

Review of Bārdule et al.: “Annual net CO₂ fluxes from drained organic soils used for agriculture in the hemiboreal region of Europe”. <https://doi.org/10.5194/egusphere-2024-2523> Preprint.

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.

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. 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 (CO₂ emissions) were performed over multiple years using closed chamber methods with gas chromatography (GC) analysis of CO₂ 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.

First, the estimates of cumulative annual CO₂ 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.

Second, a tentative method of net CO₂ 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 CO₂ from Rhet in the unvegetated plots. The authors should assess whether their estimates of carbon input are potentially overestimated.

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.

Minor comments

LINE	COMMENT
15	... in <u>the</u>
25	Be specific on which measure of dispersion around the mean you are using (SE, SD, CI...)
33-34	No EEA 2023 a and b references shown in the reference list (so delete a). I have not continued cross-checking references but strongly encourage authors to do so
38	... croplands – delete s

60-63	Awkward to read – rephrase for clarity
67-69	Rephrase for clarity
75	How can it correspond to 156% of the total ?
97-98	Not meaningful to give SE for these data – rather give SD
104	... <u>and</u> 7.4
Table 1	Specify if WTL data are annual mean
129	Specify what you exactly mean and define as the ‘uncertainty’ of the method
134	Indicate how many fluxes where excluded
135	Rather state that it was when it was lower than 20 ppm
Eq. 1	Indicate the unit of Reco
156	You don’t present a clear argument for excluding the last 30 sec
173	Give diameter of the soil sample
184	But it is not described that (and how) ash content (or LOI) is determined
188	How was VWC measured
189	Groundwater wells – is this piezometers?
197	What is understood by ‘soil surface respiration’ – not a common term. Is it just soil respiration ($R_{het} + R_{auto}$)? Be very clear on defining what you call soil surface respiration
248-249	Use parentheses () for the i.e., sentence
249	Suggest not to show decimals for these numbers
251-252	Confusing with the ‘...up to...’ Rather give absolute numbers that can be compared directly the values for deep organic soils
Fig. 2	Make the ‘a’ and ‘b’ more visible (e.g., back instead of gray)
271	... a and b
Fig. 2	How is it possible to have BD og 2000 kg/m ³ at a site with deep organic soil? I think data should be re-checked
282-284	This is far from significant ($p = 0.69$) and can not be claimed as a ‘tendency’
283	Delete ‘respectively’
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.
Fig. 3	It would be nice to have climate data to support this figure
Fig. 4	Caption: mention if the CI is 95% CI (also Fig. 6)
Fig. 4	Specify whether it is annual mean WTL that is used?
Table 3	No need to give both mean, median and range for these data (or move the Table to supplement)

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)
403	OC – already defined
427	Rephrase – the limited number of studies don't 'explain' your results, rather makes them uncertain
445	... <u>the</u> hemiboreal
455	<u>In</u> our study...
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?
470-473	Rephrase for clarity
473-474	Rephrase – last part of the sentence is not clear
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. <i>Ecol. Indic.</i> 109 , 105838 (2020). Koch, J. et al. Water-table-driven greenhouse gas emission estimates guide peatland restoration at national scale. <i>Biogeosciences</i> 20 , 2387-2403 (2023).
500	Nuances? Or should it rather be presented as problems/challenges
512-515	Unclear writing - rephrase