On behalf of all the authors I would like to thank Reviewer#2 for reading our manuscript and providing valuable comments.

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.

The evolution of hydrates under warming Ocean has been extensively studied, using different approaches by different authors as the reviewer correctly points out. However, as far as we know, our study is the only one which models the combined solid earth-hydrosphere-atmosphere system. We provide estimates with uncertainties for the flux from the seabed, flux to the atmosphere, and warming potential of methane from hydrate dissociation. We do understand and acknowledge in the manuscript that the method we apply for the methane flux estimates from the seabed has limitations. It is also mentioned in the manuscript that advancing that particular model is not the goal of this manuscript. As Stranne et al., 2016 points out, multi-phase flow dynamics may provide better flux estimates. However, this method has never been implemented over a large region such as the Arctic Ocean and is not feasible considering the large amount of input data such models demand. In this paper, we approach the problem in an earth system scale, attempting for the first time to provide estimates for flux from seabed to atmosphere and its impact on global temperatures.

The response of offshore Arctic methane hydrate to ocean warming and its impact during the next century needs to be assessed well with as many modeling approaches as possible. Methane hydrate and the underlying free gas reservoirs can comprise almost half of the planet's organic carbon and CO_2 is still continuously rising. This is why one needs to be very critical with modeling results and actual data measurements, which is also demonstrated by high variations in the attempt to quantify the GH inventory.

We further underscore that scientific consensus can only made through several independent groups analyze the available research data in objective ways with their best methods and assumption and also questioning earlier published estimates (for eg. Knutti et al., 2017, Beyond equilibrium climate sensitivity, Nature Geoscience, 10, 727). The potential for large methane releases from hydrates have been a research topic highlighted as an uncertain factor under global warming. Some recent papers have appeared as mention by the Reviewer, but the scientific community should welcome further studies from independent research groups.

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.

The hydrate inventory estimates have been varying a lot over the course of last 20-30 years, depending on the methods used and the hydrate saturation implemented, and the estimates vary drastically by many orders of magnitude. Wallmann etal 2012 and Kretschmer et al 2015 use similar methods and use a generally low hydrate saturation (modeled). The Kiel school (Biastoch, Kretschmer, Wallmann, Berndt) has shown also each approach has its limitations.

The review paper by Boswell and Collett, 2011 uses a similarly low hydrate saturation (1- 3%) particularly focusing on dispersed, low-saturation hydrate occurrences. They explicitly state this as 'background hydrate concentration'. A subsequent study by Marin-Moreno etal., 2016, combining the method by Kretschmer et al., 2015 and taking in to account hydrate occurrences in high gas flux settings, put the Arctic inventory five times the estimate by Kretschmer et al., 2015. Our estimate arrives at a much lower hydrate inventory for the Arctic, due to more robust input data used for estimating the hydrate stability thickness.

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.

The thermal modeling we have implemented is presented in detail in the supplementary material. It also includes a description on the estimation of thermal diffusivity. A 1D model cannot represent lateral variations in bottom water temperatures and heat flow. In addition, a 1D model cannot accurately reproduce lateral transport of heat through the sediments. A 3D model is necessary to accurately represent the heat transport. As far as the resolution of the model is concerned, the aspect ratio does not impact the model results. Of course, having a higher resolution is better, but is not suitable for such large areas with sparse data. Our model is very likely the highest resolution model for the Arctic so far.

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.

The bottom boundary condition is set at constant heat flow. A sentence has been added in the supplementary material in order to make this clearer for the reader.

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.

The reviewer raises a valid point here. The variation in bottom water temperature are small from year to year and they affect only the upper few tens of meters. The hydrate formation and dissociation in such shallow subsurface can be quick and there is a higher chance of gas release into the water column. However, since most of the gas released may not escape the sediments, there is a chance of double counting. Considering a monotonic variation can also add an error of similar scale. We argue that the error in the flux due to double counting is small and is well within the considered and realistic uncertainty range. We will discuss this in the manuscript.

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.

Yes. Most of the flux is due to the shrinking of hydrate stability zone from above. The change in hydrate stability thickness from below is considered only after the complete disappearance of GHSZ. The manuscript is modified to clarify this.

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.

It is unfortunate that the reviewer finds that the study does not represent any progress. As pointed out earlier, our study is the first one which estimates the transport of methane through three different realms, the solid earth, hydrosphere and atmosphere. This study pushes the focus toward combined earth system models. Considering that the reviewer has failed to identify this by reading the manuscript, we will modify the introductory chapter to make our focus and novelty of this study clearer.