Response to Anonymous Referee #1, 25 Oct 2024

We thank the reviewer for the evaluation of our manuscript. All comments are one-by-one addressed below and helped us greatly to improve our study.

I don't think you can compare the sites; they are very different. If you compare different treatments like grass/cereal, it should be on the same field in a regular field trial. In this paper, you don't know how all other variables affect emissions and/or yield.

RESPONSE #1: *Thank you for the comment. However, we are not quite sure if we understand what is meant here. Our aim was not to compare specific cropland and grassland sites with each other. Instead, we aimed to produce the first estimates on annual net CO_2 fluxes from drained organic soils in two different types of agricultural land (cropland and grassland) typical of the region (the Baltic states), and to elaborate corresponding CO_2 emission factors for this region. For this purpose, we need sites that represent the variation in such sites in the region, not sites that are comparable concerning some specific characteristics. We believe that the number of studied sites and monitoring over a 2-year period is sufficient to characterize the variation in net CO_2 emission at regional level.*

The method to measure Reco is different from Rh. Reco is probably underestimated due to the long time the chamber is over the soils compared with the Rh measurement.

RESPONSE #2:

Yes, the method to measure R_{eco} was different from R_{het}. That is to be expected because the measurements cover different conditions, one with plants and the other just the soil. The chamber methods should be designed accordingly (e.g., Pavelka et al. 2018) so that they match with the conditions. Data from the two methods were handled separately. Also, specific data quality controls were applied in both methods before readout acceptance, as described in the methods section. Both methods have been widely used in flux data collection also in earlier research (e.g., Maier et al. 2022; Ryhti et al. 2021; Barbosa et al. 2024). The validity of these methods and the reporting based on their parallel use has, to our knowledge, not been generally challenged. We agree that static chamber methods used for R_{eco} require careful consideration of the concentration gradient over time. To avoid potential risks that a build-up of CO_2 in the sealed chamber space reduced diffusion-driven soil CO_2 efflux, we conducted a thorough validation of the method, including comprehensive comparability tests of the techniques employed, prior to field measurements. These tests included an evaluation of the linearity of CO_2 concentration increase within the chambers. Simultaneously, we measured CO_2 concentration continuously using an EGM (employed in heterotrophic respiration measurements) and collected manual gas samples for gas chromatography, following the study procedure. Our analysis did not reveal any evidence of disrupted linearity, leading us to conclude that pressure build-up did not introduce bias. Noteworthily, the size of the chambers used for our R_{eco} measurements was large (a volume of 0.0655 m^3 and an area of 0.1995 m^2), which evidently prevented the formation of nonlinearity. Our team includes scientists with thorough experience in measuring greenhouse gas fluxes using different chamber techniques. As part of our quality control procedures, we included steps such as examining the raw data for linear versus non-linear patterns over time. Thus, the potential risks mentioned (compatibility of methods, reduced diffusion-driven soil CO_2 efflux) were already initially assessed, and avoided.

Pavelka et al. 2018: https://doi.org/10.1515/intag-2017-0045
Maier et al. 2022: https://doi.org/10.1002/jpln.202200199
Ryhti et al. 2021: https://doi.org/10.1016/j.agrformet.2020.108266
Barbosa et al. 2024: https://doi.org/10.1016/j.geoderma.2024.116891

If there is no significant difference, there are no differences; remove the use of tendency...

RESPONSE #3: *We rephrased the text based on the results of the statistical analysis to avoid misleading the readers regarding tendencies.*

I think using one factor to convert to Rh for all different soils is problematic.

RESPONSE #4: *We agree, but could not find a better way to come around the fact that our R_{het} measurements yielded overestimates that we could not apply (see also RESPONSE #2). We are aware that the share of R_{het} in R_{eco} can vary not only between study sites, but also between different seasons and vegetation types. The assumption that the proportion of annual soil R_{het} from R_{eco} is 64%, is based on results of a large number of previous studies (n=61, Fig. S4) conducted in temperate and boreal regions (Jian et al., 2021). These assumptions were consistent with the most conservative approach and should clearly avoid underestimation of R_{het} since our R_{eco} values additionally included the dark respiration of the aboveground plant biomass, not included in the soil surface respiration (R_s). Although the use of static factor introduces some uncertainty (Figure S4), the used approach is based on the best available and most comprehensive data. This was the best option that we could think of, but if there are in fact better ways for estimating R_{het} based on R_{eco} in grassland and cropland, we would be delighted to learn and apply such. 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 trust that obtained results are valuable, serving as the first region-level study to estimate annual net soil CO_2 emissions from cropland and grassland on drained organic soils in the hemiboreal region.*

Jian et al., 2021: https://doi.org/doi.org/10.3334/ORNLDAAC/1827

Comments in the pdf file

Line 120-123: This will underestimate the emissions. too long time will reduce the gradient in the chamber. Thats why Rh is larger than Reco.

RESPONSE #5: *Please see RESPONSE #2. We understand that the concern is likely related to potential CO_2 saturation during longish closed chamber incubation on the vegetation covered plots. As described above, we applied rigorous quality control on the data. As part of the quality control, linear increase of the CO_2 concentration over time was checked and there were only a few events where linearity was not optimal, leading to discarded readout. The potential risks of reduced diffusion-driven soil CO_2 efflux were already initially assessed and avoided.*

Line 153-155: The different methods measuring heterotrophic and ecosystem respiration is a big problem.

RESPONSE #6: *Please see RESPONSE #2. We do not see how it would be beneficial, or even feasible, to use the exact same method under clearly different conditions. The key point should be whether the different methods yield reliable estimates for the conditions for which they were designed and under which they were applied. As far as we know, the applicability of either method as such has not been challenged generally.*

Line 166: This is very shallow, surely the roots will go much beyon that?

RESPONSE #7: *We agree that the maximum rooting depth in grasslands and croplands can extend well beyond the top 20–30 cm. However, we focused on the top 30-cm soil layer to capture the most densely rooted zone and its microbially active rhizosphere, the processes of which are driving the CO_2 emissions. Designing the study, we also considered the fluctuations of the water table, and although it varied widely, being lower than 1 m in some sites and time periods, we had to consider a comparable rooting depth. However, we fully agree that deeper layers or rooting depth per site should be studied in the future.*

Line: 197: But you dont really know. This can be very differente between sites.

RESPONSE #8: *Please see RESPONSE #4. We agree, and regret that this could not be considered. This is also clearly acknowledged in the Discussion. We still think that our results are better representative of the conditions in the region considered than the existing data that are still overall rather scarce and not including any sites from the region considered. Future research can build on our findings and yield more accurate estimates, if and when resources allow the work to be continued. Probably no scientific paper so far has yielded an absolute truth concerning emissions from drained organic soils, which are an outcome of several contributing and constraining factors, but they may all be considered as necessary steps towards reliable generalizations.*

Line 202: Probably over estimation since you measure during peak temperature.

RESPONSE #9: *We fully agree. To estimate potential overestimation of annual R_{eco} due to measurements conducted only during daytime, when the temperature usually is higher than daily average temperature, a study site-specific comparison of the applied method and modelling approach based on continuous soil temperature measurements at depths of 10 cm at 10 study sites was made. Modelling approach included constructing of study-site-specific models describe relationships between logarithmically or Box-Cox transformed instantaneous R_{eco} (applied for data normalization, Box and Cox, 1964) and soil temperature at 10 cm depth (Fig. S5, Fig. S6). Hourly R_{eco} estimates were calculated by using the constructed equations (Fig. S5, Fig. S6) and continuous soil temperature measurements at depths of 10 cm for each study site where continuous soil temperature measurements were conducted (10 study sites in total). Consecutively, site-specific annual R_{eco} estimate was calculated by summing the hourly emission estimates of the year. To avoid misleading readers, we have emphasized this several times in the article (in both the discussion and conclusions section): "Based on the comparative analysis done in ten sites our annual R_{eco} estimates were overestimated by a mean of 9 % as the flux measurements were all done in daytime (Fig. S16). Previous studies have concluded that the mean CO_2 flux during the daytime is 14–23 % higher than the mean daily fluxes (Maljanen et al., 2002). This is largely caused by diurnal variation in air temperature and consequently soil temperature, which are intercorrelated variables. Thus, a regression describing variation in R_{eco} depending on soil temperature could be used for further evaluations to avoid overestimation of R_{eco}*

due to lack of measurements during the nighttime. We did not revise our estimates as the comparison could only be done in ten sites."

Box, G.E.P. and Cox, D.R.: An analysis of transformations, J. R. Stat. Soc. Ser. B, 26, 211–243, https://doi.org/10.1111/j.2517-6161.1964.tb00553.x, 1964.

Line 265: You must describe what is measured in the figure text

RESPONSE #10: *Thank you for noting the lack of some necessary information. While we think that generally, "topsoil characteristics" can be used, explanations of the abbreviated variables OC and TN were lacking, and we have added the corresponding description: "Variation in topsoil (0–20 cm soil layer) characteristics (organic carbon (OC), total nitrogen (TN), organic carbon/total nitrogen (C/N) ratio, HNO_3-extractable potassium (K), calcium (Ca), magnesium (Mg) and phosphorus (P) concentration, soil bulk density, soil pH) in the cropland and grassland sites, separately for the two soil types (deep organic soil and shallow highly decomposed organic soil)."*

Line 284: No, not different

RESPONSE #11: *We agree. The text is reworded to avoid misleading.*

Line 320: The applied funktions does not seem to fit the data.

RESPONSE #12: *We tested different types of regression curves and found that polynomial regression provided the best fit of all options evaluated.*

Line 409: cant do. the soils will be too different.

RESPONSE #13: *Please see RESPONSE #1. The soils with all their variability represent the conditions in croplands and grasslands on current or former peatlands in the region, and thus form a good, and the only feasible, basis for estimating emission factors for them. To avoid confusion, we rephrased the text.*

Line 500: probably due to the different gas measurements methods

RESPONSE #14: *Initial testing and validation of the methods were performed in order to avoid risks of method incompatibility (please see RESPONSE #2). The observed inconsistency that the magnitude of instantaneous R_{het} tended to exceed the R_{eco} is most likely explained by other methodological challenges, as described in Discussion. Measurement points established for R_{het} involved trenching, vegetation removal, and keeping the soil surface bare. This may elevate the R_{het} as compared to vegetated surface firstly by higher temperature in bare soil than under vegetation. Second, soil moisture conditions may differ from vegetated soil. Third, CO_2 from the decomposition of roots killed by the trenching is likely to further add to R_{het}. These challenges have been encountered in earlier studies as well, but their net effect was higher in our study than what we expected. We think that it is worthwhile to fully address this to inform future research so that a similar outcome can be avoided. If only nice success stories are reported in scientific papers, we learn much less than what we could, to the disadvantage of the whole scientific community.*

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 helped us greatly to 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 have done 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: *Thank you for noting this. We originally chose this approach because continuous temperature data is not available for all sites. Based on recommendation, to estimate potential overestimation of our annual R_{eco} due to measurements conducted only during daytime, when the temperature usually is higher than daily average temperature, a study-site-specific comparison of the applied method and modelling approach based on continuous soil temperature measurements at depths of 10 cm at 10 study sites was made. Modelling approach included constructing study-site-specific models to describe relationships between logarithmically or Box-Cox transformed instantaneous R_{eco} (applied for*

data normalization, Box and Cox, 1964) and soil temperature at 10 cm depth (Fig. S5, Fig. S6). Hourly R_{eco} estimates were calculated by using the constructed equations (Fig. S5, Fig. S6) and continuous soil temperature measurements at depths of 10 cm for each study site where continuous soil temperature measurements were conducted (10 study sites in total). Consecutively, site-specific annual R_{eco} estimate was calculated by summing the hourly emission estimates of the year. To avoid misleading readers, we have emphasized this several times in the article (in both the discussion and conclusions section): "Based on the comparative analysis done in ten sites our annual R_{eco} estimates were overestimated by a mean of 9 % as the flux measurements were all done in daytime (Fig. S16). Previous studies have concluded that the mean CO_2 flux during the daytime is 14–23 % higher than the mean daily fluxes (Maljanen et al., 2002). This is largely caused by diurnal variation in air temperature and consequently soil temperature, which are intercorrelated variables. Thus, a regression describing variation in R_{eco} depending on soil temperature could be used for further evaluations to avoid overestimation of R_{eco} due to lack of measurements during the nighttime. We did not revise our estimates as the comparison could only be done in ten sites."

Box, G.E.P. and Cox, D.R.: An analysis of transformations, J. R. Stat. Soc. Ser. B, 26, 211–243, https://doi.org/10.1111/j.2517-6161.1964.tb00553.x, 1964.

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 made sure that this information is easily available in the revised manuscript (additionally highlighted in the subsections 2.2 "Measurements of ecosystem respiration" and 2.7 "Estimation of annual soil net CO_2 fluxes and CO_2 emission factors").*

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.

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 have addressed 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 ± SE 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 variables and their interactions, as well as variation in management practices and intensity, there is still a rather limited number of studies 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 ± SE (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: "In total, 6.5% of all instantaneous R_{eco} results were excluded from further data processing based on data quality check".*
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 the impacts of mechanical disturbances (chamber placement and removal, movement near the chamber) the concentration values recorded during the first 15 and the last 30 seconds of the 180-second measurement period were excluded from the regression, based on results of 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, 5-7.5 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 includes 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 S7, Figure S8).*
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 S7, Figure S8).*
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.S10, S11 and S12) 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 (Fig.4, Fig. S13): "... 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. 2.7),... "*
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: "It should be noted, however, that sites with shallow highly decomposed organic soil were relatively less represented in the study."*
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 S13). 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 S17 and Figure S18). 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 (Phillips et al., 2017). Consequently, we could not estimate how much it contributed to our ecosystem flux."*