Dear Steven Bouillon,

Below you find a summary of all relevant changes made in the manuscript as well as a point-by-point response to the reviews. We copied the reviewers' comments in *italics* and respond to them in bold text.

<u>Response to reviewer comments by Pierre Taillardat</u>

We have made the following major changes in the manuscript to address the reviewer's comments:

- **We have revised abstract, introduction, discussion, and conclusions to shift the focus of the manuscript away from the effects of climate change on CH_4 emissions and towards a better understanding of the environmental and ecological controls on shoulder season CH_4 emissions, which have been shown to be poorly captured by methane models.**
- **We have added some context to the introduction related to the use of vegetation removal experiments and stable carbon isotope ratios to split CH_4 fluxes into their components (production, oxidation, transport pathways).**
- **We have streamlined the results section to better highlight the key findings of the study. This included limiting the main manuscript to the measurements taken in 2022 in the manuscript and moving figures containing the 2021 data to the appendix. This had the additional advantage that we could more directly relate our flux measurements to the pore water data which was only usable for 2022. To even more directly relate the CH_4 fluxes to the environmental and ecological data and to the pore water data, we have furthermore split our measurements into field campaigns instead of aggregating them by season. We have added figure panels showing the environmental and ecological data and described the data in the text.**
- **We have used a stable carbon isotope mass balance model and added the modeled potential CH_4 concentrations in the pore water in the absence of oxidation and transport as well as the fraction of CH_4 lost from the peat through oxidation or transport to the results section. We have added a description of the model to the methods section.**

The study "Seasonal controls on methane flux components in a boreal peatland - combining plant removal and stable isotope analyses" is an interesting field experiment conducted in a Finish boreal bog which looked at d13C-CH4 composition, CH4 concentration in peat porewater along with CH4 emissions (plant-mediated + diffusion + ebullition). The authors designed an experiment in which they were able to isolate the contribution of CH4 emission or oxidation from different vegetation types. The study was conducted during the growing season 2021 and 2022 using manual flux chamber measurements in 15 different plots (5 spatial replicates of three different treatment plots). The main findings from the study are that methane oxidation in the Sphagnum moss layer decreases total methane emissions by 82 ± 20 % while transport of methane through aerenchymatous plants increases methane emissions by 80 ± 22 %. Although not mentioned in the abstract, the authors also found higher CH4 emission at lower water table levels which raised my attention since it goes against the general consensus that greater CH4 emissions occur at higher water table levels.

The manuscript is coherent and well-detailed. I found the results section a bit lengthy and tedious, however. Removing secondary information might help increase the clarity of the text, if the authors wish to do so.

We have revised the results section to make it more concise and easier to follow, putting more emphasis on the key results of the study related to the research objectives.

The discussion was clear, well-structured and furnished with relevant references. Despite my overall enthusiasm about the study, I still have some major and minor comments that would deserve to be considered. Please see below.

Major comments:

I do not think that the study is directly investigating the effect of climate change on peatlands CH4 emissions. The authors have only conducted manual measurements over the growing season in 2021 and 2022. I would recommend the authors to focus on the methane emission pathways and avoid referring directly to climate change when discussing their results.

We agree that since we only measured during two years and since the direction of change of some environmental variables, such as of hydrological conditions, is not even clear, any conclusions on the response of CH4 emissions from boreal peatlands to climate change go beyond the scope of our study. We have therefore revised the relevant paragraphs in the abstract, introduction, discussion and conclusion sections to instead emphasize the relevance of our study to improving our understanding of seasonal differences in the processes controlling CH_4 emissions and in particular of shoulder season processes, which have been shown to be poorly captured by methane models.

Although the results and interpretation are clear within the main text (i.e. vascular plants increase CH4 emissions while Sphagnum increase methane oxidation), the overall outcome and implications of the work are confusing. In the abstract the authors wrote "The provided insights can help to improve the representation of environmental controls on the methane cycle and its seasonal dynamics in process-based models to more accurately predict future methane emissions from boreal peatlands." In the conclusion they recommend that "Better understanding the effect of peatland vegetation on CH4 emissions and its seasonal dynamics and incorporating it into process-based models will therefore greatly improve our estimates of future CH4 emissions from boreal peatlands under the changing climate." While I agree with the suggestions, I feel that the authors did not fully delivered here since they presented contrasted results without explaining how their findings should be incorporated into models and projections. Moreover, findings from the study suggest that "aerenchymatous plants increases methane emissions by 80 ± 22 %" while "Sphagnum moss layer decreases total methane emissions by 82 ± 20 %". In other words, the two processes seem to cancel each other. The strength of the paper is that the authors were able to isolate those pathways which helps understand the respective contribution of different vegetation types on methane emissions but I don't think that the findings presented are fundamentally changing the way CH4 emissions from peatlands are being measured and integrated into models. I would *recommend the authors to better link their findings with the needs for the process-based model developments they claim.*

This outside perspective has greatly helped us to reflect on the context for our study. Ito et al. (2023) found that simulated CH_4 fluxes differed strongly between process-based models during the periods of "zero-curtain" temperatures in the shoulder seasons. They attribute this observation to uncertainties in the parameterization of the dependency of CH_4 production and oxidation on peat temperatures and of the seasonally changing relative contribution of transport pathways to total CH_4 emissions.

Shifting the focus of our study towards the seasonal variation in the controls on CH_4 emissions and their components has emphasized the novelty of our findings as well as their use for improving process-based modelling of CH_4 emissions. We have emphasized our findings which improve our process-understanding of the CH_4 cycle, particularly during the shoulder seasons both in the results as well as in the discussion and conclusion section. Key results are that:

- **CH_4 transport through aerenchymatous peatlands plants continued after plant senescence.**
- **Decaying vascular plants provided additional substrate for CH_4 production at the end of the growing season.**
- **The emission of the CH_4 produced in summer and winter was partly delayed to the shoulder seasons due to accumulation of CH_4 in the pore water.**
- **CH_4 oxidation in the shoulder seasons was limited mainly by the availability of CH_4 in the pore water.**

Our results show that shoulder season CH_4 emissions are the complex result of a seasonally changing balance between CH_4 production, oxidation and transport. In order to improve their estimates of shoulder season CH4 fluxes, process-based models therefore need to account for the seasonal variation in CH_4 flux components based on changes in the water table depth, the peat temperature profile and vegetation characteristics.

Ito, A., Li, T., Qin, Z., Melton, J. R., Tian, H., Kleinen, T., et al. (2023). Cold-season methane fluxes simulated by GCP-CH_4 models. *Geophysical Research Letters*, 50, e2023GL103037. https://doi.org/10.1029/2023GL103037

I was surprised by the statement "higher CH4 emission occurred at lower water tables" which wasn't supported by any figure or statistical analysis. If this claim were to be true, it would go against the general consensus and would deserve further elaboration from the authors. Here are some global references showing the clear relationship between water table level and CH4 emissions in peatlands and wetlands.

Evans, C. D., Peacock, M., Baird, A. J., Artz, R. R. E., Burden, A., Callaghan, N., et al. (2021). Overriding water table control on managed peatland greenhouse gas emissions. Nature, 593(7860), 548–552. https://doi.org/10.1038/s41586-021-03523-1

Huang, Y., Ciais, P., Luo, Y., Zhu, D., Wang, Y., Qiu, C., et al. (2021). Tradeoff of CO2 and CH4 emissions from global peatlands under water-table drawdown. Nature Climate Change. https://doi.org/10.1038/s41558-021-01059-w

Zou, J., Ziegler, A. D., Chen, D., Mcnicol, G., Ciais, P., Jiang, X., et al. (2022). Rewetting global wetlands effectively reduces major greenhouse gas emissions. Nature Geoscience, 15(August), 627–632. https://doi.org/10.1038/s41561-022-00989-0

We have added the following paragraph discussing this unexpected observation to the discussion section:

"Higher CH_4 emissions at lower water levels in this study are unexpected and are most likely related to the covariation of the water table depth with peat temperatures and the leaf area of aerenchymatous plants, which exerted a stronger effect on CH_4 emissions than the small variations in water table depth. Higher oxidation rates in submerged Sphagnum moss due to the symbiosis between Sphagna and methanotrophs (Liebner et al., 2011) could have further contributed to higher emissions at lower water levels. An alternative explanation for the counterintuitive effect of the water table on CH_4 emissions could be the degassing of CH_4 that is trapped in the soil pores (even below the water table the peat is usually not fully water saturated) upon a drop in the water table (Moore et al., 1990; Moore and Roulet, 1993; Dinsmore et al., 2009). The number of chamber measurements showing episodic ebullition events however indicates less ebullition from the control plots following the decrease in water table between spring and summer in 2021."

Liebner, S., Zeyer, J., Wagner, D., Schubert, C., Pfeiffer, E.-M., and Knoblauch, C.: Methane oxidation associated with submerged brown mosses reduces methane emissions from Siberian polygonal tundra, Journal of Ecology, 99, 914–922, https://doi.org/10.1111/j.1365-2745.2011.01823.x, 2011.

Moore, Tim & Roulet, Nigel & Knowles, Roger. (1990). Spatial and temporal variations of methane flux from subarctic/northern Boreal fens. Global Biogeochemical Cycles - GLOBAL BIOGEOCHEM CYCLE. 4. 29-46. 10.1029/GB004i001p00029.

Moore T R and Roulet N T (1993) Methane Flux - Water-Table Relations in Northern Wetlands. Geophys Res Lett 20:587-590.

Dinsmore, Kerry & Skiba, U. & Billett, M. & Rees, Bob. (2009). Effect of water table on greenhouse gas emissions from peatland mesocosms. Plant Soil. 318. 229-242. 10.1007/s11104-008-9832-9.

I am sorry if I missed it but could the authors clearly explain how the respective contribution of aerenchymatous plants and sphagnum moss to CH4 emissions was determined since it is an important part of the study – perhaps by using a conceptual diagram.

The effects of vascular plants and of the *Sphagnum* layer on the CH_4 fluxes were calculated by subtracting the CH_4 fluxes from the vegetation removal treatments, as given in equations (1) and (2). For clarification, we have added a conceptual diagram to Figure 1c.

I also wonder how confident the authors are that the numbers provided and the approach used is relevant and representative beyond their study site?

The main goal of our study was to improve our process-understanding of seasonal differences in CH_4 fluxes and how the processes contributing the CH_4 emissions differ. The comparison between vegetation treatments and seasons provides the relative importance of CH_4 production, CH_4 oxidation and transport pathways and its seasonal variation. Based on the identified environmental and ecological controls, our findings could theoretically be applied to other sites also with different environmental conditions. Unfortunately, the majority of studies that have looked into CH_4 processes have been focused on growing season, limiting the comparison of the findings to other studies.

Environmental conditions and vegetation composition at Siikaneva bog are typical for Finnish bogs which cover large areas of the country. Since bogs are primarily rain-fed, we expect local conditions to have a smaller effect on CH_4 emissions from bogs than from fens; for example, variability in annual CH_4 emissions from bogs is substantially smaller than from fens and marshes (Treat, Virkkala et al., 2024). This lower spatial variation between bogs could make our measurements more generally representative of boreal, non-permafrost bogs which are widespread mainly in Russia, Alaska and Canada.

Furthermore, our study is based on measurements from wet hollows which cover about 20 % of Siikaneva bog (Alekseychik et al., 2021), making them the second largest microtopography type after lawns. Korrensalo et al. (2018) found that net CH_4 fluxes do not differ significantly between microtopography types at Siikaneva bog, supporting the relevance of our study results also for larger areas.

Ström et al. (2005) showed that the effect of vascular plants on CH_4 fluxes strongly depends on the plant species. Our results might therefore mainly be representative of sites where *Scheuchzeria palustris* is the dominant aerenchymatous plant species. The seasonal variation in the importance of plant transport might however still be indicative also of other aerenchymatous plant species.

The vegetation removal approach has been used before to identify plant effects on CH_4 fluxes and to split CH_4 fluxes into their components (e.g. Frenzel & Karofeld, 2000; Riutta et al., 2020). We have added some context related to vegetation removal experiments to the introduction. Depending on the water table depth and the vascular plant species, the effect of the *Sphagnum* moss layer and of the vascular plants might not be directly related to oxidation and plant transport rates of CH_4 (as shown in our study using the pore water concentrations and stable carbon isotope ratios) in other peatlands or other microtopography types within the same peatland. If a quantification of CH_4 oxidation and plant transport is intended, the acrotelm instead of the living moss layer would have to be removed and the assumptions could be tested for example using isotopic data.

Alekseychik, P., Korrensalo, A., Mammarella, I., Launiainen, S., Tuittila, E.-S., Korpela, I., and Vesala, T.: Carbon balance of a Finnish bog: temporal variability and limiting factors based on 6 years of eddy-covariance data, Biogeosciences, 18, 4681–4704, https://doi.org/10.5194/bg-18-4681-2021, 2021.

Frenzel, P., Karofeld, E. CH4 emission from a hollow-ridge complex in a raised bog: The role of CH4 production and oxidation. *Biogeochemistry* 51, 91–112 (2000). https://doi.org/10.1023/A:1006351118347

Korrensalo, A., Männistö, E., Alekseychik, P., Mammarella, I., Rinne, J., Vesala, T., and Tuittila, E.-S.: Small spatial variability in methane emission measured from a wet patterned boreal bog, Biogeosciences, 15, 1749–1761, https://doi.org/10.5194/bg-15-1749-2018, 2018.

Riutta, T., Korrensalo, A., Laine, A. M., Laine, J., and Tuittila, E.-S.: Interacting effects of vegetation components and water level on methane dynamics in a boreal fen, Biogeosciences, 17, 727–740, https://doi.org/10.5194/bg-17-727-2020, 2020.

Ström, L., Mastepanov, M. & Christensen, T.R. Species-specific Effects of Vascular Plants on Carbon Turnover and Methane Emissions from Wetlands. *Biogeochemistry* 75, 65–82 (2005). https://doi.org/10.1007/s10533-004-6124-1.

Treat, C. C., Virkkala, A.-M., Burke, E., Bruhwiler, L., Chatterjee, A., Fisher, J. B., et al. (2024). Permafrost carbon: Progress on understanding stocks and fluxes across northern terrestrial ecosystems. *Journal of Geophysical Research: Biogeosciences*, 129, e2023JG007638. https://doi.org/10.1029/2023JG007638

I wonder if a stable isotope mass balance model could help further support their findings by using a second approach that is independent of the first one. For example, previous studies were able to differentiate CH4 loss between ebullition and plant-mediated transport. Please see the reference below:

Corbett, J. E., Tfaily, M. M., Burdige, D. J., Cooper, W. T., Glaser, P. H., & Chanton, J. P. (2013). Partitioning pathways of CO2 production in peatlands with stable carbon isotopes. Biogeochemistry, 114(1–3), 327–340. https://doi.org/10.1007/s10533-012-9813-1

Holmes, M. E., Chanton, J. P., Tfaily, M. M., & Orgam, A. (2015). CO2 and CH4 isotope compositions and production pathways in a tropical peatland. Global Biogeochemical Cycles, 29, 1–18. https://doi.org/10.1111/1462-2920.13280

We have used the stable carbon isotope mass balance model by Corbett et al. (2013), as suggested by the reviewer. We have added the derived potential concentration of CH_4 dissolved in the pore water in the absence of CH_4 oxidation and transport as well as the fraction of CH_4 lost from the peat through oxidation and transport to the text and figure 3 of the results section. Uncertainties in model parameters did not allow us to separately quantify the rates of CH_4 oxidation and plant transport. We discuss this issue in appendix text A1.

Below are the minor comments I made while going through the manuscript

General: It would have been easier for the reviewers to have the line number provided for all the lines.

Abstract :

Line 1: The general statement "wetlands are highly vulnerable to climate change" is not clearly explained or mentioned in the manuscript. I wonder if it makes sense to start the abstract with this. How does a study looking at seasonal variability providing insight on an ecosystem response to climate change? The time scales are different. Moreover, the study is about peatlands not wetlands.

We have removed this sentence when redefining the focus of our study.

Line 5: I am assuming that methane emission means diffusion + ebullition? If not, better to state methane diffusion instead.

Methane emission in our study means diffusion through the peat and through plant aernchyma. Episodic ebullition events are excluded from our flux calculations as explained in the methods section. We have clarified in the discussion that CH_4 is also transported through plant aerenchyma by diffusion for the sedge species present in our measurement plots.

Line 7-8: Interesting. This may be true at the plot scale but I think water table level would still play a big role at the ecosystem scale if the authors would have considered the elevation gradient within their experimental design, for example.

We agree – the counterintuitive relation between CH_4 fluxes and water table depth is probably due to a low variation in water table depth between the spatial replicates within the hollow microtopography type. We have emphasized more strongly in the manuscript that our study focusses on the wet hollows of the bog which show little spatial variation in the generally high water level.

Line 9: "Increases" or "Contributes to"?

We have removed the respective sentence when revising the manuscript.

Line 11-12: I am not sure I understand this sentence correctly. What is left in a peatland if sphagnum and vascular plants are removed? It may be good to rephrase with the word "presence". Boreal peatlands are by definition occupied by sphagnum moss, aren't they?

We have removed the respective sentence when revising the manuscript.

Line 13-14: Care must be taken when linking environmental variables with climate change. The effect of climate change is usually described (and observed) over a decadal time scale or longer…

We agree that conclusions on the effect of climate change on the CH_4 emissions go beyond the scope of our study and have therefore removed this sentence from the abstract.

Introduction:

Line 22: It may be good to add a sentence to explain that while water-saturated peatland soil prevents organic matter oxic decomposition, they also favour anoxic degradation pathways such as methanogenesis. This will help connect the two sentences.

We have altered the introduction to the general background of the study when revising the manuscript so this comment does not apply anymore.

Line 25: Is it accurate to put at the same level vegetation composition, that soil temperature and WTD here? IMHO, the weather and climate directly influence soil temperature and WTD which in turn my affect the vegetation composition.

We have removed the respective sentence when revising the manuscript.

Line 26-29: How does "a shift in vegetation communities" will "likely result in a widespread drying trend in boreal peatlands"? I understand the hydrological feedbacks part but I don't know if one can say that vegetation communities directly influence ecosystem's moisture. Again, I wouldn't put vegetation communities at the same level than the two other environmental variables.

We have removed this sentence when revising the manuscript and referring less to climate change.

Line 28-31: Could the author be clearer here? The sentence doesn't say much. Is climate change going to increase or decrease CH4 emissions from boreal peatlands? Terms like "might considerably affect" or "altering" are very general. If the direction and magnitude of CH4 change from boreal peatlands cannot be clarified or supported by the literature, I suggest removing this part.

We agree that there is no consensus on the direction of changes in the literature. We have removed this sentence when reducing the emphasis on climate change.

Line 31: Net "flux" of CH4 produced by methanogenesis?

We have expanded the explanation of the processes involved in the peatland CH_4 cycle to:

"In peatlands, CH_4 is produced by methanogenic archaea in the anaerobic peat zone below the water table (catotelm). A part of the CH_4 is converted to CO_2 by methane oxidizing archaea (methanotrophs) mostly under aerobic conditions above the water table in the surface peat layer (acrotelm) (Hanson and Hanson, 1996). The amount of CH_4 emitted to the atmosphere furthermore depends on the pathway of CH_4 transport (Lai, 2009)."

Hanson, R. S. and Hanson, T. E.: Methanotrophic bacteria, Microbiological reviews, 60, 439–471, https://doi.org/10.1128/mr.60.2.439-471.1996, 1996.

Lai, D.: Methane dynamics in northern peatlands: a review, Pedosphere, 19, 409–421, https://doi.org/10.1016/S1002-0160(09)00003-4, 2009.

Line 34: How can a gas be stored in the peat without evading or being oxidized? Do the authors mean in the peat "pore water" as dissolved gas?

We have added that the CH_4 is dissolved in the pore water.

Line 34: I suggest replacing "CH4 flux" by "CH4 diffusion and ebullition".

Here in line 34 we mean generally the net CH_4 flux which is controlled by its three components – CH_4 production, oxidation and transport. We have tried to clarify the whole paragraph and revised the text on lines 31 – 35 as:

"In peatlands, CH_4 is produced by methanogenic archaea in the anaerobic peat zone below the water table (catotelm). A part of the CH_4 is converted to CO_2 by methane oxidizing archaea (methanotrophs) mostly under aerobic conditions above the water table in the surface peat layer (acrotelm) (Hanson and Hanson, 1996). The amount of CH_4 emitted to the atmosphere furthermore depends on the pathway of CH_4 transport (Lai, 2009). CH_4 following the concentration gradient to the atmosphere via diffusion through the peat is most prone to oxidation in the acrotelm while CH_4 emitted through aerenchyma of peatland sedges or in the form of gas bubbles (ebullition) passes by the oxidation layer. All three components of CH_4 fluxes - production, oxidation, and transport - are sensitive to changes in environmental and ecological conditions.

Hanson RS, Hanson TE. Methanotrophic bacteria. Microbiol Rev. 1996 Jun;60(2):439-71. doi: 10.1128/mr.60.2.439-471.1996.

Lai, D.: Methane dynamics in northern peatlands: a review, Pedosphere, 19, 409–421, https://doi.org/10.1016/S1002-0160(09)00003-4, 2009.

Line 38: Environmental or "Environmental and ecological"?

We have now used the terms "environmental and ecological" together for the controls on CH_4 emissions throughout the manuscript.

Line 58: I think what the authors mean here is the "carbon stable isotope ratio ($\delta^{13}C$-CH_4)"

Yes, we have changed the wording in the sentence accordingly to "…stable carbon isotope ratios of dissolved CH_4…"

Line 59: Since most of the introduction was on understanding the impact of climate change on peatlands, I wonder what kind of answers vegetation removal experiment can provide to answer the stated research question?

We have removed the part about understanding the impact of climate change on peatlands from the introduction and added the use of vegetation removal treatments: "Vegetation effects on peatland CH_4 emissions have been investigated in plant removal experiments, showing that vascular plants generally enhance CH_4 emissions through plant-mediated CH_4 transport (Frenzel and Karofeld, 2000; Riutta et al., 2020; Galera et al.,

2023) while oxidation in the living layer of *Sphagnum* moss has a decreasing effect on the CH_4 emissions (Frenzel and Karofeld, 2000)."

Frenzel, P. and Karofeld, E.: CH4 emission from a hollow-ridge complex in a raised bog: The role of CH4 production and oxidation, Biogeochemistry, 51, 91–112, https://doi.org/10.1023/A:1006351118347, 2000.

Riutta, T., Korrensalo, A., Laine, A. M., Laine, J., and Tuittila, E.-S.: Interacting effects of vegetation components and water level on methane 815 dynamics in a boreal fen, Biogeosciences, 17, 727–740, https://doi.org/10.5194/bg-17-727-2020, 2020.

Galera, L. d. A., Eckhardt, T., Beer, C., Pfeiffer, E.-M., and Knoblauch, C.: Ratio of in situ CO2 to CH4 production and its environmental controls in polygonal tundra soils of Samoylov Island, Northeastern Siberia, Journal of Geophysical Research: Biogeosciences, 128, e2022JG006 956, https://doi.org/10.1029/2022JG006956, 2023.

Line 60: The authors could mention the term "ombrotrophic" here. Nevertheless, I don't think the definition of a bog should appear after stating the research objectives.

We have added to our description of the study site that the study was carried out in an ombrotrophic bog and removed the general definition of bog from the introduction.

Line 62: CH4 emission rates.

We have rewritten our research aim and study objectives to:

"In this study, we aimed to identify the processes controlling shoulder season CH_4 emissions from wet hollows, i.e. typically high-emitting microtopographical features of a boreal bog (Turetsky et al., 2014) that are highly sensitive to changes in environmental conditions (Kotiaho et al., 2013). Our objectives were to quantify seasonal differences in (1) net CH_4 emissions; (2) CH_4 oxidation; and (3) plant-mediated CH_4 transport and to relate these to seasonal changes in environmental and ecological conditions. We achieved this by isolating the seasonal effects of vascular plants and Sphagnum moss on CH_4 emissions using vegetation removal experiments and relating the plant effects to CH_4 production, oxidation, and transport using pore water data, including the concentrations and stable carbon isotope ratios of dissolved CH_4. We considered the water level, the leaf area of vascular plants and the peat temperatures in acrotelm and catotelm as potential environmental and ecological controls on the components of CH_4 fluxes."

Turetsky, M. R., Kotowska, A., Bubier, J., Dise, N. B., Crill, P., Hornibrook, E. R., Minkkinen, K., Moore, T. R., Myers-Smith, I. H., Nykänen, H., et al.: A synthesis of methane emissions from 71 northern, temperate, and subtropical wetlands, Global change biology, 20, 2183–2197, https://doi.org/10.1111/gcb.12580, 2014.

Kotiaho, M., Fritze, H., Merilä, P., Tuomivirta, T., Väliranta, M., Korhola, A., Karofeld, E., and Tuittila, E.-S.: Actinobacteria community structure in the peat profile of boreal bogs follows a variation in the microtopographical gradient similar to vegetation, Plant and Soil, 369, 103–114, https://doi.org/10.1007/s11104-012-1546-3, 2013.

Line 63: Sorry but I couldn't find the statement that "hollows are the most sensitive to climate change" in Kokkonen et al., 2019. The term "hollow" is only mentioned once in the document.

We agree that this statement was too far-fetched and based on our own interpretation of the publication which used a water table drawdown to simulate climate change. We have remove this statement when generally referring less to climate change.

Line 83: I haven't been able to find the microtopography mapping methodology in Alekseychik et al., 2021. I am particularly interested in knowing how the difference between lawns and hollows were made since they usually follow an elevation gradient and are occupied by the same type of vegetation.

As mentioned in the description of the study site, at Siikaneva bog, hollows have been defined as wet surfaces that are dominated by *Sphagnum cuspidatum* and *Sphagnum majus* with vascular plant species adapted to wet conditions, such as *Carex limosa*, *Rhynchospora alba* and *Scheuchzeria Palustris.* While some of the same vascular plant species also grow on lawns, lawns are more intermediate in their water table and are dominated by *Sphagnum magellanicum*, *Sphagnum rubellum* and *Eriophorum vaginatum*.

Line 89: What was the area of each plot?

We have added the plot and chamber dimensions to the methods section:

"For the flux measurements, we placed a transparent cylindrical chamber with a volume of 36 l (inner height of 39.0 cm and inner diameter of 34.4 cm) on the collars at the plots (inner diameter: 30.7 cm, surrounding an area of 0.074 m^2)."

Line 90: When saying "vascular plants removed", do the authors also mean the roots or only the aboveground part? This would mean that the fresh yet dead roots were available for decomposition. For the P plot, how thick (cm) was the removed layer?

We have elaborated the description of the vegetation removal experiment in the methods section:

"We used a vegetation removal experiment, established in 2016, with one control plot and two treatments that allowed us to isolate the effects of vascular vegetation and moss on CH_4 emissions. The control plot had intact natural vegetation including *Sphagnum* mosses and vascular plants (peat-sphagnum-vascular, or PSV), one treatment had all vascular plants removed and only the Sphagnum moss layer remaining (PS), and another treatment had all vegetation removed, leaving behind a bare peat surface (P). For the plant removal treatments, all vascular plants had been clipped from an area of 0.5 m^2 (50 x 100 cm) and the area had been surrounded by polypropylene root barrier fabric 70 cm deep in the ground to keep roots from growing back into the area from the sides. Ever since, any newly growing vascular plants have been gently pulled out with their roots. We assume that the disturbance caused by establishing the plant removal plots, including the gradual death and decomposition of the below-ground parts of the clipped plants, was negligible in our study, five years after the experiment was installed (Riutta et al., 2020).

To create the P treatment, within the vegetation removal area, about 40 x 40 cm of the 4 to 5 cm thick living layer of the Sphagnum moss carpet had been cut out and placed on net fabric in a frame that could be lifted aside exposing the bare peat. Circular aluminum collars (inner diameter: 30.7 cm) for chamber measurements were permanently installed at the PSV and PS plots while at the P plots the moss layer was lifted aside and a collar was placed underneath only for the time of chamber measurements. There were five spatial replicate plot clusters within the hollow microtopography type placed along a boardwalk in Siikaneva bog, each comprising one control plot and one of each vegetation treatments (Figure 1b,c)."

Riutta, T., Korrensalo, A., Laine, A. M., Laine, J., and Tuittila, E.-S.: Interacting effects of vegetation components and water level on methane dynamics in a boreal fen, Biogeosciences, 17, 727–740, https://doi.org/10.5194/bg-17-727-2020, 2020.

Line 93: The root barrier intrusion may have cut the roots. This would mean the fresh yet dead roots were available for decomposition. Was this considered as a possible bias in the study?

Yes, we have considered the effect of the disturbance caused by establishing the plant removal plots. We established the plots originally in 2016 and did not start any measurements from the plots at least until the next growing season 2017. Data for the current study has been collected in 2021 and 2022, and thus, we assume that the effect of decomposing dead roots that were cut on the sides is negligible five years after the experiment was set up. We will further clarify this in the manuscript.

Line 112-115: What hypothesis were the authors trying to test here? Is light expected to influence CH4 emission?

We have added the following paragraph to the methods section:

The different light levels were chosen to partition the CO_2 fluxes that were measured alongside the CH_4 fluxes but that are not part of this study. Since the CH_4 fluxes did not differ significantly between the light levels ($t(64) = 1.178$, $p = 0.2432$) we treated light and dark measurements of CH_4 as temporal replicates in the data analysis.

We still tested the CH_4 fluxes for a potential light response since CH_4 oxidation has been earlier found to depend on the incoming light through a symbiosis between methanotrophs and *Sphagnum* moss (Liebner et al., 2011), as described in the introduction.

Liebner, S., Zeyer, J., Wagner, D., Schubert, C., Pfeiffer, E.-M., and Knoblauch, C.: Methane oxidation associated with submerged brown mosses reduces methane emissions from Siberian polygonal tundra, Journal of Ecology, 99, 914–922, https://doi.org/10.1111/j.1365-2745.2011.01823.x, 2011.

Line 154: Was there any statistical threshold (p value, r2) to determine if the diffusion flux was statistically significant or not?

We have added to the methods section that…

"We applied the post-hoc Tukey's HSD (honestly significant difference) test to identify significant differences ($p < 0.05$) between combinations of vegetation treatment, measurement campaign and sampling depth in the model results using the glht function of the package multcomp."

Line 160: By light conditions, do the authors mean transparent or dark chamber or based on the incoming radiation or photosynthetically active radiation?

We have changed the respective sentence to:

"We subtracted pairs of fluxes measured on the same day at the same spatial replicate and light level (transparent chamber, complete, single, or double shading of the chamber)."

Line 167: Interesting. How many times did this happen?

We have added to the methods section that…

"We discarded negative values of $F_{CH4,vascular}$ and $F_{CH4,\mathit{Sphagnum}}$ when the respective other was either also negative or missing as an additional quality indicator (10 %). We assume that these unexpected observations of higher emissions from the moss plots compared to the control and/or bare peat plots were caused by processes other than the direct vegetation effects, such as spatial or temporal variation in CH_4 emissions between the treatment plots or steady ebullition of micro-bubbles from the moss plots."

Line 176: pore water dissolved CH4

We have corrected this.

Line 189: Typo: The water samples for analysis of dissolved CH4 were kept cooled. Usually the headspace technique is done on site to avoid oxidation to happen in the meantime. I also wonder if the change of atmospheric pressure between the study site and lab may have affected the manipulation and results.

We have corrected the typo.

It is possible that there was some CH_4 oxidation happening in the pore water samples during storage but we assume the extent to be insignificant since we made sure that the samples were kept, that the storage time was not long and that we removed any air from the syringes before storage as much as possible. However, all samples were treated the same way and should therefore contain the same level of bias resulting from possible CH_4 oxidation during transport. This should sustain significant differences between the treatments but might affect the absolute values when comparing to values from the literature. We assume that the change in atmospheric pressure was negligible between field and the lab 10 km away. Further, processing the samples in the field contains other uncertainties, such as not being able to control the temperature of the water samples.

Line 195: Just out of curiosity, did the authors sometimes got a Chemdetect value of 1 when running they samples? If so, what action was taken to go around this?

Yes, we did get a Chemdetect value of 1 sometimes, also for some of our gas standards. We did not discard those measurements as long as the results were reasonable.

Line 207: Where was this reference gas / standard from?

We have added the following to the methods section:

"For this, samples of a reference gas (gas mixture purchased from Oy Linde Gas Ab with CH_4 concentration: 10 ppm, CO_2 concentration: 2000 ppm, $\delta^{13}C$-CH_4: -41.5 ‰, $\delta^{13}C$-CO_2: -35.6 ‰; $\delta^{13}C$ values of the reference gas were determined by calibrating it against four licensed standards from Air Liquide with $\delta^{13}C$-CH_4: -60 and -20 ‰, $\delta^{13}C$-CO_2: -30 and -5 ‰) were added at the beginning and at the end of each sample batch as well as after every 15 samples within the sample batch."

Line 237-238: It may have been good to explain in the introduction how each of these variables are likely to affect CH4 production and emission

We have added the following to the introduction:

"Peat temperatures and water level affect the rates of CH_4 production and oxidation by controlling the microbial activity and the thickness of the aerobic peat layer, respectively (Dunfield et al., 1993; Dise et al., 1993; Ström and Christensen, 2007). Peatland vegetation can affect all three components of CH_4 fluxes with in part opposing effects on net CH_4 emissions. Large areas of peatlands and especially of ombrotrophic bogs are typically covered by a layer of *Sphagnum* moss, which can actively enhance CH_4 oxidation rates through a symbiotic relation - methanotrophs provide the moss with CO_2 and in turn receive the oxygen released from moss photosynthesis (Larmola et al., 2010; Kip et al., 2010). Peatland sedges are adapted to high water levels by gas transport through the spongy tissue in their leaves, stems and roots (aerenchyma). On the one hand this gas transport can enhance CH_4 emissions by allowing the CH_4 to escape to the atmosphere without passing through the aerobic oxidation layer. On the other hand, oxygen can leak into the rhizosphere of aerenchymatous plants and allow for additional CH_4 oxidation in the otherwise anaerobic peat zone, thereby reducing net CH_4 emissions. Additionally, vascular plants can enhance CH_4 emissions by providing additional substrate for CH_4 production in the form of plant litter or root exudates (Joabsson et al., 1999)."

Dunfield, P., Dumont, R., Moore, T. R., et al.: Methane production and consumption in temperate and subarctic peat soils: response to temperature and pH, Soil Biology and Biochemistry, 25, 321–326, https://doi.org/10.1016/0038-0717(93)90130-4, 1993.

Dise, N. B., Gorham, E., and Verry, E. S.: Environmental factors controlling methane emissions from peatlands in northern Minnesota, Journal of Geophysical Research: Atmospheres, 98, 10 583–10 594, 1993.

Ström, L. and Christensen, T. R.: Below ground carbon turnover and greenhouse gas exchanges in a sub-arctic wetland, Soil Biology and Biochemistry, 39, 1689–1698, https://doi.org/10.1016/j.soilbio.2007.01.019, 2007.

Larmola, T., Tuittila, E.-S., Tiirola, M., Nykänen, H., Martikainen, P. J., Yrjälä, K., Tuomivirta, T., and Fritze, H.: The role of Sphagnum mosses in the methane cycling of a boreal mire, Ecology, 91, 2356–2365, https://doi.org/10.1890/09-1343.1, 2010.

Joabsson, A., Christensen, T. R., and Wallén, B.: Vascular plant controls on methane emissions from northern peatforming wetlands, Trends in Ecology & Evolution, 14, 385–388, https://doi.org/10.1016/S0169-5347(99)01649-3, 1999.

Line 259-262: Can this linear relationship be provided as a supplementary material?

We have added the following figures showing the linear relationship between the air temperatures and the water tables depths measured at Siikaneva fen and at Siikaneva bog to the appendix of the manuscript.

A linear regression for the air temperature was separately performed for the temperature range below -15 °C and equal to or above -15 °C at the fen site.

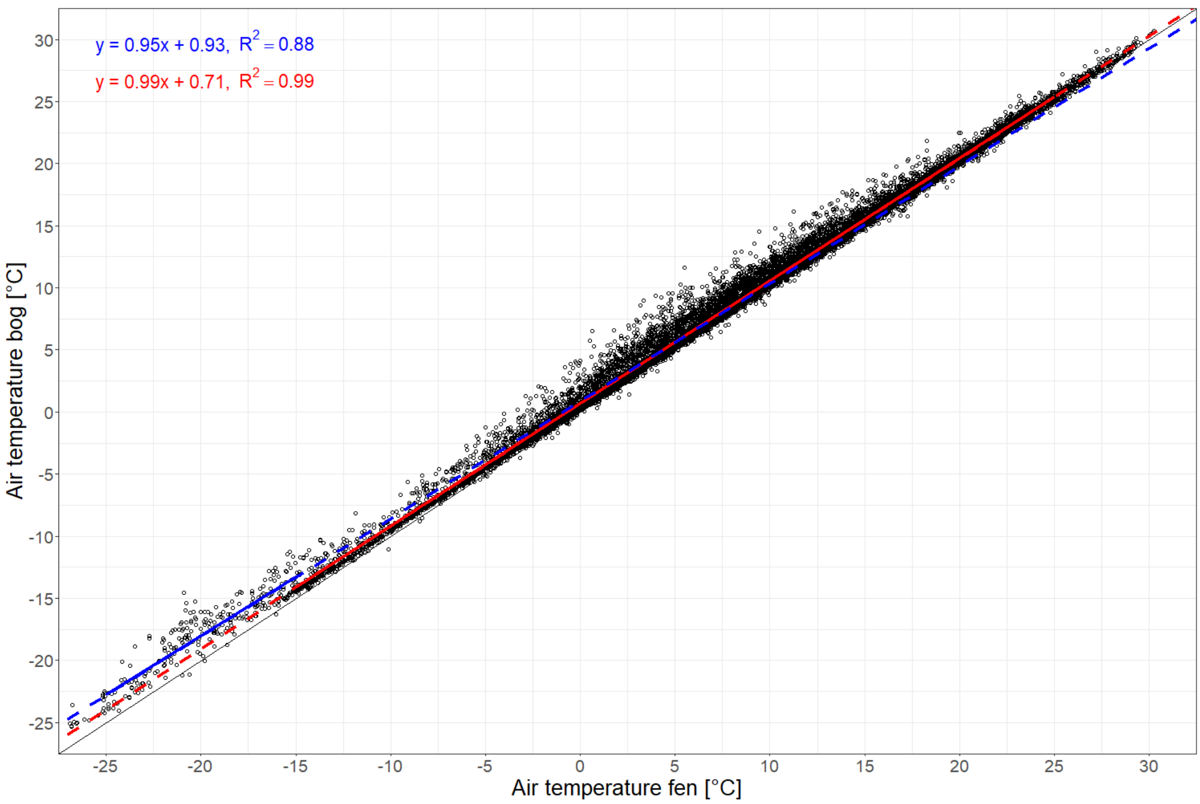

Figure A2: Linear regression between air temperatures recorded hourly at Siikaneva bog and at Siikaneva fen (https://smear.avaa.csc.fi/download; Station SMEAR II Siikaneva 1 (fen) and 2 (bog) wetland) between 2012 and 2016. The air temperature was fit using 2 linear regressions with an inflection point at -15 °C at the fen site. The linear regressions for temperatures below -15°C and equal to or above -15°C are given in blue and red, respectively.

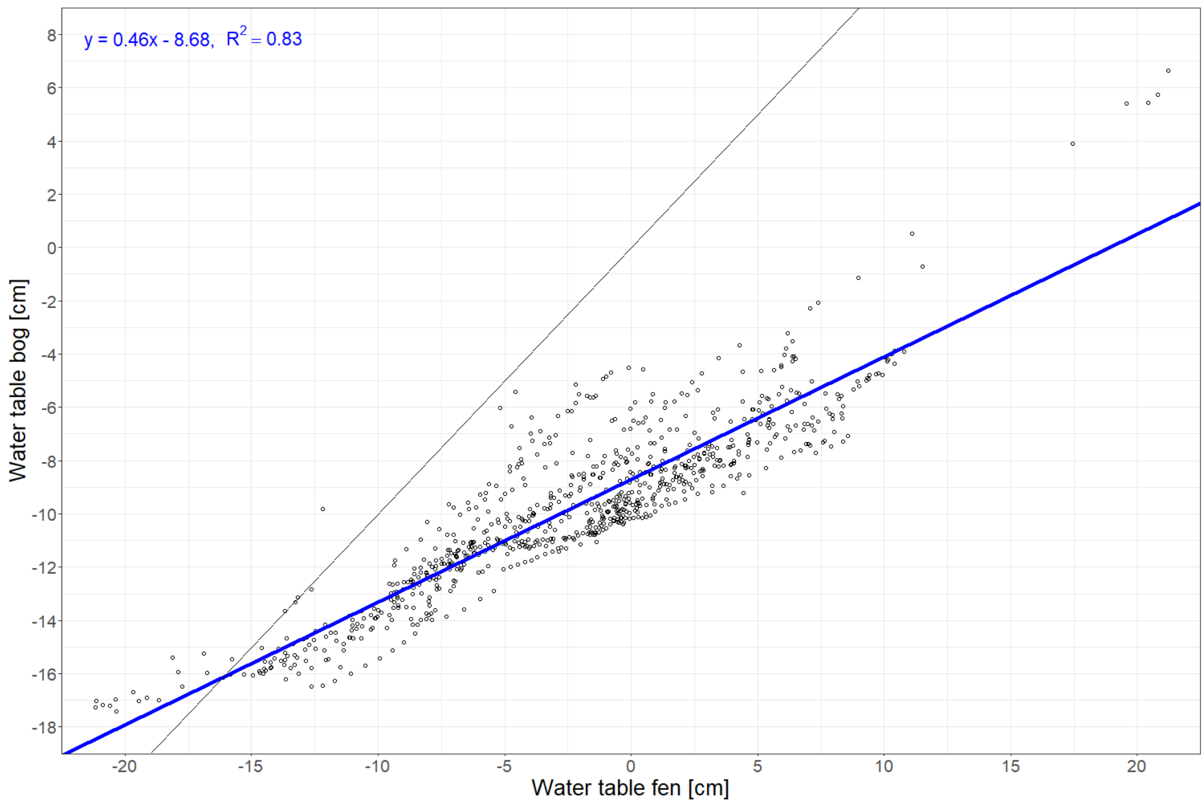

Figure: Linear regression between daily water table depths recorded at Siikaneva bog and at Siikaneva fen (https://smear.avaa.csc.fi/download; Station SMEAR II Siikaneva 1 (fen) and 2 (bog) wetland) between 2012 and 2016.

Line 265: If I understand correctly, the authors refer to "daily averaged temperature". It should be explicitly stated as such.

We have written the sentence to:

"To separate the measurement years into seasons we used the thresholds in daily mean temperatures of […]"

Line 274: OK, this answer the comment made for line 112-115. Maybe good to merge these two sentences for clarity.

We have moved the statement on similar CH_4 emissions at different light conditions to the description of the different light levels in the methods part on chamber measurements. That paragraph now reads:

"Each plot was usually measured twice - once under natural light conditions and once under dark conditions, with blackout fabric covering the chamber. In July 2021 measurements were additionally performed at two different levels of incomplete shading using one or two layers of net fabric, respectively. The different light levels were chosen to partition the CO_2 fluxes that were measured alongside the CH_4 fluxes but that are not part of this study. Since the CH_4 fluxes did not differ significantly between the light levels

(t(64) = 1.178, p = 0.2432) we treated light and dark measurements of CH_4 as temporal replicates in the data analysis.”

Line 297: What the authors mean here is “Ch4 emissions from our dataset”, I believe. The value of 2mgCH4m-2d-1 was only measured at peat + sphagnum moss, for example.

We have decided to only explicitly report the range in CH_4 emissions from the control plots:

“Emission rates ranged between a minimum of 34 mgCH_4 m^{-2} d^{-1} measured in spring and a maximum of 1025 mgCH_4 m^{-2} d^{-1} in summer.”

Line 300: Was this difference statistically significant?

The letters in figure 2 show that the presence of vascular plants led to significantly higher CH_4 emissions in early and late fall compared to the moss-only plots. In late fall, CH_4 emissions from the control plots were significantly lower than from the bare peat. From the significant differences shown in figure 2, we have concluded in the results sections that: “Both the decreasing effect of the *Sphagnum* moss and the increasing effect of the vascular plants on the CH_4 emissions were significant during the fall campaigns.”

Line 305-309: Were all these differences statistically significant?

Significant differences are shown by the letters in figure 2. We have mentioned relevant significant differences in the text of the results section.

Line 322: How was the effect of vascular plant and sphagnum calculated? Is it only a subtraction between the flux taken in different plots at the same time?

Yes, it is a simple subtraction, as explained by equations (1) and (2) the methods section and shown by the schematic in figure 1c.

Line 335-337: Should peat temperature and water table depth “influence the effect of the Sphagnum layer on CH4 fluxes” or simply “influenced CH4 fluxes”?

It is indeed the effect of the Sphagnum layer on CH_4 fluxes. We intend to identify the environmental controls on CH_4 oxidation which in this study is represented by the effect of the Sphagnum layer on the CH_4 fluxes, as justified in the discussion. We have, however, removed this sentence when revising the manuscript.

Figure 3a: The decision to merge pore water data for PA and P seems to go against the research objective…

We have added an explanation of our decision to the methods section:

We sampled once next to each control plot and once from the vegetation removal area. Since the bare peat plots were still covered with the removed moss layer sitting on net fabric apart for the short periods of flux measurements, we assumed that the investigated

pore water properties below the moss layer were similar between the moss and bare peat treatments.

Line 438-441: Can the author be more specific on how they were able to determine that HM was more important than AM based on Figure A2?

We have extended the explanation of this statement in the discussion section:

"Similar $\delta^{13}C$-CH_4 values at 50 cm depth across all measurement campaigns indicate that the stable carbon isotope ratio of CH_4 below the main root zone was mainly controlled by the pathway of methane production. As expected for a bog, below the rhizosphere, hydrogenotrophic methanogenesis, using H_2 and CO_2 to produce CH_4, dominated year-round over acetoclastic methanogenesis, using acetate as an electron acceptor. This is indicated by the low $\delta^{13}C$-CH_4 values and the high $\delta^{13}C$-CO_2 values at 50 cm depth, which result in a carbon isotope separation between CO_2 and CH_4 (ε_c) of 60 to 75 compared to the values for acetoclastic methanogenesis of 24 to 29, for hydrogenotrophic methanogenesis of 49 to 95 and for CH_4 oxidation of 4 to 30 (Whiticar, 1999) (Figure A8)."

Line 506: One word is missing here. Is it "balance"? If so, storage as dissolved gas and lateral exchange seem to be missing in the "equation".

We have revised this paragraph to clarify that CH_4 emissions depend on other processes besides CH_4 production and oxidation:

"CH_4 fluxes depend on the net balance of CH_4 production and CH_4 oxidation. The pathways of CH_4 transport further affect CH_4 fluxes by influencing the percentage of produced CH_4 that is either stored in the pore water, oxidized or directly emitted to the atmosphere."

Line 547: This is an interesting claim as it goes against most of the papers that have jointly measured WTL and CH4 emissions from peatlands. I am, however, unable to find any figure or relationship that is supporting the claim that the authors are making.

The significant relationship between CH_4 emissions and water table depth is shown in table A1. Our discussion of the unexpected water table relationship was given above.

Line 550: Again, I do not think the term "climate warming" is appropriate here.

We agree, that temperature variations between 2012 and 2022 should not be attributed to climate change without discussing the general trend in air temperatures in the region. We have removed this hypothesis from the manuscript.

Line 555: How much warmer and variable were the temperatures between the two periods mentioned?

Comparing the effective temperature sums of the growing seasons for 2021 (1484) and 2022 (1337) to the ones for 2012, 2013 and 2014 (1172, 1408, 1349) given by Korrensalo et al. (2018) showed us that based on this measure our study years were not generally warmer the former study years. We have therefore remove the sentences relating the

higher CH_4 fluxes found in our study compared to the study by Korrensalo et al. (2018) to the interannual variability in air temperatures.

Korrensalo, A., Männistö, E., Alekseychik, P., Mammarella, I., Rinne, J., Vesala, T., and Tuittila, E.-S.: Small spatial variability in methane emission measured from a wet patterned boreal bog, Biogeosciences, 15, 1749–1761, https://doi.org/10.5194/bg-151749-2018, 2018.

Figure A2: Why is there only 2 points for emissions? Could the colour code be for the sample depths and the shape code for the plot types? Additionally, the authors could considered give a CH4 concentration weighted-size of the points to show where the highest concentrations are located within the plot.

There are only two chamber measurements for which both $\delta^{13}C$-CO_2 and $\delta^{13}C$-CH_4 passed our quality control. Our quality filter, excluding $\delta^{13}C$ measurements with an r^2 of the keeling plots below 0.8 removed 79 % of the $\delta^{13}C$-CO_2 and 54 % of the $\delta^{13}C$-CH_4 measurements.

We have revised the figure following your suggestions:

We agree that including the additional information on the treatment type is valuable. It may however have made the figure difficult to access for some types of color vision deficiencies.

Following your suggestion, we have included the CH_4 concentration using the point size. This emphasizes the general increase of CH_4 concentrations with depth and with thus with ε_c values. Including the CH_4 concentrations removed the $\delta^{13}C$ values of emitted CH_4 and CO_2 from the figure.

<u>Response to reviewer comments by anonymous reviewer 2</u>

Following the comments by reviewer 2, we have added some background and previous findings for vegetation removal experiments and stable carbon isotope analyses to the introduction.

General comments:

The authors assessed the relative importance of different forcing variables on methane flux in Southern Finland. The main objective was to measure the seasonal variations of methane fluxes and the explaining variables. This study is at the crossroad of ecosystem functioning/climate change/biodiversity. As such, the questions addressed in this paper fall within the scope of BIOGEOSCIENCES.

On the whole, the manuscript is very well written and illustrated.

Although the objectives are clearly stated, the hypotheses are missing in the introduction. The authors manipulated the biodiversity by removing 1) vascular plants and 2) vascular plant and Sphagnum. They don't mention what are the expected effects of these treatments on methane flux. This information should be stated right from the introduction.

We have added previous findings of vegetation removal experiments on the effects of vegetation of CH_4 fluxes to the introduction:

"Vegetation effects on peatland CH_4 emissions have been investigated in plant removal experiments, showing that vascular plants generally enhance CH_4 emissions through plant-mediated CH_4 transport (Frenzel and Karofeld, 2000; Riutta et al., 2020; Galera et al., 2023) while oxidation in the living layer of *Sphagnum* moss has a decreasing effect on the CH_4 emissions (Frenzel and Karofeld, 2000)."

Frenzel, P. and Karofeld, E.: CH4 emission from a hollow-ridge complex in a raised bog: The role of CH4 production and oxidation, Biogeochemistry, 51, 91–112, https://doi.org/10.1023/A:1006351118347, 2000.

Riutta, T., Korrensalo, A., Laine, A. M., Laine, J., and Tuittila, E.-S.: Interacting effects of vegetation components and water level on methane 815 dynamics in a boreal fen, Biogeosciences, 17, 727–740, https://doi.org/10.5194/bg-17-727-2020, 2020.

Galera, L. d. A., Eckhardt, T., Beer, C., Pfeiffer, E.-M., and Knoblauch, C.: Ratio of in situ CO2 to CH4 production and its environmental controls in polygonal tundra soils of Samoylov Island, Northeastern Siberia, Journal of Geophysical Research: Biogeosciences, 128, e2022JG006 956, https://doi.org/10.1029/2022JG006956, 2023.

The methods are clearly described and can be reproduced. The methods and the statistical analyses used are adequate.

The discussion is relevant and supported by the results. However, some aspects of the article need to be clarified to reach a wider readership and to acknowledge a key issue in terms of soil physics between treatments.

Specific comment:

First, the readability and the understanding of the discussion on isotopic results would be greatly improved by giving some basic reminder of $\delta^{13}C$ signature of the different metabolic pathways. Also, some assertions are lacking explanation, such as the sentence of the lines 437/438-page 38. As it is, the discussion on the isotopic results is a bit hard to follow, and it fails to fully convince the reader. The manuscript would be greatly improved by clarifying all the sections dealing with isotopic results.

Along with the background on vegetation removal experiments we have also added a paragraph to the introduction explaining the use of stable carbon isotope ratios to split CH_4 fluxes into their components (production, oxidation, transport pathways):

"In previous studies, the rates and pathways of CH_4 production, oxidation, and transport have been quantified using […] stable carbon isotope modelling (e.g., Blanc-Betes et al., 2016; Dorodnikov et al., 2013; Knoblauch et al., 2015). Stable carbon isotope models make use of the characteristic trace that CH_4 production, oxidation, and transport leave in the stable carbon isotope ratios of CH_4 and CO_2 through their specific preferential use of molecules containing the lighter ^{12}C isotope."

Blanc-Betes, E., Welker, J. M., Sturchio, N. C., Chanton, J. P., and Gonzalez-Meler, M. A.: Winter precipitation and snow accumulation drive the methane sink or source strength of Arctic tussock tundra, Global Change Biology, 22, 2818–2833, https://doi.org/10.1111/gcb.13242, 2016.

Dorodnikov, M., Marushchak, M., Biasi, C., and Wilmking, M.: Effect of microtopography on isotopic composition of methane in porewater and efflux at a boreal peatland., Boreal environment research, 18, 2013.

Knoblauch, C., Spott, O., Evgrafova, S., Kutzbach, L., and Pfeiffer, E.-M.: Regulation of methane production, oxidation, and emission by vascular plants and bryophytes in ponds of the northeast Siberian polygonal tundra, Journal of Geophysical Research: Biogeosciences, 120, 2525–2541, https://doi.org/10.1002/2015JG003053, 2015.

Second, the Sphagnum removal should not be placed at the same level of vascular plant removal in terms of its effect on methane flux. Vascular plant removal (PS treatment) does not affect (or in a minor proportion) the physical condition compared to the control situation (PSV): same damping of air temperature with depth in both treatments, almost the same water table depth, supposedly. However, in the "P" treatment, the temperature amplitude at the maximum methane producing zone (just below the water table) should be greatly affected compared to the other two treatments, because of the physical removal of matter. Although the effect of a thicker peat layer on methane production and consumption is highlighted, the effect of the physical removal of a Sphagnum layer on abiotic variables and their subsequent effect on biological processes is not fully acknowledged. The "P" treatment is not only about biodiversity, but also about soil physics. It is in this sense that the "P" treatment is not at the same level as the vascular plant removal treatment. This should be more clearly stated and taken into account in the discussion. If the authors have high frequencies time series of soil temperature under each treatment at least one spatial replicate, they should add these data to the manuscript and use them to further improve the discussion.

We agree. However, the moss removal treatment was temporary in that it was limited to the few minutes of our flux measurement period. This was achieved by placing the moss layer on a mesh "tray" or frame that could be removed for the measurement period. Because of the limited time of removal, we expect the peat temperatures to be similar between the P and PS treatments and therefore did not measure the peat temperatures separately for the two treatments. We have added this information to the methods section by stating that:

"Circular aluminum collars (inner diameter: 30.7 cm) for chamber measurements were permanently installed at the PSV and PS plots while at the P plots the moss layer was lifted aside and a collar was placed underneath only for the time of chamber measurements."

and

“Since the bare peat plots were still covered with the removed moss layer sitting on net fabric apart for the short periods of flux measurements, we assumed that the investigated pore water properties below the moss layer were similar between the moss and bare peat treatments.”

We have furthermore considered the different water table conditions at the bare peat plots more explicitly in the discussion by stating that:

“The water table fell below the 4 to 5 cm thick living moss layer in summer and fall (Figure 2e) thereby exposing up to 7 cm of the peat below the living moss to oxygen.”

Technical comment:

Page 15 – line 372 : write “…was more depleted in ^{13}C …” instead of “…was more depleted in $\delta^{13}C$ …”

We have corrected this.

Page 20 – line 506 : should write “CH_4 flux is the net balance of CH4 production and CH4 oxidation” instead of “CH_4 flux is the net of CH4 production and CH4 oxidation”

We have rephrased this sentence to

“CH_4 fluxes depend on the net balance of CH_4 production and CH_4 oxidation.”