Response to reviews Yarden et al.,

We would like to thank the editor and the reviewers for the constructive comments.

Here is the response letter we sent in July. The only change was that we combined in the end all porewater analyses (from the main text and the supplementary) to figure 2.

R1

This study presents a comprehensive dataset to characterize the rates and control of methane production across thermokarst lakes of different ages in interior Alaska. The authors conducted geochemical analyses and incubation experiments with sediment cores collected from a young (BTL) and an older (GSL) thermokarst lake. They observed elevated methane production rates at BTL, which was correlated with higher carbon lability for thermal induced reactions measured by Rock Eval analyses. They discussed how methane production varies with lake evolution and sediment depth, and also the influence of permafrost thawing on microbial activity. By comparing the depth-integrated methane production rates, they propose mechanism of how lake age and thawed talik thickness affect methane production rates and fluxes. The experiments were well-designed and the methods were generally sound. However, I have a few comments that need to be addressed before acceptance of the manuscript.

We thank the reviewer for the constructive comments. We addressed all the concerned as outlined below.

Major Comments:

(1) The stable isotopes of dissolved inorganic carbon in BTL were much more enriched in 13C than GSL, and the authors interpreted this as a result of methanogenesis. However, both methane concentrations and production rates were quite similar at two sites. So I wonder if methanogenesis could lead to such a large difference in 13C-DIC between two sites. Or if this could be related to the source of DIC. I also notice that both data of 13C-DIC and 13C-CO2 were present in Table S1, but I am not sure how were 13C-CO2 measured.

Indeed, one of the interesting features of using the 13C-DIC in thermokarst lakes is that it can really help us point out the differences between those two lakes. We could argue for a different source for the DIC if we were in a different setting, but here the soil does not contain significant carbonates that could "seep" into the DIC pool. Therefore, an in-situ difference seems more logical. Methane production rates at BTL are indeed at the same order of GSL, but they are much greater in large part of the sediments. These increased rates are imprinted in the $\delta^{13}C_{DIC}$, as the isotopic composition is much more sensitive than the concentrations difference and the uncertainty of their measurements. The isotopic composition of GSL suggests insignificant imprint of methanogenesis. This leads its $\delta^{13}C_{DIC}$ values to be influenced from oxidation of organic carbon, which might include methane oxidation, as indicated by the negative values. These points were emphasized and discussed in detail in the revised Discussion chapter.

Regarding the carbon isotopic measurements: $\delta^{13}C_{CH4}$ and $\delta^{13}C_{CO2}$ were both measured from the headspace of the sediment subsamples using the PreCon and Gas Bench II interface of DeltaV

GS-IRMS of Thermo. The $\delta^{13}C_{DIC}$ was measured from the porewater sample after adding acid and producing headspace using the Gas Bench. This was clarified in the revised version.

(2) Source of methane. The observed δ13CCH4 values from the incubation experiment were mostly >-60 ‰ particularly in BTL, with many of them >-50 ‰. This seems contrary to the biological production of methane with such positive δ13CCH4 values. Any explanation for this? Do you have a parallel killed control sample for incubations and how do they like?

Methane with stable isotopic compositions that are more positive than -60‰ and even more positive than -50‰ is typical of produced biological methane in high energy environments. i.e. environments where organic carbon substrates are still quite labile as terrestrial environments. While this is typical of thermokarst lakes where labile Org C is available, it doesn't often happen in marine sediments where the bulk of the literature on methane has been established. Microbial methane in marine sediments is usually more negative because sulfate reduction typically consumes a large portion of the labile organic carbon before methanogenesis can get to it because of the large pool of sulfate from seawater in the pore spaces. So, with labile organic carbon available in thermokarst lakes, more energetically favorable pathways are used i.e. acetoclastic methanogenesis (as compared to hydrogenotrophic methanogenesis that dominates marine sediments). This produces methane less isotopically depleted relative to the source material, but still in the typical range for microbial methanogenesis (Whiticar et al., 1986; Liu et al., 2025). Also, even slight methane oxidation, as suggested above by the 13C-DIC, can contribute to these more positive values, as shown also in other lake sediments (e.g. Sivan et al., 2011). Finally, killed control samples from BTL and GSL and other lakes do not produce any methane at all. We have shown this in previous papers e.g. Lotem et al., 2023 L&O; Pellerin et al., 2022 GCB). This explanation was also added to the revised Discussion.

(3) Following the above comment, it would be nice if the authors could include more discussion about the importance of different methane production pathways.

We agree that the production pathways are an important part of the story. However, this was discussed in our previous studies on these lakes (e.g. Lotem et al., 2023 L&O; Pellerin et al., 2022 GCB). We have therefore clarified shortly the above points in the revised version and also referred the readers to the discussions on pathways elaborated in previous papers.

(4) Similar observations about the control of organic matter on methane production have been reported previously, which can be cited in this work.

Zhuang et al. 2018. Relative importance of methylotrophic methanogenesis in sediments of the Western Mediterranean Sea. Geochim. Cosmochim. Acta 224: 171-186.

Maltby et al. 2016. Microbial methanogenesis in the sulfate-reducing zone of surface sediments traversing the Peruvian margin. Biogeosciences 13: 283-299.

Berberich et al. 2020. Spatial variability of sediment methane production and methanogen communities within a eutrophic reservoir: Importance of organic matter source and quantity. Limnol. Oceanogr. 65: 1336-1358.

We thank the reviewer for suggesting adding these references, and we have updated the text to integrate these works.

(5) It is kind of confusing for the use of methane fluxes in Fig. 7. From my understanding, the production rates did not necessarily mean the emission flux from sediments to the water columns. I did not say the comparison was invalid, but please better justify it.

We totally agree and we updated the figure caption.

(6) Some figures such as Fig. S1 to Fig. S4 that contain important information should move to the main text rather than buried in the supplementary.

We agree that the figures contain important information, and should, at least partly, move to the main text, as also suggested by the second reviewer. Accordingly, we moved Fig. S1 and Fig. S2 to the main text in the revised version.

Minor Comments:

Line 37: Remove the comma.

Done

Lines 54, 317, 412: Revise and format the brackets.

Done

Line 154: What was the purpose of the additional 3 mL sample? Please clarify.

Done

Line 225: Should be "200 ℃".

Done

Figure 2: Please indicate what A, B, C, and D represent in the legend.

Done

Lines 345 and 351: The term in situ should be used consistently and italicized throughout the text.

The term *in situ* was checked throughout manuscript and updated accordingly.

Figure 7: The figure is blurred, and the resolution needs to be improved.

Done

Lines 403–407: This sentence is vague and confusing. When you talk about significant difference, you need statistical analysis to support it.

This sentence refers to methane production rates. We have rephrased this sentence to try to capture the fact that methane production rates that we measured in this study (based on concentration increase) were robust but not enough to differentiate between the rates at GSL and BTL in the deep talik. However, we believe that the two lakes biogeochemical cycling function very differently based on the other lines of evidence mentioned above. This was clarified in the revised text.

Figure 8: Please adjust the figure layout, as the overlapping text affects readability. Please provide statistics and coefficients in the figure or text.

We adjusted the figure layout. We also added the statistics and coefficients to the figure.

R2

This study focusses on the production of methane in a young and an old thermokarst lakes in Alaska and provides a comprehensive data set based on carefully collected, sub-sampled and geochemical analyzed samples. Here they could show that the methane related biogeochemistry in thermokarst lakes change over time as labile carbon stocks are metabolized first but methane production is sustained by the deepening of the talik below the lakes. The study was well designed and sampling as well as analytical methods used were carefully planned and executed. However, the following comments here and in the pdf need to be addressed before acceptance.

We thank the reviewer for the thorough, careful and helpful review, which has improved our paper significantly.

Please add more details to the methods used or provide the respective references where the methods are described in more detail. As it is the analysis could not be reproduced. For example, line 191 and following: which GC was used, at which oven temperature? What length was the column? What was the standards methane concentration for used in the calibration? Furthermore, please provide an overview of the taken cores and their length, either as table or figures. For more detailed comments please see the attached pdf.

We have expanded the Methods section to include the GC model (Thermo GC-FID), oven temperature, column type (ShinCarbon ST), and calibration standards. We also added the core sections and their lengths (Table S1) and clarified the sampling strategy in the text.

I agree with reviewer 1 that some of the supplementary figures should be moved to the main text, at least figure S1 and S2. Furthermore, I agree that the interpretation of the δ13C-CH4 is incomplete. In addition to the remarks of reviewer 1, the less depleted signal in the incubations could also be due to partial oxidation of methane which can happen in parallel to methanogenesis. This was shown in hypersaline coastal wetlands for the coupling of methylotrophic methanogenesis and AOM (see Krause and Treude 2021, https://www.sciencedirect.com/science/article/pii/S0016703721001873). Additional

information on methane production and oxidation might be gained from graphics like δ13C-CO2 vs δ13C-CH4 after Whiticar 1999, as you already have analyzed the δ13C-CO2.

We agree with all comments. We moved Fig. S1 and Fig. S2 to the main text in the revised version. These figures are now referenced in the Results and Discussion sections. Furthermore, following the reviewer comments we have revised the manuscript with more thorough interpretation of the $\delta^{13}C_{CH4}$, and the methanogenesis vs oxidation imprint and included also the input of $\delta^{13}C_{CO2}$ vs $\delta^{13}C_{CH4}$.

Regarding Figure 8, the highest production rate at BTL is always topped or at least equal to the highest rate at GSL. This somewhat contradicts your statement in line 492. This should at least be addressed. Furthermore, in the other figures you are distinguishing between center and edge. Please explain why are you showing means from both edge and center in this graph?

Thank you for pointing this out. We revised the figure caption and discussion to clarify that Figure 8 is for the upper meter of sediments (100 cm) and that below this depth, there is no measurable difference in production rates. The stark differences in rates are found in the upper meter, where both BTL cores (edge and center) have higher rates than both GSL cores. Changes were added to the text accordingly.

Line 32: More likely part of a conclusion rather than the abstract

We agree but kept it as a final statement.

Line 40: Are they really a "sink" or rather do they still store carbon? The reference does not state this directly (after a short look: the term "sink" is only mentioned in the references). Maybe you find a more suitable reference.

We revised the sentence to clarify that the soils store carbon and adjusted the reference accordingly.

Line 92: Please formulate more clearly that you are talking about fluxes into the water column here and throughout the manuscript. As you do not address dissolution, the water column methane filter and so on and did not measure flux to the atmosphere. Thus this formulation is kind of misleading.

We agree that this chosen term is confusing and have removed most wording related to fluxes throughout the manuscript, since we did not measure fluxes but accumulated rates (referring the integrated rates as the outward fluxes from the methanogenesis zone). We only state that it may be interesting to compare accumulated methane production rates to potential fluxes.

Line 126: Please add a space "609 m²"

Corrected.

Line 142: In which part of the lake were the samples drilled? Looking at the map it would seem like that you took samples from the expansion part. Please clarify this in the text or in the map.

We clarified the sampling locations in the text.

Line 145: Provide an overview of the taken cores and their length - either as table or figure - either here or in the supplements - at least referred to Table S1

We marked the core sections in table S1 and referred to it.

Line 146-150: This is misleading. As cores were transported as fast as possible but also sampled in the fields. I would assume that one core was transported and the other one was subsampled or that one core was silced and one half was subsampled and the other half was transported. Please clarify this.

We thank the reviewer and revised the text to clarify the sampling procedure and transport of cores. There was indeed only one core from each location.

Line 162: 2 mL

Corrected.

Line 191-194: Please refer to another paper with a more detailed description, or describe the method in more detail: which oven temperature, which gc, how long is the column and so on.

We expanded the method description and added references.

Line 195: Which concentrations were used for calibration?

We have included the calibration concentrations.

Line 199-201: Please provide more information on the system and the method.

We expanded the description of the analytical system and method and added a reference, as mentioned above.

Line 205-209: Please provide more details on the method used or refer to a publication with more details.

Done.

Line 231: Please provide more information.

More information was included.

Line 239-240: Please provide more information and describe in more detail.

We revised the text to include more detail.

Line 249: Move to main Text, tiles are not named A/B/C/D, please add.

We moved the figure to the main text and labeled the panels accordingly.

Line 281-285: As indicated by the phrase "In conclusion" this paragraph does not really fit here. It should be moved to the discussion or conclusion or rewritten.

We removed the text “in conclusion” but we kept the paragraph in the results section.

Line 282: methanogenesis

Corrected.

Line 288: refer to the tiles in brakets (A, B) (C,D)

Corrected. We added text to the figure caption.

Line 302: In Figure 2 you use BTL here you do not use the abbreviations. Please be consistent and if you use the full name, please provide the abbreviation in brackets as you use these in the figure.

We standardized the use of abbreviations throughout the manuscript. We also combined figures 3 and 4.

Line 333: here is a space missing between 2 and year

Corrected.

Line 345-347: Please move the Figures S1 and S2 into the main text. Futhermore, I think d13C from your incubations indicate parallel methane oxidation or cycling at a depth of about 160 or 170 cm in both BTL samples as the d13C reaches down to ~35 ‰ which is hard to explain without oxidation (see Whiticar 1999 for example). Additional 13C-CO2 and or dH2-CH4 are necessary to elucidate this clearly. This should be discussed in more detail for example with https://www.sciencedirect.com/science/article/pii/S0016703721001873.

We appreciate this suggestion. We moved the figures as suggested. As mentioned above, we have revised the discussion to include the possibility of partial methane oxidation during incubations, added $^{13}C\text{-}CO_2$ (and referred to $H_2\text{-}CH_4$ in Liu et al. 2025) and cited Krause and Treude (2021) and Whiticar (1999). This provides more insight on methane production and oxidation imprinted in the $\delta^{13}C_{CH_4}$ values.

Another possibility would be aerobic oxidation of methane under anerobic conditions which was shown for members of Methylomirabilaceae, in deeper and probably anoxic peat… from nitrite reduction to dinitrogen (Ettwig et al., 2010; Versantvoort et al., 2018). Also all the heavier d13C signals in the incubation could be explained by partial oxidation of methane, thus, a lighter deeper source is not the only explanation.

We appreciate this suggestion. We noticed indeed aerobic methanotrophy and internal oxygen production in Lake Kinneret sediments (Bar-Or et al., 2017; Elul et al., 2021). Here, it is more doubtable, as discussed in our previous publication (Lotem et al., 2023 L&O), and mentioned now in the revised version.

Line 389: Change to Freitas et al. (2025)

Thanks, done.

Line 451: If the lake expands to the east since 1949, how old is your sample site really?

The site is less than 70 years old.

Line 455-457: I somewhat have an issue with this statement in regard with the modelled depth integrated sum of production and thus flux. This has to be discussed as production does not necessarily is equal to flux.

We assume that according to second law of Fick, the integrated production rates represent the maximum/potential fluxes of methane out of the production zone. Upward diffusive fluxes can be of course much lower due to downward fluxes and oxidation. This has been clarified in the revised text.

Fig. 8 y axis: "Rate" is a bit simple, please clarify that this is the methane production rate on the y axis

We updated the axis label to specify 'Methane production rate'.

Fig. 8: Line numbers are in the figure, make sure this is just an artefact.

We removed the line numbers from the figure.

Line 492: However, the highest production rate at BTL is always topped or at least equal to the highest rate at GSL. This somewhat contradicts your statement. This should at least be addressed. In the other figures you are distinguishing between center and edge. Why are you showing means in this graph?

We revised this part and clarified the error bar in the figure.

Line 525-528: Oxidation shifts the d13C of methane towards 0, this would fit to the depth profile as the methane near the sediment/water surface is less depleted.

We agree and added, as mentioned above, a detailed discussion on methane production and oxidation, as interpreted from the isotopic profiles.

Line 530-532: The more depleted values in the sediment could also be achieved by different production ways in the sediment vs the incubations.

We included also this alternative explanation in the discussion, eventhough deeper source fit better our observations.

Line 530: This should be "more depleted" as < 0 is not enriched in the first place.

Changed.

Line 550: induced

Corrected.

Line 551: This sentences sounds odd maybe change to "*a* young methane source" or of "young methane source*s*"

We revised the sentence as suggested.

Line 568-570: This is not derived from your data, so please provide a reference, argue it in the discussion or omit from the conclusion.

We modified this sentence and added a reference.

Line 584: As the water column methane filter is not addressed, please clarify that you are talking about fluxes and emissions from the sediment into the water column.

As clarified above, we indeed talk about integrated rates and fluxes into the water colum. This was clarified better also in the revised version.

R3

I agree with the other reviewers that this is an important, well-conducted study that suits this journal. However, refinement of the figures and conclusion is necessary before publication.

We thank the reviewer for the positive feedback and agree that refining the figures and conclusion improves the clarity and impact of the manuscript.

MAJOR Comments:

147-150: After cutting, were the cores returned to the vertical position? Unclear if you sampled in the field or at the University? Was sampling, especially rhizone sampling, performed with the core vertical? These are critical details to understand how mixed your sampling horizons are, and how well sharp geochemical porewater changes can be measured as a result. Currently the text reads as if the cores were transported horizontally to the university and then rhizone sampled on their sides. Please clarify and consider commenting here or in the discussion about how porewater vertical mixing during collection and sampling can affect your results.

We have clarified in the Methods section that the cores were cut in the field, but due to the very cold weather, they were returned to the lab in Fairbanks vertically for the sampling. The rhizone sampling could be done only horizontally after cutting the corer lengthwise. The diffusion porewater profiles of concentrations and isotopes we obtained suggest no significant vertical mixing during transport. We clarified and discussed this in the revised version.

326-327: Include the uncertainty as a +- next to the absolute value. If I read the supplement correctly, the BTL center estimate would be 7.4 +- 1.8 mol/m2yr. You kept your uncertainties reasonable under challenging sampling conditions, it's good to highlight that in the text.

Thank you for this comment. We have included in the revised version the uncertainty estimates (e.g., ±1.8 mol/m²/yr for BTL center) alongside all reported methane production values in the Results section and relevant figures.

439-442: I don't follow your logic here. Can this study really tell about methane uptake? Are you assuming your incubation methane production is net between producers and AOM consumers? I think this also relates to another reviewer's comment about what the d13C values imply, you could move it down to this section. I do think you need to discuss methane consumption in the sediment and water column, but possibly as something you can't account for here, with the

references you have to support your claim that it's playing a minor role. This would be a great place for future work, especially how AOM rate evolves with age & OM lability.

We measured net production rates from the concentrations changes. We assume that AOM in our batch experiment is not significant based on our previous work on short cores (Lotem et al., 2023 L&O), and that the rates represent mainly methanogenesis. However, the in situ profiles (which show some role for methane oxidation) suggest that this should be further explored and indeed will be very interesting. We clarify it in the revised version.

403/407: directly contradictory statements. A statistically significant difference IS possible to differentiate. Please add numbers and uncertainties to explain what you mean. This paragraph could probably be shortened and combined with the text around it.

We clarified the paragraph and removed contradictory phrasing.

572-582: consider moving figure 9 and its description into a last discussion section, at the author and editor's discretion. I find your first presentation of data of this kind to be an important conclusion and the interpretation (fig 9) not quite as strong, but currently dominates the conclusion section.

We moved figure 9 from the conclusion to the discussion (above the origin part), and emphasized it's a conceptual model (see below).

Abstract/Conclusions: I am not fully convinced that your results support figure 9. From the current text and figures both the young and old lake have similar methane flux in your study and the previously published work shown in Fig 8 panel C has the younger lake with the greatest flux. As per the previous two comments, if you provide more support in the text and address the Pellerin data in the discussion you could still include fig as a hypothesis. If you really are telling a story where the oldest lake has a higher methane flux, I think there are a few places in the text where it sounds like the young/old is the same and that will need to be checked.

Agree. We revised the abstract and conclusion to clarify that figure 9 presents a conceptual model informed by our findings and previous work, rather than a definitive result. We also now explicitly address the apparent similarity in methane fluxes between the young and old lakes.

FIGURE Comments:

Much of what is currently in the supplement needs to be in the main text, in particular when showing a rate or isotopic composition the corresponding concentration also needs to be shown on the same figure. All figure captions need to refer to panel labels, please check throughout manuscript.

We moved key supplementary figures (Fig S1 and Fig. S2) to the main manuscript, as also suggested by the other reviewers, and ensured that isotopic figures include corresponding concentration data.

Consider the format of Figure 5 and S3: filled icons for edge, open for center (I would swap this personally, but not critical), circles for GSL, diamonds for BTL. Keep this convention for all the figures to make it easy for the reader to follow.

Done

Consider dropping redundant y-axis labels if you want to save space or put 4 panels together.

We removed redundant y-axis labels where appropriate to improve figure layout and allow for multi-panel comparisons.

Fig 1: Consider listing the lake ages in the figure caption. Please move the scalebar so it is not covering the other lake feature.

We added lake ages to the caption and repositioned the scale bar to avoid obscuring lake features.

Figure 2 can have one panel per lake per quantity (allows us to easily compare edge to center). Or you can keep all four cores separate but add a second x-axis with DIC concentration (would match more next to figure ¾). Need DIC concentration!

We revised Figure 2 layout to allow easier comparison between edge and center cores and added a second x-axis for DIC concentration.

Figure 3&4 need to be combined, add methane concentrations to the figure, and remove the descriptions from the figure captions into the text. Also add “methane” or “CH4” before “rate” in the axis labels. Possibly 8 panels total: top row with methane in situ concentration and production rate (2 x-axes), bottom row is Figure S2 with d13C of in situ and incubation methane.

We combined Figures 3 and 4, added methane concentrations rates, and revised axis labels and captions accordingly.

Figure 5: Consider putting the equations in the supplement and the legend into the figure caption so you can expand the inset. Currently a busy figure.

We moved equations to the supplement and expanded the figure caption to reduce visual clutter in Figure 5.

Figure 6: what’s the uncertainty of the data in Fig 6? If it’s under the symbols, please add this to the description, otherwise please show error bars or discuss in the text.

We added a note to the text clarifying that uncertainties in Figure 6 are smaller than the symbols and included error bars where appropriate.

Figure 7: Is the 15 m box showing integrated values from 0 to 15 m? Or just from 10 to 15 m? This is confusing in the figure. Perhaps you could write 0-5m, 0-10m, 0-15m, even though it seems a bit redundant. Also need hatch marks on horizontal lines at x-axis values (0,5,…,35): currently too hard to read values in the figure. Consider listing the lake ages as this is a nice summary figure and that could help readers follow your point.

We clarified in the caption that the m box in Figure 7 represents integration from surface to that depth.

Figure 8: Need Methane in axis labels. What are the numbers in the 400s under the y-axes? Please remove.

We updated axis labels in Figure 8 to specify 'Methane production rate' and removed extraneous numbers under the y-axis.

MINOR Text Comments:

64-68, do you not argue later in the manuscript that old lakes are still a large source of methane to the atmosphere if you integrate through the talik? If so, please qualify this statement in the introduction.

We revised the introduction to clarify that while older lakes may have lower surface production rates, their taliks can still contribute significantly to total methane flux.

95-99 Consider moving to methods or shortening.

We moved this background information to the Methods section for better organization.

115: have been described “by” and drop the brackets around Elder et al. (2021).

We corrected the phrasing and removed unnecessary brackets around the citation.

121-122: Confusing to read, consider rephrasing.

We rephrased the sentence for clarity.

125-143: nice descriptions

Thanks.

154-155 extract sediment for what?

We clarified the purpose of sediment extraction in the Methods section.

160-164 how long were the DIC samples stored before analysis? Were they killed?

We added information about DIC sample storage temperature, duration and preservation method.

185: is “total profile methane production rate” the biologic methane flux out of the sediment?

We clarified that 'total profile methane production rate' refers to the depth-integrated microbial methane production rates.

331: Need a reference for talik thaw ages.

We added a reference to support talik thaw age estimates.

343: “roughly calculated”

We revised the phrase 'roughly calculated' to estimated.

389: drop brackets from all but ref date.

We corrected the citation formatting to remove unnecessary brackets.

427-428: drop “which we believe to be most realistic”, and make “reflect” plural

Done

436: add “the” before “low but relatively constant”. Please give an example value here, eg for one core, how much methane is produced in the first meter vs. how much in all the meters below? Could be in percentage rather than absolute value but it will highlight your point, which I do think is an important one. You could reiterate here that a low rate over a larger distance can “outcompete” a high rate over a shorter distance.

We added percentage of methane production in the top meter versus deeper layers to illustrate the importance of depth-integrated rates.

447-453: rephrase this paragraph. It’s arguably your most important finding but difficult to read at the moment. Try writing it in reverse “This study found that as lakes mature, total thawed talik methane production rates will remain similar or even increase bc….”. Rewrite transition to the next section.

We restructured the paragraph to emphasize the key finding and improve readability.

486: Reverse this sentence order so you refer to the in-text figure first and the supplement second (also shallow, then deep).

We reordered the sentence to reference the in-text figure first and the supplement second.

492: drop “in the top meter of BTL” and replace “driven” with “due to”. Could put “available in the top meter of sediment” at the end of the sentence.

We revised the sentence as suggested.

511-516: do you mean that you have to look at age since thaw rather than age since deposition?

Yes, we clarified that we mean to the age since thaw, which is more relevant than age since deposition in this context.

533: “shows” not “showing”

We corrected the verb form to 'shows'.

537: drop “a shift towards”

We removed the phrase 'a shift towards' for conciseness.

541-544: swap these sentences, so your conclusions begin with “This study presents the first empirical data quantifying methane production and organic matter degradation of thermokarst lakes from young to mature” or something like this. Then, “this can be used to test models….

We reordered the sentences to begin with the main conclusion and follow with implications for modeling.

553-557: move to discussion

We moved this content to the Discussion section as suggested.

571-572: “while the highest methane production rates occur in the shallowest sediments of the young lake, the increase in talik depth with age also plays…..”

We revised the sentence.

574: do you mean flux when you say accumulated production rate? Consider having it in brackets? Or use consistent terminology.

Yes, we clarified the terminology and ensured a consistent use throughout the manuscript (as mentioned above to the other reviewers).

582-585: Consider rephrasing, I don’t think it really says what you want it to and people will remember the last sentence.

We appreciate this thoughtful observation and we have rephrased it.

CC1

We would like to note that while we are all researchers working in related fields, we do not specialise in the specific subfield addressed by this paper. This review was conducted as a group exercise to help us gain experience with the peer review process and to deepen our understanding of effective scientific critique. Our feedback is offered in this context, with the aim of being constructive and respectful of the authors’ work.

Summary

This manuscript presents a valuable dataset on methane production from deep sediment cores (up to 4 m) in two Yedoma thermokarst lakes of contrasting geomorphological ages. Sediment cores were collected from both locations, with samples taken from both the centre and edges to capture spatial variability. The work is novel in its vertical extent and use of Rock-Eval analysis to link organic matter lability to methane production. It improves understanding of how methane fluxes evolve as thermokarst lakes mature and provides useful empirical support for modelled predictions. Based on key findings in the study, the authors drew a conclusion that methane production is initially high in young lakes due to fresh carbon inputs and declines over time. However, some interpretive and presentational issues require clarification or improvement before the study can be considered for publication.

Major Comments

Overall, the manuscript is detailed and thoughtfully constructed. The authors have strong expertise in the subject matter, and their familiarity with the relevant methodologies and literature is evident throughout. The level of detail provided supports reproducibility and shows a commendable depth of understanding.

The study analyses only two sediment cores per lake (edge and centre), which may not adequately capture the spatial heterogeneity of methane production within each lake. Cores were collected during a single season, yet methanogenesis is known to vary seasonally. We recommend that authors acknowledge the limitations of their data samples.

Although the methane rate was measured up to 4 m depth, substantial production was observed only in the upper 100 cm. The authors should clarify the importance of below 1m. It would be beneficial to include an estimation of the methane production rate per year based on their experimental data and based on other available data sources.

Thank you for this important point. While the highest production rates were observed in the upper meter, our results show that deeper sediments contribute significantly to the depth-integrated methane production due to the increasing thickness of the talik in older lakes. We have clarified this in the Results and Discussion sections and now include a comparison of shallow vs. deep contributions to total production.

The two lakes differ geomorphologically but are both located in the Goldstream Valley and share similar climatic and geological settings. The authors should explicitly discuss the global relevance and limitations of extrapolating these findings to other permafrost lake systems with different environmental conditions.

We agree that this is an important consideration. We have added a paragraph to the Discussion section addressing the geographic and environmental specificity of our study. While both lakes are located in the Goldstream Valley and share similar climatic conditions, we caution against broad generalizations and highlight the need for comparative studies across diverse permafrost landscapes.

We feel the manuscript would benefit from a brief discussion of potential future work. Given that the study focuses on only two lakes, it would be helpful for the authors to outline how this research could be extended, whether through additional sites, longer time series to gain insight into seasonality, or broader environmental contexts. Such a discussion would help position the current work within a larger research trajectory and highlight its relevance to the field.

Thank you for this suggestion. We have added toward the end of the Discussion the potential future directions, including expanding the study to additional lakes, incorporating seasonal sampling, and integrating microbial community analyses to better understand methane cycling processes.

Minor Comments

Several figures would benefit from improved clarity and presentation.

We thank the commenters for this helpful suggestion. We have revised several figures to improve clarity and presentation, including adjusting axis labels, panel spacing, and resolution where needed.

Figures 3 & 4 present related data and appear somewhat redundant. Consider combining them to enable easier comparison between lakes and harmonise the axis scales for better interpretability. Figure 9 description is excessive and might be better explained along with the discussion in the paragraph post Figure. Figure 7 needs help regarding reading, perhaps some additional x-axis tick marks to more easily read the uncertainty/mean?

We agree that combining figures 3 and 4 improves interpretability. We have merged these figures and harmonized axis scales to facilitate comparison between lakes. We also moved the

description of Figure 9 into the main discussion text and improved the readability of Figure 7 by adding additional x-axis tick marks.

Figure 7 makes no mention of the additional error bars in the final section - is this the uncertainty of uncertainty? Is uncertainty required in Figure 7? Would it be easier to plot lines of each core sample x 3? Or as a table for exact values?

We clarified the purpose of the error bars in Figure 7 in the figure caption and text. We also considered alternative formats such as plotting individual core profiles or presenting a summary table but retained the current format for consistency with other figures.

Figures 3 and 4 present related data and appear redundant. We recommend combining them for easier comparison between the lakes and harmonising axis scales for interpretability.

As suggested, we have combined Figures 3 and 4 into a single figure with harmonized axis scales to improve interpretability and reduce redundancy.

Figure 8 is difficult to interpret due to the level of visual clustering. Including both GSL and BTL on the same plot may be contributing to this issue. If the goal is to compare the two lakes directly, it would help to make that comparative intention more explicit in the caption or main text. Alternatively, separating the data into two panels or figures might improve clarity and allow the reader to more easily interpret trends within each lake. Improving the readability of this figure would strengthen its impact and make the results more accessible.

We revised Figure 8 to improve readability by separating the data into two panels for GSL and BTL. We also clarified the comparative purpose of the figure in the caption and main text.

In-text citations should follow consistent formatting. For example, line 389 reads “(Freitas et al., 2015) also showed...”, which should be corrected to “Freitas et al. (2015) also showed...”.

We corrected the citation formatting throughout the manuscript to ensure consistency. The example on line 389 has been revised to 'Freitas et al. (2015) also showed...'.

The title suggests a broad investigation into the evolution of methane production from young to mature lakes. However, the study focuses on only two lakes. We suggest clarifying this in the title—perhaps by including the names or geographic location of the lakes—to better reflect the scope of the study and provide more context to the reader.

We revised the title to better reflect the scope of the study by including the names and geographic location of the lakes. This provides clearer context for readers and aligns with the manuscript's focus.