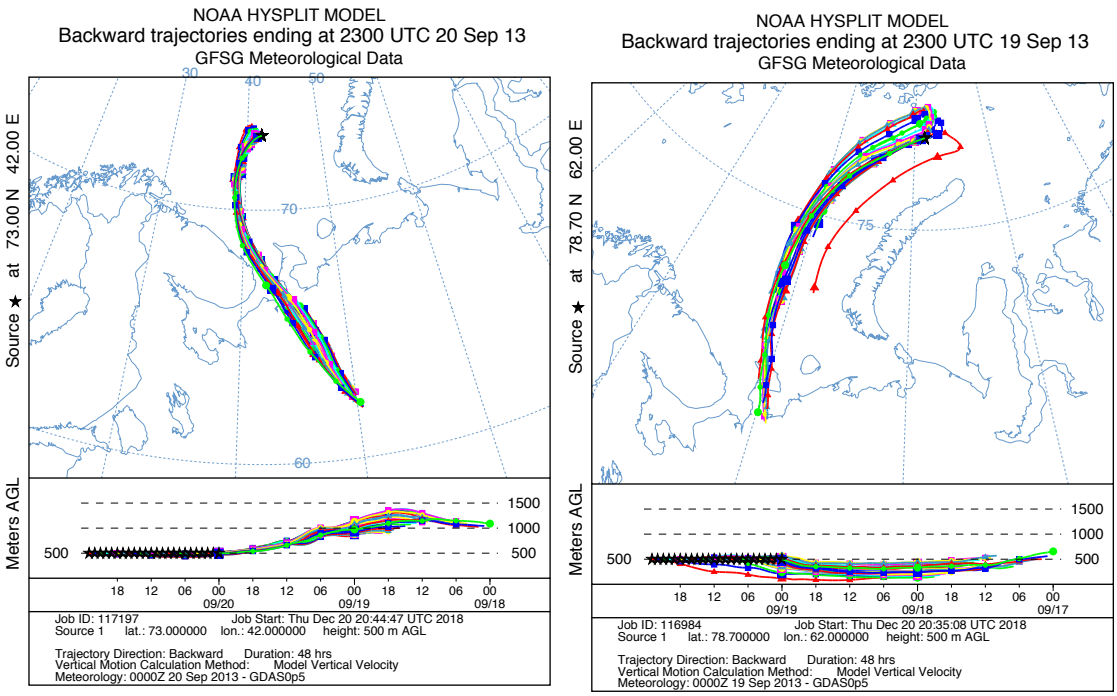
There are numerous problems with this paper, and it should not be accepted for final publication without extensive revisions. First and foremost, the TIR CH_4 retrievals cannot be used for the analysis described here. Since these instruments are sensitive mainly to the upper troposphere, the retrievals near the surface are not trustworthy. Indeed, the spatial variability of CH_4 shown in Figure 1 looks suspiciously like a response to surface topography and sea ice coverage. An additional tip-off that there are problems with the retrievals near the surface is the sharp delineation of coastlines. Although there is a discussion of the shortcomings of *in situ* data, there is no discussion of the many problems associated with the low altitude remote sensing data used here. At a minimum, the authors should show a figure comparing their 0-4km column data with *in situ* measurements at one of the many Arctic atmospheric monitoring sites (the one at Svalbard would be a good choice). This figure should appear in the main text, and not in the supplemental material. The lack of agreement between the retrievals and the shipboard data shown in the paper does not inspire optimism about the CH_4 retrievals.

Even if the TIR CH_4 retrievals were useful for this analysis (which they are not), there are still many other problems with this paper. It builds an entirely circumstantial case and ends up giving no estimates that allow readers to evaluate the potential importance of the process they champion. How many $TgCH_4$/yr are emitted to the atmosphere and how does this compare to current global emissions of ~550 $TgCH_4$/yr? Are emissions changing over time as the authors imply? The fact that CH_4 can be transported to atmosphere via bubbles in shallow water is well established in the literature, so this in itself is not new. The water depths are significantly greater in this study, and I suppose this motivates their need to consider "methane shoaling", but an examination of water column oxidation rates is still needed here to show that the proposed process is feasible. There are numbers in the literature the authors could use (e.g. Ruppel and Kessler, 2016), and it would improve the paper if the authors made an honest attempt to do this (though the misuse of the satellite data would still disqualify this paper as is).

The literature cited in this paper is cherry-picked to support the author's case. For example, there is no mention of the study of Thornton et al. (2016) for the ESAS. Although they found emissions to the atmosphere from bubbling hotspots in shallow water, they found that emissions were likely to be ~10 times lower than the estimates of Shakhova et al. cited by the authors. There is also the study of Berchet et al. (2016) who also found much smaller emissions from ESAS than the Shakhova et al. studies. In addition, Berchet et al. used $^{13}CH_4$ observations to show that the observed summertime anomalies were likely due to biogenic processes and not subsea hydrates. Furthermore, although the Reeburgh (2007) review is cited, there is no mention of the more recent comprehensive review of Ruppel and Kessler (2016). The latter review concludes that subsea hydrates are unlikely to be an important CH_4-climate feedback.

Finally, I would like to address discussion regarding the *in situ* shipboard observations shown in Figure 5. Here, CH_4 hotspots are shown and it is stated without proof that these are associated with subsea hydrates. CH_4 anomalies were also observed for the ESAS in the Shakhova et al. and Thornton et al. studies, although they also observed bubble columns in the same area. It does not seem that collocated bubble columns were observed here, but the authors would likely argue that the "methane shoaling" hypotheses can still be invoked. The authors argue that the hotspots are far enough from land that they must be due to hydrates, however, this is neither convincing or adequate. A better approach would be to use a trajectory model and the footprint approach to estimate the possible contributions from other sources. One such model can be

readily run on-line (the NOAA HYSPLIT model, https://www.ready.noaa.gov/). The two figures below suggest that it is possible for coherent airmasses to arrive at ship locations after having recently passed over regions with anthropogenic and natural CH_4 sources. I suspect it is possible that some of the high shipboard observations were actually from terrestrial natural or anthropogenic sources. To estimate the potential contribution of terrestrial CH_4 emissions to the hotspots observed by the ships, one would have to use emission estimates along with trajectories, but these are readily available in the literature. The authors should consider testing their hypothesis that the hotspots in the shipboard data are indeed caused by subsea hydrate emissions. However, if the authors were to make an honest attempt to rule out terrestrial emissions as causes of the *in situ* data hotspots, the bulk of the paper relying on the TIR retrievals is still unacceptable for publication.



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