Response to comments by Anonymous Referee #1
The author comments and answer are written without highlighting, while the comments of the Anonymous Referee #1 are highlighted in *cursive*.

The manuscript presents results of the total GHG balance (CO2, N2O and CH4) from a clearcut stand on a fertile peatland in Finland. The manuscript uses one-year measurements of eddy covariance to quantify the strength of source from clearcutting. Combining results with a UAV-based land classification and statistical modelling the authors split the source of fluxes per land class (i.e., surface-type).
My overall assessment of the project's objectives and approach is that this is very important and interesting work, especially when it addresses the full GHG balance which current literature fails to address adequately. However, I have some concerns/comments/suggestions regarding the methodology and the approach the authors took in this study. I will aim to first discuss my main concerns/comments.

Answer: We thank Referee #1 for the constructive comments that have greatly improved the manuscript. Please see below our specific answers to comments. The line numbers below refer to the revised version of the manuscript with track changes

1. *The authors claim that this study aims to investigate the impact of clearcutting on the GHG balance of forested peatlands. Yet in lines 136-138, they state that "stand regeneration was carried out in summer 2021 through ditch mounding and planting of Norway spruce seedlings". So:*
    1.1 *This is no longer a "clearcut" site since it has been replanted. It is a restock site on its second growing season (as the authors have stated multiple times throughout the manuscript) and hence the strength of source is no longer reflective of a clearcut practice (due to GPP).*

    Answer: We agree on this, text was modified just above the aims to make it clear that this paper deals with 2nd year measurements of GHG emissions. Clear-cutting, ditch mounding and replanting are common practices to establish 2nd tree generation when applying even-aged rotation forestry on drained peatland forests. The forestry measures conducted at our study site are thus common and representative for even-aged forestry. We agree our terminology was misleading, and we did not investigate impact of clearcutting but documented GHG fluxes (and GHG balance in terms of CO_2-eqv.) over 2nd post-clearcut year. Investigating the impact of clear-cutting would have required flux measurement from a reference period before clear-cutting. This is hopefully now corrected in the revised manuscript.

    1.2 *Ditch mounding was used before planting, which suggests to me that the site was disturbed prior to measurements and hence not again representative of a clearcut site. In fact, if indeed any ditch mounding was applied after clearcutting, it means that the land classification reported is also not representative of the post-felling fluxes.*

Answer: Ditch mounding is a common practice conducted on drained peatlands after clear-cutting to improve seedling survival. Both clear-cutting with heavy forest machines and ditch mounding create disturbance to peat soil surface, which is reflected in surface type proportions and their subsequent dynamics during the first years after the disturbance. Our surface type classification is done in the summer of 2022 which is the same year the EC measurements reported in this manuscript were performed. We have added clarification in Section 2.6. that the surface type classification is based on the drone imaging which were captured in June 2022.

1.3 *The authors mention that this is a fertile peatland, however, they didn't give us any further information as to how they are fertile. Was the site historically fertilised prior to planting or is because of a natural fertilisation over a number of rotations? I believe an international audience would like to know a little more information about the particulars of Finnish peatlands.*

Answer: Thanks for the comment. We have elaborated text in this regard under material and methods section, and now we provide more information on site fertility type. At present the Ränskälänkorpi research site is well-drained, Norway spruce dominated and represents mainly nutrient-rich Herb-rich (Rhtkg II) and *Vaccinium myrtillus* (Mtkg II) site types drained peatland forest (Laine et al., 2012).

2 *Fluxes presented are from a single year. I understand that authors may feel compelled to present their very interesting work as soon as the first results are available, however, it is very rare, if not I dare say totally unrealistic, to draw any conclusions on the source/sink of a site with simply a single year especially when this year is not also representative of the actual effect of the forest management practice the study claims (see point 1). There is still a huge gap in our knowledge of what is the initial pulse of GHG immediately after clearcutting, and I believe the authors may have missed the opportunity here to capture a potentially significant contribution from the first few months and prior to any planting or mounding.*

Answer: We agree with the referee that we have missed a potentially important emission contribution from the first growing season following the clearcutting. Our data presents a snapshot of continuously evolving forest patch roughly a year following the clearcut. However, we feel that it is important to report also these snapshots from rapidly changing ecosystem especially as one of our target is to characterize which surface types are impact the most to CH_4 and N_2O emissions.

We have reformulated the conclusions of the study that hopefully also reflect the fact that the temporal length of our study is limited.

3 *The modelling, although very interesting, I don't believe it has worked as expected particularly for methane. I believe the fact water table depth (WTD) or even soil moisture (theta) was ignored in the modelling was a major overlook since we know (and as the authors themselves demonstrated with Figure 3) both fluxes but particularly CH4 are strongly correlated. Furthermore, another pitfall was the choice of Tair over Tsoil. Volumetric heat capacity changes linearly with moisture, so for wet peatlands I would expect changes in Tsoil to have a bigger impact that those Tair. So, potentially, there was an underestimation of the flux and hence lower strength in the model. Finally, I believe the exclusion of some surface-types from the CH4 model may have resulted in reduced model efficiency, as it clearly worked for N2O. The authors claim that CH4 emissions were not surface dependent (lines 558-559), however, from a work at a Scottish peatland restoration site (Mazzola et al. 2021, European Journal of Soil Science) it was found that CH4 fluxes were significantly different with micro-topography, including water pools. Not considering any interaction of flux with water or moisture it is likely to result to a mismatch between model and data.*

Answer: We thank the referee for suggesting to add soil water availability describing variable to the models. We tested both the water table depth (WTD) and soil moisture, θ, as well as models where the surface type contribution of temperature was included or removed (the term with δ in the model equations). When these new models and old models from the first submission of the manuscript were compared the best model for both compounds was found to be the one with θ, 9 surface types and the δ temperature term included. Since the WTD and θ are similar metrics we do not report the modelling results for the WTD models but state that the best model was the one based on θ.

Because of the reasoning that we do not have measurements what is the water availability at different locations of the studied area we only added a general θ dependency term in the new model (Eq. 4 in the revised version of the manuscript). We have adjusted the text in the revised version of the manuscript where needed to match the new best models. We have removed figures S7 and S8 from the first submission as presenting the flux estimate for each surface type as a function of soil moisture and air temperature was challenging. The inclusion of θ in the model decreased the surface type specific fluxes (fig. 7) since part of the emissions are now attributed to the water availability term.

We have added also the reference to Mazzola et al., (2021) in the discussion section in lines 664-665.

"Also Mazzola et al., (2021) found, based on chamber measurements, that there was a clear difference between surface type specific CH_4 emissions on a restored bog site in northern Scotland"

We also tested that using T_{soil} instead of T_{air} would lead to slight improvement of the best model with N_2O but not for CH_4. Thus, we opted to keep T_{air} as the independent variable because of the reasoning above that it is likely more similar across the surface types than T_{soil} that is only measured at three locations. We have added recommendation based on this results to the methodological outlook section in the discussion on lines 673-675:

”For the best models we also tested replacing T_{air} with mean soil temperature measured at the tree locations shown in Fig. 1. For N_2O this produced slightly better fit in terms of ELPD-LOO (difference of 74 units). This suggests that especially for understanding N_2O emissions, measuring the surface type specific soil temperature would be beneficial.”

4 *I believe the uncertainty presented in Table 3 for CH4 and N2O re-enforce my opinion that the model for CH4 did not perform well (uncertainty mismatch) comparing to N2O (EC uncertainty within modelled).*

Answer: The new median model prediction for CH_4 in Table 3 is closer to the EC derived estimate and also the 95% HDI range is slightly lower. The addition of soil moisture to the model as suggested by the referee has increased the performance of CH_4 in particular.

5 *I am also surprised that N2O fluxes were not high after clearcutting. Yamulki et al. 2021 (Biogeosciences) found high N2O on an organo-mineral (30-60cm peat layer over a mineral layer). With a high fertility peatland when trees removed and WTD increases I would expect pulses of N2O. The authors demonstrated that the model was unable to capture the pulse of N2O in August. Was that pulse close to a rainfall event? If so, ignoring relationship WTD and/or theta, hindered the model’s predictive capability.*

Answer: The period with high N_2O emissions lasted approximately 10 to 15 days in late July and early August and there indeed was a relatively strong precipitation event (33.4 mm of rain during July 23) slightly before the high N_2O emissions. The precipitation event increased soil moisture and it started to decline after the precipitation event (see Figure below). As a response to this, we modified our N_2O flux model by including a common term describing N2O flux response to soil moisture as suggested by the referee, unfortunately even with this addition the model was not able to capture the peak in N_2O emissions.

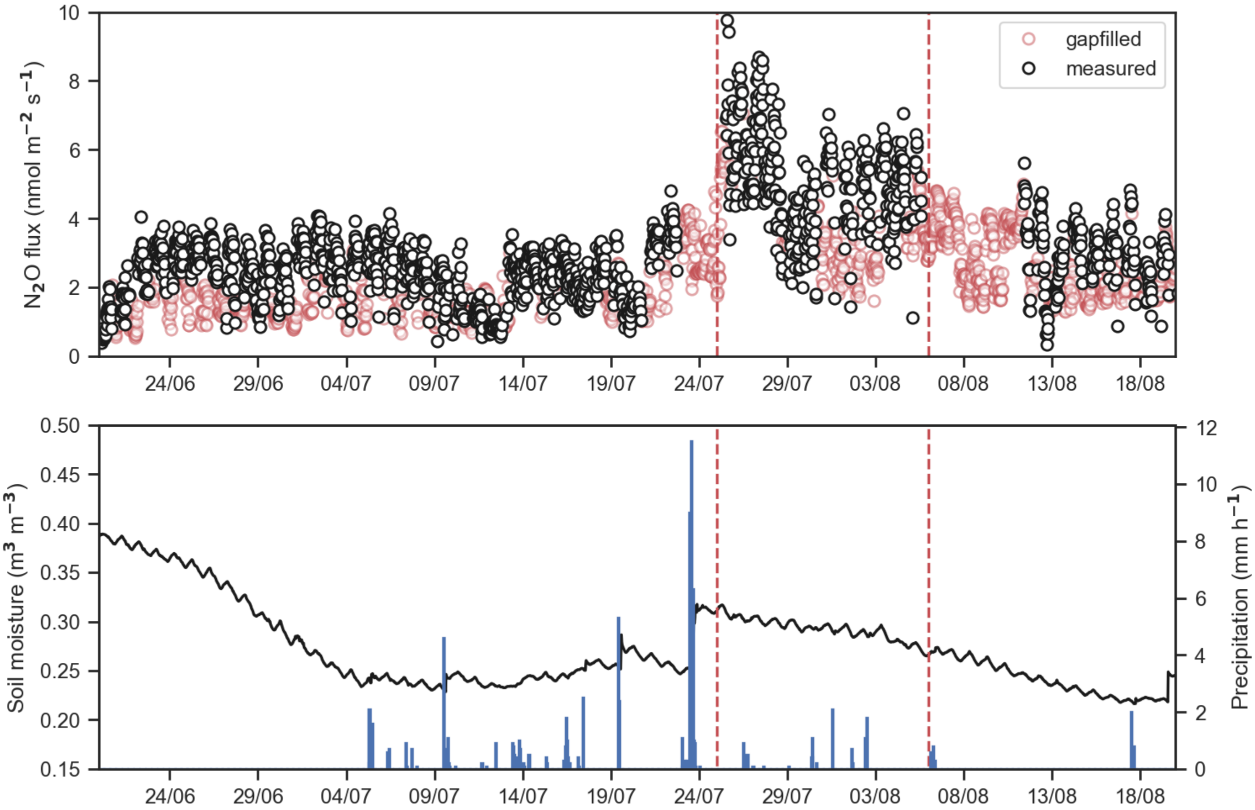

Figure 1: Coincidence of N_2O flux with precipitation events. N_2O flux (top plot), soil moisture (bottom plot, continuous line) and precipitation (bottom plot, bars) time series around the period with high N2O emissions. The approximate beginning and end of the high N2O emission period are highlighted with red dashed lines.

6 *I also found very difficult to evaluate the strength of the model's accuracy. R-squared and RMSE although they give some indication of the model's predictive capabilities, it was difficult to evaluate further the model, especially where little explanation was given for the LOO statistic. I understand this is a MC-based modelling approach, but I wonder whether a statistic like Akaike Information Criterion, or a significance level for the slope and intercept of the model vs data would be very useful to evaluate the explanatory capacity of the model.*

Answer: We have changed the model evaluation in the revised version of the manuscript. We rank the models whose parameters have been estimated with the MCMC technique using only the ELPD-LOO metric and show the performance of the best models in Fig. 4 and 5 with R^2, RMSE and also report the slope and intercept of a linear fit between the (MAP) estimated and measured flux. Additionally, we have added text how to interpret the ELPD-LOO on lines 394-396:

"The compare function ranks the models based on the expected log posterior density of the left out samples. While a single ELPD-LOO value is not easy to interpret in terms of model performance, models are straightforward to compare against each other as higher value of ELPD-LOO marks better performance."

7 *Surface-specific splitting on fluxes were performed only for CH4 and N2O, however, CO2 was ignored. Why was that? I believe it would have been a great opportunity to repeat the process for CO2.*

Answer: We considered doing similar surface-specific analysis with CO_2 flux observations but opted not to do so due to the following reasons:
1) the vegetation was rapidly recovering from the clearcut during the growing season and it is unclear how to take this recovery into account in this kind of analysis since the responses to CO_2 flux drivers change rapidly in time. For instance, due to the recovery the ecosystem CO_2 flux response to radiation was rapidly changing during the growing season. We could follow e.g., Buzacott et al., (2024) and assume certain kind of seasonal patterns for the parameters describing gross primary productivity light response curves and ecosystem respiration temperature dependence, however it is unclear what kind of seasonality would be appropriate in this recovering ecosystem. Moreover, this seasonality is likely different for different surface types resulting in many fitted parameters and hence large uncertainty.
2) our map delineating the clearcut surface into different categories is static, i.e. it does not vary in time, however pioneer species were spreading in the clearcut area during the growing season. This should be considered if the surface-specific were to be derived from CO_2 flux observations. Due to these reasons we opted to report only the ecosystem-scale CO_2 observations without trying to disaggregate the CO_2 fluxes to different surfaces.

8 *I would have liked to see more of an investigation not only how much of the flux is coming from each soil type, but what are the underlying processes by discussing correlation between vegetation, flux and climatological variables and topography.*

Answer: Thanks for the suggestion, we have done small additions to discussion about mechanisms of fluxes from surface types were added in 4.1 but at the same time, we want to avoid adding too much specific details about the underlying processes as our results would greatly benefit from comparison against chamber measurements.

9 *The manuscript presents the results of a footprint analysis, followed by a discussion on its potential limitations. It was unclear to me how the footprint was used in further analysis. More importantly, the manuscript is unclear whether footprint was used to either calculate the total area of for surface-type classification of even whether the fluxes were adjusted for footprint contribution once they have been split into different surface-type. This can have a potential major implication on how results are interpreted. It is expected, surface-types closer to the eddy covariance tower to have greater contribution. If for example, plant filled ditches are closer to the tower then potentially their contribution will be larger. Ignoring the combined effect of the surface-type distribution across the area can lead to bias. I suggest the authors review the methodology followed by Budishchev et al. 2014 (Biogeoscience) and revisit some of their approaches.*

Answer: Thank you for this comment. The footprints were utilized when deriving the surface-specific emissions based on the EC data, please see manuscript Eqs. (5-8), specifically the term $\varphi_{i,j}$ in the equation. This way the models were able to account for the heterogeneity of the clearcut surface and the model could be used e.g. to estimate surface-specific fluxes, see manuscript Fig. 7. The referee is right that the coverage of different surfaces within the EC footprint may depart from the share of those surfaces in the whole clearcut area and hence in such cases EC is observing a biased sample of the clearcut-atmosphere exchange (see also Chu et al., 2021). In response to this comment, we added a column in Table 1 where we report the mean share of each surface type in the EC footprint and compare those against their share of the overall clearcut surface. Note that the modelled flux estimates in Table 3 were already calculated so that they represent the whole clearcut surface and not the EC footprint. This was achieved by utilizing their share of the overall clearcut surface in Eq. (7) when using the fitted models for estimating the fluxes. We agree that this was not clearly articulated in the manuscript and hence tried to clarify this in Table 3 caption and by adding text on lines 548-660**.**

10 *The manuscript presents a section on footprint analysis and considerations with a discussion element. However, it was not clear to me how the footprint was used other than simply for presentation purposes. Was the footprint used for the classification of the surface-type?*

Answer: We tried to clarify the usage of footprints in our previous answer. The footprints were not used in the classification of the surface into different classes, but this was done independently with a combination of drone imaging (Sect. 2.5) and machine learning algorithms (Sect. 2.6). We added text clarifying this on manuscript line 271.

11 *Following the point from above, it wasn't clear whether the surface-type classification was for the whole of the clearcut area or for the footprint. This is potentially key to interpreting the results. Land within the footprint of the tower would have bigger contribution*

Answer: As shown in manuscript Fig. 1, the whole clearcut surface was classified into different surface categories and this was done independently from footprint analyses. We then overlaid footprints on this map with surface classes to evaluate how much different surface categories were contributing to the EC observations. This information was then in turn used in developing the model (manuscript Eqs. 3-8) which allowed us to evaluate surface-specific emissions. See our response above for the EC footprint sampling bias.

12 *Lines 649-656, the CO2 emissions from the peatland are compared to mineral soil. The authors must understand matching fluxes in these two different soil types does not equate validity of measurements due to underlying differences in carbon stocks and respiratory processes.*

Answer: We are aware that the fluxes between different surface types cannot be used to validate measurements. In the discussion section 4.3 our aim is to put our measurements into context of other post-clearcut young boreal and hemiboreal stands.

Some further comments:

1. *The introduction only lightly touches on the importance of N2O and the current gap in knowledge.*

Answer: Thank you for this comment. We have included new information on the challenges of measuring N_2O fluxes and the importance of accurately estimating them in relation to their contribution to the global GHG budget (see lines 90-94)

2. *The introduction also did not make clear what is the uniqueness of this study. In my opinion, this is a novel approach which aims to close the total GHG balance for the boreal and specifically the Fennoscandia, but it was not explicitly highlighted.*

   Answer: We have refined the last paragraph of the introduction to present why our study is needed.

3. *Figure 3 presents a correlation analysis. Are these correlations statistically significant? It was not discussed what the correlations mean for the underlying processes. Keeping the current discussion, I propose this analysis is removed. Alternatively, it can be significantly reduced to include key significant correlations which may further used in the discussion to understand processes.*

   Answer: The presented correlations are statistically significant. After consideration we decided to keep Fig. 3 in the manuscript, even though it could also be moved to supplement. The reasoning for our choice is, that with the revised version the supplement is already quite heavy and we want readers to be able to find the GHG flux correlations from the main text easily as this is something we except potential readers to be interested about. We have added a note to the start of section 2.7. that clarifies that in this study the correlation analysis is only used as a basis for selecting environmental variables for statistical flux modelling.

4. *Having said that, the manuscript has a lengthy discussion on the modelling. Although, important to highlight modelling limitation and potential pitfalls, I felt there was a little less time spend discussing the underlying processes that are related to different surface-types.*

   Answer: Please see our answer to comment 8.

5. *Figure 6 was very difficult to understand. The points and bars where too small for some variables and hence difficult to convey the message. I wonder whether there is an improved way to present the parameter values. A line across the zero would also have been helpful.*

   Answer: We have added a line across the zero and increased fonts, marker sizes and line widths for Fig. 6 to improve readability.

6. *I don't understand since we have the parameter values and range in Figure 6, why we had to set the surface-type contribution to one, to "visualise" the parameters in Figure 7. Why not simply present with the estimate surface-type contribution percentage what is the total flux from each and the percentage of the total flux measured by the eddy covariance tower? I believe this information is far more useful and citable for future*

work.

Answer: We agree with the referee that presenting the contribution of a surface type to the overall flux would be the most informative way of communicating our results. However, since we wanted to work with more normal distributed data we needed to take the log-transform of the measured flux value. Because of this transform once one takes the back transformation (Eq. 9) what is left is a multiplicative model. For multiplicative models it is challenging to determine a rule for calculating a contribution of a single surface type to the overall flux. Furthermore, none of these rules would be such that the contributions would sum to unity.

We decided to go with the current presentation where we show 1) the distribution of the estimated parameters 2) surface type specific flux distribution by assuming unity surface coverage for each surface type in turn 3) scenario-based calculations how adding a single surface type influences the estimated flux value (Figs. S7-S8)

7. *It was interesting that the study found N2O emission during snow cover. This is potentially a important find which the manuscript did not discussed in its full extend. Of course, the single year worth of data makes it very difficult but even so, it is important to highlight its importance and whether something similar has been reported before.*

   Answer: Thank you for this valuable suggestion. We have added a brief discussion of the relevance of N_2O fluxes during the snow-covered period to the annual budget, as well as the implications of winters that are not as normal as the one studied, in the revised version of the manuscript. See lines 601-606 for further details.

8. *The conclusion sections is a repetition of information already given in either the abstract or the results section. The section requires a refocus to really provide a concluding message from the study.*

   Answer: We have refined the conclusion section in the revised version of the manuscript.

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