Solid Earth Discuss.,
https://doi.org/10.5194/se-2020-149-RC1, 2021

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.

Angelo De Santis (Referee)

angelo.desantis@ingv.it

Received and published: 8 March 2021

The paper discusses the THMC reaction-diffusion equations as underlying equations for the instabilities preceding earthquakes. I understand the efforts and troubles the authors met in preparing this paper: the topic is new for seismology and the theory grown in other fields had to be fit in the seismological frame. I admit that the paper results very difficult to follow in some parts. From one side, some parts are didactical (e.g. 2.1, 2.3), others are technical (2.2), other simply colloquial (section 4): and this dichotomy is more evident because the latter concerns the main topic of the paper. Anyway, I found the paper very interesting and plenty of fresh indications for earthquake physics, with the presence of such as cross-diffusion waves and stationary waves. Nevertheless, I have two main concerns. The first regards the fact that the general theory overcomes the application to earthquakes very much. In this aspect the paper should be more balanced, in view of the fact that is dedicated to the application to earthquakes. My second concern is that the stationary waves have never been clearly found and neither weakly detected by earthquake seismology (apart from presumed but disputed findings present in Russian literature). If this is true, the entire presented framework, although intriguing and compelling, will be relegated only as a curiosity within a theoretical corner without any real application. This could be the most negative aspect of this work. My suggestion is to ask the Authors to work on the paper in order to make a "moderate" (more than minor, less than major) revision in order to let some concepts and their relationships more clear. In the following, I list also some more punctual indications.

Lines 1-10 Abstract. I found it very short with respect the many information given in the main text. I suggest to expand it in order to include something more. By the way, the same indications (with the due adaptions) can be given for the section 5 of "Discussion and conclusions". See also below.

Line 28. I suggest Crampin et al. 2013 instead of Crampin and Gao 2015, as it takes the problem with a more seismological point of view, proposing the solutions for four typical seismological conondrums.

Lines 28-29 (also 64). I suggest the term "self-affinity" instead of "self-similarity".

Line 29. Better "log frequency- log magnitude relationship"

Line 34. "standing waves": how different are they from the "KaY waves" (Yagodin, 2017), whose existence is highly disputed (e.g. Koronovsky et al. 2019)?

Line 52. "holistic way…": how far is it from the "geosystemics approach" (De Santis 2009, 2014)? Probably they both share the same foundations.

Lines 106-108. It is said "... to first order ..." but the equation (2) is of the second order in space. Caption Figure 3, first line: please correct "sale" with "scale".

Table 1. It is not fully clear how the given values of the table have been estimated.

Line 237. You mention the "autowaves" but you provide only one reference (Antonioletti et al. 2017). Are there some more references?

Line 279. "... with some apparent regularity in their recurrence ...", in the sense that the regularity is only apparent: the 2004 Parkfield event skipped the previous apparent periodicity.

Line 280. Please remove one bracket after "1997"

Line 292. Better "quasi-periodicity in space"?

Line 511. The line is broken. In addition, the present sentence is not clear.

Lines 569-570. The sentence "This approach ..." is not clear

Lines 721, 745, 749, 757. There are "???" for some figures.

Line 760. Pag. 31. Where is "Step 4" in the main text that is mentioned in the Figure 11 caption?

Lines 779-789. I think this part should be better exposed, because concerns the predictability or not of the cross-diffusion (here called rogue) waves. The conclusions are not clear: what I understand is that we should "leave the (present) analytical assessment ... aside" and pass to "investigate numerical solutions". But the next section (lines 790-798) poses many limits to this step.

Lines 895 and next ones. Web links are mostly double. Please check them.

Line 974. Why is the title of this reference in capital letters?

References

De Santis A., Geosystemics, Proceedings 3rd IASME/WSEAS International Conference on Geology and Seismology (GES'09), Cambridge, 36-40, 2009.

De Santis A., Geosystemics, entropy and criticality of earthquakes: a vision of our planet and a key of access, in "Nonlinear phenomena in Complex Systems: from Nano to Macro Scale" ed. E. Stanley and D. Matrasulov, NATO Science for Peace and Security Series – C: Environmental Security, 2014.

Koronovsky, N.V., Zakharov, V.S. & Naimark, A.A. Short-Term Earthquake Prediction: Reality, Research Promise, or a Phantom Project?. Moscow Univ. Geol. Bull. 74, 333–341 (2019). https://doi.org/10.3103/S0145875219040057

Yagodin, A.P., International Center of the Earthquake Prediction, 2017. https://sites.google.com/site/earthquakepredict/r1

Interactive comment on Solid Earth Discuss., https://doi.org/10.5194/se-2020-149, 2020.