Waves"

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A SIGNIFICANT computational error in a paper[1] on cylindrical and spherical elastic stress waves, published by the authors some years ago, has recently been found. The discontinuous-step analysis, as described on page 114 of the paper, refers to the need to insert "boundary corrections" (Eq. 16e) in the analysis. By some oversight, most probably a mix-up with an earlier version of the computer program, the boundary corrections step was omitted in the computer runs made to obtain quantitative results, some of which were published in the paper. As a consequence of repeating steps (16a)–(16l) for $i = 1$ to $i = i_m$ in the propagation procedure, $v_{i_m+1}$ never gets calculated during the step (16j) and its wrong value (assigned zero at the beginning of the program as done to all the variables in a computer program) gets used in the step (16a) causing an error in $\epsilon_r^{i_m+1}$ which in turn creates an error in $\sigma_\theta^{i_m}$ through (16d). The wrong value of $\sigma_\theta^{i_m}$ must be erased and the correct one, as dictated by the boundary conditions, must be supplied as given by (16e). The reported disagreement between the discontinuous-step solution and Suzuki's mathematical solution (Fig. 10)[1] is now found to be a consequence of an inadvertent omission of (16e). When this error is corrected, the two solutions agree satisfactorily as shown in Fig. 1.

Corrected quantitative results for cylindrical elastic waves to replace Figs. 6–12 in the original paper may be obtained

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