# Response to associate editor

## Eddy covariance fluxes of CO2, CH4 and N2O on a drained peatland forest after clearcutting by Olli-Pekka Tikkasalo et al.

We thank the associate editor for handling our manuscript and all the referees for their valuable comments to our work. Please find below the corrections made to the manuscript based on referee #2 and #3 comments.

### Referee #2

*Thank you for your thoughtful responses to the reviewer questions. The manuscript is much improved. I am satisfied with all changes, and have only two minor points that might be addressed before publication. Figure 6 is still difficult to distinguish the bold line for inner quartile from the thin line for 95% HDI. Why not just use a boxplot instead of lines?*

**Answer:** We thank referee #2 again for reviewing our manuscript. We have increased the linewidth of the central quartiles such that they resemble a boxplot.

*Type line 550: "The predicted cumulative CH4 emission is an 60% smaller than that based " should be "The predicted cumulative CH4 emission is 60% smaller than that based"*

**Answer:** This typo has been corrected.

### Referee #3

*This study presents valuable insights into the greenhouse gas fluxes (CH4, N2O, CO2) from a drained peatland forest following clearcutting. The study employs an innovative approach by combining eddy covariance (EC) measurements, statistical modeling, and surface-type classification using UAV imagery to understand the spatial and temporal variability of gas emissions.*

*Strengths of the Manuscript:*
*Limited data: There is currently limited EC data from boreal peatland clearcuts. This paper will provide timely information for decision making.*
*Novel Approach: The integration of surface-type classification with EC and statistical modeling is a notable strength of the manuscript. The use of UAV-based surface classification and Bayesian inference methods adds significant value to the methodology.*
*Detailed Data Analysis: The thorough analysis of the seasonal and spatial variability of CH4 and N2O emissions is comprehensive, and the modeling framework provides a solid foundation for understanding the drivers of these emissions in post-clearcut peatland ecosystems.*

**Answer:** We thank referee #3 for reviewing our manuscript and providing suggestions for improvement.

*Suggestions for Improvement:*
*Estimation of Uncertainty: One critical area that requires attention is the estimation of uncertainty in the results. This is particularly important when dealing with gases of small fluxes, such as CH4 and N2O. The study did not provide an analysis on the uncertainty, and this is crucial as there are multiple sources of uncertainty that could impact the results, such as model uncertainty, gap-filling uncertainty, and system uncertainty from the EC systems. A discussion or quantification of these uncertainties would help strengthen the robustness of your findings. I suggest to consider using Monte Carlo and provide a standard deviation to the flux estimates.*

**Answer:** We agree with the reviewer that uncertainty quantification is an important part of any scientific exercise. In this work, the uncertainty of annual GHG budgets derived from EC observations was estimated by gapfilling the flux time series with multiple gapfilling algorithms and by reporting the spread between the annual flux estimates. See Table 3 in the manuscript for the spread and description of the methodology in Sect. 2.3. We recognize that this approach misses some of the sources of uncertainty, e.g. random uncertainty of EC observations and uncertainties related to the post-processing of EC data, and hence likely slightly underestimates the total uncertainty. However, to our understanding standardized approaches for estimating uncertainties have not yet been developed, in particular for $CH_4$ and $N_2O$ annual emissions, and we argue that development of improved approaches is out of the scope of this manuscript. Hence, we opt to keep the uncertainty estimation as it currently is in the manuscript. However, we have added the uncertainty estimates to the manuscript abstract and added a short note on the shortcomings of this uncertainty estimation approach on line 214-217.

"However, it is possible that the spread may underestimate the total uncertainty of annual fluxes, since it does not take into account, for example, the contribution of random uncertainty associated with EC observations, and it relates only to uncertainty related to the gapfilling process."

In regards of modeling uncertainty, the 95 % confidence intervals for the modelled fluxes are already given in the Table 3. The confidence intervals were calculated based on the model parameter uncertainties.

*Spatial Partitioning of EC Data: The study rightly acknowledges the spatial nature of the EC source area; however, I believe more attention is needed to the challenges of applying EC data to narrow features like ditches, which contribute less than 2% of the EC footprint. While it is interesting to explore spatial partitioning and the variability across different surface types, it is important to note that such narrow features, like ditches, are often difficult to assess with EC systems. The fluxes from these areas are likely to be diluted by the surrounding landscape, which could result in an underestimation of the actual fluxes. While this is an interesting approach, it should be mentioned that the results may not fully capture the fluxes from these narrow features.*

**Answer:** We have added short note to discussion on lines 650-653 about the spatial representativeness of surface types in the clearcut and how that might transfer to inferred parameters

“Another possibility is that for some of the surface types their proportion inside the footprint is always so low that their contribution to the model estimate is diluted by the surrounding landscape (e.g., ditches with water surface). As a result the model might not correctly capture their contribution to the flux.“

*Site Characterization: The manuscript lacks sufficient demonstration of site-specific conditions, particularly regarding the fertility and hydrological status of the study area. Information on these conditions is essential to contextualize the observed fluxes and to understand the underlying environmental drivers. Providing more detailed background on soil fertility and the hydrological conditions at the site would strengthen the manuscript and help interpret the results in a more meaningful way.*

**Answer:** We have added information about the fertility and hydrological status to the manuscript to the methods section (lines 143-145).

*Conclusion: Overall, this manuscript offers valuable insights into greenhouse gas emissions in boreal peatland forests post-clearcutting. The methodology is innovative and provides a strong foundation for further research. However, addressing the points outlined above—particularly the estimation of uncertainty, spatial partitioning challenges, and more detailed site characterization—would significantly improve the robustness of the manuscript.*

**Answer:** We thank referee #3 for reviewing our manuscript.

*1. Line 34: ” Greenhouse gas (GHG) fluxes have been quantified (Ojanen et al., 2010)”. The citation appears a bit strange to me as there should be many GHG related papers before and after Ojanen et al 2010.*

**Answer:** This sentence is related to the quantification of GHG fluxes in Finland. We have added additional citations that measure the GHG balance in Finland.

*2. Line 47: Rotation forestry (The term R is capitalized)*

**Answer:** We have changed the r to lowercase

*3. Line 51: What is the “duration” of GHG fluxes? Please explain or use a clearer term for that.*

**Answer:** We have changed here “GHG fluxes” to “GHG emissions”

*4. The paragraph #41-#56: I think you did a clear illustration on introducing rotation forestry. However, I think this paragraph could be shortened or even combined with other paragraphs. This manuscript does not study the entire rotation period, but just one year after clearcutting. Of course clearcutting is a part of rotation forestry but it should not be a main theme that*

*worths 16 lines to introduce. Specifically, the line regarding DOC and biodiversity of rotation forestry is irrelevant and so could be removed.*
**Answer:** We feel that this paragraph is crucial for outlining the importance of the study as it considers the short-term impacts of clearcutting. For this reason we shortened the paragraph only slightly.

*5. Line 126-129: Is there any reference to the climate data? Did you do the vegetation inventory and peat depth measurements yourselves?*

**Answer:** We have added the source for the climate data information. We measured the peat depth and made vegetation surveys.

*6. Line 139: "It was completed in June 2021 in the north-western section of the CC area" What is "it"? So the harvesting was primarily done 18-Mar to 1-Apr but was completed in June?*

**Answer:** We have changed "it" to "the harvests".

*7. Section 2.1: You mentioned that "the site is a fertile and well-drained", but provided no information about that. It would be beneficial to provide more information, including CN ratio, mean WTD prior to (if available) and after clear cut.*

**Answer:** We have added this information to the methods section as suggested.

*8. Line 155: You mentioned that the EC tower is 3.1 m tall. I assume that the canopy is about zero, then using the 1:100 rule of thumb, the flux footprint 90% should be about 300m. But based on figure 2, the footprint is less than 200m. Is there any reason to this?*

**Answer:** Like stated in the manuscript, the clearcut surface is a complex mosaic of vegetation patches and logging debris with underlying undulating surface. Hence, the displacement height caused by these flow obstacles is not zero and we estimated it empirically from the observations, see the manuscript for details. Due to this non-zero displacement height, the footprint is smaller than what one would simply estimate using the rule-of-thumb and measurement height.

*9. Line 226-228: So the N2O gapfilling model has the best performance among the three GHGs in terms of R2? I am actually quite surprised of that given the small magnitude of N2O fluxes and the complexity of the gas. In the same paragraph, you mentioned only the input variables for CH4 gapfilling but what variables did you use to gapfill N2O so its performance is so good?*

**Answer:** We agree that it is slightly surprising to see that the gapfilling model has the best performance for $N_2O$ fluxes. We assume that the good performance is since 1) $N_2O$ fluxes were easily detectable at this site (high signal-to-noise ratio) and 2) there is strong seasonality in $N_2O$ fluxes, and the emissions were not as sporadic and peaked as e.g. in agricultural sites. The gapfilling model was able to capture the seasonality accurately with the normalized daily incoming potential solar radiation and its first time derivative, i.e. it was able to explain bulk of the variability. The list of predictors used to predict N2O emissions is already mentioned in the manuscript, on lines 225-229.

*10. Line 323-324: If I understand correctly, you developed the models separated for each land surface type? If so, then how did you derive the soil moisture? Did you use a single value for all land surface types, or did you consider the spatial variation of soil moisture (also soil temperature) as I assume the ditches can behave very differently ?*

**Answer:** The model parameters are fit simultaneously to each surface type using the proportion of each surface type inside the 30-min flux footprint, air temperature, soil moisture as model input and then the model estimate is compared against flux measured with the EC. Because the spatial coverage of the footprint changes, so does the fraction of each surface type in the corresponding flux value which the model estimate is compared against. This allows us to fit all the model parameters (including surface type specific parameters) simultaneously.

Unfortunately, we did not have spatially large enough sample for using different soil moisture parameter for each surface type. The soil moisture measurements were performed in the three locations shown in Fig. 1 and we use the mean of these three measurement points. Soil moisture indeed can vary quite a lot and for this reason we do not include surface type dependent soil moisture terms in the statistical models which estimate $CH_4$ and $N_2O$ flux.

*11. Line 407: Consider using a more updated GWP based on the recent IPCC reports.*

**Answer:** We have updated the GWP100 values from the latest IPCC report. The result affects significantly only $N_2O$ emission estimate.

*12. Figure 2A: Consider removing the green colour for NEE, and showing only as a line. It looks a bit misleading now, for instance, that there is only NEE in winter but no Reco.*

**Answer:** We have adjusted Fig. 2A as suggested.

*13. Figure 2D: You mentioned that you have 3 soil moisture monitoring points in Figure 1. You can consider showing the variation of Tsoil by shades also.*

**Answer:** We have added variation to the soil moisture plot.

*14. Figure 2: You have mentioned in the introduction that a fluctuating WTD could be a hot spot for N2O emissions. Have you considered also showing the time series of WTD?*

**Answer:** We opted not to show the WTD measurements because the WTD and soil moisture have a correlation of 0.72 i.e., the dynamics of WTD can be inferred from the dynamics of soil moisture. We have added the variation of WTD to the methods section as text.

*15. Figure 3: Did you include only measured flux data in the correlation analysis , or also the gapfilled data? If you include also the gapfilled flux data, do you think that this will interfere the correlation results?*

**Answer:** We included only the measured flux data except for GPP that needs the determination of $R_{eco}$ that is based on a statistical model. For this reason we cannot know how the non-gap filled GPP correlation would correlate with the other variables.

*16. Figure 3: Why only showing absolute value is higher than 0.25, instead of the statistically significant correlation? Also, it makes sense that P does not show any correlation with any variables if using 30-min data, but there should be some lag effects, so if you present also daily sum/means then the correlation result could be different. Indeed, correlation maps are symmetric so you can remove one side of the diagonal, and consider showing correlation result of daily means or other meaningful things you want to make use of the space.*

**Answer:** All the correlations presented are statistically significant. The choice of 0.25 is arbitrary. The main point of Fig. 3 is to show that the water availability related variables and temperature variables correlate with the flux values. Based on this correlation we build the models for determining surface type specific fluxes.

We have updated Fig. 3 and present now all the correlations in a lower triangle.

There might indeed be correlation if lag effects would be studied but we believe this is outside of the scope of this study.

*17. Figure 6: Is it that $\gamma$ denotes the strength of gas emissions, and $\delta$ denotes it dependence on the temperature? It seems that the information is not very clear neither in the figure or in the text.*

**Answer:** $\gamma$ doesn't directly translate to emission strength as there are also the soil moisture dependent term and the general $\alpha$ term in the model. Furthermore, the model predicts the natural logarithm of the flux which means that $\gamma$ from different surface are raised to the power of e and then multiplied together to calculate a flux value in units nmol $m^{-2}$ $s^{-1}$. For these reasons we decided not to give straightforward interpretation of the model parameters

*18. Figure 7: If $\gamma$ in figure 6 denotes the strength of gas emissions, then Figure 7 seems a bit repetitive. At least I see the relative difference across the land types are the same between $\gamma$ in figure 6 and figure 7.*

**Answer:** For the reasons outlined in above answer, we feel that Fig. 7 is needed as it shows how the model behaves if it would be used to extrapolate flux values from single surface type. The surface type specific flux is challenging to infer directly from Fig. 6.

*19. Line 526: "Finally, we calculated the total emissions for CH4 and N2O for the snow free period using the best models". I assume you mean gapfillig*

**Answer:** Here we mean the statistical models that calculate flux based on the surface type, air temperature and soil moisture. This sentence has been changed to

“Finally, we calculated the total emissions for $CH_4$ and $N_2O$ for the snow free period using the best full $\theta$ models”

*20. Line 630-631: If you separate the surface types in categories and calculate the $\delta$ and $\gamma$ separately for each surface type, then I do not think that adding C:N ratio would improve the model as it is just a constant through the year. But C:N ratio is definitely a very important number for you to explain the spatial variations.*

**Answer:** We agree with the referee and have clarified this sentence to

“Furthermore, the spatial variability of $N_2O$ emissions might be further explained by variables describing nutrient availability (e.g., C:N ratio).”

*21. Line 721: Please be consistent when writing the year in units (ie either a-1 or yr-1)*

**Answer:** We have changed the flux unit to $yr^{-1}$.