#Response to RC1 :

We thank the reviewers for their careful reading of the manuscript and for their constructive and insightful comments, which helped improve the clarity and quality of the paper.
Please note that several figures referenced in the reviewer comments, including Figures 4 and 6, have been updated to the final version for improved clarity and consistency. These updates do not affect the results, analyses, or discussion presented in the manuscript.
The lines shown in **bold** refer to the revised version of the manuscript, not to the version with underlined changes nor to the original version.

General comments :

First, I think that all three key objectives of the study would benefit for more detailed analyses. For this, you could consider splitting the study into two manuscripts – one focussing on the evaluation of the new *Sphagnum* PFT and one on the long-term reconstruction of NEE dynamics and the identification of their environmental controls.

We thank the reviewer for this suggestion. After careful consideration, we decided to retain a single manuscript to maintain a coherent presentation of the study. However, the validation of the Sphagnum PFT and the analysis of NEE dynamics have been substantially enhanced in response to reviewer comments. In particular, six figures have been significantly updated:

- Figure 4 now includes an analysis of slopes, p-values, and the standard deviation of NEE across the three periods studied.
- Figure 6 has been revised to provide a finer analysis of seasonal contributions to NEE through the distribution of SHAP values, as explained in a previous comment.
- Figure 8 now also includes, as supplementary material (Figure A10), the probability density functions (PDFs) of GPP and ER.
- Validation figures (Figure A4-6) comparing ISBA model outputs to the statistical model have been updated to reflect the actual mixed vegetation composition of the site.

These improvements strengthen both the evaluation of the Sphagnum PFT and the reconstruction of long-term NEE dynamics, providing a more detailed and comprehensive analysis within a single, coherent manuscript.

Regarding the first research objective, I have some concerns about the model validation. As you mention yourself in the discussion section of the manuscript, the model results are validated against the results of a statistical model that is based on monthly in-situ data obtained from chamber measurements. First, it would be helpful to include some information about the spatial distribution/replication of chamber measurements in the manuscripts to give an indication of their spatial representativeness (even thought this information is given in Garisoain et al., 2024).

We thank the reviewer for this comment. In the revised manuscript, we have clarified the methodology regarding water table monitoring and the spatial distribution of chamber measurements.

L80-83 : « *Piezometer wells are 50 mm diameter PVC tubes distributed across the peatland to cover the full spatial extent of the site (Figure 1 from Garisoain et al., 2024), and their* *placement corresponds to the locations where chamber measurements were conducted. Water table depth (WTD) data were recorded at 1-hour intervals, and the mean value from the nine piezometers is used throughout the study.* »

We have also changed subsection « Carbon Fluxes » **L94-100** :
« *These time series were derived from statistical models based on monthly CO2 flux measurements using the static chamber technique. The use of the statistical model allows reconstruction of daily fluxes, enabling direct comparison with the ISBA model outputs at the same temporal resolution. The reconstructed fluxes are considered spatially representative of the peatland due to the coverage and replication of the chamber measurements. In the following sections, these reconstructed datasets are used as a reference for model validation, and readers should note that the validation is performed against the derived statistical model rather than the raw measurements.* »

Second, while six years is a comparatively long time series of validation data, I am concerned about the low temporal resolution (monthly) of the in-situ data and the consequential use of a statistical model to reconstruct daily flux dynamics for validation of the IBSA model. I agree that it would be highly beneficial to instead or additionally use an eddy covariance data set or a chamber data set with higher temporal resolution for the model validation.

We thank the reviewer for this important comment. We fully agree that higher temporal resolution data, such as eddy covariance measurements or more frequent chamber observations, would provide additional confidence for model validation. In the present study, we therefore aimed to make the best use of the available data: although the chamber measurements are available at a monthly resolution, the six-year duration of the dataset is relatively long, and the statistical model allows reconstruction of daily fluxes for direct comparison with ISBA outputs.
We have clarified in the revised manuscript that the validation relies on the reconstructed statistical model rather than on raw measurements, and that caution is required when interpreting results at daily timescales. Importantly, over the six-year period, both approaches exhibit comparable orders of magnitude and similar interannual variability in carbon fluxes, and consistently identify the ecosystem as a carbon source in 2022.
Although the statistical model is associated with larger uncertainties, it is constrained by satellite-derived phenological information that is not explicitly represented in ISBA. The overall consistency between the two approaches therefore lends confidence to the statistical reconstruction and supports the main objective of this study, which is to investigate long-term carbon dynamics. Future work would indeed benefit from high-frequency flux measurements to further strengthen the evaluation of both carbon and water cycle processes.

Furthermore, I think that for model validation the in-situ data (the statistical model results) should also be compared to the ISBA model results using the actual vegetation distribution of the peatland (30% *Sphagnum*, 70% herbaceous) instead of just 100% *Sphagnum* and 100% herbaceous species.

We thank the reviewer for this suggestion. This comparison using the actual vegetation distribution of the peatland (approximately 30% Sphagnum, 70% herbaceous) has in fact been systematically carried out for all figures comparing ISBA model outputs with the statistical model, as shown in Figures A4–A6. We agree that it is important to highlight this, as it clearly demonstrates that the mixed vegetation scenario yields better performance metrics for NEE compared to the simulations with only Sphagnum or only herbaceous layers. These results have now been more explicitly referenced in the main text (L367-372) and incorporated into the discussion (see « Model validation and vegetation sensitivity » section) to emphasize the benefits of representing mixed PFTs for capturing ecosystem-scale carbon fluxes.

From your model validation it seems that the *Sphagnum* PFT improves model performance (alignment between IBSA results and in-situ data) mainly for ecosystem respiration (ll. 330-332). From my point of view this is a key result that requires a paragraph in the discussion section relating this finding to the Sphagnum PFT parameterization.

We thank the reviewer for highlighting this point. Indeed, one of the key outcomes of our study is that the inclusion of the Sphagnum PFT improves the representation of ecosystem respiration at the site. We have changed the discussion section « model validation and vegetation sensitivity » which appears now as (**L503-530**) :

« *The primary objective of this study was to evaluate the newly implemented Sphagnum PFT in the ISBA land surface model. Sphagnum photosynthetic activity is linked to water content in the top 10 cm of soil. While the model reproduces site-scale carbon fluxes reasonably well, a more detailed validation of the water cycle would require eddy covariance data and multi-site evaluations to assess parameter transferability. The aim was not to optimize parameters, but to test whether realistic behavior could be reproduced at a well-instrumented site using literature-derived values.*

The mixed representation, combining Sphagnum and herbaceous PFTs, accounts for contrasting responses to soil moisture. We removed the influence of soil moisture on respiration (θ(z)) for Sphagnum, allowing the moss layer to maintain microbial activity under dry conditions, while it was retained for herbaceous layers to preserve the soil moisture sensitivity of heterotrophic respiration. Although respiration from the herbaceous component alone is not improved, retaining θ(z) is consistent with previous validations of the ISBA model for herbaceous vegetation, ensuring the parameterization remains grounded in established formulations (Gibelin et al.,2008).

Importantly, the combination of PFTs captures contrasting responses to soil moisture, introducing functional diversity that likely increases the robustness of ecosystem carbon fluxes. This mechanism is reflected in the observed improvement of NEE on the mixed vegetation dataset, even if GPP and respiration alone do not always show large gains. These results also highlight the broader uncertainty in representing heterotrophic respiration as a function of soil moisture: classical formulations derived from mineral soils may not adequately capture responses in organic soils, as noted in other peatland modeling studies (Qiu et al., 2019 ; Guenet et al., 2024), emphasizing the need for further research on moisture–respiration parameterizations.

By combining PFTs with contrasting functional responses, the model captures compensatory dynamics across vegetation types: herbaceous layers respond strongly to moisture deficits, while Sphagnum maintains near surface moisture and microbial activity. This functional diversity improves site scale carbon flux estimates and suggests increased model robustness under variable hydrological conditions, which could be further enhanced by including interactive dynamics between Sphagnum mosses and herbaceous following the work of (Kim *and Verma, 1996) but also competition and coupled carbon–water processes (Lippmann et al., 2023; Heijmans et al., 2008; Wu and Blodau, 2013a; Gong et al., 2020).*

In addition, we have clarified the methodology in the revised manuscript to better describe how θ(z) was applied across vegetation types. Specifically, the changes are summarized in the Methods section: (**L284-288**)
"In this study, soil moisture did not appear to strongly constrain organic matter decomposition in near surface moss dominated layers, and we aimed to preserve contrasting drought responses across vegetation types. Therefore, θ(z) was removed for Sphagnum, while it was retained for herbaceous to maintain drought sensitivity where moisture deficits are expected to constrain belowground carbon turnover."

As an idea for future refinements of the *Sphagnum* PFT parameterization: As you already consider CH4 oxidation as a source of CO2, it would be interesting to add the (enhancing) effect of a *Sphagnum* layer on CH4 oxidation (e.g. Larmola et al., 2010; https://doi.org/10.1890/09-1343.1).

We thank the reviewer for this insightful suggestion. Indeed, incorporating the enhancing effect of a Sphagnum layer on CH4 oxidation could represent an interesting refinement of the PFT parameterization, particularly given that CH4 oxidation contributes to CO2 fluxes. While this mechanism is not currently included in our model, we recognize its potential relevance for future developments and have noted it as a possible avenue for improving the representation of peatland carbon cycling.

Regarding the second and third research objectives on long term NEE dynamics and their drivers, you hint at the growing importance of shoulder season fluxes with a lengthening growing season. I think that this finding would be worth investigating further as in-situ measurements in northern peatlands and consequently many studies are often limited to the growing season.

We thank the reviewer for this suggestion. In response, we have expanded the analysis of seasonal contributions to NEE and improved Figure 6 by including the distribution of SHAP values by season. This now provides a clearer view of the direction and magnitude of each season's contribution to NEE fluxes, including the shoulder seasons, which highlights their growing importance as the growing season lengthens.

L.459-471 are now : « *Figure 6 (a) shows that across all time periods, summer is the season contributing the most to annual NEE variability, accounting for approximately 39 % of the total contribution over the 1959-2022 period. Autumn and spring follow, alternating in second place depending on the period, with comparable contributions of around 23-25 %. Winter consistently exhibits the lowest contribution, around 11 % of the annual NEE variability. The distribution of signed SHAP values for the full period (Figure 6 (b)) further highlights the seasonal dynamics. Summer displays a wide variability, with the median and central 50 % of values slightly below zero, but with long positive tails reflecting years with strong CO2 release to the atmosphere. Autumn is generally shifted toward positive values, with extreme positive contributions in some years. Spring is centered slightly below zero, with extremes toward negative values. Winter shows relatively low variability, with a skewed distribution*

including many negative contributions but a long positive tail, indicating occasional meaningful contributions despite its overall smaller role.
Overall, these results confirm that summer dominates the interannual variability of annual NEE, but also reveal that other seasons particularly autumn and spring can contribute substantially in certain years. This supports the focus on summer NEE drivers while recognizing the importance of seasonal context. »

Minor comments:

I do not think that there is a need to always repeat all the information that is given in the figure captions also in the main text. Instead, I would recommend to just refer to the Figure in brackets at the end of a sentence.

We thank the reviewer for this suggestion. While we agree that redundancy should be minimized, we chose to briefly restate some key information in the main text to ensure readability without requiring the reader to constantly refer to the figures.

In the Materials and Methods chapter it would be helpful to clarify further which ones of the described model equations you altered and which ones were kept as they are. As the manuscript and especially the methods chapter is quite long you could consider leaving out sections 2.4.6 to 2.4.10 as the described process parameterizations were already included in the original model (if I am not mistaken) and instead refer to a respective description of the original model.

We thank the reviewer for this suggestion. We agree that, given the length of the Methods section, it is important to clearly distinguish between equations that were modified in this study and those inherited from the original ISBA formulation. In the current version of the manuscript, we already explicitly indicate throughout Section 2.4 which parameterizations were newly implemented or altered (notably the Sphagnum-specific developments) and which were kept unchanged from the baseline model. Following the reviewers' comments, we also added further clarifications regarding the treatment of heterotrophic respiration and the soil moisture limitation function $\theta(z)$. In addition, Table A1 provides a direct comparison between the new parameterization and the original one, helping the reader identify what has been changed versus what has been kept.
Regarding the suggestion to remove Sections 2.4.6 to 2.4.10, we respectfully prefer to keep it in the manuscript. Although these processes are part of the original model, we consider it important to provide the full set of governing equations used in the simulations for clarity and readability, and to make the study as self-contained and reproducible as possible. We do, however, consistently reference the original ISBA documentation/publications when describing inherited parameterizations, so that readers can consult the baseline model descriptions if needed.

L. 289: What do you mean with "reaction"? Maybe "decomposition"?

Thank you, we've rephrased : « *The last part of the equation considers oxygen availability with Cmax(z) representing the maximum carbon mass producible from available oxygen.*»

Figure 3: Which vegetation distribution was used for these model simulations? 30% *Sphagnum* and 70% herbaceous?

Thank you for the comment, all simulations shown in Figure 3 use the same vegetation distribution as the rest of Section 4, namely 70% herbaceous plants and 30% *Sphagnum* mosses, as derived from the cartography of Henry et al. (2014). This information was already stated in the experimental protocol, but to make it more visible to readers, we have added a short introductory paragraph at the beginning of Section 4 **(L405-407)**:

"This section analyzes long-term NEE dynamics and their drivers using ISBA simulations forced by the S2M reanalysis. Unless stated otherwise, all simulations and analyses presented in this section use the site vegetation distribution derived from Henry et al. (2014), i.e., 70% herbaceous plants and 30% Sphagnum *mosses."*

This clarification ensures that readers understand that the vegetation distribution is consistent across all figures and analyses in Section 4, including Figure 3.

L. 391 "a) shows a significant […]

Thank you.

l. 371 consistent and increasing accumulation, right? Maybe this could be worth mentioning.

Thank you for this suggestion. Figure 4 has been updated to include the analysis of slopes, p-values, and the standard deviation of NEE across the three periods studied, which allows a clearer assessment of the consistent and increasing accumulation highlighted by the reviewer. The figure now presents the data more clearly and supports the discussion of temporal NEE dynamics. **(L409-427)**

« Figure 4 illustrates (a) the annual Net Ecosystem Exchange (NEE) and (b) the cumulative NEE from 1959 to 2022. The lowest annual NEE is modelled in 2011 (−171 gC.m^{-2}.yr^{-1}), while the highest value occurs in 2022 (122 gC.m^{-2}.yr^{-1}).

Panel (b) shows a long-term decrease in cumulative NEE, indicating that the ecosystem acts as a net carbon sink over the entire study period. Piecewise linear trends computed for three successive 22 year periods (1959–1980, 1980–2001, and 2001–2022) reveal a progressive intensification of carbon uptake, as evidenced by increasingly negative slopes of cumulative NEE. The comparison of slopes between periods indicates that this intensification is not linear through time. The strongest increase in carbon sequestration occurs between the first and second periods, while the rate of intensification decreases after the early 2000s, suggesting a slowdown in the acceleration of the carbon sink despite continued strengthening.

Interannual variability, quantified by the standard deviation of annual NEE and reported as text annotations for each period, shows a marked temporal evolution. Variability is highest during the early period (1959–1980), decreases substantially during the phase of strongest sink intensification (1980–2001), and slightly increases again during the most recent period (2001–2022). This pattern suggests that the period of rapid carbon sink strengthening *coincides with a more stable interannual behaviour, whereas recent decades combine sustained carbon uptake with a renewed increase in year-to-year variability.*

Overall, despite substantial annual fluctuations, the long term signal remains robust and highlights a persistent accumulation of carbon by vegetation over the past six decades. »

Figure 5: I think that the seasonality in NEE, GPP, and ER would be easier to see here if they were not displayed as cumulative fluxes. Only ll. 388-390 refer to the cumulative representation – Figure 5 could therefore be moved to the appendix and be replaced by the respective annual time series.

We thank the reviewer for this suggestion. We agree that displaying seasonal fluxes as time series could provide a different perspective. However, presenting cumulative fluxes in Figure 5 has several advantages. First, it avoids redundancy with Figure 1, which already shows the seasonal dynamics of NEE, GPP, and ER as annual time series. Second, cumulative fluxes provide complementary information on the total carbon balance over time, which is particularly relevant for peatland carbon cycle studies, where cumulative fluxes are commonly reported. Therefore, we decided to retain Figure 5 in its cumulative form, as it adds meaningful insight beyond the seasonal time series.

Figure 6: Please add a definition of the seasons here.

Thank you, the definition of the seasons have been added (**L458**) : « *(a) Seasonal contributions to annual NEE across four time periods : 1959-1980 in blue, 1980-2001 in orange, 2001-2022 in green, 1959-2022 in grey. Each bar represents the relative importance of a season in explaining the total NEE, as determined by Shapley regression coefficients. Seasons are defined as winter (December–February), spring (March–June), summer (July–August), and autumn (September–November). (b) Distribution of SHAP values by season for 1959–2022. Positive or negative SHAP values indicate the direction of each season's contribution to annual NEE, showing whether a season increases or decreases the yearly flux. »*

l. 408: Remove one "seem to"

Thank you for noticing. As Figure 6 has been updated, "seem to" has been removed and no longer appears in the manuscript (**L459-471**). « *Figure 6 (a) shows that across all time periods, summer is the season contributing the most to annual NEE variability, accounting for approximately 39 % of the total contribution over the 1959-2022 period. Autumn and spring follow, alternating in second place depending on the period, with comparable contributions of around 23-25 %. Winter consistently exhibits the lowest contribution, around 11 % of the annual NEE variability.*
The distribution of signed SHAP values for the full period (Figure 6 (b)) further highlights the seasonal dynamics. Summer displays a wide variability, with the median and central 50 % of values slightly below zero, but with long positive tails reflecting years with strong CO2 release to the atmosphere. Autumn is generally shifted toward positive values, with extreme positive contributions in some years. Spring is centered slightly below zero, with extremes toward negative values. Winter shows relatively low variability, with a skewed distribution

including many negative contributions but a long positive tail, indicating occasional meaningful contributions despite its overall smaller role.
Overall, these results confirm that summer dominates the interannual variability of annual NEE, but also reveal that other seasons particularly autumn and spring can contribute substantially in certain years. This supports the focus on summer NEE drivers while recognizing the importance of seasonal context. »

Figure 7 (caption): I don't understand what you mean by "bold colormap" and "shaded colormap".

Thank you for pointing this out. We have revised the caption of Figure 7 (**L472**).
« (a) Summer net ecosystem exchanges (in red) compared to annual (in blue) from 1959 to 2022, as simulated by ISBA … »

I think that Figure 8 together with ll. 443-453 could be moved from the Discussion to the Results chapter.

We thank the reviewer for this suggestion. Following your advice, Figure 8 and the corresponding text (**L 485–499**) have been moved from the Discussion to the Results section.

The Discussion chapter could be strengthened by more specifically discussing the key findings of the study. In it's current form a large part of the discussion refers to shortcomings of the study and future research objectives.

We thank the reviewer for this suggestion. In response, the Discussion section has been revised and strengthened to more directly highlight the key findings of the study. In particular, we now emphasize the role of the Sphagnum PFT in improving the representation of heterotrophic respiration and the effects of combining Sphagnum and herbaceous vegetation, as well as how these findings enhance the understanding of ecosystem-scale carbon fluxes.

l. 473-480: I don't quite understand the introduction of VPD here. First you state that VPD is important to consider (without a reference) but in the next sentences you show that VPD does not have a significant effect on NEE at your research site or in northern peatlands in general. Maybe rephrasing this paragraph can clarify your point.

Thank you, we've rephrased this paragraph with (**L544-551**) :
« Vapor Pressure Deficit (VPD) is generally an important factor to consider, particularly for vegetation development, as it influences both GPP and plant transpiration (Fu et al., 2022). However, at the Bernadouze site, VPD is low and exhibits little variation (Figure A9 (a)), suggesting it has a limited effect on NEE. Furthermore, VPD is strongly correlated with temperature, which captures much of its potential influence. A recent study in northern hemisphere peatlands (Chen et al., 2023) also indicates that VPD has a neutral effect on vegetation and does not necessarily induce stomatal closure in vascular plants. The humid conditions at the site, along with the presence of bryophytes, help satisfy atmospheric water demand. Overall, air temperature and water table depth remain the primary drivers explaining NEE variability. »

Figures A4-A6: It would be interesting to add another column of plots comparing the results of the statistical model with the ones from the ISBA model assuming 30% *Sphagnum* and 70% herbaceous species.

We thank the reviewer for this suggestion. Following your advice, an additional column of plots has been added to Figures A4-A6, showing the comparison between the statistical model results and the ISBA simulations using the mixed vegetation distribution of 30% *Sphagnum* and 70% herbaceous species. These new plots highlight that the mixed vegetation generally improves the representation of NEE compared to simulations with a single PFT.

Figure A5: Please add to the figure captions which years are included in the figures.

Thank you, we have updated the captions :
« Daily annual cycle (2017-2022) of (a) Gross Primary Productivity, (b) Ecosystem Respiration, (c) Net Ecosystem Exchange, (d) Water Table Depth from the statistical model in black, the ISBA Sphagnum model in orange, the ISBA herbaceous model in blue, and the ISBA mixed vegetation in green. »

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Fu, Z., Ciais, P., Prentice, I. C., Gentine, P., Makowski, D., Bastos, A., Luo, X., Green, J. K., Stoy, P. C., Yang, H., and Hajima, T.: Atmospheric Dryness Reduces Photosynthesis along a Large Range of Soil Water Deficits, Nature Communications, 13, 989, https://doi.org/10.1038/s41467-022-28652-7, 2022.

#Response to RC2 :

We thank the reviewers for their careful reading of the manuscript and for their constructive and insightful comments, which helped improve the clarity and quality of the paper.
Please note that several figures referenced in the reviewer comments, including Figures 4 and 6, have been updated to the final version for improved clarity and consistency. These updates do **not** affect the results, analyses, or discussion presented in the manuscript.
The lines shown in **bold** refer to the revised version of the manuscript, not to the version with underlined changes nor to the original version.

General comments :

There should be more care taken, however, to explain how the model is being validated against a different model of in-situ data rather than the site data in its rawest form. This often cannot be helped, but it warrants more discussion after presenting the model validation results to ensure readers are not misled.

We thank the reviewer for pointing this out. The ISBA model was validated against the statistical model derived from the in-situ chamber data because the raw measurements are monthly and sparse, while the ISBA model outputs daily fluxes. The statistical model allows reconstruction of daily NEE and associated fluxes for comparison with the model outputs. We have added a clarification in the Methods and Discussion sections to ensure that readers understand that the validation is performed against a derived statistical model rather than the raw measurements, and that this approach does not compromise the reliability of the comparison.

L94-100 are now revised as follows:
« *These time series were derived from statistical models based on monthly CO2 flux measurements using the static chamber technique. The use of the statistical model allows reconstruction of daily fluxes, enabling direct comparison with the ISBA model outputs at the same temporal resolution. In the following sections, these reconstructed datasets are used as a reference for model validation, and readers should note that the validation is performed against the derived statistical model rather than the raw measurements.* »

Formatting figures should be checked for colour-blindness suitability. If any of your figures were in black & white, they would be very difficult to read – especially in Figure 3, where the mean and maximum air temperatures are only differentiated by the axis label colours, and Figure 5, were line thickness and colour intensity are similar such that they cannot be distinguished in black and white.

We thank the reviewer for this comment. Figures 3 and 5 have been updated to improve clarity and colour-blindness accessibility. Line types and thicknesses have been adjusted, and colours have been chosen to be easily distinguishable both in colour and in black & white reproduction. We have carefully checked all figures to ensure they remain legible and interpretable under these conditions.

Line-by-line comments :

43: another citation here would be useful, unless you are citing Turunen (2003) consistently throughout the paragraph – in that case, present that work more formally to better indicate that it informs multiple lines in the paragraph.

We thank the reviewer for this suggestion. **L45-47**, we have added another citation regarding the definition and use of ARCA (Chaudhary et al., 2017) to complement Turunen (2003) and make it clear that both sources inform the discussion in this paragraph.

68: specify that 'average depth' refers to peat deposits.

We thank the reviewer for this comment. We have clarified that the average depth refers specifically to the peat deposits (**L74**): *« The peatland has an average peat depth of 2 m »*

73: it would be useful to have some indication about the positioning of the piezometers within the site – nearby specific vegetation types? Placed randomly? Placed in a grid?

We thank the reviewer for this comment. We have clarified the manuscript (**L80-83**) :
« Piezometer wells are 50 mm diameter PVC tubes, distributed across the peatland to cover the full spatial extent of the site (Figure 1 from Garisoain (2024)), and their placement corresponds to the locations where chamber measurements were conducted. Water table depth (WTD) data were recorded at 1-hour intervals, and the mean value from the nine piezometers is used throughout the study. »
Vegetation composition is described earlier in the Methods section.

76: the 'Couserans massif' phrase is unclear to those not familiar with French geography and may be worth introducing in section 2.1.

We thank the reviewer for this comment. We have clarified in Section 2.1(**L86-87**) that the Couserans massif is a mountainous region in the central Pyrenees, southwestern France, to provide geographic context for international readers.

78-79: the sentence "The vertical resolution ... carefully considered" does not make sense. Reword.
79: by 'this', are you referring to the model, the model's resolution, or something else? Be more specific.

Thank you, we rephrase with (**L88-91**) : « *The S2M model has a vertical resolution of 300 m and provides hourly outputs. The topographical features of the peatland, including altitude, slope, and aspect, were carefully accounted for to ensure that the atmospheric variables extracted for the site accurately represent local conditions. »*

81: the 'see also' phrase should be expanded to a full sentence to claim how the vegetation is portrayed in your 2023 paper's figure.

L74-78 *« The vegetation comprises species typical of both ombrotrophic areas, such as Sphagnum palustre and Sphagnum capillifolium, and minerotrophic areas, including Carex demissa and Equisetum fluviatile (Henry et al, (2014). This distribution is illustrated in Figure 1 of Garisoain et al (2023), which shows the spatial arrangement of vegetation types across the study site. »*

123: you have inconsistencies with naming equations compared to page 4. This continues throughout the paper.

Thank you. The equation refers to the equation labeled (B5) in the Appendix.

167: explain more. Vertical distribution? Is this constant throughout the simulation or an initial condition? You get into it more by line 205, but it would be better to make it clearer here too.

Thank you, we've hadded some clarifications (**L186-187**).
« The brown Sphagnum biomass (Bbrown) is uniformly distributed over the top 10 cm of the soil profile, and this vertical distribution is maintained throughout the simulation .»

225: cite the CENTURY model for thoroughness.

Thank you, see (Parton et al., 1993).

332: R_2 values below 0.5 warrants more explanation for their inclusion. Also, in the referenced Figure A4, you do not define subfigures (e) and (f).

Some r^2 values are below 0.5, but are presented to provide a complete overview of model performance across all conditions.

364: it would be useful to re-state that reported values are coming from ISBA in this section even if it seems redundant. This recurs in line 413 when discussing the dryness index – it may be useful to re-state that this is/isn't an ISBA output as it's not easy to keep track.

We thank the reviewer for this comment. We have updated the figure legends for Figures 4 and 7 to clearly indicate the origin of the data displayed, specifying which values are direct outputs from ISBA and which are derived (e.g., the dryness index), to improve clarity and help readers track the data sources.

367-369: you present the results qualitatively. It could be useful to put a number to the acceleration in NEE decline by comparing simple linear regressions for the two periods you describe. The same could be said for year-to-year variability; you could compare

average change year-to-year for the two periods. This may not be statistically significant, but will make the point more concrete.

We thank the reviewer for this constructive suggestion. Following this recommendation, we have quantified both the acceleration in NEE decline and the evolution of year to year variability using simple and transparent metrics.
Specifically, we fitted linear regressions to cumulative NEE over three successive 22-year periods (1959–1980, 1980–2001, and 2001–2022). The resulting slopes show a clear intensification of the carbon sink, with mean slopes changing from −6.7 gC m^{-2} yr^{-1} (1959–1980) to −23.2 gC m^{-2} yr^{-1} (1980–2001), and further to −31.9 gC m^{-2} yr^{-1} (2001–2022). We also examined the associated p-values of these slopes, indicate that the linear trends are statistically significant for the two most recent periods, while the trend over the earliest period is weaker. Comparison of slopes shows that the strongest acceleration occurs between the first two periods, while the increase in sink strength slows down after the early 2000s. These values are now explicitly reported and illustrated in Figure 4.
In addition, year to year variability was quantified as the standard deviation of annual NEE for each period. Variability is highest during the early period (82.7 gC m^{-2} yr^{-1}), decreases substantially during the phase of strongest sink intensification (66.8 gC m^{-2} yr^{-1}), and slightly increases again during the most recent period (72.2 gC m^{-2} yr^{-1}). Although these differences are not formally tested for statistical significance, they provide a concrete comparison of interannual variability across periods, as suggested by the reviewer.
These additions have been incorporated into the Results section (**L409-427**) and Figure 4, making the interpretation of both acceleration and variability more quantitative and explicit.

375: How did you pick the time 'several time periods' displayed in the figure? You kind of explain in line 386, but it is unclear if this selection was intentional to show this point, or if the finding occurred organically from blocking out the three periods of time blindly.

We thank the reviewer for this important clarification. The subdivision of the time series into three successive periods of approximately 22 years was defined a priori, based on minimum temporal lengths commonly used in climatology (WMO, 2017 ; https://www.agroorbi.pt/livroagrometeorologia/DocsProg/Temas&ExercíciosExtraPorCapítulo/Cap1_Introdução/Docs/WMO%20Guidelines%20on%20the%20Calculation%20of%20Climate%20Normals_en.pdf). This duration ensures a sufficient number of observations for robust linear trend estimation while reducing the influence of short-term variability. Given the 64-year length of the record, a slight overlap was allowed so that each period remained long enough for trend calculation while maintaining three comparable intervals. The periods were selected based on this criterion and data availability, rather than on the evolution of the NEE time series itself, so that differences in slopes and variability emerged organically from the analysis rather than from intentional tuning.
This clarification has been added to the manuscript to make the rationale for the period selection explicit **(L432-435).**

411: 'or contribution of 48% Figure 6' in the parentheses does not make sense. Do you mean to use the previous figure to contribute to the point that summer NEE 'drives' cumulative NEE? If so, give his its own sentence.

Thank you, we've rephrased with (**L474-477**) :

« The cumulative summer NEE tends to "drive" the cumulative annual NEE almost always sharing the same sign, except in years near equilibrium (cumulative annual NEE = 0). This relationship is supported by a strong correlation between summer and annual cumulative NEE (r2 =0.71, Figure A7) and by the fact that summer NEE contributes approximately 40 % of the interannual variability of cumulative NEE (Figure 6). »

510-511: the sentence 'Over the long term … carbon sources' deserves a citation, or more notes on where you've seen this in your data. If van der Woude et al. (2023) is the citation, you can connect the previous phrase with this one using a semicolon to make this abundantly clear.

We thank the reviewer for this comment. We have clarified this section in the manuscript to distinguish between observations from our data and relevant literature. In our peatland, greening and higher summer GPP fluxes, together with increasing contributions from spring and autumn, currently buffer summer carbon losses, although some years show a partial imbalance. To contextualize this pattern, we cite van der Woude et al. (2023), who report similar mechanisms in European forest ecosystems under extreme droughts, where compensatory processes were insufficient to maintain carbon balance. This reference is provided as a **useful analogy**, illustrating how extreme events can challenge compensatory mechanisms; it complements our observations without implying that the same effect has been directly measured in peatlands. The revised text (**L579-585**) now explicitly conveys that prolonged or intensified droughts could potentially challenge the peatland's long term carbon sink function.

Table A1: The inclusion of the value for SWI_c is confusing if not given more context. Does this mean SWI for C3 herbaceous plants? This is specified in the main body of the paper. Where did you get this value?

We thank the reviewer for this comment. The value for SWIc was removed from Table A1, as it did not add relevant information and its inclusion without context could be confusing. All necessary context for soil water content is already provided in the main text.

More nitpicky formatting notes:

Note that before the text begins, you need to capitalise the second name of the Correspondence Author.

Thank you.

20: change 'most' to 'more' to avoid superlatives.

Done. (**L21**)

27-32: consider abbreviations for the models you describe, especially those you repeat later. The phrase "developed as offline tools … framework" is awkward and could be rephrased.

Thank you, we have rephrased (**L29-31**): *« Dynamic vegetation and ecosystem models, developed as offline tools without atmospheric, climate, or carbon feedbacks, have since incorporated these processes into their frameworks. »*

40-41: I do not believe "Average" should be capitalised, and you should capitalise the components of LORCA contributing to its acronym.

Thank you.(**L44**)

49: abbreviate 'continental surface models' to CSMs in tandem with changes from lines 27-32.

Done (**L32**).

58: do not separate objective with full stops. Rather, use semicolons.

Thank you. (**L62-65**)

80: italicise all Genus and species names for plants.

Thank you.

95: fix citations - Goudriaan (1986) and Jacobs (1994).

Thank you. (**L111**)

96: it's a bit confusing to introduce eq 2 before eq 1. It makes sense for (1) to be first, so restructure the sentence to match this order.

Thank you. (**L112**)

100: 'That's to say' is a clunky phrase; use 'meaning' instead. I think it would read better for *Ci* and *CO2* to have separate units despite being the same.

Thank you. (**L115-116**)

104: you have 'et' instead of 'and'. You need an 'is' after *Ia.*

Thank you. (**L120**)

106: add *Rd* in the beginning of the phrase like you do for the previous eqs.

Thank you.

107: 'type of PFT (plant type)' is clunky. Reword – maybe 'PFT parameters'?

Thank you. (**L123**)

117: instead of a comma, connect your two references (Shi et al and Walker et al) with an 'and'.

Thank you.

121: 'Although they are not the same physical quality' is either incomplete, or should be connected to the prior statement with a comma. Maybe restate the year (1994) rather than just saying 'Jacobs'' as it's on a new page from the original citation.

Thank you. (**L134**)

124: fix citation – Gong et al. (2020).

Thank you. (**L139**)

128: you do not define ω_{opt}.

Thank you. (**L147**)

132-133: italicise *Sphagnum.*

Thank you.

136: rather than 'a day', say 'one day' or 'a timestep of one day'

Thank you. (**L154**)

142: 'SLA (constants e and f)' is ambiguous.

Thank you.

156: change 'grass/herbaceous' to 'grass/herbaceous PFTs'

Thank you. (**L175**)

158: change 'plant functional type (PFT)' to simply 'PFT' or 'the selected PFT'

Thank you. (**L175**)

162: 'B' is missing formatting. Add a comma after B_{brown}.

Thank you. (**L181**)

174-178: make this more concise; it does not flow well.

We've rephrased (**L194-199**) :
« The precipitation interception reservoir is considered negligible. Consequently, Sphagnum mosses receive water primarily through capillary action from the soil within 10 cm of the surface. Although direct interception of rainfall also contributes in reality, this process is not explicitly modeled here. Instead, we account only for the effect of capillarity by relating the water content of Sphagnum to that of the upper 10 cm of soil. Precipitation that would otherwise be intercepted bypasses the moss canopy and infiltrates directly into the soil, thereby influencing both soil and Sphagnum water content. »

193: the line would read better as: 'Below a threshold value, as the *Sphagnum* mosses dry out, the resistance of the *Sphagnum* increases linearly, allowing the retention of a minimal threshold of water in the mosses. As such, we define:' Omit space before colon.

Thank you. (**L213-215**)

198: define SWI_{sp} on line 195. You replace 'Water' with 'Wetness' on page 8 – make consistent.

Thank you. (**L225**)

206-208 : Move the sentence 'Beyond 10 cm … SWI_{sp}=0' after the sentence following.

Thank you. (**L230**)

212: no need for a new line here in my opinion.

Thank you.

228: no need for 'see', you can just cite using (Gibelin et al., 2008).

Thank you. (**L251**)

233 : the word 'set' is unclear. Reword more explicitly. Is this an initial condition ? Default ?

Thank you (**L256**).
« In our simulations, the SOC fraction in each layer is fixed at 1, reflecting the assumption of a completely organic soil. »

243 : missing subscript for O_2.

Thank you.

256 : italicise *i* and *j*.

Thank you. (**L281**)

258: fix citation - (Morel et al., 2019).

Thank you. (**L283**)

259 : remove contraction.

Thank you.

262: 'potentially *a* limiting reaction'

Thank you.

265: rather than saying gas transport by plants, you could say plant-mediated transport, which explains the abbreviation better.

Thank you. (**L292**)

267: rather than saying vegetation type, you shold say PFT to stay consistent.

Thank you.

277: no need for period after 'summation'.

Thank you.

281: fix citation – Garisoain et al. (2024).

Thank you. (**L309**)

283: add ‘the’ between ‘use’ and ‘daily’. Put a period after *dif(t).*

Thank you.

287: no need for period after ‘A3’.

Thank you.

289 : I do not believe you define the dryness index as DI explicitly anywhere. Equations 18 and 19 seem not to be well explained, even if they are simple.

We’ve changed this paragraph to be more explicit (**L309-322**) :
The dryness index is based on the work Garisoain (2024) (see Figure A3 (b)). We first define the daily soil water deficit dif(t): …

To consider only periods of positive water deficit, we define the function f(t) as:…

The Dryness Index (DI) is then calculated by integrating f(t) over the summer period: …
This formulation ensures that only days with a positive soil water deficit contribute to the DI, providing a simple and physically meaningful measure of summer dryness.

295 : you begin to introduce unnecessary extra spaces here.

Thank you.

308-309: omit sentence ‘In other words … are introduced’. You are clear enough in this paragraph without over-explaining.

Thank you.

320-321 : capitalise Section.

Thank you.

323 : two full stops here by accident.

Thank you.

326 : capitalise Carbon. In the Figure 1 below, put y-axis units in parentheses.

Thank you.

332 & 344: missing a comma before 'Figure'.

Thank you.

Figure 5: using 'minus' to mean negative sounds clunky – I would simply say 'gross primary productivity' and explain it is negative to visually demonstrate it in balance with ER to create NEE. You should omit the space between 'periods' and the colon.

Thank you. We've changed the Figure and the captions :
« Seasonal evolution of cumulated (a) gross primary productivity (shown with negative sign by convention), … »

386: omit 'the'.

Thank you.

Figure 7: omit 'the' and 'one' on either side of 'annual'.

Thank you.

411: comma after R_2 before figure A7.

Thank you. (**L476**)

444 : make singular –'the black curve represents…'

Thank you. (**L487**)

455: the opening phrase is clunky and could likely be said in fewer words.

We've rephrased and changed the discussion paragraph (**L503-530**) :
« *The primary objective of this study was to evaluate the newly implemented Sphagnum PFT in the ISBA land surface model. Sphagnum photosynthetic activity is linked to water content in the top 10 cm of soil. While the model reproduces site scale carbon fluxes reasonably well,*

a more detailed validation of the water cycle would require eddy covariance data and multi-site evaluations to assess parameter transferability. The aim was not to optimize parameters, but to test whether realistic behavior could be reproduced at a well instrumented site using literature derived values.

The mixed representation, combining Sphagnum and herbaceous PFTs, accounts for contrasting responses to soil moisture. theta(z) was removed for Sphagnu, allowing the moss layer to maintain microbial activity under dry conditions, while it was retained for herbaceous layers to preserve the soil moisture sensitivity of heterotrophic respiration. Model dynamics further support this choice, as herbaceous layers withdraw more water from the topsoil than Sphagnum, increasing the sensitivity of soil respiration to moisture fluctuations. Although respiration from the herbaceous component alone is not improved, retaining theta(z) is consistent with previous validations of the ISBA model for herbaceous vegetation, ensuring the parameterization remains grounded in established formulations (Gibelin et al., 2008)
*Importantly, the combination of PFTs captures contrasting responses to soil moisture, introducing functional diversity that likely increases the robustness of ecosystem carbon fluxes. This mechanism is reflected in the observed improvement of NEE on the mixed vegetation dataset, even if GPP and respiration alone do not always show large gains. These results also highlight the broader uncertainty in representing heterotrophic respiration as a function of soil moisture: classical formulations derived from mineral soils may not adequately capture responses in organic soils, as noted in other peatland modeling studies (Guenet et al., 2024), emphasizing the need for further research on moisture/respiration parameterizations. *

By combining PFTs with contrasting functional responses, the model captures compensatory dynamics across vegetation types: herbaceous layers respond strongly to moisture deficits, while Sphagnum maintains near surface moisture and microbial activity. This functional diversity improves site scale carbon flux estimates and suggests increased model robustness under variable hydrological conditions, which could be further enhanced by including interactive dynamics between Sphagnum mosses and herbaceous following the work of Kim and Verma (1996) but also competition and coupled carbon/water processes (Lippmann et al., 2023; Heijmans et al., 2008; Wu and Blodau, 2013a; Gong et al., 2020).»

494-495: restructure the sentence as so –'Despite some differences in seasonal representation, both ISBA and the statistical model by Garisoain et al. (2024) agree that …'

Thank you. (**L565-566**)

520-521: italicise *Sphagnum*.

Thank you.

Figure A6: 'Hourly' misspelled.

Thank you.

Table A1: 'functional' misspelled in title. No need for a space between Notes 1 and 2. Semicolon would be better after D_{xmax} = 0.4. You haven't properly formatted g_m or D_{max}. Page number for p.38 is the wrong font.

Thank you.

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