Discussion of “Disentangling the biotic and abiotic drivers of evapotranspiration in a degraded raised bog”

By Flemming et al.

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

Reviewer #1

General comments
The manuscript “Seasonal dynamics of vegetation effect on peatland surface energy balance" by Flemming et al. is now much improved as corrections were made and many edits were done. I appreciate the authors’ efforts. However, there’re still many major issues remaining that need further efforts from the authors. I highly recommend the authors to use this change to put more thorough thinking on manuscript organization.

Response: We thank the reviewer for their constructive assessment. In the following we provide a thorough revision according to their comments.

[Major comments]
1. Still contradictory findings/statements
First, one of the key messages is “atmospheric demand (VPD) plays a crucial role, especially outside the growing season. The finding that stomatal regulation significantly limits transpiration – even in water abundant peatlands – highlights that these ecosystems are more biologically controlled” (Line 489-492). However, the authors emphasized that it is indeed the physical relationship helping to explain why VPD shows a strong influence on LE, particularly outside the growing season when stomatal regulation is largely absent (Line 438-439). So it’s quite confusing, the effects of VPD on LE are via physical or biological pathways?

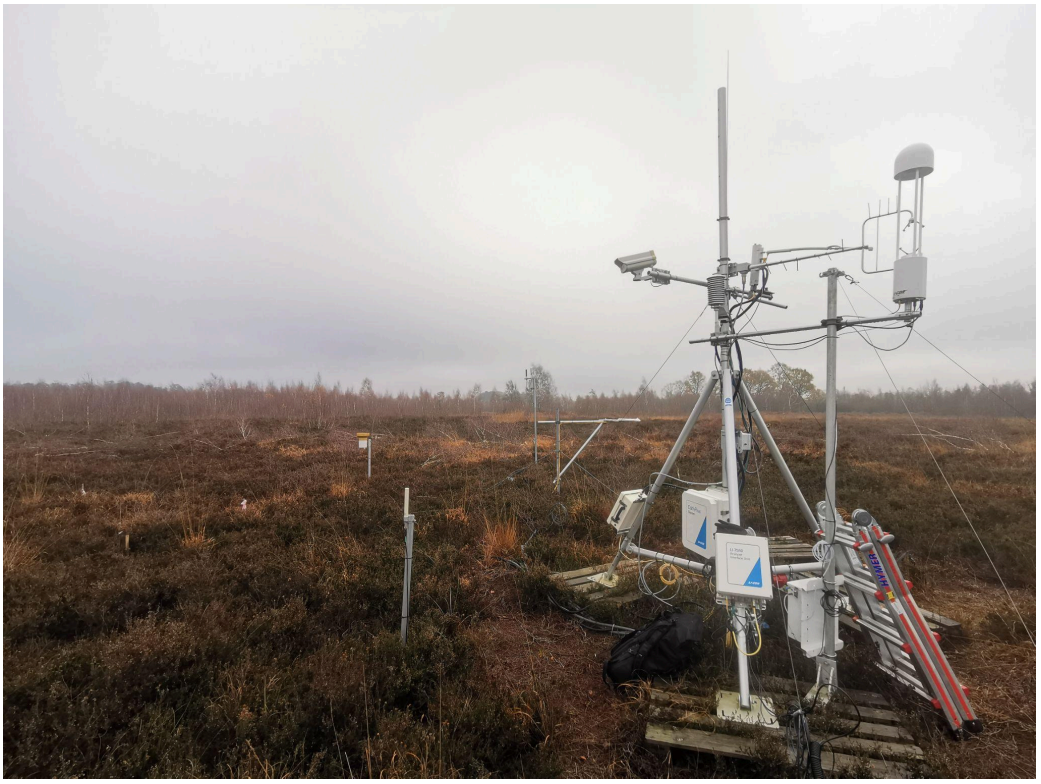
Response: Thank you for pointing out the imprecise wording. During the growing season, ET is regulated more biologically (via stomatal response to VPD) and outside the growing season more physically by VPD´s direct effect on evaporation demand. We revised the text in line 539 to make this interpretation clear.

2. Missing mechanisms - effects of vegetation height changes on LE
The manuscript doesn’t provide any information on vegetation height. Looking at the EC measurement height, it’s about 1.5m? And is it steady over a year, or does it change between growing and non-growing seasons? I’m asking because canopy roughness and structure can influence turbulence, hence LE (e.g., Ahongshangbam et al., 2025; Lee et al., 2021). If you don’t have manual measurements, you can estimate it using EC flux data following Pennypacker and Baldocchi (2016).

Ref:

https://doi.org/10.1016/j.agrformet.2025.110862
https://doi.org/10.1029/2021JG006532
https://doi.org/10.1007/s10546-015-0090-0

Response: The EC measurement height is at 2.8 m. We have added this information in line 116.
We agree that changes in canopy roughness and structure influences turbulence. In DE-Amv, however, vegetation is dominated by the evergreen and woody *Calluna vulgaris*, which has a stable vegetation height of 30 cm throughout the year. Although *Molinia caerulea* and *Betula* ssp. exist at the site, but they do not dominate the flux footprint. Please see below a picture from the site for your information. We do not include this picture in the manuscript to save space but we clarified this point by adding text in lines 108 to 110.



3. The end of introduction needs some revision
The current introduction reads much better than the original version; however, the end of current iteration reads a bit awkward. It's strange that you suddenly claim EC is the best method to provide high-frequency LE data on Line 90 and it's without citation. I recommend to move the final sentence to Line 87 like: "We analyzed three full years of eddy covariance (EC) and micrometeorological observations from a drained bog ecosystem in northwestern Germany, aiming to 1) …"

Response: We thank the reviewer for this suggestion. We have now revised the Introduction following the reviewer´s suggestion (see lines 93-94).

4. Results 3.1 and 3.2

I was shocked how the authors decide to have 3.1 by just having average EBC. Is it also performing better than other wetlands? Are there any seasonal changes? Maybe during the raining season, the lateral flows are more pronouncing and cause lower EBC? The authors wrote some nice, but short, discussions on reasons of EBC.
Then, it's even more shocking the 3.2 has basically only one sentence that can go with 3.3 as the starting instead of standalone, strange independent sub-section.

Response: We thank the reviewer for the helpful comments.
Regarding Chapter 3.1:
In lines 436 and 437, we already compared the EBC with other wetlands. We wrote "The energy balance closure was 83%, which falls within the typical range reported for the EC method (70-90%; Twine et al., 2000) and is above average for wetlands which is in average around 77% (Nicolini et al., 2026)".
In order to address the reviewer´s question about seasonal changes and effect of rainy season in EBC, we divided the data into three groups: days with precipitation (P) = 0, $0 < P \leq 2$, and $P > 2$. For $P > 2$, the slope of the EBC regression line was 0.83; for lower or no precipitation, it was 0.82. For the growing seasons the slope was 0.83 and for the non-growing seasons it was 0.71. We included these results and a corresponding discussion in the revised version of the manuscript (lines 245 to 249, Fig. 1, 440 and 441, and 445 to 449).

With regards to Sections 3.2 and 3.3, we followed the reviewer's suggestion and moved Section 3.2 to the beginning of Section 3.3 to avoid having an unnecessarily short section.

5. The number of figures and tables
I ask the authors to re-evaluate if all the figures and tables are necessary to present in the main text, or some can be moved to supplementary. For example, I don't see the substantial contribution of Table 1, as all sensors are quite typical for a EC tower. Fig 1 and 3 are a bit "redundant" for the main text and Table 2-5 looks like a casual dump of model statistics. I can't see why Fig 7 and 8 need to be separated and not merged, and I think it's better to be showed together with Fig 10 and 11. And for these four figures (7, 8, 10, 11), how many samples are there for each bin, is distribution ok?

Response: We thank the reviewer for their suggestion that helped us be more selective with our choice of figures and tables. We moved Table 1 to the Supplementary. We moved Figure 3 to the Supplementary and left Figure 1 in the main text. We combined Figures 10 and 11 (now Figure 6a and 6b) and removed Section 3.5.1 after which subsequently Figures 7 and 8 were removed as well (see more details in our response to Review #2).
We added rug plots to the x-axis of Figures 10 and 11 (now Fig. 6a and 6b) to show the data distribution and revised the Figure description accordingly.
In Table 2 (now Table 1), we removed the columns "F-stat." and "p (F-stat.)" and marked the model significance with *.
Tables 3 through 5 were combined into a single table (Table 2), retaining only the coefficients and using * to indicate significance.

[Minor comments]
Line 23-24: You don't need "which was estimated from the inverted Penman-Monteith model at the daily and sub-daily (30-min) scale" as it's provided on Line 16.

Response: We removed this part of the sentence, to avoid unnecessary repetition.

Section 2.7: GLSAR was not properly defined, you defined GLS and AR, but not GLSAR.

Response: We now defined GLS and AR together as GLSAR (lines 200 and 201).

Fig 2: Be consistent to use W m-2.

Response: We changed the unit in the y-axis label to W m^{-2}.

Fig 3: Didn't define H, as what you did in other figures.

Response: We now define H as sensible heat flux. Also this figure was removed to the Supplementary (see our response above).

Section 3.4: You used DW in the methods but DB here in the text and table captions?

Response: We changed DB to DW.

Fig 6: superscript -1 for s-1

Response: This figure has been removed (see our answers to Review #2).

Line 432-436: The sentences are a bit difficult to read, for example, what do you mean "variable component of Rn"? Also I don't think here is the place to justify the analysis methods you used.

Response: We have removed the text which was misplaced.

Line 443: "consistent with a limited number of studies", what studies? I don't see citation.

Response: We have added the relevant citations for this statement (line 484).

Line 497: Why "if Molinia caerulea, which tolerates higher moisture, expands, LE could decrease"? It's not enough to just cite a paper (Gobin et al., 2015). And it's quite absurd to show this one sentence suddenly in the conclusion, no?

Response: This is a valid point. We have now removed this statement which was misplaced.

Reviewer #2

Flemming and others study LE dynamics in a peat bog. I'm not convinced that the analysis is moving in the right direction and lots of interesting new findings like the SWC threshold beyond which LE becomes limited, the dry 2025, and more are buried amongst analyses that shouldn't be performed like the analysis of daily-averaged VPD on daily-averaged gc.

Response: We thank the reviewer for their assessment and take their comments as a great opportunity to improve our manuscript.

I don't like the introductory sentence of the abstract. Latent heat cools the surface but the latent heat of condensation releases it elsewhere. It just helps move energy around the earth system in different ways. The intro to the abstract is rather weak; focus instead on why peatlands are interesting to study in this context.

Response: We thank the reviewer for this suggestion. We removed the introductory sentence of the Abstract and instead added a statement on why peatlands are interesting to study in the context of their climate cooling capacity.
The introductory statement of the Abstract now reads:
"Despite covering only approximately 3% of the Earth's land surface, peatlands are recognized as a key nature-based solution for climate change mitigation due to their substantial carbon storage and high potential for restoration. Beyond carbon sequestration, peatlands also influence the local climate through land–atmosphere energy exchange, with one of their key cooling mechanisms being the regulation of latent heat fluxes (LE)," (lines 6-9).

Why was the daily analysis performed? This just introduces lots of Jensen's inequalities and obscures the study of gs responses to environmental variability. It looks like previous comments (which I hadn't consulted until now) discusses this so I'm still a bit confused why the daily analysis still plays a prominent role.

Response: We followed the suggestion of the reviewer and removed the daily analysis.

The GPP / LE analysis is a bit circular.

Response: Mechanistically and naturally yes because carbon-water fluxes are coupled (e.g., determined by ecosystem's water-use efficiency) and that is the aspect we leverage our analysis on. However, methodologically, GPP and LE analysis is not circular because LE is not derived from CO_2 measurements. It is calculated from the turbulent covariance of vertical wind velocity and water vapor density. Although both quantities are measured using the same eddy covariance framework, they rely on different scalar measurements (CO_2 versus water vapor) and independent flux calculations.

VPD should impact gs at sub-daily time scales and SM usually at weekly or longer time scales. This analysis is time scale dependent. Also, don't use a polynomial analysis as this is insufficiently mechanistic.

Response: We appreciate the reviewer's suggestions. A brief review of the literature did not show us that the relationship between g_c and SWC is best examined at a weekly resolution or lower. We attempted this nonetheless, resulting in 11 data points for 10-day g_c and SWC; based on these few points, we observed a slight, linear decrease in g_c as SWC increased. In addition, the reviewer strongly pointed out Jensen-like errors for daily VPD averages, which would also occur when averaging g_c and SWC and could be even more pronounced for weekly averages. Weekly samples would introduce aliasing, since SWC exhibits a diurnal variation at a depth of 5 cm on many days. For the reasons mentioned, we are retaining the 30-minute resolution of SWC and g_c in the manuscript. This preserves the advantage of the high-temporal-resolution data from eddy covariance. We have now also discussed possible causes for the negative response of g_c to high SWC (lines 489-492).

In this study, we have access to field-measured data, which allows us to investigate the relationships with as few assumptions as possible, rather than having to model them mechanistically. Since g_c, unlike SWC, cannot be measured directly in our measurement setup, it must be modeled based on the measured data. The relationship between SWC and g_c, however, is intentionally not modeled based on physical equations, as the aim is to investigate the true relationship which is why we have chosen a polynomial regression which best described the data.

In the response letter, I'm not familiar with the idea that soil heat flux is often ignored when applying the Penman-Monteith equation. It's a central part of the equation and all eddy covariance analyses that I can think of have incorporated it. FAO-type analyses of course not always.

Response: We had not ignored SHF. Our analysis already contained SHF. This point was mentioned in line 147, equation 2.

In the response letter, I agree that excluding days after rain will help with isolating transpiration but this is still a peatland and evaporation may be greater than one realizes.

Response: We agree with the reviewer that the proportion of transpiration (T) relative to evapotranspiration (ET) in our study area is unknown. In our view, the assumption that T/ET is close to 1 when precipitation effect is removed is reasonable, since the area has a nearly closed vegetation cover and is a drained peatland without open water bodies.
To be able to describe the error of the change point as a function of T/ET, we performed the change point detection using different proportions of T relative to ET. Starting at a proportion of 80% T relative to ET during the growing season and without interception, there is no change in the change point. When T/ET = 0.5, the changepoint is 7.2 hPa VPD instead of 7.6 hPa VPD. However the difference is not large.
We have included a corresponding figure in the Supplementary and incorporated these results into the Discussion (lines 522-523).

63: these passages and more would be simplified by noting the energy vs water limitation of ET (peatlands will almost always be energy limited, unless perhaps drained or under severe drought).

Response: We have modified the text according to the suggestion of the reviewer to make it more simple. The new text now reads:

“Peatlands are considered energy-limited ecosystems (e.g., Alekseychik et al., 2017 (raised bog, boreal); Gunawardhana et al., 2021 (alpine sphag. valley bog); Sonnentag et al., 2010 (minerotrophic fen, boreal)) in which groundwater table depth (WTD) correlates with LE only when it is near or above the soil surface (Humphreys et al., 2006; Gunawardhana et al., 2021; Lafleur et al., 2005),” lines 67-73.

and

“As typical for peatlands, DE-Amv is an energy-limited ecosystem, with R_n influencing LE through two main mechanisms (Table 3). [...],” in lines 468-469.

and

“The absence of a g_c limitation for decreases in SWC further confirms that DE-Amv is energy-limited rather than water-limited, meaning that soil dryness does not limit transpiration in DE-Amv,” in lines 492-493.

However, with respect to our specific study site, please note that DE-Amv is a drained peatland which are not necessarily always energy-limited.

130: would be more accurate as conductance for evaporation and interception rather than the processes themselves.

Response: We thank the reviewer for pointing this out. We rephrased the sentence to “Surface conductance is the sum of g_c, soil conductance and canopy interception conductance (Nguyen et al., 2021),” in lines 140 and 141.

Regarding the Loescher ref., it’s mostly sensitive to air pressure and can effectively be treated as a constant in your case in which elevation isn’t changing.

Response: We thank the reviewer for the clarification. Since air pressure varies not only with altitude but also with the macro weather conditions, and since we already determine the psychrometric constant as a dependency on air pressure, we will continue to assume that it is not a constant value.

Re: Euler’s number, just note that exp refers to exponentiation because it’s both the number and the operation.

Response: We rephrased the sentence in lines 163 to 164. It now reads “exp refers to the natural exponential function.”

The notion that the inverted PM equation assumes no storage….this isn’t true. More terms can be added to the heat flux terms including energy stored in biomass, that required by photosynthesis, energy stored above the soil heat flux plate that goes unmeasured under many field instrumentation implementations, etc.

Response: We appreciate the clarification. The original wording was not precise enough. The revised sentence reads: “The inverted Penman-Monteith equation does not account for missing energy storage terms (for example, the heating of the soil above the SHF sensors or the heating of the vegetation). Additional energy terms to R_n and SHF were not considered due to instrumental limitations.” We moved this sentence to lines 170-173.

What does the delayed response to Rn mean in practice? LE lagged Rn? This is probably due to Rn increasing T which is a term in the slope of the saturation vapor pressure function in the PM equation, so Rn is still determining LE, just through its impacts on T as well as Rn.

Response: What we meant was that the decline in LE was slower relative to R_n. We have reworded the sentence to “While LE increased in the morning together with R_n, its afternoon decline was slower than that of R_n for approximately 5 hours (Fig. 2),” lines 269-270.

Focusing more on the low precipitation year in 2025 seems generally more interesting to me, but this still didn’t decrease SWC to a point where it’s likely to limit plant function with a minimum of 49%, which is quite well-watered. Also, were the hydroprobe measurements adjusted for peat soils? The default is probably for mineral soils when converting mV readings to SWC.

Response: The hydroprobe sensor is adjusted for organic soil. We added this information to the Methods section (lines 118).

For the Fig. 3 caption, yes, but a negative number means that either LE or H is negative but not the other.

Response: This Figure was moved to the Supplementary as Figure S4 (see our answers to Review #1) and its caption was revised.
We revised the caption of Figure S4. The caption now reads:
“Bowen ratio (B) for the measurement period (1 January 2023 to 31 December 2025): The values are aggregated to three-day values.”

3.4: I guess I find that this analysis stands to reason for an energy-limited ecosystem and I’m not fully convinced that the analysis disentangles the multiple non-linear relationships between GPP, VPD, and LE.

Response: The nonlinear relationships in the measured data were minimized by removing monthly seasonal variability (lines 205–208). We compared the GLSAR AR(1) models with generalized additive mixed models (GAMMs) incorporating spline terms and the same autoregressive structure. For the year-round data, GAMM AR(1) and GLSAR AR(1) models had nearly identical support ($\Delta AIC < 1.5$), whereas for the seasonal subsets, AIC values favored the GLSAR models. Similarly, models including transformed predictors did not improve model performance.
Furthermore, the residualized explanatory variables were plotted against the residuals of LE in residual plots, and these plots suggest sufficient linearity.

We now have described the aforementioned model selection in the Methods section. In lines 231-236 we wrote:

"The GLSAR AR(1) models were compared with generalized additive mixed models (GAMMs) incorporating spline terms and the same autoregressive structure using the Akaike information criterion (AIC) (Akaike, 1974). The GLSAR AR(1) model based on year-round data was considered equivalent to the corresponding GAMM AR(1) model (ΔAIC < 1.5). In contrast, the GLSAR AR(1) models for the growing seasons and non-growing seasons were preferred over the corresponding GAMM AR(1) models based on their lower AIC values. Data transformations did not increase model performances, indicated by increased AIC values."

The analysis beginning line 348 is more interesting suggesting a threshold of 65% SWC in peat soil for water logging / decreased LE.

Response: This analysis is part of the averaged data which will be removed after we removed the analysis at the daily scale, to address earlier reviewer comments. Since Figure 6b (formerly Figure 10) also shows the water logging, we still discussed this topic and accordingly added this point to the Discussion (lines 489-492).

Figure 7: as noted in the original comments I'm not a fan of averaging VPD to study its impacts on gc. It's basically a graph of how long nighttime, with VPD near 0 is, and/or has to deal with different atmospheric conditions if local time instead of zenith angle is used for determining daytime conditions. It's easy enough to re-do this anaysis for half-hourly conditions.

Response: We have removed the analysis of g_c at a daily scale from the manuscript entirely.

Fig. 8 will be far less prone to this methodological problem. This analysis is fine. This is why Figs. 8 and 10 have about the same result. For succinctness, just keep one.

Response: Figure 8 is about SWC and Figure 10 about VPD. If Figure 7 and 10 are meant, we only kept Figure 10 (now Figure 6; the 30-min resolution) and removed Figure 7 and 8.

375: yes, do this. Remove the analysis in Fig. 7 and corresponding text. Interestingly, Fig. 10 shows about what Körner et al. and so many studies show for gs sensitivity to VPD around 10 hPa!

Response: We did remove the daily analysis (and with that Figure 7).
Based on our observations, there is no general threshold of 10 hPa VPD at which g_c is limited. This findings confirm that the threshold at which g_c is limited, varies with ecosystem type and vegetation cover. In addition to our own findings, we provide text in the Introduction and the Discussion section where we address the known differences in ecophysiological strategies among different vegetation types based on the body of literature (lines 57 to 66 and 74 to 82, 517 to 521).