“Peatland evaporation across hemispheres: contrasting controls and sensitivity to climate warming driven by plant functional types” - author responses to reviewers

Reviewer 1

Preprint egusphere-2023-1926 Peatland evaporation across hemispheres: contrasting controls and sensitivity to climate warming driven by plant functional types

General remarks:

This manuscript preprint presents a detailed analysis of evaporation and energy partitioning at two peatland bogs located in contrasting climatic conditions. While the two sites have many similarities, they show striking differences in their energy partitioning as well as in the response of evaporation, evaporative fraction, and canopy conductance to increasing VPD. The authors propose that the main reason for the differences is the difference in the dominant vegetation between the two sites.

This is a very timely and important topic which will help us improve our understanding of wetland ecosystems and their response to (and potential resilience to) a changing climate. It highlights the importance of considering the vegetation composition and structure when investigating evaporation and energy partitioning at wetland sites. Much of the research on wetlands and peatlands is based on boreal peatlands in the northern hemisphere. This study highlights that the results from these sites are not necessarily transferable to peatlands in different climates and more studies are needed in these under-represented systems.

The manuscript is overall very well written and was a pleasure to read. I found the analyses to be mostly thorough and scientifically sound and have very few comments. I recommend some minor revisions as outlined below.

We thank the reviewer for these very positive comments about the relevance and quality of our draft manuscript.

The main general comment I have is to include more of an acknowledgment of confounding factors and correlations with other variables when discussing some of the trends, especially when discussing the response to VPD. For example, high VPD conditions usually co-occur with high air temperature and often under high incoming radiation. VPD can also be correlated with wind speed/turbulence or other climatic factors. All of these can have direct impacts on evaporation as well. I think some of these correlations should be looked at and acknowledged, to ensure the VPD response is not overinflated.

We agree that the meteorological drivers of evaporation are often highly correlated and that it is important that we acknowledge this. Other researchers have similarly grappled with these interdependencies when trying to determine the main controls on (especially) the transpiration component of evaporation, as well as CO_2 exchange, which share a common pathway into and out of the plant.

To acknowledge the interactions as well as to justify our choice of VPD as the key driver, we have:

1. Inserted a new sentence in the introduction:

"Plant response to changing VPD is known to affect transpiration rates and CO_2 uptake (Grossiord et al., 2020)."

2. Inserted the following text in the discussion section 4.2:

"Our analysis focused on VPD as the driver of changes in E, EF, and g_c. However, VPD is strongly driven by air temperature and humidity, with temperature, in turn, responding strongly to solar radiation via the surface radiation balance (Chang & Root, 1975; Grossiord et al., 2020). Disentangling the drivers of water vapour fluxes in vegetated environments is therefore challenging. At Kopuatai bog, Goodrich et al. (2015) used modified light-response models to disentangle the primary seasonal drivers of gross primary production (GPP) for the E. robustum canopy. Summertime GPP was strongly limited under high VPD conditions rather than high air temperature, which they concluded was due to VPD-induced stomatal closure, implying that the transpiration component of E would be affected by changes in VPD. VPD has also been observed to drive stomatal response in other wetland settings (Takagi et al., 1998; Otieno et al., 2012; Aurela et al., 2007)."

Connected to this comment, I also feel there is scope to do a bit more with the wealth of data available. I would have really liked to see maybe a modelling component where the response of E, EF, and gc to other climatic factors is investigated in a bit more detail to see which factors are the main drivers for differences between the sites. I realize modelling might add a large component to the manuscript, but at least an investigation of the response patterns of E or EF to other climatic factors would be valuable in my view. At the moment the analysis focusses on VPD and VPD only, which, while very interesting, is somewhat limiting the impact of the work in my opinion.

We focused on VPD as the main driver of changes in E, EF and g_c because it has been shown to have a strong climate-warming signal and is known to be a key driver of plant response (Grossiord et al., 2020). Other studies have also focused on VPD (e.g. Helbig et al., 2020) as a climate-change-driven variable that shows different impacts on plant functional types. We feel that adding a modelling component to an already substantial data synthesis study could dilute the value of our observationally-based work which was already highlighted by this reviewer.

Specific comments:

Methods

Line 174: If I understand this section correctly there are 10 possible half hour data points between 10:00-14:30. Only filtering out periods with less than three 30-min data points (i.e. less than 30%) seems quite generous as an inclusion criterion

The threshold for inclusion has been raised to 50% (days with less than five 30-min data points were removed), and all results updated based on this change. Only very slight changes to the results were observed, and we agree that our methodology is now more robust.

Results

Lines 277/278: The Rn values at which H is described as being greater than LE (for Kopuatai) or LE as greater than H (for Mer Bleue) are a bit ambiguous when the uncertainty intervals are taken into considerations in Figure 4. Are these thresholds mentioned in the text of Rn>=250W/m2 and >=350W/m2, respectively, based on statistical testing for difference between the two components considering the uncertainty estimates? E.g. for Mer Bleue at an Rn of 350W/m2 the plotted standard deviations still overlap considerably with the mean values for both LE and H, so it is a bit debatable whether one can be classified as being 'greater' than the other in a statistically significant context. It is obvious from the figure that the two sites have very different patterns of energy partitioning, but I would recommend to be more careful and clear with the language here.

The threshold values mentioned in the text have been adjusted such that they reflect the points where error bars no longer overlap between *LE* and *H*. The new text is as follows:

"At Kopuatai, *H* was a much larger component of the energy balance than *LE* (Fig. 5a); *H* was greater than *LE* at $R_n \geq 450$ W m^{-2}, which was reflected in a mean Bowen ratio (β) of 2.0 (β range based on binned *H* and *LE* values was 0.43–3.11). At Mer Bleue, however, *LE* was either similar to or greater than *H* (Fig. 5b). As a result, mean β was 0.80, with a range of 0.63–1.05."

Discussion

Lines 394-401: I think this also supports the hypothesis mentioned earlier that evaporation from below canopy water is suppressed at Kopuatai, as partitioning towards LE increases significantly as soon as 'open water' is available in the form of canopy interception. If there was a significant contribution from evaporation of below canopy water the difference in energy partitioning between wet and dry conditions would likely not be as stark (which is exactly what is happening at Mer Bleue where there is very little difference in partitioning when comparing wet and dry).

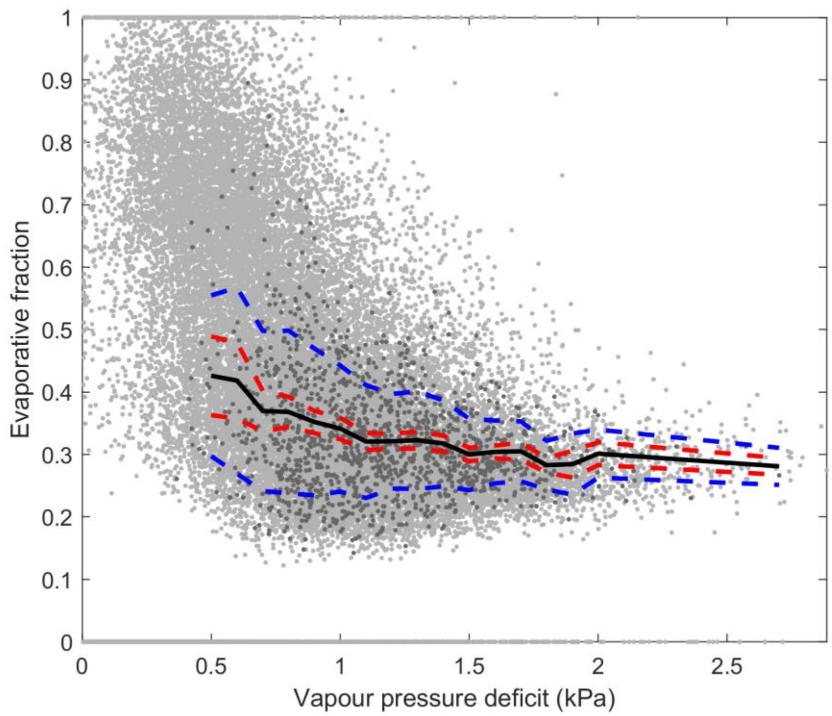
We thank the reviewer for this insight. We have added the following text:

"This contrast in energy partitioning likely occurred due to suppression of *E* from the moist peat surface by the dense standing litter layer of *E. robustum* during dry canopy conditions."

Figure 7 and Appendix D

In the Appendix Fig D1 the variability of EF for Kopuatai seems to be much larger at low VPD values, for both the unfiltered 30 min data as well as the growing season dry canopy MoD data points. This does not seem to be reflected in the shading of the 95% confidence interval in Figure 7 which barely changes with changing VPD. How was the 95% confidence interval determined? At Mer Bleue (Fig D2) the variability of the MoD dry canopy data does not seem to change as much with increasing VPD, so lines up better with the 95% CI depicted in Fig 7.

We agree that it is unintuitive that the spread of the raw and MoD mean data points in Figure D1 was not reflected in the 95% confidence intervals. However, having modified the inclusion threshold to 50% (see above), the 95% CIs are now wider at low VPD compared to high VPD (perhaps due to improved data quality; see red dotted line in the image below). These 95% CIs are still not as large as expected at low VPD, however, this could be due to large bin counts from the fourth VPD bin (0.8 kPa) and higher. This trend is much clearer when plotting the same data with standard deviations (see blue dotted line in the image below), as the errors get considerably larger at low VPD; therefore, we do not consider this to be an issue.



References:

Aurela, M., Riutta, T., Laurila, T., Tuovinen, J.-P., Vesala, T., Tuittila, E.-S., Rinne, J., Haapanala, S., and Laine, J.: CO2 exchange of a sedge fen in southern Finland - the impact of a drought period, Tellus B, 59, https://doi.org/10.1111/j.1600-0889.2007.00309.x, 2007.

Chang, J. and Root, B.: On the relationship between mean monthly global radiation and air temperature, Arch. Met. Geoph. Biokl., 23, 13-30, https://doi.org/10.1007/BF02247305, 1975.

Grossiord, C., Buckley, T. N., Cernusak, L. A., Novick, K. A., Poulter, B., Siegwolf, R. T. W., Sperry, J. S., and McDowell, N. G.: Plant responses to rising vapor pressure deficit, New Phytol., 226, 1550-1566, https://doi.org/10.1111/nph.16485, 2020.

Otieno, D., Lindner, S., Muhr, J., and Borken, W.: Sensitivity of peatland herbaceous vegetation to vapor pressure deficit influences net ecosystem CO2 exchange, Wetlands, 32, 895-905, https://doi.org/10.1007/s13157-012-0322-8, 2012.

Takagi, K., Tsuboya, T., and Takahashi, H.: Diurnal hystereses of stomatal and bulk surface conductances in relation to vapor pressure deficit in a cool-temperate wetland, Agr. Forest Meteorol., 91, 177-191, https://doi.org/10.1016/S0168-1923(98)00078-1, 1998.

Reviewer 2 - Joe Melton

Speranskaya et al. use two peatlands with different vegetation types (Kopuatai, NZ and Mer Bleue, Canada) to investigate evaporation (E) differences. Kopuatai has a vascular plant as its most dominant cover while Mer Bleue has more Sphagnum. This difference in the stomatal control of the vegetation is likely the reason for different responses of E to VPD and could then have implications on how the peatlands would respond to warming/drying changes. I find the paper to be well written with clear figures and very straight forward both in its presentation and in its findings.

My principle complaint is the choice of bogs for the comparisons. It would have made for a more clear cut comparison if the non-vascular bog were to have been more similar to Kopuatai (no snow, less seasonal, etc.). I realize that might have been a tall order to arrange, but it would have made the interpretation and comparisons even more clear. Alternatively, it would have been interesting to see even more peatlands included (perhaps in a gradient of vascular plant coverage). I think the paper would have been much enriched if a look at more peatlands could have provided a bit stronger support to the findings about the importance of the vascular plants, or if by picking out drought periods in the record it could have been demonstrated that the vascular plants really would protect these sites better than those with more moss cover. Table 2 does get at this a little bit, but to a less extent than I am suggesting here. Otherwise, I have only minor comments so I think the paper can be accepted after they are addressed.

We thank the reviewer for the positive comments on the quality of our paper, and for the suggested changes to its scope. In terms of selection of a comparison peatland, we chose Mer Bleue due to the length of its data record and its mid-latitude temperate climate status hence being a reasonable *Sphagnum*-dominated peat bog analog to Kopuatai. We also believe that a cross-hemisphere comparison is a novel approach to take, especially given the paucity of long-term EC datasets available from Southern Hemisphere bogs. We think that the inclusion of other peatlands might have been valuable, however, the use of only two sites enables a greater focus on describing the characteristics of their evaporation regimes, without this being lost within a wider synthesis. Nevertheless, a comparison of more sites (e.g., along a gradient of vascular plant coverage) could be an interesting avenue to explore in future research, now that the key distinctive features of the evaporation regime at Kopuatai have been described.

Minor comments:

Line 139 - Please add what is the difference in height between the hummocks and hollows. The WTD is used but one needs the heights to better understand how that might impact upon fluxes in the bog.

The difference between hummock and hollow heights at Mer Bleue has been added (while Kopuatai does not have such topography).

L147 - The window is 100 consecutive 'available' half-hours. This was then unclear to me how much this window might contract and expand due to gaps in the records. Did these windows ever get very large?

The window of 100 available half-hours is fixed and centres on the data gap. The observations used to develop the radiation relationship to fill the gap are not consecutive (and this wording has been corrected in the paper). For periods where there is more missing data, the period of time covered by the 100 half hour dataset gets larger but the fixed number of data points ensures a statistically consistent gap-filling process and avoids the creation of large outliers that can occur with a fixed window size. The window period is 3 to 5 days for 80% of the dataset gaps and less than 10 days for 98% of the dataset gaps. For the rare occurrence of larger window periods, the maximum window period in summer (when *LE* is most variable) was 20 days. Longer window periods occurred (rarely) in winter but this was when *LE* was near zero and had limited temporal variability.

L 223 - here is what I mean about knowing the hummock to hollow height. The 377 mm is only a bit below the hollow surface (I think they are 30 cm at Mer Bleue) so is actually closer to Kopuatai than it otherwise looks (at least over the part of the landscape that is hollows, which I think is roughly 50% at Mer Bleue).

We agree that this may appear to be cause for concern, as the WTD is measured relative to the hummock surface, which is on average 0.25 m above the hollow surface at Mer Bleue. However, since there is a 70% cover of hummocks at Mer Bleue, the majority of the peat surface at Mer Bleue experiences a WTD similar to the measurements used in this paper. Therefore, our view is that, despite the height differences between hummocks and hollows, the actual WTD at Mer Bleue is still considerably lower than at Kopuatai (and less stable). We have added the following sentence to Section 2.1.2: "The hummock height at Mer Bleue is 0.25 m, with a 70% cover of hummocks."

A few places, I think you want to say 'radiation received' rather than 'receipts' which sounds weird to my ear.

The wording of those phrases has been changed.

Fig 2 - it would be good to add the site labels to the figures themselves. Yes, they are in the caption but it is easier to read if they are directly on the figure.

Site labels have been added for Figures 3-6 (previously Figures 2-5).

Fig 4 and 5 - Rn is in the caption but not in the figures

This is not quite correct – R_n constitutes the independent variable (x-axis) of these figures. We have re-worded the figure captions to make this clearer.

I would combine Fig 6 - 8 into a multipanel figure, it would make them easier to see and compare across and take up less pages.

Thank you for this suggestion, we have combined these into the new Figure 7.

I suggest calling gc, 'calculated gc' to make it clear that it is not directly measured but derived.

As it is clearly stated in the Methods section that "g_c was calculated…", we feel that changing the name of g_c to "calculated g_c" throughout the document is unnecessary. This has been standard practice in many previous papers.

Table 2: It may help people if (New Zealand) was added after Aotearoa as I note the authors even use New Zealand in their affiliation lines so Aotearoa is not yet fully adopted. (Also doesn't it only refer to the North Island?)

Aotearoa has been added to all instances of New Zealand in the text, for consistency. This name refers to all of New Zealand.

L 361 -It would be interesting to conduct an experiment where the litter layer is removed to see if the litter layer really does have much impact. Totally outside the scope of this paper, but a neat experiment nonetheless.

We thank the reviewer for this suggestion – this will be noted as a potential future research topic.

A figure showing the actual study locations would be a nice touch.

We have added photographs of the two study sites (Figure 1). We have not used a map because scale issues would render this meaningless. Lat/long coordinates are provided in the text to enable readers to locate and inspect the sites using (e.g.) Google Earth.

List of all relevant changes made in the manuscript

Following peer review, several changes were made to the manuscript (note that line numbers pertain to the ***track changes version*** of the manuscript):

- **Line 53:** Added text "Plant responses to changing VPD are known to affect transpiration rates and CO_2 uptake (Grossiord et al., 2020)." (Response to Reviewer 1)
- **Line 130:** Added text "The hummock height at Mer Bleue is 0.25 m, with a 70% cover of hummocks". On **Line 145**, we have also added "the Kopuatai peat surface lacks hummocks and hollows" (Response to Reviewer 2)
- **Line 163:** Added a new figure (Figure 1), containing photos of the two research sites. The numbers of the remaining figures were modified to reflect this change. (Response to Reviewer 2)
- **Line 192:** Text modified to "Days with less than **five (50%)** acceptable 30-minute data points for all variables were removed to ensure representative MoD means.", based on comments made by Reviewer 1. Following this change to our methodology, the analysis was re-done and relevant values in the text and graphs were updated.
- **Line 257:** Wording was changed from "receipts" to "received" (Response to Reviewer 2)
- **Line 300:** Modified text in order to improve the clarity of our results to "At Kopuatai, *H* was a much larger component of the energy balance than *LE* (Fig. 5a); *H* was greater than *LE* at $R_n \geq$ **450** W m^{-2}, which was reflected in a mean Bowen ratio (β) of 2.0 (β range based on binned *H* and *LE* values was 0.43–3.11). At Mer Bleue, however, *LE* was **either similar to or** greater than *H* (Fig. 5b). As a result, mean β was 0.80, with a range of 0.63–1.05."(Response to Reviewer 1).
- **Line 306 & 315:** Figure 5 & 6 captions were modified to "Relationship between binned mean middle-of-day latent and sensible heat fluxes (*LE* and *H*) **versus** net radiation (R_n)…" to make it clearer that R_n is the independent (x-axis) variable. (Response to Reviewer 2)
- **Line ~326:** Figure 7 was created by joining three previously separate figures (as per the advice of Reviewer 2).

- **Line 375:** Added a new paragraph on VPD as a driver of evaporation (Response to Reviewer 1).
- **Line 427:** Added text "This contrast in energy partitioning likely occurred due to suppression of *E* from the moist peat surface by the dense standing litter layer of *E. robustum* during dry canopy conditions." (Response to Reviewer 1)
- Site labels have been added to Figures 3-6 (Response to Reviewer 2)
- Aotearoa has been added to all instances of New Zealand (and vice versa) for consistency (Response to Reviewer 2)
- Added new references to the manuscript relating to the above changes.

The remainder of changes in the manuscript are purely editorial, i.e., adding or re-wording sentences to improve clarity, and correcting any mistakes. While many of these changes are minor, the larger ones are listed below:

- **Line 107:** Re-worded a sentence and broke it into two for clarity, however the information conveyed remains the same.
- **Line 115:** Removed a sentence that was redundant.
- **Line 151:** Re-worded and relocated part of a sentence to improve clarity.
- **Line 161:** Added text "For both sites, gap-filled flux data were used for seasonal to annual *E* totals, while other analyses only used filtered measurements." to clarify the types of data used for our analyses.
- **Line 194:** Added text "However, for wet canopy conditions, 90% (Kopuatai) and 92% (Mer Bleue) of MoD 30-minute data were rejected. For the following analyses, 95% confidence intervals were calculated for MoD means as the standard error multiplied by 1.96." to further clarify the methods that we used.
- **Line 202:** Added text "…; however, no other filters were applied to the data for this particular component of the analysis." to further clarify the methods that we used.
- **Line 447:** Added text "Similar impacts were observed at Kopuatai during a severe drought in 2013, where monthly total ecosystem respiration increased in response to a slightly deeper water table, leading to a small reduction in the annual net C balance (Goodrich et al., 2017)." to provide more supporting evidence for the strong C sink at Kopuatai.
- Re-ordered and corrected some references.

Please note that the layout of the final and track changes versions of the manuscript is slightly different, as some of the text was shifted in the final version to make more efficient use of the space available.