Solid Earth Discuss.,
https://doi.org/10.5194/se-2018-11-SC1, 2018

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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Received and published: 15 May 2018

Thank you for the comments. We would like to answer/clarify some of the points raised by the reviewer:

1. Modelling approach

The reviewer claims that "no reference is made to the stress field generated by well documented on going glacial rebound effect". This is not correct. We state several times that the study area was most likely affected by glacially induced stresses during the Quaternary (pages 1, 3, 5-6). A whole paragraph of the discussion section is ded-

icated to the discussion of the influence of glacial isostatic adjustment. We reference state-of-the-art work on the stresses generated by glacial isostatic adjustment (Lund & Schmidt, 2011). We suggest that the influence of glacial stresses is probably particularly important to explain seepage activity along the entire Vestnesa Ridge in the past, where seabed pockmarks does not show seepage activity at present.

The reviewer states that we "identify" two other possible sources of stress. This is also incorrect or reflects a misunderstanding. We mention that the study area is affected by other sources of stress (topography, subsurface density contrasts, erosion/deposition, and during the Quaternary also glacially induced flexural stresses). However, we explain that we do this modeling exercise with a clear intention of investigating the stress from tectonic spreading exclusively. It is unfortunate that the reviewer missed this point. This is the essence of the paper. Are there spatial variations in the stress field in the region due to the way the Molloy and the Knipovich ridges are spreading with respect to each other?

The reviewer "disagree with the 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". While the present study focusses on stress from spreading, we clearly do not claim that other stress sources are negligible. However, models of stress from GIA show that the present-day stress at formerly glaciated margins is small, on the order of 1-2 MPa (Lund et al., 2009; Lund & Schmidt, 2011; van der Wal et al., 2013; Steffen et al., 2006; Steffen et al., 2014). As mentioned, we believe there may have been a more significant effect of glacial stresses on the region in the past. This is work in progress.

2. Ridge asymmetry

The reviewer correctly writes that the Okada model assumes symmetry of the spreading centers. We are aware that some mid-ocean ridges are known to be asymmetric, however, the geometry of the spreading centers in the Fram Strait is not well known and asymmetry has to our knowledge not been documented. Thus, a discussion of asymmetry can be hardly fit into the scope of our present study. Other studies in the region have also assumed symmetry of the spreading ridges (for example, Johnson et al, 2015). Based on the comment from the reviewer, we will revise the manuscript to describe this assumption explicitly.

The Okada model does, admittedly, involve simplifying assumptions, such as symmetry. However, Árnadóttir et al. (2009) demonstrated that the deformation field from the complex plate boundary in Iceland could be modelled using Okada models. The predicted stress directions from Okada models are in general agreement with other models of plate tectonic forces (for example, Bott, 1991; Gölke & Coblentz, 1996; Fejerskov & Lindholm, 2000; Naliboff et al., 2012). The good agreement between the predicted stress field and the observed focal mechanisms, furthermore, indicate that the model correctly predicts the first order stress field at upper crustal depths. We mention that we do not attempt to analyze the total magnitude of stresses, but relative stress variations in the region caused only by oblique spreading. We plan to test and validate the results of this work in future using other modelling approaches.

3. Stress relief

The reviewer claims that we consider "that hydraulic fractures may remain stable for long durations of time". We do not really go into depletion of pressure following hydraulic fracturing. Here 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. The relevant idea here is that seepage would then occur exclusively in places where the stress regime favors a permeable behavior of faults and fractures. We agree that the formation of hydraulic fractures potentially leads to more complex cycles of pore pressure depletion and build-up that will in turn influence the timing of seepage activity. This is however a local mechanism that makes sense to investigate in the context of detailed imaging of gas chimneys or other near surface fluid flow migrating features (e.g., Hustoft et al., 2009, Hustoft et al., 2010; Gay et al., 2007; Gay et al., 2012). Here we are interested in the regional and more long-

term stress regime (e.g., stressing from plate tectonics, glacial isostatic adjustment) and the relation with pore fluid pressure in terms of effective stresses.

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