Comments on egusphere-2026-3607

Summary

The manuscript by Sert and colleagues presents the results of a synthesis on Arctic methane measurements conducted during several campaigns over nearly a decade. The main goal of the study is to evaluate the potential effects of sedimentary methane release in water column biogeochemistry of the North Atlantic sector of the Arctic and the Barents Sea. To this end, the authors used the methane dataset in combination with ancillary geochemical and hydrographic data to i) assess methane distribution in the region and ii) quantify measurable changes in nutrient and carbon dynamics related to methane release.

Overall assessment

The topic of the manuscript is relevant, entails an impressive compilation of in-situ observations and it is generally well written. Nonetheless, in my opinion there are several caveats that would need to be revised before considering publication. One of the major issues I see is that the novelty does not come across clearly. Such a data compilation for the region is new and valuable or course, but the conclusions that the water column biogeochemistry (especially in deep water areas) is not noticeably affected by methane release, and that the sedimentary emissions are highly heterogeneous are not precisely new. Furthermore, while clearly the observations provide a comprehensive overview of the regional distribution, I could not identify substantial evidence in direct support of the arguments regarding methane-derived carbon in the system (most arguments were drawn based on literature).

Another issue I see is the weight that is given to the regional variability in environmental conditions. Within the methods section ("2.1 Study Areas"), the authors provide an extensive description of the hydrographic and geological properties of the sub-regions they have identified. Yet, later in the manuscript the authors state that actually (based on their isotopic data) all sub-regions could be objectively clustered in two groups ("Atlantic" and "Barents Sea"). Knowing this, it appears unjustified to have all the details on all regions if the dynamics come down to the main water source. At the end, the large descriptions presented in the methods result being only loosely connected with the results and the conclusions of the study. In this regard, I wondered whether an angle that would emphasize the impact of this data compilation would be to aim exploring mechanistic explanations for the methane distribution based on the data at hand and compare the two main areas in view of projected changes (e.g. increased Atlantic inflow into the Arctic). For instance, if the methane filter is more effective in the Barents Sea, could it be that warmer waters form the Atlantic will make the filter weaker, leading to enhanced release? The ancillary data should allow checking for such a targeted comparison which would be, I think, insightful.

Overall, I see this could be a valuable contribution, but I would recommend revising the framing of the study based on the type of observations that it comprises. On the following I list additional comments which I hope are useful for the authors when preparing their revision.

Specific comments

l. 46–50: This is a good point. However, as large as the data set is, it is also a collection of localized measurements that represent only a snapshot of what happens on a certain location. For instance, most data sets are confined to summer, which while understandable given the difficulties of sampling in the Arctic during winter, is not discussed here. I would argue that assessing mechanistic explanations for the methane (and oxidation rates) distribution could be a significant improvement,

because it would provide insights on how the system might change under varying environmental conditions.

l.57: There is also the paper by Thornton et al. (2020; Science Advances, DOI: 10.1126/sciadv.aay7934). Perhaps the authors might consider citing it in this context.

l.122 and ff.: Basic data on accuracy and calibration procedures for the methane measurements is missing. In particular, with regards to calibration, it is unclear which reference gases were used and the calibration scales to which those can be traced back to. Considering that the dataset contains samples that span nearly a decade, I doubt the same reference gases have been used. However, in any case it is important to mention this to show the extent at which data are comparable.

l.128: A note indicating that isotopic oxygen measurements are being used as a water mass tracer would be useful for the reader.

l. 147–149: This is also an instance in which it would be important to provide details on how the data was quality-controlled and harmonised before being combined.

l. 152–153: The details of this are essential for the interpretation of the methane data in this study. As such, there should be clarity on how this background concentration was computed.

l.166: To my understanding, works in preparation are not allowed as references.

l. 205–207: A table would be clearer for presenting all these values.

l. 205–213: Also, perhaps the authors could consider replacing the T-S in figure 2 by a T-S plot in which methane is depicted as 3 (z) axis, such that the large-scale distribution can be more clearly seen.

l. 216–217: For the reader it would be easier to see a column with the numbers of the regions according to Fig. 1.

l. 290–310: This section is fairly descriptive and in a way redundant considering Table 2 and Figure 3.

l. 325–326: In the methods there is no mention of ROV sampling. This should be revised.

l. 340–345: Along the lines of one of my comments above: if the data can be clustered in two groups (i.e. Atlantic-dominated and Barents Sea), it would be more consistent (and potentially clearer) to keep the separation in this figure instead of showing the individual sub-regions.

l.384: It is not clear what is meant with “stable nutrient couplings”.

l. 416–421: This paragraph is redundant with the introduction (c.f. l.61–64) and the connection with own data is not sufficiently clear. Also, given that this is an important argument in explaining the differences between conditions that foster methane accumulation vs rapid dilution and transport, I would kindly suggest exploring the possibility of computing a proxy for water column stability to better substantiate the interpretation.

Kind regards,

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