Response Letter of Reviewer 1 - Second Round Review

I thank the authors for their careful revision of the manuscript and for the detailed responses to my comments. 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. I have only a few minor comments:

1. Absence of control incubations

Thank you for the detailed explanation regarding the lack of control incubations without brackish water addition. I appreciate the clarification and agree with several of the points raised.

First, I agree that for anaerobic incubations the sediments or soils need to be water-saturated in order to create suitable conditions for microbial activity. In many incubation studies this indeed requires the addition of water, particularly when working with relatively dry material. I also agree that distilled water is not naturally present in environmental systems and therefore does not perfectly represent field conditions.

However, the purpose of experimental controls is not necessarily to reproduce natural conditions perfectly, but rather to isolate the effect of a specific experimental variable. In this case, the key variable of interest is the addition of sulfate (and other solutes) with brackish water. A control treatment without these added constituents would therefore help constrain how the amended conditions influence methane production relative to a baseline incubation.

Instead of distilled water, alternative options could include sterile-filtered tap water or freshwater from a nearby water body, which may better approximate natural freshwater conditions while still allowing the experimental variable (brackish water addition) to be isolated. Even if such controls are not a perfect representation of environmental conditions, they can still provide valuable context for interpreting the experimental results.

Therefore, my intention in suggesting control incubations was not to imply that they would completely resolve all limitations of the experimental design, but rather that they would help to better constrain the role of brackish water addition relative to baseline methane production potential. Without such a comparison, it becomes more difficult to determine whether the observed methane production rates are primarily driven by substrate availability, microbial community responses to water addition, or the geochemical influence of the added brackish water.

I appreciate that these limitation is now more clearly acknowledged in the revised manuscript. My suggestion is therefore mainly to ensure that the interpretation of the results remains appropriately cautious when discussing the role of brackish water and sulfate in regulating methanogenesis.

2. Incubation methodology clarification

You 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. Please clarify this 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.

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, please briefly discuss the potential influence of substrate depletion or microbial community shifts on the calculated rates.

Aside from the minor points listed above, I have no further major concerns.