

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

Anonymous Referee #1

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The paper proposes to correlate the location of active methane seepage zones observed along the west-Svalbard margin to tectonic stresses associated with the Molloy and Knipovich spreading centers of North Atlantic Ridge. They identify two other possible sources of stress, namely gravitational stresses due to topography and flexural stresses due to sediment erosion and deposition.

To my surprise no reference is made to the stress field generated by well documented on going glacial rebound effect.

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-

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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.

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.

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 are most likely significant and the appropriateness 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.

Interactive comment on Solid Earth Discuss., <https://doi.org/10.5194/se-2018-11>, 2018.

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