Comments to the Author

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.

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.

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 ($F_{CO} = \alpha * PAR + \beta$). 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.

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

Minor comments

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)

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

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

Correction: Change to "…addressed by <u>the</u> eddy covariance (EC) technique…" (add the definite article).

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

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

Change to "…skewness of <u>the</u> CO mixing ratio and vertical wind component was between –2 and 2…"

Line 94: "…and <u>flux stationary</u> was less than 0.3…"

"Stationary" is an adjective. The metric is "stationarity". Change to "…and flux stationarity was less than 0.3…"

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.

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

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.

Line 134: Why MSE and not RMSE?

Line 182 and other

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

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

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

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

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).

Line 228-230: Seems more like Methods

Line 232 and elsewhere: I suggest to shorten Tsoil, Tair, etc. To T_A, T_S

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

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

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)?

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.

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

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.

Line 289: Do you maybe have 1-2 more references 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…"

Line 298: "explain", not explains

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

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

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"

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.

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

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.

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

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

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

Line 341: The higher consumption under oxic conditions is expected, as CO is reactive and $2CO + O_2$ -> CO_2...

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.

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.

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

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.

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

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.

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

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

Line 378, "In the modelling,..."

Either "In the model", or "During modelling", or "The model made the assumption that". This is also wrong elsewhere.

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

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.

Line 391: "...which should be investigated futher in future studies."

Spelling error: "...which should be investigated further in future studies."

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..."

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.

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."

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

Line 402: "...new data set valuable for modeling..."

 "Data set" should be one word here, i.e. "dataset"

Bibliography

Muller, Jonathan D., Rafat Qubaja, Eugene Koh, Rafael Stern, Yasmin L. Bohak, Fyodor Tatarinov, Eyal Rotenberg, and Dan Yakir. 2025. 'Leaf Carbon Monoxide Emissions under Different Drought, Heat, and Light Conditions in the Field'. *New Phytologist* n/a (n/a). https://doi.org/10.1111/nph.20424.

Wang, Meng, and Weibiao Liao. 2016. 'Carbon Monoxide as a Signaling Molecule in Plants'. *Frontiers in Plant Science* 7. https://www.frontiersin.org/article/10.3389/fpls.2016.00572.