# Peer-Review of Alexie Roy-Lafontaine et al.

Title: Addition of brackish water to tundra soils does not inhibit methane production: implications for Arctic coastal methane production

**General comments:**

The authors present a novel, valuable and well-executed study on methane production dynamics in Arctic coastal soils under the influence of brackish water, simulating coastal processes such as tidal flooding and storm surges.

The study shows that methane production is significantly higher in coastal permafrost soils compared to inland sites. By combining field sampling along a land-to-ocean gradient near Tuktoyaktuk, NWT, Canada with long-term anoxic incubations and stable $CH_4$ isotope analyses, the study offers new insights into methane dynamics in underexplored coastal permafrost environments.
Contrary to traditional assumptions, the presence of sulfate-rich brackish water does not suppress $CH_4$ production in coastal and inland sites. Instead, sea level rise and coastal processes such as tides and storm surges may enhance emissions by increasing the lability of organic matter. The findings also suggest that methanogenesis can coexist with sulfate reduction, potentially through syntrophic or methylotrophic pathways.

**Strengths of the study:**

- Innovative experimental design that simulates realistic coastal environmental change.
- Comprehensive data collection across inland, coastal, and marine sites, combined with robust geochemical and isotopic analyses.
- The study provides important insight into underexplored Arctic coastal methane sources.
- The extrapolation of findings to landscape-scale $CH_4$ production enhances the relevance of the results.
- The authors discuss the potential overestimation due to closed-system incubations.

**Specific comments and recommendations:**

1) Did you include control incubations without brackish water? If not, I would suggest this for the next incubation study as the comparison would strengthen the interpretation of the brackish water effect and sulfate inhibition hypothesis.

2) I am missing a discussion on methane oxidation since in the ocean a large percentage of the produced $CH_4$ is oxidised before reaching the atmosphere.

3) Consider discussing the seasonality or temporal dynamics of coastal $CH_4$ production, even if only conceptually.

4) In my opinion, you are using the term "active layer" also for unfrozen zones beneath water bodies, which are referred to as "taliks" (for example, at the Harbour site). This should be expressed more precisely throughout the manuscript.

Line 15: I agree in general that the processes of erosion and subsidence on carbon emissions are understudied but I would be careful with the wording here. There are some publications e.g. Tanski et al., 2019 measuring C release caused by erosion.

Line 42: For a more concrete statement, could you give examples for rapid environmental changes?

Line 55: I like this figure which represents your study sites. In my mind it would fit better in section 2.1. Consider moving.

Line 71: A reference from an Arctic study would fit better here.

Line 114 – Figure 2: For a better orientation please add a dot for Tuktoyaktuk or add to the description that the Harbour is located in Tuktoyaktuk. Further I strongly recommend to add detail maps of the individual study sites (eg. high resolution satellite images where the coring locations are marked) to get a better understanding of the landscape and the exact sampling positions.

Line 118: I like the clear explanation on why you chose the sampling sites.

Line 125: Please add a range of the core lengths.

Line 132: In figure 1 pondlets are labled as thaw ponds. For consistency I would recommend to use the same terms. You could also label each core and refer to that in the text.

Line 141: If I'm not mistaken you are using the terms profile and core equally but they represent different approaches. I guess from a trough you collected a core? Please explain and/or revise the manuscript accordingly. It would be helpful to have profile/ core pictures and some additional information such as a brief description and profile depth /core length for all sites. Especially for the profiles numbered with 10 a detail map would be ideal to get an understanding of the location and the environment.

Line 183-186: Do I understand correctly that the gas concentration measurements were not continuous throughout the entire 339-day period? Was the final measurement on day 339? What do you mean by back calculating? Were the $CH_4$ production rates calculated solely from the linear accumulation observed during the first 16 weeks?

Line 217: I am no expert on stable methane isotopy

Line 238-244 (Figure 3): I like your figures but to me the graphs in plot c and d are hard to distinguish, especially 10A and 10C. Think of adjusting the colours. All information needed is written in the figure caption but I think the legend could be improved by adding some more

information such as location name. You are talking about the active layer at the harbour site. Do you really mean active layer or rather talik? Below waterbodies the unfrozen layer usually is called talik.

Line 266 (Figure 4): Add to the caption that the error bars are the grey lines. To me the error bars miss the end point marking but if you note in the caption that the grey lines are the error bars, it's clear.

Line 303 (Figure 6): I suggest to relocate some of the information given in the caption to the figure itself, such as "acetoclastic" and "hydrogenotrophic methanogenesis" you also could add "permafrost" to panel b. Please label the vertical line. This makes it easier for the reader to understand your figure.

Line 342: In general, it is true that long-term sulfate input is inhibiting methanogenesis, but there are field studies which show that low sulfate concentrations or recent inundation is either not impacting or even promoting methanogenesis. Please distinguish this statement and incorporate more recent findings. For example, Yang et al., 2023 (https://onlinelibrary.wiley.com/doi/10.1111/gcb.16649); Jenrich et al., 2024 (https://onlinelibrary.wiley.com/doi/abs/10.1002/ppp.2251) and Jenrich et al., 2025 (https://bg.copernicus.org/articles/22/2069/2025/).

Line 355: You could add Yang et al., 2023 as an example for thermokarst lagoons which are a transition zone from terrestrial to marine environments.

Line 414-417: Very cool finding! I agree with that. In a recent study I also found that $CH_4$ and $CO_2$ production is highest during the first stages of land-sea transition and that $CH_4$ production decreases with increasing marine influence.

Line 456: Great to see that in situ flux measurements and incubation results are in line.

Line 468-474: I like the comparison and reasoning, and I agree that Arctic soils represent an important source of $CH_4$. However, to strengthen the argument on a global scale, it would be helpful to include a size comparison between Arctic coastal wetlands and tropical coastal wetlands.

494: I like the upscaling approach to show the impact of sea water inundation on a bigger landscape scale.