

**Abulafia *et al.* Reply:** Klein *et al.* [1] agree with the main point of our Letter [2], namely the observation of a crossover from elastic collective creep to plastic creep in YBaCuO (YBCO). However, they criticize the use of Eq. (1) in describing the *plastic* creep and rather propose to adhere to Eq. (2) which originates from the theory of *elastic* collective creep. To justify this approach they introduce a new term of “plastic collective creep” which so far has no published theoretical basis. Thus, it is not clear whether this approach can explain the details of our experimental results at high fields, especially the decrease of the exponent  $\mu$  and the activation energy  $U$  with the induction  $B$ . In the absence of a known theory for the dependence of  $U$  upon  $j$  for plastic creep in the limit  $j \rightarrow 0$ , we adopted Eq. (1) to describe the situation far from that limit. The question of diverging barriers in the plastic creep regime in the limit  $j \rightarrow 0$  is of significant interest, however its resolution has to await more detailed theoretical and experimental investigations.

Careful reading of our Letter clearly shows that we realize that Eq. (2) can mathematically be fitted to our data. In fact, the inset of Fig. 2 in our Letter shows the results of such fits, namely a strong decrease of  $\mu$  with field above the peak, tending towards approximately 0.2. This anomalous field dependence of  $\mu$  was one of the reasons which led us to abandon the elastic creep model at high fields. In their Comment, Klein *et al.* repeat this analysis and consent that the anomalous behavior of  $\mu$  is not consistent with the elastic model. Nevertheless, they choose to adopt Eq. (2) on the basis of the quality of the fit to data digitized from Ref. [2]. However, as demonstrated in Fig. 1(a), our original data for these and other fields, can be reasonably described by Eq. (1). Apparently, fits are rarely unique and, one can also obtain good fits for the same data using other expressions, e.g., the logarithmic barrier  $U = U_0 \ln(j_0/j)$ , as shown in Fig. 1(b). For obvious reasons we prefer the fit which is based on a physical model.

Klein *et al.*'s suggestion to include all the  $B$  dependence in the exponent  $\mu$  may sound interesting. However, the key question is what is the physical basis of such a formula and what is the physical significance of  $U_0$  and  $j_0$  being field independent. Clearly, this is against the spirit of the collective creep model which predicts strong field dependence of  $U_0$  and  $j_0$  in the relevant regimes of small and large bundles. Without describing the physical model which leads to such crucial modifications, their treatment becomes just a mathematical exercise.

Finally, we note that the cited model of Ertas and Nelson [3] gives no predictions for the behavior of  $\mu$ . Furthermore, this model relates to the *sharp* “fishtail” peak in BiSrCaCuO [4], which may be of entirely

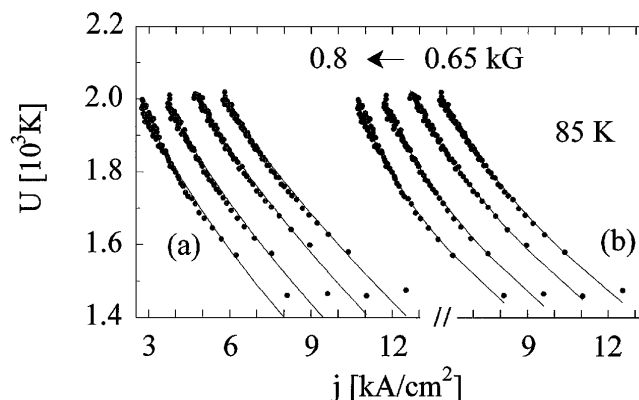


FIG. 1.  $U$  vs  $j$  curves in YBCO for 0.65, 0.7, 0.75, and 0.8 T. The solid lines in (a) and (b) are fits to Eq. (1) of the Comment [1] and to a logarithmic diverging barrier, respectively.

different origin than the *broad* peak of YBCO [4]. The transition to the disordered solid, as implied by Klein *et al.* citing Ref. [3] is expected to occur in YBCO at fields  $\sim 10$  T, therefore it is not relevant to our case.

In summary, Klein *et al.* agree with our data and conclusions, and their analysis based on Eq. (2) was already described in Ref. [2]. The only new point in the Comment is the introduction of a new term of plastic collective creep based on as yet unknown model. When this model is developed and published it would be possible to judge its validity and relevance to our work.

Y. Abulafia,<sup>1</sup> A. Shaulov,<sup>1</sup> R. Prozorov,<sup>1</sup> Y. Wolfus,<sup>1</sup>  
L. Burlachkov,<sup>1</sup> Y. Yeshurun,<sup>1</sup> E. Zeldov,<sup>2</sup> V. Vinokur,<sup>3</sup>  
and V. B. Geshkenbein<sup>4</sup>

<sup>1</sup>Department of Physics  
Bar Ilan University  
Ramat Gan 52900 Israel

<sup>2</sup>Condensed Matter Physics  
The Weizmann Institute  
Rehovot 76100 Israel

<sup>3</sup>Argonne National Laboratory  
Argonne, Illinois 60439

<sup>4</sup>Theoretical Physics  
ETH, Zurich, Switzerland

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