In response to the reviewers' comments, we have revised our response document to explicitly detail the modifications implemented in the submitted manuscript. We thank both reviewers for their insightful and constructive analyses, which we believe have strengthened the manuscript. The response document is different from the initial response to reviews because we have added additional material to the one stated originally. The responses are in Blue, the text modification/insertions are in **Bold Blue**.

R1

Thank you for taking the time to review our study with very insightful and constructive comments. Here are our answers to the specific comments highlighted by R1:

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.

We agree with R1 that controlling for addition of brackish water is difficult and a limitation of our study. However, the solution to understanding brackish water addition is not as trivial as adding control incubations without brackish water. First, many sediments are dry when sampled. When these are put under anaerobic conditions, the results are, expectedly, zero activity of methanogens. One solution to this is to add water to the sediment in order to at least have aqueous water-saturated pore spaces in all incubations. One solution to this is to add distilled water to incubations. Distilled water incubations have indeed been added in the next set of incubations studies from our group. However we must also stress that a comparison between distilled water incubations and brackish water incubations is not a tool that will solve all the shortcomings of our study by revealing the role of brackish water. Incubations are sensitive experiments subject to a unique set of conditions. While brackish water addition is an environmentally relevant process that occurs on the Arctic coast, under no circumstance is addition of distilled water something that would occur in the environment and all conclusions from these should be taken very cautiously. For example, I would not think that we could calculate the effect of brackish water addition by substracting our brackish water incubations results from our "control" incubations. It could get complicated…and potentially completely wrong, very quickly. However, to frame our results as carefully as possible we did respond to this comment by modifying in the current manuscript by comparing incubation studies without addition of brackish water that were performed near Tuktoyaktuk (Lapham et al., 2020) and compared qualitatively to our results:

Line 347. We note that our experimental design did not include parallel incubations without brackish water or with sulfate concentration gradients; therefore, our interpretation relies in part on comparison with previous incubations of Tuktoyaktuk soils conducted without brackish water addition (Lapham et al., 2020) and should be regarded as exploratory rather than definitive.

2. I am missing a discussion on methane oxidation since in the ocean a large percentage of the produced CH4 is oxidised before reaching the atmosphere.

We will add a few sentences in the discussion part where we discuss oxidation in permafrost soils. One thing that needs to be made clear however is that methane dynamics in the ocean seafloor are not at all related to the processes producing methane in our coastal sites. Methane produced in the seafloor is produced below the sulfate-methane transition zone, often many meters or even tens of meters below the sediment water interface. This methane diffuses upwards through the sulfate-methane transition zone where it is oxidized by specialized anerobic methane oxidizers that exist in consortia that can take decades to develop. This is not the case in our coastal sites where the dynamic nature of the soils or sediment mean that stratified zones such as those seen in the seafloor are not well defined and stable. We have therefore focused the discussion on dynamic coastal settings. Here is the modified text to enhance the discussion directly related to methane oxidation in the context of our study:

Line 522: While AOM represents a major sink for CH_4 in marine sediments (Knittel and Boetius, 2009; Reeburgh, 2007), the very different biogeochemical and hydrological characteristics of our coastal sites suggest that the role of AOM in these environments may diverge from that observed in fully marine systems. Recent work in coastal thermokarst lagoons, which can present key similarities to our coastal study sites due to episodic or persistent brackish water intrusion, have been shown to exhibit strong AOM control on CH_4 dynamics, particularly in sulfate-rich settings where AOM may constitute a major CH_4 sink (Yang et al., 2023).

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

Thank you for this suggestion. We will add a discussion on seasonal CH_4 production in the introduction to increase the clarity that our study evaluates CH4 dynamics during open-water season. Here is the suggested modification text:

Line 54. During growing season, where atmospheric temperatures allow for active layer to thaw and vegetation to grow, hydrological conditions in polygons play a pivotal role in shaping the pathways of OM decomposition and consequently influence the resulting CO_2 and CH_4 production. Well drained oxic conditions allow microbes to decompose OM rapidly, leading to the production of CO_2 (Jones et al., 2020). Conversely, water saturation restricts oxygen availability, promoting anaerobic respiration and fermentation, inducing both CO_2 and CH_4 production (Lipson et al., 2012; Turetsky et al., 2008). Thus, coastal changes and higher atmospheric temperatures during open-water season can swiftly alter water saturation conditions in polygons, in many cases significantly enhancing fermentation and CH4 production (Elberling et al., 2013; Holm et al., 2020; Treat et al., 2015).

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.

Correct, we modified for pondlets, troughs and harbour site. **Those changes are marked all along 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.

We agree that carbon release by erosion has been the subject of many studies. However, we also think that the fate of carbon in system that will be subject to subsidence are understudied. We will modify the text to make make it clear that erosion and subsidence are two different processes that affect coastal. Here is the modification:

Line 15. In Arctic regions where coastal sediments contain permafrost, global climate change drives processes such as erosion and subsidence. The contribution of these processes to carbon emissions, especially from ground subsidence, are still uncertain.

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

Here's the sentence at line 42: "Inputs and outputs of the Arctic carbon biogeochemical cycle are known to be reshaped by rapid environmental changes (Couture et al., 2018)." We would prefer to keep the sentence as is because we think naming all environmental changes would lengthen and divert from the main focus of the paper. We name a few of those rapid environmental changes at line 35 with references to literature.
However, if pressed, we could add that "rapid environmental changes are permafrost thaw, sea ice decline, sea level rise, coastal erosion, land subsidence, changes in hydrology and river discharge, warming temperatures, higher frequency in storm events and shifts in vegetation and land cover" to name a few more examples.

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

We moved the figure to section 2.1 (line 127) but kept the reference to this figure in the introduction as we believe it gives a simple visualization of polygonal ground to the reader.

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

We understand the reviewer's point. However, we prefer to retain the reference to coastal environments from the Brittany coast (France), as it provides valuable background context on carbon biogeochemistry from a well-studied system. Our intention here is to illustrate general biogeochemical processes in coastal environments rather than to focus specifically on Arctic systems. Substituting this reference with one from the Arctic would, in our view, shift the focus away from the broader conceptual framework we aimed to establish in this section.

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.

Thank you for the suggestion. We added that the Harbour is located in Tuktoyaktuk in the description, and we also added high-res satellite images of our study sites in the SI**. Here's the added figure in SI:**

Figure S3. High-resolution satellite imageries of a) Reindeer Point (10A, 10B, 10C, 10D) and b)Toker Point (07, 08, 09) (ESRI, 2022).

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

Thank you!

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

Yes, good point. The final manuscript was updated to indicate that the cores collected with the UWITEC gravity corer were approximately 25 cm in length (line 118).

Line 132: In figure 1 pondlets are labeled 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.

We corrected the figure for "thaw pond" but didn't label the cores to keep the figure as light as possible. Here's the modified figure:

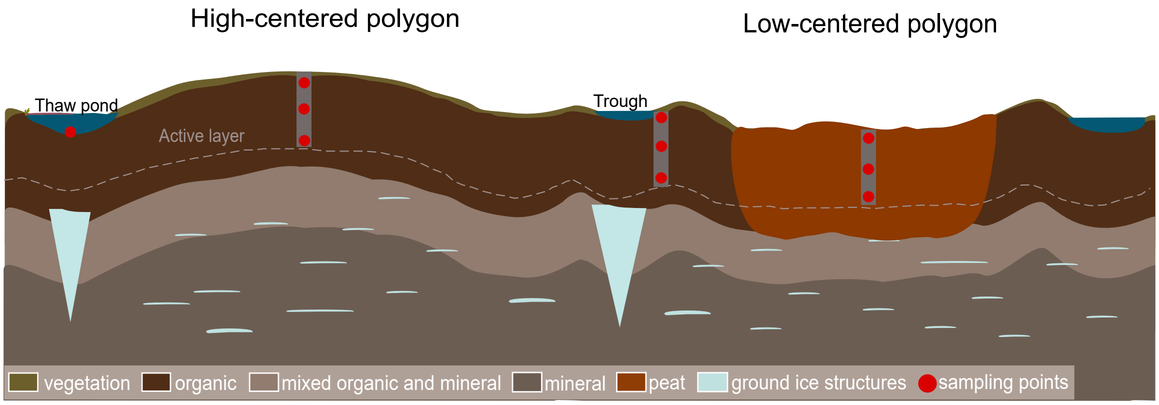


Figure 1. Schematic representation of polygonal tundra with peat accumulation as seen in continuous permafrost environments and sampling design for this study. High-centered polygons are associated with drier conditions, while low-centered polygons, troughs and pondlets are associated with humid or water-saturated conditions. Vegetation cover and OM reflect the hydrology of sites. Not to scale.

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.

The cores refer to the samples taken with the UWITEC gravity corer in the harbour. All soil samples were profiles. For troughs, we obtained a profile from the sides of the trough (water-saturated soil). We will add pictures of collected profiles and cores with their length as well as high-res satellite images. In the methods section, lines 123 to 129, we differentiate between cores and profiles, stating that cores were taken in the Harbour and profiles were collected from TP and RP sites. However, we acknowledge that in the abstract, line 22, we did not differentiate as a matter of being more concise. Here's the edit brought at line 22:
To better constrain CH_4 production dynamics along the land to ocean continuum, sediment cores were collected from nearshore marine sediments and soil profiles from the active layer of the coastal (intertidal) zone and inland soils.

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?

We have fixed the wording to remove the word backcalculated as this is not what was meant. What we meant was that the rates were calculated from the linear accumulation observed during the incubations. The final measurement was on day 339. We were not able to measure gas concentrations in the incubations the first few weeks.
Line 193: The resulting production rates were calculated from the linear accumulation measured during the incubation period

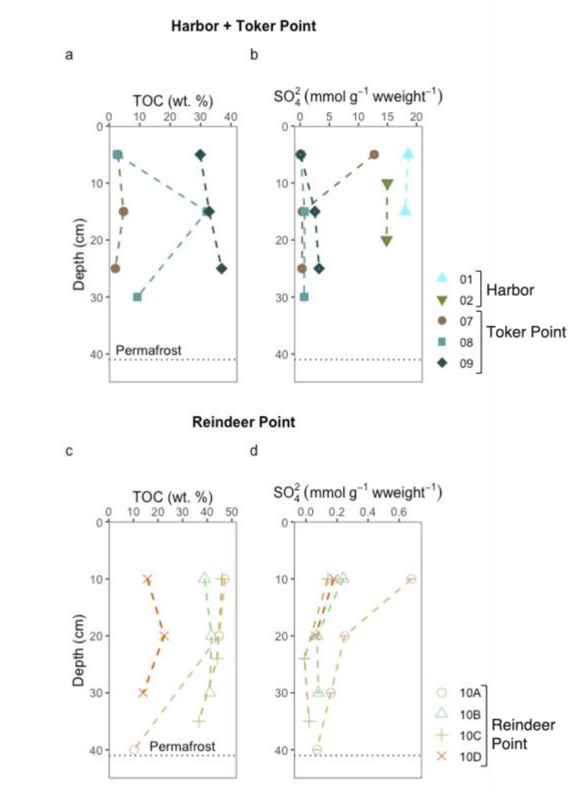
Line 217: I am no expert on stable methane isotopy

That's ok! We thank you for your constructive comments!

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.

We added specific location name in the legends of the graph (see below). We have also fixed the wording throughout the manuscript. When the samples were taken from perennially thawing and freezing sites, these are now referred to as active layer. Sites (such as in the ocean) where samples were taken that do not freeze and thaw perennially, are referred to as talik.

Line 247.



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.

We added that the error bars are the grey lines in the caption, thank you. (line 277).

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.

Thank you for pointing this out. We labeled the vertical line for ebullition (see below). As for adding “permafrost” to panel b, since all our figures are labeled the same way, we kept as is. We wish to keep the theroretical explanation of acetoclastic vs hydrogenotrophic methanogenesis in the figure caption as it’s not a discussion point or a result.

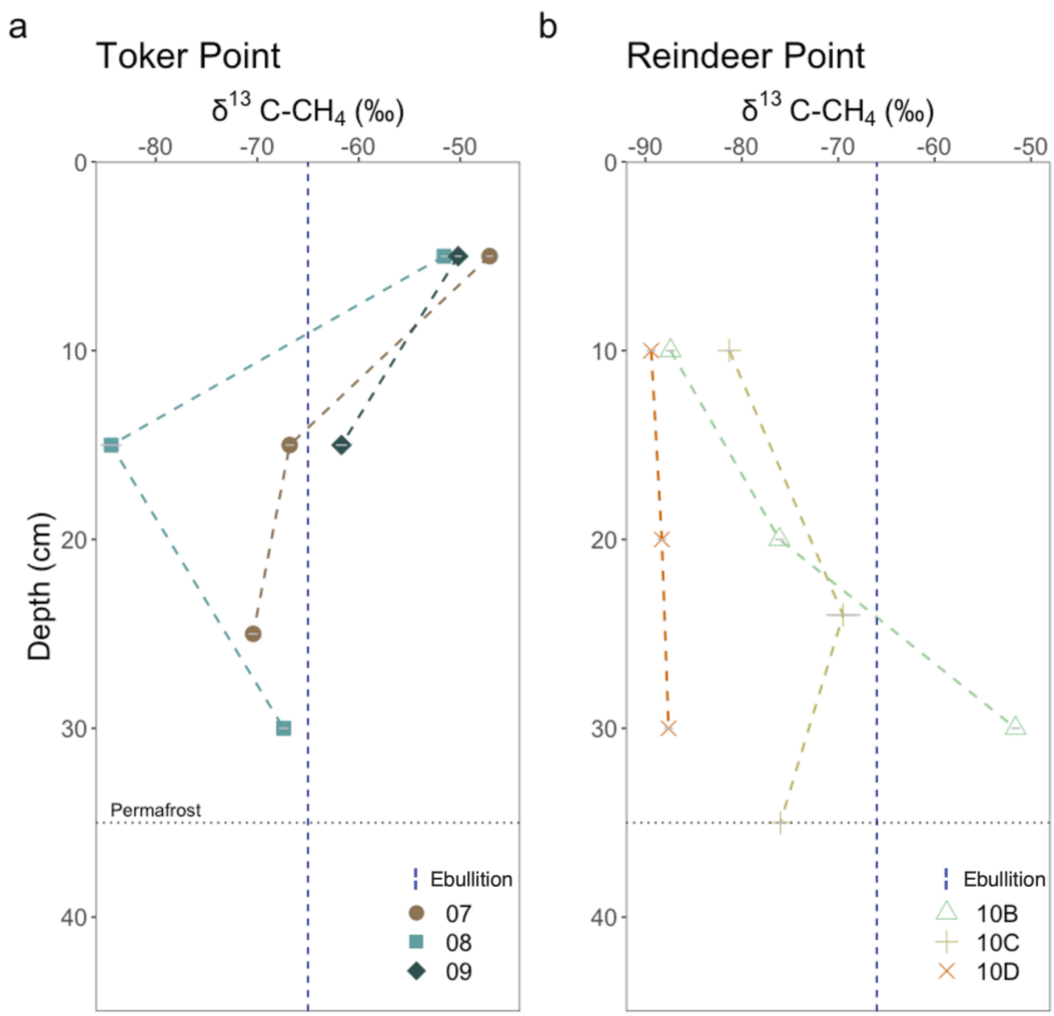


Figure 6. Isotopic composition of CH_4 produced in brackish water incubations from (a) TP and (b) RP. Each datapoint corresponds to the mean value of two or three measurements done on one incubation, depending on the headspace concentration. The dashed vertical lines correspond to in situ ebullition CH_4 collected in pondlets at each sampling site (n=1). These values give information on the pathways used by the soil microbes to produce CH_4. $\delta^{13}C$ between -65‰ and -50‰ is typically associated with acetoclastic methanogenesis, while $\delta^{13}C$ between -110‰ and -60‰ is associated with hydrogenotrophic methanogenesis

(Hornibrook et al., 1997, 2000). The grey error bars on each point represents the analytical uncertainty on the measured value. If not visible, the uncertainty is smaller than the point.

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/).

Thank you for this input, we added nuance with more recent findings. Thank you for providing literature on the matter. **Here are the edits at line 365:**
This hypothesis is also consistent with field observations; organic matter mineralization in brackish wetlands is consistently dominated by bacterial sulfate reduction (Bridgham et al., 2013; Torres-Alvarado et al., 2005) where little to no CH_4 emissions are observed (Pönisch et al., 2022; Petersen et al., 2023; Kroeger et al., 2017). However, recent field studies show that in coastal permafrost soils, inundation and low sulfate concentrations do not necessarily suppress methanogenesis (Jenrich et al., 2025; Jenrich et al., 2024; Yang et al., 2023). These contrasting observations reveal a key knowledge gap in how marine influence controls carbon mineralization pathways in permafrost systems. By experimentally testing brackish water additions across sites with contrasting marine exposure, our study provides new mechanistic insight into the regulation of OM degradation and CH_4 production under ongoing Arctic coastal change.

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.

Thank you, very relevant, we added that reference to thermokarst ponds (line 388).

Line 414-417: Very cool finding! I agree with that. In a recent study I also found that CH4 and CO2 production is highest during the first stages of land-sea transition and that CH4 production decreases with increasing marine influence.

Awesome, we are glad that our findings align!

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

So are we!!!

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.
Yes, thank you for the relevant idea. **Here are the edits that were inserted starting at line 505:**

CH_4 emissions and production within areas of coastal influence thus appear of similar magnitude. By comparison, mangrove forests, which are a major global source of CH_4 but a very different environment from coastal Arctic polygon terrain, had average CH_4 fluxes to the atmosphere of 0.3 +/- 0.1 mmol m^{-2} d^{-1} (Rosentreter et al., 2018). In another study, the average measured CH_4 flux from a Yangtze Estuary (China) tidal salt marsh, with a subtropical monsoon climate, was 2.4 mmol m^{-2} d^{-1} (Li et al., 2021). These reported values are similar to our study as well as other studies in the region. When considered alongside the global distribution of coastal wetlands, this similarity in flux magnitude becomes particularly relevant. Tropical coastal wetlands are dominated by mangroves (~147,000 km^2), whereas Arctic wetlands cover approximately 3.5 million km^2 (Wortington et al., 2024). Even if only a small fraction of Arctic wetlands is located within coastal zones, their total extent is comparable to the global mangrove area (Wortington et al., 2024), suggesting that permafrost Arctic coastal wetlands could represent a non-negligible component of the global CH_4 budget and warrant further investigation.

This is also an exercise we enjoyed doing!

R2

We thank R2 to have taken the time to review our study. Here are our answers to the comments.

1. No control experiment without brackish water addition was set up, making it impossible to clarify the specific impact of brackish water itself on methane production (e.g., whether there is a promoting effect). Additionally, no experimental groups with different sulfate concentration gradients were established, which weakens the persuasiveness of the key conclusions of this study.

We appreciate the reviewer's comment and agree that control incubations without brackish water addition, as well as experiments with defined sulfate gradients, would have strengthened the study. This has been pointed out by the two reviewer and has been addressed. We agree that these "control" treatments were not included in our experimental design. To partially address this gap, we compared our results with those from a previous incubation study on Tuktoyaktuk soils (Lapham et al., 2020), where no brackish water addition was applied. This provides a useful point of reference, although we acknowledge that it is not a direct control within our dataset. We have revised the manuscript to more clearly acknowledge this limitation and to emphasize that our conclusions regarding the role of brackish water and sulfate should be viewed in this comparative and exploratory context.
Text added to manuscript at line 347: We note that our experimental design did not include parallel incubations without brackish water or with sulfate concentration gradients; therefore, our interpretation relies in part on comparison with previous incubations of Tuktoyaktuk soils conducted without brackish water addition (Lapham et al., 2020), and should be regarded as exploratory rather than definitive.

This being said, we are unsure of how useful control incubations would be or how we would use them to gain additional information. I would like to refer you to the discussion we wrote to a similar point made by R1. Here are the main points pasted below:

We agree with R1 that controlling for addition of brackish water is difficult and a limitation of our study. However, the solution to understanding brackish water addition is not as trivial as adding control incubations without brackish water. First, many sediments are dry when sampled. When these are put under anaerobic conditions, the results are, expectedly, zero activity of methanogens but this is not relevant because we simply have dry material. One solution to this is to add water to the sediment in order to at least have aqueous water-saturated pore spaces in all incubations. Adding distilled water to incubations does this. Distilled water incubations have indeed been added in the next set of incubations studies from our group. However we must also stress that a comparison between distilled water incubations and brackish water incubations is not a tool that will solve all the shortcomings of our study by revealing the role of brackish water. Incubations are sensitive experiments subject to a unique set of conditions. While brackish water addition is an environmentally relevant process that occurs on the Arctic coast, under no circumstance is addition of distilled water something that would occur in the environment and all conclusions from these should be taken very cautiously. For example, I would not think that we could calculate the effect of brackish water addition by subtracting our brackish water incubations results from our "control" incubations. It could get complicated…and potentially completely wrong, very quickly. However, to frame our results as carefully as possible we did respond to this comment my modifying in the current manuscript by comparing incubation studies without addition of brackish water that were performed near Tuktoyaktuk (Lapham et al., 2020) and compared qualitatively to our results.

2. The incubation period lasted as long as 339 days, but the MS did not elaborate on how stable experimental conditions (such as temperature fluctuations and substrate consumption) were maintained during this process, which may affect the reliability of the results.

We thank the reviewer for raising this point. We have revised the Methods section (line 183) to more clearly describe the incubation conditions. The incubations were maintained at a constant temperature of 4 °C throughout the entire 339-day period, with no fluctuations. Substrate concentrations were not actively controlled or monitored, aside from repeated measurements of headspace methane. While we acknowledge that substrate depletion may have occurred over the long incubation period, our primary goal was to assess potential methane production under stable temperature conditions, rather than to simulate closed substrate-balanced systems.
Here's the edited text at line 183: Incubations were kept at a constant temperature of 4°C throughout the entire 339 days incubation period with no fluctuations. Substrate concentrations were not actively controlled or monitored, aside from repeated measurements of headspace methane.

3.No determination was made on sulfate reduction rates or related functional genes (such as the *dsrA* gene of desulfovibrio). The coexistence of sulfate reduction and methane production was only inferred from the "smell of sulfide," resulting in insufficiently direct and adequate evidence.

We thank the reviewer for this important comment. We agree that our study did not directly quantify sulfate reduction rates or functional genes (e.g., dsrA) and that the qualitative note of sulfide odor is not a sufficient line of evidence on its own. As stated in the Methods, monitoring sulfate reduction would have required tracer-based rate measurements (e.g., 35S-sulfate assays) or destructive sulfide extraction methods (AVS/CRS), which were beyond the scope of this study and would have required additional expertise, replicates, and instrumentation. Moreover, given the high concentrations of reactive iron minerals in these soils, dissolved sulfide would likely have been scavenged rapidly, complicating direct detection and quantification.

We also note that microbial community characterization (e.g., detection of sulfate-reducing taxa or the dsrA gene) would not by itself demonstrate active sulfate reduction, as the presence of sulfate reducers does not necessarily imply metabolic activity. For this reason, we believe that such measurements, while valuable, would not have provided conclusive evidence in the context of our experimental design. In light of this, we have revised the manuscript (line 381) to clarify that the coexistence of methane production and sulfate reduction was not demonstrated directly in our incubations. We now present the sulfide odor as an anecdotal observation only, without attaching mechanistic interpretation or weight to it. We acknowledge this as a limitation and suggest that future studies combining tracer-based sulfate reduction assays and microbial functional gene analyses would be necessary to rigorously test this question.

Here's the edited text starting at line 381:

While sulfate reduction rates were not measured and therefore not demonstrated directly in our incubations, a strong sulfide smell was recorded when opening most of the incubations at the end of the experiment. This observation may indicate the coexistence of sulfate reduction and methanogenesis during the incubations. However, to rigorously assess this observation, future studies should include tracer-based sulfate reduction assays and microbial functional gene analysis.

4. In Figure 6, the correlation analysis between the stable carbon isotope composition of methane and methane production pathways only cites the research of Hornibrook et al. (1997, 2000), without conducting in-depth cross-validation with other data in this paper (such as TOC content and sulfate concentration).

We appreciate the reviewer's comment. Our interpretation of $\delta^{13}C\text{-}CH_4$ patterns in relation to methane production pathways was guided by established fractionation frameworks (Hornibrook et al., 1997, 2000). We did explore relationships between $\delta^{13}C\text{-}CH_4$ and TOC and sulfate concentrations but didn't find relevant correlation. We then decided to not focus our discussion on those points. However, we added a figure in SI (Figure S4) to support our exploration and added a justification in our discussion section (line 483):

To evaluate whether $\delta^{13}C\text{–}CH_4$ covaried with other geochemical properties measured in this study, $\delta^{13}C\text{–}CH_4$ values were examined alongside TOC content and sulfate concentrations; however, no consistent relationships were observed across landforms or depths (Fig. S4), indicating that methanogenic pathway signatures are not straightforwardly predicted by bulk TOC or sulfate availability at the scale investigated. However, it is clear that future work should integrate measurements of organic matter degradation, microbial community composition, and pore water chemistry to better resolve the mechanisms driving spatial variability in methane production.

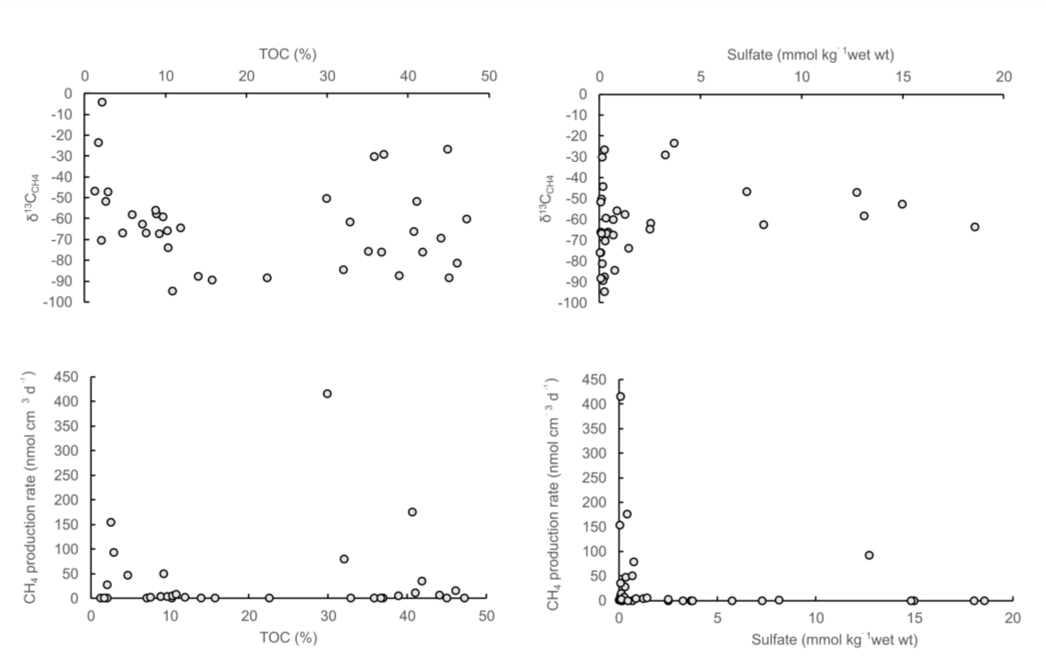


Figure S4. Relationships between stable carbon isotope composition of methane ($\delta^{13}C$-CH_4), total organic carbon (TOC), sulfate concentration, and methane production rates across all sampled depths and sites. Upper panels show $\delta^{13}C$- CH_4 as a function of TOC (a) and sulfate concentration (b). Lower panels show CH_4 production rates as a function of TOC (c) and sulfate concentration (d). Each point represents one sampled depth. No consistent relationship is observed between $\delta^{13}C$- CH_4 or CH_4 production rates and either TOC content or sulfate concentration across landforms or depths, indicating that isotopic signatures and CH_4 production are not straightforwardly predicted by bulk organic carbon abundance or sulfate availability at the scale investigated.

5. Regarding the explanation for the high methane production rate in the coastal zone, it is mentioned that "it may be related to the labile organic matter brought by goose feces" in lines 445-446, but there is a lack of direct experimental evidence (such as analytical data on organic matter in goose feces), making this explanation somewhat tenuous.

We agree with R2 that this is anecdotal evidence. We use this in the manuscript to highlight one observation that stood out at this site. However, it cannot be mechanistically linked with the high methane production rates. The heterogenous environment in coastal NWT makes it difficult to pinpoint the reasons for specific observations and anomalies but it is important for us to be able to report the non-quantitative observations that we noted in the field. We also need to take into account that the goal of the study was not to establish correlations between specific local phenomenon like goose feces and methane production rates but this was nonetheless an observation that stood out that we saw pertinent to report. **We have adjusted the text to ensure that the reader does not interpret this as a mechanistic relationship (line 462):**

This elevated methane production rate coincided with the presence of substantial goose fecal deposits at TP, profile 09. While this observation suggests a potential local

input of labile organic matter and nutrients (e.g., N and P) and possibly a distinct surface microbial community, no direct measurements were conducted to establish a mechanistic link. This site-specific observation is therefore reported as contextual field information rather than evidence of causation.

6. The study found significant differences in methane production rates among different landforms (such as high-centered and low-centered polygons), but key environmental factors of various landforms (such as organic matter degradation rate, microbial community composition, and pore water chemical gradients) were not systematically measured. That is, it is unclear whether factors such as hydrological differences caused by landforms (e.g., water saturation), organic matter activity (as indicated by $\delta^{13}C$-TOC), or microbial community structure dominate the spatial heterogeneity of methane production.

We thank the reviewer for this insightful comment. We agree that our study did not systematically measure the key environmental drivers (e.g., organic matter degradation rates, microbial community composition, $\delta^{13}C$-TOC, pore water geochemistry) that could mechanistically explain the spatial heterogeneity in methane production across landforms. Our primary objective was to quantify and compare methane production potential among contrasting polygonal landforms as a first step toward identifying where hotspots of methane cycling occur. We fully recognize that disentangling the relative influence of hydrology, organic matter activity, and microbial community structure requires a more targeted study design. We have revised the discussion to acknowledge these limitations and to emphasize that future work should integrate these biogeochemical and microbial datasets to better constrain the drivers of methane production variability (line 486).
However, it is clear that future work should integrate measurements of organic matter degradation, microbial community composition, and pore water chemistry to better resolve the mechanisms driving spatial variability in methane production.

7. In the discussion, the explanation of the mechanism by which sulfate does not inhibit methane production (such as non-competitive methanogenesis and syntrophic methanogenesis) was not analyzed in combination with the specific data of this study, making it relatively general.

That's correct, we did not give too deep about the mechanisms and we are limited to using general statements on what could explain the mechanisms observed in our incubation set. Future studies could dig deeper into the processes which prevented the inhibition.

8. For the determination of sulfate and chloride ion concentrations (Method 2.2), it is clearly stated that "only one measurement was performed for each sample." Although stability tests showed a variation rate of < 3%, there is a lack of biological replicates (such as different sampling points of the same landform type), making it impossible to rule out the interference of spatial heterogeneity.
We thank the reviewer for pointing this out. Our discussion of sulfate and methane production mechanisms (e.g., non-competitive and syntrophic methanogenesis) was indeed presented in general terms and not explicitly linked to the data from this study. In our dataset, we observed active methane production even in the presence of measurable sulfate concentrations. However, we did not collect the complementary measurements (e.g., specific microbial functional groups, detailed electron acceptor fluxes) that would allow us

to directly test the relative contributions of non-competitive methanogenesis versus syntrophic interactions in our sites. We have revised the text to make this distinction clearer: while our results are consistent with mechanisms reported in other Arctic and sub-Arctic systems, our data do not allow us to resolve the exact pathway. We now emphasize this limitation and suggest that targeted microbiological and isotopic analyses would be necessary in future studies to address the underlying mechanisms (line 325).

Before discussing the effects of brackish water addition in incubation experiments, it is important to clarify the role of sulfate measured in situ within soil and sediment profiles. Across the studied sites, sulfate concentrations varied with depth and between landforms; however, this spatial variability did not show a consistent relationship with methane production rates measured in the incubations (Fig S4). A few layers clearly contained higher sulfate amounts. However, layers characterized by higher or lower sulfate concentrations did not systematically correspond to lower or higher CH_4 production, indicating that in situ sulfate availability alone does not explain the observed patterns in methane production across profiles. This interpretation is subject to important limitations. Sulfate and chloride concentrations were measured at single points within each profile and were not replicated across multiple locations within the same landform, preventing resolution of fine-scale spatial heterogeneity in electron-acceptor availability. As a result, sulfate concentrations are interpreted here as first-order indicators of geochemical context rather than as spatially representative or mechanistic controls on methane production. Given these constraints, we focus the following discussion on the experimental addition of sulfate via brackish water during anoxic incubations, which evaluate how episodic marine influence may affect methane production potential in coastal permafrost environments.

10. All figures and tables (such as Figure 4, Figure 5, and Table 1) only display means and standard deviations, without statistical tests (such as t-test and ANOVA) to verify the significance of differences between groups (such as different landform types and different sites).

We thank the reviewer for this comment. We agree that statistical tests can be valuable when differences between groups are subtle. In our dataset, however, most of the differences among landform types and sites are large and exceed the range of variability (as shown by the standard deviations). For this reason, we consider the presentation of means with standard deviations sufficient to convey the magnitude of contrasts, without requiring formal hypothesis testing. Our primary goal was to highlight the pronounced differences in methane production potential and geochemical context across coastal and inland sites. We have added clarification in the discussion to explain this rationale.
Line 450. Differences among sites and along landforms in each specific sites are generally large and exceeded the range of variability as shown by the uncertainty (Fig 5), supporting the use of means with standard deviations to convey contrasts without formal statistical tests. This approach allowed us to highlight general differences in CH_4 production potential and geochemical context across coastal and inland sites.

9. In the stable carbon isotope analysis ($\delta^{13}C$-CH_4), only one incubation vial was used for each depth. Although 2-3 instrument replicate measurements were conducted, no biological

replicates (such as parallel incubation vials for the same treatment) were set up, making it difficult to distinguish between real differences among samples and experimental errors.

We appreciate this important point. It is correct that only one incubation vial per depth was used for $\delta^{13}C\text{-}CH_4$ analysis, with 2–3 instrument replicates but without biological replicates. At each depth, we conducted four incubation vials in total, prioritizing robust estimates of methane production rates as the central focus of the study. Ideally, additional incubations per depth would have been performed to allow for biological replication in the isotope analyses as well. However, given logistical and sample constraints, this was not feasible. I will also note here that while methane production rates are quite variable between biological replicates, the carbon isotopes in methane rarely show a correlation with rate within biological replicates. This information was not included in this study but our tests have demonstrated this. We have revised the text to explicitly acknowledge that while the isotope results provide valuable insight into methane cycling processes, they should be interpreted with caution in the absence of biological replication (line 239).
While those isotopic analyses results provide valuable insight into methane cycling processes, they should be interpreted with caution in the absence of biological replication.

We sincerely thank the two reviewers for the time spent reviewing our manuscript. We understand that this is an additional task and burden on their busy schedule and appreciate the commitment to the process of peer-review and the contribution to our scientific community.