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Rebuttal of Denn's Note

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First of all, I thank Professor Denn for paying special attention to my papers of draw resonance study and then presenting his own points on the matter. It is indeed a great pleasure to see my papers induce an R&D note from a respectful professor of chemical engineering and to have an opportunity to respond to his criticism. The answers to the Denn's arguments are shown in the following:

1. Denn argues that "the transformation $x, t \rightarrow x, A(x, t)$ is valid only if $(\partial A/\partial t)_x$ never vanishes, which is of course impossible in a system exhibiting sustained oscillations."

Answer: During draw resonance, $(\partial A/\partial t)_x$ vanishes periodically due to the oscillatory nature of $A(x, t)$ and hence the transformation $(x, t) \rightarrow (x, A(x, t))$ is not a one-to-one correspondence. However, since this is nothing more than the fact that $t(A, x)$ is obviously a multi-valued function in draw resonance, the derivations in the paper using the transformation are all valid.

2. Denn repeatedly argues that I stated the wave velocity, U, is constant in time and space in the case of Newtonian fluids, whereas it is truly not.

Answer: He misunderstood the fact that I never said U is constant in time and space, but only said U is constant along the spin line, meaning it is independent of x, in order to emphasize this nature of U in Equation (13). This becomes important later when U is an explicit function of x for power-law and Maxwell fluids in Part II of the paper. U can never be independent of t, even for Newtonian fluids, because the draw-down ratio, r, is always a function of t in draw resonance. My statement after Equation (13) perhaps caused Denn to misunderstand the true meaning of Equation (13).

3. Denn argues that when I derive that $(\partial V/\partial X)_t$ and $(\partial V/\partial X)_A$ are independent of x, I "fail to recognize that a process with a constant area would have to be described by a different set of forces and a different set of equations."

Answer: This seemingly plausible statement at first glance is really incorrect. The force of the thread-line with a constant area is still represented by the same equation

$$F = \mu A \left(\frac{\partial V}{\partial X}\right)_t$$

and the same continuity equation applies here, too.

4. Denn further argues that $(\partial A/\partial X)_t = 0$ cannot be true to be used in the derivation of $(\partial V/\partial X)_A$.

Answer: As he correctly says, "the only way in which the change in V with X can be evaluated at constant A is to consider adjacent positions having the same area at different times." The fictitious process where A is held constant is nothing but a mathematical procedure to derive the expression of a partial derivative, $(\partial V/\partial X)_A$, where $(\partial A/\partial X)_t = 0$ by the definition of the process.

5. Denn argues that "any stability theory predicting such a 'subcritical' instability must therefore be amplitude-dependent, and my theory does not contain any dependence on amplitude of the disturbance, and it must be incorrect if it can not predict the critical value obtained from the linear stability theory."

Answer: I do not agree that my critical value 19.744 is a subcritical instability. The reason I didn't include amplitude in my study is that Part I and II papers exclusively deal with the critical point of stability where amplitude is asymtotically zero. The subject of draw resonance amplitude will be thoroughly investigated from the viewpoint of the degree of stability/instability with non-zero amplitudes in the Part III of the series. The responsibility for explaining the discrepancy in critical draw ratios between my value of 19.744 and Denn's value of 20.21 lies with Professor Denn. My value 19.744 is the result of analytical procedure while his is obviously not.

6. Denn argues that my conclusion that assumption of zero isotropic pressure (in Maxwell fluids) turns out to be a very good one with negligible error, is quite misleading.

Answer: By comparing Figure 2 in my paper (Hyun, 1978) and Figure 5 of Denn's paper (Fisher and Denn, 1976), it is seen that despite the drastic nature of the assumption, the basic stability diagram of the system shows an almost identical pattern. So the assumption proves to be a good one, in the sense that the physics of the system is kept intact throughout the resulting analytical treatment.

7. Denn shows that the wave velocity, U, is a function of x and t for viscoelastic fluids.

Answer: We agree. As I stated earlier, in my papers, U is a function of t for Newtonian fluids and a function of x and t for power-law and Maxwell fluids, since r is a function of t in draw resonance.

I sincerely hope that I have cleared all the misunderstanding about my papers and that the facts will be clear to all in this field when Parts I, II and III of my paper are read together.

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Digital Control of Time-Delay Processes

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A digital controller for time-delay processes was recently proposed by Gautam and Mutharasan (1978). This note examines the control logic in more detail and compares it with previous work.

THE PROPOSED CONTROLLER

The digital controller of Gautam and Mutharasan can be written for the process system of Figure 1 as

$$m_i = K\ (y^{set} - y_i) + \frac{y^{set}}{K_p} - \overline{L}_{i-(n+1)} \tag{1}$$

where $\overline{L}_{i-(n+1)}$ is an estimated past load, based on an assumed process model. The calculation of this load term, detailed in the cited paper, is based on:

a) Assuming a known first-order/delay model $K_p e^{-\alpha s}/(\tau s + 1)$ for the process transfer function.

b) The delay α defined in terms of the sampling period T as $\alpha = (n + \beta)T$, where n is an integer and $0 \le \beta \le 1$.

c) From the integrated model equation and the output data, it is possible to calculate an earlier input $x_{i-(n+1)}$.

d) Since the earlier controller output $m_{i-(n+1)}$ is known, then $\overline{L}_{i-(n+1)}$ is estimated as $x_{i-(n+1)} - m_{i-(n+1)}$. The input data for this calculation are the present and past samples of the process output, along with previously-generated data.

The first term in the controller equation above is the usual proportional-mode output error term. The gain K is an adjustable controller parameter. Although no tuning guide is given, tuning should not generally be a problem. If the time delay is relatively large, the term can introduce oscillation into the output. This is because the undelayed output is unmeasurable, and the delayed output y_i must be used in the error term.

The second term is an "ideal preload," based on the process gain K_p. This has been discussed by Shinsky (1967) and Bohl and McAvoy (1976) as an improvement over the integral mode for setpoint changes. The improved response of the proposed controller over a PID controller for setpoint changes is probably due largely to this term. If the actual process gain differs from the assumed value (see Gautam and Mutharasan, Figure 4, curve $e_\tau = 0.25$, $e_k = -0.25$), then the speed of response can be affected significantly.

The model-based third term, designed to correct for load changes and model error, is the novel part of the controller. It is evident from Figure 1 that L_i, if known and constant over the sampling period, could be subtracted from the controller output m_i to exactly compensate for the load in a feedforward manner. Since L_i is not known and cannot be estimated, the approximation proposed is to use the estimate $\overline{L}_{i-(n+1)}$ of an earlier load effect. As can be visualized in Figure 2 for the case where $\alpha = 3.3T$, there is no reason to expect the approximation $\overline{L}_{i-4}$ to be a good estimate of the current load, unless the load is constant.

The role of the third term can be better viewed as a feedback mechanism. The present output y_i is used to generate an earlier

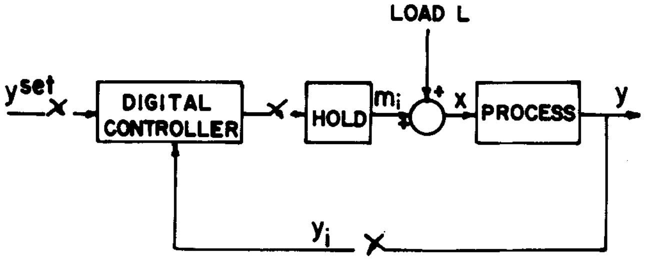

Figure 1. The controlled process system.

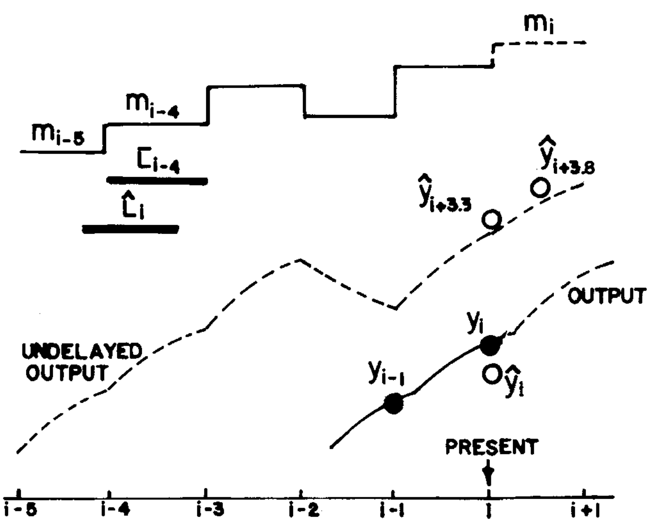

Figure 2. Variables of the two controllers for a time-delay of 3.3 units.

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