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Interactive comment on "Satellite ice extent, sea surface temperature, and atmospheric methane trends in the Barents and Kara Seas" *by* Ira Leifer et al.

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The manuscript by Leifer et al. is a bit perplexing to me. While some of the presented data is genuinely interesting, the authors appear to be highly determined to explain the observed anomalies in their data to be solely due to a local release of methane from gas hydrates or subsea permafrost. But in doing so, a vast body of established research on methane emissions from the ocean is ignored, and alternate, non-local causes for the observed anomalies are too easily discarded or not even considered. This distracts from other data presented in this paper that are interesting, such as the increase in sea surface temperatures. In fact, the editor of an earlier version of this paper, at this journal, suggested not to include the methane component, but apparently this advice was not followed while resubmitting (see https://www.the-cryosphere-discuss.net/tc-2018-75/tc-2018-75-EC1.pdf). Unfortunately, the methane analysis has too many weaknesses and should not be included in this manuscript.

First of all, I would like to point out that this paper does not present a single methane measurement from the ocean itself. If there was a local hot spot of methane emissions from the ocean, the concentration of methane in the surface water should be elevated also. Measurements of this could have been useful to, at least, compute the diffusive flux to the atmosphere. In addition, sonar could have been used to show that bubbles were actually rising to the surface, but also this is missing. What we get instead, is a flawed satellite product (as pointed out in the comment from Lori Bruhwiler) and a time series of atmospheric methane concentrations. But anyone who works with flux measurements knows that atmospheric concentrations are not the same as fluxes since concentrations are strongly influenced by advection. The comment from Lori Bruhwiler shows that this may well have been the case here. To discard that option, and to provide an indication that the methane is emitted locally, in-situ confirmation of raised methane concentrations in the seawater are essential. Besides, contrary to the authors' claim, Fisher et al. (2011) showed that the studied part of the Arctic Ocean is not a large source of methane. Air masses that travel over the Barents Sea towards Svalbard are not enriched in methane by the ocean. Rather, isotopic analysis and air mass back trajectories point towards terrestrial wetland sources and gas field emissions from West Siberia.

But there are other shortcomings. The main author knows very well that most methane is lost from bubbles while they ascend up the water column. In fact, according to his research, and as stated in this manuscript, 75% of methane is lost in water as shallow as 70 m (Leifer et al. 2017). Overall, the waters in the studied area are much deeper. The authors hypothesize that emissions are still possible due to an oily layer around the bubbles, which would prevent the methane from dissolving into the water, but this can't be true for all hot spot regions indicated in this paper. In any case, this is pure speculation since not a single bubble was observed.

It is established knowledge that methane hydrates off the coast of Svalbard are releasing methane bubbles into the ocean water. In fact, they may have been doing this for $>$3000 years (Berndt et al. 2014). But these hydrates are located at $\sim$250 m depth and almost none of this methane reaches the atmosphere, as pointed out by the extensive study by Lund Myhre et al. (2016). This study is only briefly mentioned by the authors – in the supplement – and discarded as a 'snap shot', even though previous ocean cruises in the same area also showed that bubbles don't reach the surface (Westbrook et al. 2009). Rather, the minute methane emissions from that area are associated with upwelling of cold, nutrient-rich water, which stimulates in-situ production and therefore CO_2 uptake (Pohlman et al. 2017). This uptake of CO_2 is up to 231 times stronger in radiative forcing than the release of methane. Despite all this strong in-situ evidence that there are no significant methane emissions in this area, this paper ignores this and relies on problematic satellite retrievals to hypothesize that methane is reaching the atmosphere along the west coast of Svalbard.

There are many other problems with this manuscript. The authors claim that sea ice reduction increases methane emissions to the atmosphere (line 61-62) but no reference is given to support this. Their own atmospheric observations agree extremely poorly with the satellite product, especially on the return journey in September, but this is brushed over in line 415. Despite the poor match between in-situ measurements and the satellite product in the month of September, the satellite product is uncritically used to show a high trend in that month in Figure 10. Also, the authors refer to Figure S7 as if it shows trends, but the figure shows means.

Given the problems with the satellite retrievals and the limited information that can be derived from the atmospheric observations alone (without measurements in the sea water or an analysis of remote transport), alternative explanations for the observed patterns cannot be excluded. This paper, therefore, does not present convincing evidence that the Barents and Kara Seas are large sources of methane to the atmosphere. It is important to note that previous claims of high emissions from the Arctic Ocean, in particular the Laptev Sea, were strongly adjusted downwards by later studies (Berchet et al. 2016; Thornton et al. 2016). Extraordinary claims of large emissions require extraordinary evidence. This paper does not present it.

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