Reviewer comments in black

Author response in blue

Changes in the revised manuscript in *italic*

Reviewer #1

The measurement of carbon monoxide (CO) fluxes in terrestrial ecosystems, especially Arctic peatlands, is limited in the scientific literature. This study addresses this gap by offering novel insights into CO flux dynamics in these underrepresented regions. Although the biogenic fluxes measured are relatively small, CO is a significant indirect greenhouse gas that influences the troposphere's oxidative capacity. In high-latitude regions, where anthropogenic sources are limited, biogenic CO sources may significantly impact atmospheric chemistry.

This research contributes to a deeper understanding of the role of terrestrial ecosystems in the global CO budget. It challenges existing models by demonstrating that Arctic peatlands can act as net sources of CO, albeit with small absolute emissions. The study is methodologically sound, with detailed descriptions of the eddy covariance setup, the data processing, and the statistical analyses. The results are well discussed; however, a few points require further clarification.

We thank the reviewer for their constructive comments on improving the manuscript. Below, we provide a detailed point-by-point response to each of the reviewer's comments and suggestions.

Specific comments:

Materials and methods

1. Line 55: I think you mean "mean annual precipitation sum/total"? please clarify, as "accumulated" might be misleading, and the reader might understand that the number is the accumulated precipitation sum over those years

Thank you for pointing this out. We agree that the term "accumulated" can be misleading in this context. We have revised the sentence: "The mean annual temperature and ~~accumulated~~ **the mean annual precipitation** (1991–2020) for the area were 0.5^{0}C and 347 mm, respectively (SMHI, 2024)."

2. Line 99-100: please explain briefly why this roughness length and boundary layer height were chosen

Thank you for the comment. A roughness length of 0.3 m was estimated from the data assuming a logarithmic wind profile in neutral atmospheric conditions.

Since no direct measurements of boundary layer height (e.g., lidar and radio soundings) were available at the site, we adopted a constant value of 1000 m, which represents a typical average daytime condition in high latitudes. At low measurement height (2.2 m), the footprint model is not sensitive to the assumed boundary layer height. Therefore, we believe this approximation is justified for our analysis.

We have included this information to the revised manuscript: *"We assumed a constant boundary layer height of 1000 m, **because the model is insensitive to boundary layer height at low measurement heights,** and estimated the roughness length to be 0.03 m **based on the logarithmic wind profile in neutral atmospheric surface layer**"*.

3. Line 127: please explain briefly what the AIC is/shows

Thank you for the suggestion. We have added a brief explanation of the Akaike Information Criterion (AIC) briefly to the manuscript. The revised text:

"*To assess ~~the~~ importance of the variables **in linear regression,** the Akaike Information Criterion (AIC) was used. **The AIC is a metric used to compare the fit of different regression models, designed to identify the model that best balances goodness of fit and model complexity (i.e., the number of model parameters) (Akaike, 1973). The preferred model is the one with the lowest AIC value. In our case, this criterion was used to assess whether the added complexity of including temperature as a driver of CO flux is justified in addition to PAR.**"*

Results/Discussion

4. Regarding the flux footprint: the two main wind directions are SW and NW. Is one of them more dominant during the day, the other more dominant during the night? If that is the case, one of the fractions (wet or dry) would dominate the daytime fluxes, the other one the nighttime fluxes. This might create a bias as during the nighttime friction velocity is lower and turbulence is lower and therefore fluxes might be underestimated. What implications does this have for your modeling results and their interpretation?

Thank you for raising this point. We analyzed wind direction distributions under stable and unstable conditions in the Supplementary Material (Fig. S1 e–f). Stable conditions generally correspond to nighttime with low turbulence, and unstable conditions to daytime with stronger turbulence. The histograms indicate no significant difference in wind directions between stable and unstable conditions across the full dataset.

However, since Fig. S1 includes data from the entire year—including winter—and our model was developed using data only from the vegetative period (March to October), we conducted a separate analysis of daytime and nighttime fluxes specifically during the vegetative period (see below, added to the Supplementary). Our analysis shows that during daytime, the NW footprint is somewhat more dominant, while during nighttime, the SE footprint occurs more frequently. Although this difference is moderate, it does suggest some diurnal variation in source area contributions.

We acknowledge that this could introduce a potential bias, as nighttime conditions with lower friction velocity and reduced turbulence may lead to underestimation of fluxes, particularly from the SE footprint. However, we have not taken it into account in our modeling, and we believe the impact on our modeling results and interpretations is limited. We have revised the text in the Results and added a brief discussion to the Discussion:

"*The distribution of wind directions was consistent across different seasons and stability classes (Fig. S1), although slight day–night differences were observed during the non-frozen period, with SE winds more common at night and NW winds more frequent during the day (Fig. S2).*"

"*We also found that the SE footprint contained a higher proportion of nighttime data compared to the NW footprint, which may introduce a potential bias in the model, as fluxes in the SE region could be underestimated due to the lower turbulent conditions (Fig. S2). However, we consider the impact on our modeling approach and results is minimal.*"

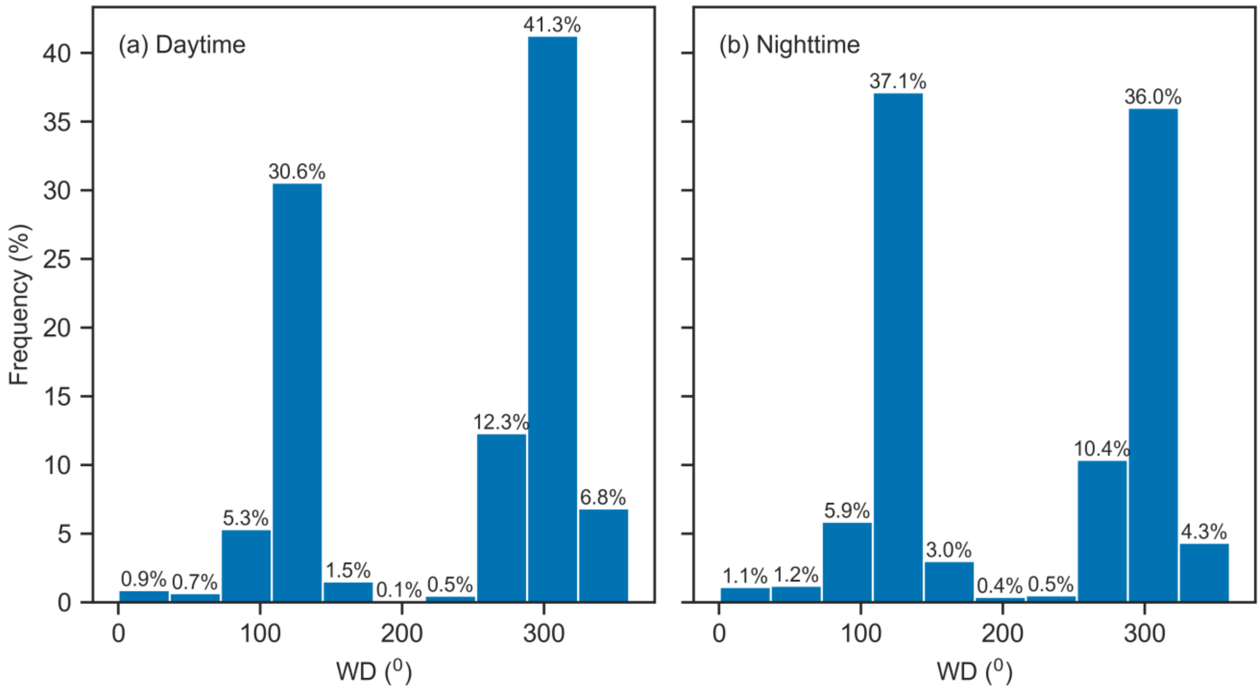


Figure S2: Wind direction distributions: a) daytime (PAR > 10 µmol m^{-2} s^{-1}) (PAR > 10 µmol m^{-2} s^{-1}) and b) nighttime (PAR < 10 µmol m^{-2} s^{-1}) using data only from the active period (from March to October).

5. You use correlation coefficients as well as random forest and SHAP values to identify the main drivers of the CO flux. However, then you use only PAR for your model, even though your analyses show that Tair and Tsoil were very important as well. You could add for example some kind of limiting function coefficient, like αdry ∗ PAR * f(temperature) + βdry. f(temperature) will be 1 if it's at optimal range.

We thank the reviewer for this comment. A similar concern was also raised by another reviewer (see General Comment 2 in Review 2). In response, we revised our model to include air temperature as an additional parameter alongside PAR. This modification led to several updates throughout the manuscript, including changes to the model formulation in Section 2.6.2; revisions to old Figures 7, 8, S7, and S8; updates to old Tables S1, S2, and S3; and revisions to the results (Sections 3.3.1 and 3.3.2) and discussion (Sections 4.3.1 and 4.3.2). A more detailed explanation of this change is provided in our response to Reviewer 2.

6. Line 189-204: it is not quite clear which parts describe the seasonal cycle and which the diurnal cycle, and there seems to be some redundant information, like the site being a CO source in spring and summer and a sink in autumn is mentioned multiple times. Please rewrite in a more concise way.

Thank you for this comment. We agree that the section could be made clearer and more concise. We have revised the text, such that the first paragraph now describes the seasonal cycle, while the second paragraph describes the diurnal cycle. The revised text:

"The ecosystem-scale half-hourly CO fluxes ranged from –0.29 to 0.34 nmol m^{-2} s^{-1} (25th and 75th percentiles), showing both net uptake and emission. The fluxes had strong seasonal variability, with the site acting as a net CO source in spring and summer (average median fluxes of 0.17 and 0.24 nmol m^{-2} s^{-1}, respectively), and as a net sink in autumn (–0.31 nmol m^{-2} s^{-1}) (Fig. 2.). The wintertime flux was minor (-0.09 nmol m^{-2} s^{-1}) compared to the fluxes of other seasons. This seasonal pattern was consistent across both years.

The CO flux showed a systematic diurnal cycle during the vegetative period, with daytime emissions and nighttime uptake. Emissions peaked at noon, reaching 1.11 nmol m^{-2} s^{-1} in summer and 0.73 nmol m^{-2} s^{-1} in spring, while nighttime uptake was strongest in autumn (-0.44 nmol m^{-2} s^{-1}). In contrast, winter fluxes lacked a clear diurnal cycle. The diurnal pattern reflected seasonal differences, with net positive daily fluxes (emissions) in spring and summer, and net negative fluxes (uptake) in autumn."

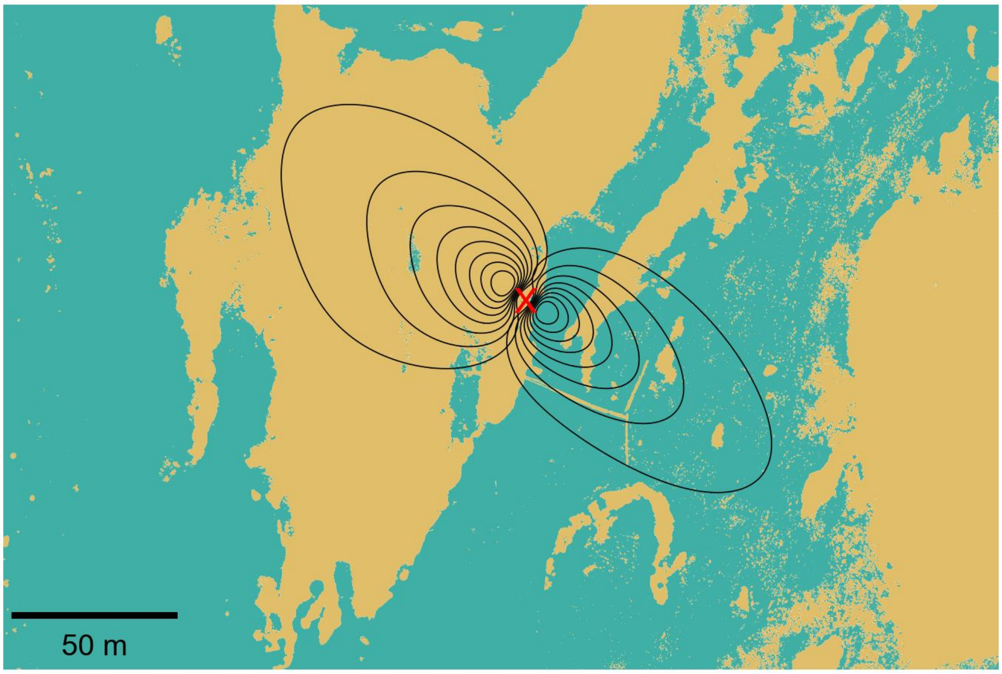
7. Fig.1: A satellite image additionally to the DEM map might be beneficial here, so that the readers can get a better impression of what the ecosystem and vegetation look like.

Thank you for this suggestion. We agree that including an aerial image will help readers get a better impression of the study site. We propose a new Figure 1, which includes (a) an orthomosaic image and (b) a DEM map, both derived from the same drone imagery. To save space, we chose to exclude the southeastern (SE) and northwestern (NW) portions of the dry and wet "pie plots" from the figure. Information about the wet and dry fractions is provided in the Results section (lines 184–186). The figure caption has been updated to reflect the revised content.

New figure 1:

(a)

(b)



A revised figure caption:

"Figure 1. (a) Aerial drone image of the study site and (b) the surface map derived from the digital elevation map (DEM) and flux footprints in the northwest (NW) and southeast (SE) directions. Black lines represent flux footprint contours from 10% to 80% and the location of

the EC tower is marked by a red cross. The yellow color indicates the dry surface and the turquoise color the wet surface."

Author contributions:

8. Mari Pihlatie is listed as an author, but not mentioned in the Author contributions. Please clarify in how far this author made a qualifying contribution to the manuscript.

We thank the reviewer for pointing this out. Mari Pihlatie contributed to writing and revising the manuscript, as well as helping with data analysis and interpretation of the results. The Author contributions section has been revised as follows:

"A.L. and I.M. designed the study. A.L., I.M., E.L. and A.M. participated in the field measurements. A.B., K.M.K., I.M., M.P. contributed to data analysis and helped interpret the results. A.L. performed the data processing, data analysis and wrote the original draft. All authors contributed the reviewing and editing the final version."

Technical corrections:

Materials and methods:

Line 96: clarify what is "w"

w refers to vertical wind velocity component. We changed w to vertical wind velocity component in the revised manuscript.

Line 107: remove second "(Tsoil)"

Corrected as suggested.

Line 109: please indicate the Fluxnet ID of the site so that the data can be found easier on the carbon portal

Thank you for this suggestion. We added the site ID and revised the text to improve the readability of the text. The edited text:

"Ancillary data used in this study were obtained from the Integrated Carbon Observation System (ICOS) measurements (Lundin et al., 2023). These data include relative humidity (RH), air pressure, air temperature (Ta), photosynthetically active radiation (PAR), water table depth (WTD), soil temperature (Ts), and soil water content (SWC) at 10 cm depth. Ts and WTD represent the average of four measurement plots, while SWC is based on the average of two measurement plots. A detailed description of the ICOS instrumentation at the Stordalen peatland site (SE-Sto), along with access to the ancillary dataset, is available through the ICOS Carbon Portal (https://data.icos-cp.eu/portal/, last access: 10 July 2025)."

Results:

Line 174-175: "~~; with~~ the minimum value was observed on […]"

We have revised the sentence as follows: "*The air temperature during the measurement period ranged from –38.8°C to 27.3°C; the minimum value was observed on 4 January 2024, and the maximum value on 22 July 2024.*"

Line 177: "for the second measurement year~~,~~ it was […]" → comma not needed

We revised the text to "The total accumulated precipitation was 325 mm in the first measurement year and 298 mm in the second year."

Description Fig.1: "tower is marked by a red cross."

Corrected as suggested.

Description Fig. 6, first line: explain abbreviations (SHAP, RF).

Corrected as suggested.

Description Fig. 6, last line: change into: "The SHAP values were calculated using data collected from March to November."

Corrected as "*The SHAP values were calculated using the data collected from March to November.*"

Description Fig. 7, second line: "Homogeneous parameters represent~~s~~ the […]"

Corrected as suggested.

Discussion:

Line 298: "which could explain~~s~~ "

Corrected as suggested

Line 313: why would you recommend future studies to take wintertime fluxes into account? It seems counterintuitive to state that it would be important to look into that, after stating that you assume minimal CO activity during winter and exclude it from your analysis. Please specify here why wintertime fluxes should still be looked into.

This comment was raised by both reviewers, and we agree that the sentence appears counterintuitive, especially given our finding that wintertime fluxes are very small and not significant in the annual budget. Therefore, we have removed the sentence from the manuscript.

Line 320-321: change into: "suggests ~~towards~~ an underlying abiotic process […]"

Corrected as suggested.

Line 335: change: "both thermal production and soil consumption are ~~both~~ likely driven by […]"

Corrected as suggested.

Line 337: the sentence is not complete, please correct

Thank you for pointing this out, we revised this part of the text as following:

"In addition to temperature, SWC has been proposed as a potential driver of CO uptake. Low SWC can limit microbial processes, while high SWC may inhibit gas diffusion in the soil (Moxley and Smith, 1998)."

Line: 367: change to: "In the model~~ing~~, non-forested boreal wetlands are identified as […]".

Corrected as suggested.

Line 368: do you mean "which corresponds to an average annual flux […] for non-forested boreal wetlands globally." ? please clarify the difference between what you refer to in the first part and in the second part of the sentence.

Thank you for pointing this out. The first and second part of the sentence should refer to the same thing. The value of –0.18 Tg CO yr^{-1} represents the total modeled flux for non-forested boreal wetlands globally. To express that as a per-area, we divided the total flux by the total non-forested boreal wetland area ($0.83*10^6$ km^2), which corresponds to an average annual flux of –217 mg CO m^{-2} yr^{-1}. We found a miscalculation in the original version, which has now corrected to the revised text:

"In that model, non-forested boreal wetlands are classified as a small CO sink, with an average annual flux of –217 mg CO m^{-2} yr^{-1}."

Line 373: "that the wet and dry classes do~~es~~ not […]"

Corrected as suggested.

Line 376: change to: "the surface structure is slowly becoming more wet […]"

Corrected as suggested.

Line 378: "In the model~~ing~~, […]"

We revised the sentence.

Line 391: comma after "estimates" not needed

We revised the sentence.

Conclusions:

Line 401: "this study provides a new dataset"

Corrected as suggested.

Reviewer #2:

This manuscript by Laasonen et al. presents a novel and highly valuable dataset of carbon monoxide (CO) fluxes from an Arctic peatland in northern Sweden, measured over two years using the eddy covariance (EC) technique. The study investigates the drivers of CO exchange in a heterogeneous landscape characterized by dry palsas and wet hollows, differentiated in their study through a footprint analysis. The authors employ advanced analytical techniques, including Random Forest models with SHAP analysis and Bayesian inference, to partition fluxes and identify key environmental controls. Their primary findings are that the peatland acts as a net CO source (unlike some modelling studies suggest), with daytime emissions strongly driven by solar radiation and nighttime uptake primarily occurring in the drier parts of the landscape. By modelling the distinct contributions of wet and dry surfaces, they estimate that wet areas are a consistent source of CO while dry areas are a net sink. The authors compellingly argue that current global models, which often categorize northern wetlands as CO sinks, may be underestimating a significant biogenic source, which may have important implications for our understanding of atmospheric chemistry and the global CO budget.

This is an excellent study addressing a critical knowledge gap. The methods are state-of-the-art and rigorous, and the conclusions are well-supported and impactful. My main recommendation is for the authors not to undersell their achievements: The Introduction and Discussion could slightly better frame the significance and novelty of their work and results.

Secondly, this study strongly focuses on the development of a CO emission model for arctic peatlands, while the measurements appear secondary. The authors may want to clarify their aims: Is this primarily a model development study, or a CO flux study presenting new findings and mechanisms in a globally important ecosystem? The authors clearly show that they know what they did, and show rigorous technical ability, but it would be a pity not to stress the significance of their novel and interesting findings more, including the mechanistic processes.

Lastly, I suggest a minor structural revision: Currently, the Results section contains detailed descriptions of the model and its evaluation metrics. This gives the impression that the paper's primary focus is on model development. Based on my understanding, the model serves as a tool to interpret the experimental measurements, rather than being the primary contribution itself. Therefore, relocating the technical details of the model to the Methods section (or Supplementary Information) would be more appropriate. This change would allow the Results section to focus on presenting the scientific findings.

We thank the reviewer for their positive and constructive feedback. We are pleased that the reviewer considers the study to be novel and impactful and that they recognize the value of the dataset and the analytical approaches used.

We agree with the reviewer that the significance and novelty of our findings could be more clearly emphasized. In response, we have revised Discussion sections to better highlight the implications of our results for understanding CO dynamics in Arctic peatlands.

We also appreciate the suggestion to clarify the primary aim of the study. The main focus is to present new insights into CO exchange processes in a heterogeneous Arctic peatland. The modeling components are used primarily as tools to interpret the measurements and to disentangle the contributions of wet and dry portions of the peatland to the CO fluxes. We have updated the text in the Methods, Results, and Discussion to make this aim clearer to the reader.

Finally, we agree with the suggestion to revise the structure of the Results section. To improve readability and clarity, we have moved detailed descriptions of the model structure and evaluation metrics to the Methods section and Supplementary Material, allowing to focus more on the scientific findings in Results and mechanistic processes in Discussion. In addition, we decided to move Figures 7 and 8 from the main text to the Supplement.

Please see our point-by-point responses below for further details.

General comments

1. Data Coverage and its Implication for Annual Budgets

The reported data coverage of 34% (line 98) is quite low for an EC study, which needs more thorough discussion. This low coverage could introduce biases in the analysis, especially concerning the annual flux estimates, as this amount of data may not be representative of all conditions. Here, a more detailed analysis of the data gaps (e.g., in the Supplement) would be welcome, for example analysing if the gaps are randomly distributed. A short discussion of how this potential bias might affect the interpretation of the results would also be helpful, especially since annual budgets are based on models trained with this dataset.

We thank the reviewer for this important comment. We agree that the data coverage of 34% is relatively low, though it is within the typical range for EC flux datasets collected in high-latitudes and challenging field environments, as well as for gases with low signal-to-noise ratio.

Data filtering was performed following quality control procedures (Mauder & Foken, 2004; Kohonen et al., 2020), where only data with a quality flag of 0 (highest quality) were accepted. The most common reason for data removal was failure to meet the flux stationarity criterion (stationarity threshold < 0.3). By using the stationarity threshold < 1 would increase the total data coverage to 43%, but our tests show that this does not significantly affect the main conclusions of our study.

We analyzed the distributions of data gaps (see Table. 1 in this document) and found the data coverage is higher during daytime and summer months. This is expected as in daytime and summer there are more favorable measurement conditions and more turbulence. We will add this Table to the Supplement.

We would also like to clarify that the actual total data coverage is 31.7%, not 34% as previously stated. The discrepancy arose from an earlier calculation that used EddyUH flux data files rather than the final flux outputs. Since EddyUH does not include missing values for unprocessed periods, this resulted in an overestimation of data coverage, particularly during a longer gap in February 2024.

"The data coverage across the different seasons is summarized in Table S1."

We have added a brief discussion in the manuscript on how this data coverage and filtering might influence the representativeness of the annual flux estimates and the potential uncertainties in the modeling results.

"*Our data coverage for the full measurement period was 31.7%, which is relatively low but within the expected range for EC measurements for gases with low signal-to-noise ratio. In the data filtering, we followed standard quality control procedure (Mauder and Foken, 2004) with the most common reason for data exclusion being failure to meet the stationarity criterion. The limited data coverage causes uncertainty in the annual fluxes, especially during nighttime and spring and autumn seasons when fewer data points are available (Table S1).*"

Table S1: The data coverage in different seasons.

Season	Daytime coverage (%)	Nighttime coverage (%)	Total coverage (%)
Winter	19.7	16.1	21.5
Spring	33.8	9.4	31.0
Summer	42.4	25.0	42.1
Autumn	35.8	14.0	31.9
Total data	35.7	14.0	31.7

2. Modelling CO flux

A recent study (Muller et al. 2025) discusses the emission of CO from plants. This flux may be minor compared to soil fluxes discussed in this study, but shouldn't be ignored. The same study also analyses the effect of PAR and air temperature, additionally linking CO emissions to transpiration and stomatal conductance. In the present study, Laasonen et al. similarly identify PAR, air and soil temperature as important drivers (Spearman correlation, SHAP values), but the Bayesian model used to generate the annual budgets only includes PAR (FCO = α * PAR + β). The reasoning for this reduced parameter set should be explained better. Note also that the correlation between PAR and air temperature or soil temperature needs to be carefully evaluated, as this could lead to problems in model assessment. Therefore, it could be beneficial for the authors to justify the exclusion of Tair from the annual model by showing that having Tair does not significantly improve performance or change the main conclusions.

We thank the reviewer for this comment regarding model parametrization. A similar concern was also raised by another reviewer (Review 1), and in response, we revised our model to include air temperature (Ta) as an additional parameter alongside PAR. This modification led to several updates throughout the manuscript, including changes to the model formulation in Section 2.6.2; revisions to old Figures 7, 8, S7, and S8; updates to old Tables S1, S2, and S3; and revisions to the abstract and results (Sections 3.3.1 and 3.3.2) and discussion (Sections 4.3.1 and 4.3.2). Please see our detailed response and revisions below.

To better understand the independent contribution of Ta to CO flux, we first analyzed the relationship between CO flux and Ta while minimizing the influence of PAR. Specifically, we used the residuals from a linear model including only PAR ($Fco = \alpha \cdot PAR + \beta$) and examined their relationship with Ta. This analysis revealed a non-linear relationship between CO flux and Ta (see new figure to Supplement below). Based on this finding, we modeled the effect of Ta using a second-degree polynomial and included an interaction term between PAR and Tato capture the combined effect of PAR and Ta.

Equations 1 and 2 in the main text (Lines 139 and 142) were updated as follows:

$$Fco = \alpha * PAR + \beta 1 * Ta + \beta 2 * Ta^2 + \gamma * PAR * Ta + \delta$$

$$Fco = f_{dry} * (\alpha_{dry} * PAR + \beta 1_{dry} * Ta + \beta 2_{dry} * Ta^2 + \gamma_{dry} * PAR * Ta + \delta_{dry}) + (1 - f_{dry}) * (\alpha_{wet} * PAR + \beta 1_{wet} * Ta + \beta 2_{wet} * Ta^2 + d_{wet} * PAR * Ta + \delta_{wet})$$

In this formulation, the terms $b*Ta + c*Ta^2$ represent the non-linear effect of air temperature, while PAR*Ta captures the interaction between PAR and Ta in the model.

To asses the impact of adding Ta to the model, we compared model performance metrics (RMSE and R^2) between the updated model and the original PAR-only model (see the Table below). The air temperature improved the model performance across both the wet and dry footprints, as well as in the homogeneous model that combines both footprints in all seasons. Based on these results, it is justified to include the air temperature to the model, even though it requires several revisions to the manuscript.

New figure to Supplement showing the relationship between CO flux and air temperature (Figure S4):

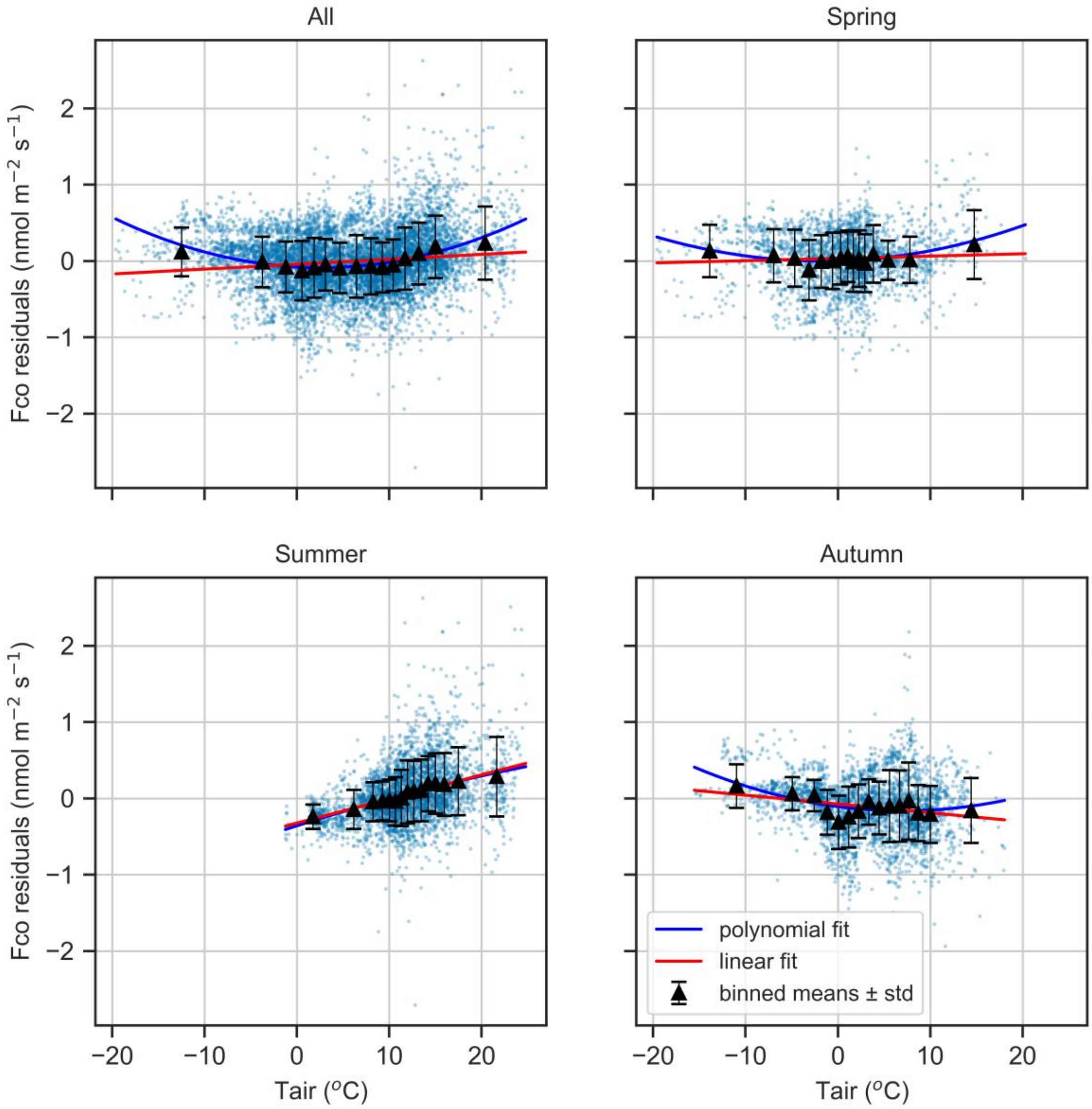


Figure S4: "*CO flux residuals from a linear model (F_{co} = a*PAR + c) plotted against air temperature. Residuals are shown as 30-minute flux data (blue dots), aggregated into binned means ± standard deviation (black triangle). Both least-squares linear (red) and second-degree polynomial (blue) fits are applied to the data.*"

A new table comparing the performance of the updated model with the original model. This table will be included in the Supplement (Table S2).

Table S2: Comparison of model performance using only photosynthetically active radiation (PAR) as an explanatory variable versus including air temperature (Ta).

Season	RMSE (only PAR)	RMSE (PAR and Ta)	R^2 (only PAR)	R^2 (PAR and Ta)
All	0.389	0.364	0.639	0.684
All dry	0.404	0.374	0.650	0.700
All wet	0.339	0.329	0.640	0.659
Spring (all)	0.355	0.335	0.678	0.713
Spring (dry)	0.375	0.335	0.646	0.717
Spring (wet)	0.336	0.316	0.691	0.727
Summer (all)	0.383	0.396	0.720	0.701
Summer (dry)	0.399	0.385	0.747	0.765
Summer (wet)	0.337	0.331	0.693	0.704
Autumn (all)	0.381	0.377	0.169	0.183
Autumn (dry)	0.384	0.380	0.230	0.246
Autumn (wet)	0.301	0.308	0.196	0.158

Next, we present all the changes made to the original manuscript. The updated figures and tables are presented after the listed changes.

- Lines 10-11: The annual cumulative flux values have changed compared to the previous version due to the updated model. In addition, we now state that these annual fluxes are estimated using the model.

  "*We estimated by modeling that annual CO fluxes from the dry parts of the peatland were ~~-44~~ **–43.3** and ~~–52~~ **-32.2** mg CO m^{-2} yr^{-1}, and from the wet parts were **70.8** and ~~84~~ **71.3** mg CO m^{-2} yr^{-1} in 2022–2023 and 2023–2024, respectively.*"

- Lines 139 and 142: Equations 1 and 2 have been updated to include air temperature.

  "$F_{co} = \alpha * PAR + \beta 1 * Ta + \beta 2 * Ta^2 + \gamma * PAR * Ta + \delta$"

  "$F_{co} = f_{dry} * (\alpha_{dry} * PAR + \beta 1_{dry} * Ta + \beta 2_{dry} * Ta^2 + \gamma_{dry} * PAR * Ta + \delta_{dry}) + (1 - f_{dry}) * (\alpha_{wet} * PAR + \beta 1_{wet} * Ta + \beta 2_{wet} * Ta^2 + d_{wet} * PAR * Ta + \delta_{wet})$"

- Lines 136 – 168: We have improved the readability of Section 2.6.2 and included all necessary details regarding the model updates, including the revised equations and model structure. The updated section is as follows:

  - Line 157: "*The priors for the second model run are presented in Table S3* ***and the posterior distributions from the second run are shown in Fig. S8***."

- Line 167: ***“The models were initially fitted using data from March to November, excluding winter months. To investigate potential seasonal variability in the model parameters, separate analyses were subsequently conducted for each season (spring, summer, and autumn). An initial model using only PAR was tested, but Ta was added because it improved model performance (Table S2). The posterior parameter estimates from the final model were then used to simulate CO fluxes from both wet and dry surface types. Annual estimates were derived by applying these posterior parameters to observed PAR and Ta data from March to November, under the assumption that wintertime fluxes were negligible and therefore set to zero.”***

- Lines 144-146: **“*α represents the sensitivity of CO fluxes to PAR;* β1 and β1 capture the linear and quadratic effects of Ta, respectively; γ represents the interaction between PAR and Ta, and δ is the intercept term*. The model parameters, α, β1, β2, γ, and δ were estimated using a Bayesian inference approach*”**

- Lines 245-262:
  - Lines 245-251: This section has been removed from the Results and partially integrated into the Methods
  - Lines 252-257: The revised paragraph:

    “Seasonal and surface-type-dependent variability was evident in the estimated model parameters, highlighting the influence of both environmental conditions and surface heterogeneity on CO exchange dynamics (Fig. S9). The seasonal differences were less pronounced when Ta was included as an explanatory variable, compared to the model using only PAR, suggesting that part of the observed seasonality was explained by temperature. The intercept parameter (δ) exhibited clear seasonal patterns: it was higher compared to other seasons in spring (δ_{dry} = –0.125 nmol m^{-2} s^{-1} and δ_{wet} = –0.106 nmol m^{-2} s^{-1}), indicating reduced CO uptake when the soil remained frozen. In contrast, lower intercepts were observed in summer (δ_{dry} = –0.572 nmol m^{-2} s^{-1} and δ_{wet} = –0.231 nmol m^{-2} s^{-1}) and autumn (δ_{dry} = –0.582 nmol m^{-2} s^{-1} and δ_{wet} = –0.175 nmol m^{-2} s^{-1}), reflecting enhanced uptake during warmer conditions. Across all seasons, the intercept was lower on dry surfaces than on wet surfaces, with the largest differences occurring in summer and autumn. Seasonal and surface-dependent variations were also apparent in other model parameters; however, the interpretation is complicated by the collinearity

between PAR and Ta, which may confound individual parameter estimates and limit the ability to isolate their respective effects.''

- Revised text in Lines 258-262:

*"Model performance was calculated using the posterior parameter sets from the second run and is presented in Table S2. The RMSE between different models ranged from **0.32** ~~0.33~~ nmol m –2 s –1 to **0.37** ~~0.40~~ nmol m –2 s –1 and R^2 values ranged from **0.20** ~~0.17~~ to **0.77** ~~0.74~~. Overall, the model performance was ~~best~~ highest in summer and lowest ~~poorest~~ in autumn. The mean of the predicted values follows the 1:1 line, with no obvious bias towards high or low values (Fig. S8). The model performance was slightly better in the heterogenous surface models compared to the homogeneous surface models**, with an average RMSE improvement of approximately 0.015 nmol m^{-2} s^{-1} and R^2 increases of 0.042.**"*

- Lines 264-272:

*"We estimated the annual cumulative fluxes by ~~using~~ **applying** the posterior parameters from ~~the second model run~~ **our seasonal model to the PAR and Tair data from March to November (Fig. S11). The difference in annual fluxes between the seasonally parameterized and non-seasonally parameterized models was small. However, as we observed seasonal variation in model parameters, we chose to use the seasonal model for calculating annual fluxes to better represent temporal dynamics**. ~~For the final annual cumulative flux estimates, we used the seasonal parametrization.~~ The annual cumulative flux for dry surfaces was ~~–44.0~~ **–43.3** mg CO m^{-2} yr^{-1} in 2022–2023 and ~~–51.5~~ **-32.2** mg CO m^{-2} yr^{-1} in 2023–2024, while for wet surfaces, it was ~~92.7~~ **70.8** mg CO m^{-2} yr^{-1} in 2022–2023 and ~~84.4~~ **71.3** mg CO m^{-2} yr^{-1} in 2023–2024. There was a significant difference between wet and dry surfaces, with dry surfaces acting as CO sinks and wet surfaces as CO sources. Interannual variability in annual cumulative fluxes was minor. The cumulative annual flux in the homogeneous model was ~~11.6~~ **-0.03** mg CO m^{-2} yr^{-1} in 2022–2023 and ~~4.2~~ **11.4** mg CO m^{-2} yr^{-1} in 2023–2024. The confidence intervals and standard deviations of annual estimates are presented in Table S5."*

- Lines 348-370: We revise this part of the discussion and added new text:

"We used the regression model to estimate CO fluxes from the dry and wet surfaces, and to calculate the annual fluxes from these two surfaces. The modeling approach has its own limitations in terms of data coverage as well as the modeling approach. Our data coverage for the full measurement period was 31.7%, which is relatively low but within the expected range for EC measurements for gases with low signal-to-noise ratio. In the data filtering, we

followed standard quality control procedure (Mauder & Foken, 2004) with most common reason for data exclusion being failure to meet the stationarity criterion. The limited data coverage causes uncertainty in the annual fluxes, especially during nighttime and spring and autumn seasons when fewer data points are available (Table S1).

We observed seasonal variability in the model parameters (Fig. S9), and thus to reduce the potential seasonal bias caused by uneven data distribution, we applied seasonal parameterization in the model. However, the comparison between the seasonal and non-seasonal models showed no significant difference in annual flux estimates (Fig. S11), suggesting that the seasonal biases do not lead to major errors in the overall annual budgets.

It is important to note that the annual fluxes reported in this study are based on modeled estimates. The model performed well for the existing dataset and was used as a tool to estimate fluxes for both wet and dry surfaces. However, we did not test the model's predictive power on unseen data. In particular, the second-degree polynomial function used to represent the temperature response may not generalize well to other years or conditions. Furthermore, the use of this function during winter may lead to overestimation of fluxes at low temperatures, as the polynomial structure predicts emissions in cold conditions.

The heterogeneous surface-structure models are found to perform better than homogeneous models in heterogeneous EC footprints (Ludwig et al., 2024; Tikkasalo et al., 2025). ~~However, we did not find significant difference in model performance between the heterogeneous and homogeneous models.~~ ***In our analysis, the heterogeneous model performed better than the homogeneous model, reducing RMSE 2.4–7.5 %.*** *~~In our case, t~~ The parameter distributions of the homogeneous model typically settled between the wet and dry parameter distributions, most often closer to the dry distributions. The reason that the homogenous parameters were closer to the dry surface type is likely related to wind directions, which show a slight bias toward the NW (Fig. S1). If the wind direction distributions were more strongly biased toward a single wind direction, a larger difference in model performance between the heterogeneous and homogeneous models could be expected.* ***We also found that the SE footprint contained a higher proportion of nighttime data compared to the NW footprint, which may introduce a potential bias in the model, as fluxes in the SE region could be underestimated due to the more low turbulent conditions (Fig. S2). However, we consider the impact on our modeling approach and results is minimal."***

- Lines 372-392:

- We have removed Lines 383–388 from the Discussion, as the statements were no longer accurate. The Tair in the model improved performance in early spring, which contradicts the original text.

- Figure S9 (old 7): We updated the figure to reflect the new model parameters (α and β1, β2, γ, and δ). Following the recommendation to revise the manuscript structure, we have moved this figure to the Supplement.

- Figure S11 (old 8): We updated the figure to present the revised annual CO fluxes. Following the recommendation to revise the manuscript structure, we have moved this figure to the Supplement.

- Figure S8 (old S7): Similar to Figure 7, this figure has been updated to reflect the new model parameters (α and β1, β2, γ, and, δ) from the revised model.

- Figure S10 (old S8): We have updated the figure to reflect the revised model. While there are no major visual changes, minor improvements in model performance can be observed—most notably in the spring panels (b and f), where the updated model more accurately captures the observed fluxes.

- Table S3 (old S1): We have updated the table to reflect the revised model.

- Table S4 (old S2): We have updated the table to reflect the results of the revised model. The new values show lower RMSE and higher R^2, indicating improved model performance compared to the original version.

- Table S5 (old S3): We have updated the table to reflect the results of the revised model. These changes do not significantly affect the main outcomes regarding the annual CO fluxes.

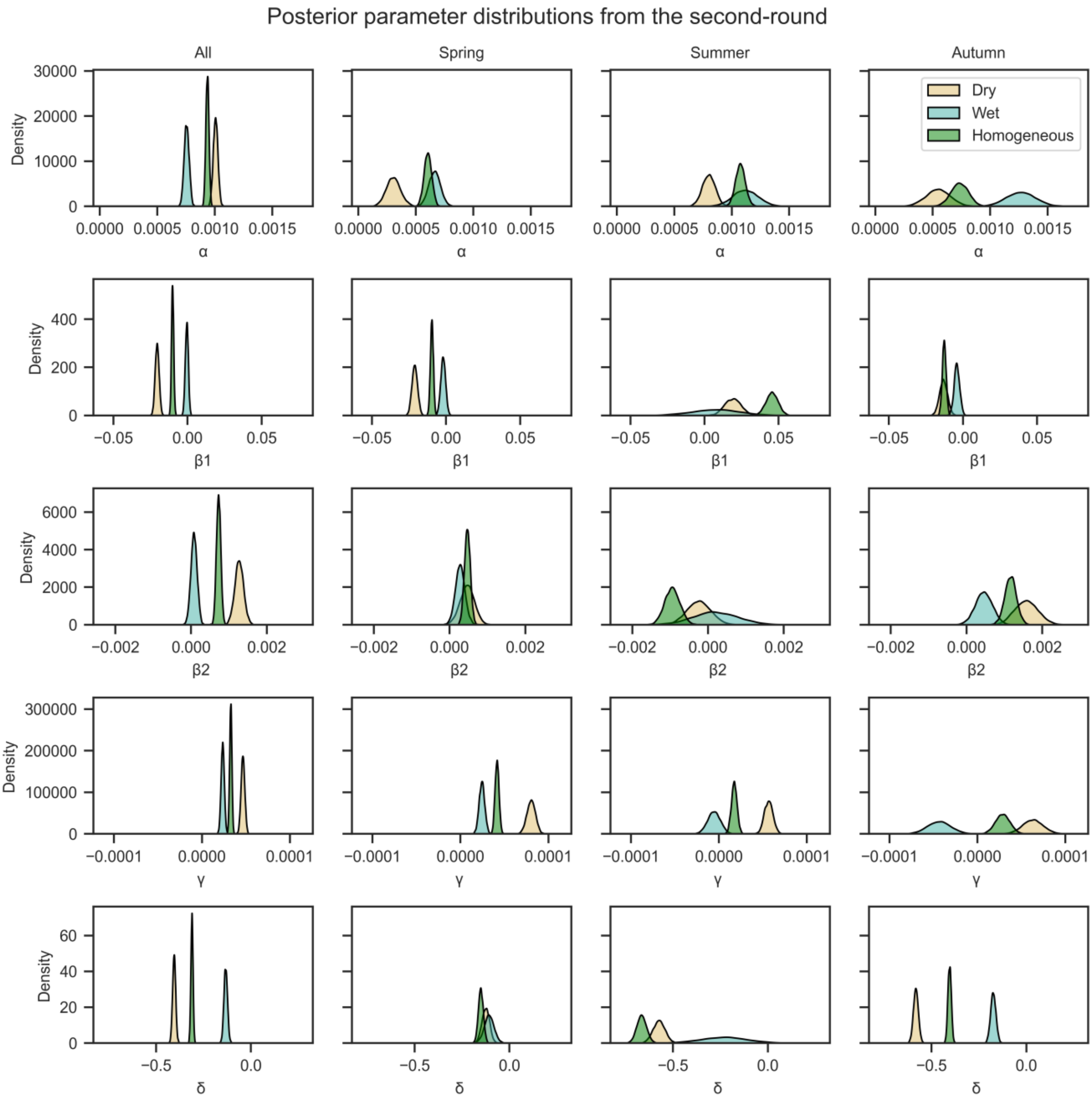


Figure S9: Posterior parameter distributions of the model parameters α and β1, β2, γ, and δ after the second model run. The parameters for wet (turquoise) and dry (yellow) are estimated considering the mixed contributions from both wet and dry surfaces. Homogeneous parameters represent the parameters without considering surface structure (green).

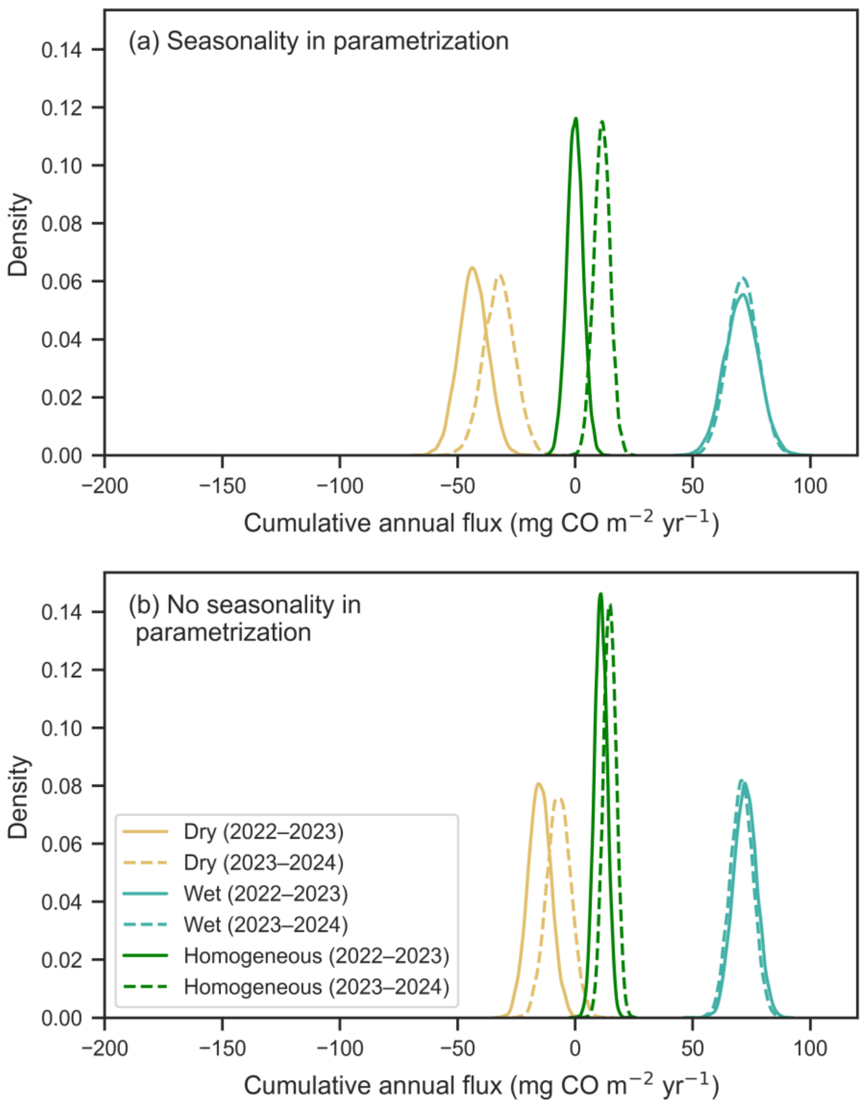


Figure S11: Probability distribution of cumulative annual fluxes in wet (turquoise), dry (yellow) surfaces and in homogeneous surface (green) (a) using seasonal parametrization and (b) using no seasonality in parametrization.

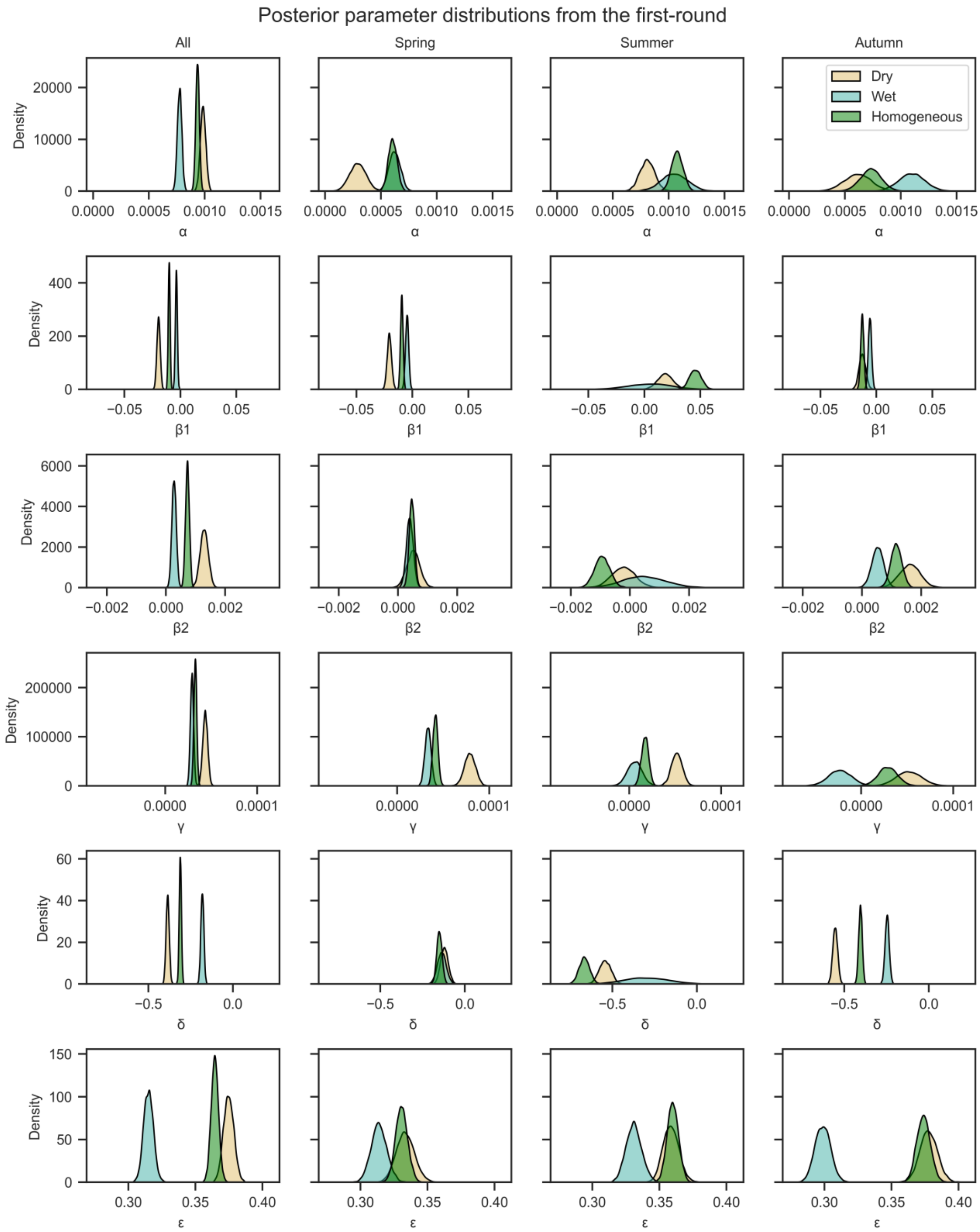


Figure S8: Posterior parameter distributions of the model parameters α and β1, β2, γ, δ and residuals (ϵ) after the first model run. The parameters are estimated separately for wet (turquoise) and dry (yellow). Homogeneous parameters represent the parameters without considering surface structure (green).

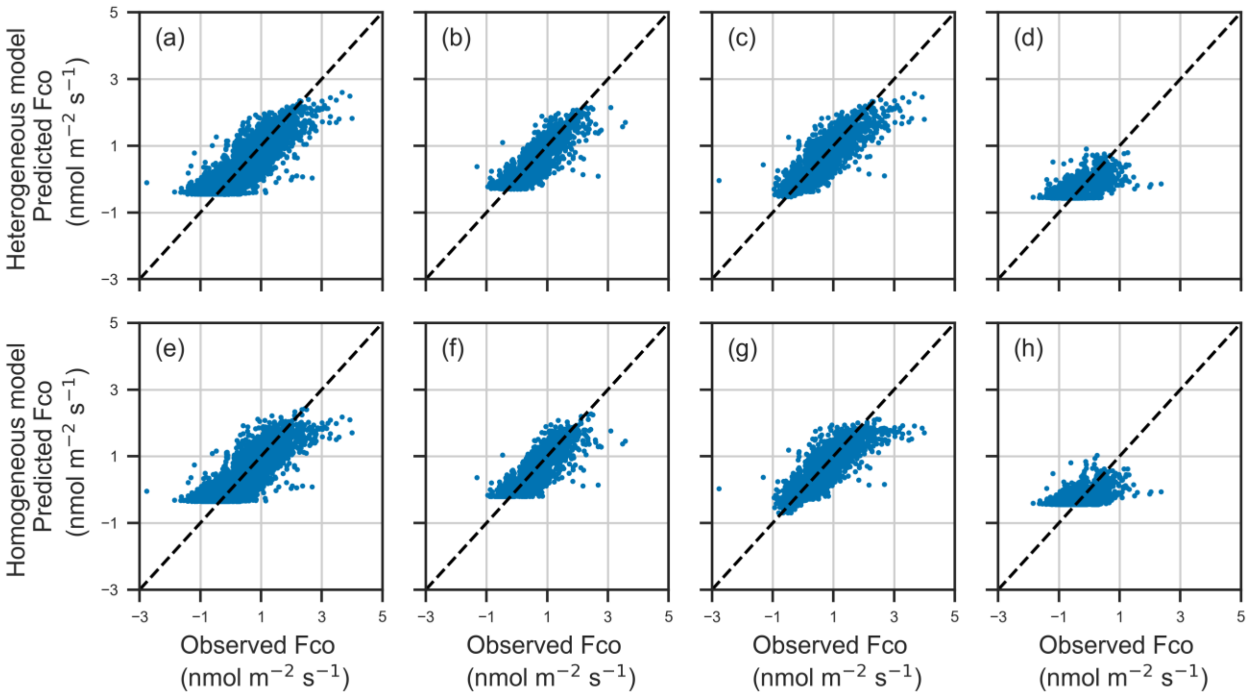


Figure S10: Predicted versus observed CO fluxes for: a) and e) all data, b) and f) spring, c) and g) summer, and d) and h) autumn. The top row shows fluxes from the heterogeneous model, while the bottom row shows fluxes from the homogeneous model. The black line represents the 1:1 relationship between observed and predicted values, and the blue dots represent 30-minute flux measurements.

Table S3: Priors distributions used in Bayesian inference approach.

Uniform distributions in first model run		
Parameter	Lower limit	Upper limit
α, α_{dry}, α_{wet}	0	1
$\beta1$, $\beta1_{\text{dry}}$, $\beta1_{\text{wet}}$	-1	1
$\beta2$, $\beta2_{\text{dry}}$, $\beta2_{\text{wet}}$	-1	1
γ, γ_{dry}, γ_{wet}	-1	1
δ, δ_{dry}, δ_{wet}	-1	1
ϵ	0	1
Normal distributions in second model run (All data)		
Parameter	Mean	Standard deviation
α, α_{dry}, α_{wet}	0.000935, 0.000984, 0.000774	1.570e-5, 2.373e-5, 1.986e-5
$\beta1$, $\beta1_{\text{dry}}$, $\beta1_{\text{wet}}$	-0.0101, -0.0196, -0.00368	0.000815, 0.00151, 0.000872
$\beta2$, $\beta2_{\text{dry}}$, $\beta2_{\text{wet}}$	0.000726, 0.00129, 0.000269	6.563e-5, 0.000135, 7.116e-5
γ, γ_{dry}, γ_{wet}	3.263e-5, 4.352e-5, 2.913e-5	1.507e-6, 2.579e-6, 1.717e-6
δ, δ_{dry}, δ_{wet}	-0.311, -0.386, -0.187	0.00645, 0.00910, 0.00730
ϵ	0.364, 0.375, 0.315	0.00269, 0.00378, 0.00363
Normal distributions in second model run (Spring)		
α, α_{dry}, α_{wet}	0.000604, 0.000293, 0.000623	3.900e-5, 7.249e-5, 4.945e-5
$\beta1$, $\beta1_{\text{dry}}$, $\beta1_{\text{wet}}$	-0.00957, -0.0206, -0.00478	0.00112, 0.00193, 0.00139
$\beta2$, $\beta2_{\text{dry}}$, $\beta2_{\text{wet}}$	0.000468, 0.000508, 0.000400	9.0156e-5, 0.000219, 0.000113
γ, γ_{dry}, γ_{wet}	4.155e-5, 7.905e-5, 3.335e-5	2.691e-6, 5.871e-6, 3.200e-6
δ, δ_{dry}, δ_{wet}	-0.121, -0.138, -0.151	0.015, 0.0222, 0.0256
ϵ	0.331, 0.334, 0.314	0.00442, 0.00689, 0.00568
Normal distributions in second model run (Summer)		
Parameter	Mean	Standard deviation
α, α_{dry}, α_{wet}	0.00107, 0.000806, 0.00105	5.252e-5, 6.737e-5, 0.000114
$\beta1$, $\beta1_{\text{dry}}$, $\beta1_{\text{wet}}$	0.0455, 0.0188, 0.00467	0.00519, 0.00662, 0.0186
$\beta2$, $\beta2_{\text{dry}}$, $\beta2_{\text{wet}}$	-0.000946, -0.000235, 0.000479	0.000253, 0.000378, 0.000696
γ, γ_{dry}, γ_{wet}	1.732e-5, 5.178e-5, 5.791e-6	4.020e-6, 5.984e-6, 7.716e-6
δ, δ_{dry}, δ_{wet}	-0.666, -0.544, -0.296	0.0342, 0.129, 0.0304
ϵ	0.360, 0.359, 0.331	0.00425, 0.00595, 0.00591
Normal distributions in second model run (Autumn)		
Parameter	Mean	Standard deviation
α, α_{dry}, α_{wet}	0.000733, 0.000615, 0.000868	0.000121, 6.580e-5, 9.0107e-5
$\beta1$, $\beta1_{\text{dry}}$, $\beta1_{\text{wet}}$	-0.0127 ,-0.0127, -0.00644	0.00142, 0.00303, 0.00144
$\beta2$, $\beta2_{\text{dry}}$, $\beta2_{\text{wet}}$	0.00116, 0.00163, 0.000330	0.000184, 0.000353, 0.000181
γ, γ_{dry}, γ_{wet}	2.888e-5, 5.191e-5, 5.248e-6	1.005e-5, 1.369e-5, 4.707e-6
δ, δ_{dry}, δ_{wet}	-0.405, -0.554, -0.235	0.0102, 0.0146, 0.0112
ϵ	0.374, 0.378, 0.299	0.00492, 0.00676, 0.00579

Table S4: The model performance of the CO flux using posterior parameters from the second model run. Full indicates surface-type-specific model and simple is the linear model without considering surface structure.

Model	RMSE	R^2
Full (All)	0.352	0.705
Simple (All)	0.364	0.684
Full (Spring)	0.322	0.734
Simple (Spring)	0.330	0.721
Full (Summer)	0.347	0.770
Simple (Summer)	0.360	0.754
Full (Autumn)	0.345	0.318
Simple (Autumn)	0.373	0.200

Table S5: Annual cumulative CO fluxes for wet, dry and homogeneous surfaces, presented as mean, standard deviation (std), 25th percentile (Q25), and 75th percentile (Q75) confidence intervals.

Year	Stat	Dry	Wet	Homogeneous
2022–2023	Q25	-47.5	66.0	-2.3
(seasonal model)	Mean ± std	-43.3 ± 6.1	70.8 ± 7.1	-0.03± 3.4
	Q75	-39.2	75.6	2.3
2023–2024	Q25	-36.7	67.0	9.2
(seasonal model)	Mean ± std	-32.2 ± 6.4	71.3 ± 6.4	11.4± 3.4
	Q75	-27.8	75.5	13.6
2022–2023	Q25	-18.4	68.9	9.1
(no seasonal parameterization)	Mean ± std	-15.1 ± 4.8	72.3 ± 4.8	10.9± 2.7
	Q75	-11.9	75.6	12.8
2023–2024	Q25	-10.3	67.5	12.9
(no seasonal parameterization)	Mean ± std	-6.9 ± 3.5	70.8 ± 4.8	14.8± 2.7
	Q75	-3.5	74.1	16.6

3. Results & Discussion not very quantitative

A number of paragraphs in the results and discussion could be improved by being more quantitative, by giving some more numbers, as the current text makes it sound rather qualitative

We thank the reviewer for this suggestion. We agree that increasing the quantitative detail strengthens the interpretation of the results and improves the overall clarity and impact of the manuscript.

In response, we have revised several sections of the Results and Discussion to include more specific numerical values and comparisons. Specifically, we made the following revisions:

- Line 215: *"The analysis revealed a strong linear relationship between CO flux and PAR ($R^2 = 0.996$, $p = 1.47e\text{-}8$), with a regression slope of 0.0012 nmol m^{-2} s^{-1} and intercept of –0.29 nmol m^{-2} s^{-1} (Fig. 5). The CO flux approached zero at approximately 250 µmol m^{-2} s^{-1} PAR, a threshold that aligned with seasonal shifts in net CO flux observed in the time series (Fig. 2). A nonlinear relationship was found between the CO flux and TA (Fig. 5). Including TA in the linear model reduced the AIC from 9014 (PAR only) to 8836, suggesting that Ta is also a significant explanatory variable for CO exchange."*

- Lines 238–242: *"We analyzed the CO fluxes from the NW and SE footprints and found that fluxes from the NW footprint were consistently lower than those from the SE footprint throughout the study period (Fig. 7). On average, the net flux from the NW footprint was –0.03 nmol m^{-2} s^{-1}, whereas the net flux from the SE footprint was 0.13 nmol m^{-2} s^{-1}. The nighttime flux from the NW footprint was on average 2.1 times larger than in the SE footprint (–0.23 nmol m^{-2} s^{-1} in NW vs. –0.11 nmol m^{-2} s^{-1} in SE). For example, in July, the mean nighttime flux from the NW footprint was –0.27 nmol m^{-2} s^{-1}, compared to –0.14 nmol m^{-2} s^{-1} from the SE footprint. This pattern was observed across all months, with the exception of April when the SE footprint exhibited slightly lower fluxes (0.05 nmol m^{-2} s^{-1} in NW vs. 0.02 nmol m^{-2} s^{-1} in SE). The consistently lower nighttime fluxes from the NW footprint suggest greater soil uptake of CO in this area compared to the SE footprint."*

- Line 252-257: "*The intercept (δ) showed the clearest seasonal patterns: it was higher compared to other seasons in spring (δ_{dry} = –0.125 nmol m^{-2} s^{-1} and δ_{wet} = –0.106 nmol m^{-2} s^{-1}), indicating lower CO uptake when the soil was still frozen. In contrast, lower intercepts were observed in summer (δ_{dry} = –0.572 nmol m^{-2} s^{-1} and δ_{wet} = –0.23 nmol m^{-2} s^{-1}) and autumn (δ_{dry} = –0.582 nmol m^{-2} s^{-1} and δ_{wet} = –0.175 nmol m^{-2} s^{-1}), increased uptake during warmer conditions.*"

- *Line 261: "The model performance was slightly better in the heterogenous surface models compared to the homogeneous surface models, with an average RMSE improvement of approximately 0.015 nmol m^{-2} s^{-1} and R^2 increases of about 0.042."*

- Line 356: *"In our analysis, the heterogeneous model performed better than the homogenous model, reducing RMSE 2.4–7.5 %."*

- Line 376: The term long-term to "*The land cover changes have been observed on decadal timescales (Varner et al., 2014)*".

Minor comments

Introduction

Line 30: There is an important study on the mechanisms of CO production in plants, showing that there is a biological component to CO fluxes (Wang and Liao 2016)

We added a sentence regarding biological CO production. The edited text is following:

"Terrestrial ecosystems can act as net sources or sinks of CO, depending on the relative contributions of emissions from vegetation and soil production and consumption. CO production from vegetation and soil ***~~is related~~*** ***is commonly considered to result from*** *~~to~~ abiotic processes, in which organic matter, litter, or plant material are degraded by radiation or temperature (Tarr et al., 1995; Derendorp et al., 2011; Lee et al., 2012; Bruhn et al., 2013; Fraser et al., 2015; Van Asperen et al., 2015).* ***However, biological CO production from plants has also been reported (Wang and Liao, 2016).*** *"*

Line 31: Chamber measurements were also done on plants (Muller et al. 2025)

We have added Muller et al. 2025 to the citation.

Line 35: "...addressed by eddy covariance (EC) technique..."

Correction: Change to "...addressed by the eddy covariance (EC) technique..."(add the definite article).

Corrected as suggested.

Methods

Line 70: I assume that this orientation of the sonic anemometer is related to the dominant wind direction?

Yes, we agree. The sonic anemometer was oriented to minimize flow distortion caused by the Gill arm from the dominant wind direction. In practice, this meant the sensor's north was aligned 10° east of geographic north.

*"The sonic anemometer****'s north*** *was ~~oriented~~* ***aligned*** *10° east ~~relative~~* ***of*** *~~to the~~ geographic north."*

Line 93: "...skewness of CO mixing ratio and vertical wind component was between –2 and 2..."

Change to "...skewness of the CO mixing ratio and vertical wind component was between –2 and 2..."

Corrected as suggested.

We also corrected "*kurtosis of CO mixing ratio and vertical wind component was between 1 and 8*" to "*kurtosis of the CO mixing ratio and vertical wind component was between 1 and 8*".

Line 94: "...and flux stationary was less than 0.3..." "Stationary" is an adjective. The metric is "stationarity".

Change to "...and flux stationarity was less than 0.3..."

We changed the wording from stationary to stationarity.

Line 97: "...standard deviation of w larger than 2 ms^{-1}..." Make sure to mention that "w" is vertical wind velocity here, as it's the first use of w.

Thank you for pointing this out. We have changed *w* to *vertical wind velocity component* in the revised manuscript.

Line 107: "...soil water content (SWC) at a 10 cm depth (Tsoil) were also used." Remove "(Tsoil)" here, it is mentioned earlier and wrong here

Corrected as suggested.

Line 130: "...was performed with Python 3.12.17." You don't need to cite specific sub-versions of Python. As long as the major version (Python 3) or even sub-version (3.12) is mentioned, your code is still compatible.

We revised the text as suggested to "*... was performed with Python 3.12.*". We thought that mentioning the sub-version as there might be differences in the performance, but we agree that mentioning the sub-sub-version is not necessary.

Line 134: Why MSE and not RMSE?

We chose Mean Squared Error (MSE) as our evaluation metric because it penalizes outliers more heavily than Root Mean Squared Error (RMSE). While Root Mean Squared Error (RMSE) could have been used as well, we think MSE works better in our data set.

Results

Line 182 and other You are not using the degree symbol (°), instead you are using a super-scripted 0. Please fix across the manuscript

Thank you for pointing this out, a super-scripted 0 is replaced by the degree symbol in the revised manuscript.

Line 215: Some parts of this section read more like results than mehods

We edited the text as follows:

"The analysis revealed a strong linear relationship between CO flux and PAR ($R^2 = 0.996$, $p = 1.47e-8$), with a regression slope of 0.0012 nmol m^{-2} s^{-1} and intercept of –0.29 nmol m^{-2}

s $^{-1}$ (Fig. 5). The CO flux approached zero at approximately 250 µmol m $^{-2}$ s $^{-1}$ PAR, a threshold that aligned with seasonal shifts in net CO flux observed in the time series (Fig. 2). A nonlinear relationship was found between the CO flux and TA (Fig. 5, S4). Including TA in the linear model reduced the AIC from 9014 (PAR only) to 8836, suggesting that Ta is also a significant explanatory variable for CO exchange."

Line 219: "…while adding the Tair…" Remove "the"

Corrected as suggested.

Figure 3: Please replace the months in the figure with seasons for clarity

Thank you for this suggestion, we have updated the figure. The new figure has seasons instead of the calendar months.

Line 227 & Discussion: The relationship between CO flux and the fdry is mentioned as being negative in nighttime data, indicating higher consumption in drier areas. The SHAP analysis also supports this. The discussion could be slightly strengthened by more directly linking this observation to the proposed mechanism of oxic vs. anoxic conditions, which is a key part of the story (Discussion).

Thank you for this suggestion. We agree that the relationship between CO flux and surface wetness/dryness is an important finding. We have strengthened the Discussion to link observations more directly to mechanism of oxic vs anoxic condition. Please see the more detailed reply to the comment Line 341.

Line 228-230: Seems more like Methods

We moved this text to section 2.6.1. in the Methods.

Line 232 and elsewhere: I suggest to shorten Tsoil, Tair, etc. To TA, TS

We decided to follow this recommendation and will change the Tsoil to Ts and Tair to Ta through the text and figures. Updated figures will be presented in the revised manuscript.

Figure 4: You may find that doing the correlation matrix separately for the dry and wet footprints could yield different results… Of course, this is only possible if you were able to separate the dataset fully into the two footprints as sources

We performed the correlation analysis separately for the dry and wet footprints, as suggested. However, we chose not to include these results in the manuscript, as they did not significantly change the main findings in the text

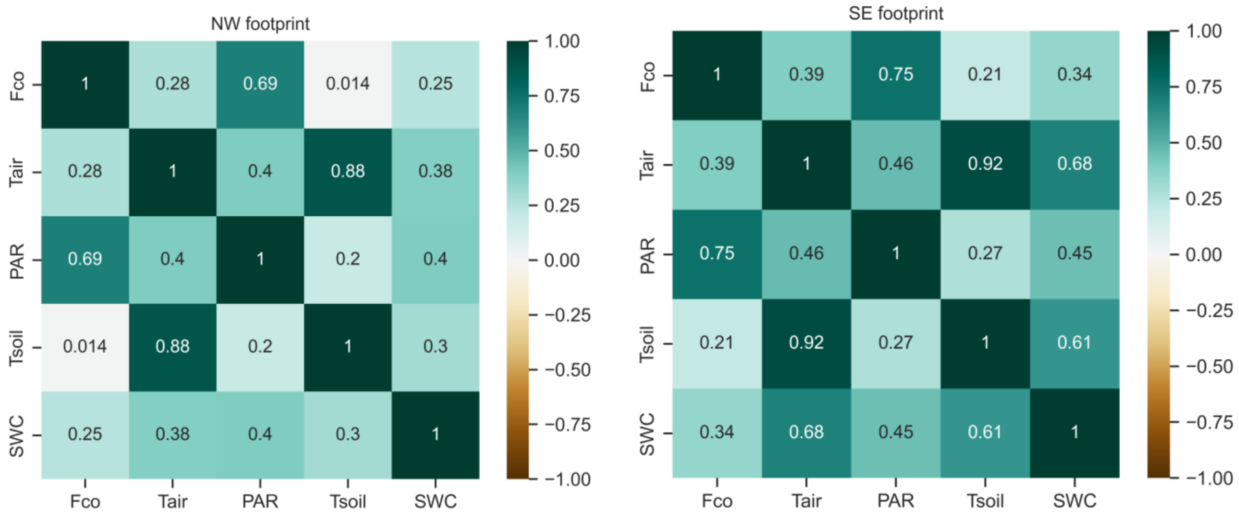


Figure: Correlation matrix for NW and SE footprints separately. The left plot corresponds to the NW footprint, while the right plot corresponds to the SE footprint

Lines 238-242: The paragraph is not very quantitative, it would be good to be given some more numbers

We edited the text to more quantitative. Here is the revised version:

"We analyzed the CO fluxes from the NW and SE footprints and found that fluxes from the NW footprint were consistently lower than those from the SE footprint throughout the study period (Fig. S6). On average, the net flux from the NW footprint was –0.03 nmol m^{-2} s^{-1}, whereas the net flux from the SE footprint was 0.13 nmol m^{-2} s^{-1}. The nighttime flux from the NW footprint was on average 2.1 times larger than in the SE footprint (–0.23 nmol m^{-2} s^{-1} in NW vs. –0.11 nmol m^{-2} s^{-1} in SE). For example, in July, the mean nighttime flux from the NW footprint was –0.27 nmol m^{-2} s^{-1}, compared to –0.14 nmol m^{-2} s^{-1} from the SE footprint. This pattern was observed across all months, with the exception of April, when the SE footprint exhibited slightly lower fluxes (0.05 nmol m^{-2} s^{-1} in NW vs. 0.02 nmol m^{-2} s^{-1} in SE). The consistently lower nighttime fluxes from the NW footprint suggest greater soil uptake of CO in this area compared to the SE footprint."

Figure 6: This analysis may benefit from being split into 2 categories (dry NW, wet SE). Also, do I understand correctl that PAR here leads to CO absorption (<0)?

Thank you for this suggestion. We performed the analysis by splitting the data into NW and SE footprints. The main difference from the original analysis is that f_{dry} is not identified as important driver when splitting the data compared to using all the data together. This is expected, as there are less variations in the f_{dry} variable within the NW and SE footprints. We decided not to include this analysis in the manuscript because it does not provide any additional information for the SHAP analysis.

SHAP values less than 0 do not directly indicate CO absorption (uptake). Instead, they represent the marginal contribution of a feature to lowering the predicted CO flux relative to the model's baseline (average) prediction. For example, at low PAR values (blue dots), negative SHAP values mean that PAR is pushing the predicted CO flux downward compared to the baseline. Conversely, high PAR values contribute to higher predicted CO fluxes. The majority of PAR data points (the large blue cluster) correspond to low PAR and negative SHAP values, largely reflecting nighttime conditions when uptake was observed.

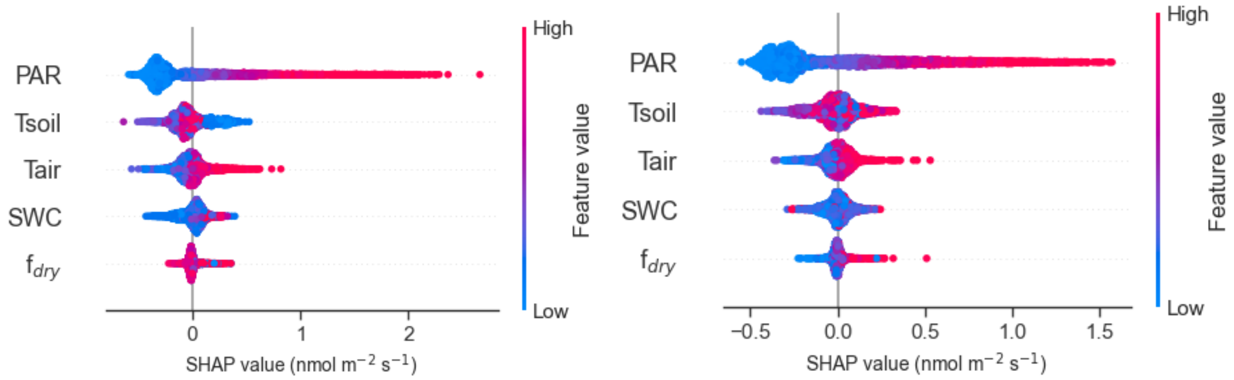


Figure: SHAP analysis for NW and SE footprints separately. The left plot corresponds to the NW footprint, while the right plot corresponds to the SE footprint

Lines 245-250: This paragraph reads more like a Methods paragraph. This may be appropriate if this a model development paper, but make sure to make this clear in your aims if so.

We have chosen to move this part of the text partly to the Methods and revised the text in the Methods section to avoid repetition.

Line 256: Qualitative statement "more negative…", please add some numbers to illustrate.

We edited the paragraph to be more quantitative, please see above.

Figures 7 & 8: This figure presents model metrics. If this is not a model development paper primarily, this belongs in the methods section to show that the model is good. If this is a model development paper, then it is appropriate here as the result is the model. Please clarify in the aims.

Thank you for this comment. We agree that the primary aim of this study is not model development, but rather to use the model as a tool to support our analysis of heterogenous eddy covariance fluxes. Therefore, we have moved Figures 7 and 8 to the Supplementary Information and now briefly describe the model performance in the Methods section to support the validity of the modeled results.

We also decided to move Figure S6 to the main text to show the difference in fluxes between NW and SE footprints.

Discussion

Line 289: Do you maybe have 1-2 more references for this statement?

We added Tarr et al. (1995) as a reference for this statement.

Line 293: “In high latitudes, dark conditions during mid-summer are limited, and therefore we have only a little nighttime data available for the summer months.” “a little” is informal, maybe correct to: “...we have only limited nighttime data available...” or “...we have only a small amount of nighttime data available...”

We revise the sentence to “*only limited nighttime data available*”

Line 298: “explain”, not explains

Corrected as suggested.

Line 298: Is there evidence in the literature of CO-consumption in microbes? If so, please cite or clarify

We added two references (King and Weber, 2007; Cordero et al., 2019) from the literature showing the CO consumption by microbes.

Line 305: “Similar shift has…” Either: “A similar shift has” or “Similar shifts have”

Corrected as “A similar shift has”

Line 311: “…while the frozen soil likely ceased the CO consumption.” This is a wrong use of the verb “to cease”. Maybe use “while CO consumption likely ceased due to frozen soil”

Corrected as suggested.

Line 313: The zero wintertime flux is mentioned multiple times, including your suggestion to study it more in the future. But what benefit would it be to do that if the flux is indeed 0? This sentence doesn’t really belong at the end of this paragraph, mentioning it in the “future research” section (end of discussion) makes more sense, but some rationale should be given why this is supposedly important if you are saying that there is no flux. Otherwise, this doesn’t add up.

This comment was raised by both reviewers, and we agree that the sentence appears counterintuitive, especially given our finding that wintertime fluxes are very small and not significant in the annual budget. Therefore, we have removed the sentence from the manuscript and will instead address recommendations for future research at the end of the Discussion section.

Line 318: Cite (Muller et al. 2025) here

We have added Muller et al. 2025 to the citation.

Line 321: Note that there is also biotic production of CO, though it may be low in magnitude compared to your soil findings. See for example (Wang and Liao 2016; Muller et al. 2025) and literature cited therein. Make sure to shortly discuss this here too, since your sites are not devoid of vegetation.

Thank you for pointing out the biotic production of CO. We have expanded the discussion to cover also the biotic production of CO:

"However, thermal production (Lee et al., 2012; Van Asperen et al., 2015) and biotic production of living plants (Wang and Liao, 2016) have also been reported as potential sources of CO at the ecosystem scale. For example, a recent study found that heat-controlled biogenic CO production from plants is linked to biotic processes rather than photoproduction (Muller et al., 2025)."

"However, we cannot exclude the possibility of heat-controlled biotic sources contributing to CO fluxes (Muller et al., 2025)."

Other minor changes:

Line 333: net fluxes were primarily driven by radiation ~~photoproduction~~,

Line 335: As mentioned earlier, thermal production, which is the one potential source of CO and soil consumption are both likely driven by temperature, which may lead to similar responses for each process, thereby minimizing the changes observed in net flux (King, 2000).

Line 323: Again, note other CO productions mechanisms (see Muller et al., 2025 and literature therein)

Please see the revised text above regarding CO production mechanisms.

Line 325: Tair and PAR are often correlated. Make sure to pay attention to this, it could affect your models. This comment is important with regards to your methods

We have revised our model to include both PAR and Tair as explanatory variables. We also included the interaction term PAR*Tair to account their combined effect.

We added two sentences to the Discussion:

"*This was also supported by our residual analysis, which revealed a non-linear relationship in the flux residuals derived from the linear model of PAR. Due to the correlation between temperature and radiation, it is challenging to fully disentangle their independent effects on CO fluxes.*"

Line 328: There is some biotic production, see comments above

Please see the revised text above regarding CO production mechanisms.

Line 341: The higher consumption under oxic conditions is expected, as CO is reactive and 2CO + O2 -> CO2…

We added more discussion on this to the paragraph:

"The higher consumption observed in drier conditions suggests that CO ***uptake*** *~~consumption~~ is larger under oxic conditions than under anoxic conditions~~, which.~~* ***This*** *is consistent with other studies,* ***which have found that most CO consumption occurs under oxic conditions (Funk et al., 1994; Rich and King, 1999). This is expected, as CO is reactive and can be oxidized to CO2 (Bartholomew and Alexander, 1979; King and Weber, 2007)"***

Line 343-345: How is this sentence on methane related to CO? It could be used to argue that you expect to find differences between the footprints (in your hypotheses), but here I don't see how it is related.

We decided to remove this sentence from the manuscript.

Line 347: The paper should more clearly state that the reported annual fluxes are simulated from the parametrized model, not derived from gap-filled measured data. For instance, in section 3.3.2, a clarifying sentence like "We estimated the annual cumulative fluxes by applying the posterior parameters from our seasonal model to the full year's PAR data..." would improve clarity for the reader.

Corrected as suggested.

We reworded the sentence in Line 264 " We estimated the annual cumulative fluxes by applying the posterior parameters from our seasonal model to the PAR and Tair data from March to November.*"*

Line 362-365: State clearly that the annual budget is modelled, as mentioned above

We reworded the paragraph:

"The modelled annual fluxes in this study ranged from –32 to 71 mg CO m^{-2} yr^{-1}. When compared with annual fluxes reported in other EC studies, particularly from temperate regions where values range from 360 to 880 mg CO m^{-2} yr^{-1} (Cowan et al., 2018; Murphy et al., 2023), our results indicate a lower contribution of biogenic CO emissions from Arctic peatlands relative to temperate grasslands."

Lines 362-370: These 2 paragraphs do not seem well connected to the rest of the text, and with each other. Please improve the flow if possible.

We have moved the text to the section 4.1. and 4.4 (Future research).

Line 370: "...current process-based models incorrectly define wetlands as CO sinks instead of CO sources..." Maybe soften this a little: "...current process-based models may incorrectly...” Note that this a very important finding and could be stressed more in the conclusions/abstract

Corrected as suggested.

Line 371: Why not call this section something like “Future research” and give it a more positive spin, rather than emphasizing the limitations? This is a great study, and that shouldn’t be lost by a strong focus on limitations.

We agree with the reviewer that we should not undersell our findings. We changed the name of the section to “Future research” and will revise this part of the manuscript to the final manuscript to give a more positive spin and to include recommendations for future research.

Line 372: “Solving…” suggests that you are solving something. This sentence does not make clear what you are solving.

Original sentence: *“Solving heterogeneous EC fluxes relies on an accurate surface cover map.”*

Revised sentence: *“Accurately characterizing heterogenous EC fluxes requires an accurate surface cover map.”*

Line 376: Long-term, how long? How quickly are these climate-change related changes happening in this environment, so that this would matter?

We added a more discussion on this manuscript and cited the paper studying the land cover changes in Stordalen peatland from 1970 to 2014 (Varner et al., 2014). In the paper, they have estimated that the land cover changes at the site can be observed on decadal time scales.

Please see our revised text below (the reply to comment regarding in Lines 372-392)

Line 378: “In the modelling,…” Either “In the model”, or “During modelling”, or “The model made the assumption that”. This is also wrong elsewhere.

Thank you for pointing this out, we have revised the text in Line 378 and in Line 367.

Line 378: *“In the model, we assumed that the flux from each wet and dry pixel had uniform responses within each area.”*

Lines 367: “*In the model, non-forested boreal wetlands are identified as...*”

Lines 372-392: This section is a bit wordy, could it be made more concise?

We also followed the recommendations to change the discussion to focus more on future research and reworded the section:

4.4. Future research

“The Stordalen peatland has slowly transitioned from dry, permafrost dominated palsa areas to wetter, sedge dominated fens due to global warming (Varner et al., 2014). The land cover changes have been observed on decadal timescales (Varner et al., 2014). This is important also in terms of CO exchange, because in the future, we can expect increased surface wetness (more sedge- and open water-dominated vegetation), which may also lead to higher CO emissions. To better understand the annual variability and future changes of CO fluxes, longer term measurements are needed.

In our two-year study period, we did not expect significant changes in the wet and dry surface classes at either seasonal or annual scale. This assumption is important, as accurately characterizing heterogenous EC fluxes, we need an accurate surface cover classification. The seasonality of surface wetness in the Stordalen peatland was studied by Łakomiec et al. (2021) and they did not observe any significant seasonal changes in wet and dry classes. However, in the model, we assumed that the flux from each wet and dry pixel contributed equally to the total flux. In practice, this assumption may not be valid, as the vegetation within each surface class may not be completely homogeneous. Especially in the wet class, the surface structure is a mixture of open water areas, sedges, and mosses, which likely contribute differently to the flux. We can expect seasonal and annual variations in open water areas and sedge cover on the peatland, even though it does not directly affect our wet and dry classification. To better understand the contribution of different surface structures within the wet and dry classes, other methods, such as chamber measurements are needed.

Although the annual CO flux from the Stordalen peatland is relatively low, our findings suggest that current process-based models may inaccurately represent wetlands as CO sinks rather than sources (Guenther et al., 2012; Liu et al., 2018). When compared to the process-based CO model by Liu et al., 2018, our CO fluxes show a clear divergence. In that model, non-forested boreal wetlands are classified as a small CO sink, with an average annual flux of –217 mg CO m^{-2} yr^{-1}. In contrast, our results indicate that these ecosystems may act as net CO sources, emphasizing the need for further research to better understand the environmental drivers and variability of CO fluxes at the ecosystem scale in high latitude wetlands.”

Lines 383-388: This paragraph again describes the model well (i.e. what you did), but in the discussion, it would be better to focus on the impact of the findings rather. Unless, again, this is primarily a model development paper.

Thank you for pointing this out. As the primary aim of the manuscript is not model development, we have removed this paragraph from the discussion section.

Line 391: “…which should be investigated futher in future studies.” Spelling error: “…which should be investigated further in future studies.”

This sentence is removed from the manuscript.

Conclusions

Line 396: "...atmosphere, which is partly…." The "which" here refers to the atmosphere, but you mean the fact that these sources are unknown. Maybe "...atmosphere. The reasons these sources were unknown so far is partly…"

Corrected as suggested.

Line 396: I suggest removing the first sentence. It is clear from the Introduction that CO is important, and makes the conclusion unnecessarily wordy. Then, the text could add emphasis by saying "global CO budget" in line 397 to balance it out.

Corrected as suggested. We removed the first sentence of the paragraph and added "global CO budget" in line 397.

Line 397: "...but also due to the lack of knowledge of CO processes." The phrasing is slightly awkward. Maybe "...but also to an incomplete understanding of CO processes."

Corrected as suggested.

Line 398-400: Limitations shouldn't be repeated in the conclusions section.

The limitations in the conclusion section have been removed from the text.

"~~This study was limited to a single peatland and two years of data. Thus, to capture the annual variations and to obtain a broader understanding of CO flux dynamics in wetlands in response to changing climate, continuous, long-term measurements from multiple wetland sites are necessary.~~"

The revised text in conclusions:

"To interpret the role of wetlands in the ***global*** *CO budget, we studied ecosystem-scale CO fluxes in Arctic peatlands. Our results revealed previously unknown biogenic sources of CO from northern peatlands to the atmosphere.* ***The reason that these sources were unknown is*** *partly due to the lack of long-term measurements at the ecosystem level, but also to an incomplete understanding of CO processes. We also report that CO flux magnitude depends on surface wetness with uptake from dry areas and emission from wet areas. This study provides a new dataset valuable for modeling and new parametrization of current process-based CO models. Our study suggests that current global models may underestimate the CO source from northern wetlands."*

Line 402: "...new data set valuable for modeling..." "Data set" should be one word here, i.e. "dataset"

Corrected as suggested.

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