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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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We would like to thank the reviewers for the tremendous effort and careful review of this paper. We realize that the paper touches a number of challenging fundamental issues and is therefore more difficult and time-consuming to review than the average publication. We are extremely grateful for the patience and detailed feedback providing useful criticism and suggestion for modifications.

The reviews of the original paper suffered from two significant issues.

By not having the second paper available both reviewers were missing the important

application of this paper presented in part 2. Unfortunately, the second part was not yet available to the reviewers at the time of their reviews as the submission had been delayed and the paper had not yet been released for viewing. The second part is now accessible to the reviewers and available on https://doi.org/10.5194/se-2020-149, in review, 2020.

Another significant problem was the deficient introduction which did not specify why this paper was focusing just on the theory aspect and how the two papers join together. The original intention was indeed to submit both papers in one cast but delays in the writing of the second manuscript led to a placeholder introduction in part 1. We apologize for this shortcoming.

The introduction has now been entirely rewritten. It includes the necessary link to the second part. It also lists the relevant background in terms of the GEOPROC themes, references and prior work in the development of the new theory and explains the innovations in this paper that overcome the issues encountered in earlier attempts to address the dynamic regime of acceleration waves. The introduction also provides a short overview of the sections to come and in conjunction with the update and rewritten conclusion should present a clear logical structure of the paper.

This paper was written with the specific aim to derive and explain the new theory in detail which did not receive any criticism. Please find below the address of the specific points raised by the reviewers.

Jean-Paul Ampuero (Referee) General Comment: 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.

Reply: We acknowledge that the introduction jumped straight into the topic without giving the reader a hands-on introduction to the motivation, the state of the art in the various fields and the important innovation in this paper. We have therefore completely rewritten the introduction to give a much clearer picture of why we did not in this paper specify non-dimensional parameters relevant to Earth processes.

The analyses expected by the reviewer have indeed been our first approach which should have been cited in order to explain why a numerical non-dimensional analysis of non-dimensional exploration is not fully satisfactory. We now explain in the introduction the background of these papers. We were able to use the numerical solution of the system of coupled pde's using non-dimensional parameters to show that NVTS events (Poulet, Veveakis et al. 2014) and geological faults (Poulet, Veveakis et al. 2014) can be modelled by the approach. However, a sound theoretical description and interpretation of the local processes resulting in the interesting macroscopic phenomena has been lacking. Numerical solutions of coupled THMC-equation have a notorious problem of modelling instabilities out of a perturbed state and while sophisticated solutions (arc continuation method) have been developed a proper understanding of the instabilities and analytical criterion for the onset of dynamic instabilities did not exist prior to this work. A very important problem is the non-uniqueness of the solutions and the associated uncertainty relationships which requires new solution strategies.

All three shortcomings are overcome in the present contribution. We acknowledge, however, that the new theory is in its infancy state and special care needs to be taken before applying it to real-world systems. The first part is therefore presenting the theoretical derivation and the second part delves into possible applications and proposed experimental tests to verify the applicability of the theory to Earth processes

In this paper we focus on following innovations that cannot be found elsewhere in the literature: (i) a multiscale extension of the theory of thermodynamics of irreversible processes to include dynamic events by using a meso-scale - macro-scale model discussed in part 1 and illustrated in further details in part 2; (ii) a generalization of the theory of cross-diffusion waves from chemical systems to generalized THMC thermodynamic-force flux pairs; (iii) a transfer of knowledge from classical quantum mechanics to characterise any system at a larger scale in order to deal with the discreteness of multiscale material behaviour and the quantum-like uncertainty relationships. All of these aspects are listed in the introduction and. the conclusion summarizes the theoretical findings of this paper

References to our earlier work on non-dimensional analyses of NVTS events and geological faulting in poromechanic, reactive systems now added to the introduction Poulet, T., E. Veveakis, K. Regenauer-Lieb and D. A. Yuen (2014). "Thermo-Poro-Mechanics of chemically active creeping faults. 3: The role of Serpentinite in Episodic Tremor and Slip sequences, and transition to chaos." Journal of Geophysical Research: Solid Earth 119(6): 4606–4625 Poulet, T., M. Veveakis, M. Herwegh, T. Buckingham and K. Regenauer-Lieb (2014). "Modeling episodic fluid-release events in the ductile carbonates of the Glarus thrust." Geophysical Research Letters 41(20): 7121-7128.

Jean-Paul Ampuero (Referee) detailed comments: 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 sec- tion. 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.

Reply: We fully acknowledge the deficiency of the original introduction. The new introduction and the con-jointly rewritten conclusion should solve all of the above issues also putting this paper into the rich context of the GEOPROC conference series.

Jean-Paul Ampuero (Referee) detailed comments: 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.

Reply: This misconception is due to the poor quality of the original introduction and the unavailability of the second paper. The first reviewer also stumbled over this. The rationale in the introduction and the second paper should alleviate this concern. The goalposts of the first and second paper are now clearly articulated in the introduction. " It is fair to say that the theory is rather in its infancy state, and special care needs to be taken before considering a direct application to the aforementioned systems. The first part therefore presents the theoretical derivation, and the second part delves into possible applications and proposed experimental tests to verify the applicability of the theory.".

Jean-Paul Ampuero (Referee) detailed comments: 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.

Reply: We exchanged the word postulate by hypothesis

Jean-Paul Ampuero (Referee) detailed comments: 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).

Reply: very good suggestion! We did not present our earlier work where we are applying coupled Multiphysics pde's numerically (Poulet, Veveakis et al. 2014) to show that they can replicate slow slip events. The earlier work is critically reviewed in part 2 but following suggestions we also highlight the discussion of the second paper in the revised version in the introduction of part 1.

Jean-Paul Ampuero (Referee) detailed comments: 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.

Reply: Right! This needs further explanations. We have removed the notation of geo-wave mechanics as it distracts from the main points in this paper. The paper is already rich in theoretical concepts and following this strand may be interesting but possibly leading too far astray. It may be more interesting to the theoretical physics community. We felt that we should, however, acknowledge somewhere inspiration from the quantum approach.

The new introduction, therefore, promises a discussion of the link to quantum statistical approaches that allow overcoming the problem of dealing with discrete patterns and the uncertainty relationship due to path dependence of the integration of the thermodynamic system (incomplete heat and work differentials). The revised conclusion summarizes this point succinctly. Part 2 provides around equation 29a+b a full discussion of the uncertainty principles of quantum systems applied to the systems at a larger scale which stems from the analogy of Schrödinger's equation (equation 28). The formal analogy to Schrödinger's equation is not mentioned, however, for the reasons stated above. While this analogy is an interesting discussion it does not aid the understanding of the paper.

Jean-Paul Ampuero (Referee) detailed comments: 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 ?

Reply: Correct the subscript is unclear it only applies to the N physical diffusion processes. This has been corrected in the revision.

Jean-Paul Ampuero (Referee) detailed comments: 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.

Reply: We have shortened the mixture theory section in the revision. " In the following discussion, we generalise the discussion of the meso-scale mass exchange processes using mixture theory applied to HM coupling as presented in \cite{Hu2019b}. We show the physics of cross-diffusion follows from a reactive source term at the macroscale that requests a cross-diffusion term at the meso-scale for thermodynamic consistency. The full derivation is found in \cite{Hu2019b}. Here we summarize the main conclusion from the mixture theory analysis."

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