

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.

### **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
2. Line 99-100: please explain briefly why this roughness length and boundary layer height were chosen
3. Line 127: please explain briefly what the AIC is/shows

#### *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?
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  $\alpha_{dry} * PAR * f(\text{temperature}) + \beta_{dry}$ .  
 $f(\text{temperature})$  will be 1 if it's at optimal range.
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.

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.

#### *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.

#### **Technical corrections:**

##### *Materials and methods*

Line 96: clarify what is “w”

Line 107: remove second “(Tsoil)”

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

##### *Results*

Line 174-175: “~~with~~ the minimum value was observed on [...]”

Line 177: “for the second measurement year, it was [...]” → comma not needed

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

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

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

Description Fig. 7, second line: “Homogeneous parameters represents~~s~~ the [...]”

##### *Discussion*

Line 298: “which could explains~~s~~”

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.

Line 320-321: change into: “suggests ~~towards~~ an underlying abiotic process [...]”

Line 335: change: “both thermal production and soil consumption are ~~both~~ likely driven by [...]”

Line 337: the sentence is not complete, please correct

Line: 367: change to: “In the modeling, non-forested boreal wetlands are identified as [...]”.

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.

Line 373: “that the wet and dry classes ~~does~~ not [...]”

Line 376: change to: “the surface structure is slowly becoming more wet [...]”

Line 378: “In the modeling, [...]”

Line 391: comma after “estimates” not needed

### *Conclusions:*

Line 401: “this study provides a new dataset”