The authors of this manuscript (ms) estimate the climate impact of released methane from oceanic gas hydrates in the Arctic to the atmosphere towards the end of the 21st century by integrating hydrate stability and atmospheric modeling. The estimated gas emissions to the atmosphere are used to calculate the change in radiative forcing, which is novel as far as I know.

Although it is quite clear that a lot of work has gone into this project, my main concern is that, in its current form, the paper does not clearly present truly novel results. More specific questions and comments on the ms are outlined in the text below.

Method and novelty

Estimates of future methane gas emissions to the atmosphere from dissociating marine hydrate deposits have been the subject of several previous publications using the same or similar methods. The method applied here, referred to as the hydrate stability approach in Stranne et al. (2016a), is based on calculated changes in the GHSZ thickness over time, as a result of an assumed seafloor temperature increase. Any hydrate situated in the destabilized sediment volume is then assumed to be dissociated instantaneously and the produced methane gas is assumed to be released instantaneously into the ocean (and atmosphere). There are significant problems with this approach, but if we leave that for now (I will get back to that later on in this review), I still have concerns regarding the motivation for this paper. The hydrate stability approach has been applied, for the same purpose as in the present ms, in at least three previous publications:

Biastoch et al. (2011): Hydrate saturation estimates of 2.4% (60–70°N) to 6.1% (north of 70°N) based on ODP data and numerical modeling (Klauda & Sandler, 2005). They conclude that “Under transient conditions we estimated an additional average methane flux of only 162 Mt CH_4 yr^{-1} from melting Arctic hydrates over the next 100 years (auxiliary material) – a value lower than the current anthropogenic input of (600 Mt yr^{-1})”.

Hunter et al. (2013): Similar to Biastoch but they include predicted sea level rise, and they assume a hydrate saturation of 1%. They claim that “Predicted dissociation rates are particularly sensitive to the modelled vertical hydrate distribution within sediments” and they conclude that “Under the worst case business-as-usual scenario (RCP8.5), upper estimates of resulting global seafloor methane fluxes could exceed estimates of natural global fluxes by 2100 (> 30-50 Tg CH_4 yr^{-1}), although subsequent oxidation in the water column could reduce peak atmospheric release rates to 0.75–1.4Tg CH_4 yr^{-1}”. This is about 0.2% of the natural and anthropogenic CH_4 emissions.

Kretschmer et al. (2015): Also similar but with initial hydrate saturation based on modeling rather than assuming a certain homogeneous saturation. They state: “Most important is certainly the inventory calculation. The constant mean hydrate pore filling of 2.4% to 6.1% in Biastoch et al. [2011] led to a much higher inventory of 9000 Gt C for the present climate”. Kretschmer (2015) arrive at the almost 2 orders of magnitude lower value of ∼116 Gt C. Yet, they reach a similar conclusion as was reached in Biastoch (2011), Hunter (2013) and also in the present paper: “Compared to the present-day annual emissions of anthropogenic methane, the amount of methane released from melting hydrates by 2100 is small and will not have a major impact on the global climate.”

1) As was noted by both Kretschemer et al. and Hunter and al., the results from the hydrate stability approach is particularly sensitive to the assumed initial hydrate inventory. But even with assumptions on the initial hydrate inventory differing by 2 orders of magnitude (!), Kretschmet and Biastoch end up with the same conclusion: no major impact on the global climate over the next 100 years. In the light of this, I do not understand the motivation for the present study.

2) One difference between this ms and some previous efforts is that yearly averages of seafloor temperature from CMIP model outputs are used here. From my understanding, combining the hydrate stability approach with interannual variations in the seafloor temperature forcing can be difficult.

Consider a warming period, during which the GHSZ thins and methane gas forms (which in this approach is assumed to be instantaneously released to the atmosphere), followed by a cooling period where the GHSZ is partly restored (this can happen, even if there is a general warming trend, and is reflected in the wiggles seen in Fig. 2b). It is then important to keep track of the fact that some of the gas was already released during the previous warming period, when estimating the gas emissions during the next warming period. Otherwise the same gas will be counted as being released twice (or more). An easy way to get around this problem is to follow the approach of Biastoch et al. and Kretschmenr et al., where the linear warming trend for each grid cell is applied. I think the authors need to reassure the reader that the problem of "double counting" gas emissions has been considered, or explain why this would not be a problem.

The estimated gas emissions to the atmosphere are used to calculate the change in radiative forcing. This is novel as far as I know.

Present day hydrate inventory

One of the most sensitive aspects of the approach used in this ms is the estimate of the present day hydrate inventory. This is pointed out explicitly by both Hunter et al. (2013) and Kretschmer et al. (2015).

The authors of the present ms write: "we adopt hydrate saturation estimates derived from analysis of ocean-bottom seismic data from offshore Svalbard (Hustoft et al., 2009;Chabert et al., 2011;Westbrook et al., 2008). Based on these studies, we apply a constant hydrate saturation of 9 ± 3 % of pore space throughout the gas hydrate stability zone in the Arctic sediments."

3) The assumed 9% homogeneous hydrate saturation is the largest assumed saturation I have seen, and considering the importance of the assumed initial saturation (as stated above), the authors need to explain how they arrived at this number in much greater detail. My opinion is that they should also put forth convincing arguments to why this number should be representative for the whole Arctic Ocean. Personally, for reasons presented below, I believe this is a severe overestimate.

4) The 9% saturation assumed in the present manuscript is substantially larger than what is assumed in Biastoch et al. (2011): 2.4% (60–70°N) to 6.1% (north of 70°N). Yet, the authors end up with a significantly smaller present day hydrate inventory in the Arctic Ocean (2500 Gt C) compared to Biastoch et al. (9000 Gt C). There must therefore be some serious differences in terms of estimated sediment porosity (or possibly definition of the Arctic Ocean). The authors should discuss these differences, as there seems to be an error or an erroneous assumption in one of these two calculations.

5) It should be noted that Biastoch et al. (2011) based their estimate on the work by Klauda and Sandler (2005), and that those results stick out in comparison with other estimates (see Fig. 1 below). The Klauda and Sandler estimate is actually more than one order of magnitude larger than any other estimate. In the light of this, I think it is hard to defend the assumed 9% hydrate saturation, as it is even higher than the assumption made by Biastoch et al. (and quite significantly so).

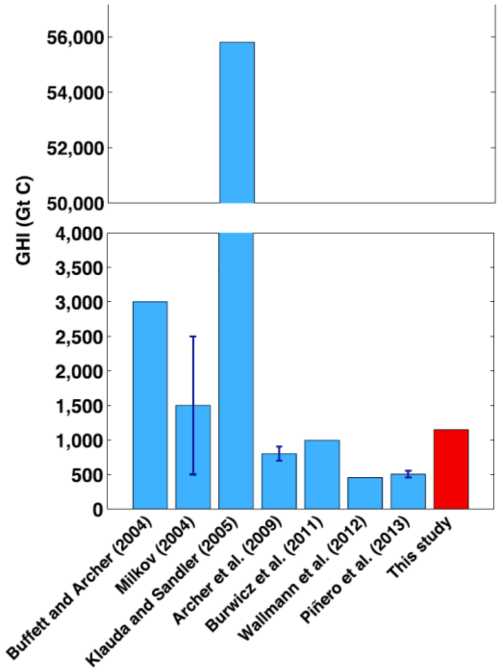

Fig. 1 Global estimates of methane hydrate inventories. Taken from Kretschmer et al. (2015).

6) The high saturation suggested by the authors is especially troublesome when considering the results presented by Miller at al. (2017). They studied the pore water chemistry of 32 sediment cores taken on the shelf slope along the East Siberian Sea. They conclude that the data "strongly suggest that gas hydrates do not occur on any of our depth transects spread across the continental slope in this region of the Arctic Ocean". They state that "This contradicts previous modeling and discussions, which due to the lack of data are almost entirely based on assumption".

7) Also important in this context is the assumption of a homogeneous methane hydrate saturation. It was shown by Stranne et al. (2016b) that this assumption is not appropriate. Due to lowering of the relative sea level during glacial periods, the hydrates in the upper part of the present day GHSZ would have been dissociated and outgassed during these periods of time. The forming of a hydrate deposit is slow, and the effect of such outgassing has therefor a very large impact on estimates of future methane gas emissions in the Arctic Ocean. This aspect should at least be mentioned.

The authors state that: "this manuscript is not an effort to improve on the methodology or the estimate of hydrate volume in the Arctic marine sediments". However, as I have outlined above, the outcome of the modeling exercise performed in this ms is sensitive to the assumed present day hydrate inventory, and it needs therefore to be treated with some care. An updated and more realistic estimate of the present day hydrate inventory would have made a nice contribution in general, and one that would benefit the hydrate modeling community in particular.

Methane oxidation within shallow marine sediments

8) P13L294 – The authors mention that methane is consumed in shallow sediments. Considering the main objective of the present ms it might be appropriate to discuss this in a little more detail. There are many publications on this subject. For example, according to Boetius & Wenzhöfer (2013) the proportion of methane consumed varies with fluid flow rate, ranging from 80% in seeps with slow fluid flow to less than 20% in seeps where fluid flow is high.

The hydrate stability approach

The authors state: "In our estimate, we assume no heat changes during hydrate dissociation or gas retention in sediments, and no delay in the time taken for the gas to migrate through the sediments to the seafloor. These effects may slow-down methane flux to the water column in the short term (100 years) by up to >70% (Stranne et al., 2016a)".

9) The formulation “up to more than 70%” seems a bit odd to me. Perhaps it would be more accurate to simply state “more than 70%”? Furthermore, it is not clear to me how the authors ended up with this number. Stranne et al. (2016a) conclude that on a centennial time scale, the hydrate stability approach can overestimate gas escape quantities by orders of magnitude.

10) Summary

a. Due to neglected dynamic processes (the endothermic dissociation reaction and the fact that it takes time for the produced gas to reach the ocean), Stranne et al. (2016a) showed that the method applied in this ms severely overestimates gas emissions (possibly orders of magnitude).
b. In addition, the assumed hydrate saturation is probably largely overestimated (see Fig. 1 above, and points 5-7), and it has been shown that the hydrate stability approach is sensitive to the assumed hydrate saturation (Kretschmer et al, and Hunter et al.).
c. Stranne et al. (2016b) showed that the estimated seafloor gas emissions are reduced by almost an order of magnitude when the effect of glacial-interglacial sea levels is considered (not considered in the present study).
d. Also, the authors do not take into consideration consumption of methane within shallow sediments and by benthic communities.
e. Overall, the Biastoch results should be regarded as hugely overestimated - Kretschemr et al. arrived at an estimate that is more than one order of magnitude smaller, and this is without considering points *a*, *c* and *d* above.
f. Considering that Biastoch et al. reached the conclusion that these emissions will not affect the global climate, I do not see how yet another overestimate of future methane gas emissions is contributing with new knowledge.

I do wonder if an easier way to arrive at the conclusion reached in the present ms (that the change in radiative forcing is negligible) would be to calculate the change in radiate forcing based on emission estimates from previous publications. I also wonder if there already exist studies on the radiative forcing sensitivity to changes in the methane gas emissions. In that case the answer to the main question, regarding changes in radiative forcing due to marine hydrate dissociation in the Arctic, would have been readily available before writing this paper.

As I have already pointed out, I do not fail to recognize the amount of work that has gone into this project. I also realize that my critique is quite harsh. Some of the criticism presented above may be incorrect or irrelevant, and I hope the authors will be able to counter such critique without too much effort.

Specific comments on the text

P2 L20: I would delete the word extremely

11) P2 L40-41: The authors claim that gas seepage is directly connected with dissociating hydrates. As I understand it, the evidence for this remains inconclusive. Furthermore I am not sure that the reference to Berndt et al. (2014) is appropriate. Berndt et al. (2014) present evidence that seepage off Svalbard has been ongoing for at least 3000 years. They state that they “found no direct evidence in the heat flow data that would suggest that the slope sediments experienced decadal-scale warming.” They conclude: “Thus, it is unlikely that an anthropogenic decadal-scale bottom-water temperature rise is the primary reason for the origin of the observed gas flares”. Although they suggest that hydrates are dissociating and forming as a result of seasonal temperature variations, this would have little to do with warming seafloor temperatures. These results seem to contradict the present ms, as well as many previous modeling papers. It would be nice if the authors could discuss this apparent contradiction.

12) P5L110-111: It is stated that the Oslo CTM3 model was run with the extra methane flux until the atmospheric methane burden reached a new equilibrium. I do not understand this method, and the authors could perhaps describe this procedure in some more detail. Was the CTM3 run to a new

equilibrium after each new yearly addition of methane to the atmosphere? Why equilibrium and not transient model run over the 21st century?

P7L158: The year of maximum release seems irrelevant. Variations in ocean temperature output from coupled global climate system models are not predictions of the actual future year-to-year variations but represent the climate variability.

13) P8L168-170: The estimate for the Arctic presented in the Kretschmer et. al. (2015) paper, of 140 Mt carbon, should be mentioned and should be compared to the estimate in the present ms. The difference is two (!) orders of magnitude and is thus not in agreement.

P8L174: For the many reasons given above (points 9 and 10a-d), I do not think it is accurate to describe this number as a "lower limit".

14) P8L183: The Marín-Moreno reference is a study conducted for the South Shetland Margin, Antarctic Peninsula. I assume the authors meant (Marín-Moreno et al., 2015). However, the number taken from this study is the *maximum rate*. It is very important to note in this context that there is zero methane gas emission before year 2060-2085 (depending model run). Note also that Marín-Moreno et al. claim that their estimate is more than one order of magnitude lower than the Biastoch estimate, and that the estimated emissions in the present ms is more than 30% larger than the Biastoch estimate.

P13L299: Incomplete sentence.

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