

Response to reviewer 1

Reviewers' comments are marked in black, and the authors' responses are in blue.

General comments:

This study presents two years of eddy covariance CO₂ measurements from an unmanaged peatland drained 60 years ago in Iceland. The authors show that the site is still a significant and stable carbon source despite different weather conditions in the two years of measurements. They highlight specifically the importance of continuous carbon emissions during the non-growing season to the annual carbon balance of the site. Having year-round measurements in such a location is an important scientific contribution and the study helps to highlight that emissions factors used for IPCC reporting may overestimate emissions for drained Icelandic peatlands. However, I'm concerned that the results may have been partly confounded by emissions from the fuel cell used to power the flux tower. In addition, the results text is too detailed and the study performs some unnecessary analyses that could be improved to be of more interest to the reader, please see below for more details.

We thank the reviewer for their constructive feedback and the opportunity to improve our manuscript. We have carefully considered all suggestions and have made revisions to the text to clarify our methodology and findings. In particular, we appreciate the concern regarding potential interference from the EFOY fuel cell and have provided a detailed technical justification for its placement and operation. Please find our point-by-point responses to all comments below.

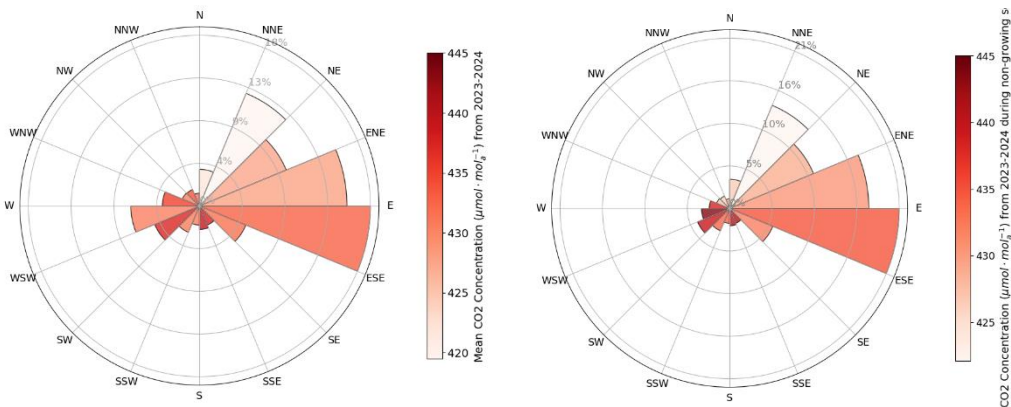
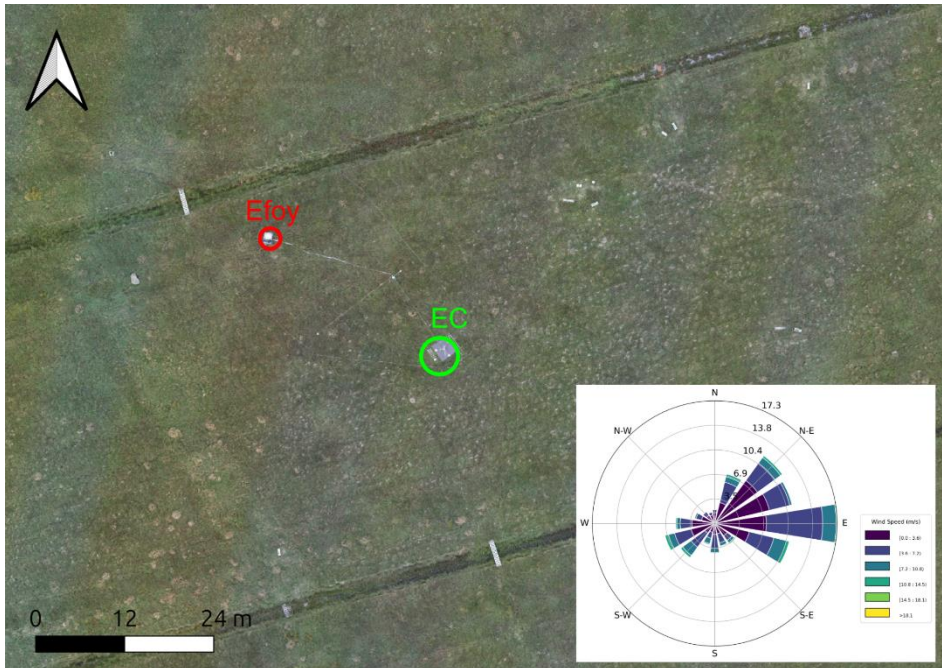
Specific comments:

1. 1.140 says a methanol fuel-cell generator was used to power the site. These fuel cells emit CO₂, which could have been measured by the gas analyzer and confounded the study results. I am concerned that the emissions from the fuel cell would have increased the measured NEE especially during the non-growing season when the generator was likely most used and the ecosystem fluxes would have been small and this period is one of the main focus points of the paper. Where was the generator placed relative to the gas analyzer? One solution could be to exclude EC measurements from when the wind came from the direction the fuel cell was located in (e.g. see Dunn et al 2007, *A long-term record of carbon exchange in a boreal black spruce forest*)

Thank you for pointing this out which we agree is very important to be explained in the manuscript. For selecting the location of the EFOY fuel cell we carefully considered the site's prevailing wind directions to ensure the EFOY fuel cell would not interfere with the Eddy Covariance (EC) system measurements. The drone imagery in Figure 1a confirms the fuel cell is located to the northwest (NW) of the EC system, a direction from which the wind rarely blows.

Furthermore, potential pollution from the fuel cell is avoided. This is supported by our 2023–2024 dataset (Figure 1b), which shows minimal CO₂ concentration originating from the NW sector. Figure 2c further confirms this, showing that even during the non-growing seasons of 2023 and 2024, CO₂ contributions from NW remain negligible.

This important point has been mentioned in the main text of the paper (l. 140), and the plots have been added to the Appendix (Appendix A).



2. 1. 237 please clarify what kind of cross-validation techniques were used and how much data was used for model training and validation. Please also provide information on how you checked for model overfitting.

We evaluated the model using 5-fold cross-validation on 14,610 observations (cleaned data). We trained the model on 80% of the data and tested on 20% in each iteration so every point was independently validated. To assess overfitting, we compared training and validation performance. The small R^2 gap of 0.028 (0.944 to 0.916) and a modest RMSE increase (1.113 to 1.367) indicated good generalization.

This part has been updated in the main text of the manuscript (l. 239).

3. 1. 249. An XGBoost algorithm is used to partition NEE but no information is provided about its accuracy, whether the authors checked for model overfitting or any cross-validation methods used to test its accuracy. Please provide this information in the main text or in an appendix

Thank you for your helpful comment. We evaluated the performance of the XGBoost model using 5-fold cross-validation, but it has not been mentioned in the paper. To assess potential overfitting,

we compared model performance on the training data and on unseen validation data. The results showed a modest and acceptable generalization gap, with an in-sample R^2 of 0.690 (RMSE = 0.796) and a cross-validated R^2 of 0.592 (RMSE = 0.913).

This information has been added to the methodology section (l. 258) and results (l. 369).

4. l. 260 please clarify what is meant by ‘bootstrap resampling of the entire processing chain’

This means the entire workflow (including Lloyd–Taylor fitting, XGBoost training, and flux partitioning) was repeated 30 times. In each iteration, the model was rebuilt from scratch using a randomly sampled version of the training data. This ensures that the final standard deviations account for the total uncertainty generated at every stage of the modeling process. This part has been updated in the main text to clarify the method (l. 266).

5. Section 2.4.2: ER and GPP are modelled variables which we know are primarily driven by temperature and light availability respectively. So I don’t think that performing linear regressions between these fluxes and temperature and PAR is particularly useful in the context of this study. In fact, the results from all the regression and correlation analysis are hardly discussed in the discussion section. It would make more sense to use partial regressions for example, to assess the effect of water table depth on ER while holding temperature constant. Since ER is partly modelled with an XGBoost algorithm, why not just present the results of the SHAP analysis to understand what variables beyond temperature influence ER?

Thank you for this important comment. We acknowledge the concern about analyzing modeled fluxes in relation to the same environmental drivers used in their estimation. However, our objective was not to establish these well-known relationships, but rather to explain why the NEE did not change much between two climatically different years. Specifically, we show that the small NEE response is a combined effect of the (expected) primary impact of the environmental drivers on R_{eco} (exponential response) and the more unexpected compensating effect on GPP (rates and duration). The method we used to compare the light and GPP sums (as $NDVI \times PAR_{acc}$) is a vegetation proxy closely related to intercepted light in a continuous canopy (fPAR), which typically has been found to be linearly related to GPP. We agree that these aspects should have been more clearly explained and the manuscript has now been revised to include this (l. 279 and l. 443).

Further, the SHAP analysis that the reviewer suggested has also now been included as an Appendix (Appendix B).

6. Section 3.2 I don’t think it’s necessary to describe all the weather data in so much detail, given that it is already presented in Figure 3. I suggest shortening this section to give a general overview of the weather in both years

Thank you for your suggestion. As this is the first paper from this study site, we initially described its characteristics in detail to provide a reference for future studies. However, we agree that this section can be shorter, and it has been revised accordingly in the main text (Section 3.2).

7. Similarly, I suggest removing some of the numbers written in Sections 3.4, 3.5 and 3.6 to help make the text less dry and more easily readable. I don’t think that much detail is needed in the main text when the data is already presented in figures and tables

Thank you for your suggestion, the text has been revised in the mentioned sections (Sections 3.4, 3.5 and 3.6).

Technical corrections:

1. l. 223 change ‘power outings’ to ‘power outages’
The word has been changed to ‘power outages’ (l. 226)
2. l. 236 this sentence can be removed, it is repetitive

The sentence has been removed.

3. l. 295 the maximum footprint would be almost infinite, I think you mean the 80% footprint remained within the study area

Thank you for your suggestion, the maximum footprint has been changed to 80% footprint (l. 302)

4. l. 356 'within the expected range', please provide a citation for this

A reference has been added to the text (l. 356)

5. Figure 3. please make sure the plot legends don't cover the time series

The figure has been updated (Figure 3)

6. Figure 4. please decrease the size of the plotted points so it's easier to see the individual data points

The figure has been updated (Figure 4)

7. Figure 5. I suggest to remove the dots indicating the peak magnitudes, they just look odd. Please also explain what the shading around the lines represents

The figure has been updated, and the dots have been removed. The caption has been updated to explain the shading around the lines which represent the 95% confidence intervals (Figure 5)

8. Figure 6. Instead of presenting the cumulative NEE on the same plot as the time series, I suggest to plot it in a separate subplot, with both years on top of each other so they can be compared. I suggest removing the cumulative GPP and ER, they don't add much useful information

Thank you for your suggestion, the figure and the caption have been updated accordingly (Figure 6)