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Solid Earth
Discussions

Interactive comment on "Correlation between tectonic stress regimes and methane seepage on the west-Svalbard margin" *by* Andreia Plaza-Faverola and Marie Keiding

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We are thankful for the comments. Based on this and comments from referee two, we will restructure and rephrase in places the manuscript to: 1) be sure the main focus of the paper is conveyed clearly: this is a qualitative rather than quantitative study; 2) present a more substantial discussion about the robustness of the resulting tensile zone related to tectonic spreading and the importance of this first approach.

Specific actions on referee 1's comments:

Referee: Authors consider that only spreading centers are relevant and propose to

use Okada's elastic solution for dislocations in an infinite elastic space to model the stress gener-ated by the two spreading centers they are interested in. Interestingly they place the spreading centers below the brittle-ductile transition and assume a 7 mm/y opening rate. Hence, not only they use an elastic solution for analyzing the opening of a dislocation in the ductile part of the lithosphere, but they assume symmetry for the velocity of plates on both sides of the ridge, a feature which ought to be discussed.

Authors: While we agree that asymmetry of the spreading centres in Fram Strait is possible, the presently available magnetic data for the region is not of a quality that justifies such assumption (see, e.g., the NAG-TEC magnetic anomaly map; we note that this question may be resolved after the acquisition of new aeromagnetic data on the west-Svalbard margin by the Geological Survey of Norway).

We again underline the fact that the predicted stresses agree very well with the observed earthquake focal mechanisms indicating that the predicted stress field is - to a first order - a realistic representation of the stresses in the region.

Action: We will modify the discussion to clearly describe the limitations and strengths of implementing Okada's solutions in the investigated setting. We will also describe explicitly that the model assumes symmetry.

Referee: Finally they consider that the pore pressure associated with the seepage of methane is larger than the minimum principal stress in the rock formation. But when pore fluid pressure is larger than the minimum principal stress, a hydraulic fracture is formed that keeps propagating till the pressure is released and becomes smaller than the minimum principal stress. This should have been discussed.

Authors: We propose a conceptual model to describe how the formation or opening of already in place faults or new tension fractures may increase secondary permeability and thus lead to seepage. We agree that the formation of a hydraulic fractures potentially leads to cycles of pore pressure depletion and build-up that will in turn influence the timing of seepage activity.

Action: We will extend on what we already had in the discussion about the cyclic character of the system and how pressure depletion and regional stresses may be interacting.

Referee: I personally completely disagree with authors proposition that the glacial rebound does not affect presently the stress field and is negligible as compared to the effect of the spreading centers. In addition topography effects ae most likely significant an the appropriatness of neglecting them should be demonstrated. Independently, because of the above mentioned difficulties concerning the proposed model : 1) with using Okada's elastic solution for modeling the stress field generated by a dislocation in a ductile material, 2) by assuming symmetry of plate motions on both sides of the ridge, 3) by considering that hydraulic fractures may remain stable for long durations of time, I cannot accept the paper as is. I propose a complete revision that will include a discussion showing why all my comments here above are irrelevant.

Action: Please see above.

Referee: In lines 110 to 113 of authors paper, it is written: "Because the model only incorporates plate spreading, it is likely that the actual stress field on the west-Svalbard margin differs to some extent from the stress field predicted by our model. However, by excluding all other sources of stress, we are able to investigate the influence of tectonic stress exclusively." I consider this statement demonstrates an error of judgement: the ongoing methane seepage depends on the coupling between fluid pressure and the presently existing complete stress field, as explained here after.

On line 114, authors state that they use Okada model of dislocations for modeling what they call tectonic stresses. This assumes elasticity. In elasticity, if four different loading processes are considered, the superposition of all of them at the same time implies that the resulting stress field may be evaluated from the sum of the four stress fields computed independently for each of the loading processes. Authors have listed as loading mechanisms: A ridge opening, B topography, C effect of sediment erosion-

deposition, D flexural stresses due to glaciation. Hence, according to authors, present stress field result from A+B+C+D. Claiming that it can be investigated by looking at A only, implies that B+C+D are negligible. This requires a demonstration! Nowhere have I seen in the paper computations for B, C, and D.

Authors: As previously described, the purpose of the present paper is to report on a modeling exercise with the intention of investigating the kind of stresses (in a qualitative way) generated exclusively by oblique spreading at the mid-ocean ridges in the Fram Strait (Knipovich and Molloy) and how these stresses (alone) seemingly correlate with the distribution of seepage activity.

As the referee recalls from the main text, seepage depends on the coupling between fluid pressure and the present stress field, which is most likely a result of a number of different sources. In this study, we did not attempt to model the entire stress field, rather, we attempt to investigate the influence of plate spreading by considering this source alone. Whether plate spreading is the dominant source of stress or not, will be further investigated by establishing a numerical model of higher complexity that includes additional sources like stresses related to isostatic rebound (works in progress) and topography (what the referee terms as B, C etc.).

While the modeling approach here is simplified, i.e., based on assumptions of isotropic, homogeneous, elastic rheology as well as flat earth and spreading symmetry, it represents a first and important step for the investigation of near-surface stresses in this Arctic region (the study area is adjacent and hence highly vulnerable to mid-ocean ridge stresses, probably more than anything else).

Action: We will modify the paper accordingly to make clear that the study intends to report on an important qualitative observation that forms the basis for understanding the interaction between regional processes and near-surface fluid dynamics in Arctic settings. We will elaborate on the arguments for considering the tectonic stresses to be dominant in this study area.

Referee: When I say “no reference is made to well documented on going glacial rebound”, this is precisely what I mean. I do not mean authors have not cited previous work, I am saying they have not compared the magnitude of the glacial rebound effect to that of ridge opening at the location of methane seepage. As a reviewer of a scientific paper, I am careful to check facts, not speculations. I do not consider that authors response to my review do address properly the issue of quantifying effects B, C, and D.

Authors: We realize that we misunderstood the phrasing here. The comment regards the same issue as above, i.e., that we do not attempt to quantify all possible sources of stress (A+B+C etc). We can, however, include a more qualitative discussion about the possible magnitude of stress from glacial isostatic rebound (based on analogy with the models from the Fennoscandia area; by e.g. Lund et al., 2009; Steffen et al., 2006).

Referee: I also do not wish to get involved into endless discussions on whether authors understand what is hydraulic fracturing or not, etc: : :. I just did what I consider the work of a reviewer should be, i.e. check facts or validity of computations; I will leave the editor in chief decide whether my comments are relevant or not. I will stop here my time devoted to this paper and do not wish to be further involved in reviews for the journal “Solid Earth”. Indeed, I am not interested in discussing opinions: : :my small education just helps me with scientific demonstrations within my very small field of expertise... C2

Authors: We thank the reviewer for devoting time to review the paper and apologize for any misunderstandings.

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