

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

**Ira Leifer et al.**

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Dear Jenny,

Although upon submission, I felt that this manuscript was one of the best edited I have submitted, I find myself agreeing that the manuscript needed major work at the editorial level along. In that regards, we have followed the suggestion of Reviewer #2 and greatly reduced (about 40%) the introductory material both through moving some material to supplemental and by removing repetition after significant re-organization to improve the flow.

With respect to the very significant point that SST is not current, we acknowledge that this is true, but missed our point, which we evidently did not explain clearly. Specifically, that fact that there is CH<sub>4</sub> emissions requires seabed heating, which cannot be explained if SST trends only represent a skin effect or changes in downwelling thermal radiation. Moreover, the importance of shoaling argues that just as seabed methane is not being effectively transported vertically to the sea surface, then atmospheric heat transfer to the upper ocean cannot be effectively transported to the seabed (to affect seabed methane emissions). That said, we recognize that correlation is not causation, and that proving causation is beyond the scope of this manuscript. Thus, we now reference the hypothesis that SST trends are consistent with increasing heat transport by currents.

We do acknowledge that SST depends on a number of factors including downwelling radiation and atmospheric temperature (and hence transport). Thus although our analysis relied on the spatial localization of SST changes that would be hard to explain from large scale atmospheric processes, we agree that this must be introduced and properly discussed.

We also identified many small errors in Fig. 4a, which we have corrected. Currents in Fig. 4a also have been improved. Also, we have improved all other figures for clarity and readability and as suggested by the reviewers.

We would like to argue that the key highlights from our manuscript make it still suitable for The Cryosphere, the novel mechanisms of shoaling which affects our understanding of the fate of seabed methane, particularly in a sea like the Barents Sea, where currents drive water to shoal as it exits and crosses banks. Also important is the discovery of methane sources associated with Franz Josef Land and off the west coast of Novaya Zemlya and from the areas of predicted oil and gas reservoirs in the Barents Sea.

Although we describe the relationship of SST anomaly trends to currents as a hypothesis, we believe the synoptic satellite data that our analysis leverages is sufficiently

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strong and consistent with the hypothesis that it would be of value to The Cryosphere community to spark discussion and to encourage the collection of field data to validate or disprove the hypothesis.

Thus we respectfully request your consideration of our paper to continue in the review process and/or as a new submission.

Sincerely, Ira Leifer and co-authors

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