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Earth System Dynamics Discussions

Interactive comment on "Climatic impact of Arctic Ocean methane hydrate dissociation in the 21$^{\mathrm{st}}$-century" *by* Sunil Vadakkepuliyambatta et al.

Anonymous Referee #2

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The manuscript "Climatic impact of Artic Ocean methane hydrate dissociation in the 21st-century" by Vadakkepuliyambatta and co-workers investigates a possible positive feedback between warming bottom waters in the Artic region and Global Warming. The paper is generally well written and had it been presented five years ago it would have only required minor to moderate revisions. However, in the light of the current state-of-research it is difficult to understand the motivation for this study. There is extensive literature going back to Biastoch et al., GRL, 2011 that uses the same or more sophisticated methods. The consistent answer from all these studies is: there is no feedback over the next 100 years.

Unfortunately, the authors have not put their study into the context of this literature and they have also not convinced me that they have anything new to add. I am really sorry

for being so negative as I, of course, acknowledge the amount of work that went into this study. My recommendation is, nevertheless, to re-design the study and to carefully review what the remaining open questions are. In its present form, I, unfortunately, do not find the manuscript convincing and ready for publication.

Major comments:

Novelty: The fate of hydrates under global warming has been extensively studied. Most relevant are:

Biastoch et al., GRL, 2011: Used ensemble mean climate-model predictions to investigate warming bottom waters as a driver of methane hydrate dissociation in the Artic region. The methodology is very similar to the one presented in the here-discussed manuscript. Conclusion: no feedback between global warming and dissociating hydrates over next 100 years.

Ruppel, Nature Education, 2014: insightful short review of the topic. Conclusion: no feedback over next 100 years.

Hunter, EPSL, 2013: Similar to Biastoch et al., 2011 but more processes are included. Conclusion: no feedback over next 100 years.

Berndt et al., Science, 2014: Observed venting offshore Svalbard most likely not related to contemporaneous global warming.

Kretschmer et al., 2015: Integration of a climate model with global and regional hydrate inventory modeling. Approach similar to Biastoch et al and Hunter et al. but more elaborate effort to constrain the global hydrate inventory. Conclusion: no feedback over next 100 years.

These selected previous studies represent progressive improvement on our understanding of hydrate deposits under global warming over the past 7 years. I, unfortunately, do not see how the presented manuscript fits into this. A quick look into the most recent literature reveals what kind of new models and approaches are needed to better link environmental changes at the seafloor to hydrate dynamics. For example, Stranne, GRL, 2016 and Wallmann, Nat Comm, 2018, investigate multi-phase flow dynamics in shallow sediment in order to relate the time scales of fluid as well gas transport and temperature/pressure changes. This is probably a better direction than re-iterating simplified (and too high) flux estimates based on changes in stability zone thickness.

General approach: Inventory: One of the big challenges to this kind of study is that first a distribution of gas hydrates for the contemporary (Artic) ocean has to be derived. Everything that comes after (i.e. changes in stability zone thickness, possible hydrate dissociation) critically depends on this. Here the authors assume a constant pore filling. This seems too simple and the chosen value is most likely way too high.

Look at Wallmann et al., 2012, Kretschmer et al., 2015, and the review by of Boswell and Collett, 2011. They all report global hydrate inventories smaller than what the authors here propose for the Artic alone. The authors need to resolve this discrepancy.

Thermal and hydrate modeling: The thermal modeling is poorly described. Please elaborate more how the assumed thermal diffusivity was derived and why a 3D model is used (as opposed to 1D). It really doesn't sound like a good idea to have an aspect ratio of 1000 between lateral and vertical grid spacing. Also, a vertical grid spacing of 5m sounds way to coarse to resolve heat transport within the top 10s of meters. This needs to be improved.

What kind of bottom boundary condition is used? It should be constant heat flow but the text seems to suggest constant temperature it used. Please clarify.

Non-monotic change in bottom water temperature: It does not sound like a good idea to directly impose annually changing bottom water temperatures (for the goals of this study). This may lead to double counting of emissions as hydrates form and dissociate on different time scales.

GHSZ modeling: I couldn't find it in the text: do the authors discriminate between shrinking of the stability zone from above and below? A reduction in stability zone thickness from below will most likely not result in venting (gas is recycled into the hydrate zone above). Only where the seafloor is moving out of the stability zone venting is likely to occur. This should be discussed and clarified.

Summary:

In conclusion, I think the manuscript in its present form does not represent significant progress in our understanding of marine gas hydrates in the earth system over the next 100 years. The general question if there could be a positive feedback has been resolved by previous studies.

I therefore suggest that the authors re-design their study. It should be made clear in the introductory sections what the current state-of-research is, where the shortcomings of previous studies are, and how this manuscript represents a step forward.

I am sorry for being so negative and it is very possible that I have missed something. I hope the authors can clarify the raised issues in a revised version.

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