

We thank both referees for their careful assessment of our manuscript, “Vegetation structure drives carbon dioxide and methane dynamics in adjacent boreal fen and bog ecosystems”, and for their constructive comments. Below, we respond to each comment in turn and describe the corresponding changes made or proposed. Responses in the past tense refer to changes already made to the manuscript, whereas responses in the future tense describe changes that will be and are being incorporated into the revised manuscript.

Reviewer 1 comments, Katharina Jentsch

Summary

In their manuscript titled “Vegetation structure drives carbon dioxide and methane dynamics in adjacent boreal fen and bog ecosystems”, the authors characterize differences in CO₂ and CH₄ fluxes and their temporal patterns on different time scales between adjacent bog and fen ecosystems. They investigate possible explanations for the identified fen-bog differences in carbon fluxes considering biotic and abiotic environmental controls. They found that overall, more CO₂ is taken up and more CH₄ is emitted from the fen than from the bog and that carbon fluxes vary more strongly over the year in the fen than in the bog. They explain these differences by the higher abundance of (aerenchymatous) plants in the fen, which facilitate CH₄ transport, provide substrate for CH₄ production and allow for higher but also more variable rates of photosynthesis over the year.

Relevance

The topic of the presented manuscript is highly relevant and topical as it enhances our understanding of how carbon fluxes can change in the course of both commonly occurring natural fen-bog transitions but also with climate change driven changes in peatland types. The key finding of the important role of vegetation composition will be important to reduce uncertainties in larger scale flux estimates and model predictions.

Overall comments

Overall, the study is highly relevant and the results are presented in a very clear, well-structured and easy-to-follow way, supported by sound data analyses and informative figures. In my opinion, the manuscript is already in a very good state and I only have a few minor comments and suggestions.

The results chapter is quite long and everything presented here is relevant and interesting, I think that the structure could be made a bit clearer to make it even easier to follow. This could be done even just by slightly adjusting the section headings: Mostly, the heading of section 3.3.3 should be changed to include environmental drivers and thereby clarify that it is not merely describe temporal patterns as already done in section 3.2.

Thank you for the suggestion. The heading of Section 3.3.3 has been changed to “Seasonal differences in CH₄ responses to environmental drivers” to clarify that the section focuses on environmental controls of CH₄ fluxes rather than solely on temporal patterns.

Furthermore, while I very much like the figures used to support the results, I think that, if necessary, their number could be reduced by combining figures 7, 9, and 10 as they all essentially describe the temperature response of carbon fluxes. The figures could be adjusted to support only the key aspects mentioned in the main text and additional figures could be moved to the supplement.

Thank you for this suggestion. We have been reviewing Figures 7, 9 and 10 to reduce overlap and retain the key results in the main text. As part of this revision, we will move Figure 9 to the supplementary material.

Furthermore, I think that the introductory section of chapter 3.2 could be shortened by focussing only on differences in the overall range of carbon fluxes between fen and bog and keeping the description of seasonal dynamics to section 3.2.2, thereby clarifying the structure and avoiding repetitions.

Thank you for the suggestion. Section 3.2 has been revised accordingly, with the introductory text shortened to focus on the overall range of carbon fluxes, while the description of seasonal dynamics has been moved to Section 3.2.2 to avoid repetition.

In the analysis of potential drivers of C fluxes, for the relationship with LAI and photosynthesis-related variables, you refer to results from (linear mixed effects) models. It would be helpful to present model structures and parameters in a table in the supplement.

Thank you for this suggestion. We will add a supplementary table presenting the model structures, including fixed and random effects, together with parameter estimates and their uncertainty, and refer to this table in the main text.

In the interpretation of your results, you focus very strongly on differences in vegetation structure as explanation for fen-bog differences in carbon fluxes. While an important role of vegetation is also supported by your data analyses – this interpretation is probably also supported by the fact that LAI is the only variable considered that really differs between fen and bog in your study as climatic conditions are similar. Are there further variables that might differ between the two peatland types that you did not specifically consider in your study but that might also contribute to the observed differences in carbon fluxes? I think this could be an (optional) addition to the discussion chapter.

Thank you for this helpful comment. We agree that other ecosystem characteristics may contribute to the observed fen–bog differences in carbon fluxes. We have expanded the Discussion to consider differences in water table depth and the potential influence of nutrient availability, peat chemistry, pH and microtopography. We will also clarify that, although our analyses support an important role for vegetation structure, its influence cannot be fully separated from these co-varying factors in this observational comparison.

What I think should be added to the discussion though is a short statement on differences in water table depth as this is the only other variable besides LAI that differs between fen and bog. Could there be direct effects of the differences in water table depth besides its effect on vegetation composition that might contribute to differences in carbon fluxes?

Thank you for this suggestion. We have expanded the Discussion to acknowledge the potential contribution of water table differences to the observed carbon fluxes. However, our analyses did not identify consistent relationships between WTD and the fluxes, making its role difficult to distinguish from other ecosystem controls. Rinne et al. (2018) and Alekseychik et al. (2021) have also discussed this in their papers on the fen and the bog, we will include further discussions citing their findings. We will also include a supplementary figure that examines the relationship between WTD and CO₂ & CH₄ fluxes after accounting for their relationships with temperature and GPP. These analyses will help assess whether associations with WTD become clearer after accounting for these other controls.

Furthermore, I think you should be a bit careful in calling the bog a “shrub-dominated” ecosystem (e.g., lines 552 and 568). From my experience, shrubs mostly occur on hummocks at Siikaneva bog which are not the dominating microtopographic feature. While LAI is higher at the fen site, I would still say that the bog is similarly dominated by sedges. Instead of explaining fen-bog differences in seasonal variation in C fluxes by the dominance of sedges in the fen compared to a dominance of shrubs in the bog (lines 567-571), you could also consider differences in the species composition of sedges between fen and bog. For example, Korrensalo et al. (2022) and Jentzsch et al. (2025) different sedge species reach their peak LAI at different times of the year. Differences in sedge species composition and species diversity could therefore contribute to fen-bog differences in seasonal variability in C fluxes.

Thank you for this helpful clarification. We have revised the description of the bog to avoid characterising it as shrub-dominated and adjust the associated interpretation. We will also discuss differences in sedge species composition and phenology as potential contributors to the contrasting seasonal flux patterns, drawing on the suggested references.

Specific comments

l. 15 I think that if you say that peatland “increasingly” undergo fen-to-bog transitions, I think that you should also refer to climate change here.

Changed according to comment.

l. 29 You could consider expressing CH₄ fluxes in CO₂ equivalents to evaluate the overall C balance of Siikaneva fen vs. Siikaneva bog (at least for the active season).

We thank the reviewer for suggesting a combined assessment of CO₂ and CH₄ exchange using global warming potentials. We agree that the climatic implications of the contrasting fluxes warrant discussion. However, the objective of this study is to compare contemporary CO₂ and CH₄ exchange and their ecological controls in an adjacent fen and bog.

As discussed by Rinne et al. (2025), GWP-weighted contemporary fluxes do not adequately represent the current climate forcing of established natural mires, for which accumulated peat carbon is also relevant. A GWP-based comparison would therefore provide a time-horizon-dependent expression of the measured fluxes in CO₂-equivalent units, rather than establish the relative current climate effects of the two ecosystems. Furthermore, our measurements cover May–October and do not represent complete annual greenhouse-gas balances.

We therefore propose to retain the separate CO₂ and CH₄ assessments and expand the Discussion to explain this distinction and the limitations of inferring overall climate effects from the observed fluxes.

l. 41 You could add a transitional sentence here, linking CH₄ production to high water tables.

Very good observation and makes the text flow better. Thank you, it has been added.

l. 99 ff. Here, I would be curious to learn a bit more about the reasons for fen-bog differences in CO₂ fluxes identified in the literature. As a first guess I would hypothesize that CO₂ uptake is higher at a fen due to higher nutrient availability, plant productivity and photosynthetic uptake. What processes could your hypothesis of lower CO₂ assimilation at the fen be based in addition to previous observations?

Very good comment. We have added the explanations there, of respiration being higher in fens due to more available C in the system.

l. 115 The term “aapa mire” has not been introduced before – is it used as an equivalent to fen here? Please add a brief definition or stick to the term “fen” instead.

It has been changed to fen, for better coherence.

l. 118 remove the first “and”

Thank you, it has been removed.

Figure 1: consider adding a north arrow

Thank you for the suggestion. A north arrow has been added to Figure 1.

l. 129-130 “Lower lying areas...” Move this sentence to after the next sentence to first introduce microtopography in general and then name the individual microtopographic features.

Moving it makes it read much more easily. Thanks.

l. 150 Also add for the LI-7000 whether it is an open- or closed-path gas analyser.

We have added a better explanation to distinguish that it is a closed path analyser.

Section 2.4.3: In section 3.2.3, you refer to cumulative sums of CH₄ fluxes – Did you gapfil CH₄ fluxes for these calculations? If so, how?

Yes, the cumulative CH₄ fluxes were calculated using gap-filled data. The gap-filling procedure is described in Section 2.2.1. We have added an explicit reference to this procedure in Section 3.2.3 to clarify how the cumulative sums were calculated.

l. 260: what did you base your definition of the two seasons on? Increasing v.s. decreasing temperatures, plant activity etc.?

Thank you for this question. We have clarified that the division into May–July and August–October was based on the seasonal patterns of GPP and CH₄ fluxes, which generally peaked in July. These periods therefore broadly represent the increasing and decreasing phases of biological activity and CH₄ emissions.

l. 295 Consider adding to the methods section or to the discussion in which microtopographic feature (of the bog) the WTD sensor was installed.

Thank you for this suggestion. We have added a description of the microtopographic setting of the WTD sensor at each site in Section 2.3.

Figure 2: Just out of curiosity (no need to discuss in the manuscript): LAI at the bog in 2012 is very low compared to the other years while climatic conditions seem similar apart from rather high water levels. Do you think that LAI was actually this much lower in 2012 or could it be due to a methodological difference in measurements or something similar?

Thank you for this observation. The same measurement methods were used in 2012 as in the other years for collecting the LAI data. The wetter, cloudier and colder conditions in 2012 may have contributed to the lower LAI, although we cannot establish the cause conclusively. The same low LAI values for 2012 are also reported in the earlier publications of these data (Korrensalo et al., 2018, 2020).

l. 337-338 Add a figure reference to support the maximum in photosynthetic activity between July and August.

Thank you for the suggestion. We have added a reference to Fig. 3, which shows the seasonal time series and supports the timing of maximum photosynthetic activity.

Figure 5: Here you split the active season into 3 periods while in the methods section you define two seasons. Consider changing in the methods in case you do not use the two-season-definition in your data analyses.

Thank you for pointing this out. We use two different subdivisions of the active season for different analytical purposes. For the PCA, the active season was divided into three two-month periods (May–June, July–August, and September–October) to represent seasonal progression in the multivariate analysis. In contrast, the seasonal CH₄ response analysis (Fig. 9) uses two periods (May–July and August–October) to distinguish the rising and declining phases of the seasonal cycle and assess seasonal hysteresis. We have clarified this distinction and the rationale for the different groupings in the Methods.

Figure 7: Here you define nighttime vs. daytime based on PAR, while in l. 340-341 you use a definition based on the time of day. Please consider using being consistent with this definition throughout the manuscript.

Thank you for pointing this out. The two definitions are used for different analytical purposes. Clock time is used in the diurnal analysis to characterise flux variability over the 24-hour cycle, whereas PAR <10 $\mu\text{mol m}^{-2} \text{s}^{-1}$ is used for the nighttime NEE analysis to identify periods with negligible photosynthetic activity. A PAR-based definition is particularly appropriate during the active season because of the long and seasonally varying daylight hours. We have clarified the rationale for these definitions in the Methods.

Figure 9: Why did you chose to show correlations with soil temperatures at 5 cm depth instead of at deeper depths where you found stronger correlations at least at the fen?

Thank you for pointing this out. We agree that CH₄ showed a stronger overall relationship with soil temperature at greater depths, particularly at the fen. However, Fig. 9 was intended specifically to examine seasonal hysteresis in the CH₄–temperature relationship, following previous studies demonstrating differences between warming and cooling phases (Chang et al., 2020, 2021). We also examined the hysteresis using soil temperature at 35 cm, but the separation between the early- and late-season relationships was less pronounced than at 5 cm. We therefore retained the 5 cm soil temperature in Fig. 9 and have clarified the rationale for this choice in the Methods.

l. 443 maybe “variation” or “variability” instead of “excursions”? e.g.,

Has been changed accordingly.

l. 466, 467 In several instances you use words expressing a contrast such as “although”, “while”, “but” in sentences that present several but not necessarily contrasting observations. Consider checking the manuscript for this and replacing by “and” or similar.

Thank you for pointing this out. We have reviewed the manuscript for the use of contrastive conjunctions such as “although”, “while”, and “but”, and revised instances where the observations were complementary rather than contrasting.

l. 466 Mean active-season LUE “at the fen”...

“at the fen” has been added to text.

section 4.1: could fen-bog differences in cumulative sums of carbon fluxes change when considering the whole year instead of limiting studies to the active seasons? Considering your finding of higher seasonal variability at the fen, I think that considering the full year might at least reduce fen-bog differences unless you assume that photosynthesis is negligible outside of the active season. This might be a point worth discussing.

We agree that including the full year could affect the magnitude of the observed fen–bog differences. However, insufficient wintertime data coverage at the bog site prevented a reliable full-year comparison. We have therefore clarified that the cumulative flux estimates and the resulting fen–bog differences represent the active-season (May–October) rather than annual carbon balances. We have also added a discussion noting that previous studies have shown non-growing-season CO₂ and CH₄ fluxes to make a non-negligible contribution to annual peatland carbon budgets, including measurements from Siikaneva fen (Rinne et al., 2007) and bog (Alekseychik et al., 2021). Consequently, we emphasise that the active-season differences observed here cannot necessarily be extrapolated to annual fen–bog differences.

l. 516 To support the effect of plant-mediated CH₄ transport, you could refer to your own finding of the higher influence of LAlaer compared to LAltot instead of only referring to the literature.

Thank you for the suggestion. We have revised the Discussion to support the role of plant-mediated CH₄ transport with our own results, specifically the stronger relationship observed between CH₄ fluxes and LAlaer compared with LAltot.

l. 535 “less temporal variability” instead of “more stable temporal variability”?

Thank you for this suggestion, it has been changed according to the comment.

l. 541: What could be possible reasons for temperature sensitivity being more stable across depths at the bog? Could this be related to differences in water table depth or vegetation species composition/ rooting depths?

Thank you for the suggestion. We have expanded the Discussion to consider hydrology and vegetation structure as possible explanations for the more uniform temperature sensitivity across soil depths at the bog. We will discuss how WTD may affect the vertical distribution of methanogenic conditions and how differences in rooting characteristics and the abundance of aerenchymatous vegetation may influence the coupling between deeper peat processes and surface CH₄ emissions. These mechanisms are presented as possible explanations rather than effects directly tested in this study.

l. 575 Why would these special conditions in 2016 result in anomalous C fluxes only at the fen and not at the bog?

Thank you for raising this point. We agree that the previously suggested meteorological explanation alone does not explain why the anomalous carbon fluxes in 2016 occurred at the fen but not at the adjacent bog. We have therefore revised this discussion. The weak net CO₂ sink at the fen in 2016 was not associated with reduced productivity, as both GPP and TER were high, but rather reflected increased respiratory losses relative to photosynthetic uptake. Previous work has identified unusual spring CO₂ emissions at the fen in 2016, which may have contributed to the annual anomaly. However, because the bog experienced similar climatic

conditions without a comparable response, we now emphasise that the underlying cause of the ecosystem-specific response cannot be resolved from the present analysis and may reflect the greater sensitivity of fen carbon cycling to environmental variability.

l. 602 bog-like

Has been changed according to comment.