In closing, a quotation from R. B. Green’s paper is provided: "I have found this paper very interesting and well written. Some brief comments are listed below:

1. I feel that the comment [just preceding equation (21)], that the range of L > 500 is not difficult to achieve in practice, is mildly misleading in that the "low" values of L are in fact difficult to attain. It is to be noted that the usual simple ejector corresponds to L = ∞. In this regard it is important to note that the restriction to L > 300 arises, in fact, because the ejected fluid reaches a low L (high magnetic field) would be so short as to violate the requirement of large length to width of the channel. (Assumption 7.)

2. It would seem that an interesting extension of the work would be to consider the combined effects of finite viscosity and finite magnetic field. We may note here that Professor Heiser’s assumption of zero viscosity led to conservative predictions for the required channel lengths.

3. A further interesting extension of the work would be to consider the mixing of two streams of conducting fluids of different density and electrical conductivity, such as suggested in "condensing ejector" systems.

W. C. MOFFATT. The author has contributed an interesting addition to the rapidly growing literature describing MHD devices. While many of these applications are of academic rather than practical interest, they nonetheless serve to illustrate various aspects of hydromagnetic interactions and can frequently form the basis of instructive laboratory experiments.

In Professor Heiser’s analysis there are two points that are perhaps worthy of note. The first is concerned with the assumption that the electric field E, seen by the primary and secondary fluids is the same [equations (13) and (14)]. This will be true only if the side walls of the channel in the x-y plane are electrically conducting. If the walls are nonconducting, as stated in assumption 1, the current densities in the two streams will not be as given in the present paper, but will be related to the width of the primary and secondary streams. (This point has been confirmed in private communication with the author.) Also, it would be of interest to estimate the electric flow loss coefficient β on the ejector performance. In particular, if the magnetic field at the ejector inlet and exit is so tailored that β approaches zero, K will become zero and Γ will approach infinity. (This last is true in spite of the author’s statement that Γ = ∞ corresponds to the case of a simple frictionless ejector.) Since such values lead to indeterminant results for the mixing length [equation (20)], it is not possible to present a plot in terms of the author’s variables. However, setting β = 0 results in considerably simplified forms of equation (9) (which can then be solved directly for m₃/m₂) and equation (10), which becomes a straightforward expression for (pₑ − pₑ₀)A/μ₃.

Using these relations, and equations (16-18), the writer has computed the mixing-length ratio d/b (assuming β = 0) for the example case treated by the author, and found that d/b = 3.0. Since this solution is essentially the same (within slide rule accuracy) as that given in the paper, there is some question of the necessity of introducing the considerable complication of end losses into the analysis, at least for cases of experimental interest.

This parameter is substantially by computing an end-loss interaction parameter Nₑ = 2(pₑ − pₑ₀)/(μₑ₀) with the mixing-length interaction parameter Nₑ. Taking the ratios of these quantities yields, after simple algebraic manipulation,

\[ Nₑ/N = 3 \beta (1 + m₃/m₂) \]

Under the most severe circumstances of large α and small Γ, this ratio is on the order of 0.05, and for the example case is approximately 0.01. It is therefore apparent that end losses do not play an important role in the ejector performance.

The author has stated his assumptions very clearly and has given a lucid account of the ranges of parameters for which they are valid. In his third assumption he has restricted his analysis to the special case in which the exit static pressure is equal to the secondary-inlet stagnation pressure (pₑ = pₑ₀). One will naturally wonder what effect the additional parameter (pₑ = pₑ₀) will have on the solution, and an attempt to answer this question would be a logical extension of his work. With this additional information available, one could compare the efficiency of an MHD ejector as a means for moving a fluid with the efficiency of a conventional MHD pump. The ejector is more versatile since it has two controllable parameters instead of one, but does one sacrifice anything for this versatility?

The range of validity of the laminar solution is based on the criterion R < 6.25 x 10⁹ N⁴, which is based on experiments on flow between rigid walls. In the case of the ejector, the boundary of the faster moving primary fluid is not a rigid wall but a slower moving fluid, and hence the stability criterion will probably have to be modified. Here, certainly, experiments are crucial.

The criterion the author gives for determining the mixing length is obtained by relating the ratio of primary to mass average velocity at the exit section to the corresponding ratio in the entrance section; however, for some applications it would seem more appropriate to require that \( \frac{V_p}{V_m} - 1 \) be below a specified value.

G. C. OATES. I have found this paper very interesting and well written. Some brief comments are listed below:

1. I feel that the comment [just preceding equation (21)], that the range of L > 500 is not difficult to achieve in practice, is mildly misleading in that the "low" values of L are in fact difficult to attain. It is to be noted that the usual simple ejector corresponds to L = ∞. In this regard it is important to note that the restriction to L > 300 arises, in fact, because the ejected fluid reaches a low L (high magnetic field) would be so short as to violate the requirement of large length to width of the channel. (Assumption 7.)

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3. A further interesting extension of the work would be to consider the mixing of two streams of conducting fluids of different density and electrical conductivity, such as suggested in "condensing ejector" systems.

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4. Associate Professor, Department of Aeronautics and Astronautics, School of Engineering, Massachusetts Institute of Technology, Cambridge, Mass.
4. As a final comment, I feel that it would be of great interest to run an experiment on such a MHD ejector operating within the restrictions requested under the list of assumptions. Such an experiment would obviously shed light on the validity of the many assumptions.

To sum up, I feel the paper has interest primarily because several of the MHD concepts of today involve the use of ejectors, and such a system is naturally suited for use with MHD working fluids. The paper also gives a further example of the possible uses of MHD techniques to control fluid vorticity.

**Author's Closure**

I would like to thank Professors Moffatt, Anderson, and Oates for their careful reading of this paper and pertinent comments. They have certainly indicated many of the interesting questions that should be answered about magnetohydrodynamic ejectors.

With respect to the remarks of Moffatt, it should be noted that $\beta$ or zero can only be approached by extending the region of magnetic field, which is expensive and increases the ordinary frictional losses. When $\beta$ is zero, all the performance curves reduce to that of the simple frictionless ejector ($T = \infty$) except for their careful reading of this paper and pertinent comments. They have certainly indicated many of the interesting questions that should be answered about magnetohydrodynamic ejectors.

An experiment was performed in order to check the accuracy of the mathematical model of the problem selected by the authors. The data give no evidence that any refinements such as considering $h$ to depend upon geometrical parameters of the cone, or fluid properties, would improve the accuracy of the theory. Furthermore, there is no indication from the results that the assumptions inherent in the small-deflection shell theory used in any way inhibit the accuracy of the theory.

The theory slightly overestimates the stress (especially $\sigma_4$) as determined by experiment by the strain gages located at $x = 12$ in. above nucleate boiling. Assuming that the authors provided a means in their test apparatus for insuring symmetrical loading, then the discrepancy could be due to the fact that the boundary conditions $N_2 = 0$ and $M_2 = 0$ at $x = L$ may not have been fully realized in the experiment. For example, equation (22) shows that a larger positive $M$ would reduce $\sigma_4$ and $\sigma_5$.

Comments of G. W. Smith and R. L. Armstrong are gratefully acknowledged by the discussers.

**Authors' Closure**

It should be pointed out that Fig. 4 represents only the experimental results obtained for $2h/k$ and no attempt was made to obtain a theoretical value of this quantity subject to the experimental environment. The authors feel that the independence of stresses from water velocity results from the low values of water velocity that were experimentally obtained with their apparatus.

Limited results were obtained from the strain gage nearest to the cone tip, but because of the difficulty in obtaining a good bond between the strain gage and the shell near the tip, these results were erratic and appeared to be too inconclusive for presentation. The boundary condition at $x = L$ should only influence the stresses in the $z$-direction locally, and the discrepancy between theory and experiment could possibly be from a temperature gradient in the $z$-direction that was not accounted for in the analytical solution.

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3 C. W. Smith.

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**Rigid-Plastic Beams Under Uniformly Distributed Impulses**

J. S. HUMPHREYS.

This paper is an interesting study of the relative effects of including large deflections and strain-hardening in a simple rigid-plastic problem. The authors neglect axial constraints and demonstrate for the resulting beam-bending problem that the addition of strain-hardening effects produces more significant corrections than the addition of consideration of geometry changes. It is worth noting, however, that the inclusion of axial (or "in-plane") constraints, which will be present in some form in almost any real structural situation, will have a large effect in terms of reducing final plastic deformations. For the clamped beam, as shown both experimentally and analytically, for example, in Fig. 4 of a previous paper by this writer, the maximum deformation is lower than that predicted by the simple beam solution by a factor of between 2 and 6 (for the impulse range considered by the present authors). The strength of this effect suggests that axial friction present in the experiments may account for a considerable part of the discrepancies still seen in the authors' improved results, particularly in Fig. 12, rather than simply additional strain-hardening effects that have been ignored.

At the end of the paper the authors mention observing "interac-

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