Reviewer Report

Ms Number: se-2020-149

Full Title: Cross-DiffusionWaves resulting from multiscale, Multiphysics instabilities: Application to earthquakes

Authors: Regenauer-Lieb et al

GENERAL COMMENTS

This contribution builds on the Part I companion paper and applies the theory developed there to develop a new generic approach for illuminating the role that coupled THMC processes may play in triggering earthquake rupture (shear) instability through the generation of spatially and temporally organized spikes in field variables such as pore fluid pressure. The wider applications go far beyond the case of earthquake triggering, as complex THMC coupling and feedback processes, operating on different characteristic length and time scales, are clearly evident in the multiscale self-organization and self-similarity seen in geological structures and in geophysical observations. Since the approach followed is so fundamental, it is inevitable that the paper is rather generic in nature and that significant introductory/didactic material has to be presented. Nonetheless, I feel that the authors have responded very effectively to the criticisms levelled in the first round of the review procedure and have successfully focused the paper down more specifically on application to earthquake instability. I found the paper, along with Part I, a fascinating read, and am encouraged to see how the authors and their co-workers are exploring the exceedingly challenging topic of reaction-diffusion processes and Onsager-type cross-coupling between the corresponding thermodynamic force-flux relations. The paper will be quite abstract and unfamiliar for many readers. However, it is based on sound physical principles, is well explained, well-referenced and well-structured. It should be welcomed into the literature as providing a new and inspirational basis for quantitatively exploring the complexity of geological and geophysical reality. In my judgement, the manuscript is acceptable for publication, though I would suggest a few minor revisions for the sake of clarity and completeness, as detailed below. No further review is needed in my opinion.

REPLY: We value very much the helpful comments of the reviewer which facilitated a rare process. Innovations in such a challenging topic are normally very hard to read and the exciting discoveries made may be lost to the broader readership. The review facilitated a process where through the concise application to the NLS the information entropy of the paper has decreased, and the paper is in a much sharper logical form. At the same time relegating the explanatory notes to the supplementary material the paper remains available to a much broader readership that is unfamiliar with the complex feedbacks of thermodynamic force-flux relationships. We would like to thank the reviewer for making the additional effort of fine tuning through the minor suggestion and appreciate the enormous effort of providing such a detailed positive criticism.

DETAILED SUGGESTIONS

A) Abstract, line 11. “a rogue wave would appear as a sudden fluid pressure spike on the future fault plane”. This suggests that only faulting of intact rock is important or considered here, which is not the case. Best reformulate “future fault plane” to make clear that such a spike is most likely to trigger unstable slip on a pre-existing (near-critically stressed) fault. This might be worth emphasizing in the main body of the article also.

REPLY: Now changed to “In the context of hydromechanical coupling, a rogue wave would appear as a sudden fluid pressure spike. This spike is likely to cause unstable slip on a pre-existing (near-critically stressed) fault acting as a trigger for the ultimate (shear) seismic moment release.”

B) Lines 15-16. “Although the paper is formulated for earth sciences the approach constitutes a generic theory for any material and therefore lacks experimental evidence.” I do not see how this follows. Surely, the more general a theory is the more likely it would be that someone has found evidence for expression of some aspect of it. Perhaps drop the last 5 words? The issue of experimental evidence/validation is dealt with later, after all.

Reply: The 5 words have been deleted

C) Lines 38-39. “The particles are subject to a pressure exerted by the solid matrix and the pressure of the pore fluid”. Does this mean that only the hydrostatic component of (effective) stress supported by the solid is considered in the analysis? In general, deviatoric stresses would be accounted for too, of course, but if that is the case here, would it not be better to replace “pressure exerted by the solid matrix” with something like ‘thermodynamic force exerted by the stress supported by the solid matrix.’

REPLY: Now changed as suggested

D)Lines 179-183. I fully agree with the statement that is made here. However, I would advise adding a short qualifier making it clear that “initial” material and environmental heterogeneities at multiple scales, will tend to complicate the picture.

Reply: That is quite true. The qualifier has been added.

D) Lines 189-194. Quite a number of papers have been published on the structure and slip/seismogenic behaviour of faults in carbonate rocks, in nature and experiment, based on (quantitative) considerations of crystal plastic, diffusive and “superplastic” deformation mechanisms – and considering/reporting decarbonation more qualitatively. The recent papers by the Durham (e.g. De Paola 2015) and Utrecht (Chen, Verberne) groups come to mind, for example, as well as the somewhat broader work in a more rate-and-state framework by Aharanov and Scholtz. These mostly focus on feedback between shear/frictional heating and the above deformation (or asperity contact) mechanisms, rather than coupled reaction-diffusion in the present sense. Perhaps it is worth mentioning that the present type of model is not the only way of explaining localized fault structures in carbonates, or indicating how the approaches differ.

Reply: We have added a qualifier at the end of the paragraph stating that “We would like to emphasise at this point that chemical dehydration reaction-diffusion processes are only a special case of THMC feedback. A number of other recently proposed feedback processes need to be investigated for completeness, e.g. shear heating and phase changes triggered on asperity contacts (Hayward et al. 2016, Aharonov and Scholz 2019).”

E) Line 331. What is meant here by “effective stress weakens”? Are the authors referring to a decrease in the effective (mean?) stress supported, i.e. to some measure of strength?

Reply: replaced by strength

F) Lines 434-435. “In geomechanical laboratory experiments the solid matrix is known to respond to external forces by a nonlinear reaction commonly expressed in a power law”. Presumably a rheological law (power law creep) can be treated in the same way as a reaction rate description in the present treatment. Both are dissipative kinetic processes, of course, but it might be useful to clarify briefly whether both can be treated equivalently.

Reply: We have added “Both chemical and mechanical reaction source terms are dissipative kinetic processes, and our approach can lead to the same laboratory-derived results. The difference is that the laboratory laws can strictly only be extrapolated for the laboratory conditions, while the physics-based approach is more generic.”

G) Equations 12. It would be helpful to many readers, I think, if a couple of lines could be added here to clarify what physical/chemical conditions the chosen parameters imply.

Reply: Setting self-diffusion to zero implies that nonlocal cross-diffusion processes are happening very fast and trigger large internal-fluxes between solid and fluid. Additionally, the cross-coupled fluxes are maximized in normative sense by setting the cross-diffusion coefficients to unity and opposite sign. In other words, we consider the extreme condition where the ratio of cross-diffusion over self-diffusion coefficients tends to infinity.