

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 manuscript proposes an approach for coupling processes that the authors define “Multiphysics” because due to different kinds of instabilities. Although the work looks intriguing in approaching the problem with a complete and integrated view, in my opinion, there are some parts that are not yet clear.

The main problem I found in reviewing this paper is that the authors sometime, instead of explaining directly the question, prefer to resort to another companion paper that, shamefacedly, is not yet available (at least to me). In general, I would suggest to describe the most important passages of that companion paper (a dedicated section

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would be useful), so also the present work can be self-consistent and explanatory.

Another important aspect is that if the new theory wants to replace the conventional present one, the latter should be concisely described and the critical issues pointed out, so justifying the replacement with the new one. This aspect is neglected.

For the rest, the paper deserves attention and, once all critical points are solved, could be of great interest by the scientific community.

Other points/doubts/comments

Lines 10-11. The final sentence of the Abstract is not fully clear. The same problem is found in the main text.

Lines 12 and following. The Introduction is too short and poor.

Lines 50-52. Very interesting, but not clear at all from this work. This sentence remains suspended, Also because of the reference to the companion paper which is not yet available.

Lines 63-64. This should be better explained and extended. Otherwise, also this sentence remains suspended.

Line 91. Eq. 1. For dimensional coherence, the body force f should be alone, not multiplied by ρ . Consequently, adapt in the following part of the text.

Line 104. definition of I as Identity matrix should be moved below (after all this series of expressions).

Line 119. Putting in the same name "elastic" and "plastic" looks contradictory. What do you mean? that the strain rate can be in different conditions? But this would be said in the text.

Line 287, Eq. 21a. Is here missing a dV_{REV} ?

Lines 378-380. I cannot have access to this other work (Regenauer-Lieb et al., 2020).

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It would have interesting to have a look at it. Otherwise, I suggest to mention the most significant passages of that work.

Lines 403-404. Not convincing and neither clear. This point should be more clarified in this work (and not only referred to the companion paper). For instance, how is the new theory better than the conventional one that describes earthquake processes?

Interactive comment on Solid Earth Discuss., <https://doi.org/10.5194/se-2020-46>, 2020.

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