

Biogeosciences Discuss., author comment AC1  
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## Reply on RC1

Mélissa Laurent et al.

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Author 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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Laurent Biogeosciences Reviews & Response to reviewers:

*We thank the three reviewers for their helpful comments and suggestions, which help to improve our manuscript. In response to the thoughtful and constructive comments from the reviewers, we made major revisions to this manuscript throughout all sections and most figures. A summary of these changes is given here, as well as the detailed response to reviewers below. Attached is a PDF file containing the responses to all reviewers. The major changes include:*

- *Most of the reviewers pointed out that the results would be more relevant and interesting if the incubation time was longer in order to overcome the lag time before methane production that we observed. Unexpectedly, our results showed that after two months of incubation at 20degC, under anaerobic conditions, only one sample layer produced CH<sub>4</sub>. We originally focused on a short-term incubation because we believe that it is essential to quantify C production under realistic timescale of a growing season (~60 days) in Kurungnakh Island because the aim of this study was to quantify the C production during the growing season under wet conditions communities and identify factors (microbial abundance, substrate availability) that would limit CH<sub>4</sub> production in this case study site. However, since only the active layer of the floodplain started producing CH<sub>4</sub>, we decided to keep the incubation running to see whether the other cores would produce CH<sub>4</sub>.*
- *We've included this additional incubation data from days 68 to 363 of the extended experiment. We incorporated additional incubation data into the revised manuscript throughout and have produced a new figure with the cumulative production over a 363-day period. The revised manuscript now shows anaerobic CO<sub>2</sub> and CH<sub>4</sub> production over a 363-day period.*

*To summarize the results from this longer incubation period, the floodplain core produced CH<sub>4</sub> within the first 60 days due to the already established methanogen communities, as we showed in the initial manuscript. After 6 months of incubation, the permafrost layers from the Yedoma cores started producing CH<sub>4</sub>. This important result was not included in the earlier manuscript version with the shorter incubation time, as noted by all the Reviewers. Old Figure 4 shows that the permafrost layer in P15 and P16 has a lower methanogen concentration than P17-A, so we attribute the difference in lag time primarily due to the time required for the Yedoma samples to activate the methanogen communities*

and to produce CH<sub>4</sub>. We hypothesize that the lack of methanogens in the P16-A and P15-A could be due to the dry condition induced by the landscape position. This indicates that methanogenesis is unlikely established after permafrost thaw in these sediments unless colonized by methanogens and the lack of response of CH<sub>4</sub> to the glucose addition and continued anaerobic CO<sub>2</sub> production also reduces the likelihood that substrate availability limits CH<sub>4</sub> production despite the lower C abundance compared with the floodplain soil (Table 1).

Additionally, the results section has been clarified to distinguish missing versus zero data within the microbial dataset.

- The introduction has been revised to address the concerns of the reviewers, to be more precise and specific about permafrost carbon, the permafrost carbon feedback, and earlier incubation studies. We both narrow the focus to findings from earlier incubation experiments and elaborate on the specifics of the findings regarding the landscape position. In detail, we include a definition, discuss what differs across landscape positions, and expand on the links between microbial abundance and CO<sub>2</sub> and CH<sub>4</sub> production.
- We substantially revised the methods section to include more details addressing the criticisms of the reviewers and clarified terminology throughout the manuscript (e.g. "production").
- We substantially revised the discussion in order to address the criticism from the reviewers about the overly broad implications of this study despite the limited number of permafrost cores. It is now substantially shorter. We remove Figure 5 (the conceptual diagram) in the revisions. We clarify throughout the manuscript that this is a case study based on permafrost cores from Kurungnakh Island, Siberia, Russia. We shortened and narrowed the discussion to a case study, which aims to understand and quantify the potential C production in this limited region by integrating information about the influence of landscape position, microbial data, and, soil parameters to understand the factors controlling C production in this site within the Yedoma dominated region. We would like to note, however, the importance of these particular permafrost cores collected at a remote field site in Arctic Siberia, and this data given that it is no longer possible to re-sample at these sites in the foreseeable future.

## **Reviewer 1:**

The manuscript of Laurent and co-workers present data from an anaerobic short term incubation study of six samples from three different permafrost affected soils in a transect from ice-complex deposits into a floodplain in the Lena Delta, Russia. The authors incubated the samples at 4°C and 20°C, and measured for 60 days CH<sub>4</sub> and CO<sub>2</sub> concentrations. At the end of this incubation, they added glucose and measured for another week. Furthermore, they measured the abundance of mcrA genes (methanogenes) and pmoA genes (aerobic methane oxidizers).

We urgently need to better understand the consequences of thawing permafrost in the northern hemisphere on the global carbon cycle. In this respect, the study is concerned with an unquestionable important topic.

However, the main result of the study is that except for one sample, no consistent methane production was observed and that methanogens were still in the lag-phase during the short-term incubation experiment. This means that the experiment was too short to gain information about methanogenesis in most of the samples. Consequently, there is only limited information in the presented Q<sub>10</sub> values for methanogenesis or the calculated CO<sub>2</sub>:CH<sub>4</sub> ratios. The remaining results are mainly a confirmation of established

knowledge. I suggest that the authors better elaborate, which new information or insights the reader gets from this study.

*As explained in the main response, we aimed to simulate wet summer conditions during the growing season. Our results showed that after two months of incubation, only the active layer of the floodplain produced CH<sub>4</sub>. The absence of CH<sub>4</sub> production for the other active layers at 20degC was unexpected. The lack of methanogens and CH<sub>4</sub> production after two months show that the methanogen communities were not established. Even though most of our samples did not produce CH<sub>4</sub> within the 2 months, we still believe it is important to capture the behaviour of these samples for C production during the growing season. We continued to measure this incubation experiment after the two months presented in this study, as mentioned in the overall summary above. These additional measurements from days 68 to 363 have been added to the study in response to this concern, raised both here and by the other reviewers.*

*We note that after 6 months of incubation, methane was produced in more samples. Methane was produced consistently across the cores in the permafrost layers at 20degC after 6 months, in both the floodplain (P17) and the Yedoma cores (P15, P16). The active layer of the floodplain core produced methane at both 20degC and 4degC throughout the incubation. However, the active layer of the well-drained Yedoma core (P15) neither produced methane after one year, or at 20degC or with a substrate addition of glucose. The methane production rates of the samples at 4degC were overall very low. The samples that started producing CH<sub>4</sub> after one year of the incubation were those for which methanogenic communities were measured at the beginning of incubation. Therefore, although the methanogenic communities in the Yedoma permafrost layers needed more time to become active, these samples have a high potential for CH<sub>4</sub> production after a long thaw period.*

*This study is a case study in Kurungnakh Island. The geologic history of this site is well known, however few studies have worked on the potential CH<sub>4</sub> and CO<sub>2</sub> production after permafrost thaw at two temperatures. Here, we contribute to quantify and understand the potential C loss after permafrost thaw along a slope profile from the active and permafrost layers, and compare them with a floodplain in Kurungnakh Island. Our findings show that the landscape position plays a key role in the establishment of methanogen communities during the growing season. It is likely that under field condition, only the floodplain produce CH<sub>4</sub> during the growing season, this season might not be long enough for the upland and slope to establish methanogen communities. However, with longer thaw time they produce CH<sub>4</sub> in the permafrost layer. To our knowledge, these landscape position patterns have not been clearly shown for permafrost sites in Siberia.*

Furthermore, the description of methods is in part insufficient to evaluate their suitability

*We have substantially revised the methods based these comments to add further details.*

and the references repeatedly do not support the statement in the text (see detailed comments).

*We have substantially revised the methods and the references.*

The discussion should substantially be shortened. In its current form its very lengthy, extensively repeats results and itself.

*The revised discussion is both shorter and narrower and clarifies the importance of this study as a case study for the Lena Delta region.*

The microbial data on methanogenesis are interesting but the importance of the microbial data about aerobic CH<sub>4</sub> oxidation remains obscure, since the experiments were done under anoxic conditions.

*Thank you for this comment. We agree that the quantification of methanotrophs was not relevant for the study. We removed the data in the revised version.*

Finally, I suggest clearly differentiating between production and emission. The data presented here are data on CH<sub>4</sub> and CO<sub>2</sub> production. There are no data on in situ CH<sub>4</sub> and CO<sub>2</sub> emissions. Particularly in the discussion, 'emission' is used for both the production in highly artificial laboratory incubations and in situ CH<sub>4</sub> and CO<sub>2</sub> fluxes. But incubations give only very limited information, if any, about in situ fluxes.

*Thanks for this comment. We have changed "emission" by "production" throughout the manuscript unless otherwise necessary.*

### **Specific comments:**

**L33:** 822 Pg is the C in permafrost, not in permafrost soils. Please clearly differentiate between permafrost (permanently frozen) and permafrost soils (soils containing permafrost).

*We revised this sentence and the introduction to specify differences in C stocks between permanently frozen and permafrost soils.*

**L34:** Obu et al. 2019 reports that permafrost affected soils cover 14.6% of the northern hemisphere. 21.8% of the northern hemisphere is the permafrost region, i.e. the region where permafrost might be found (but not necessarily underlying 100% of the soils). Please clarify.

*Revised this sentence during revisions and now focus on the extent of the permafrost C stock.*

**L38:** Here is a misunderstanding of permafrost. The upper part of permafrost does not thaw in summer, in this case it would not be permafrost (see the definition given in line 34-35).

*Revised as suggested: "the upper part of the permafrost affected soils thaws (active layer)".*

**L44:** This sentence is unclear. Who is "providing decomposable C"?

*We have significantly revised the introduction. This paragraph is deleted in the revised introduction.*

**L50:** The review of Schuur et al., 2015 does not present data on aerobic CH<sub>4</sub> production. Better cite original data.

*Removed during revisions (see major comment #3 above).*

**L79:** The studies cited here report GHG production rates from incubation studies, which do not give much information about 'C emissions released from different landscape forms'. Please clarify.

*We have significantly revised the introduction. This paragraph is deleted in the revised introduction (see major comment #3 above).*

**L81:** The meaning of this sentence is unclear. Do you mean that microbes with a certain function may be active even if the redox conditions are not suitable for the respective process? Please clarify.

*We have significantly revised the introduction. This paragraph is deleted in the revised introduction.*

**L85:** This is a bit strange question in the context of this study. There are numerous studies on the importance of microbes and redox conditions on e.g. methane production and oxidation, but this study is not addressing redox conditions. Furthermore, in situ C emissions are strongly affected by vegetation, which is not mentioned at all. Please clarify.

*We have significantly revised the introduction. This paragraph is deleted in the revised introduction.*

**L90:** To prevent confusion, I recommend to replace 'emission' by 'production'. In that case, the reader does not expect data on in situ GHG fluxes.

*We have changed emissions to production throughout the manuscript.*

**L133:** Fuchs et al., 2018 determined the bulk density 'by dividing the dry weight of a sample by its original volume'. How may the bulk density be determined by the water content of the soil without knowing the volume of the sample? Particularly when the samples are not water saturated. Please explain.

*Thank you for pointing this out. First, we apologize for the wrong reference, which may have caused this confusion. We corrected the reference by "Fuchs 2019". In this thesis, Fuchs plotted the bulk density in relation to the absolute water content of one thousand samples and was able to calculate a transfer function from absolute water content to dry bulk density. To calculate the bulk density, he divided the dry weight of a sample by its initial volume. With the data from the bulk density and the water content, they established a transfer function to determine the bulk density of a sample when the volume is unknown. As we explained in the manuscript, we did not calculate the bulk density, but estimated it according to this transfer function. This transfer function is in the supplementary material XY. In addition, the samples used for our study come from the same area as most of the samples used to establish this correlation. We changed the manuscript and explained the bulk density estimation in more details.*

**L162:** Please explain in more detail how the CO<sub>2</sub> and CH<sub>4</sub> production rates were determined. Did you consider DIC in the soil water? At pH > 7 this might be more than in the headspace. How did you calculate rates from single concentration measurements? I could not find a method in the cited reference (Robertson et al., 1999) that enables the determination of production rates from single gas concentration measurements.

*For samples with pH > 7, water contents are very low (Table 1), so we assumed that a negligible amount of CO<sub>2</sub> was stored as DIC in the sample water. However, we agreed that this might underestimate C mineralization and now mention it in the text.*

*We did not calculate the production from single gas measurements, but used the change in concentration of CO<sub>2</sub> and CH<sub>4</sub> over the incubation time. We first converted the concentration from ppm to µg/L using the ideal gas law and then used a linear regression between each measurement point to calculate the change in concentration over time, then calculated the mineralisation rate with the headspace and the volume of the dry content. (after Robertson et al., 1999, Exchangeable Ions, pH and Cation Exchange Capacity, p. 266-267)*

**L164:** As the equation is written Gf gives the factor by which glucose addition increases gas production, the unit is not %.

*The equation has been changed to have %.*

**L205:** P16-F has a EC of 479  $\mu\text{S cm}^{-1}$ .

*Revised as suggested.*

**L215:** <0.3% L219: ... P17-A .... P17-F

*Revised as suggested.*

**L301:** Could you give the detection limit of your mcrA quantification? Can you measure 76 gene copies per gram?

*The detection limit of the mcrA quantification is  $4,3 \times 10^3$  copies per gram. We edited Figure 4 by adding the expression "non detected" and "below detection limit".*

**L329:** Is there a concentration of carbon below which it may not be decomposed? Please explain.

*C mineralization depends on the quantity and the availability of the OC. This means that samples with very low C content can have high turnover if the C is easily decomposable. Therefore, it is complicated to say that there is a threshold below which C mineralization does not occur. Here, we clarified that our C and N contents from our samples are in the same range as those in the study of Strauss et al (2013), and thus, the bioavailability of OC should not be a limited factor for C mineralization.*

**L329ff:** This is correct as long as sufficient sulphate or nitrate is available, which is generally not the case in terrestrial soils. The reason for low methanogen abundance is probably rather the high redox potential in these soils.

*We deleted this part in the revised version.*

**L385:** Please explain what you mean by 'favourable to C mineralization'.

*We compared our soil characteristic results with other studies to elucidate whether TOC and N content were limited factors for C mineralization. Other studies showed that with similar, or even lower, TOC and N content C mineralization was possible, and therefore we concluded that C mineralization was not limited by the quality and/or quantity of the OM. Hence, by "favourable to C mineralisation" we mean "not limiting for C mineralisation".*

*We added more recent studies to the references to support this statement.*

**L417:** Which discrepancies do you mean? 'Cumulative emissions' (production) are the consequence of the observed production rates. Please explain.

*We revised this sentence to explain that there is high variability in CH<sub>4</sub> production rates within floodplain environments. We compare our results of CH<sub>4</sub> production (cumulative production and production rates) with the ones from Herbst (2022). However, the sentence construction is not clear and we have changed it to make it easier to understand.*

**L419:** What do you mean by 'methane conditions'. Please explain.

*We did not mean "methane conditions", but "methane production". Thanks for noting this mistake.*

**L448f:** In a completely anaerobic incubation experiment, landscape position might not be relevant since CO<sub>2</sub> production depends on C and N availability. However, at in situ conditions the redox potential differs and hence likely also CO<sub>2</sub> production. Please clarify.

*Thank you for this remark. Here, we were referring in anaerobic incubation experiment. We clarified this sentence.*

**Fig 5:** This figure gives no new information or concept. It is quite similar to several figures that have been published previously, even from the same region. Furthermore, the current manuscript gives no information about in situ fluxes. I suggest removing it.

*We agreed that this figure was not relevant. We removed it.*

Please also note the supplement to this comment:

<https://bg.copernicus.org/preprints/bg-2022-122/bg-2022-122-AC1-supplement.pdf>