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Comment on bg-2021-276

Anonymous Referee #2

Referee comment on "Cutting peatland CO_2 emissions with water management practices" by Jim Boonman et al., Biogeosciences Discuss., https://doi.org/10.5194/bg-2021-276-RC8, 2022

Studies on the rewetting impact on the carbon dynamics of used peatlands are of great importance, because there are still large knowledge gaps and contradictions in this regard. Moreover, solid knowledge on this is a key prerequisite for the development of effective measures to revitalize peatlands and mitigate human-induced climate change.

However, due to fundamental shortcomings, this manuscript unfortunately cannot contribute to clarifying the aforementioned deficits and challenges. In particular, this is due to the following substantively interrelated issues.

- The authors fail to demonstrate the link between the so-called potential microbial respiration they simulate and the real mineralization of peat. This starts with the lack of a clear definition for this term. Second, they did not verify whether the data used for simulation from third-party laboratory experiments even reflect the behavior of the peat substrates studied here. This is because it is well known that the chemical properties of peat (peat quality) play a major role in determining its decomposition kinetics (e.g., Leifeld et al., 2012: Sensitivity of peatland carbon loss to organic matter quality. Geophys. Res. Lett., 39, p. L14704). Third, the comparison of simulated potential microbial respiration with measured ecosystem respiration made here is also not meaningful because neither the actual proportion of possibly congruent heterotrophic respiration in ecosystem respiration as such is known, nor whether it does not change significantly in the course of plant development or multiple grass cutting. The latter is very obvious (Fig. 6), but, as mentioned, has not been verified.

- As a proxy, so to speak, in lieu of not calibrating and validating the model with real data on peat mineralization from their own and other researchers' field plots, the correlative relationships between the simulated potential respiration rates and the measured components of a C budget were used. However, the authors ignored the point that the factors used in the model, soil temperature and, most importantly, WFPS, were determined by the groundwater level in the same way as all components of the C budget (Fig. 5a-c, Tab. 4, Fig. 10). In other words, all the results obtained on the intensively used peat meadows studied here were controlled by the groundwater level in the same way,

i.e., they were not independent of each other. Therefore, contrary to the authors' claims, the close correlative relationship between the components of the C budget and simulated potential mineralization is not evidence that their simulation approach can be used to determine the success of rewetting more precisely than conventional analysis of the relationships between groundwater level and the C budget.

Therefore, the novelty value of the presented studies with respect to the climate impact of rewetted peatlands and factors relevant in this context is very low. It would have made much more sense if the authors had focused on elucidating the role of other factors that are also important in this regard independently of the groundwater level (e.g., nutrient supply, type and productivity of vegetation, duration of rewetting). This is because there is only fragmentary information on this so far.

In view of this, I recommend rejection of the manuscript.

However, this does not mean that the results on the influence of the SDSI approach on the C-dynamics and the C budget in combination with hydrological modeling cannot be published in a newly designed manuscript after all. But that is not reasonable until the results of at least three years of monitoring are available.