Comments *Henk van Hardeveld* on manuscript 'Cutting peatland CO_2 emissions with rewetting measures (Biogeosciences discussion BG-2021-276)'

Introduction

We thank Henk van Hardeveld for his critical look and thoughtful comments that will improve the quality and readability our manuscript. Especially the comment on featuring more prominently our novel method to estimate peat respiration and the comment on highlighting the quantitative comparison with previous studies will both certainly raise the impact of our study. We are happy to apply revisions to improve our manuscript as formulated in the answers to the referee comments below.

RC1: 'Comment on bg-2021-276, line 68: Submerged drain subsurface irrigation (SDSI) systems', Henk van Hardeveld, 24 Nov 2021

Throughout the years, these systems have been described in various ways. E.g., Querner et al. (2012) call them subsurface drains, Weideveld et al. (2021) call them subsoil irrigation and drainage systems, Hoogland et al. (2020; DOI:10.5194/piahs-382-747-2020) refer to them as drain infiltration, and Hoekstra et al. (2020; DOI: 10.5194/piahs-382-741-2020) favor pressurized drainage for a system similar to that on the Assendelft site. So why coin yet another name instead of using (parts of) a previous one, especially when the new name is less concise? Subsurface (and/or subsoil) seems superfluous: where else would the drains be? And submerged is not accurate all the times: a part of the appeal of these systems is that after a heavy rain shower, you can use them as conventional, non-submerged drains to more rapidly drain a field. Would (pressurized) drain irrigation systems not suffice?

Reply on comment RC1:

We acknowledge the need for the peatland community to establish a standardized term when referring to the technique that we called SDSI. In hindsight, we chose the term submerged drain subsurface irrigation (SDSI) as we thought this term would be most consistent, since it would indicate that drains remain *submerged* at all times and that the irrigation technique targets supplying water to the *subsurface* (subsoil) rather than the rooting zone which is conventionally targeted when discussing irrigation. We agree with your comment and think that it is important to use consistent terminology to avoid any confusion in the scientific debate and therefore, we will adhere to the term subsoil irrigation and drainage systems (SSI) that was used in Weideveld et al. (2021) and revise our manuscript accordingly. We believe that the term of Weideveld et al. (2021) is the best term available to describe the technique. As a matter of fact, when we chose the term SDSI the research of Weideveld et al. (2021) was not published yet.

RC2: : 'Comment on bg-2021-276, line 95–96: aim', Henk van Hardeveld, 24 Nov 2021

Two questions regarding your aim. First, a minor technical point, can you try to state your aim without using brackets? Surely, every part of your aim must by definition be important? Second, more importantly, can you try to allign your aim and your narrative more closely? I think the most important legacy of this paper will be that you introduce a novel method to more accurately assess the impacts of water management strategies on peat decomposition and greenhouse gase emission. So, must your new approach not take central stage? in your aim you mention various strategies, hydrological settings and meteorological conditions. But this strikes me as merely an afterthought. Once you have designed a better approach, by definition it will allow you to betrer explore the effectivity of strategies in different settings. It is nice that you do, don't get me wrong, but I think its is merely to demonstrate the added value of your approach.

In addition, please focus your Introduction on the processes that your approach addresses, avoid too much focus on anecdotal case studies such as you decribe in line 80–85, using vague phrases like "was suspected" and "the authors think". You might be aware that there has been much controversy about drain irrigation systems, sparked by a paper in bulletin 2018-06 of the International Mire Conservation Group. Arguably, the essence of this "knowledge war" is about a wide range in observed effectivities of these systems, and the question to what extent it is valid to use estimations based on water tables to estimate their effectivity. You method may help to settle this debate. For instance, in line 581–588 you make a strong point by using your method to explain why previous case studies in various settings come up with different conclusions.

Moreover, as your method might pave the way for better impact assessments, the comparison with previous methods should be better addressed in the Introduction section. I think Section 4.4 is one of the hightlights of your work, yet the previous methods are only discussed in very general terms in line 87–91.

Reply on comment RC2:

We were pleased to read that the novel process-based approach to estimate effectivity of peatland water management strategies is appreciated and are thankful for the comment that this approach should deserve more attention throughout the manuscript.

Based on this comment, we agree that our research aims can be presented more accurate and will revise our aim in the manuscript. The general aim is to measure, simulate and explore the effects that water management strategies may have on soil wetness and soil temperature and on the carbon balance, with emphasis on peat respiration. To achieve this aim, we presented the measurements and configured a model for an extended analysis. The novel process-based approach including soil moisture and temperature to simulate potential respiration indeed played a central role and therefore, we will elaborate upon this in the introduction of the revised manuscript.

We agree that we should limit the amount of discussion in the introduction to keep it concise and to promote readability and will rewrite this section

Thank you for the compliment that you consider Sect. 4.4 as one of the highlights of our work. The comparison provides confidence in the simulation results. We agree that the section is important for the international peatland community and will improve the introduction of this section in the revised manuscript.

RC3: 'Comment on bg-2021-276: Section 2.2.3 and Fig. 4', Henk van Hardeveld, 24 Nov 2021

I like this part of your approach, but please explain it more clearly. Fig. 4 is featured quite prominent, but the shapes seem random. I suspect this is not the case, that you have designed several categories. You merely state that they are "loosely based on the shape found by Säurich et al. (2019)". Can you elaborate on that? Especially because "the" shape of Säurich et al. (2019) does not exist. They present a wide variety of shapes and also mention that the variety would have been even bigger if they had included shapes found by other research.

RC4: 'Comment on bg-2021-276: Results and Discussion', Henk van Hardeveld, 24 Nov 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?

Reply on comments RC3 and RC4:

We thank you for the appraisal of Sect. 2.2.3. in which we discuss and present the tested curves that describe the relation between WFPS and potential respiration rate.

In the text we indeed refer to the shapes presented in figures in Säurich et al. (2019) and we agree that we need to be more specific on this. The WFPS-respiration shapes we refer to are based on Fig. 4a in the research article that includes CO2-C emissions over a range of WFPS for fen and bog earthified topsoils.

The curves we tested (Fig. 4) were not random. In fact, we constructed a starting curve (curve 1) and tested the effect of changing four properties of the curve: the starting value of reparation rate at 1 WFPS, the shape of the curve (bèta distribution, normal distribution, linear), the (range of) WFPS value(s) with maximal potential respiration and (in case of distributions) the effect of standard deviation or width. The testing results of the curves (comparison of Reco and the potential respiration rate calculated with the different curves) revealed curve properties that led to unsatisfactory correlations between R_{eco} and potential respiration rate. We revealed that certain WFPS-relations and curve characteristics were invaluable which should be excluded. We think that we can improve Sect. 2.2.3 by describing and visualizing the structure we used while constructing the testing curves and by elaborating more upon our methodology.

We are pleased to read that you think that our approach including temperature and soil moisture conditions is a step forward compared to conventional water table based approaches and agree that the sensitivity of our approach needs to be tested. We will include a sensitivity analysis in our revised manuscript in which we test the effect of offsets in soil temperatures (similar test as changing the Q10 of our temperature-respiration rate curve), and the effect of the chosen WFPS-respiration rate curve, on the simulated effectivity for the model simulations that represent our research locations best. Within this analysis, we will exclude WFPS-respiration rate curves that produced unsatisfactory results. However, the seven curves that were performing well with a mean Pearson correlation > 0.55 in Table S2.1 will be tested with a sensitivity analysis.

We understand your point about the comparison between measurements and simulation results. The model was setup to represent a simplified version of a common managed peatland cross-section in the Netherlands. We chose one standard parcel width, implemented one simple soil profile consisting of only three horizons and did not implement vegetation growth and harvest. Hence, the model runs that we chose for the comparison with research sites were not based on specific field conditions, but the boundary conditions that we varied matched these boundary conditions as much as possible. Accordingly, the statements we make about the model performance must be placed in perspective. We think that the assumptions we made were realistic, and that the assumptions have similar consequences for each model run. The text could imply that we expect our model was specifically aligned with field conditions (line 317), this is however, not the case. We will clarify this in the introduction and methodology of the revised manuscript.

WFPS-respiration rate curve 8 indeed performed unexpectedly well. We think that decreases in respiration rate when WFPS < 0.65 were not significantly represented in the total simulated respiration rate. In Fig. S2.1, you can clearly see that the average WFPS in the top 30 cm of the soil profile will not drop below 0.65. Therefore, the effect of water shortage for microbes is rare in these model runs. However, this does not mean that conditions with a low WFPS that dampens microbial respiration activity do not occur. We know that the decrease in microbial respiration rate is still important for dry situations occurring in particular soil zones.

We think that more research is needed to improve the estimations on temperature- and WFPS-respiration curves. The perfect potential respiration curves will be difficult to define, as respiration is also influenced by dynamics in microbial soil communities, variations in decomposition state, soil aggregates, chemical status of the soil and management history. In our research we explore the potential of this approach, we gained trust in the application by various comparisons, but we cannot state that our relations between temperature/WFPS and potential respiration rate are perfect.

RC5: 'Comment on bg-2021-276: Fig. 6', Henk van Hardeveld, 24 Nov 2021

Can you elaborate on the potential respiration rate? How do you explain that Reco for the sites with and without irrigation drains are quite similar, but the potential respiration rate is much lower at the site with irrigation drains than at the control site? And how do you explain the sharp drop in potential respiration rate in September that is not matched at all by the measurements? It seems the drop can be related to high modelled water tables and the chosen WFPS optimum curve with zero activity at WFPS = 1. How do you interprete that?

Technical question regarding Fig. 6 (a): can you better explain which lines are plotted on which axes and add units to the third axis?

Suggestion: can you also show these graphs for the Vlist site?

Reply on RC5:

Thank you for your comment on potential respiration rate. The R_{eco} that was measured includes peat respiration, plant respiration, respiration of fresh and easily degradable organic matter and anaerobic respiration. The potential respiration rate does only refer to peat respiration. We think that the water management strategies will mostly affect peat respiration and that the other forms of respiration are likely to only be slightly affected by strategies. That makes that the effect of the water management strategies that is reflected by R_{eco} is buffered by these other components, and seems lower than the difference in potential respiration rate we simulated. We will highlight the differences between R_{eco} and potential respiration rate in our revised manuscript.

The drop in potential respiration rate in September 2020 is indeed caused by the high modelled water tables and WFPS, and is a result of the WFPS optimum curve that we use. We think that the discrepancy between timing of the drop in measured R_{eco} for the control parcel in Assendelft and the simulated potential respiration rate could be caused by a delay in the depletion of oxygen in the soil after saturation. Additionally, we see an increase in measured R_{eco} for Assendelft control during the wet event in September that was not simulated. This could be a result of air that is pressed out of the soil during a large rewetting event, or other forms of respiration such as anaerobic respiration that was mentioned before.

The suggestion about Fig. 6 is very helpful: we will update Fig. 6 and the captions of the figure. A similar graph as in Fig. 6b can already be found for Vlist in the Supplementary information, Fig. S2.1, but was not included in the manuscript to reduce the length of the manuscript.

RC6: 'Comment on bg-2021-276: Fig. 8', Henk van Hardeveld, 24 Nov 2021

Arguably, the results for the Assendelft and Vlist sites are quite different. So it would seem pressurized irrigation drains and regular irrigation drains are two quite different systems, with different potential respiration rates in similar settings. Can you distinguish between both categories in the Figure? Currently, it is unclear which dots may refer to a pressurized system.

Reply on RC6:

The drain pressure in Assendelft was 30 cm higher than in Vlist and therefore, the differences we found in the results were expected. The selection of model runs did not specifically involve parcels with pressurized drainage, but we made the assumption that pressurized SDSI was represented by runs consisting of SDSI with higher ditch water levels as compared to the control situation. We understand that we should elaborate upon this in the manuscript and will do so in the revised manuscript.

RC7: 'Comment on bg-2021-276: Section 4.4', Henk van Hardeveld, 24 Nov 2021

The comparison between your approach and previous methods is very valuable. But the chosen relations seem a bit random. On the one hand, the relation of Fritz et al. (2017) was found in a semi-scientific Dutch magazine, which is only accessible by sending an e-mail to the authors. On the other hand, the often used relation of Couwenberg et al. (2011; doi.org/10.1007/s10750-011-0729-x) is lacking. As is the often used relation of Van den Akker et al. (2008), which uses the average summer water table, which in line 622 you claim is better than the average annual water table such as used by the chosen relations. Your comparisons will make a stronger point if you include those relations as well.

Technical comments: please explain what the dots in Fig. 11 are, change the units of the x axis of Fig. 11 and Fig. 10 into m below surface, and change the caption of Fig. 11 (in a concise manner) to match everything that you present.

Reply on RC7:

We are delighted to read your compliment on Sect. 4.4. We understand that the chosen relations that we compare with our results seem a bit random, especially the relation of Fritz et al. (2017). The relation of van den Akker et al. (2008) is not based on average groundwater levels but on ditch water levels and can therefore not be included. We agree that we should indeed include the model from Couwenberg et al. (2011) and might discard the relation of Fritz. et al (2017) within our revised manuscript.

The dots in Fig. 11 refer to model control simulations, we will update the legend and caption of Fig. 11. Furthermore, we will update the units on the x-axes of Fig. 10 and 11.