

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 2 comments:

This manuscript focuses on comparing adjacent bogs and fens ecosystems using eddy covariance towers between 2011 and 2016 to isolate the effects of vegetation, hydrology, and peatland type on carbon exchange (CO₂ and CH₄ fluxes). The authors found that, contrary to their original hypothesis, the fen was a stronger sink in terms of CO₂ but also emitted more CH₄ than the bog. These differences were mostly attributed to vegetation structure and phenology. The fen had higher LAI, gross photosynthetic uptake and light use efficiency compared to the bog, which led to higher net carbon uptake. At the same time, CH₄ emissions were greater in the fen due to plant mediated CH₄ transport. I think this study is very timely and interesting, both considering current environmental conditions, as well as future climate projections because it increases our understanding of the role of vegetation on ecosystem fluxes, particularly methane. Overall, I think this manuscript presents a valuable contribution to our understanding of peatland carbon cycling and is suitable for publication after minor revisions.

General comments:

Overall, the manuscript is both data and analysis rich, clear and well written, methods are described in a way that allows for replication, the figures are clear and show relevant information, and the analyses performed are adequate for the objectives presented in the paper. However, some of the results could be synthesized a bit better, and the main results could be highlighted more. Currently, all the figures make it a bit too information dense and hard to follow.

Introduction: Very good introduction. Encompasses the relevant information to understand the paper and highlights research gaps.

Methods:

- Figure 1 is good, but maybe you should consider overlapping the tower footprints. I think that would add important information. If not overlapping in the map, I still think you should add your tower footprints and energy balance closures in the supplementary information.

Thank you for this suggestion. We agree that showing the flux footprints would improve the spatial context of the comparison, and we are preparing a revised Figure 1 with the footprint contours for both towers overlaid on the map.

We also appreciate the suggestion to report energy balance closure as an additional diagnostic of the eddy covariance measurements. However, we have chosen not to include this analysis in the present study, which focuses on differences in CO₂ and CH₄ exchange between the fen and bog. The EBC closure has been reported in previous studies, namely Alekseychik et al. (2021) for the bog and Nicolini et al. (2026) for the fen.

- Not sure if it's necessary to add the VPD calculation. It is a fairly standard variable.

That is a very valid point, it has been removed from the text.

Results:

This section is, in general, very informative and well written. However, as I mentioned above, there is quite a lot of figures and repeated information that could be summarised a bit better.

- Figure 3 shows weekly fluxes for the whole study period, and 4 shows monthly diurnal courses, so the text in those sections have repeated information, perhaps you could consider removing the weekly section and just focus on diurnal, seasonal and annual variations across sites. Likewise, Figures 7, 9 and 10 could also be merged as they all show similar information.

Thank you for this suggestion. We will retain Figure 3 because it shows the chronological development of the fluxes throughout the study period, whereas Figure 4 illustrates their monthly diurnal patterns. We consider these complementary perspectives useful for understanding the differences between the sites. However, we have substantially shortened the text accompanying Figure 3 to reduce repetition and focus on the main temporal patterns.

We have also considered how best to consolidate Figures 7, 9 and 10 to reduce overlap, and will move Figure 9 to the supplementary material.

- The photosynthetic parameters could be in a table either in the main text or supplementary information, and then briefly compared in the text.

Thank you for this suggestion. We will present the photosynthetic parameters in a table and shorten the accompanying text to focus on the main differences between the sites.

- You mention linear mixed-effect models quite a few times, but don't provide the model structure or the parameters obtained from those, I think you could add these in the supplementary material as well and just refer to them in the main text.

Very good observation, thank you. 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 it in the main text.

Discussion:

- You state that 'the fen acted as a stronger CO₂ sink than the bog during the active-season.' However, the results show that the mean values are relatively similar and the standard deviations overlap considerably. It is therefore not clear whether the difference between ecosystems is statistically significant. I suggest clarifying this point and either presenting a statistical comparison or softening the wording in the Discussion and Conclusions.

Thank you for this comment. We have now compared the May–October cumulative CO₂ budgets using a two-sided paired t-test, with years paired between sites for five years at both sites (2012–2016). Although mean net CO₂ uptake was greater at the fen, the difference between sites was not statistically significant (mean fen-minus-bog difference: -10.9 g C m^{-2} per active season; 95% CI: -84.2 to 62.3 ; $p = 0.701$; $n = 5$ years). We will therefore revise the Discussion and Conclusions to describe the greater mean uptake at the fen as a numerical difference and clarify that it was not statistically significant across the five seasonal budgets.

However, separate analyses of paired daily CO₂ fluxes within individual years, accounting for temporal autocorrelation, indicated significantly greater net uptake at the fen during the available observations in 2012, 2013 and 2015. We will revise the Discussion and Conclusions to distinguish these within-year results from the absence of a statistically significant difference across the five seasonal budgets.

In contrast, the paired comparison of seasonal CH₄ budgets indicated significantly higher emissions at the fen (mean difference: 3.59 g C m⁻² per active season; 95% CI: 0.61–6.58; $p = 0.029$).

- Water table depth was generally higher at the fen compared to the bog, could this also explain the higher CH₄ emissions and higher CO₂ uptake rather than the vegetation structure alone? Given the extensive literature identifying water table depth as a primary control on peatland CO₂ and CH₄ exchange, I think the relatively limited role of WTD in this study deserves a more thorough discussion. In general, some questions that I think should be discussed are: Was the limited role of WTD due to the wet nature of both sites? Because only one logger was used at each site and EC measurements integrate large spatial heterogeneity? Because vegetation integrates long-term hydrological conditions better than instantaneous WTD?

Thank you for this helpful comment. We agree that the higher water table at the fen could contribute to the observed differences in CH₄ emissions and net CO₂ uptake, and that our discussion should more clearly acknowledge hydrological controls alongside vegetation structure. Our analyses did not identify a consistent relationship between WTD and the measured fluxes. However, this does not exclude a contribution of hydrology to the differences between the sites.

We will expand the discussion to consider the three explanations suggested by the reviewer. First, the generally wet conditions at both sites may have limited the expression of water-table controls over the observed range. Second, a single WTD logger at each site may not adequately represent the hydrological heterogeneity within the larger, temporally varying EC footprint. Third, vegetation composition may reflect longer-term hydrological conditions that are not fully captured by contemporaneous WTD measurements. These are plausible explanations rather than mechanisms demonstrated by our analyses.

We will also include supplementary figures examining the relationships 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.

- The manuscript attributes many of the observed differences to vegetation structure and phenology. While this interpretation is supported by the analyses presented, the two ecosystems also differ in nutrient status, peat chemistry, pH, and microtopography. A brief discussion acknowledging the potential influence of these co-varying factors would strengthen the interpretation.

Thank you for this valid and helpful comment. We agree that differences in nutrient status, peat chemistry, pH and microtopography may also contribute to the observed differences in CO₂ and CH₄ exchange. We have expanded the discussion to acknowledge these co-varying factors and clarify that, although our analyses support

the role of vegetation structure and phenology, their influence cannot be fully separated from other ecosystem characteristics in this observational comparison.

- Considering you have both CO₂ and CH₄ for both sites, instead of just analysing them separately, can you say something about the GHG budget in terms of CO₂ equivalents? It seems like the fen would have a higher emission in terms of CO₂-eq/radiative forcing compared to the bog due to the higher CH₄ and I think that should also be discussed. Moreover, I think the discussion on fen-to-bog transitions could be expanded as it was one of the main topics discussed in the introduction, but it is only slightly glossed over in the discussion.

Thank you for this helpful comment. We agree that the implications of the contrasting CO₂ and CH₄ fluxes warrant further discussion. The higher CH₄ emissions at the fen could offset some or all of its greater net CO₂ uptake when expressed in CO₂-equivalent units. However, the resulting balance would depend on the magnitude of both fluxes and the time horizon selected for the GWP calculation; higher CH₄ emissions alone do not establish the relative balance.

As discussed in our response to Reviewer 1, and drawing on Rinne et al. (2025), GWP-weighted contemporary fluxes should not be interpreted as the current climate forcing of established natural peatlands, for which their history of carbon accumulation and greenhouse-gas exchange is also relevant. Furthermore, our May–October measurements 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 their contrasting climatic implications and the limitations of inferring the relative climate effects of the two ecosystems.

We also agree that the discussion of fen-to-bog transitions should be expanded. We will discuss how associated changes in vegetation, hydrology and nutrient availability may influence both CO₂ uptake and CH₄ emissions. We will clarify that our comparison provides insight into differences between established fen and bog ecosystems, but does not directly quantify the trajectory or climatic consequences of a fen-to-bog transition.

Specific comments:

- Line 403: use ‘than’ instead of ‘then at the fen’

Thanks for pointing it out, it has been changed.

- Line 581: the PCA is figure 5, not 6

Very good observation, we have changed the figure reference in the text.

- There’s ‘active-season’ and ‘active season’ usage in the manuscript, choose one and standardise.

All incidences of active season have been changed to active-season.

- Figure 4 caption appears to have a missing letter? ‘Monthly diurnal courses ‘of’...

Has been fixed accordingly. Thank you for pointing it out.

- Figure 5 caption: "LAI data from the fen was only available", should be "were only available".

Has been fixed accordingly, thanks for pointing it out.

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