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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 paper describes earthquakes rupture histories inferred with dynamic simulations in which thermal pressurization (TP) has a dominant effect. On their dynamic simulations the authors study in particular the breakdown energy considering punctual estimates as well as global/average values.

One of the conclusions is that local breakdown energy can exhibit large spatial variations across the fault and large temporal variations on the same location in different earthquake ruptures.

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Discussion paper

In the literature, dynamic models of real events show heterogeneous distribution of dynamic parameters (usually in terms of initial stress) to reproduce seismological data.

I completely agree with the main goal of this paper because the authors try to explain an observed feature (the heterogeneity of dynamic models in space and in time) of real events.

The authors compare their theoretical results with estimates coming from real events and find many differences in the scaling relation. I think the study is well designed and the manuscript is clearly written however I have many moderate comments.

My first doubt concerns the definition of breakdown energy. In the literature there is still a confusion about the meaning of fracture energy and I think the authors must do an additional effort to clarify the meaning of this parameter. The shear cracks with the cohesive-zone have been proposed in the literature to overcome the singularity of early crack models and in these model fracture energy is considered as the energy absorbed behind the tip and needed to allow the crack to propagate. In these models, fracture energy surely contains the contribution of surface energy but not heat. In fact, all the dissipations are ascribed to frictional heating (the area below the minimum stress). Recently in the literature a new definition of fracture energy (as well as the discussion about the meaning of fracture energy on real fault planes) has been proposed to reconcile seismological measurements, geological observations and laboratory experiments and to obtain a coherent understanding of the governing physical processes. Tinti et al (2005) proposed to call "breakdown work" the area below the traction versus slip curve and above the minimum traction value reached when the slip is still increasing (it has been called "work" even if it is an energy, so we can think to it as a breakdown energy). The idea to change the definition and also the symbol (Wb instead of G) was made because all the potential contributions and processes that occur at different length scales during propagation can contribute to this energy: heat, breakage of the asperities as well as comminution of the fault gouge, thermal pressurization, flash heating or other processes absorbed in the fault plane, virtually assumed with an infinitesimal thickness.

For its general definition, this work (or energy) is not constrained to be absorbed just behind the rupture front. The area corresponding to the breakdown work is the only area we are able to “measure” during real events because we cannot know the absolute value of initial stress (it is usually assumed a priori). Because we represent the fault plane as a mathematical virtual plane this area can contain different contribution of energy, also heat.

Looking figure 5 and 7 it seems to me that the authors are generalizing the computation of breakdown energy in the same way.

Dynamic models proposed in the literature for real events have to choose a particular data-set of dynamic parameters (e.g., tau_yield, tau_dyn and Dc or mu_s, mu_dyn, sigma_n for SW law, or a,b,L for R&S law) that fix in some way the breakdown energy of a specific event. The choice of SW law imposes the G value a priori and does not allow for multiple seismic cycles while the use of R&S law in multiple events allows for a local and temporal variation of G.

I think the study can improve by:

1) showing the same figures (5 or 7) for dynamic simulations without TP;

2) trying to better explain the meaning of the breakdown energy;

3) focusing the interpretation of the results not only on the TP effects but on the difficulties with real events and with current resolution of data to constrain the slip velocity function. More info on this issue are surely useful also for kinematic modelers.

Specific comments

1) Line 17: shear crack or shear pulse (Heaton 1990).

2) Line 28: Probably in this sentence the authors would have mentioned Cocco and Tinti 2008 instead of Cocco et al 2004.

3) Figure 1: the authors should explicitly underline that the radiated energy can be

computed from the blue area only if the plot represents an average estimate of slip and stress and it is not a punctual plot – as the authors have correctly written in the axes labels (see Kanamori and Rivera 2006).

4) Line 45 : "If slip weakening were the fundamental constitutive behavior describing fault resistance during dynamic rupture, then its parameters - the peak resistance, dynamic resistance, and breakdown energy G - would be expected to be material properties". I think this sentence is not correct. The authors should say that in literature the SW law is frequently adopted for a single event because it allows to assume (or retrieve) only three parameters (mu_static and mu_dynamic and Dc) and because the slip-weakening behavior has been observed also with R&S law. Often, assuming a SW law simplifies the modeling but it does not mean that the authors believe that G is a material property as well as Dc. From laboratory it has been seen that the first two parameters (mu_static and mu_dynamic) are material properties even if they also can slightly change due to different conditions of the rock fabric and fault deformation (strain). Differently, Dc is a debated parameter because it is still not well constrained. So, make the inference that in the literature G is expected to be a material property is too strong and erroneous.

5) Line 52: "…and remaining variations in rupture speed are largely controlled by the breakdown energy in such linear slip-weakening representations (Guatteri and Spudich, 2000)." I don't understand this sentence.

6) Line 55: Many other papers suggest the relation among Breakdown energy and slip (Cocco and Tinti 2008, Brantut and Viesca 2017, Nielsen et al 2016, Selvadurai 2019)

7) Line 56: "which is inconsistent with the breakdown energy being a material property as assumed in linear slip-weakening laws". I don't understand: who believes that energy is a property of the materials?

8) Line 57: I suggest to add that Perry et al 2020 results have been obtained assuming constant L parameter on the fault plane.

9) Line 61: Nielsen et al 2016 say that their measures saturate in the rotary lab machine.

10) Line 65: Please cite Brantut and Viesca 2017.

11) Line 85: Also inferring the dynamic parameters from pseudo-dynamic models (Ide and Takeo 1997, Bouchon et al 1998, Tinti et al 2005, Causse et al 2014) suggest more complex slip weakening behaviors with heterogeneous traction evolutions and heterogeneous dynamic levels.

12) Line 88: I don't think the reader can understand the meaning of "Furthermore, the shear heating itself would depend not on the energy counted as "breakdown" but on the overall dissipated energy, making the fault weakening - and hence rupture dynamics - dependent on the absolute stress levels, and not just on stress changes, as typically considered by analogy with traditional fracture mechanics."

13) Why in figure 3 the authors didn't use a more complete dataset?

14) Lines 95-98: I perfectly agree with this sentence but I would further stress the issue of the slip rate because it's a significant uncertainty of kinematic models.

15) Equation (13) is essentially equivalent to equation (1) in Tinti et al 2005 (assuming only one component) or equation (7) in Cocco and Tinti 2008.

16) Line 182: I cannot appreciate the meaning because I cannot read Lambert et al in review. But I suppose that also in that case it depends on the assumption of minimum stress level to compute G during the slip evolution.

17) I suggest to change the symbol given to the breakdown energy because it is different to the meaning of fracture energy coming from cohesive-shear cracks.

18) How the conditions obtained in a 2D models to arrest the rupture can influence the results respect to a 3D model? How the 2D results have to be scaled to 3D to be compared with estimates from the literature?

19) Line 225: The authors should underline that the initial stress is the only heterogeneous distribution included in these models and therefore it is the most important parameter that affects the size of the event.

20) Figure 7: I would expect a temporal variability of G for different events because fault properties vary with time (and the equivalent tau_yield and tau_min are varying during the seismic cycles). The observables up to date seem to give robust information of the average behavior of G but not on the local estimates. The main problem is that we are not able to infer the slip rate on the fault planes with the actual resolution of seismic data. Results shown in Figure 7 can stress the idea that is very difficult to constrain the local distribution of G because it can change frequently with time as demonstrated theoretically.

21) Figure 6-7: I really like these figures that show how different can be locally the traction evolution as a function of slip due to different slip velocities. I image that the different points on the fault show a very different traction evolution. I suspect that sometime is very difficult to decide which is the minimum traction to fix the area below the curve. Probably there exist many other fault points with heterogeneous slip weakening behavior whose dynamic values reached toward the final slip vary but lies above the first important minimum value, so the area doesn't not increase too much. Moreover, I expect to observe a similar behavior imposing local stress heterogeneities able to produce secondary cracks propagations. I have two comments for these figures: (1) I suggest to the authors to discuss about the importance of the knowledge of slip rate more in general and not only linked to TP. (2) in figure 7 the authors have selected particular points in which the slip rates have surely two peaks values due to the complexity of the rupture front (as I can see from panels A). Should the authors add a panel with the slip rate function? Central column represents a test case in which G is more similar because the rupture front is smoother and slip rate is simpler. This is the reason why in the literature the average estimates of G are considered more robust than local estimates, both when proposed by spontaneous dynamic models and when calculated

on pseudo dynamic models, i.e. constrained by the kinematics of the event.

22) Probably the reader needs to start from a simpler condition: How is Figure 7 if the authors simulate R&S law with homogeneous parameters, heterogeneous initial stress and without TP?

23) How is the traction evolution (figure 7) if the authors model an event with R&S law, homogeneous constitutive parameters and constant initial stress with TP (maybe it can be the first modeled event of the seismic cycle)? In this way the reader can appreciate when the effects of TP occur.

24) I suggest to the authors to write that the breakdown energy is the only measurable energy and a future challenge is to understand what does really it represent.

Technical corrections: 1) Line 60: high slip rate (>10ˆ3 m/s) I think there is a mistake or in the number or in the units . . .This number is too high if represents m/s.

Literature to add in the references among many other papers:

1) Gu and Wong 1991

2) Tinti et al 2005. JGR

3) Causse et al 2014 GJI

4) Nielsen et al 2016: G: Fracture energy, friction and dissipation in earthquakes, J. Seismol,

5) Cocco et al 2016, On the scale dependence of earthquake stress drop, J. Seismol.

6) Selvadurai, P. A. (2019). JGR

7) Brantut and Viesca 2017

8) Bizzarri 2010, JGR

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