pressure region. The lubricant behaved like a solid, not only between the input and output bars, but also along a part of the contact area between the bars and the collar. This could be determined because the momentum loss increased as the oil layers between the bars decreased until suddenly at zero lubricant thickness between the bars the momentum loss disappeared.

The same phenomenon is probably causing the jump in the measured data in Fig. 11, and the too high value of the stiffness for the lubricant found in the Kolsky experiment.

The author states that the techniques used in the experiments do not show any sign of transition into a glassy state. I think that the opposite is clearly shown by the jump in the measured data at about 10 percent compression. This also shows that all plate compression experiments were conducted at pressures far above the glass transition pressure. I should like to have the author's comments about why only 5P4E was used in the experiments.

If a polyalphaolefin had been used, the volume ratio at the glass transition should have been about 0.8, and the change in behavior at that pressure should have been more clearly seen.

Further on in the author's discussion of reference [14], a misunderstanding of the value of some words is obvious. As long as the lubricant is liquid the molecules are not tightly packed. By definition the molecules have space enough to pass each other and fill a container to its full form.

When the liquid is compressed, the free volume needed for the motion of the molecules decreases until the molecules are not able to move past each other any longer. If this compression is fast compared to the relaxation time of the compressed molecules, the lubricant will be compressed into a glassy solid state. For normal lubricating oils working at room or elevated temperatures the compression needed will be in the range 10 to 30 percent or, using the nomenclature of this paper, the volume ratio will be 0.9 to 0.7 to get into the glassy state.

Additional Reference

Jacobson, Bo, 1974, "An Experimental Determination of the Solidification Velocity for Mineral Oil," *ASLE Transactions*, Vol. 17, No. 4, pp. 290–294.

Author's Closure

The author would like to express his appreciation of the very interesting discussions by Prof. Jacobson and by Drs. Bair and Winer, and chooses to comment on the discussions in that order.

In the particular case that $\bar{\eta}=0$ in the Jacobson and Vinet (1987) model, equation (20) does indeed become essentially the same as the Hugoniot-derived relation presented in the paper, with the additional restriction that $a=1$ in the latter relation. We note that while $a=1$ appears to provide a reasonable description of the nonlinearity for 5P4E, there is no reason to expect that this will be so for other EHD lubricants. We will soon have information on other lubricants (Feng and Ramesh, 1991) that should help pin down the model.

As far as the glass transition is concerned, the author agrees that there is in fact evidence of a change in behavior in the final pressure/volume-ratio curve (Fig. 5 in the paper). The results presented in the discussion by Bair and Winer indicate that this change is associated with the glass transition. However, what the author wished to point out in the original paper is that a change in behavior at the glass transition pressure is not observed in any *single* Kolsky bar test, unlike the distinct change in slope observed (for example) in a given dilatometric measurement. This is perhaps to be expected, since in general a single Kolsky bar test does not provide a pressure/volume-ratio curve in itself, but only *one* point on a plot such as Fig. 5; further, the risetimes associated with the pulses may mask any change in behavior.

We are currently investigating other lubricants, including a mineral oil; our studies on 5P4E are more extensive, in part because there is a great body of information on this lubricant. The results on other materials will be presented in a forthcoming paper (Feng and Ramesh, 1991).

The author would like to thank Drs. Bair and Winer for presenting the pressure-volume curve from the dilatometric measurements, and agrees with the discussers in that it appears that evidence of a structural relaxation in compressibility is absent for the time scales investigated. Further, their data appear to show quite clearly that the knee in the Kolsky bar data is associated with the glass transition. The author would also like to comment on the effect of temperature on the scatter in the Kolsky bar results, in that we have since learned that the lubricant initial temperature may have varied by as much as 3°C during the tests presented in this paper (largely as a result of handling). More accurate results, at fixed initial temperatures both above and below the glass transition temperature at ambient pressure, will be presented in the paper by Feng and Ramesh.