Referee comments Koskingen et al. *Covariation of redox potential profiles and water table level in peatland sites representing different drainage regimes: implications for ecological modelling*

General comments

The manuscript features the undervalued but fundamental redox potential that serves as indicator of peat mineralization processes. Besides, the redox potential is coupled to measurements of enzyme activity. The authors raise interesting hypotheses and present valuable results (although figures and tables might still be distilled better). The results are well-integrated in literature in the discussion, and conclusions are communicated clearly. Nevertheless, I believe that certain improvements should be made before publication. In particular, I have concerns regarding the methodology used to determine the Fe-reduction isopotential (and the associated hypothesis), as well as the discussion regarding bi-modality in Eh (and the answers provided to this hypothesis). Please find more specific comments below.

Major comments

Methodology

E_h and reduction thresholds are impacted by pH, therefore it is important to mention the pH when referring to Eh values or reduction ranges (in text and tables). In fact, the authors normalized the Eh measurements for pH 7 by applying a correction slope of 59.2 mV pH^{-1} (Nernst equation). This correction may become problematic for the Eh threshold of Fe reduction, as the correction slope differs for Fe (177.6 mV pH^{-1}) as protons are involved in half-reactions. This is depicted in Pourbaix diagrams for iron. At a common peat soil pH of 5 and a certain Eh, the iron in the soil might be reduced according to iron Pourbaix diagrams, but after normalization with a slope of 59.2 mV pH^{-1} to pH 7, the normalized Eh instead might indicate that the iron in the soil is oxidized. Therefore, I would either suggest to normalize the Eh to a common pH value (closer to the average of soil pH measurements), or to use a different Eh correction slope when assessing the Fe-reducing isopotential. As a result, answers to the associated hypothesis might change.

Results

As a reader, it is difficult to find the subplot that is referred to in the text. The amount of figures and subfigures is quite high. I would recommend to reduce the amount of figures and subplots showing wavelet coherences. For example, Fig. 4-6 show very similar

results, one of these figures is enough for the reader to understand patterns in wavelet temperature coherence (the results that are similar could be moved to the supplements). WTL wavelet coherences could be represented within a separate figure. Furthermore, it would be very helpful to include direct references within the figures in such a way that the reader directly understands which variables/probes/study sites are represented by a subplot (or subplot row or column). Additionally, I would suggest to combine correlation tables and/or move some correlation tables to the appendix.

Discussion

The authors raise the hypothesis that the redox potential shows bi-modal behaviour at the ombrotrophic plot, and that more nutrient rich conditions result in a multi-modal distribution. Probability plots (Fig. 12) confirm this hypothesis. However, I believe that the results are insufficiently placed into context of groundwater level fluctuations in the discussion. In fact, the groundwater level is much more stable at the OM plot compared to the ME plot, which would also result in less Eh variability (also see Boonman et al. 2024, https://doi.org/10.1016/j.geoderma.2023.116728) and a higher likeliness of bi-modal behaviour (CO_2 reduction below the groundwater level, O_2 reduction above). Based on this nuance, I think that the hypothesis about bi-modality cannot be confirmed.

Conclusion & hypotheses

The first and third hypothesis in the conclusion seem to be similar. Furthermore, the arrangement of hypotheses in the introduction deviates from the arrangement in the conclusions. The research actually features many hypothesis which is sometimes confusing. Perhaps some of the hypotheses could be combined or the hypotheses could be restructured. Also, it would be nice if implications of study outcomes could be added to the conclusion section.

Minor comments

Line 89: Because generally more TEA's are present in minerotrophic peatlands.

Table 1: The Eh values and ranges presented in the table lack referencing (and the pH value at which these Eh values and ranges were determined).

Line 123: "potentially bringing in electron acceptors such as Fe to the mesotrophic (ME) plots." Have measurements been done that confirm this?

Line 307: Please add a reference.

Line 314: “Note, however…”. Does this sentence refer to Mars and Wassen (1999)?

Line 315: For relations between Eh and groundwater level, also see Boonman et al. (2024, https://doi.org/10.1016/j.geoderma.2023.116728)

Line 360: See also Estop-Aragones et al. (2012, http://dx.doi.org/10.1029/2011JG001888) for discussion on saturated pores, redox potential and oxygen presence.