We thank the editor for giving us opportunity to respond to the concerns of reviewer 3. Of course, we are disappointed to receive such a critical review with few constructive comments to improve our manuscript. We do not agree with the evaluation of reviewer as we describe below.

The motive of the work is misguided. As stated in the abstract, "the effects of rewetting efforts on microbial respiration rate are largely unknown". This statement does not consider extensive work in California, USA which has quantified the effects of rewetting of peat on GHG emissions and NECB (e.g. Hemes et al. 2019).

- In general, literature indeed reports that wetter conditions tend to lower CO_2 emissions and increase methane emissions. However, rewetting by raising ditch/surface water levels, periodic inundations, or by subsurface irrigation will affect water table, soil moisture and soil temperature differently. Consequently, microbial respiration will also be affected differently for each of these rewetting measures. This causes for example that currently there is no consensus on the effects of SSI (as described in Sect. 1, line 73). This is why we state that *"the effects of rewetting efforts on microbial respiration rate are largely unknown".*
- We found that Hemes et al. (2019) included only one drained agricultural peat grassland site that was not representable for the situation of the peatlands that we describe in our article as this site was intermittently inundated and located in a much warmer climate. Furthermore, groundwater tables and soil moisture content were not measured within the research making it impossible to quantify the effects of rewetting on soil hydrology itself. The literature that was used within our manuscript gives an accurate representation of the available knowledge on peat decomposition in managed agricultural peat grasslands.
- We can, however, improve referencing to non-EU literature to illustrate that agricultural peat decomposition is globally occurring and that it is relevant to improve process understanding and knowledge on mitigation strategies.

Second, the results of this study do not add significantly add to the scientific literature related to the effect of peat rewetting. As stated in the article numerous authors have demonstrated the GHG emissions reduction associated with peat rewetting.

- Our scientific advances are: We present (1) new data on the effects of SSI, a (2) novel methodology to simulate peat decomposition (and CO_2 emission-reduction) in which soil moisture and soil temperature control microbial respiration and spatial differences within agricultural fields are accounted for (2D profile) and (3) show that spatial differences in CO_2 emission-reduction after implementing measures can be understood and modeled by site specific hydrology and meteorology over time. Apart from annual WTD, no other drivers or field boundary conditions (like seepage, meteorology, soil temperature or soil moisture) have been investigated in literature.
- We qualified and quantified the effects of SSI on soil respiration, which has not been done by previous researchers. These effects (qualitatively and quantitively) have largely been unknown. Therefore, we do not understand and do not agree with the point of reviewer 3 that we do not add significant knowledge.

Third, the methods and modeling are problematic and do not support the statement that "Our findings can contribute to peatland management, to better decide on where and how water management practices would be effective". Technical problems include the following.

- The aim of our modelling is to show that with nationally available soil datasets we can describe groundwater levels, soil moisture, soil temperature, CO_2 emissions and reduction of these emissions by SSI reasonably well. Well enough to 1) explore the effect of environmental conditions such as seepage, weather and soil type of the reduction of CO_2 emissions by two types of rewetting: raising the surface water level and subsurface irrigation, and to 2) recommend this modelling-method to improve our estimations of countrywide emissions from peatland that are currently still based on landcover specific emission factors and universal water table – emission relationships (Sect 1, line 89).

• Groundwater modeling results show poor agreement with measured values.

- Measuring and modelling groundwater table depth in a peat soil is a challenge especially due to the high saturated water content and thick capillary fringe (substantial differences in groundwater table may correspond to small differences in air-filled pores space relevant for decomposition), surface movement due to swell and shrinkage and preferential flows likely to occur within dry summers when peat shrinkage imposes preferential flow paths. We did elaborate upon the groundwater modelling results in Sect. 4.2. In our opinion the results were satisfactory.

• Water filled pore space modeling results show poor agreement with measured values.

- Seasonal patterns in soil moisture are generally well described by the model. However, absolute water content, and absolute changes in water content are typically difficult to measure accurately in peat soils. Soil hysteresis and the high saturated water contents (>85%) causes that there is no strong relationship between soil moisture sensor values and water content measurements. The measurements do show a strong seasonal signal that relates to the air-filled pore-space relevant for decomposition
- Drying and wetting hysteresis due to peat shrinking and swelling and soil heterogeneity over depth introduce serious challenges when interpreting WFPS measurements. Currently, we are working on optimization of sensor data interpretation by including tensiometer datasets.
- We know that peat may need several months to fully swell and re-wet, while the model may indicate that the soil is completely saturated in early autumn (as was the case in Assendelft as described in Sect. 3.3). At present, no alternative theory or model exists to deal with these particular hydrological properties of peat (Sect. 4.2).

• No input model parameters such as hydraulic conductivity values for the hydrologic model were measured. The lack of measurement of input values resulted in a ill-conditioned model.

- When developing our model no measurements of hydraulic conductivity or water retention characteristics were available. Therefore, we decided to design a model based on general characteristics of a drained peat soil. Unlike reviewer 3 we think that the lack of site-specific input values is an advantage of our methodology, as it makes our results broadly applicable to a variety of agriculturally drained peat soils. Our results support this claim.

• The simulations do not account for varying soil carbon contents which have been shown to affect CO2 emissions and subsidence (e.g. Deverel et al. 2016).

- The soil organic matter (OM) density within our peat soils is very similar between the four measuring plots (15.64, 14.81, 14.82, 14.40 g OM cm^{-3} for Vlist control, Vlist SSI, Assendelft control, Assendelft SSI, resp. in the top 1.2 m of the profile). When implementing measures on a particular site we know that the soil, and soil organic matter content, will remain similar. Indeed, site characteristics may have an effect on the magnitude of the flux, but not on the effectivity of a certain water management measure. We clarified this within the revised version of our manuscript.
- The research of Deverel et al. (2016) includes all soils within a delta area, including shallow peat soils (< 1 m thick) and low organic matter content soils. In our research locations, soil organic matter content is much less variable and our results only apply to thick peat soils (>1.5 m of peat).

• Methane emissions were not accounted for in the NECB calculations. These have been shown to be significant in drained peat soils (e.g. Glenn et al., 1993, Anthony and Silva, 2021) and pasture on organic soil (e.g. Hatala et al. 2012).

• Nitrous oxide emissions were also not considered. These have been shown to be significant in drained peat soils (e.g. Anthony and Silver, 2021).

- It was never the aim of our research to make a net ecosystem greenhouse gas balance, but a carbon balance to estimate the CO_2 emissions from peat oxidation. There will be a small part of the carbon emitted as CH_4, but this is insignificant compared to the carbon emitted by CO_2, which was shown in unpublished CH_4 data from a previous study from the Assendelft research plots (including the SSI plot with highest groundwater levels).
- Quantifying methane and nitrous oxide emissions was beyond the scope of this research and we mention that groundwater levels that approach the surface may induce CH_4 or N_2O emissions (Sect. 4.3) and therefore suggest to keep groundwater levels below -0.2 m (Line 740, Sect 4.5).
- Nevertheless, methane and nitrous oxide emissions can indeed be significant when determining the total greenhouse gas budget, especially when soil inundation occurs (as described in Anthony and Silver, (2021); Hemes et al. (2019), which is not the case at our research locations. Hatala et al. (2012) found that most CH_4 emissions emerge from ditches. It would be interesting to measure CO_2, CH_4 and N_2O emissions in future research when aiming for complete greenhouse gas budgets (again this is not similar to NECB).
- To avoid any confusion in the future we checked the manuscript for misleading usage of the term *greenhouse gas emissions* within the revised version of the article.

Lastly, because of these issues, I conclude that the study does not provides a greater “process-based understanding in these rewetting effects on peat decomposition”. And I disagree that there has been a successful “integrating of high quality field measurements and literature relationships with an advanced hydrological modelling approach.”.

Still, reviewer 3 did not present alternatives for an improved process-based understanding of peat decomposition or alternatives to quantify the effects of water management strategies (except expensive measurement protocols). We regret to read that reviewer 3 neither mentions anything about our findings on the effects of SSI, our reasoning to focus on water/air filled pores and soil temperature, nor the measured yearly carbon balances which were successfully reproduced in our simulations. We remain very content with the outcomes that we could realize.

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