Referee #1:

Comment: The manuscript has improved substantially. In particular, I appreciate the clarification of terminology (active layer vs. talik), the improved figures and legends, the additional discussion on methane oxidation and seasonality, and the inclusion of recent literature addressing sulfate - methanogenesis interactions in coastal permafrost environments as well as the comparison between Arctic coastal wetlands and tropical coastal wetlands. Overall, the manuscript is clearer and better contextualized. If my two method related comments are addressed, I have no further concerns in publishing the manuscript.

Response: Thank you for your time and your insightful comments. We are glad that the manuscript modification was able to address your comments and is now clearer.

Comment: The authors state that CH_4 accumulation was measured every two weeks for 16 weeks and that measurements were not performed during the first weeks of the incubation. This should be clarified more precisely by indicating the day on which the measurements began. For example, the text could specify that CH_4 accumulation was measured biweekly from day X until day 339.

Response:

We have modified the sentence accordingly to specify the frequency and interval (line 185): CH_4 accumulation was measured bimonthly from day 227 until day 339.

To remove redundancy given this change, we removed "every two weeks" from the text at line 193.

Comment: The manuscript should clarify how the linear accumulation used to calculate methane production rates was verified (e.g., regression statistics or time interval used). Additionally, because incubations lasted 339 days and substrate concentrations were not monitored, the potential influence of substrate depletion or microbial community shifts on the calculated rates should be discussed briefly.

Response:

Sentence at line 193: The resulting production rates were calculated from the linear accumulation measured during the incubation period, and values are expressed in nmol of CH_4 per cubic centimeters of wet material per day (nmol cm^{-3} d^{-1}).

Change: The resulting production rates were calculated from the CH_4 accumulation measured during the incubation period. The whole measurement interval was retained regardless of CH_4 concentration dynamics to avoid selective exclusion. Rates were derived from the slope of concentration change over time and are expressed in nmol cm^{-3} d^{-1} and the standard deviation from triplicate incubation was reported as the uncertainty estimate

To address the part of the comment on substrate depletion, we added this part in the incubation limitation section at line 446:
The long duration of our incubations (339 days), combined with the absence of substrate monitoring, may have influenced the calculated production rates. While environmental conditions were fixed after the transition from in situ to incubation settings, internal changes such as substrate depletion, accumulation of metabolic byproducts, and shifts in microbial community composition likely occurred over time. These processes may have altered methanogenic activity relative to initial conditions, particularly as more labile organic matter

may have become depleted. Such temporal dynamics are inherent to long-term incubation experiments and may lead to deviations from in situ rates.

Referee #2:

I thank the authors for their careful revisions and for providing a detailed and thoughtful response to the reviewers' comments. The manuscript has improved in several important respects. I also appreciate that the authors now explicitly acknowledge several methodological limitations, which increases the transparency of the study.

That said, there remain a few substantive issues that merit consideration. The authors appropriately recognize that their experimental design did not include parallel incubations without brackish water addition or sulfate concentration gradients, and they now describe their interpretation as exploratory rather than definitive. This clarification is important. However, the framing of the results in parts of the manuscript still occasionally implies a causal conclusion regarding the absence of sulfate inhibition. Based on the current design, the data clearly demonstrate that methane production occurs under brackish water addition, but they do not directly test whether sulfate suppresses methanogenesis relative to baseline conditions. Without incubations lacking brackish water and/or a sulfate gradient experiment, the study cannot quantify inhibition effects or rule out relative suppression. I therefore recommend ensuring that the wording throughout the manuscript consistently reflects the exploratory nature of the experiment and avoids causal interpretations that exceed what the design can robustly support.

In addition, while the authors have added a statement noting that isotopic results should be interpreted cautiously due to limited biological replication, several aspects of the study still rely on single-profile representations of geomorphological units and limited replication for certain measurements (e.g., sulfate concentrations and isotopic analyses). This does not invalidate the data, but it does constrain the strength of landscape-scale extrapolations. The manuscript would benefit from maintaining a proportional framing of its broader implications, particularly when extending incubation results to landscape-level methane production estimates.

The clarification regarding the 339-day incubation period and the calculation of methane production rates from linear accumulation is helpful. Nevertheless, long-term incubations can alter microbial community structure and substrate availability relative to in situ conditions. While such limitations are common to incubation-based studies, briefly reiterating this point in the discussion would further strengthen the methodological balance of the manuscript.

We thank R2 for the time put in reviewing our MS and the constructive comments on our work. Our response is broken down into three parts:

Comment: However, the framing of the results in parts of the manuscript still occasionally implies a causal conclusion regarding the absence of sulfate inhibition. Based on the current design, the data clearly demonstrate that methane production occurs under brackish water addition, but they do not directly test whether sulfate suppresses methanogenesis relative to baseline conditions. Without incubations lacking brackish water and/or a sulfate gradient experiment, the study cannot quantify inhibition effects or rule out relative suppression. I therefore recommend ensuring that the wording throughout the manuscript consistently reflects the exploratory nature of the experiment and avoids causal interpretations that exceed what the design can robustly support.

Response: We have fixed the wording throughout the MS to reflect the exploratory nature of our experiment in various sections of the discussion (lines 373, 375, 380, 390, 456, 459, 490, 516 and conclusion section).

Line 373: removed "mechanistic" to disconnect regulation of OM degradation from brackish water addition
Line 375: language disconnected from the direct inhibition by the presence of sulfate
Line 380: Directly indicated the reservation that we did not have gradients of sulfate concentrations to inform our incubations
Line 390: stated explicitly production in the presence of sulfate in our incubations rather than generally
Line 456: Limited the scope to specifically our incubations rather than generally
Line 459: removed the direct relationship suggestion, putting emphasis on exploratory nature of the report
Line 490: Added a significant reservation to our interpretation and future work
Lines 576 and rest of conclusion: Added significant reservations of our finding to the conclusion.

Comment: In addition, while the authors have added a statement noting that isotopic results should be interpreted cautiously due to limited biological replication, several aspects of the study still rely on single-profile representations of geomorphological units and limited replication for certain measurements (e.g., sulfate concentrations and isotopic analyses). This does not invalidate the data, but it does constrain the strength of landscape-scale extrapolations. The manuscript would benefit from maintaining a proportional framing of its broader implications, particularly when extending incubation results to landscape-level methane production estimates.

Response:
We have fixed the wording through our interpretations of isotopic results (line 520, 593) and conclusion paragraph.
Line 520: Inserted additional text to give limitations of our measurements explicitly
Line 593: Inserted additional text to give limitations of our extrapolation based on our limited set of samples. We specified that this extrapolation is rather a fisrt-order estimate to give the reader an idea of the potential implications of our findings.
Conclusion: Added text to reiterate that our findings are based on a limited number of profiles and samples and to state that our findings provide insights into Arctic methane cycling and caution is needed when extrapolating to broader landscapes.

Comment: The clarification regarding the 339-day incubation period and the calculation of methane production rates from linear accumulation is helpful. Nevertheless, long-term incubations can alter microbial community structure and substrate availability relative to in situ conditions. While such limitations are common to incubation-based studies, briefly reiterating this point in the discussion would further strengthen the methodological balance of the manuscript.

Response:
Please see our answer and edits suggested for R1 comment. We believe that these modification address fully also the concern of R2.