Reply to "Comment on 'Portevin–Le Chatelier effect' "

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We address the preceding Comment [Phys. Rev. E **65**, 053501 (2002) of Kubin *et al.*]. We show that there are no inconsistencies in our experiments, simply variations in our results due to variations on the order of 1% in the widths of our samples. We note that our model, with five adjustable parameters, does not attempt to reproduce all features of their model, which has 16 free parameters. We record their model with a complete set of parameter values for posterity. Previously published numerical solutions of their model are incorrect but we have identified and corrected the errors, and have verified that the two models make identical predictions for the velocity selection of Portevin–Le Chatelier fronts.

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We set out a number of years ago to investigate Portevin–Le Chatelier fronts. We learned about the questions in this area from some of the authors of the Comment and their collaborators. We performed a series of experiments, and found a phenomenological model that explained the results in simple ways. We were able to find analytical expressions for quantities that had appeared mysterious, such as front speeds, front widths, and the dependence of these quantities on experimental parameters [1].

In writing a Comment on our paper, Kubin *et al.* [2] criticize us in two ways. They find inconsistencies in our experimental results, and they find that our theory is not ''state-of-the-art.''

The criticism concerning experimental inconsistency needs only a brief reply. As Kubin *et al.* point out, the load levels for band velocities of 1300 μm/sec differ between our Figs. 10 and 11. The load levels differ by about 1%. We have reported forces, not stresses. The samples sent us by ALCOA vary in width by around 1%, which leads to load variation of that order. That is one reason for error bars in Fig. 10; the value we report there after averaging over around ten samples is 1135 ± 6 N, so the value of 1125 N in Fig. 11 is quite consistent with what we report in Fig. 10. They also express doubt about whether our observed bands truly move continuously. Video analysis of many different fronts confirm the continuity of our front motion.

The criticism by Kubin *et al.* of our theory requires a longer discussion. We learned several things in the course of writing this Reply, including the fact that we did in fact make a mistake. Our mistake resulted from not understanding why most of the published discussions of Portevin–Le Chatelier front dynamics were wrong.

As we performed our experimental work, we fully intended to employ the equations for Portevin–Le Chatelier fronts proposed by Kubin, Estrin, and others (Refs. [3–9]). Since several variants of the model by Kubin *et al.* appear in the literature, since the authors have never explicitly published their results in the form of an equation of motion, and since parameter values must be tracked down through several nested references, we think it may be helpful to record the results in one place.

The spatial variables are plastic strain $\epsilon(x,t)$ and an activation time $t_a(x,t)$. The system is pulled by a machine of compliance C at rate v. The evolution equations are

$$\frac{\partial\epsilon(x,t)}{\partial t}=\dot{\epsilon}_0\exp(\{\sigma(t)-\sigma_f(\epsilon)+D\nabla^2\epsilon\}/S_i(\epsilon)-P_1\{1-\exp[-K_3\exp(-Qn/k_BT)\epsilon^{mn}(t_a/K_2)^{2/3}]\}), \quad (1)$$

$$\frac{\partial t_a(\epsilon,\dot{\epsilon})}{\partial t}=1-t_a/t_w(\epsilon,\dot{\epsilon}) \quad (2)$$

where

$$S_i(\epsilon)=S_{i0}+S_{i1}\sqrt{\epsilon},$$

$$t_w(\epsilon,\dot{\epsilon})=t_{w0}+t_{w1}\epsilon^{\beta}/(\dot{\epsilon}^2+\dot{\epsilon}_1^2), \quad (3)$$

$$\sigma_f(\epsilon)=\sigma_{f0}+\sigma_{f1}(1-\exp[-\epsilon/\epsilon_{\sigma_f}]), \quad (4)$$

$$\sigma(t)=\frac{1}{C}\left[\frac{vt}{L}-\frac{1}{L}\int_0^L dx\,\epsilon(x,t)\right], \quad (5)$$

with parameter values given in Table I. Apart from clearly determined quantities such as the length of the sample L, there are 16 free parameters.

In pinning down the model, we found the paper by McCormick and Ling [3] most helpful. That paper does describe traveling fronts produced by Eqs. (1), and shows pictures from a numerical study. We reproduced their numerical results, but found that they had no relation to experimental

TABLE I. Parameter values for model of Kubin, Estrin, Ling, and McCormick.

$\dot{\epsilon}_0$ (sec^{-1})	1
D (MPa mm^2)	1000
P_1	12.07
K_3	1700
Q (eV)	0.59
n	1/3
$k_B T$ (eV)	0.0259
m	1
K_2 (sec)	2.16×10^{-3}
S_{i0} (MPa)	0.41
S_{i1} (MPa)	2.9
t_{w0} (sec)	3.6×10^{-5}
t_{w1}	2.16×10^{-3}
$\dot{\epsilon}_1$ (sec^{-1})	10^{-16}
β	0.68
σ_{f0} (MPa)	38
σ_{f1} (MPa)	130
ϵ_{σ_f}	0.056
C (MPa^{-1})	4×10^{-5}
L (mm)	100
v (mm/sec)	varies, typically 10^{-5}–10^{-2}

reality. The traveling fronts produced by those authors required a strain gradient to be placed in the sample as an initial condition. Our experiments had no strain gradient initially. When we removed the strain gradient from the initial condition, no fronts propagated.

It was at this point we made our mistake. We concluded from the numerical trials that the equations of Kubin *et al.* do not support traveling fronts. However, the difficulty lay in too completely reproducing prior results, including errors. McCormick and Ling [3] represented their sample with a grid of $N=250$ points. We copied them, but this value for N leads to numerical artifacts. The strain diffusion constant D produces a natural length scale $\sqrt{D/\sigma_f}$. This length scale must be much larger than the grid spacing. So, for a grid of $N=250$ describing a 10 cm sample, one needs D on the order of 10 000, which is larger than any values given in the literature. Alternatively, one has just to take a smaller grid spacing. Upon making either of these choices, one does obtain smoothly traveling fronts from Eq. (1).

In writing our Comment, we developed a different phenomenology that stripped most of the complexity out of the model described by Eq. (1), reducing the number of free parameters from 16 to 5, and choosing simple forms for terms that made analytical solutions possible. We deliberately discarded terms involving temperature dependence because we wanted to focus on the relation between front motion and loading conditions.

Many of the main results in our analysis are independent of model details. For example, front speed v_f in an initially uniform sample, which for years was claimed to be a mystery, is simply given by

$$v_f = \frac{v_{\mathrm{end}}}{\delta\epsilon}, \tag{6}$$

where v_{end} is the speed of the end of the sample and $\delta\epsilon$ is the strain jump across the front. The numerical solutions of Eq. (2) obey this relation too; for the parameters given in Table I $\delta\epsilon \approx 2\times 10^{-3}$. It is also easy to calculate front speeds in samples with strain gradients.

We learned recently that Haehner, Ziegenbein, and Neuhaeuser [10] have arrived at these same conclusions, that they have carried out experiments more comprehensive than the ones we have published, and that they have analyzed a variant of Eq. (1) along the lines we have just outlined, finding analytical expressions for $\delta\epsilon$ and many other things as well [11].

Therefore, we agree with Kubin *et al.* that the problem of velocity selection for Portevin–Le Chatelier bands is solved, and that the best recent expressions will even explain front speed and width in terms of temperature and other parameters. Nevertheless, this work is very recent, and most of the details are still unpublished. We agree on many essential points, with our model being simpler, and theirs providing a more detailed understanding. It does appear that quantitative agreement between theory and experiment for Portevin-Le Chatelier bands has only now been achieved.

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