Response to Referee #2 (Lars Elsgaard), 28 Oct 2024

We thank the reviewer for the constructive evaluation and in-depth review of our manuscript. All comments are one-by-one addressed below and will help us greatly to further improve our study.

Major comments

The study addresses a timely and important topic related to greenhouse gas (GHG) emissions from deep and shallow peat soils, where the latter typically represent soils with low organic carbon (OC) content, transitioning towards 'mineral' or 'peaty' soils due to long-term agricultural management. Such studies are generally lacking, and particularly so in the Baltic states, where this research provides the first estimates of emission factors for organic soils.

RESPONSE #1: *Thank you for the positive assessment of the topic's actuality and importance.*

The introduction is well-written, though a few sections may benefit from rephrasing for clarity. Relevant references are used, but some need verification against the reference list.

RESPONSE #2: *We rephrased several sections throughout the manuscript to improve clarity and avoid misleading the reader. The reference list was also checked and clarified.*

The study's significance lies in its coverage of 20 sites across Estonia, Latvia, and Lithuania, encompassing contrasting land use, water table conditions, and peat thickness. Measurements of ecosystem and soil heterotrophic respiration (CO_2 emissions) were performed over multiple years using closed chamber methods with gas chromatography (GC) analysis of CO_2 or portable gas analyzers. Supporting data on temperature and other physicochemical soil parameters are presented, along with estimates of annual carbon input to the soil from vegetation. Overall, it's a comprehensive setup, though there are major concerns the authors should address.

RESPONSE #3: *We are grateful for this generally positive assessment and are willing to do what we can to remedy the recognized issues. We have carefully considered all concerns presented, and corresponding responses are provided below.*

First, the estimates of cumulative annual CO_2 emissions appear overly simplistic and lack sufficient explanation. It seems that one (or two) measurement days are upscaled to a monthly total by simply multiplying by the number of days in the month. Why don't the authors take advantage of general upscaling using temperature as the main driver (as shown to be relevant for these data), for example, using continuous time series of soil (or air) temperatures to provide more accurate cumulative emission estimates? This is commonly done in studies using models like the Lloyd and Taylor model. Such upscaling would strengthen the cumulative data.

RESPONSE #4: *We originally chose this approach because continuous temperature data is not available for all sites. However, we can re-evaluate for which sites such data are directly available, and for which sites data from a nearby site could perhaps be used. We can further try creating regressions for estimating continuous temperature time series for the sites without continuous data*

based on the relationships between the manually measured temperatures and the corresponding temperature values from the continuous data of the closest site. In that way, we can provide alternative estimates for at least some sites, to facilitate comparison to the estimates obtained by the undeniably simplistic extrapolation. That will no doubt facilitate assessing the reliability of the estimates based on interpolation also for such sites for which continuous data are not available and cannot be generated. In any case, we will discuss this issue more rigorously in the revised manuscript.

Second, a tentative method of net CO_2 emissions is applied, based on cumulative Rhet fluxes and estimated inputs of carbon from vegetation. However, it should be considered that a portion of this carbon input is likely respired within the same year, in addition to the CO_2 from Rhet in the unvegetated plots. The authors should assess whether their estimates of carbon input are potentially overestimated.

***RESPONSE #5**: Thank you for noting this, as obviously, we had provided insufficient information in the manuscript. While the magnitude of the inputs was measured for some sites and estimated for other sites, the inputs were, in every case, added in the gas flux measurement locations similarly to the rest of the sites. Thus, the decomposition of the residues was included in the R_{eco} fluxes measured and used in the estimation of the annual net CO_2 emissions. This ensures that the carbon inputs are not overestimated or need an additional analysis concerning their decomposition rate. We will make sure that this information is easily available in the revised manuscript.*

Third, the issue of Rhet exceeding Reco weakens the results, and using a fixed factor to convert Reco to Rhet introduces significant uncertainty into the findings. While the data has merit and offers some interesting general conclusions, the authors should more clearly emphasize that their results for net fluxes are tentative and uncertain.

***RESPONSE #6**: We agree that the issue of R_{het} exceeding R_{eco} weakens the results. Still, that is something that we are not able to change. We had to try and find a way to reach our aims nevertheless, and we would like to emphasize that reported methodological challenges can be valuable for further studies as well. There are always tradeoffs related to data intensity when one is studying a small number of sites with lower representativeness for a region, and a high number of sites that cover more of the variations in relevant site conditions. Also, the used approach and reporting the specifics provide the possibility to improve the estimation by applying condition-specific factors (that could be obtained in further studies). Thus, we believe that the obtained results are valuable as the first region-level study estimating annual net soil CO_2 emissions from cropland and grassland on drained organic soils in the hemiboreal region. To avoid misleading the readers, we have addressed all the limitations both in the Discussion and Conclusion sections. We will further check, and revise where needed, all conclusive statements to emphasize that the net flux estimates are tentative and uncertain.*

Minor comments

RESPONSE #7: *Thank you for providing many suggestions including recommendations for technical corrections. We genuinely appreciate the time you have dedicated to helping us to make our article as readable and accessible as possible for the reader. We will address all of these suggestions (please see in the table below).*

Line	Comment	Response
15	… in the	*Corrected.*
25	Be specific on which measure of dispersion around the mean you are using (SE, SD, CI…)	*We clarified that "mean values ± S.E. are presented".*
33-34	No EEA 2023 a and b references shown in the reference list (so delete a). I have not continued cross-checking refences but strongly encourage authors to do so	*Corrected. Additionally, reference list was cross-checked.*
38	… croplands – delete s	*Corrected.*
60-63	Awkward to read – rephrase for clarity	*Rephrased as follows: "Relative to the number of the affecting factors and their potential interactions, as well as variation in management practices and intensity, there is still a rather limited number of sites that provide comprehensive information on the annual net CO_2 fluxes from drained organic soils used for agriculture."*
67-69	Rephrase for clarity	*Rephrased as follows: Yet, some studies have highlighted that also soils with comparatively low SOC concentration (<15.0 %, Tiemeyer et al., 2016), which do not fall under the definition of organic soils by the IPCC (Eggleston et al., 2006), may have high CO_2 emissions (Leiber-Sauheitl et al., 2014; Eickenscheidt et al., 2015; Liang et al., 2024).*
75	How can it correspond to 156% of the total?	*We clarified the text. In general, total net GHG emissions reflect the sum of GHG emissions and CO_2 removals (or difference between total GHG emissions and CO_2 removals, for instance, by living biomass). If part of the CO_2 emissions from organic soils are compensated by CO_2 removals in living biomass, CO_2 emissions from organic soils can account for > 100% of total net GHG emissions in the category.*

97-98	Not meaningful to give SE for these data – rather give SD	*We agree with the recommendation, corrected.*
104	… and 7.4	*Corrected.*
Table 1	Specify if WTL data are annual mean	*In Table 1, WTL data reflects study period mean. Specified as follows: "Mean soil water-table level ± S.E. (range) during the study period, cm below the surface".*
129	Specify what you exactly mean and define as the 'uncertainty' of the method	*We clarified as follows: "The expanded uncertainty (equal to two times the combined uncertainty) of the method was estimated to be 4.8 % (Magnusson et al., 2003)". Included reference additionally provide detailed description of calculation of expanded uncertainty of the method.*
134	Indicate how many fluxes where excluded	*Thank you for the suggestion, the subsection has been supplemented with information on how many fluxes were excluded.*
135	Rather state that it was when it was lower than 20 ppm	*Thank you for the suggestion, we corrected accordingly.*
Eq. 1	Indicate the unit of Reco	*The unit of the R_{eco} is provided just below the equation, where all the variables included in the equation are explained (including units).*
156	You don't present a clear argument for excluding the last 30 sec	*We supplemented the text with additional explanation: " To avoid possible mechanical disturbance (impact on CO_2 concentration changes over the time) due to chamber positioning, removing of and movement near the chamber, concentration values obtained during the first 15 and the last 30 seconds of the measurement period (180 seconds in total) were excluded from the regression (based on results obtained during the method validation)."*
173	Give diameter of the soil sample	*We clarified as follows: "... using a soil sample probe (diameter 5 cm) …".*
184	But it is not described that (and how) ash content (or LOI) is determined	*Method used to determine ash content is provided above: "...; ash content according to the LVS EN ISO 18122:2022; ..."*
188	How was VWC measured	*The subsection has been supplemented with information on devices used to measure soil temperature and moisture.*
189	Groundwater wells – is this piezometers?	*We clarified as follows: "... using groundwater wells (piezometer tubes, 5 cm in diameter, perforated and coated with nylon mesh) …".*

197	What is understood by 'soil surface respiration' – not a common term. Is it just soil respiration (Rhet + Rauto)? Be very clear on defining what you call soil surface respiration	*We clarified as follows: "...soil surface respiration (R_s) which reflects R_{het} and the dark respiration of the belowground plant biomass, ..."*
248-249	Use parentheses () for the i.e., sentence	*Corrected.*
249	Suggest not to show decimals for these numbers	*Corrected.*
251-252	Confusing with the '...up to...' Rather give absolute numbers that can be compared directly the values for deep organic soils	*Corrected (absolute numbers instead of "up to" are provided).*
Fig. 2	Make the 'a' and 'b' more visible (e.g., back instead of gray)	*Corrected (Figure 2, Figure S5, Figure S6).*
271	… a and b	*Corrected.*
Fig. 2	How is it possible to have BD og 2000 kg/m3 at a site with deep organic soil? I think data should be re-checked	*Thank you for your consideration. We found an error in data from two sites with deep organic soils (incorrect sample volume was used in calculations). This error has been corrected (Figure 2, Figure S5, Figure S6).*
282-284	This is far from significant (p = 0.69) and can not be claimed as a 'tendency'	*We agree with your objection. The text is rewording to avoid misleading.*
283	Delete 'respectively'	*Corrected.*
248-286	These can not at all be claimed to have 'a slight tendency of higher mean Reco'. Rephrase with respect for the statistical analysis.	*We agree with your objection. The text is rewording to avoid misleading.*
Fig. 3	It would be nice to have climate data to support this figure	*We prepared additional figures (Fig.S8, S9 and S10) supporting Fig. 3 with information on relevant environmental variables (water-table level below soil surface, air temperature and soil temperature at 10 cm depth).*
Fig. 4	Caption: mention if the CI is 95% CI (also Fig. 6)	*Clarified.*

Fig. 4	Specify whether it is annual mean WTL that is used?	*We clarified as follows: "... as a function (polynomial regression) of air temperature, soil temperature at 10 cm depth and water-table level measured during each gas sampling event".*
Table 3	No need to give both mean, median and range for these data (or move the Table to supplement)	*We wish all these parameters to be easily available for readers. The table was moved to the supplement (-> Table S5).*
Table 4	Caption: be specific and state explicitly that all Rhet data were calculated as 64% of Reco (not enough to refer back to section 2.8; the caption should be sufficient in itself)	*We clarified as follows: "Annual ecosystem respiration (R_{eco}), heterotrophic soil respiration (R_{het}) estimated from R_{eco} (64 % of annual R_{eco} as described in Sect. 2.8),..."*
403	OC – already defined	*Corrected.*
427	Rephrase – the limited number of studies don't 'explain' your results, rather makes them uncertain	*We rephrased as follows: "However, it could be related to the limited number of study sites with shallow-drained organic soils in grassland (n = 2), which increases uncertainty."*
445	… the hemiboreal	*Corrected.*
455	In our study…	*Corrected.*
468-469	But have you plotted the cumulative CO2 emissions against annual mean WTL? And are your WTL data corrected for whether the WT is in the peat layer or in a sand/mineral layer below the peat?	*We have plotted instantaneous ecosystem respiration (R_{eco}) in cropland and grassland as a function (polynomial regression) of water-table level measured during each gas sampling event (Figure 4). Based on your comment, we additionally added figures reflecting the relationship between annual R_{eco} and annual net soil CO_2 emissions and mean water-table level in study sites in cropland and grassland in supplementary material (Figure S13 and Figure S14). However, no significant correlations/relationships were found. WTL data was not corrected for whether the WT is in the peat layer or in a sand/mineral layer below the peat.*
470-473	Rephrase for clarity	*To improve clarity, we rephrased and supplemented the paragraph that discusses the CO_2 emission response to water-table level.*
473-474	Rephrase – last part of the sentence is not clear	*To improve clarity, we rephrased and supplemented the paragraph that discusses the CO_2 emission response to water-table level.*

477-479	Unclear writing. Note that linear relationships are presented by Evans et al. (2021) whereas asymptotic relations are presented by Tiemeyer et al. (2020) and Koch et al. (2023). Tiemeyer, B. et al. A new methodology for organic soils in national greenhouse gas inventories: Data synthesis, derivation and application. Ecol. Indic. 109, 105838 (2020). Koch, J. et al. Water-table-driven greenhouse gas emission estimates guide peatland restoration at national scale. Biogeosciences 20, 2387-2403 (2023).	*Thank you for the suggestion. To improve clarity, we rephrased and supplemented the paragraph that discusses the CO_2 emission response to water-table level. In addition, we supplemented the paragraph with the latest findings of asymptotic relations. It complemented the paragraph well.*
500	Nuances? Or should it rather be presented as problems/challenges	*Thank you for the suggestion, we changed as follows: "The observed inconsistency is most likely explained by methodological challenges".*
512-515	Unclear writing - rephrase	*We rephrased as follows: "This "additional" CO_2 flux should logically be at its highest during late summer, when the plants are fully developed. However, the share of aboveground autotrophic respiration in ecosystem respiration in cropland or grassland has rarely been reported, and the published results vary widely and have relatively large uncertainties. Consequently, we were not able to estimate how much it contributed in our sites."*