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Dr. Walczak's response to these comments prompts me to add the following remarks. The third paragraph of his reply needs to be considered in the light of his statement in the introduction of his first paper[1] that no systematic treatment of the effect of mechanical perturbation on polymer melt behavior has been reported in the literature. I feel he is mistaken in this view and wish to reiterate that refs. 5, 7, and 9 make it perfectly clear that shear pretreatment changes a polymer's melt flow characteristics. Die swell (and hence end correction and apparent viscosity) is changed markedly by shearing level as measured by total shear strain and the recovery of properties after shearing is a function of both time and polymer viscosity level (ref. 7, p. 112). Further, contrary to the implication in Dr. Walczak's response, I have not maintained that his shear history parameter was used previously. What I do maintain is that some progress towards a theoretical understanding of Dr. Walczak's results might be achieved by considering his results in terms of earlier work on the effect of mechanical perturbations on viscoelasticity, specifically the use of total shear strain as a variable.

I do not agree with Dr. Walczak's fourth paragraph that the verbal interpretation of the apparent dependence of the critical shear rate is a matter of preference. In my view his Figure 9[1] is wrong. To plot this way distorts the real situation and can lead to incorrect conclusions. I personally made such an error[15] on the basis of plots similar to Dr. Walczak's Figure 9, concluding incorrectly that acceleration effects were important in the entry melt fracture phenomenon. My intention was *not* that attributed to me by Dr. Walczak, namely to avoid a "conflict of authors' rights."

I disagree with paragraph six of Dr. Walczak's response and his interpretation of the Berens and Folt result. I have seen equivalent results in cases where there were no surface active agents or other foreign material present.

Dr. Walczak's paragraph seven misses the point of my comments. I was supporting his position.

I would add to Dr. Walczak's final paragraph the experimental observation that the dead space size depends not only on shear rate but also, above the inlet melt fracture point, on time.[13] As noted by Bagley and Birks,[13] "Since material from the dead space is now surging intermittently through the capillary, the dead space must decrease in size." Eventually, the dead space "has been reduced in size so far that it cannot surge over far enough to reach the capillary and hence the dead space size remains constant."

In summary, I hope this discussion will serve to stimulate publication of more data and information in this scientifically interesting and industrially important rheological area.

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Post Scriptum

From the exchange of the published and unpublished notes, it is evident that Dr. Bagley holds different views and takes another approach to the shear history question. Obviously, it is not possible to reach any common denominator by means of written words. Maybe, some discussion at an appropriate occasion would permit to resolve the differences. For the time being, I am accepting a substantial disagreement.

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