Discussion of "Seasonal dynamics of vegetation effect on peatland surface energy balance"

By Flemming et al.

We would like to thank the editor and the reviewer for assessing our manuscript. In the following we provide a point-by-point response to the reviewer's comments. Reviewer's comments are marked in black and authors' responses are marked in blue. Line numbers correspond to the revised version with tracked changes.

Author Response to Review #1

The manuscript "Seasonal dynamics of vegetation effect on peatland surface energy balance" by Flemming et al. report energy fluxes and look into their drivers at a restored peatland using 1 year of eddy covariance data. Despite the topic is interesting, the manuscript is very poorly written with confusing justification on hypotheses, weak analyses to support tests of hypotheses, and messy abstract, introduction, and discussion. Additionally, the results are quite trivial that radiation is the most important driver of LE and VPD can additionally exert impacts on LE via biotic controls are very well known. Furthermore, there's no discussions on limitations of analyses while the authors only used 1 year of data and didn't consider lagging effects. I am afraid to say the manuscript has many fundamental issues that the authors have to address. I was not able to finish reading the entire manuscript because all the confusions from the fundamental parts. I strongly encourage the authors to put substantial efforts into refining the manuscript to make it matching the quality of Biogeosciences. Here are my major comments:

Response: We thank the reviewer for their time and constructive feedback, which helped us substantially improve the manuscript. We have carefully addressed all comments and revised the manuscript to enhance clarity, analytical rigor, and the overall positioning of the study. In addition, we expanded the analysis from one year to three years, incorporating the most recent observations from the study site.

[Major comments]

1.Abstract is not well written

As the first piece to be seen for a manuscript, a nicely constructed abstract is critical. However, the current iteration has many errors and confusing spots that make it difficult for readers to get the importance of this study.

Response: We have entirely rewritten the abstract to improve the clarity, taking into account the following comments.

- The goal of this study is not clear. The authors said "energy fluxes in these ecosystems are strongly influenced by climate conditions" but wanted to "separate the influence of climate and vegetation remains challenging". If it's strongly influenced by climate conditions, why is it necessary to separate vegetation effects?

Response: We have revised the Abstract to clarify that the focus of this study is on evapotranspiration (and latent energy flux, LE) and mentioned the research gap regarding the interaction between abiotic and biotic LE drivers (lines 9-12). We wrote:

"The interaction between abiotic (e.g., atmospheric and soil meteorological conditions) and biotic (e.g., canopy conductance, gross primary productivity) drivers on LE remains insufficiently understood, particularly in the context of drained peatlands dominated by heath and dwarf-shrub vegetation."

Additionally we listed our study goals in the Abstract (lines 13-16):

"Using high frequency CO_2, H_2O, energy flux, and meteorological measurements we 1) explored the daily and seasonal dynamics of LE, 2) quantified the influence of biotic and abiotic drivers on LE, and 3) determined the vegetation physiological thresholds at which canopy conductance (gc) limits LE due to an increase in atmospheric and soil dryness."

- Linked to the previous point, by saying only GLSAR, it's unclear how it can do the separation.

Response: We have removed the model name (GLSAR) from the Abstract and explained more clearly how we analyzed the drivers of LE (lines 21-22):

"Using daily mean data, detrending of mean seasonality, and removal of time lag, the variable component of the drivers was extracted and the effect size on LE was determined."

- I can't follow the justification of jumping from surface energy fluxes to LE, how about sensible heat flux (H)? Isn't vegetation also affecting H, e.g., leaf wetness, structure (subjecting to what species), etc? And how can you claim both GPP and VPD must be jointly considered in analyses of "turbulent energy fluxes" in the end?

Response: Our goal in this study was to focus on evapotranspiration (and thus LE), but we agree that this point was not clear. In the revised version, we explained the link between evapotranspiration and LE (lines 7-8 and 48-52), and removed any misleading text regarding energy fluxes and focused the paper on LE in all sections. We have also changed the title to "Disentangling the biotic and abiotic drivers of evapotranspiration in a degraded raised bog" to make sure the focus of the paper is reflected more clearly.

- The authors specifically mentioned that abiotic drivers were evaluated using daily and seasonal patterns; however, not for biotic ones but only saying "continuous time series".

Response: In the revised version of the abstract, we have specified the biotic and abiotic drivers, and clarified the temporal resolution of all data. We wrote (lines 16-20):

“To disentangle the biotic and abiotic drivers of LE, we derived a continuous time series of canopy conductance (g_c) by inverting the Penman-Monteith equation using 30-minute LE fluxes measured with the eddy covariance method, which were also aggregated to daily values. This approach enabled us to estimate the daily and subdaily g_c alongside gross primary production (GPP) and continuous daily meteorological variables, including net radiation (R_n), air temperature (T_a), vapor pressure deficit (VPD, i.e., atmospheric dryness), groundwater table depth (WTD), and soil water content (SWC, i.e., soil dryness).”

- How did seasonal variations in vegetation productivity exert a strong influence on mean daily LE (Line 18-19)? And is vegetation productivity here GPP? And what’s the temporal scale of LE dynamics (the end of Line 19) then?

Response: We thank the reviewer for helping us address these points of ambiguity. We have clarified throughout the manuscript, including in the abstract, that vegetation productivity refers to gross primary productivity (GPP). We also specified the temporal scale of LE dynamics in lines 16-18 of the revised abstract. In addition, we clarified in lines 22–24 how GPP influences LE differently during the growing and non-growing seasons. The quantitative comparison of these effects have been omitted from the abstract due to space constraints but presented in the Results section (lines 388–417).

- How did the “nonlinear” effect determine (Line 21)?

Response: In the revised version of the Abstract we wrote:

“With increase in atmospheric dryness, canopy conductance significantly limited LE above a VPD threshold of 7.6 hPa at the subdaily scale, and at a threshold of 4.6 hPa at the mean daily scale” lines 27-28.

- Mix used of present and past tense

Response: We have revised the manuscript to ensure consistent and appropriate tense usage throughout the text.

2. Similarly, the introduction was not well structured and does not solve issues in the abstract

Response: We have taken this comment carefully into account and substantially revised the Introduction section.

- The author spent more than 50% of the introduction stressing the importance and drivers of LE, but suddenly said they would investigate energy flux partitioning (Line 53)?

Response: We have substantially revised the Introduction to improve focus and coherence. The revised version now centers explicitly on ET and LE and clearly articulates the relationship between the two (lines 48-52). We have also removed references to energy flux partitioning to avoid any misleading framing.

- The entire second paragraph (Line 40-49) contains many pieces of information but no convergence. What is the purpose of saying knowledge gap in Central European then importance of WTD, Rn, VPD, and vegetation?

Response: We thank the reviewer for their comment which helped us improve the clarity of the text. In the revised version, the current state of knowledge and the research gaps are separated by different paragraphs (lines 79-87 and lines 88-92).

- Hypotheses (b) and (c) are very confusing. So, if Rn is the most important driver, why is it even necessary to know canopy conductance can have the greatest influence on LE in spring? Readers can get confused, why did you focus on unimportant drivers?

Response: The confusing hypotheses have been removed in the revised introduction. In the revised version, we address the importance of understanding the interactions between abiotic and biotic drivers of LE (lines 94-102). We wrote:

“Peatland conservation measures, including rewetting and vegetation management practices such as tree removal, are widely implemented across Central Europe and can directly influence LE. To identify effective conservation and restoration strategies, it is therefore essential to improve our understanding of the magnitude of these effects and the interactions among the factors controlling LE. In addition to active management, anthropogenic climate change is expected to alter peatland vegetation composition, potentially promoting shifts toward more shrubby vegetation even in the absence of direct human intervention (Heijmans et al., 2013). Such vegetation shifts are facilitated by lowered water levels and increasing temperatures (Heijmans et al., 2013) both of which are expected to intensify under future climate conditions (Shekhar et al., 2024). Understanding the interactions between biotic and abiotic controls on LE is thus critical for predicting how peatland carbon and water cycling will respond to climate change and associated shifts in environmental drivers.”

- Where’s the elaboration on importance of separating climate and vegetation control on LE? This is your first sentence in the abstract.

Response: We added the importance of disentangling biotic from abiotic drivers of LE. We wrote in the Abstract (lines 8-9):

“Given the major role that LE plays in linking water, energy, and carbon cycles, it is important to understand the drivers of LE, and how they interact with one another across different time scales.

And in the Introduction (lines 69-78):

“In drained peatlands vegetation cover often shifts from mosses to heath dwarf-shrub vegetation. Vascular plants can actively regulate transpiration by opening and closing their stomata. The strategies plants have for responding to soil and air dryness vary among species (Morgan et al., 2025; Fu et al., 2022). Previous experiments in controlled environments show that transpiration in Calluna vulgaris

and Molinia caerulea (vegetation that dominate our peatland of study) increases with rising vapor pressure deficit (VPD) under moist soil conditions (Gobin et al., 2015). These experiments also show that when soil is dry, transpiration of C. vulgaris initially increases with rising VPD, but decreases after a certain dryness point. Meanwhile M. caerulea shows no change in transpiration with increasing VPD under dry soil conditions (Gobin et al., 2015). These observations indicate variation in the strength of stomatal regulation among vegetation types, highlighting the complex role of vegetation in controlling transpiration. This variability can lead to contrasting effects on latent heat flux (LE) under conditions of increased warming and drying (Massmann et al., 2019)."

- Gerling et al., (2019) looked into soil moisture and ET but not WTD

Response: We removed the reference to Gerling et al. (2019).

3. Weakly linked hypotheses and tests

Ignoring the confusing hypotheses mentioned above, it's very difficult to find justification of how those analyses in the methods test the hypotheses. Here I put the hypotheses:

(a) LE exceeds H in summer, while the opposite holds in winter, due to higher VPD and active vegetation during the growing season

(b) Rn is the primary driver of LE, as global radiation is the main energy source for the ecosystem

(c) canopy conductance (gc) – and its influence on LE – is greatest in spring, when leaves are fully developed but stomatal regulation is still weak under low VPD conditions.

Response: We thank the reviewer for their comment. We have now streamlined the focus of the study, removed ambiguities and revised the research objectives. We clarified (in lines 14-16 and lines 103-105) that our research objectives were to:

"1) investigate the diurnal and seasonal dynamics LE measured directly using the eddy covariance (EC) method, 2) quantify the relative influence of biotic (GPP, g_c) and abiotic drivers (R_n, VPD, T_a, WTD, SWC) on LE, and 3) identify physiological thresholds at which canopy response to environmental conditions constrains LE."

- The authors first surprisingly added 2 sentences (Line 131-132) on testing VPD control on gc. What's the purpose? Influence of gc on LE is not confirmed at all.

Response: We thank the reviewer for their comment. This section is now entirely revised. We added a paragraph in the introduction (lines 69-78), where we introduce different transpiration strategies of the main species found in the peatland of study, and explained the link between g_c and LE in lines 76-78.

- In Section 2.6, the final analysis, the authors said they examined the biotic and abiotic variables influencing LE by including Rn, GPP, VPD, Ta and WTD. Then

where to test the link of gc to LE? And GPP is the only biotic factor, why spending that many efforts to obtain gc?

Response: We have now clarified in the Introduction that both g_c and GPP were taken into account as the biotic variables influencing LE (lines 84-87, line 104).

Given that g_c is derived from LE measurements, the limitation it exerts on LE is assessed, by investigating whether drier soil or atmosphere limit g_c which would in turn reduce LE. This approach, which was selected to avoid any statistical circularity, is now explained in lines 265-268.

4. Messy uses of abbreviations

The authors either defined an abbreviation but not using it consistently or randomly use abbreviations without definitions. Here are some examples, and I don't think it's reviewer's task to do this typesetting, and it's greatly disrupting my reading.

Line 33: You just acronymized transpiration

Line 56: You've defined EC. Check all the following lines (e.g., 74, 76, …)

Line 85: Subscript 2 and what's GPP?

Table 1: What's PAR?

Line 98: You've defined P

Line 117: You've defined evaporation

Line 119: You've defined precipitation

….

Response: We have revised the manuscript carefully and corrected all typesetting mistakes. PAR is now removed from Table 1.

5. The manuscript was not even finished properly

Why is there only one subsection (4.1) and why aren't discussions on other hypotheses in independent subsections? Why is the overall conclusion (Line 358-361) ended so casually? And why is it included in subsection 4.1 (Drivers of latent energy)?

Response: We have substantially revised the Discussion and structured it into several subsections to improve its readability. The Discussion is now divided into 4.1) Energy balance closure and data quality, 4.2) Temporal dynamics of latent heat flux, 4.3) Drivers of latent heat flux 4.4) Limitations of modelled canopy conductance.

We also added a Conclusion section at the end of the manuscript (Section 5).

6. Missed mentioned limitations

Do you this your findings will stand when another year of data is included? Where could be the limitations? Do you think lagging effects can be significant, why and why not?

Response: We now used three years of data (2023 to 2025) that became available since this paper was originally prepared. Accordingly we updated all analysis, presentation of results (Figures and Tables) and the discussion of results based on the updated analysis, and substantially revised the text.

Additionally, in the Discussion, we addressed potential legacy effects and clarified the limitations associated with interpreting changes in quantified effect sizes following extreme events (lines 550-555). We also discuss how interannual and seasonal variability across different or additional years of data may influence the estimated effect size of R_n (lines 529-533).

[Minor comments]

Line 11: What does d mean here? … the periods “d” when …

Response: “ d” was a typo and was removed when the abstract was rewritten.

Line 24: SOC is not needed, only used once

Response: We removed the abbreviation of soil organic content.

Line 29-30: This should go with Line 25-26

Response: We moved this statement from the revised manuscript.

Line 33: italicize “Betula spp.”

Response: This part is removed in the revised Introduction. We made sure all mentioned species are italicized.

Line 41: What’s LE? Not defined yet in the main text.

Response: We did check all abbreviations in the text to eliminate this and similar errors.

Line 144: “two separating the growing season (March 15 to October 15) from the non-growing season” reads very awkwardly. So it’s “2 growing seasons” or “2 seasons with 1 growing season and 1 non-growing season”?

Response: We revised this statement to make it clear. It now reads:

“Three models were fitted: one covering the entire period, from start of 2023 to end of 2025, a second model covering the growing season, and a third model covering the non-growing season,” (lines 257-258).

Author Response to Review #2

Flemming and others study heat fluxes in a temperate peatland. The study makes some interesting points but needs heavy revision before the analysis is publishable.

Response: We thank the reviewer for their time and their constructive feedback. We carefully considered all comments and substantially revised the manuscript to improve clarity, analytical rigor, and the position of the study. In addition, we extended the analysis from one year to three years, using the most recent observations from the study site that became available since this paper was first prepared.

The authors are basically asking when a temperate peatland shifts from being energy limited to water limited, but use a nonlinear equation (3) after having averaged data to daily time steps, which introduces a Jensen's inequality. The ecosystem limitation index (ELI) might be more useful to explore (https://agupubs.onlinelibrary.wiley.com/doi/10.1029/2019JD031672, https://www.nature.com/articles/s41558-022-01403-8)

Response: We thank the reviewer for this comment. In the revised version of the manuscript, we modeled canopy conductance (g_c) at both daily and 30-minute time scale (new Figures 9, 10, and 11), and added text to discuss the strengths and limitations of each (section 4.4, lines 574-585). g_c at a 30-minute temporal resolution had substantially more outliers and was filtered for the time period between 8:00h and 16:00h to remove time of day when vegetation is less photosynthetically active (lines 221-224). Additionally outliers were removed with a standardized approach following Rogiers et al., (2005) (lines 218-221).

In order to further improve the modeling of g_c at both daily and subdaily timescales, we integrated the often-neglected soil heat flux (SHF) component in the Penman-Monteith equation (line 196, Equation 2).

The third objective of the study was changed to "3) identify physiological thresholds at which canopy response to environmental conditions constrains LE." (line 105) to make the goal of the study clear.

We added a section titled "4.4 Limitations of modeled canopy conductance" in which we discussed the limitations of using 30-minute daily mean values for g_c (lines 574–585). This section also includes a discussion on Jensen's inequality which now addresses the point raised by the reviewer.

Some opportunities to explore high frequency flux partitioning to explore transpiration and evaporation separately (https://www.sciencedirect.com/science/article/pii/S0168192321004767) were missed but likely less important if the question focuses more clearly on the shift between energy and water limitation. The problem is that ecosystem and canopy conductance are frequently confused; the analysis as written deals with the former rather than the latter, albeit with careful high frequency flux partitioning the canopy term can be isolated.

Response: We thank the reviewer for this comment. This is indeed an important aspect which we have already considered. Ecosystem surface and canopy conductance are different entities and should not be confused. In order to minimize the influence of surface evaporation from ET, we restricted the analysis of g_c to days without a precipitation event and less than 2 mm precipitation the day before. This approach, which follows Gharun et al., (2020) was used to ensure that ET is dominated by transpiration.

We have revised the Methods section to clarify the description of our approach and the rationale behind it (lines 190-193):

"The Penman-Monteith equation includes surface conductance as a variable instead of g_c. Surface conductance is the sum of g_c, soil evaporation and canopy interception evaporation. During the growing season, soil evaporation can be neglected; furthermore, data was filtered by P, to exclude interception evaporation, such that surface resistance ≈ g_c can be assumed (Matos et al., 2025; Gharun et al., 2020)."

The analysis also needs to separate the role of soil moisture vs. VPD on controlling surface fluxes as the latter may become important as things dry out apart from WTD.

Response: We thank the reviewer for this insightful comment. In the revised manuscript, we incorporated measured soil water content (SWC) into both the driver analysis and the assessment of canopy conductance, allowing us to better disentangle the respective influences of soil moisture and vapor pressure deficit (VPD) on energy fluxes. We also revised the text throughout the manuscript accordingly (in lines 20, 25, 26, 104, 167, 242-247, 260, 390-391, 400-401, 435, 442-445, 471-474, 545-554; Figures 8 and 11; Tables 2-5) to reflect these additions and clarify the increasing role of atmospheric demand under drying conditions, beyond the effects of WTD alone.

A careful read for usage and errors (like on line 11, superscripting on 102, subscript on 126, etc) would improve readability.

Response: The revised manuscript has been carefully checked for abbreviations, typos, and other editorial mistakes.

29: also methane even when (partially) drained

Response: We thank the reviewer for this comment. We intentionally kept the statement focused on the effects of anthropogenic drainage, where increases in CO_2 emissions are generally the dominant response. While CH_4 emissions can still occur under partially drained conditions, including them here would have broadened the scope of the statement and reduced its clarity. We therefore retained the focus on CO_2 dynamics (lines 60-61).

On 33, are you referring specifically to shrubification and or woody vegetation including trees in general or is there a reason why Betula in particular is noted? Or does rooting depth play a key role?

Response: In the revised version of the manuscript, we have rephrased the sentence so that it refers to shrubification and woody vegetation in general (lines 63-65).

40: I would somewhat disagree that Central European peatlands are unexplored, but upon a brief review it's true that there have been few to no studies on energy fluxes. What's more important to me is that it's described why it's important given the research that's gone on. In boreal and tropical systems.

Response: In the revised introduction, we now distinguish more clearly between the existing body of research and the remaining knowledge gaps, particularly regarding energy fluxes. We added three paragraphs that (i) summarize the current state of knowledge on peatland functioning (lines 79–87), (ii) identify the key gaps in our understanding, especially in relation to energy exchange processes (lines 88-92), and (iii) explain why addressing these gaps is important in the context of existing findings (lines 94-102).

46: this is very unsurprising as it's easy to see why peatlands are characteristically energy limited

Response: In the revised introduction, we have kept the discussion of the current state of knowledge regarding R_n brief and have also highlighted the gaps in our understanding regarding the variability of the influence of other environmental factors on evapotranspiration (lines 88–92).

80: not sure what the own data processing is meant to refer to

Response: We have corrected the typo in the revised manuscript. It now reads "data processing" (line 287).

103: there's actually a reference by Loscher et al. on this https://journals.ametsoc.org/view/journals/hydr/10/5/2009jhm1148_1.xml

Response: We thank the reviewer for their suggestion. We added this reference to support the statement regarding the psychrometric constant (line 198).

No reason to use dots in equations

Response: We have removed dots from the equations (lines 196, 202-213, 234).

What literature source does 2a come from?

Response: Dingman (2015). We added this reference to the revised manuscript (line 201). We also found a typo in Equation 2a, which was corrected in the revised version (line 202).

2e is unconventionally annotated, using e would be more common, or exp() rather than exp^superscript

Response: We have corrected the formatting of equation 2e and 2f (lines 208 and 210).

117: this isn't true because the Penman-Monteith equation is derived from the energy balance expression which includes soil and other heat fluxes. It also doesn't assume that conductance comes through the stomata because it is the result of the original Penman equation for open water.

Response: We thank the reviewer for their comment. We have rephrased the paragraph to make it clear that it refers to the assumptions of the methodology used in the manuscript. This also includes a more thorough explanation of why the selected conditions are necessary to make sure ET is closest to transpiration (lines 189-195). We have written:

"g_c was modeled in a modified way according to Runkle et al. (2014), which used the inversed Penman-Monteith equation. The Penman-Monteith equation includes surface conductance as a variable instead of g_c. Surface conductance is the sum of g_c, soil evaporation and canopy interception evaporation. During the growing season, soil evaporation can be neglected; furthermore, data was filtered for P, to exclude interception evaporation, such that surface resistance ≈ g_c can be assumed (Matos et al., 2025; Gharun et al., 2020). g_c was first calculated by inverting the Penman-Monteith equation ($g_{c,PM}$), for days where no P on this day and less than 2 mm P the day before occurred (D = {I | P(i)=0 mm P(i-1) < 2 mm}, where D is the set of days used and i is the index of the days) (Dingman, 2015)."

131: how was growing season defined?

Response: We used GPP from eddy covariance measurements, to define the start and end of the growing season. The start and end of the growing season were determined for each year by fitting a double sigmoid function to GPP fluxes. Start and end of the growing season were determined as the first maximum and last minimum of the third derivative of a double sigmoid function.

We added a new section "2.4 Growing season" to the Methods , where we explain the methodology in detail (lines 184-187). A Figure showing the results of identified growing seasons defined from GPP measurements is provided in Supplementary Fig. B1.

I don't understand in equation 3 why gc would need to be further modeled

Response: g_c needed to be further modelled because filtering for days without interception causes systematic gaps and therefore may cause a systematic bias. To prevent this bias, we modeled a continuous time series of g_c.

We added the explanation to section 2.5 (lines 225-232).

134: there are more robust ways of doing this using a nonlinear modeling approach

Response: The breakpoints are derived statistically by fitting a local polynomial regression and defining the maximum of the regression as the regime change in g_c.

To further improve the method, we also added a 95% confidence interval for the regression, using a bootstrapping approach with 1000 iterations (line 245).

We added the description of our approach in the Method sections (lines 243-247).

We also marked the breakpoints in Figures 7, 8, 10, and 11 to make them more clear.

Fig. 1 are these daily averages?

Response: In the revised version of the manuscript we present the test of the energy balance closure on a 30-min timescale, which provides a more robust estimate. This is now shown in Figure 1. The 30-min temporal resolution is now clarified in the caption of Figure 1. We also added more detailed information on how we estimated the energy balance closure, to the Methods section (lines 182-183).

Throughout, the role of VPD vs. soil moisture should be separated to understand demand vs supply controls.

Response: In the revised version of the manuscript, we also examined the relationship between g_c and SWC, using the same methodology as for VPD.

We added the new methodology in Section “2.5 Sub-daily and daily time series of canopy conductance” in lines 241-247. The results are now shown in Figures 8 and 11 and in lines 442-445 and 469-474.

We have also added SWC to the driver analysis and added this new variable in Section “2.7 Statistical analysis”, line 260. The results were added to Tables 3-5 in Section “3.4 Driver analysis” and the corresponding text (lines 390-391 and 400-401).

The results of this additional analysis are discussed in lines 545-555.

298: mismatches in observational footprints are unlikely the cause of lack of energy balance closure (on average) and advective water flow will be a non-trivial component of lack of closure in this peatland. Also, large eddies that exceed the eddy covariance measurement period are certainly turbulent with high Reynolds number. They’re just turbulent atmospheric motions that look like advection to the eddy covariance system.

Response: We did revise this statement to provide a more comprehensive explanation for the observed energy balance closure amount, incorporating the suggestions of the reviewer. We have added lines 498-506:

“In our experimental setup, part of the residual imbalance likely arose from several well-known limitations of the EC method (Mauder et al., 2024). In peatland environments, advective transport of heat and water can represent a non-negligible component of the energy balance residual. In addition, large turbulent eddies with time scales exceeding the EC averaging period may contribute to the imbalance, as these motions are not fully captured by standard flux calculations. Further sources of imbalance include low pass filtering of water vapor by the intake tube of the gas analyzer, general measurement uncertainties, unaccounted energy storage (e.g., in soil or biomass), and errors introduced during data processing (Mauder et al., 2024;

Nicolini et al., 2026). Despite these limitations, the observed closure indicates that the energy flux measurements are of sufficient quality to support robust analysis and interpretation."

337: it wasn't shown here that vegetation contributes 1/3 to LE in the present study (if this is what is being referred to here).

Response: Thank you for flagging this error. We have corrected this statement (lines 558-559).

346: from the analysis, the ecosystem conductance rather than the canopy conductance was studied. Transpiration was never partitioned from the ET data. Per the point in this passage, the relationship above about 10 hPa in Fig. 7 would be constrained by an exponentially-decreasing function. This won't be quite as apparent if the data are averaged, which they were, and I'm not sure why they were. Averaging VPD results in a Jensen's inequality and I don't know why people use daily VPD averages. In Fig. 7 and throughout the manuscript, half-hourly values should be used to understand the functional relationship between fluxes and VPD. VPD is relatively unimportant (usually below 10 hPa) until it becomes important (above that), and averaging these two things obscures the true response.

Response: Please see our response to your previous comment. We have minimized the effect of evaporation on ET by selecting periods at least 1 day immediately after a rainfall event or two days after a rainfall event of more than 2 mm day^{-1} in total. Additionally, we now also calculated g_c using the modified Penman-Monteith equation at a 30-minute resolution. To minimize energy storage effects and also to minimize higher uncertainties at night, due to the violation of the assumption that $g_s \approx g_c$, these results were additionally filtered for 10h to 16h Both results, the daily resolution and the 30-minute resolution, are now presented side by side. Both approaches are compared and discussed in the revised version of the manuscript in terms of their strengths and weaknesses (lines 574-585).

References

Dingman, S. L.: Physical hydrology, Third edition, Waveland Press Inc, Long Grove, Illinois, 643 pp., 2015.

Gharun M, Hörtnagl L, Paul-Limoges E, Ghiasi S, Feigenwinter I, Burri S, Marquardt K, Etzold S, Zweifel R, Eugster W, Buchmann N (2020) Physiological response of Swiss ecosystems to 2018 drought across plant types and elevation. Philosophical Transactions of the Royal Society B 375: 20190521, doi: 10.1098/rstb.2019.0521

Matos, K. A., Bowen, G. J., Good, S. P., and Allen, S. T.: Evapotranspiration Partitioning Across US Ecoregions: A Multi-Site Study Using Field Stable-Isotope Observations, Geophysical Research Letters, 52, doi:10.1029/2025gl115367, 2025.

Rogiers N, Eugster W, Furger M, Siegwolf R. 2005 Effect of land management on ecosystem carbon fluxes at a subalpine grassland site in the Swiss Alps. Theor. Appl. Climatol., 80, 187-203. (doi:10.1007/s00704-004-0099-7)