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.

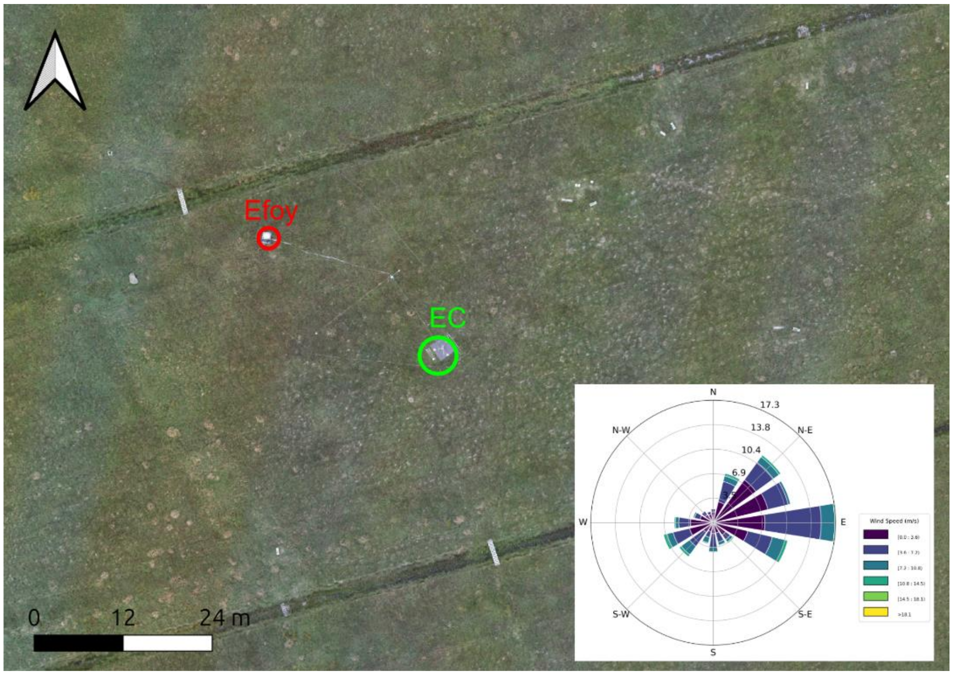
Specific comments:

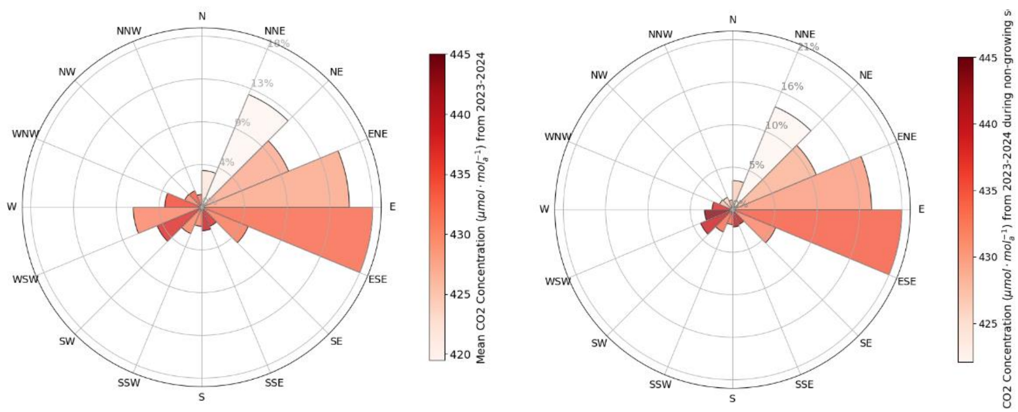
1. l.140 says a methanol fuel-cell generator was used to power the site. These fuel cells emit CO2, which could have been measured by the gas analyzer and confounded the study results. I am concerned that the emissions from the fuel cell would have increased the measured NEE especially during the non-growing 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 CO_2 concentration originating from the NW sector. Figure 2c further confirms this, showing that even during the non-growing seasons of 2023 and 2024, CO_2 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. l. 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. l. 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 ER (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 × PAR_{acc} biweekly sum) 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)

Response to reviewer 2

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

This study used eddy covariance to assess the carbon balance of an unmanaged drained peatland in Iceland. The partitioned carbon fluxes (gross primary productivity and ecosystem respiration) were analyzed alongside environmental drivers to understand processes of carbon uptake and release in this system. The two-year sampling period allowed them to capture processes under both dry and warm conditions, and wet and cool conditions.

This research will have important impacts for both scientific inquiry and national reporting. This study adds to the literature on the influence of water level on NEE, and how changes in GPP can be compensated by changes in ecosystem respiration. It also highlighted the need for yearly emission monitoring, especially in regions with mild temperatures during the non-growing season. This study will also support updating emission factors for unmanaged drained peatlands in Iceland to better align with current research.

This paper was well prepared and clearly written. I propose minor revisions. Below I listed out potential areas for improvement.

We sincerely thank the reviewer for their positive assessment of our manuscript, as well as for their constructive suggestions. We appreciate the opportunity to address the specific points raised, which have helped us further refine the clarity and quality of our work. Please find our detailed, point-by-point responses to all comments below.

Line 118: How far away was the weather station from the study site?

The Hafnarfjall station is ~10km away and the Neðra-Skarð is ~5 km away and the data showed strong correlation with the measured data by the Eddy Covarinace system in our site. This information has been added to each station's information (Table 1).

Line 145: Was soil moisture assessed at 10 cm below the surface, or is this an average soil moisture across the top 10 cm? Additionally, were any calibrations performed on it for organic soil?

The soil moisture was measured at 10 cm depth, representing a localized measurement around the probe rods rather than an average across the top 10 cm. No site-specific calibration for organic soil was performed; and the factory calibration for organic soil was used. The text has been updated to include this information (Line 146)

Line 295: Can you add in a comment here about whether the dominant vegetation characteristics changed after raising the tower and increasing the footprint area?

The dominant vegetation characteristics did not change after increasing the footprint and the area is very homogeneous. This information has been added to the text (Line 303).

Line 302: Can you clarify this. You first say that there was no discernible spatial trend in depth variation, and then you say that deeper peat was found in certain sections, and shallower peat in other sections.

Thank you for highlighting this important point. The statement regarding "no discernible spatial trend" was intended to justify the choice of the interpolation method (IDW), which emphasizes local variation between measurement points rather than assuming a broad, systematic gradient across the site. The subsequent reference to deeper and shallower peat explains patterns observed in the interpolated surface.

We agree that this distinction was not clear in the original text and have revised the manuscript to make it more clear (Line 309).

Figure 4: Many of the black points are hidden behind the blue points. Is it possible to make the blue points slightly transparent, or some other modification to improve readability?

The figure has been updated to enhance the visibility of the black points. (Figure 4).

Table 3: Can you include how many days were in the growing and non-growing season for each year in the table caption? This will help readers better interpret the data presented.

The numbers of days have been included in the caption of the table (Table 3).

Figure 7: I suggested mentioning that your weekly aggregated fluxes were computed using non-gap filled data in the figure caption.

The caption of the figure has been updated to include this additional information (Figure 7).

Line 454: Do you mean exposure to aerobic microbial decomposition?

Yes, thank you for your suggestion, we have added this detail to the text (Line 450).

Figure 7: I think it is the same Reco vs WL plot in this figure, and in Appendix B2. But the R2 and p-values are slightly different in the main text versus appendix.

Thank you for your helpful comment. The plots in Appendix B2 were mistakenly generated using gap-filled data rather than observed data while the plot in the main text is correct. These have now been replaced with plots based on observed data, and the text has also been revised accordingly (Appendix B2).

Section 4.1: Can you include some discussion of how your GPP values compare to other unmanaged drained peatlands? Your work is contributing to EFs for unmanaged peatlands in Iceland. Is the vegetation at this site representative of conditions commonly found?

The discussion has been revised to include a comparison of GPP and R_{eco} between our site and other unmanaged drained peatlands in Iceland. Our results showed that the annual GPP (−7.85 to −8.8 t CO_2-C ha^{-1}) and R_{eco} (12.2 to 12.7 t CO_2-C ha^{-1}) observed here were somewhat higher than most chamber-based estimates from other Icelandic sites (GPP: −4.3 to −7.4 t CO_2-C ha^{-1} and R_{eco}: 7.19 to 12.3 t CO_2-C ha^{-1}) likely reflecting the broader spatial footprint and continuous temporal coverage of the EC method.

Additionally, we have clarified the representativeness of our study site within the regional context of unmanaged drained peatlands. These ecosystems are characterized by a shift toward graminoid-dominated communities and are classified as "Grassland on organic soil" in the Icelandic National Inventory Report (Lines 467 and 483).

Line 525: Have other studies also seen this compensation mechanism?

Some other studies have seen high covariance between R_{eco} and GPP which has led to small interannual difference in NEE such as Wohlfahrt et al. (2008). This reference has been added to the text (Line 533).

Line 537: Can you clarify if this statement is based on interpretation of the negative linear relationship between Reco and WL, or if you tested if fluxes were significantly higher in 2023 compared to 2024. "In fact, Reco showed a significantly enhanced response to lower WL."

Thank you for requesting this clarification. Our statement was primarily based on the relationship observed between R_{eco} and WL. However, interannual comparisons also confirm that overall fluxes were significantly higher in 2023 compared to 2024. Specifically, the median R_{eco} in 2023 (4.087 µmol m^{-2} s^{-1}) was significantly greater than in 2024 (2.751µmol m^{-2} s^{-1}; Mann-Whitney U test, $p < 0.0001$). This information has been added to the text (Line 446).

Line 538: Can you justify this line a bit more. "The authors concluded that the mean annual effective water-table depth represents the overwhelmingly dominant control on CO2 fluxes in these ecosystems." By CO2 fluxes do you mean NEE, or GPP or Reco? Earlier you show the WL did not have a significant effect on Reco is 2024, but did in 2023. You also say that NEE is similar between years.

Thank you for pointing this out. That sentence was intended to reflect the findings of Evans et al. (2021), but we agree the phrasing was unclear. In our study, this effect appeared to be threshold-dependent, occurring only in 2023 when the WL dropped below a critical point. We observed a significant relationship between WL and R_{eco} during 2023, as higher temperatures and reduced precipitation led to deeper WL and greater exposure of the peat profile to aerobic conditions. While WL was not the primary driver of the overall higher R_{eco}, it had secondary influence. Furthermore, NEE remained generally similar between the two years, likely due to a compensatory mechanism where the conditions simultaneously enhanced GPP. The text has been revised to make the explanation clear (Line 545).