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Solid Earth
Discussions

Interactive comment on "Cross-Diffusion Waves as a trigger for multiscale, multiphysics Instabilities: Application to earthquakes" *by* Klaus Regenauer-Lieb et al.

Anonymous Referee #2

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I had high expectations for this manuscript, having reviewed the "Part 1" paper of this two-part series. The premise for this current manuscript is that rogue fluid pressure waves can be quantified via cross coupling of off-diagonal diffusivity terms in reaction diffusion partial differential equations, and such waves are earthquake triggering. This is a companion paper to "Part 1" which described the general instability and coupling features. The paper offers a great high-end discussion of earthquake physics from the perspective of THMC processes across time and length scales and as such, it seems at the start of the paper that it offers a good overview and thus a good inclusion in the special issue. Placing an important instability such as earthquake triggering in the

context of broader THMC instabilities is certainly an interesting precept for a paper.

However I think the paper frequently veers off course, and during a first read every time I was met with a promise of discussing earthquake physics the paper lumbers off onto barely related topics (Lotka-Volterra chemical oscillators, solitons, dislocation theory, spinodal decomposition, and on and on.) Perhaps the paper would be more readable if these sections were omitted or at least shortened (we don't, for example, need a lengthy discussion on wave propagation, predator-prey problems, and spinodal decomposition just to introduce cross terms in a diffusion matrix– this makes me think that part of this paper were in fact prepared originally as a textbook). Having already reviewed "Part 1", I was really looking forward to the meat of this paper which is the mathematical workup to the cross diffusional rogue wave treatment in the context of earthquake instability, where much of this ancillary discussion would have already occurred in Part 1. It is perhaps too late to consider this option (i.e. placing the background of THMC instabilities in paper 1, leaving paper 2 to focus on earthquakes). But that would be the ideal situation.

The linking of chemical oscillators to rogue waves as presented however is unfortunately ad hoc at best: although a summary of reaction-diffusion instabilities and related phenomena like Leisegang bands is perhaps an appropriate summary for a journal issue devoted to coupled THMC modeling, frankly the Leisegang phenomena and its related dynamics have little to do with cross diffusional terms and in particular with rogue waves. It seems the paper relies on the facts that both are "waves" to make a similarity argument, despite the inherent differences in the physics. The discussion on deformation bands is certainly appropriate but again the discussion loses robustness when linking to dislocation processes. It would do the paper well to focus on earthquakes and cross diffusional phenomena as the title suggests and forgo all this extra, unnecessary, and frankly unrelated stuff.

I am no expert in the propagation of electro-cardio waves and the associated instability, but the arguments presented in this paper would be better served if the so-called cross diffusional approach used to describe this (FitzHugh-Nagumo oscillator) were more fleshed out (section 4.1), rather than mentioned in passing. Time spent on analogy between the FitzHugh-Nagumo mechano-electro-chemical oscillator would do much to improve the paper and not the Volterra chemical wave dynamics which owe nothing to cross diffusional terms. By the time the paper starts actually discussing the earthquake instability, there are only a few pages left with only a disappointing arm-waving level of treatment. I did not read the Discussion and Conclusion section.

I think a paper linking seismically observed “tremors” as fluid-motion, rogue wave physics, the interesting rice-krispie earthquake-like experiments, discussions of compaction waves, direct application of coarse-graining, and perhaps the FitzHugh-Nagumo oscillator (if at all applicable) would be a readable paper appropriate for this special issue. The rest just seems like unnecessary fluff. The appendices are unnecessary. I would recommend a rewrite along these lines.

Some additional comments:

1. Line 1/abstract - "Instabilities appears twice - change second occurrence to "source dominated source mechanisms". 2. Line 8 “These are here interpreted as a trigger. . .” 3. Line 13 “In this paper (Part 2) we investigate. . ... 4. Line 17 “Patterns in Our Planet” probably should be italicized 5. Line 23 – capitalize “Part 1” here and throughout the paper 6. Line 43 – don’t capitalize “earth” 7. Line 63 – add coma after parenthesized term 8. Line 92 – does the Regenauer et al. 2020 paper refer to “Paper 1”? If so this reference needs to be updated. 9. Page 5, Figure Caption to Figure 2 – should provide a reference or two to the coarse graining discussed in the figure – (does this figure come from another reference? If so it should be treated accordingly. 10. Line 108 – should the linearized form of equation (1) contain a different notation for the “linearized” reaction rate expressed in equation 2? 11. 11. Line 110 – should reference Table 1 here. 12. Line 170 – eliminate the first comma 13. Line 183 Eliminate the comma 14. The paper would lose nothing in range and scope of content if Figures 3-8 were eliminated, along with accompanying text (sections 2.1-2.4)– this material has been

well covered in other literature. Section 2.5 provides a link with Paper 1, and thus should be included. 15. Following a discussion of periodic earthquakes (as arises from a spring-slider block sort of instability) on the heasl of Lotka-Volterra oscillators is a bit disingenuous, as the physics manifest in the PDEs are really quite different. 16. Line 307 – should be “Punchbowl” fault 17. Line 376 – update the 2020 reference. 18. Line 442 – again, should probably capitalize “Part 1” and will need to update the reference. 19. Line 492 – I disagree that cross diffusion terms from Onsager assumptions are the same or a “new” form of chemical wave at any scale, let alone at the “smallest scale. 20. Line 510 – the sentence is incomplete. 21. Line 532 – maybe reference a Turing paper for scholarly completeness? 22. Line 632 – should reference papers by Olsen and Olsen and Holcomb here 23. Line 685 – Finally! A promise of the application of cross-diffusional terms to earthquake triggering!! Oh wait, but first we need to hear about solitons!! 24. Line 721 – the paper is unfinished here – what figure is being referred to? Same in line 745 and line 749.

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