LETTERS TO THE EDITOR

TO THE EDITOR:

In a recent article Johns (1972) refers to an earlier paper by Han (1971). Although Johns correctly pointed out that the particular flow field considered by Han is viscometric, his subsequent discussion is incorrect. This letter is intended to point out some of his inaccuracies.

First of all, Johns' Equations (3a) to (3c) are incorrect. They should read, with his notation:

$$\mu = \eta_o \frac{1 + \dfrac{2}{3}\lambda_1\lambda_2 |\nabla f|^2}{1 + \dfrac{2}{3}\lambda_1^2 |\nabla f|^2} \quad (1a)$$

$$\sigma_1 = 0 \quad (1b)$$

$$\sigma_2 = \frac{2\eta_o (\lambda_1 - \lambda_2) |\nabla f|^2}{1 + \dfrac{2}{3}\lambda_1^2 |\nabla f|^2} \quad (1c)$$

The above expressions were first derived by Williams and Bird (1962). Unfortunately, Johns obtained his Equations (3a) to (3c) by setting $V_{z,x} = 0$ in Han's Equations (9) to (14), which inadvertently contained some algebraic errors.

Therefore, a comparison of Equations (1a) to (1c) given above and Johns' Equations (3a) to (3c) shows clearly that wrong equations were derived and wrong conclusions reached.

Whereas Han (1971) referred to those theoretical studies (Ericksen, 1956; Green and Rivlin, 1956; Langlois and Rivlin, 1963) which predicted the possibility of having secondary circulating motion when viscoelastic fluids flow through noncircular ducts, he also noted that there was little experimental evidence to conclusively support the theoretically predicted behavior in the case of polymer melt flow in particular. Viscoelastic though they may be, it is not apparent why polymer melts should exhibit the same flow instabilities as polymer solutions do. It would appear that whether or not a particular flow instability may be observable for both polymer melts and solutions depends sometimes on the fluid velocity and other times on the stress level, depending on which of the rheological or flow variables is primarily responsible for its happening. For instance, one of the most conspicuous flow instabilities, the commonly known *melt fracture*, has been observed only in polymer melt flow, so far, although the fluid elasticity is believed to be responsible for its occurrence. At the same time it is generally agreed among researchers that shear stress, but not shear rate, is the controlling variable for the onset of melt fracture. In other words, polymer solutions can never give rise to such high values of shear stress (say, above 1 million dynes/cm^2) as polymer melts can.

In recent years this writer has carried out three different kinds of experiments which should be able to demonstrate the onset of secondary circulating motion, if noticeable, in the flow of polymer melts through rectangular ducts.

The first kind of experiment conducted was the extrusion of blends of incompatible polymer systems. Figure 1 shows a representative microphotograph of the extrudate cross section in which the white portion is polystyrene and the black portion is polypropylene. Details of the extrusion experiment and photographic technique are given in a recent paper by Han and Yu (1971). It is clearly seen in Figure 1 that there is no evidence of a secondary flow pattern, as theoretically predicted.

The second kind of experiment carried out was the measurement of stress-birefringent patterns, and the third kind was the measurement of local velocities of tracer particles suspended in the flowing polymer melts, using the technique of streak photography. Polymers tested were polystyrene, polypropylene, and high density polyethylene at various temperatures and at widely varying flow rates. In both of these experiments, transparent test cells of rectangular cross section were constructed with a fused quartz of special quality, which can withstand temperatures up to 300°C and pressures up to 35 Kg/cm^2 (500 lb./sq. in.). Note that before the data were recorded by photography a great deal of effort had been put into making all possible visual observations. In no case was secondary circulating motion observed in these experiments. Details of these experimental studies will be reported in a future publication (Han and Drexler, 1972).

These apparent failures to detect secondary flow patterns are not too surprising when one realizes how slow, in terms of Reynolds number, the flow of polymer melts is. Because of extremely high viscosities, polymer melts often give rise to Reynolds numbers less than 0.01. In this context, it is important to note an earlier theoretical study by Langlois and Rivlin (1963). Using the general constitutive equations advanced by Rivlin and Ericksen (1955), they discussed the slow flow of a viscoelastic fluid along a straight, noncircular tube and showed that secondary flows would not arise until the fourth-order approximation. Therefore, based on the experimental evidence available in the literature, it can be said that in the flow of polymer melts through noncircular ducts, the possibility of having secondary circulating motion is very small, and this is attributable to the extremely slow motion characteristic of polymer melt flow.

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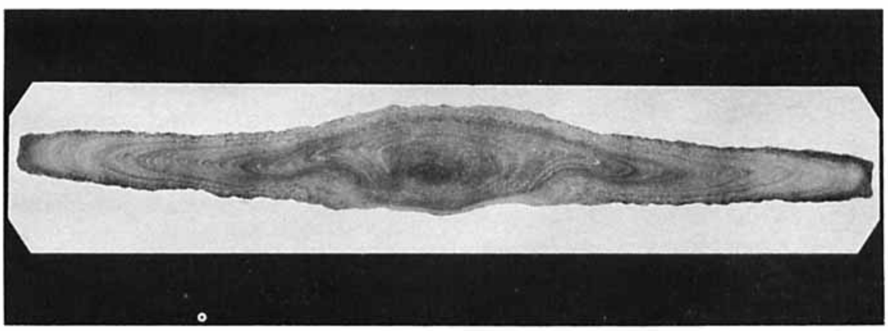

Fig. 1. The extrudate cross section for 80 wt.% polystyrene/20 wt.% polypropylene blend at 200°C.

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TO THE EDITOR: ON MOTIONS IN RECTANGULAR CYLINDERS

The two sides of the dispute over Han's 1971 paper have been set forth in Johns (1972) and Han (1972). Part, but not all, of the difference of opinion arises because Equations (9) to (12), (15), and (16) of Han (1971) used in Johns (1972) are now said to be in error. Thus, it need be added that given the stress system reported in the erratum, the calculations of Johns lead to the conclusion that unidirectional flow of a Williams-Bird fluid in a cylinder of arbitrary cross section is completely compatible with the dynamic equations; otherwise the paper of Johns is correct as it stands. The material in Han (1972) on instabilities has nothing to do with the dispute. At the small velocities under investigation, the secondary component in question is not to be thought of as arising because the straight-line candidate for the primary flow is unstable but rather because the straight-line candidate is incompatible with the dynamic equations. Thus, the material regarding instabilities, such as melt-fracture, etc., is not in the least pertinent.

In a larger sense there is an important distinction that is difficult to get out of either Han (1971) or Han (1972)—the distinction between the fluid itself and the rheological model. Its importance can be understood most easily by following the line of thought that led to the paper of Johns (1972).

The pretext of the paper of Han (1971) is that it is a pioneering experimental study of a nonviscometric motion. However, of necessity, several postulates must precede the analysis of the experimental data. The first, the postulate of a straight-line or rectilinear velocity field, which must certainly be viscometric, directly contradicts the purpose of the investigation. This is the central point of Johns (1972) and is conceded by Han (1972). The second postulate, the Williams and Bird model, reflects the taste of the rheologist and is presumably made because of a certain feel the rheologist has for the material and for the responses predicted by the available models. Having found the theoretical part of Han (1971) to be inconsistent with its objectives and the analysis of the data to be inappropriate, nonetheless, I thought that it was still possible that the experiments themselves were giving pressure differences in a secondary flow field. At this point, however, there's not much that can be done outside of a simple calculation to see what would happen if the material really were a Williams-Bird fluid. It is easy to see if a Williams-Bird fluid will or will not go through a cylinder in straight-line motion, all that are needed are the three material functions and a result of Ericksen. Now it happens that there is a limiting case of the motion postulted by Han (1971) that is viscometric in the coordinate system use there. Thus, I set $v_{z,x} = 0$ and got what I thought were the material functions for a Williams-Bird fluid and concluded that inasmuch as $\frac{\sigma_1}{\mu|\nabla f|^2}$ came out nonconstant that the experimental motion may have a secondary component and that the pressure differences may very well be reflecting that transverse component. Thus, I concluded that the experimental part of the paper might be of use, that is, that a small perturbation of the straight-line motion most likely manifested itself under the experimental conditions.

It happens, however, that the stress components originally listed by Han were not correct. Setting $v_{z,x} = 0$ in the equations of the erratum, I now find that the material functions are not those of Johns (1972). In fact, it turns out that $\sigma_1 = 0$, which is a sufficient condition that a Williams-Bird fluid go through a rectangular cylinder in straight-line motion. Thus, a dilemma arises. Either a secondary field exists or it does not, although it might be there but be too small to see. If it is there, the Williams and Bird model is a poor choice for the system studied but if it is not, then the original investigation is not a pioneering study of nonviscometric flows but merely another study of straight-line motions of ill-defined materials, the motivation seemingly being a lack of understanding. Inasmuch as the secondary component, if it exists, seems likely to be small, it is unlikely that the dilemma can be resolved. Nonetheless, though the sensitivity of the stress system to a small nonparallel component of the velocity field is not in general known, the amplification is not overly important. The question is not one of smallness, it is one of existence. Thus, if the flow is straight-line, the material functions of Williams and Bird can be used in the analysis of the data, even for flow in a rectangular cylinder inasmuch as the surface of the cylinder is a coordinate surface in the coordinate system suggested by Johns (1972); if the flow is not straight-line, they cannot be used. There is, in fact, no basis for their use as an approximation to an exact analysis of the experimental data even if the flow has but a slight nonstraight-line component. Of course, what has just been said also holds for the extension of any straight-line based formulae to nonstraight-line motions, such as the formulae of Han (1971), and is, in fact, the concluding point of Johns (1972).

Finally, other sources of perturbations, for example, roughness, axial curvature, etc., might be expected to interdict the use of formulae proceeding from the assumption of a straight-line motion. The impact of such perturbations on rheologically complex systems where the balance of transverse forces is critical and highly sensitive to the constitution of the material is largely unexplored.

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TO THE EDITOR: ON COMPARISON OF OPTIMIZATION METHODS

In a recent communication Jaspan and Coull (1972) presented a comparison of optimization methods in establishing the optimal control of a tubular reactor whose behavior is given by two nonlinear ordinary differential equations.

In view of the significant improvement to control vector iteration (CVI) methods for the optimization of chemical engineering systems as proposed by Rao and Luus (1972), we would like to supplement some of the statements made by Jaspan and Coull (1972) with reference to the system reported by them.

Jaspan and Coull stated that the initially guessed control policies as given by Fine and Bankoff (1967) and Lee (1968) are the only two initial control policies that will give convergence to the optimum when CVI method is applied. Using the proposed algorithm of Rao and Luus, a constant temperature initial control policy

$$u^{(0)}(t) = K, \quad 0 \leq t < 10,$$
$$250 \leq K \leq 380$$

is found to be successful to give convergence to the optimum. While using the initial control policy of Lee, the algorithm of Rao and Luus gives convergence to the optimum $x_2\ (t_f) = 0.6801$ in 8 iterations and 1.3 seconds of computation time using IBM 370/165 computer (integration time step = 0.1). The method is thus more efficient when compared to the BCI, GBCI, and Horn's method (Jaspan and Coull, 1972.). The significance of the method of Rao and Luus (1972) lies in the systematic approach to determine the stepping parameter ϵ with respect to the system characteristics. For this system, the stepping parameter so determined is found to be in the range of 10^2 to 10^4. With such a systematic way of obtaining ϵ, we should further state that the choice of stepping parameter is no longer an art.

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TO THE EDITOR:

On reading Professor McGee's editorial in the November 1971 issue I was impressed by his statement that the *Journal's* contents "are not calculated to excite anybody." The reason for being impressed was that such a statement seemed to be an appropriate criticism for a publication like, say, Playboy, but it appeared to me as rather irrelevant as applied to the *Journal*. I read the *Journal* when I want to learn something basic about chemical engineering, I do something else when I want to get excited.

Yet this seems to be a very personal attitude, since Dr. Christensen (Jan. 1972, p. 256) advocates "stimulating articles," and Dr. Chase (May, 1972, p. 669) also seems to desire a Playboy-type *Journal*, and yearns for more photographs—perhaps a central folder on crystallinity of polyethylene would just be ideal.

I wonder whether these are the standards by which the *Journal's* contents should be judged. McGee's statement that the predominant attitude of practicing engineers toward the *Journal* is "one of overwhelming disinterest" is a serious one, and, if true, the reasons of such a situation should be investigated. If the material published on the *Journal* is irrelevant to chemical engineering, the Institute should revise its standards for accepting papers.

If on the other hand, practicing engineers fail to see the relevance of, for example, research in heat transfer (a subject McGee seems to dismiss) or on cryobiological preservation (a subject preferred by McGee), then the Institute should revise its standards of acceptability of individuals to the profession.

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BOOKS (continued from page 1282)

the sticky valves in his sports car or the fouled heat-exchange surfaces that raise his plant operating costs.

The latter hard surface detergency aspects and the related topics of emulsions and foams are *not* included in the present book (although chapters on Dishwashing and Cleaning of Metals are to appear in a second volume). Essentially, the ten authors of the present volume discuss the attachment, removal, and redeposition of soils upon organic fibers and fabrics, and the very numerous methods which have been used in studies of these processes. W. C. Powe adds an informative chapter on the origin and composition of laundry soils.

The theories of particulate soil adherence and removal in terms of the van der Waals and electrical double-layer forces are presented in a straightforward fashion by H. Lange (actually an abridgment by the editors of an earlier publication). Hans Schott of Temple University contributes two chapters on the removal of particulate and organic soils. These briefly consider Zisman's extensive work on wettability of surfaces, the rolling-up of oily deposits, and the various surface interactions of soil, fiber, fabric and surfactants. He includes a wealth of theory and observations from 200 references. I feel that a wrap-up section by Schott summarizing this material would have strengthened the book considerably.

Overall, the book's treatment of its subjects is simple and reasonably up-to-date with references up to 1970. Illustrations vary in quality from excellent to mere photos of boxes with knobs. Editing appears to have been done quickly, in that chapters on test methods and equipment overlap in coverage, and several errors and unclear statements remain.

The information and bibliographic references in the book should help engineers who are involved with fiber technology and with laundry problems. For the rest of us, it is a humbling reminder that surfactants and fiber surfaces are still difficult to characterize, that the process of cleansing is far from simple, that the judging of its effectiveness involves most of the difficulties of appearance-measurement, and that even the simulation of the dirt is controversial. As Davis aptly says, "One of the chief needs of detergency research now is to find a realistic soil upon which all laboratories can agree and seek a common ground."

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