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Interactive comment on "Rupture-dependent breakdown energy in fault models with thermo-hydro-mechanical processes" *by* Valère Lambert and Nadia Lapusta

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This study examines the dependence of dissipated energy on rupture history using simulations of earthquake ruptures with thermal pressurization as the dominant fault weakening mechanism. Dissipated energy can be divided into work done by sliding against the residual strength of the fault (referred to in this study as dynamic strength) and breakdown energy. Breakdown energy is one of the few earthquake source properties that can be indirectly estimated from far-field seismic radiation, and numerous observational studies have explored the dependence of breakdown energy on earthquake magnitude, local slip, etc. Likewise, theoretical studies of proposed fault weakening processes, like thermal pressurization, provide predictions of how breakdown energy depends on slip and various parameters (like thermal and fluid transport properties of the fault zone). However, in order to obtain closed form analytical solutions, these theoretical studies often make assumptions like constant slip velocity, that are unlikely to be to be met in reality. The current study utilizes more complex earthquake simulations with thermal pressurization that provide more realistic rupture and slip histories. The authors calculate breakdown energy, both locally at each point on the fault and in a suitably averaged sense, and compare to both theoretical predictions and observational constraints. The main conclusions are that local breakdown energy can exhibit large spatial variations across the fault, due the complex rupture history, and can be quite different from the average breakdown energy that is estimated from far-field seismic observations. The study thus provides an important caveat for researchers who hope to infer fault weakening mechanisms from seismic observations. The study is well designed and the manuscript is clearly written; I recommend publication after addressing the following minor comments:

1. Line 45. It is stated that peak and dynamic strengths are expected to be material properties of a fault, but I disagree that this is how people usually think about it. It is more common to regard static and dynamic friction coefficients as material properties, recognizing that shear strength depends on both friction and effective normal stress. I think it is widely understood that the ambient effective normal stress is not a material property of fault, but depends on tectonic loading and fluid state. I recommend explaining this in more detail, pointing out that for the set-up in your 2D simulations, the ambient effective normal stress is a prescribed quantity that is unaltered by fault slip (unlike in a dipping fault configuration, etc.).

2. Line 53. The LEFM relationship between rupture velocity and breakdown energy requires that the small-scale yielding criterion is met. You later explain this, but it might be helpful to mention small-scale yielding here. (Optional)

3. Line 60. I think you mean greater than 1 m/s, not 10ˆ3 m/s!

4. Line 136. “or” should be “of”

5. Equation (11). Should the integral be over Sigma, not Omega? In any case, please make sure to define Sigma and/or Omega.

6. Line 194, lines 258-260, line 339, and elsewhere. (Depending on how you choose to respond to this suggestion, this could warrant a major revision.) You compare your simulation results to the two closed-form thermal pressurization solutions in Rice (2006), both of which utilize the constant slip velocity assumption. However, this was improved upon by Viesca and Garagash (2015) to account for a more realistic slip velocity history that accounts, in the context of a steadily propagating rupture, for elastodynamic relations between slip and stress change. Viesca and Garagash along provide solutions for thermal pressurization and the dependence of breakdown energy on slip. I think your paper would be substantially strengthened by comparing your simulation results to their theoretical predictions, in addition to the Rice (2006) predictions. This might provide insight into the validity of a steady state solution for describing more complex ruptures that accelerate, decelerate, interact with arrest waves, etc. Perhaps there are situations where the steady state solution is a good approximation, or maybe not. It would be very useful to know this since it will help guide the field to either invest more time in developing steady state solutions for other weakening mechanisms or to instead shift toward fully numerical rupture simulations like you have done.

7. Figure 1a. What is the small white triangle? Should this region be shaded blue?

8. Figure 3 and elsewhere. You utilize a 2D model, but then make a comparison to observationally interred breakdown energy from real earthquakes in 3D. It would be useful to add a few sentences or a paragraph discussing whether or not the 2D idealization alters the predicted scaling behavior. Many people familiar with wave propagation understand that there are substantial differences between 2D and 3D, but for various reasons (discussed, for example, in Freund’s Dynamic Fracture Mechanics textbook) this is far less the case for fracture problems. Please comment on this to avoid confusion

and to give readers more confidence that the comparison you’ve made is relevant.

9. Figure 5. It appears that there is weakening that is confined to very small slip, prior to the main effects of thermal pressurization. Is this due to the drop in friction coefficient from standard rate-and-state effects? If so, it might be possible to capture this (small) contribution to breakdown energy through a typical LEFM fracture energy idealization, as was done by Brantut and Rice (GRL, 2011). Consider commenting on this.

–Eric M. Dunham

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