

Biogeosciences Discuss., referee comment RC2  
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## Comment on bg-2022-122

Anonymous Referee #2

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Referee comment on "Relationships between greenhouse gas production and landscape position during short-term permafrost thaw under anaerobic conditions in the Lena Delta" by Mélissa Laurent et al., Biogeosciences Discuss.,  
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### Summary:

This study uses anaerobic incubations to examine CH<sub>4</sub> and CO<sub>2</sub> production in soils from different deltaic landscape positions, ranging from upland to floodplain; and explores how these gas production rates relate to landscape position, soil characteristics (e.g. water, C, and N, content), and methanogen and methanotroph abundances (via gene copy numbers). Landscape position was found to have the strongest influence on CH<sub>4</sub> production, with the floodplain active layer producing the most CH<sub>4</sub>, likely due to having the best-established methanogen community. In contrast, weaker trends were observed for CO<sub>2</sub> production, which was related to a combination of soil characteristics (specifically C and N content) and landscape position (with floodplain soils producing the most CO<sub>2</sub>).

### Overall comments:

This paper aims to address some major knowledge gaps on a consequential subject--namely, the controls on potential carbon feedbacks from warming in permafrost regions. These controls are poorly understood due to the many interacting factors (e.g. temperatures, redox conditions, organic matter quality, composition of legacy microbial communities, etc.) that affect CH<sub>4</sub> and CO<sub>2</sub> release, and this paper does contribute somewhat to the knowledge base. However, I found this paper to be lacking in terms of the strength of the findings.

First of all, only three cores were analyzed, despite the heterogeneity of the landscape. This is partially compensated by comparisons with other studies, including an extensive discussion of the results in comparison to other cores from the same region analyzed by Herbst (2022). However, the bibliographic entry for Herbst (2022) did not include a link to that manuscript, and I was unable to find it through a web search. Is that manuscript

planned to be published in the near future?

Second, the conclusions about microbial abundances do not seem supported by the sparse amount of data shown in Fig. 4. This issue might be partially addressed by better delineating zero abundances vs. truly missing values in the figure, but that depends on how much of the data is actually missing. (see specific comments below)

Finally, the Discussion and Conclusions include numerous statements about how the results can improve predictions of greenhouse gas release under permafrost thaw, but the most significant result (higher methanogenesis in the floodplain active layer) doesn't seem to directly address the effects of permafrost thaw, as the P17-A sample is from the active layer of a floodplain--unless that floodplain location is part of a thermokarst feature, or that sample was formerly part of the permafrost before active layer deepening; but this is unclear from the site description.

### **Specific comments:**

li 83-85: Remove extraneous reference text outside the parentheses (3 occurrences).

li 132-133: After looking at Fuchs et al. (2018), I could not find any information about the "relationship between absolute water content and bulk density."

li 142-143: How much sterilized tap water was added to the low-moisture samples?

li 164-165: Does the "cumulative emissions" used for calculating the Glucose Factor also include the time before the day 60 glucose additions? If so, this factor might be overly sensitive to random variations in production rates before the additions. (Also, the wording of this sentence is unclear. Suggested rephrasing: "The impact of glucose on CH<sub>4</sub> and CO<sub>2</sub> production was quantified as a glucose factor, calculated using the cumulative C emissions at 67 days:")

li 167-168: Related to the above, the phrase "total CH<sub>4</sub> production rate at i days" implies an instantaneous rate measured at several (i) timepoints, as opposed to cumulative only at 67 days (from line 164). Which method was used? (If only cumulative, then the phrase "at i days" seems extraneous.)

li 173: I assume "after glucose addition" means at Day 67? Please clarify.

li 202: Typo ("Kuskal-Wallis" should be "Kruskal-Wallis").

li 216, "the lowest [C:N] were in P17": Did you mean P16 (Table 1)?

Table 2: Typo in second-to-last row of first column (first occurrence of "P17-F" seems like it should be "P17-A").

li 247: It would be clearer to cite Table 3 (from which the  $42.53 \pm 15.79$  value is directly derived) in addition to Figure 3.

li 251-252, "P15 and P16 behaved similarly, with higher CH<sub>4</sub> production for the active layer at 4 °C than at 20 °C": This doesn't appear to be true for P16, based on its active layer Q10 being >1 (see Table 2).

li 253, "...and no difference for the permafrost layer": This also seems surprising, given that for P16 (Fig. 2b), the blue line (permafrost at 4 degC) is noticeably higher than the red (permafrost at 20 degC).

Figure 2: Several comments:

- Dashes are missing from the lines in Fig. 2b.
- In Fig. 2c, due to the very high Active Layer 20 degC values, it's impossible to see what's happening with the other samples. Would it be possible to create another version of this panel (perhaps for the Supplement) with the very high CH<sub>4</sub> values removed, so that the differences in the other lines can be seen?
- Some of the plots, particularly Fig. 2f, show negative CO<sub>2</sub> production rates. How would you explain these?

Figure 4, li 320-321, "Absence of values for some samples is due to either low DNA concentration or failure in qPCR run.": Can you indicate on the figure (maybe using a symbol) which empty values were due to which cause (low concentration vs. failed qPCR run)? This delineation of zero vs. missing values would help a lot with the interpretation of this figure, as a zero (or below detection limit) concentration still represents the information that concentrations were low, as opposed to not measured at all.

li 330: Wrong table references for CO<sub>2</sub> production (and move "Table 1" reference to line 329 or 331 about C and N contents)?

li 390-391, "methanogen concentration before incubation showed the highest numbers in

the floodplain (Figure 4c)": I can't tell whether this statement is supported by Figure 4, as the zero values aren't distinguished from an absence of measurement (see Figure 4 comment above). If all the empty values are actually missing (i.e. due to failed qPCR), then no direct comparisons of the pre-incubation samples would be possible between P17 and the other sites.

li 396, "little change in methanogen quantity after 60 days of incubation": This doesn't appear true for the P16 permafrost layer incubated at 20 degC, which had much higher mcrA (Fig. 4b).

li 404, "after permafrost thaw": Are the portions of the floodplains sampled by Herbst (2022) part of thermokarst features?

li 451: Typo; "three time" should be "three times".

li 461-463: Invalid sentence structure; did you mean for the end to read "**CH4 production** will likely increase" ?

li 472, "methanotrophic": Did you mean "methanogenic" ? or both?

Supplementary Figure 1: Which incubation temperature is shown here (or is it an average of both)?