

The manuscript is well-structured and addresses the important topic of CO₂ and CH₄ emissions from managed organic soils, with clear implications for climate-smart forestry. Strengths include the semi-controlled setup to isolate water table effects and the integration of plant physiology with GHG fluxes.

However, clarification on the replication structure is needed (n=1 box per water level, split into veg/bare sections?) and extrapolation to annual fluxes (t C ha⁻¹ year⁻¹) from ~2.5 months of data is bold, especially with seasonal spikes like June CH₄ extremes. It would be good to present GHG fluxes as short-term rates per day or hour to avoid overgeneralization, and/or please provide details on the scaling method used, including your assumptions about non-measured periods.

Answer: Yes, this study does not include replicates for the different sections. We acknowledge this limitation, however, as this was a pilot study, we faced technical constraints. To partly address this, we introduced replication at other levels, for example by planting multiple (n = 5) poplars within the vegetated section, carrying out multiple rounds of GHG emission monitoring. We also agree that this limitation was not sufficiently acknowledged in the text. Therefore, we have added a statement at the end of the Discussion highlighting these constraints (Lines 468–472).

The presentation of GHG fluxes as short-term rates was also suggested by another reviewer. Accordingly, we revised the calculations and now present fluxes rate per day. We agree that the previous approach could lead to overgeneralization and overestimation of annual fluxes. Initially, we used that approach because it is widely applied in the literature and we aimed to ensure comparability with other studies. However, the revised approach is more appropriate for our data.

Specific comments:

Line 7: No need to capitalize “g” in “greenhouse”

Answer: Done (Line 8).

Line 11-15: Some of the info here is repeated, like no need to tell me twice that the experiment is in semi-controlled conditions

Answer: Corrected (Line 14).

Abstract: Some info on the effect of organic soil melioration on GHGs would be nice as this is the initial framing. Or, if the study doesn't provide answers to this question, focus on groundwater table as that seems to be the main study factor. Maybe also inform the reader in the abstract that they are going to learn about autotrophic vs heterotrophic respiration.

Answer: We included statement with a main take home message at the end of the abstract, summarizing that the results showed, that groundwater together with plant productivity and environmental condition influence GHG emissions (Lines 22-23). We also indicated in the middle of abstract that both autotrophic and heterotrophic respiration were determined (Line 16).

Line 76: I am convinced that the light intensity in the greenhouse was consistent with the region, but the argument about shading is less convincing. Shading was consistent with which conditions exactly? Field conditions? In young poplar forest, old poplar forest, peat area? Shrub zones, the treeline?

Answer: We agree that this lacks evidence and excluded shading from the text (Line 86)

Line 63: But no data on diurnal variability is presented in this study. Only arithmetic means per day, or day/night splits. This effectively erases any insights to diurnal variability, like duration of sunrise/sunset, how long during the day temperatures are at the optimum for given plant species, how quick temperature rises/falls etc. Maybe some diurnal plots could be added?

Answer. As in this research we also address the correlations with soil temperature, which is known to be a driver for soil emissions, we now included analyses depending on different air temperature intervals in both bare and plots with poplar. We chose these intervals according to the optimal temperature for poplar in our region (20–25°C DOI:10.3390/plants12051152), and we have also have observed in other studies that during daytime, with increasing temperatures, the photosynthetic activity slows down for poplar. In other studies, we conducted photosynthetic activity measurements in the morning, before temperature reaches too high levels (DOI:10.1101/438069).

We chose air temperature intervals during daytime, the active photosynthetic period, below optimum (<20°C), optimum (20-25°C), and above optimum (>25°C). We did, however, observe a tendency of increasing emissions depending on temperature, although the statistical analysis did not show any significant results. The main effect on poplar was seen in the later observation period for groups -25 and -35 cm (highest plant biomass), when the biomass exceeds its maximum, and at the highest temperatures, where it can be seen that sections with poplar emit more CO₂. This could be due to increased respiration processes in plants and soil and decreased photosynthesis. However, these data had high variability and also different sample sizes in each temperature interval, therefore the results should be interpreted with caution.

We included this section under 3.2 subsection in the results (Lines 317-330). We also included additional information in method section (Lines 223-225), and also talked more about this in discussion section (412-417).

Line 69: Does this mean ALL studied soils were organic soils? In that case, the study does not test the effect of organic content in soil, as no control exists for this factor. So please remove from the abstract.

Answer: Thank you for your question. Yes, all soils included in this study is organic, but we did not tested different organic contents in this study. Only mineral soil tested, was the control in the highest water table level, we now indicated this in method section (Lines 88-96, 108-109). We did not mention it earlier, because the scope of this study was not to see the differences between mineral and organic, we installed this control, just to be sure our system works properly and can distinguished the differences in GHG emissions form mineral and peat soils. We carried out soil chemical analyses only to see how they corelate with other our observed parameters, not to compare organic content in soil. We will remove plural form of 'Soils' in the title, as this manuscript includes only 1 soil (Line 1). Sorry if we are mistaken if this was the issue place, but we did not find a place in abstract where we mentioned soil organic content.

Line 86/117: Please clarify the replication structure: How many independent replicates for each soil organic matter content x water table x plant level?

Answer: Thank you for your suggestion, we now clarify that all water table levels except -2 cm was installed only in one repetition, the -2 cm was installed in two repetitions, because we create the control of two soils: mineral and peat, which we used to create the substrate used in this study (Lines 97-99). We did not include earlier this in to the study, because it was just to make sure that our created study design is able to detect the differences of the substare. It was effective, and now we also include this aspect in the methos section. We also added addition figure in appendix, however, we still do not think this should be added to result section main text, as it was not so much part of the experiment, as the more the way to calibrate study design usage (Lines 488-495).

Line 100: So, this study uses a single mixed substrate (peat + mineral soil from two depths, layered consistently across all boxes) to meet the >20% organic matter threshold for "organic soil."? It's thence not testing varying soil organic matter content as a factor, everything is presented by groundwater level only, and no significant differences in OM are analyzed or graphed. Please rewrite abstract and intro accordingly.

Answer: We have now included in the text a clarification that there are no differences in soil organic matter (OM) between the sections at the beginning of the study (Line 117). We

agree that this study does not test varying soil organic matter, but rather focuses on how groundwater level and vegetation influence the carbon cycle, which includes soil OM and emissions.

If the abstract was written in a way that could mislead the reader into thinking that our aim was to investigate differences in emissions related to varying OM, then we acknowledge that we may not have clearly communicated the main idea of the research. However, we are currently unable to identify a specific place in the abstract where such an interpretation might arise, and we would greatly appreciate it if you could indicate which part may be problematic.

At this stage, we have revised the abstract to reduce emphasis on organic soil as a varying factor and instead focus more clearly on high emissions from soils with high organic carbon content, which are primarily regulated by groundwater level (Lines 7-23).

Line 126: I do not know how often OPUS is updated, but it seems a bit strange to me to provide the date and time stamp of the version.

Answer: We excluded this (Line 144)

Line 133/134: Why express GHG as yearly fluxes when measurements cover much shorter periods? It would be more meaningful to present data per hour or per day if it must be.

Answer: We acknowledge this drawback and recalculated these values per day (Line 162).

137: every 5 mins. What happened between these periods?

Answer: We selected this period because the system used detects soil temperature with a precision no higher than 0.5 °C, therefore, it is unable to capture small changes occurring within this period. We also verified that one of the fastest detectable changes of 0.5 °C occurred over approximately 40 minutes. We also included accuracy in method section (Line 164).

Line 138-142: At what time of day/light intensity was chlorophyll fluorescence measured? Were the measurements standardized for time of day/light intensity?

Answer: We did not measure chlorophyll fluorescence, for observation of chlorophyll a and chlorophyll b we used nondestructive method using spectrophotometer (Lines 147-149). We acknowledge that this may be misleading, as in some parts we referred to Chl a and Chl b content, when it is really Chl content index, we will edit this throughout the manuscript.

We carried out samples with a near-infrared 'The SpectraVue' leaf spectrometer in the morning hours to avoid heat stress (Lines 173-174)

Line 214/15: Please rephrase

Answer: Edited (Line 254-255)

Line 222-25: Why is non-normality an “issue”?

Answer: We rephrased this part, we agree that this is not an issue, but just data distribution (Lines 264-265)

Line 233:34: what does the R² tell us here?

Answer: R² indicates how much variability is explained by the model. Although the higher R² value (R² = 0.37) is still not particularly high, the main focus was to demonstrate moderate (for -2 cm and -15 cm) variation in comparison to low (for -25 cm and -35 cm) variation. We also included this limitation aspect in the Results section (Lines 295-297).

Line 241: No need to capitalize “d” in “day”

Answer: Edited (Line 286)

Figure 5: After reading the abstract I was really expecting a figure showing GHG-emissions as a function of soil organic C content. Since this is not possible from the experimental design, maybe rephrase abstract/intro focusing on water table instead of OM content.

Answer: We revised our abstract to be focus more on groundwater rather than organic matter of soil (Lines 7-23). We also made some changes in introduction highlighting the focus on water table regulation (Lines 37-38, 40, 78)

Figure 4+6: I am not entirely convinced that we can assume linearity across the sparse measurements.

Answer: Given the relatively sparse measurements, strict linearity cannot be assumed. The regression lines are used to illustrate general temporal trends rather than to define a predictive relationship. Due to the high variability typical for physiological data under semi-controlled conditions, linear regression is applied here as a descriptive tool to highlight differences in temporal dynamics between treatments, which are not as clearly visible from summary statistics alone.

Although the R² values are moderate, the regression analysis indicates that responses at higher groundwater levels (-2 cm) are more structured compared to deeper drained conditions, where variability is higher and trends are less consistent. This supports the interpretation that temperature sensitivity is stronger under shallow groundwater conditions, even if the overall explanatory power of the model remains limited.

As in the previous comment, we have already included a statement in the Results section explaining the meaning of R^2 values in Figure 6; we will also add a short comment about Figure 4 in the paragraph where these results are explained (Lines 276–277).

Line 285: ... can decrease productivity at the young seedling growth stages investigated here. Would it be the same for more mature trees?

We agree, that the thresholds for this may differ depending on different stages of tree maturation, therefore we included this in discussion (Lines 352-354)

Line 292: Leaf? Leaves?

Answer: Edited (Line 360)

Line 312: initial?

Answer: Edited (Line 382)

Line 339: The proportion was not really measured but only inferred from parallel measurement of unplanted soil (where the microbial community may be entirely different). Therefore, I'd be in favor of a more careful interpretation. Do you have measurements to show that microbial biomass/community composition were not significantly different between planted and unplanted soils?

Answer: Thank you for your comment. We do not have measurements of microbial biomass and communities; the only reference is that we used identical substrates for both bare soil and poplar sections. Therefore, we know that at the beginning of the experiment the microbial communities in both sections were similar. We now include a part in the Discussion section (Lines 421–426) stating that changes in microbial communities during and at the end of the experiment were not controlled and may have occurred. However, we also highlight that, although this is clearly heterotrophic respiration, it is indirectly driven by plants and can be attributed to a plant effect. We also mention that this may lead to an overestimation of autotrophic respiration.

Line 376: Diurnal variability was not really presented in this manuscript?

Answer: We will now exclude the word “strongly” from this conclusion, as the results indicate that temporal variation (between periods) influenced this effect strongly, while the results from the diurnal analysis indicated only possible trends without significant values. Therefore, it cannot be described as a strong influence (Line 476).

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