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DISCUSSION

T. M. Drzewiecki[4]

A general comment is in order prior to the detailed discussion. There is not enough information given on the experimental setup and the fluidic amplifier geometry to enable an independent worker to duplicate the experiment. There is little description on the generation of the "step" in flow. Since the signal flow was measured by a turbine flow meter, some mention could be made as to its dynamic response. Since during switching the input control impedance is rapidly changing it should be mentioned how the constant flow is achieved. The title mentions setbacks but only one is used.

The following discussion bears on a detailed examination of the work reported on.

1 It is difficult to see the relevance of using control flow rates in excess of 0.25 of the supply flow since for values greater than this the control flow velocity at the exit, where it must pass between the supply jet and the attachment wall is greater than the supply velocity. This is due to the fact that the spacing between the supply jet and attachment wall, at best, is $0.25W$. By continuity it is clear that when $q_c = 1.0$, $u_c = 4u_0$ since $A_c = 0.25A_0$.

There is no mention of control pressures, in fact they were not measured, yet a simple application of Bernoulli's equation at the control, assuming an ambient exit condition, yields the fact that when $q_c = 0.25$ the control pressure must be 100 percent of the supply pressure and 1600 percent of the supply pressure when $q_c = 1.0$. If this amplifier recovered 100 percent of the supply pressure at the output port then its pressure gain would be less than 1.0 for $q_c > 0.25$, hardly a desirable operating characteristic. Most amplifiers operate at a power gain greater than 1.0 yet at $q_c = 1$, 16 times the supply power is being provided to the control. More useful information could have been obtained if the study centered at a much lower range of control flows (i.e., $0 < q_c < 0.25$).

2 The empirical equation given relating the separation point and the attachment point is only valid for $q_c \geq 0.2$. The authors say that only for $q_c > 0.1$ does the jet immediately touch the opposite wall. It is therefore distinctly possible that at the lower range (normal amplifier operating range) the mechanics of switching are of the "end-wall" type, and there is no relation between X_E (separation) and X_i (attachment). The use, therefore, of this relationship in any analysis must be with due caution.

3 The author's reporting of a vena contracta for $q_c > 0.3$ is not surprising since the control jet velocity is greater than the supply jet and is essentially squashing the supply jet. It would seem that the mechanics of an analysis of such a case would warrant considering the control jet as entraining the supply jet. Definitely the maximum velocity in the field is determined by the powerful control jet.

4 The authors indicate that the "close-wall" switching is faster than "end-wall" switching and use the data of Lush (their references [7, 8]) to point this out. It was particularly unfortunate that this comparison was made against their own data when their opposite control was blocked. Lush reported that during his experiment the inactive control was actually passed between 10 and 17 percent flow. If this is noted and Lush's data compared with the author's data at 15 percent bias flow, Fig. 17, one sees that there is little difference.

This would indicate that there is no inherent difference in the mechanism of the switching. It was not surprising that the authors' amplifier switched faster with the opposite control blocked, since it is well known that close-wall amplifiers can be switched by blocking a control. There is a misprint in the paper; Lush used a 15 deg wall angle.

5 The analytical study presented does not take into effect the large control signals on the supply jet. The empirical equation determining the separation point cannot be used for $q_c < 0.2$, thus it does not seem strange at all that the analysis fails to predict the switching time. It fails below $q_c = 0.2$ because the mechanism is probably "end-wall," and fails above probably because the velocity profile is so grossly distorted by the control.

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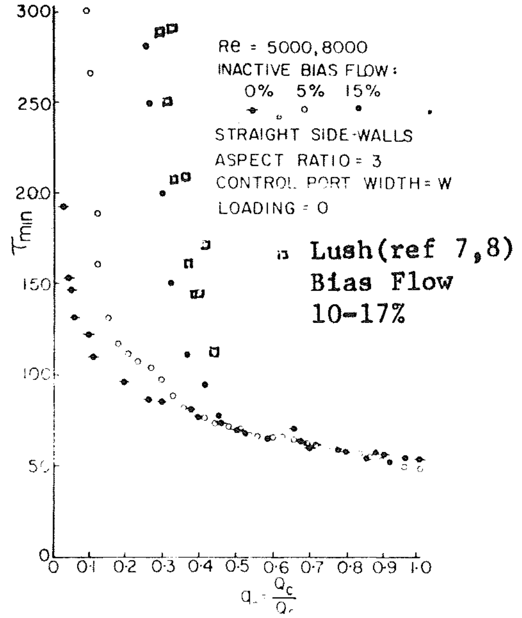

Fig. 17 Effect of inactive bias flow on the minimum switching times in the straight-walled amplifier

The basic purpose of an analysis is to provide a power of prediction. This study has not provided one.

6 In conclusion I feel that this paper has little to present in the way of useful new information. The data on the curved wall were not unexpected and in general since no static characteristics were given on the amplifiers the applicability of the data to other amplifiers is singularly limited. The present data is not compared at all with that of Muller (their reference [3]) who also did a study on pulse duration on close-wall amplifiers, in 1964, also using water as the working fluid. Since most close-wall amplifiers operate at a control flow $q_c < 0.1$ and pressure $p_c < 0.5$ it would have been more meaningful to have concentrated on that range.

The analysis would have been only valid for the geometry presented, but due to the predominance of the control, the basic assumptions are definitely suspect, thus its applicability is at best limited to a new geometric representation of the switching jet.

T. Sarpkaya[5]

No technology in modern history was born with as much promise of immortality, immunity to environmental conditions, and a favorable economic position as was fluidics, primarily because of the incredible simplicity of the appearance and isolated operation of its earliest elements. However, a dozen of years and hundreds of papers which resulted in a descriptive set of rules-of-thumb rather than in a prescriptive set of design criteria have taught us that the mastery of the simultaneous use of both the brains and brawns of a fluid medium, i. e., imparting and processing information as well as transmitting power with fluids, is at best an exceedingly complex proposition. The source of major difficulty, and perhaps of some disillusionment, lay not so much in the control theory but rather in the prediction of the behavior of the motion of the working fluid. Thus, any further attempt made, similar to that by the present authors, to understand the static as well as dynamic fluid-fluid and fluid-solid interaction in fluidic elements is a welcome contribution. Perhaps, as a consequence of such efforts that the theoretical and experimental findings arrived in the past will somehow be unified and everything will fall in its proper place in a cross-flow puzzle.

The authors state that "the geometry of the convex-walled amplifier was similar to that tested by Sarpkaya." However, a comparison of the geometry of the amplifier reported by this discusser with that shown in Fig. 1 of the present authors clearly shows that the two amplifiers are far from being similar. The following table shows some of the major differences:

	by Sarpkaya	by the present authors
Included angle	24 deg	30 deg
Cusp radius	1.5 w	2/3 w
Control width	0.5 w	w/3 and w
Reynolds No.	(around) 60,000	(around) 2000 -8000.

Without further elaborating on the geometric and dynamic *dissimilarities* of the amplifiers and the test conditions of the authors and the discusser, it suffices to state that the authors have inadvertently conveyed the impression that the straight-walled amplifier had better switching characteristics and load insensitivity than those of the convex-walled amplifier reported by this discusser. The correction of this point through this discussion is, of course, appreciated. It is evident that a rather small cusp radius can contribute very little to the returning flow, as noted by the authors, and even result in undesirable amplifier performance. Similar comments are applicable to the other differences in geometry (as tabulated above) and to the rest of the conclusions of the authors. The authors are encouraged to carry out additional experiments with a cusp radius of 1.5 *w* and at Reynolds numbers well above 8000.

The foregoing comments stem not from a criticism of the authors' work but rather from a full recognition of the fact that each amplifier has its own idiosyncrasies dependent on the small variations of its geometry and the Reynolds number.

M. Epstein[6]

The authors study of the switching dynamics of bistable fluidic amplifiers with low setbacks is a noteworthy contribution and contains a considerable amount of well presented experimental information and valuable data. The analysis, which uses a quasi steady state approach, follows that of Lush [7, 8, 9][7] and Epstein [10, 23] and includes a correction for some unsteady effects.

Some detailed comments are offered on the theoretical approach:

1 The possibility to extend his analysis of phases I and II to the case of opposite wall switching transients by using experimentally determined and generalized jet path equation was suggested by Epstein [23] (p. 107). In the present work the jet path is defined with the aid of the empirical relation $X_1 = X_1(X_E)$ which is valid as the authors point out only for the geometry used in the study. The important question whether it is possible to derive a generalized jet path equation for the case of opposite wall switching transient needs, thus, still to be answered.

2 In quasi steady state analysis the use of $(q_r/q_2) = (q_r/q_2)_{s.s.}$ seems justified in view of the relatively slow down-wall-movement of the reattachment zone compared to the jet velocity (if this is the case); that is, we assume that in this case the fraction of the total jet flow arriving at the reattachment zone

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[7] Numbers [23-28] in brackets designate Additional References at end of discussion.

which returns into the bubble is the same as in steady state. However, the use of $dv/d\tau \neq 0$ and $dv/d\bar{\tau} = 0$ simultaneously is, in the discussers opinion, a misinterpretation of a quasi steady state analysis and may lead to appreciable errors *also when the switching process is slow.*

It can be shown that the relative error in dv/d. i.e., the ratio of the computed values corrected for unsteady effects to the uncorrected values of $dv/d\tau$ is given by

$$\frac{(dv/d\tau)\text{ corrected}}{(dv/d\tau)_{s.s\text{ anal}}} = 1 - \frac{(q_r/q_2)_{s.s\text{ anal}}}{(q_c + q_r - q_e)_{s.s\text{ anal}}} \cdot \frac{d\bar{v}}{d\tau}$$

Now, for a very slow process $dv/d\tau \rightarrow$ o, *but* at the same time the denominator $(q_c + q_r - q_e) \rightarrow 0$. Fig. 15 clearly shows that the relative correction is almost independent of the switching times.

It should also be pointed out that the authors' treatment of the unsteady effects (that is, accounting for $dv/d\tau$) is essentially identical with that of Epstein [23] (see e.g., Fig. 15 p. 39 and pp. 60 to 64 and 147 to 152).

3 At the end of the "Analytical Study" section the authors state "The effect of *mass hold-up in the jet* is thus a powerful one, and appears to be sufficient to account for discrepancies between earlier theories and experiments without *resort to unusual values of spread parameters.*"

The discusser has used in his analysis of the end-wall-type switching transient a value of $\sigma > 30$ (see Epstein [10, 23]) and feels, therefore, that some clarification of the value and the meaning of σ is in place.

There is much confusion in the use of σ. In the solution (Goertler [24] and Reichardt [25]) of the two-dimensional, incompressible, turbulent jet emanating from an infinitely narrow slot, σ is used as the jet spread parameter and, thus, satisfies the equation (see e.g., Sawyer [26]).

$$\text{Entrainment rate} \triangleq \frac{1}{U_c}\frac{dQ}{dX} = \frac{1}{\sigma}$$

In Bourque's theory [27], σ is an *average* value of the spread parameter for the whole jet length from the nozzle (which is of *finite width*!) to the reattachment zone. This jet length *includes*, thus, the region where the flow is not fully developed (NFDF). As an example: if we consider a jet with NFDF region of $X_0 = 5.2\ W$ and use Simson profile with $K = 0.2644$ (values of X_0 and K which are used by the authors in their analysis), and define the average value of $\sigma(\bar{\sigma})$ by

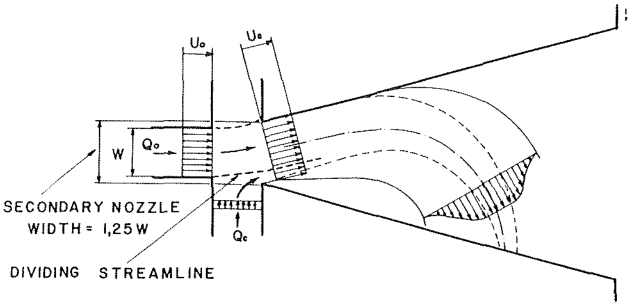
$$\frac{1}{\bar{\sigma}} = \frac{1}{U_e}\frac{\Delta Q}{\Delta X}$$

we get for the NFDF region the *average* value of

$$\bar{\sigma} = 1/(K - W/X_0) = 13.8 > 7.67!$$

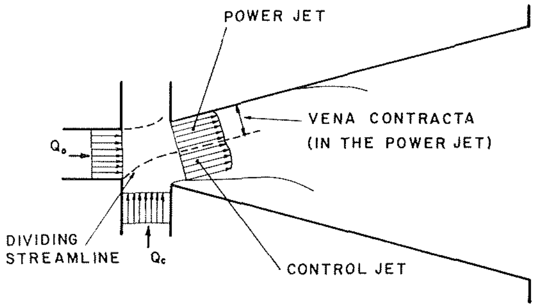
All that for a *free* two dimensional jet and *not for a confined jet.*

The velocity profile measurements of Lush [8] made at various cross section along the confined jet in a bistable fluidic amplifier with a power nozzle of aspect ratio 1.0, permit to compute average values of $\sigma = 23 - 32$ for $q_c = 0.2 - 0.3$ for jet length of approximately 13 W. The discusser, who made these computations, assumed the formation of a *combined jet* (from the control and power jets) in these computations; it should be stressed that if this assumption is not made (on the basis of the above mentioned Lush's measurement it is possible to show that *there is a combined jet*) smaller values of $\bar{\sigma} = 14$ to 20 are obtained, and these are equivalent to higher entrainment rates, since in this case the power jet appears to "entrain the control jet", and this is obviously a misinterpretation of the concept of entrainment.



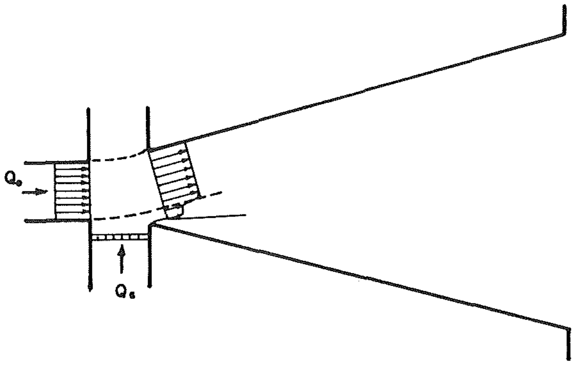
$Q_c \approx 0{,}25\ Q_o$

Fig. 18(a)



$Q_c \approx Q_o$

Fig. 18(b)



$Q_c < 0{,}25\ Q_o$

Fig. 18 (c)

Fig. 18 Approximate velocity profiles at the secondary nozzle

Regarding the mass hold up in the jet; it should be pointed out that there is a direct connection between the jet spread parameter (σ) and the mass hold up in the jet, i.e., the smaller the value of σ the higher is the entrainment rate and a jet which is obtained is spreading faster and has a larger mass hold up. In other words: no jet is possible which entrains fluid at a rate corresponding to e.g., $\sigma = 7.67$ which at the same time has a mass hold up corresponding to $\sigma \neq 7.67$.

In the present work the correction for unsteady effects (the upper line in Fig. 15) based on $K = 0.2644$ (which is very nearly *equivalent to* $\sigma = 7.67$) yields an *over-correction.* This is an indication that the volume v of the *confined jet* "is less than the volume of a free jet of the same length" (as the authors justly state); this is, however, also equivalent to saying that the value of σ of the *confined jet* is larger than that of the free jet.

Although the discusser shares the authors opinion that the computed switching times would increase, when effects of mass storage are taken into account—these unsteady effects cannot be used to account for discrepancies between the measured and the computed initial reattachment distances (since this is a steady state phenomenon). *One has to use, therefore, values of* σ which correspond to the *confined* jet in Lush's experiments, and which are much larger than the values of σ of a *free* jet.

4 The analytical model of the authors assumes the deflection of the power jet by the control jet, but neglects at the same time the deflection of the control jet by the power jet. (See also ASME Paper 71-WA/Flcs-6.) These *two* jets (or the combined jet) must pass through the secondary nozzle (Fig. 18(*a*)) the width of which is reported to be 1.25 $W (= W + 2S)$. It is therefore not surprising that for $q_c > 0.3$ a *vena contracta* occurs in the *power jet*. In this connection Figs. 18(*a*), (*b*), and (*c*) seem very instructive to the discusser. Fig. 18(*a*) shows the approximate velocity profile at the secondary nozzle when $Qc \approx 0.25\ Qo$; the power jet does not have a vena contracta, therefore its width remains approximately W and its velocity $Uc \approx Uo$. Since in this case the control flow of $Qo/4$ has to pass through approximately $W/4$ its velocity equals that of the power jet. We see, thus, that what was actually obtained is a *combined* jet with a *width of approximately 1.25 W* and a velocity of $Uc \approx Uo$.

Fig. 18(*b*) shows the situation for $Qc \approx Qo$; in this case there is a vena contracta in the *power jet*, and the total flow rate (of the power plus the control jet) which has to pass through the secondary nozzle is approximately 2 Qo. The velocity profile in this case will not be much different from that shown in the figure. Here it has been assumed (in this figure) that the spill-out flow through the inactive control port is small. This flow however, depends on the resistance of this port and must not necessarily be small.

Fig. 18(*c*) shows the situation for $Qc < 0.25\ Qo$. From these figures it is clear that the velocity profile *will not be* like the one shown in Fig. 3 of 71-WA/Flcs-6. In the discusser's opinion a *combined jet* approach will therefore yield better analytical results for all cases when Qc is not very small (say: $< 0.15\ Qo$). Of course, it is possible to base the analysis on the power jet, but in this case it is necessary to consider the change in its entraining properties due to the high flow velocities in the bubble (see e.g., Sabin [28]), and consider separately the control jet. The discusser's opinion is that failing to consider a *combined jet* (or the effect of the control jet on the power jet entraining properties as well as the unsteady effects mentioned above) is one of the sources of the discrepancies between Lush's theory and his experimental results.

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Authors' Closure

The authors agree with Professor Sarpkaya that small changes in geometry can produce significant effects on performance, and it was for this reason that conclusion No. 10 stressed that the results of this investigation with respect to the convex-walled amplifier must not be regarded as a general attribute of these amplifiers. They do, however, indicate certain pitfalls which may be encountered in their use.

With respect to Dr. Epstein's first comment it is indeed true that no generalized jet path equation is available at this point, and additional research in this area is desirable.

His second and third comments relate to the appropriate value of spread parameter and to the mass holdup in the jet during the transient. Dr. Epstein states that the jet parameter σ and the jet volume growth rate are uniquely related, and that "no jet is possible which entrains at a rate corresponding to $\sigma = 7.67$ which has a mass holdup corresponding to $\sigma \neq 7.67$". While this statement is certainly true for a free jet with zero longitudinal pressure gradient, it need not be true for a confined jet with an axial pressure gradient. It is perfectly possible for a confined jet to have a smaller volume spread rate than a free jet, but to have the same entrainment. In order to make a fundamental test of the importance of the mass holdup effect, we have applied the theory to predict the maximum switching time, i. e., the switching time when the control flow is removed as soon as the switching process becomes self-sustaining. In this case, all extraneous effects introduced by the control jet are absent during most of the process, which occurs quite slowly, and the mass holdup effects is a minimum. Nonetheless, as shown in Fig. 19, neglect of this effect results in a substantial underestimate of the total switching time. In consequence, therefore, while the authors agree with Dr. Epstein's general remarks in comment 4 that a combined jet including the effects of the opposite wall would provide a better representation of the velocity profile, we still believe that the mass holdup effect is essential for accurate prediction of switching times.

In response to Mr. Drzewiecki's initial comment, it should be pointed out that within the confines of an ASME paper it was not possible to give the information he desires. However, full details of the apparatus and instrumentation are provided in

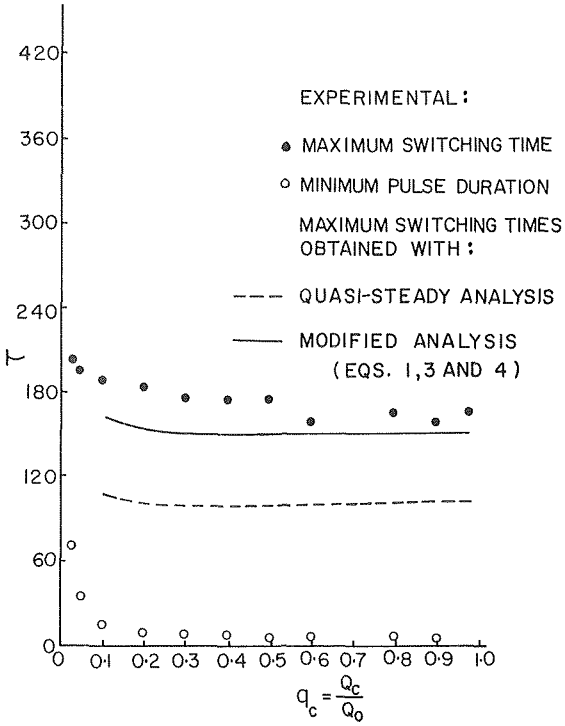

Fig. 19 Experimental and theoretical maximum switching times obtained with the straight walled amplifier

[20] and [21]. With respect to item 1, there were two reasons for covering a large range in control flows. First, while large control flows may not at present be of general interest, it was simple to obtain this information which may be needed at some time in the future. Second, a good theory must be able to predict the behavior at high control flows as well as low control flows, and the experimental data will therefore provide a useful check of new analytical approaches. Since over fifty data points are given in [20] and [21] for $q_c \leq 0.25$ for each configuration and Reynolds number it is difficult to understand Mr. Drzewiecki's request for additional data in this range.

We disagree completely with Mr. Drzewiecki's comments under item 4. Recent tests on our own apparatus with a large opposite wall setback (to produce the end-wall type of switching) and no opposite control port have shown large switching times of the same order as those obtained by Lush. Clearly the difference between switching times with end-wall switching and contacting-both-walls switching is substantial regardless of bias flows. In item 5, Mr. Drzewiecki states that the empirical equation determining the separation point cannot be used for $q_c < 0.2$. This is not correct, although perhaps the absence of data points in Fig. 5 for $q_c < 0.2$ gave him this impression. In fact, the empirical equation is valid for $q_c \geq 0.08$ and the theory should also be applicable for $q_c \geq 0.08$. With respect to item 6, Muller's model had different sidewall setbacks on the two sides. It is for this reason that his data was not compared with the present theory.

The theory represents an attempt to improve fundamental understanding of the type of switching which occurs in real bistable amplifiers which in general have small setbacks. Considering the difficulty of analyzing the complex phenomena which occur in these devices, it appears encouraging to us that an extension of the approach used by Lush and Epstein for the end-wall type of switching should give reasonably good results. Further refinements along the lines suggested by Dr. Epstein are certainly desirable.