REPLY TO KARPITSCHKA ET AL.:

The Neumann force balance does not hold in dynamical elastowetting

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In their letter, Karpitschka et al. (1) discuss our claim that the predictions of our theoretical description for the spreading of a droplet on a soft solid layer based on a global-dissipation approach (2) differ from the outcomes of Karpitschka et al.'s model based on a local-force-balance analysis (3). In particular, Karpitschka et al. (1) claim that our conclusion results from a misstep in our calculation. We explain here the motivations behind our approach and why we think that their model is different from ours, the latter being the only one able to reproduce our extensive set of experimental data.

Let us state first that we agree with Karpitschka et al. (1) that "models based on energy dissipation or on force balance" should be "equivalent." However, contrary to their claim, the dissipation power P_{visc} due to viscoelastic stresses cannot be transformed into a contour integral in general. In particular, care must be taken when fields such as strains or stresses present jumps in their value. Such a jump occurs in the elastowetting problem, as the sign of the first derivative of the strain field jumps from negative to positive in the vicinity of the contact line. The solid must be divided into two regions A and B separated by a surface Γ that encompasses the contact line and that is normal to the flat elastomer surface. The dissipation P_{visc} in this system reads

$$P_{visc} = \int_{A\cup B} \sigma_{ij}\dot{\epsilon}_{ij} \mathrm{d}^2 s = \oint_{\partial A\cup\partial B/\Gamma} \sigma_{ij} n_j \dot{u}_i \mathrm{d}l + \int_{\Gamma} [\sigma_{ij}\dot{u}_i] n_j \mathrm{d}l, \quad [1]$$

where the symbol f denotes the jump of f across Γ. The last term in the equation above does not vanish for arbitrary thickness and rheology. Thus, we conclude that the simple form of the divergence theorem on which equation 1 in ref. 1 is based cannot be used here. As a final note, we would like to indicate that current work in our group shows that configurational forces contribute to global dissipation besides Newtonian forces.

In our paper (2), instead of calculating the full dissipation we choose a simpler, approximated route. Indeed, we do not enforce Neumann's force balance at the contact line and we hypothesize that dissipation can be represented by a single term ($\sim \int_{A\cup B} \sigma_{zx}\dot{\epsilon}_{zx} \mathrm{d}^2 s$). Thus, our model is not equivalent to Karpitschka et al.'s. The remaining dissipative term is then calculated under some approximations, as an effective representation of the full dissipation. The resulting formula provides an excellent fit to the experimental data for the dependence of the dynamic contact angle on the thickness of the elastomer layer, indicating that the approximations underlying our description are reasonable. In contrast, Karpitschka et al.'s model does not capture our data. In other words, our results do not support the hypothesis that the Neumann force balance holds at the tip of a moving contact line, as stated in our paper.

We note that our model is only one step among many others that remain to build a thorough and sound description of the dynamics of elastowetting. We agree with Karpitschka et al. that work remains to be done to explain the vast amount of observations that has been reported in the literature on the topic of elastowetting.

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The authors declare no conflict of interest.

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