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Solid Earth Discussions

Interactive comment on "Cross-Diffusion Waves as a trigger for Multiscale, Multi-Physics Instabilities: Theory" *by* Klaus Regenauer-Lieb et al.

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This paper presents expectations about patterns in the solutions of a set of equations that are proposed to be relevant to Earth processes. The authors introduce equations of THMC coupled processes that include cross-diffusion terms (gradients of one quantity induce diffusion of another quantity) and then leverage on existing literature on the topic to anticipate the rich patterns the solutions could develop. No new simulations nor values of relevant non-dimensional parameters in the context of Earth processes are presented to support the proposed ideas. Hence no quantitative predictions are made, based on the model proposed, that could serve as testable hypotheses for future field

or laboratory observations.

This was a very difficult paper to review because of the poor quality of the writing. While grammar and spelling are perfect, the logical structure of the paper is deficient, at times hard to discern. Here are concrete examples:

1. The introduction opens with an argumentum ad verecundiam (akin to "a Nobel prize winner said . . ."). One would rather expect arguments to be supported by peer-reviewed references.

2. The introduction is 4 sentences long, cites no references, is obscure and poorly interconnected, and overall fails to accomplish the basic goals of an introduction section. What is the Earth science problem addressed? What observations and open questions motivate this work? Why does it matter for an Earth scientist? What is the state-of-the-art in understanding these phenomena? What is the research gap to be filled? One example: the topic of reactive transport is abundantly treated in the Earth science literature, that thread could be explored and cited here.

3. The concept of cross-diffusion is so important that it is in the title, but it is not defined until section 5.

4. Moving goalposts: Near line 50, the reader gets the impression that the paper will be about earthquake precursors. An exciting prospect, indeed. But there is no substantial treatment of earthquake processes in the rest of the paper. The earthquake topic appears, almost as an afterthought, in two sentences near the end of the paper. Most of the paper seems to be about compaction bands.

5. The choice of words (like "postulate" instead of "hypothesize" in line 53) gives the sense of the work being a theoretical musing rather than an effort to develop testable hypotheses. Observational and experimental challenges are described later in the paper, as well as references to ongoing related work (in review?), but placing those instead in the introduction section would help the reader understand the aim and scope

of the paper.

6. Line 62: If you are really after earthquake precursors that are analogous to travelling waves, there may be interesting connections to draw between your models and slow slip phenomena (abundant literature on that topic, including observations, experiments and modeling).

7. Line 80, “We call this extension (geo-)wave mechanics due to its mathematical similarity with quantum physics approaches”: unfortunately, such analogy to quantum mechanics is not elaborated anywhere in the paper.

8. Unclear notations. For instance, in equation 15, why a subscript i in the Laplacian and in the coordinates (x,y,z), instead of applying the same Laplacian to N physical quantities that have a subscript i ?

9. Section 5 is basically section 4 of Hu et al (2020). Without loss of clarity or emphasis, this section can be substantially shortened by referring to those previously published results.

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