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Interactive comment on "Can anaerobic oxidation of methane prevent seafloor gas escape in a warming climate?" *by* Christian Stranne et al.

Anonymous Referee #2

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Methane gas transport within the hydrate stability zone has been long recognized since the extensive work done at Hydrate Ridge (ODP Leg204) (Torres et al., 2004, Milkov et al., 2004, Liu and Flemings, 2006, Torres et al., 2011). Researchers are also puzzled by the appearance of methane gas in hydrate stability zone due to the obvious violation of thermodynamic prediction that only dissolved phase and gas hydrate are allowed. Hydraulic-fracturing as a result of gas over-pressure and geochemical inhibition have been proposed as two competing explanations (see Torres et al., 2011 for review). To advance our current knowledge on such issue and provide a holistic view of how methane gas migrates within gas hydrate stability zone, numerical modeling that adequately considers the transport of multi-phase fluids, geomechanics of the sediments, and thermodynamics (and kinetics) of gas hydrate is one of the important way

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Discussion paper

forward. In this work, the authors performed model sensitivity tests with a numerical model that couples geomechanical with AOM to understand the relationship between gas production (through hydrate dissociation), gas migration (through hydraulic fracturing or permeable layers) and gas release from the sediments. The authors primarily focus on the migration of methane gas within sediments of different permeability and investigate how methane consumption through AOM is controlled by gas transport. The modeling approach adopted by the authors is indeed novel and adequate to the research question at hand. However, I found the work premature with a few assumptions require more careful assessment. There are a few recent papers also discuss the transport of methane gas in hydrate stability zone (Liu et al., 2019, Fu et al., 2018, Meyer et al., 2018). Though AOM is not considered in these papers, the transport mechanism should be similar. The authors should discuss and compare with these recent works. Major comments: (1) The lack of hydrate formation at shallow depth: A brief introduction about how hydrate stability is modeled in the T+H model should be given. I wonder why there is no hydrate formation at the shallower depth (<20 meters) where methane concentration can be over saturation and P-T conditions are suitable? The authors should present the phase diagram for methane within the model frame so that it will be clear to see where and when gas hydrate can form/dissociate in the model. The lack of hydrate formation in the shallow depth can significantly impact the model outcome as a) hydrate formation as a result of gas seepage can take up the pore space and greatly reduce permeability; b) the gas flux towards seafloor may be greatly reduced as the result of hydrate formation; c) the amount of methane consumed by AOM may also increase as the retention time of methane in the sediments increased. (2) Constant thickness for SRZ: the authors spent a bit of effort try to justify the assigned constant SRZ (5 meters) in their model by saying this represents a “typical” value of SRZ. I am not sure what is typical for SRZ thickness as it is a function of organic matter degradation rate plus the flux of methane in the sediments and should vary with water depths and organic matter supply to the ocean (e.g., see the global compilation by Bowles et al., 2014). Also, the observed thickness of SRZ can

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be greatly biased by the type of sampling tool with meter-scale SRZ to be the most often recovered through gravity coring at locations with mostly diffusion-dominated fluid regime. In cold seeps where methane gas bubbles escape from the sediments (which resembles more closely to the case here), cm-thick SRZ can be recovered only through precise push-coring with underwater robots (e.g., ROVs). In the current model, the authors decoupled the AOM rates and the thickness of SRZ and used AOM rate as high as 1 micromole/cm3/day which correspond to cm-thick SRZ in cold seeps. I find the assigned 5 meter of SRZ too much off from a realistic scenario. (3) Besides AOM, the authors should also consider AeOM (aerobic oxidation of methane) which is likely more important than AOM in seeps with high flux of methane (e.g., Boetius and Wenzhofer, 2013). AeOM operates in the first few cm of sediments and serves as the last line of defense with respect to methane leakage. (4) I feel like the title is misleading as the impact of ocean warming on gas hydrate stability is not modeled in this work. The scenario considered is applicable to any situation with a great supply of methane from greater depths and not necessarily related to gas hydrate dissociation. The connection to gas hydrate dissociation can be strengthened by relating the methane production rate assigned in the model with realistic numbers, such as dissociation of certain % of gas hydrate for a given time. Discussion about under what circumstance such hydrate dissociation rate could occur will help to connect the modeled scenario with real world situations. Minor comments: P2L1-4: at several places in the paper, the authors intend to link anthropogenic ocean warming with hydrate destabilization. Such connection is hypothesized mostly from numerical modeling without any confirmation from field observations. On the other hand, recent works on the cold seeps around Svalbard, have shown that the gas emission cannot be attributed to gas hydrate dissociation as a result of contemporary ocean warming (Berndt et al., 2014, Wallmann et al., 2018, Hong et al., 2017). I would advise the authors to modify these statements according to these recent findings. P2L17: I don't think AOM is an overlooked process. Extensive work has done for the past three decades at least. P3L10-11: See my major comment (3). Aerobic oxidation of methane is also a very important process stopping methane from

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escape (Boetius and Wenzhofer, 2013). It is probably more important in places with high methane flux, such as the condition focused in this work. P3L13: More precisely, AOM only controls the flux from sediment to the ocean P3L18-19: again, no need to emphasize anthropogenic. Regardless of the trigger, warming of ocean will result in hydrate destabilization. P4L8: Should be Moridis et al. There are three authors contributing to this manual. P4L9-10: Goes back to my major comment (1). it seems like that there is no formation of gas hydrate assigned for depth shallower than 20 meters despite it is still within gas hydrate stability zone. P4L10-11: though such condition with feather edge hydrate stability is vulnerable to climate change, the contribution to global methane emission as a result of gas hydrate dissociation is probably small due to such thin hydrate stability zone and lower quantity of overall hydrate comparing to locations where hydrate stability zone could extend to hundreds of meters. P5L14-15: See my major comments (2). The constant thickness of SRZ is a potential problem despite the authors have tried to convince the readers otherwise. P6L4: Again, see my major comments (2). The so call "typical SRZ thickness" requires more justification. P8L12: I assume it is also methane gas in the matrix flow? P11L28: See my major comment (4). What is the rate for CH4 production? Is it of a realistic rate? P12L3: correct CH4 (subscript 4) throughout the text and figures. P12L5: ocean warming is not modeled. Delete this statement. P14L26: The AOM efficiency should be defined in the method. P15L5: this is a weird sentence. AOM is important for consuming the methane release from hydrate dissociation due to climate change

P15L31: check parentheses

Water depth controls the phase boundary of methane, and therefore how much methane is in the dissolved phase that is available for AOM. The water depth of sites reported from Boetius and Wenzhofer range from 560 to 4000 meters with widely different methane saturation. This water depth factor is also something that require considered. P15L37: check parentheses P16L1: lower than what? and how does the lower bulk average AOM rate reconcile the discrepancy? P16L4: I don't think PKF can be

called a deep sea cold seep since the seeps are located at water depths shallower than 350 meters. P16L5-7: Isnt it the same for your simulated case with high permeability that the high permeability remains high regardless of the in situ pore space? P16L10: doesn't need to emphasize anthropogenic warming as your model results cannot differentiate the different triggers of warming. P16L12-13: Please check this sentence again. I don't quite follow.

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