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 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.

The revised analysis is much improved but I'm still not comfortable with the idea that evaporation is negligible during the growing season, especially when the authors don't consider potential effects of dew. Fortunately there is public code for high frequency partitioning that can be used to show approximately what sorts of biases are incurred by assuming that all LE is transpiration due to canopy gc. Minor comments follow.

Response: We would like to thank the reviewer for the valuable feedback. We have addressed the issues of potential dew and the partitioning of ET and will discuss them in the comments below.

In the abstract when I see 'Using high frequency CO2, H2O, energy flux, and meteorological measurements’ I think about high frequency flux partitioning like flux variance similarity or the work by Zahn & Bob-Zeid and others that use 10-20 Hz data for flux partitioning. Will this be explored here?

Response: The quoted sentence from line 13 does not refer to flux partitioning, but rather to high-frequency CO_2 and H_2O fluxes. In contrast, the gas chamber method - which is often used in practice - involves collecting very low frequency point measurements. Since the abstract is not intended to discuss methods not used in this study, we will not further specify how the methodology used here differs from other methods.

The introduction is much improved, the discussion of Molinia caerulea in particular is really interesting and helps motivate the study

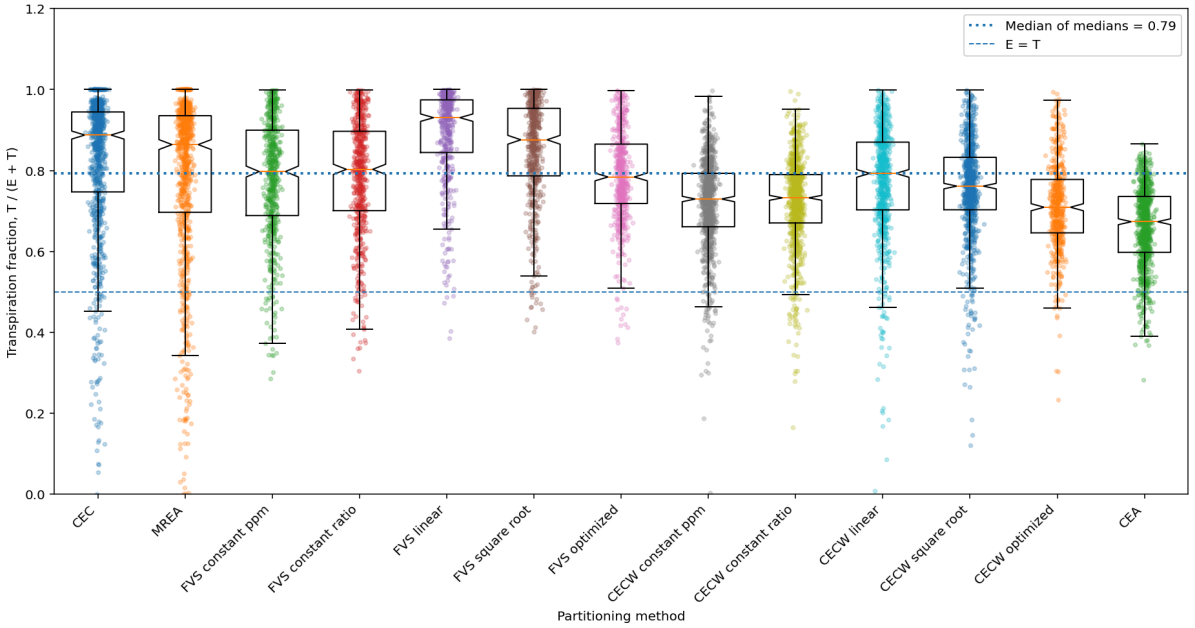
Response: We thank the reviewer for the positive feedback.

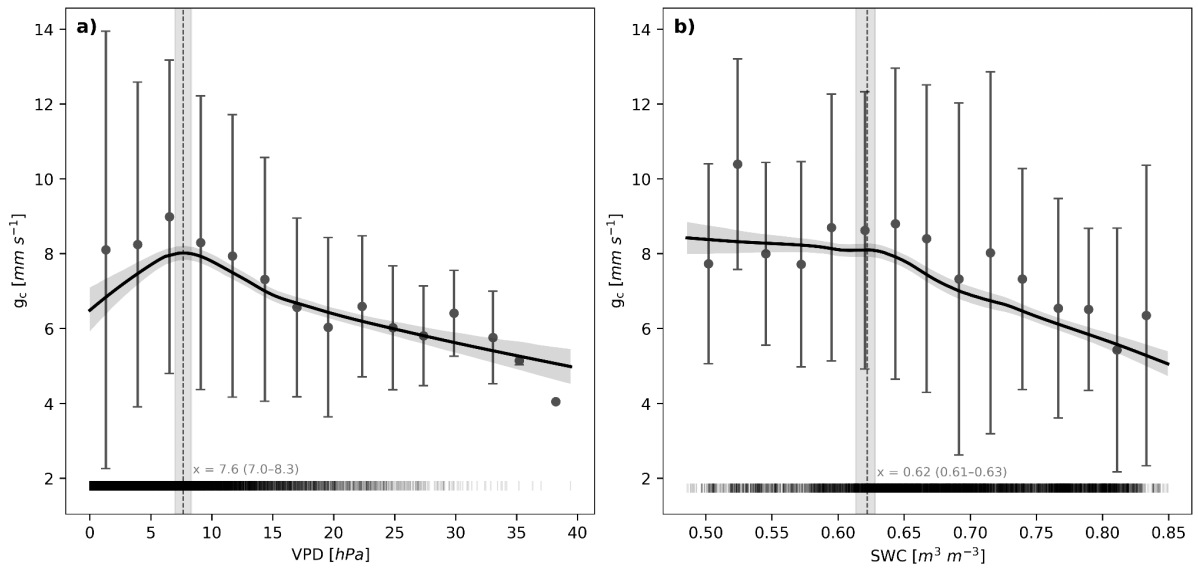
127: this doesn’t consider dew, which is probably pretty pronounced at this site. I’m still not comfortable with the idea that soil evaporation can be neglected; high frequency partitioning work consistently demonstrates that it is non-trivial most of the time. This can be checked using for example https://github.com/einaraz/PartitioningMethods. It can be hard to disentangle these things sometimes with low-statured vegetation but running a few of the approaches will give insight. Everything will be made easier because there’s already a python workflow.

Response: We tested the impact of potential dew by removing all data points where VPD was less than 2 hPa. After filtering out times when air humidity was high (or when air temperature was close to the dew point), we found no changes in g_c thresholds compared to the data used in the manuscript. Consequently, we did not systematically filter the data

further to identify potential dew. In our view, when cleaning data, data should only be removed if doing so eliminates misleading signals that have a meaningful impact on the results. Otherwise, additional sources of error are introduced without any discernible benefit.

Using the partitioning library suggested by the reviewer, we have now partitioned ET into E and T. As expected, different methodologies have yielded significantly different results (see first Figure). Given the high sensitivity of ET partitioning methods to various input parameters (e.g., water use efficiency) and their low robustness, it is currently still very difficult to interpret ET partitioning results reliably.
We share the reviewer's view that the assumption T ≈ ET under the defined conditions is unverifiable without ET partitioning. We have therefore tested a hybrid solution in which we continued to use ET with the filters described in the methodology and additionally added another filter: T/ET(FVS_constant_ratio) >= 0.8. This allowed the more robust high frequency H_2O flux measurements to be used for calculating g_c, and ensured that data used to calculate g_c ~ VPD and g_c ~ SWC thresholds were more reliably limited to the range in which no change in these thresholds was observed. FVS was chosen because it exhibits the smallest and most uniform error according to Zahn et al. (2024). g_c ~ VPD and g_c ~ SWC thresholds state the same as before applying the additional filter (see second Figure, which has the additional filter implemented). Since no change in the thresholds was observed and the additional filter results in systematic gaps in the data, we have decided to include the here shown results in the supplementary material (see Supplementary Section S5) but to continue using the previous methodology in the manuscript.





2h doesn't need an equation, 1000 can be written in the text

Response: We removed equation 2h and instead defined ρ_w = 1000 kg m^{-3} in line 137.

Fig. 1 is very interesting this is a bit higher than most wetlands on average and the full closure during ideal conditions is exemplary. For non growing seasons was freezing and phase transition an important contributor and/or perhaps more lateral flows without active vegetation transpiring?

Response: We agree with the reviewer that the issue of the unbalanced energy balance is an interesting one. In this paper, the energy balance serves as confirmation of sufficient data quality, as is now stated in lines 119-120. An analysis of the reasons for an unbalanced energy balance is not intended here and would go beyond the scope of this study.
We now explicitly mention lateral transport in the discussion (lines 350-352). We continue to not explicitly mention freezing as a cause of the energy balance error, since unaccounted energy storages (such as water) have already been discussed (lines 354-357) and days with frost in DE-Amv are rare during the study period (Figure 4).

326: what's the uncertainty associated with the 7.6 hPa threshold?

Response: In order to plot a 95% confidence interval for the change point, the revised version of the manuscript now saves the maxima obtained during bootstrapping and then plots the range from quantile 0.025 to quantile 0.975. We have updated Figure 6 and its description, as well as the text (lines 162-164, 328-329), accordingly.

I still really like Fig. 6b but the polynomial regression and binning analysis seem to show slightly different breakpoints between 0.6 and 0.7. How certain is the 0.62 breakpoint value?

Response: Similar to Fig. 6a, we now determine the 95% confidence interval of the threshold using bootstrapping. In addition, the threshold is now defined as the rightmost local maximum. The reason for this is that, due to the horizontal LOWESS curve before the

threshold, the global maximum is often found at the left end of the curve, even though the threshold is not located there. We have updated Figure 6 and its description, as well as the text (lines 162-164, 331), accordingly.

References

Zahn, E., Ghannam, K., Chamecki, M., Moene, A.F., Kustas, W.P., Good, S., Bou-Zeid, E., 2024. Numerical Investigation of Observational Flux Partitioning Methods for Water Vapor and Carbon Dioxide. J. Geophys. Res. 129 (6).