Biogeosciences Discussions

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Comment on bg-2021-276: Results and Discussion

Henk van Hardeveld (Referee)

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-RC4, 2021

I strongly suggest that you analyze the sensitivity of your assessment.

Part of the controversy surrounding methods to assess the impacts of water management strategies in peatlands centers on their validity range. E.g., are methods derived on sites without drain infiltration systems also valid for sites with drain infiltration systems? If your method is to rise above such controversy, you cannot suffice by stating that your model simulates the water table dynamics "reasonably well" (line 318), or that the modelled temperatures were merely "slightly too high" (line 337). According to the approach of Van den Akker et al. (2008), a 20 cm offset in the summer water table may cause up to 60% extra emission. And assuming a Q10 of 2–3, a 1.45 °C offset in temperature may cause a 10–17% increase in microbiological activity. This raises the question to what extent you can accurately choose which WPFS optimum curve to use in your model? You have chosen shape 16, with a correlation of 0.591. But shape 8, which seems highly improbable has an almost similar correlation of 0.590.

Regardless of the results of your sensitivity analysis, I believe your approach will be a step forward compared to the current water table based approaches. But I do like to know just how robust your method is. Will a slight offset in your hydrological model or the chosen shape of the WPFS optimum curve produce similar, of very different results? And in case of high sensitivity, what is needed to accurately pinpoint which WPFS optimum curve to use? Multiple years of monitoring results on multiple sites, perhaps? In other words, are we there yet? Or are we merely still moving towards a better approach?