

Biogeosciences Discuss., author comment AC3
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Reply on RC8

Jim Boonman et al.

Author comment on "Cutting peatland CO₂ emissions with water management practices"
by Jim Boonman et al., Biogeosciences Discuss.,
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We thank the reviewer for her/his time to read and review our submitted manuscript. We -as authors of the article- feel that we have made fundamental progress in understanding the effects of rewetting measures in agricultural drained peatlands on peat mineralization. We explore, with a model and previously published relationships between peat mineralization and soil moisture and -temperature, the predicted mineralization reduction by raising ditch water levels and applying subsurface irrigation: the two most discussed water management measures for reducing peat mineralization in the Netherlands. We show that the degree of expected reduction strongly depends on regional hydrologic setting and weather conditions during the year. Moreover, we show that the now widely used indirect relationship between average groundwater level and mineralization to predict emissions from peatlands is likely to be different for raising ditch surface water levels and applying subsurface irrigation, due to contrasting interplay between groundwater level, soil temperature and soil moisture between both measures. The latter is a hugely relevant result. Without this insight we now likely overestimate the reduction effects of subsurface irrigation. In addition we show that some locations are expected to be more suited for certain measures than others, which provides guidance on where to first apply these measures. Therefore, it was a disappointment to read that the reviewer did not see these merits and instead focused on aspects that were not studied or asks for longer datasets that do not fit the timeframe of our research.

First of all, the comments of reviewer 2 about the circularity in our reasoning made us realize that we need to sharpen the objective and structure of our paper. The objective of our paper is to explore the expected effects of raising surface water tables and applying subsurface irrigation, by integrating literature relationships between peat mineralization and soil moisture/temperature into an advanced groundwater flow, soil moisture and soil temperature model (HYDRUS). To our knowledge, we are the first to publish such an approach and explore the effectivity of water management measures over a range of environmental settings. We corroborated our modelling results with one year of measured datasets at two contrasting locations. This is indeed, as the reviewer mentions, not enough to calibrate and validate the model components or gain fundamental new insights in small scale soil processes. However, it is enough to show that our modelling results with parameters derived from literature are realistic on the field-scale and that we can confidently explore the effects of the proposed measures on the emission of agricultural fields.

Secondly, reviewer 2 misunderstood our definition of potential microbial respiration rate and how this term relates to actual/real mineralization. We agree with the reviewer that

our manuscript should be improved with a clear definition. Potential microbial respiration rate is the fraction of the maximum amount of microbial mineralization rate from a particular volume of peat. When studying the effect of water management measures on peat decomposition at a particular location we assume that the properties of the soil profile (including quality of the organic matter) remain similar. In fact, the potential mineralization of the peat substrates that were included within our research has been measured in the laboratory, as opposed to the claim of the reviewer that we did not verify the data used for simulation. It was found that maximum mineralization rates on the control and SSI parcel were similar, and that the variation in the maximal mineralization rate of five different intensively used lowland fen (meso- eutrophic) peatlands was low. Therefore, we can safely use the maximum mineralization rate as we define it. When discussing recently drained or bog (oligotrophic) peatlands (that were not included within our study), this maximum mineralization rate might differ. Within our revised manuscript, we will elaborate upon this line of reasoning.

Thirdly, we understand the concerns of the reviewer about the comparison of the modelled potential microbial respiration rate and R_{eco} in Fig. 6 (Sect. 3.4.1). In the section we mention that we are very aware of the fact that this R_{eco} includes plant respiration which is influenced crop development/harvesting. Unfortunately, no exact methodology is available to disentangle various forms of respiration (peat respiration, plant respiration, respiration of root exudates, respiration of dead plant material – fractionation of young and old carbon). At the moment, our team is researching these contributions to respiration, but these results are beyond the scope of our paper. However, we did not use R_{eco} to invent mineralization dependency on soil moisture, but we used results that were found within laboratory studies of Säurich et al. (2019) to define this relationship. We cautiously interpreted the general shape of published datasets of lab experiments and presented 16 relations that mostly consist of this shape. Consequently, we selected the curves that explain most of the variation in observed R_{eco} . Even though we know that plant respiration could be influenced by soil moisture, the foundation of our methodology is based on laboratory studies reported in literature. Therefore, the R_{eco} is less important than reviewer 2 argued. As reviewer 1 also commented on this approach we will explain this with more detail and include a sensitivity test evaluating also the other curves.

Fourthly, reviewer 2 mentions that it is problematic that soil moisture content is related to the groundwater level. Although the two variables are indeed strongly related, soil moisture is a more sophisticated measure of groundwater hydrology and is represented by precipitation dynamics, evaporation and water retention characteristics. Unlike the groundwater level, soil moisture varies with depth, and this adds an extra dimension to the estimation of peat mineralization in comparison to using the groundwater level alone. Therefore, we do not understand the claim of reviewer 2 that there is a disadvantage in using soil moisture dynamics as compared to using the groundwater level when estimating peat mineralization.

Finally, the reviewer suggests that our method to estimate (potential) peat mineralization would not give better results than standard relationships with C-budget (C-loss) and groundwater level. It is remarkable that the reviewer promotes (average) groundwater table height as predictor for peat mineralization, while we know that this predictor falls short -as mentioned in the introduction of the manuscript and as shown by our results (Fig. 10, 11). In fact, both the standard approach and our approach are a strong simplification of the many outcomes we may encounter in the field. The reasoning of the

reviewer supports a one-sided view on the effectivity of water management strategies to reduce peat mineralization. As a consequence, there is a risk that water management strategies are less effective than previously thought or that they may even have adverse effects. The view of the reviewer on standard relationships between peat mineralization and groundwater level is exactly the view we attempt to nuance with our research: Yes, creating wetter conditions limits peat mineralization, but if we take both soil moisture and soil temperature into account the expected reduction by subsurface irrigation is not as large as expected from groundwater levels alone. This needs to become clear and needs to be taken into account in national greenhouse gas budgeting (i.e. budgets that need to be reported within the European Climate Monitoring Mechanism).

In summary, we think the second reviewer did not fully grasp the objectives of our paper. With the proposed improvements in reply to the comments of reviewer 1 & 2 we think we can substantially improve our manuscript and that it will become a valuable and innovative contribution to the discussion on rewetting measures in agricultural peatlands. We look forward to getting this opportunity.