

We would like to thank the editor for taking the time to assess the work presented in this manuscript. In response to the feedback and suggestions provided we have revised the manuscript as recommended by both reviewers and the editor. We have edited the structure of the manuscript and its content to define the potential of paludiculture in heterogeneous rewetted peatlands as the main finding. In the revised manuscript the modelling work supports findings on heterogeneity by highlighting the importance of considering water table depth fluctuations during the rewetting process. No further discussion and analysis on the modelling component were included in the revised manuscript in order to keep the focus on heterogeneity and have a clear storyline. Further discussion on drivers such as water table depth, nutrients and heterogeneity was included in sections 4.1.3, 4.2, and 4.4 and supported by additional references.

In the text below we present the updated detailed answers to the comments from the reviewers referencing also the specific lines and manuscript sections where changes were implemented when applicable. We indicate lines and sections in the revised manuscript which is the manuscript with track changes we are also submitting. Reviewer comments are in *italic* followed by our answers in plain text.

We would like to thank referee # 1 for taking the time to review our manuscript. We appreciate the feedback, input, and thoughtful comments. We worked to address each comment and implemented the suggestions. The text below includes the reviewer's comments in *italics* followed by our answer in plain text.

***Structure and Clarity of Major Findings:***

*The manuscript would benefit from a clear outline of its key findings. Since no significant treatment effects on GHG emissions were detected, the focus shifts to the modeling improvements. However, the practical significance of incorporating hourly soil temperature measurements could be discussed more thoroughly. For instance, exploring intermediate temporal resolutions, such as bi-weekly data (aligned with the sampling interval), might provide a more balanced view of the model's utility and feasibility. Also, this shift in focus during the manuscript often makes it difficult to follow. The manuscript may be clearer if focused on one topic, the modelling or the treatment aspect. One does not exclude the next, but should rather support the other instead of being seemingly separate and only partially connected stories.*

Answer: The results section of the manuscript was edited to follow a more clear structure presenting the findings which aligns with the objectives and the discussion. In the structure of the revised manuscript, results from the C budget connect to the heterogeneity of the rewetted peatland under paludiculture which is addressed by using the data on water table dynamics and pore water chemistry through the modeling approaches. This will help to bring up the finding on the potential of paludiculture applied in nutrient rich areas, which both reviewers agree should be raised as a major finding in the manuscript. We would like to point out that including WTD data at increasingly higher temporal resolution improves the accuracy of the models, however, the most relevant comparison of models performed in this study is between the hourly data and the mean annual data because current emission factors for rewetted peatlands are established based on mean annual WTD, which according to our findings can

underestimate  $R_{eco}$  in these peatlands. Additional intermediate temporal resolutions were not included in revised manuscript in order to not divert into further exploration of models and keep the focus on the main finding.

### ***Sampling Design and Representativeness:***

*The study's sampling design raises some concerns regarding its ability to address the research questions. With only four plots and three treatments (control and two fertilized/harvested treatments), there are limited replicates ( $n=4$ ) in what appears to be a highly heterogeneous environment. This complicates statistical analyses. The PCA results suggest significant variation among plots based on nutrient levels and water table depth, possibly impacting the patterns strongly. A seasonal or event-based analysis might help uncover more nuanced patterns and drivers. Instead of focussing on annual budgets drivers of GHG emissions and NEE could be explored throughout the whole sampling period. Additionally, it would be helpful to clarify how well control and fertilized plots were spatially separated to avoid fertilizer spillover effects.*

Answer: When the experiment was designed, we did not expect this large heterogeneity among the replicate blocks because the fields seemed uniform and without differences in topography. We thought that for the study area, four blocks were a sufficient number of replicates to conduct the study. Retrospectively, we agree with the reviewer that more replicate blocks would have strengthened the statistical analyses. The PCA, correlations, and the linear mixed model analyses were conducted as event-based analyzes, because in them we included the  $R_{eco}$  and GPP obtained at each measurement campaign and we relate them to potential drivers (WTD, RVI, PAR, and soil temperature), as well as the pore water nutrients obtained at each measurement campaign. In these analyses we did not use annual averages. The effect of WTD, nutrients, and heterogeneity is now further discussed sections 4.1.3, 4.2, and 4.4 in the revised manuscript. Following the recommendation of the reviewer we have included the number of samples used in these analyses. We conducted statistical comparisons for the treatments and blocks based on annual budgets because emissions are normally evaluated and quantified in terms of annual budgets, and this makes the results presented in this manuscript more relatable to other studies. Spillovers were avoided by performing measurements within 55 x 55 cm collars located in the middle of the 3 m wide treatment plots. Thus, fringes of 1.45 m on each side of the area of gas measurements were present with the same treatment.

### ***Long-Term Context and Ecosystem Carbon Balance:***

*The manuscript briefly mentions long-term changes in GHG emissions and net ecosystem carbon balance (NECB), but this aspect is not explored in detail. Comparing emissions and NECB to the baseline conditions at the start of the experiment (3 years ago) could add significant insight into GHG emission development. Pursuing this analysis would provide a deeper understanding of how management practices influence peatland carbon dynamics over time. Especially as there may be a delayed effect on carbon balances (as the authors also point out) of rewetting and implementing paludiculture. Alternatively, the authors could focus on the fact that RVI, water table and soil temperature were more important for CO<sub>2</sub> emissions than the treatments, correct?*

Answer: A comparison to baseline conditions would be useful, however, those baseline measurements were not taken, and therefore we are not able to perform that comparison. We refer to differences in NECB during the third production year after reed canary grass establishment. Even though we could not compare our results to baseline conditions, we compared them to results from 2020 to 2021 presented in Nielsen et al (2024), and in doing so we analyzed trends in C emissions as rewetting progressed at the study site. We explored the importance of WTD, RVI, soil temperature, and PAR in  $R_{eco}$  and GPP in figure 5 (figure 7 in revised manuscript), in which we see the variability of fitted parameters corresponding to WTD, RVI, soil temperature, and PAR as they fit to the  $R_{eco}$  and GPP models of each of the plots

### ***Abstract Accuracy:***

*The abstract states that "biomass harvest reduces GHG emissions from productive rewetted peatland areas in comparison with no management." However, this conclusion does not appear to align with the results, as no significant treatment effects on GHG emissions were observed. For example, line 447 notes that there was "similar NECB for all treatments." Revising this statement for consistency with the findings would improve the abstract's accuracy and alignment with the study's outcomes. In general, often marginal significances or non-significant trends are stated and discussed rather than the consequences of non-significant differences between treatments.*

Answer: Abstract was revised and edited for accuracy. Even though no significant differences between management strategies were found on NECB, we found marginally significant differences in GPP, as well as statistical differences in CO<sub>2</sub> emissions and pore water nutrients between blocks. Based on these differences, we state that management "might potentially reduce GHG emissions from the more productive rewetted peatland areas in comparison with no management, whereas on the less productive areas it might be beneficial to leave the biomass unmanaged." on the revised abstract.

### ***Statistical Details and Manuscript Quality:***

*Including sample sizes and test statistics throughout the manuscript would enhance clarity. The text size in all figures would need to be increased and some of the captions clarified. Additionally, improving the precision and clarity of the text would help communicate the findings more effectively. There are many small errors and unclear statements. A careful and thorough revision would be needed to address these issues and improve the overall readability (see specific comments).*

Answer: Manuscript was revised and edited accordingly. Specific comments regarding statistical information to be added and figures were addressed and incorporated in revised version.

### **Specific comments**

*L19: What were these treatments? A short explanation would help to better follow the results*

Answer: a short explanation was included (lines 21 to 23 revised manuscript).

*L22: Please explain abbreviation when used for the first time*

Answer: change implemented.

*L25 – 26: Yes this makes sense, but is probably not a major finding of the study major finding – please revise to say something more to the effect of “ The use of high temporal resolution water table depth facilitated capture ecosystem respiration (Reco) peaks ”*

Answer: change was implemented.

*L28 -30 : Please highlight the general direction of the differences between the 2 blocks – particularly for CO2 emissions. Was this motioned in the results section?*

Answer: further explanation was included in the abstract. Differences between blocks in CO<sub>2</sub> emissions are mentioned in the results section 3.2 (section 3.1 of the revised manuscript) in the second paragraph, while differences between blocks in pore water nutrients are mentioned in section 3.4 in the second paragraph.

*L32 – 33: This is likely due to a low sample number that limited ability to detect significant differences. Also very different or highly variable annual climate can cause this. High levels of Natural heterogeneity suggest that there is indeed no significant difference to be expected – is this the message ? Else, please suggest reasons why “expected” differences were not found (eg. low sample N, or need for longer measurement time possibly precluded your ability to detect a difference).*

Answer: We agree with the reviewer in the sense that more block replicates would most likely have resulted in a more significant treatment effect. However, as stated in the answer to the general comments above, we decided to use four block replicates based on the observation of the uniformity and similar topography of the block replicates. A previous study by Nielsen et al. (2024), which is also referenced in our manuscript, also found no significant differences in NECB between the same three management strategies at the study site for the 2020 to 2021 period, which possibly suggest that a longer study period would still indicate no differences between management strategies.

*L33 -35 : To me, this is your main finding, and a relevant one ! Please consider using this as a major finding as little literature specifically focuses on this aspect. Also please support this better in the results and discussion if possible (see comment above)?*

Answer: In the revised manuscript this is now highlighted as the major finding. Further edits in the structure of the results and discussion sections on the revised manuscript also highlight it as the major finding.

*L48-49 : What about drainage effects on N<sub>2</sub>O (especially since you mention leaching of N in the same sentence)?*

Answer: Peat drainage may also lead to increased N<sub>2</sub>O emissions by releasing N bound to C in peat soils. In these sentences in the introduction though, we try to keep the focus on what we measure in our study such as CO<sub>2</sub>, DOC, DON, and thereby be concise in the introduction section. However, in response to a comment from reviewer 2, we have included a discussion

on the implications of N<sub>2</sub>O emissions, which were not measured in this study, in section 4.5 in the revised manuscript.

*L50 – 54: What about N<sub>2</sub>O emissions ? And more importantly, what about increased CH<sub>4</sub> emissions upon increasing the water table ?*

Answer: In this sentence we want to relate peatland drainage to emissions. We then focus on the relation between drainage, WTD, and CO<sub>2</sub>. Since we focus on CO<sub>2</sub> emissions in this study, we would like to stay within the topic. We agree that N<sub>2</sub>O emissions can be significant in rewetting peatlands, but this is not the focus of this study. Since we also report CH<sub>4</sub> emissions, we included an explanation of the trade-off between CO<sub>2</sub>, and CH<sub>4</sub> with changing WTD in the introduction (lines 73 to 79 revised manuscript).

*L56 – 57: Peat degradation as a result of draining causes this, not the act of draining itself. Please provide a bit more details and specificity, or state there are relationships between degree/length of drainage and higher bulk density etc*

Answer: We agree, sentences were edited to state that peat degradation is the cause of these changes and not drainage itself (line 82 revised manuscript).

*L58-59: I would expect a degradation of the humic compounds (due to microbial degradation of the organic matter) upon increasing oxygen due to drainage. Can you cite this?*

Answer: The reviewer is correct. We have corrected these sentences regarding a decrease in humic substances and polyphenols upon peat degradation. Sentences were then linked to the next sentence to add references (lines 84 to 86 revised manuscript).

*L65 -67 – consider moving up this statement or moving down the previous (L50-54)*

Answer: Sentences were reorganized as suggested: Sentences on the trade-off between CO<sub>2</sub> and CH<sub>4</sub> were moved to the previous paragraph (lines 73 to 79 revised manuscript), and additionally, sentences on the interest on peatland restoration were moved to the next paragraph (lines 99 to 101 revised manuscript).

*L 73-75: Can the clarity of this sentence be improved?*

Answer: Sentence was rephased for clarity (lines 103 to 105 revised manuscript).

*L76: Missing comma after “peatlands”*

Answer: comma included.

*L77: Do you mean Paludiculture with ‘it’?*

Answer: That is correct. We tried not to repeat paludiculture again because we already used the word in the previous sentence.

*L80-81: Which materials? Can you cite this?*

Answer: Insulation materials. A reference was included (lines 113 and 114 revised manuscript).

*L81 -82: Maybe state why – because of constant crop removal whereby the nutrients are incorporated into the biomass that is removed*

Answer: A sentence was added to the text to explain this effect (line 115 and 116 revised manuscript).

*L83-85: This statement is true, but not well connected with the previous text. What would you like to point out with this?*

Answer: This sentence is part of the justification, therefore it was relocated to the beginning of the next paragraph (lines 120 to 122 revised manuscript), which includes the justification of the study.

*L87: Missing comma after "level".*

Answer: comma added.

*L89: Isn't uncertainty by definition "unexplained" ? Please consider removing "unexplained".*

Answer: The reviewer is correct. "Unexplained" was removed.

*L96" Comma missing before (2)*

Answer: comma added.

*L98: Missing comma after (WTD)*

Answer: comma added.

*L113-114: Mean minimum relative to what time period ? (the study period ?), please state in the text. Also, what is the mean based on ? Average of all annual minima 2019 -2022 ? Please be clear.*

Answer: Information was included to make clear that the means were for the study period and that the minimum and maximum correspond to means among experimental blocks (lines 151 to 153 revised manuscript).

*L114- 116: Please be clear the time period considered*

Answer: Sentence was edited (line 154 revised manuscript).

*L117: Missing comma after 2m*

Answer: comma was included.

*Figure 1a : Scale is not legible, no North indication – I would suggest not using Google earth for such images*

Answer: A new map was included replacing the Google Earth image.

*Figure 1b – Please use white writing – red is hard read on the green background, also increase font size to increase legibility*

Answer: White writing is now used. Font size has been increased.

*Figure 1: Why is the order of sub-figures shifting from left to right and then right to left?*

Answer: Figure was rearranged.

*L129 : please be consistent with full month names and abbreviations*

Answer: Full month names were included.

*Table 1: remove units under C:N ratio*

Answer: Units were removed.

*L139-142: Maybe the experimental design could be described in more detail? Were the '0-cut, 2-cut, 5-cut' treatments replicated in all plot. The authors mention other treatments- Were these also present in the investigated plots. In total 4 blocks were investigated, each containing 3 treatments, correct?*

Answer: Correct, there were four blocks each containing three treatments. Text was edited and sentences were added to make clarify the experimental design used (lines 185 to 189 revised manuscript).

*L145: I assume 0-cut, 2-cut and 5-cut stands for how often the sites were harvested. A short statement (eg ' , referring to number of harvests ' indicating this at this point or further up could make it easier for the reader to follow.*

Answer: statement “referring to the number of harvest events applied” was included in the first mention of the three management strategies in the description of the experimental design.

*L149 – none à non*

Answer: changed to “non”

*L152: continuous at what resolution?*

Answer: soil temperature and water table depth were measured continuously at hourly intervals. This was added in the manuscript for clarification (line 199 revised manuscript).

*L171: sec à seconds or “s”*

Answer: changed to s

*L172: stored “using”*

Answer: “using” was included.

*L185-186 : Was this actually checked / confirmed before successive (increasingly) shaded measurements ? or was it assumed ?*

Answer: The re-establishment of atmospheric concentrations was confirmed by measuring the concentrations in the chamber while the chamber was aerated before each successive measurement because the analyzer remained connected to the chamber between measurements.

*L195 – 198: How were ebullitions (if any) treated ?*

Answer: The “fluxx” function of the Flux package used to calculate these fluxes finds linear conditions within the data, thereby excluding rapid fluctuations in concentrations. This way

ebullitions were excluded for CH<sub>4</sub> fluxes calculations. This explanation was included for clarification (lines 242 to 244 revised manuscript).

*L198: Could there have been a potentially significant temperature effect between the first and last (100% dark) measurement that could have affected fluxes ? What does air/soil temperature show ?*

Answer: For temperature control, the chamber had an internal metal cooling rib in direct contact with an outer 1.7-L reservoir (on the chamber backside), accommodating four cooling elements. Two fans were attached to the cooling rib and responded to the difference between inside and outside temperature, so that cooling fans started when the difference between temperatures was higher than 1 °C and stopped when temperatures were equalized. This system is described in Detail in Elsgaard et al. (2012), which is referenced in the chamber's description at the beginning of section 2.3.

*L204-205: Is there something missing in this sentence? The ratio between the near-infrared to red light reflectance and what?*

Answer: The RVI was calculated as the ratio between the near infrared reflectance and the red light reflectance. The sentence was edited for clarification (line 255 revised manuscript).

*L217: delete 'and it was calculated'*

Answer: change accepted. Sentence was edited for clarification (line 267 revised manuscript).

*L236: Comma missing before RVI*

Answer: comma included.

*L263-265: What kind of pump was used and at what flow rate ?*

Answer: No pump was used, instead, a syringe attached to the tube with the aquarium air stone was used to extract the water samples. This text was included for clarification (lines 314 to 315 revised manuscript).

*L 278: Were the statistics based on measured or modelled values?*

Answer: Statistical comparisons to test the effect of harvest treatment and block on Reco, GPP, NEE, and NECB were performed using modelled values taken to annual budgets, whereas statistical comparisons on correlations, PCA, and linear mixed models were performed using the measured Reco and NEE.

*L286-288: This sounds like forward selection was used, yes?*

Answer: Each water chemistry parameter was added independently to the base Reco and GPP models. For example, TOC was added to model 5 (base Reco model) and its performance was compared to the base model's performance, afterwards, DOC was added to model 5 (without TOC) and its performance was compared to the base model, and so forth with all tested water chemistry parameters.

*L311-312: The last sentence repeats what the previous one stated*

Answer: The second sentence was deleted (lines 423 and 424 revised manuscript).

*L314: Doesn't GPP usually increase with PAR and soil temperature ?*

Answer: Correct. GPP increases when PAR and soil temperature increase due to more vigorous growth of the vegetation under higher temperatures and warmer conditions. In the manuscript, we state that GPP was negatively correlated to Ts and PAR because, as stated at the beginning of the 2.5 section, we consider (from an atmospheric perspective) Reco as a positive CO<sub>2</sub> flux and GPP as a negative CO<sub>2</sub> flux. Additional explanation on increases of GPP as larger CO<sub>2</sub> uptake were included following recommendation of reviewer 2 (line 428 revised manuscript).

*Figure 4: here seems to be a relatively strong effect of the blocks? How does this look when the treatments are color coded? Why does GPP decrease with increasing PAR? NEE decreases with increasing Reco, however I would expect that if respiration increases at the same GPP the NEE reduces (considering Reco-GPP=NEE)?*

Answer: Yes. This figure highlights the differences between the blocks and the heterogeneity of the peatland. We did previously a plot color-coding with the treatments but they did not show a clear effect and they were superimposing each other so we decided not to add that figure. The GPP decrease with increasing PAR because we used a negative sign for GPP (see previous comment). The negative correlation seen between R<sub>eco</sub> and NEE is due to GPP being more dominant than R<sub>eco</sub> in determining the magnitude of the NEE. NEE can be positive or negative depending on the balance between GPP and R<sub>eco</sub>. For the PCA and correlation analyses we used the measured Reco and NEE (pore water sampling was done at the same time as flux measurement campaigns). For the NEE, we used the measurement under fully transparent conditions (0% PAR blocked), which was of a larger magnitude compared to the Reco generally, and this resulted in the NEE being negatively correlated to R<sub>eco</sub> and positively correlated to GPP.

*Figure 5: Panel A and B are not described. It would be better for the reader to include abbreviations in the figure caption.*

Answer: Figure and caption were edited.

*L340: Please mention the test, and n. Was this tested using the modelled GPP or the measured GPP?*

Answer: The test and n was included (line 355 revised manuscript). This was tested using the annual budgets, therefore we used the modelled data taken to annual budgets to perform these comparisons.

*Figure 6: Please increase text size in plots*

Answer: text size was increased and figure was edited as recommended by reviewer 2.

*Figure 7: Please increase text size*

Answer: text size was increased and figure was edited as recommended by reviewer 2.

*Line 392-396: This is hard to follow, consider simplifying and starting with: Including hourly Ts improved the model by X percent.....*

Answer: Text was edited in lines 511 to 513 revised manuscript for clarification.

*Line 394: Which table does 'table A4' refer to?*

Answer: table A4 is located in the appendix.

*Line 398: Which Reco values? Compared to which other values? And why are there only two values for NECB when there are many more treatments?*

Answer: The sentence was edited for clarification (lines 511 to 514 revised manuscript). The Reco values refer to the Reco calculated using annual means of WTD and soil temperature as described in the same paragraph in the manuscript. There are two values because these are mean NECB values among all plots.

*Figure 8: Please increase text size*

Answer: Text was increased and edited as recommended by reviewer 2.

*Table 5: Here a table set up similar to table 3 would help to understand the patterns better. It seems that there were very clear trends in the blocks, complicated by the fertilization per treatment. Please add the number of samples*

Answer: In this table the blocks are averaged by treatments and the treatments are averaged by block, therefore, a setup similar to table 3 could not be implemented. With a setup similar to table 3 we would not be able to show the statistical comparisons performed between blocks and harvest treatments which is important for the discussion on peatland heterogeneity. Therefore, we consider that Table 5 should remain in its current structure. N values were included in the caption.

*Table 6: Does 'N' mean 'not significant 'ns'? What are the sample sizes?*

Answer: correct N meant not significant. Table has been edited, now "Ns" as "not significant" has been included. Sample sizes have been included.

*Line 456: please add standard deviations*

Answer: standard deviations were included.

*Line 458: Should NECB not be given in units C and not CO<sub>2</sub>-C, since this includes yields and is not based only on emissions? I am not sure what is compared here : 'our NECB is larger than emissions' is NECB compared directly with CO<sub>2</sub> emissions?*

Answer: In this paragraph we compare our NECB to studies that reported NECB for Danish, German, and UK drained and rewetted peatlands. The referenced studies reported NECB based on CO<sub>2</sub> emissions in units of t C ha<sup>-1</sup> yr<sup>-1</sup>. Units were changed to t C ha<sup>-1</sup> yr<sup>-1</sup> in the whole paragraph for clarification. To make clear that we report NECB of CO<sub>2</sub> and following recommendation of reviewer 2, this was included when NECB is mentioned in abstract, objectives, methods, and in caption of tables.

*468-469: What is the considered time frame here?*

Answer: In these sentences, time frame is the period between 2020 and 2022 in which there were changes in management and a progressive increase in the WTD at the study site.

*L 473: Please insert 'and ' before 'drove'*

Answer: change was not implemented because it would have change the meaning of the sentence. Our sentence in line 591 revised manuscript states that “This naturally stimulated CO<sub>2</sub> production in the peat and together with plant respiration drove the high annual R<sub>eco</sub>” is intended to state that lower WTD along with higher soil temp. naturally stimulated CO<sub>2</sub> production and this, together with plant respiration drove higher R<sub>eco</sub>.

*L480: What happened 3 years ago?*

Answer: Three years before the study was conducted, reed canary grass was established at the study site. This is explained in section 2.2. Sentence was modified for clarification (line 607 revised manuscript).

*L495: Was this reduction significant?*

Answer: Differences in NEE and NECB between harvest treatments were not significant due to variability among blocks. However, in this case, we are referring to the potential reduction in emissions that could be achieved in the highly productive (blocks C and D) by harvesting the biomass as opposed to not harvesting it.

*L516: How are these managed? How do the CH<sub>4</sub> emissions compare to not managed peatlands?*

Answer: The referenced studies compiled CH<sub>4</sub> emissions from both pristine and rewetted peatlands in Denmark and Germany. Text was included for clarification in lines 593 to 594 (revised manuscript). Pristine peatlands have generally lower emissions than rewetted sites, however, CH<sub>4</sub> emissions vary considerably as explained in the introduction. Since the focus of this manuscript is CO<sub>2</sub> emissions, we included the information in this paragraph to compare our CH<sub>4</sub> emissions to values reported in the literature but did not elaborate in the processes controlling them in order to not divert from the manuscript’s focus.

*Line 525: delete ‘some’*

Answer: “some” was deleted.

*L532 which study site? Please indicate if the same one that the current study is focused on is meant?*

Answer: Correct, this is the same study site. The sentence was edited (lines 674 to 675 revised manuscript).

*L 549: Use percent to make this result more generalizable. Is this significant? How could this impact budgets that were made with simpler models?*

Answer: The text was edited and the percent values were included (line 702 revised manuscript). In this sentence, we compared the total mean NECB that would be obtained with models 2 and 3 vs. the one obtained with model 4, therefore, the values were compared descriptively, and no statistical significance was tested. The budget results we report in this study were all obtained with our best model (model 4). Budgets that were calculated with models 2 and 3 were only to be compared to model 4 results.

*L550: Model 4 also included Ts as explanatory variable*

Answer: correct. Sentence was removed for clarification.

*588-590: Please also indicate how big this improvement was*

Answer: the text was edited and magnitude of improvement was included (line 738 revised manuscript).

*622-625: Please rephrase these two sentences to make them clearer. In the same sentence it is stated that paludiculture provides an opportunity for emission reduction but also that harvested biomass makes out a considerable amount of GHG emissions from cultivated RCG in fen peatlands. Please clarify.*

Answer: Sentences were rephrased and an explanatory sentence was included (lines 784 to 788 revised manuscript).

*L628: If I understand correctly biogas production would then still lead to an overall GHG emission from cultivated RCG in fen peatlands?*

Answer: That is correct because the biogas would still be carbon being lost to the atmosphere, however if biogas from paludiculture is used it would replace the use of fossil fuels from other sources, therefore we suggest this as a possible use of the harvested biomass.

*L637-639: were these measurements compared and tested?*

Answer: The NECB from 2020 to 2021 at the study site was previously reported by Nielsen et al (2024). As part of the discussion, in section 4.1, this previously reported NECB is compared with our reported NECB. The relation between the NECB and the ongoing rewetting process is also discussed. The text was edited in the conclusion for clarification.

*L641-644: Is this based on a non-significant trend or was this significant. If this is only a trend it should probably not be highlighted in the conclusion.*

Answer: These statements are based on significant differences found in emissions and pore water nutrients between blocks which are discussed in sections 4.2 and 4.4. Sentence was edited in conclusion to indicate that a potential reduction in C fluxes could be achieved in nutrient rich areas.

We thank referee # 2 for reviewing our manuscript, for the thoughtful comments and the valuable feedback on how to improve the manuscript. We have revised the manuscript accordingly based on the feedback provided. The text below addresses each of the comments and questions raised. The text in *italics* are the reviewer's original comments followed by our answer in plain text.

**General comment:**

*The manuscript could use some improvement regarding the presentation of the results: The present narrative closely follows the comparison of different modelling approaches and, in my view, undersells the strong points of the data itself. While it is good to consider methodological/modelling questions, I think the results are not particularly groundbreaking (isn't using more data always better?). It therefore remains unclear to me, why the authors regard this information as important enough to justify it being the main point of the*

*manuscript's title. However, if the methodological questions are going to be kept as the main selling point of the manuscript, the presentation of the results should be focused more on supporting their claims, e.g. by showing figures of measured vs. modelled values or comparisons of calibration/validation (at least in the appendix). So far, it seems like the models have only been calibrated, not yet validated*

Answer: The results section and the title were edited to reflect a more clear structure which also aligns with the objectives and the discussion. Now results from the C budget open the door to the heterogeneity findings, then results from the modeling approaches are presented and linked to heterogeneity followed by the additional explanation of carbon fluxes by the pore water chemistry results. This, along with further discussion on the potential of paludiculture, nutrient concentrations, and differences in water table depth (lines 644-654 and 760-777 of the revised manuscript) (as recommended in reviewer 2 specific comments) will help to bring up the finding on the potential of paludiculture applied in nutrient rich areas, which both reviewers agree should be raised as a major finding in the manuscript. In the structure of the revised manuscript, the modeling work is used to emphasize the importance of higher temporal resolution data in heterogeneous rewetting peatlands. The novelty of this modeling work, as it is also explained in the specific comments below, resides in the fact that emission factors of rewetted peatlands were established based on mean annual water table data, which, our findings indicate, can lead to an underestimation of  $R_{eco}$ .

### **Specific Comments:**

*Figures: In my view all of them need to be reworked somewhat to make their message clearer and their presentation more efficient. See Technical Comments for details.*

Answer: All figures were edited following recommendations of both reviewers.

*L25/26: What is the novelty of the finding, that more data is always more accurate? Also, how do you know the additional Reco peaks you modelled actually happened?*

Answer: The text was edited in abstract. The words “better” and “peaks” were replaced. The edited text describes how modeling of Reco is improved by high temporal resolution WTD data. The modelled Reco peaks were the result of implementing high temporal resolution data and constitute the most detailed results obtained in this manuscript. The reviewer is correct, since these peaks are modelled, we cannot be certain that they occurred. However, results from the model evaluation seen in table 2 (table 4 in revised manuscript) show model performances for model 4 with  $R^2$  values between 0.74 and 0.97, additionally, plots of modelled vs. measured  $R_{eco}$  were included in the appendix to support the high model performances obtained. Our approach was to compare the results obtained using hourly input data to results obtained using mean annual input data. The novelty of the finding is that, using mean annual WTD data to establish emission factors, as it is currently done, can lead to an underestimation of  $CO_2$  emissions in rewetting peatlands.

*L67-71: To make the overall pattern of the  $CO_2/CH_4$ -tradeoff as observed in the literature clearer to readers, a statement highlighting the general benefits of peatland rewetting for the climate (even when considering  $CH_4$ ) should be added here.*

Answer: A statement regarding the delayed reestablishment of the C sink function of rewetted peatlands was included supported by references (lines 79 to 81 revised manuscript).

*L163: “depending on meteorological conditions” à What does this mean, exactly?*

Answer: Measurements were conducted on days with predominantly clear sky conditions. The text was edited for clarification (lines 209 to 210 revised manuscript). This was done to obtain the gradient in light intensities by using the shrouds in the chamber.

*L196: “if a possible leakage was identified” à How was this done?*

Answer: Leakages were identified when negative or non-linear  $R_{\text{eco}}$  fluxes were measured. We expected clear linear behavior in  $\text{CO}_2$  fluxes under dark conditions, when this was not observed it was an indication of possible leakage. Text was included for clarification (line 246 revised manuscript).

*L197-198: Why did you only consider  $\text{CH}_4$  from opaque measurements?*

Answer: We did this because most studies with manual chambers measure  $\text{CH}_4$  fluxes under opaque conditions and we wanted to obtain  $\text{CH}_4$  fluxes under comparable conditions. We did a preliminary comparison (not included in the manuscript) between the  $\text{CH}_4$  fluxes of the transparent and opaque measurements and did not see clear differences so we decide to only use the opaque ones.

*L264-265: “placed 20 cm below the water table in each piezometer” à so the absolute depth of the measurements changes over the study period? How did this affect your results?*

Answer: Correct. The depth is the soil profile at which samples were collected varied depending on changes in the WTD. However, since all plots and blocks experienced similar variations in WTD throughout the study (Fig. 2), comparisons between them were not affected. Pore water samples were collected 20 cm below the water table because we considered that at that depth (closer to the groundwater/soil interface), they reflected better the biogeochemical conditions in which GHG emissions were taking place.

*L307-308: “In general, the 0-cut plots provided the best model performances” à Any explanation?*

Answer: That is correct. Harvesting events were more difficult to model even though pre- and post-harvesting RVI measurements were included in models. In terms of the models, harvesting is an abrupt change in the biomass with minor changes in soil temperature and WTD, therefore it is expected that modeling of harvest events would be challenging.

*L356-357: There seems to be a hydrological gradient in your study area that could explain the observed patterns. I think the influence of water level on the different results for the blocks needs to be discussed more.*

Answer: We agree, further discussion on the influence of water table differences among blocks was included in the revised manuscript. This is now discussed in lines 755 to 766 in the revised manuscript.

*L368: Supports the explanation of water levels having an influence, see previous comment.*

Answer: Further discussion was included in the revised manuscript as indicated in previous comment.

*Line 376: NECB technically also needs to include carbon from CH<sub>4</sub> emissions (although it won't make much of a difference).*

Answer: Correct. To make clear that we are reporting NECB of CO<sub>2</sub>, this was included in the abstract, objectives, methods, and in the caption of tables. Additionally, in section 4.1 we calculated CH<sub>4</sub> contribution to the NECB in terms of CO<sub>2</sub>e as 11.3% but this is not further discussed to keep the focus on the CO<sub>2</sub>.

*Figure 8: Adding measured values would be helpful, in order to assess which model is more accurate.*

Answer: Prior to this figure, a model evaluation was performed (table 2) in which, based on measured values, the best performing R<sub>eco</sub> model was selected. The best performing model (model 4) was then used to test the effects of the different temporal resolutions of the input data in the sensitivity analysis, and these results are shown in Figure 8. We consider that figure 8 should be focused on the sensitivity analysis. Including the measured R<sub>eco</sub> values could alter the flow of the manuscript and also saturate the figure therefore we consider that measured values should not be included in Figure 8. Even though measured values were not included in this figure, additional plots of measured vs. modelled Reco were included in the appendix (Figure A2) to support model performance findings.

*L454-457: Please add information whether the values for your comparison also came from fertilized peatlands.*

Answer: The dataset used by Koch et al (2023), which is used to compare our measurements, included fertilized peatlands. This information was included in the revised manuscript (line 574) for clarification.

*L539-541: Why is this a less important finding than the sensitivity in regard to frequency of water level measurements?*

Answer: We consider the findings on heterogeneity for accurate estimation of emissions in rewetted peatlands to be an important finding of this manuscript. It is also related to findings on the importance of high frequency WTD data because they both aim at a more complete understanding of peatland heterogeneity and variability. The Manuscript was edited in its structure and further discussion was included in sections 4.1.3, 4.2, and 4.4 to highlight this finding on heterogeneity's importance in emission estimations from rewetted peatlands as the major finding.

*L584-585: Could it not also be a result of fertilization?*

Answer: Correct. Fertilization can also contribute to these nutrient concentrations. Sentence was edited to include this (line 733 revised manuscript).

*L593-594: Could also be a result of less peat mineralization?*

Answer: Yes, higher WTD would reduce peat mineralization, however, for the particular case of the study site, yearly WTD fluctuations were conducive to peat mineralization taking place

when WTD decreased. Further discussion on the relation between WTD fluctuations and nutrients was included in the revised manuscript in lines 760 to 771.

*L606: This might be a good place to discuss the implications of water level differences, as mentioned above.*

Answer: Further discussion was included in revised manuscript as indicated in previous comment.

*L634 The implications for your conclusions of not knowing about nitrous oxide emissions should be discussed somewhere.*

Answer: A discussion regarding N<sub>2</sub>O emissions has been included in lines 793 to 797 of the revised manuscript.

### ***Technical Comments:***

*L38 “global C cycle” à explain abbreviation?*

Answer: abbreviation was explained.

*L44 add comma after “conditions” to make sentence more readable*

Answer: change implemented.

*L47 “sp.” After genus name should not be italicized*

Answer: change implemented.

*L84 add “with or without paludiculture” or similar after “peatlands”*

Answer: “including paludiculture” was included in the sentence (line 121 revised manuscript).

*Figure 1: Generally: Panels should be arranged in rows as abcd, white space should be removed. (A) Why is why is this map oval? remove unreadable text (google earth etc.), use 2-dimensional marker. (B) red on green is hard to read, explain letters/blocks, text needs to be larger.*

Answer: The figure was edited by adopting all recommended changes. The map was also changed as recommended by Reviewer 1.

*Figure 2: This figure needs to be reworked to improve the space/infomation-ratio and quick readability. Both panels should be combined.*

Answer: The proportions were adjusted in figure to make a better use of space. We consider that panels A and B should remain independent because they convey important information that can be presented more clearly this way. Panel A shows the changes in the WTD as the poorly drained peatland rewetted (from 2019 to 2022), while panel B shows differences in WTD between the blocks in this study (2021 to 2022). A rectangle was included to make clear that panel B zooms in a section of panel A figure. Caption was also edited.

*Figure 3: I think this would work better as a table.*

Answer: We did consider using a table for this information previously, however, since it is rather a limited amount of information and we already have several tables in the manuscript, we decided to include this information as a figure. We consider this information is better as a figure because it also gives a visual break to the reader.

*L261: “electroconductivity” with lower case “e”*

Answer: Change implemented.

*L314: “GPP was negatively correlated to Ts...” This kind of wording is always very confusing when talking about negative values. Add “(larger CO2 uptake)” or similar.*

Answer: An explanatory sentence was included (line 427 revised manuscript).

*Figure 4: What is your main point for this figure? If it is the direction of arrows for fluxes and environmental parameters, the colours should be chosen so that these arrows stand out more, and the labels should be larger. Right now, the feature that stands out most is the oval marking the measurements of block A. I assume, this is not what you want to lead the readers to see first and what is the most interesting about this figure?*

Answer: The main point of this figure is to show the existing correlations in the data using the PCA plots but also to show the heterogeneity of the blocks. Therefore, both the arrows and the colored ovals are important in the figure. The figure has been edited to highlight the arrows, the color scheme of the blocks has been changed and the labels have been enlarged (Figure is now Figure 6 in revised manuscript).

*Figure 5: x-axis labels are far too small.*

Answer: the figure and captions were edited (Figure is now Figure 7 in revised manuscript).

*L358: “higher”à see comment above, add “(more positive) or similar to make the direction clearer*

Answer: Change was implemented (line 373 revised manuscript). Additionally, wording on GPP was revised and edited accordingly in the whole manuscript.

*Figure 6: Text in the figure is very hard to read and should be larger. The colours (blue and green) are too similar and hard to distinguish*

Answer: Figure was edited by increasing text size and changing the color scheme (Figure is now Figure 4 in revised manuscript).

*Figure 7: Text too small, lines too thin, red/green contrast should be avoided to make figure readable for people with colour-blindness.*

Answer: Figure was edited by increasing text, changing color scheme, and making lines thicker (Figure is now Figure 5 in revised manuscript).

*Figure 8: See Figure 7.*

Answer: Figure was edited by increasing text and changing color scheme.

*L594: “.” Or “;” instead of “,”?*

Answer: Change implemented.

*Figure A1: In its present form I don't know how useful this figure is, at least I cannot read almost any of the variable names?*

Answer: This plot with results from correlations supplements the results seen in PCA plots. It provides statistical information on individual correlations. We included this plot as an appendix to give the reader the possibility to explore these individual correlations. Since Appendix information is accessed online, we expect readability to be better, however the text in the labels was increased and the resolution of the plot was also improved.