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 m^2"

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-CO_2 (and referred to H_2-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.