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On an Analysis of Draw Resonance by Hyun

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Draw resonance is an oscillatory instability that is sometimes observed in the continuous drawing ("spinning") of polymer filaments. Experimental observations and theoretical understanding of the phenomenon are discussed in recent reviews by Pearson (1976), Petrie and Denn (1976), and Kase and Denn (1978).

An analysis of draw resonance that purports to provide analytical solutions and physical insight has been reported in two papers in this journal by Hyun (1978*a*, *b*). The approach is based on the notion of "throughput waves," and exploits the hyperbolic nature of the governing partial differential equations. Hyun obtains results for the onset of draw resonance that are close to those obtained by other investigators from more classical methods of hydrodynamic stability theory, but there are important unnoted quantitative differences. We show here that Hyun's analysis is incorrect.

CONTRADICTION

For slow speed, isothermal spinning, the equations of conservation of mass and momentum used by all workers are

$$\text{mass:}\quad \left.\frac{\partial A}{\partial t}\right)_x + \left.\frac{\partial Q}{\partial x}\right)_t = 0 \qquad (1a)$$

$$Q = Av \qquad (1b)$$

$$\text{momentum:}\quad F = \text{function of } t \text{ but not } x \qquad (2)$$

It is simplest to restrict the discussion to a Newtonian fluid, for which we have the further constitutive relation

$$\text{constitutive:}\quad F = 3\eta A \left.\frac{\partial v}{\partial x}\right)_t \qquad (3)$$

This system of equations requires three spatial boundary conditions; the important one for our discussion is imposition of a fixed takeup velocity,

$$v(L, t) = \text{constant} \qquad (4)$$

Hyun has rewritten the continuity equation in a form that emphasizes its hyperbolic nature:

$$\left.\frac{\partial Q}{\partial t}\right)_x + U \left.\frac{\partial Q}{\partial x}\right)_t = 0 \qquad (5)$$

It follows from direct substitution that

$$U = \left.\frac{\partial Q}{\partial t}\right)_x \Big/ \left.\frac{\partial A}{\partial t}\right)_x \qquad (6)$$

Hyun has implicitly made the following transformation of independent variables:

$$x, t \rightarrow x, A(x, t) \qquad (7)$$

This transformation is valid only if $\partial A/\partial t)_x$ never vanishes, which is of course impossible in a system exhibiting sustained oscillations. The invalid transformation may be a partial cause of the incorrect solution, but we believe that the root cause is a more serious error discussed later. If we assume however that Equation (7) is valid, then, following Hyun,

$$U = \left.\frac{\partial Q}{\partial A}\right)_x = v + A \left.\frac{\partial v}{\partial A}\right)_x \qquad (8)$$

The second equality in Equation (8) is not used by Hyun, but follows directly from Equation (1b).

The development is now adequate to demonstrate that Hyun's result is incorrect. Through a further series of manipulations on Equation (8) he argues that U is a constant in time and space, given by his Equation (13):

$$\text{Hyun:}\quad U = \frac{v_o(r-1)}{\ln r} \qquad (9)$$

r is the drawdown (area reduction) ratio, which we have generally called D_R and which others have called E.

We now suppose that U is indeed a constant, and show that a contradiction results. Equation (8) can be integrated for constant U to give v as a function of the independent variable A at each value of the independent variable x:

$$v = \frac{v_o(r-1)}{\ln r} + \frac{K(x)}{A} \qquad (10)$$

$K(x)$ is a constant of integration that may depend on x, but not on A. At the takeup, $x = L$, the velocity $v(L, t)$ is a constant, Equation (4), equal to rv_o. Thus,

$$\frac{K(L)}{A} = \frac{v_o}{\ln r}\{r \ln r - r + 1\} \qquad (11)$$

The right-hand side of Equation (11) is a constant, and $K(L)$ must be independent of A. Thus, when $x = L$, A must be a constant; i.e., if Equation (9) is correct, *then the takeup area cannot vary in time and draw resonance*

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is impossible. The remainder of Hyun's analysis follows from Equation (9) and is thus demonstrably incorrect.

ERROR IN LOGIC

A major reason for the incorrect solution, beyond the invalid transformation, appears to be a fundamental error in logic. Hyun further writes Equation (8) as

$$U = - A \frac{\partial v}{\partial x}\Big)_A \Big/ \frac{\partial A}{\partial x}\Big)_Q \qquad (12)$$

He states, "The derivation of $\partial v/\partial x)_A$ requires considering a fictitious process where A is held constant along the threadline." He then uses the fact that $\partial A/\partial x)_t = 0$ in this fictitious process and that F is independent of x, Equation (3), to conclude that $\partial v/\partial x)_t$ is a constant and that v is always linear in x. *He fails 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.*

The partial derivative $\partial v/\partial x)_A$ must be evaluated for physically admissible processes; i.e., for those processes that are described by Equations (1) through (4). 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. In that case, it cannot be true that $\partial A/\partial x)_t = 0$, and the remaining steps leading to Equation (9) have no validity.

INCORRECT CRITICAL DRAW RATIO

Hyun has calculated a critical draw ratio of 19.744 for slow speed, isothermal Newtonian spinning, independent of the magnitude of the perturbation. When his work was first presented at the New York meeting of the Society of Rheology in February, 1977, we observed that this value differed from the critical value of 20.21 reported for the onset of instability to infinitesimal perturbations by all previous workers (a partial list includes Kase et al. 1966, Pearson and Matovich 1969, Gelder 1971, Kase 1974, Fisher and Denn 1975). Therefore, we questioned whether the result could indeed be a necessary and sufficient condition for instability, as claimed then and in the published work. In the discussion following the public presentation, the difference between 19.744 and 20.21 was attributed to computational difficulties in determining the latter value; in the published papers the difference was not discussed at all.

There is an analytical solution for the frequency response of slow speed, isothermal Newtonian spinning that reduces the problem to either a straightforward quadrature (Kase et al. 1966, Kase 1974) or to tabulated sine and cosine integrals (Pearson and Matovich 1969). The latter result is given explicitly in Kase and Denn (1978). The quadrature is integration step size convergent and has been checked against the sine and cosine integral formulation; the critical draw ratio computed in this way is 20.218. There is no possibility that a critical draw ratio as low as 19.744 can be correct for infinitesimal perturbations.

It is important here to understand just what can and cannot be learned from linear stability theory. Linear theory cannot exclude an instability at a draw ratio below 20.21, but such an instability would have to be of finite amplitude. Any stability theory predicting such a "subcritical" instability must therefore be amplitude-dependent. Hyun's theory does not contain any dependence on the amplitude of the disturbance, and it must therefore be incorrect if it cannot predict the critical value obtained from linear stability theory.

All of the above comments apply to the analysis of a viscoelastic Maxwell liquid (Hyun 1978*b*), but some further observations are required here. Hyun's analysis is not really for a Maxwell fluid, but for an approximation in which the isotropic pressure has been neglected. This approximation has been fully studied by Zeichner (1973), and some of his results are reported in Denn (1975). The approximation gives steady state spinning forces that differ by up to 50% from the exact solution for a Maxwell fluid (Denn et al. 1975). This should not be surprising, since the neglected term contributes one third of the total stress in the limit of a Newtonian fluid. Hyun's conclusion that "the assumption of zero isotropic pressure turns out to be a very good one with negligible error" is quite misleading.

Hyun has compared his stability calculations to those of Fisher and Denn (1976) and concluded that they are "almost identical." Fisher and Denn solved the complete Maxwell fluid equations. The correct comparison is with Zeichner (1973), who considered the same set of equations as Hyun. Zeichner's stability calculations are given as Figure 12.2 in Denn (1975). In some instances they differ from Hyun's by more than 35%, which is an even greater error than for the Newtonian fluid.

CONCLUSION

Draw resonance is seen from a control-theoretic formulation to be a consequence of an over-compensating feedback control system to maintain constant takeup (Kase and Denn 1978). This mechanism is not incompatible with Hyun's notion of reflected throughput waves, nor does the former follow from the latter. The observation that the equations are hyperbolic is, of course, not new, but the wave velocity is a function of space and time and cannot in any event be calculated by the method proposed by Hyun. The wave velocity can be calculated in the neighborhood of the critical draw ratio using estimates for v and A given by Fisher and Denn (1975, 1976), but this method requires prior solution of the hydrodynamic stability problem in order to obtain the eigenvalues and eigenfunctions.

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NOTATION

A = area
F = force
K = integration constant
L = length
$_o$ = value at $z = 0$
Q = flow rate
r = draw ratio
t = time
U = wave velocity
v = velocity
x = distance
η = viscosity

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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.