Solid Earth Discuss.,
https://doi.org/10.5194/se-2020-118-RC1, 2020

Solid Earth
Discussions
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Interactive comment on "Extracting microphysical fault friction parameters from laboratory- and field injection experiments" *by* Martijn Peter Anton van den Ende et al.

Anonymous Referee #1

Received and published: 9 September 2020

The manuscript present new insights regarding the inversion of frictional parameters from laboratory and field injection experiments. The authors are using a Bayesian inversion approach, using the evolution of the normal dilatancy with slip to outputs the best solution for geometric and frictional parameters used in CNS model (L, H and phi_0 and phi_c). Then They compute the solution for the slip rate based on the micro-physical model. The paper is well written and is of broad interest for the community. I have few comments that I think could improve the clarity of the manuscript.

Major comments

1. Comments regarding the Figure of the paper: The figures are of high quality, but

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should be completed following the comments above.

Figure 1: Authors could also present the evolution of the shear stress on one of the plots.

Figure 2: Authors should complete the legends of the axis. The Y axis of the distribution plots looks to refer to a probability function rather than the parameter. Authors should also present the units of each parameter, also because the scale is different between the lab and the field experiments.

Figure 4: Maybe it could be relevant to plot directly the experimental values of dilatancy at a given slip versus the theoretical values to show the trend and the robustness of the prediction. Same could be also done for slip rate as a given time.

Figure 5: Same comment for panel b.

Figure 6: Same comments than for figure 2.

2. The model used here to describe the time evolution of slip at the onset of fault reactivation is based on the properties of the gouge layer. From the micromechanic model, the physical response of the system, and notably an upper bound value for the dilatancy, is expected (in my opinion) to be controlled by the average grain size of the grains. However, in the present study, the grain size is not an important parameter. Can authors comment on it?

3. The results presented here show that a good fit of the experimental observations is obtained using the parameters output from the inversion. However, it looks that the model explains well the increase in slip rate, but does it also explain the decrease in slip velocity observed in the field injection experiments that occur just after the onset of rapid slip (Cappa et al., figure 1)? Or is the dilatancy I am also surprise to see how tight variations in initial porosity can induce such large variations in the slip history presented in figure 4c and 4d. Do authors things that this behavior could be observed in the laboratory?

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4. The main observations made here is that the values of H allowing to fit the experimental data recorded during the field injection are strongly larger than the upper bound definition proposed by Niemeijer and Spiers. The model proposed here explain dilation by shear at the contacts of gouge grains. However, along fault interface, dilation is expected to be mostly controlled by fault geometry and long scale roughness encountered within the slip domains. It is stated in the text lines 166-174 but do author think about an adaptation of the model to include a second dilation angle due to fault geometry?

5. Finally, this model is expected to describe the slip behavior of the fault in drained conditions (homogeneous fluid pressure distribution), however, I believe it is not the case in the fluid injection experiments where the slip front outgrowths the fluid pressure front (Bhattacharya and Viesca). In addition, a fluid pressure gradient is expected, even at the scale of the laboratory in partially drained conditions (Passelegue et al., 2018) maybe authors should add a small comment about it in the manuscript.

Minor comments:

Equation 1b: I am probably wrong but I am not sure that the equation described here refer directly to d_phi/d_t. It looks to me more related to the dilatancy rate of the gouge layer.

Lines 98: It is probably not changing a lot, but I wonder if you should not compute Delay_L/L=(phi-phi_0)/(1-/phi_0) to consider the initial fraction of the matrix.

Maybe I missed it but I do not see the definition of a∼ in the text, that should be of the form d\mu∼/d(ln V) or something like that.

Part 4.1: I wonder about the relevance of this part here since the limitation of RSF has been already mentioned in the introduction.

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