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Interactive comment on "Characteristics of earthquake ruptures and dynamic off-fault deformation on propagating faults" *by* Simon Preuss et al.

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This paper uses the Seismo-Thermal-Mechanical code with rate and state friction (STM_RSF) to explore lateral strike-slip fault propagation across multiple earthquake cycles. The work builds upon Preuss et al (2019) with additional formulations that allow for more realistic rheology. The findings present insights that until now were not available because the code used here can simulate the evolution of stress state throughout the both the aseismic and seismic portions of the earthquake cycle. The results of the parametric study presented here shows several interesting trends that warrant further investigation and could greatly impact our understanding of fault propagation over multiple earthquake cycles. I greatly appreciate the systematic nature of the presentation where the authors first test different rheology, then successively add new implementations to the code. The new implementation of fault width overcomes many disadvantages over other techniques and thus, provides a valuable approach for many future investigations. I offer a few suggestions that might strengthen the manuscript.

A. The assumption of lateral propagation of strike-slip faults does not consider how crustal faults might form by the upward propagation and/or linkage of early fault segments. Experiments of strike-slip fault evolution show upward propagation with the formation of an early set of echelon faults that link to form a through-going strike-slip fault (e.g., Tchalenko, 1970; Hatem et al., JSG 2017). We don't have reason to believe that crustal strike-slip faults would initiate differently from experiment observations. The text uses results of Perrin et al. (2016) that faults are most mature along their centers to justify lateral propagation. Lateral variation in fault maturity don't preclude early upward propagation that would produce echelon segments that may link earlier along some portions of the fault than others. Unlike the quasi-2D simulations in this paper, the base of 3D laboratory experiments distributes shear within the suprajacent material in a manner analogous to crustal systems where mid crustal deformation drives upper crustal faulting. I'm not saying that the investigation of lateral propagation of strike-slip faults within this paper is unreasonable. This is a great first step towards understanding the complex evolution of strike-slip faults but may be only part of the story. To strengthen the implications of the paper, the introduction and discussion of the manuscript should include consideration of the 3D context of these strike-slip faults. How might the findings differ if strike-slip faults initiate with upward propagation followed by linkage?

B. The conclusions that the strike-slip faults grow predominantly in the aseismic period of the earthquake cycle is based on the assessment that the rate weakening results, with their fast coseismic growth, are unreasonable. We do have evidence that faults can link during earthquake rupture along previously unmapped segments. This process of quick coseismic linkage could look a lot like the results of the rate weakening models that propagate towards the edges of the model. If the models had a second fault segment, perhaps the rate weakening models would showing linkage of the segments in a very reasonable way. For this reason, perhaps the rate weakening rheology may be overlooked should not be considered completely unreasonable.

C. This question is related to the comment of B. Rate neutral and rate weakening are rheologies that we expect in the crust. In this study, they are excluded because the models do not produce more than one earthquake. What model parameters could be altered (for example, in a future study) to get more than one earthquake cycle for the rate neutral and rate weakening models?

D. Some parameters seem a bit outside of expected crustal ranges. The width of the plastic yielding fan seems large. The static frictional strength of the faults of 0.6 is high when we consider that crustal faults have fluids. The 20 km choice for maximum fault zone width for the heuristic fault zone thickness, needs stronger substantiation.

E. Something to consider in the discussion of the paper is the role of nearby faults on the 'bending' of fault traces. While fault strike may bend in response to changing slip conditions, most crustal faults develop within a complex system where they might interact with nearby faults. Slip on a nearby fault (such as the Garlock fault near the San Andreas) may in many cases have larger impact on the bending of faults than difference in aseismic and coseismic lateral propagation. Additionally, immature faults may develop bends in their earliest stages when neighboring segments that are not co-linear link up to form a single fault surface (Hatem et al., JSG 2017).

F. The manuscript strives to address a wide range of conditions/questions. I wonder if some parts of the manuscript, such as the HPT models, might be best served as supplemental material.

Specific comments

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Discussion paper

The paper is very well written. I have a few specific comments that may strengthen the writing in places.

Throughout the manuscript (eg. Line 102, 298 and many others): ‘Fault extension’ reads a bit odd since extension is a strain term. The text might be clearer with use of ‘fault propagation’.

Line 40 (and there abouts): The use of Riedel terminology for splay fractures strikes me as a bit odd because we typically refer to Riedels as the early formed echelon fault segments. The fractures within the damage zone are more commonly called splay cracks. You may find papers by Cooke (JGR 1997) and Willemse and Pollard (1998) helpful because they show the range of orientation of splay fractures that can develop with different conditions on the fault.

Line 51: Define SCEC.

Line 213: Mixing strain (extensional) and stress (compressional) term. Make these both either strain or stress.

Line 252: spelling of strengthening

Line 253: ‘This results’ is ambiguous. This what? Being more clear will help the reader.

Line 254: Better than what?

Line 399 and throughout: I’m not a fan of the acronym OOF for optimally oriented fault model. Why not just spell it out since you already have a lot of acronyms and only use OOF for one section of the paper?

Line 497: This is just one paper, for which there is a rich literature. Add e.g. and some more citations.

Line 540: Another interesting study is a quasi-static dynamic model of Savage and Cooke (JSG 2010). That study differs from the ones citated in that it does not limit damage development along pre-existing mesh. So, the results of Savage and Cooke

(JSG 2010) might be interesting to compare to your new model results.

Line 573-574: This is an interesting result. I believe that this finding confirms results of Jiang and Lapusta – it could be helpful to cite their work here.

Line 583: There is a rich literature on the development of new faults that are more efficient. Add e.g to this reference.

Line 594: Cooke (JGR 1997) show that changes in friction distribution near fault tips alters the stress concentration and the angle of the splay crack. The change in friction arises in the transition between mature fault with static friction to immature fault with higher friction. Could this process be contributing to your observation of changing splay angle?

Line 602: Add Cooke (JGR 1997) to this reference list as it is very much related to these other good papers.

Line 628: The width of the plastic fan in the models is larger than that seen in Savage and Brodsky (2011).

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