“Water table driven greenhouse gas emission estimate guides peatland restoration at national scale” – bg-2023-23

[Reviewer comments in *italic blue font*; author replies normal font]

The paper by Koch et al considers the effect of peatland rewetting on GHG emissions in Denmark. It uses a large dataset of water table measurements to test and validate a model for national upscaling. It then looks at prioritising the rewetting of wetter for drier fields to make recommendations. The manuscript is interesting and timely, considering current interests in peatland rewetting for climate mitigation. It is well-written and I enjoyed reading it. I have only minor suggestions. Note however that I have no experience of machine learning so cannot comment at all on that part of the manuscript.

Reply: We would like to thank the reviewer for their thoughtful review and for acknowledging the novelty and relevance of our work. Please find our point by point replies and concrete plans for the revision below.

Section 2.1. It might be helpful to readers to specific the climate zone somewhere here. A few words of extra text around L82 specifying agriculture (cows + specific kinds of crops?) might also be informative.

Reply: We agree to this point raised by the reviewer. More details on the study site description will surely be appreciated by the readership outside of Denmark.

Plan for revision: We will extend the study site description with information on climate and agricultural practices with special emphasize on peatlands.

Section 2.2. Perhaps I misunderstand something, but why is the median used on L89 but mean on L93?

Reply: The median was used for the WTD data originating from wells that had multiple summer observations. The median is preferred here, because it is less sensitive to outliers which we find favorable for curating a WTD training dataset for long-term average summer WTD. The mean was applied to the revisited soil auger sites (2010 and 2021). Here only two measurements are available and calculating a median is not possible, thus the mean was function was applied.

Plan for revision: We will revisit the section introducing the WTD and make sure it is stated clearly why the median is applied to the well data but the mean function is applied to the soil auger data.

L160. A slightly pedantic comment, but I’m not sure I’d describe static chamber sampling with syringe sampling and GC analysis (as in Petersen et al 2012) as “state of the art” (there’s nothing wrong with this method, but neither is it anything incredibly advanced).

Reply: We agree.

Plan for revision: The wording will be changed when introducing the static chamber sampling method.

L171. It is good to see that waterborne C losses are accounted for, using the IPCC defaults.

Reply: Thank your for that acknowledgment.

L180. 10% seems quite high here. The IPCC Wetlands Supplement has 0.05 (5%) as Fracditch for boreal/temperate drained grasslands and croplands.

Reply: Methane emissions from ditches are only estimated for grids (10 m * 10 m) where we know that ditches are present. For those grids a Fracditch value of 10% corresponds to a 1 m wide ditch water surface, which is generally suitable for Danish conditions. For grids without ditches, Fracditch is set to 0 %. This technicality varies from the IPCC Wetlands Supplement, where the Fracditch is applied to all grids independently whether ditches are present or not, which explains the generally lower value compared to the one we are employing.

Plan for revision: The technical implementation on how drains are mapped and how and where the $CH_{4\ ditch}$ is estimated will be extended.

Figure 6. I think measured vs modelled plots are more "honest" when plotted as square panels and the same scale on both axes. That gives a true visual representation of the model.

Reply: We agree that figure 6 can be improved. Reviewer #1 also commented on the figure content and layout. We chose to not plot square panels, because of the applied WTD transformation which causes a strong underestimation for deeper WTD. In order to make the presentation "honest" we decided to plot the 1:1 line. Scales are different for the two panels because the number of points varies substantially.

Plan for revision: The figure will be adjusted in a way that the top panel shows all WTD data and the bottom panel will focus on well&auger observations between 0 – 1.0 m. While revisiting the figure we will experiment with square panels and same scales on the colorbars.

Figure 7. I wonder how much panel a is skewed by the data point around 1.2 m WTD? Without this point would the relationship be linear, as in Evans et al (2021)? Does it make sense for CO2 emissions to stabilize at 0.4 m WTD? This is mentioned in the discussion but I'm not sure I'm convinced.

Reply: We agree that the choice of function type for building the CO2 response function is crucial. This point was also addressed by Reviewer #1. The literature supports the use of both, Gompertz and linear functions. The stabilization of CO2 at a certain WTD may in reality also relate to a limited peat thickness. Peat depth, in combination with the WTD, marks the lower boundary of the unsaturated peat. Mapping of peat depth is currently undertaken and since the data are not available at present this discussion becomes very hypothetical.

Plan for revision: In the revised manuscript we will add a new discussion section where we will explore the choice of function type for building the CO2 response function as a main uncertainty and its effect on the overall GHG estimation as well as for the rewetting potential will be investigated. We will stick to the Gompertz model for presenting the key results. We will explore the possibilities to fit a linear model to the Danish GHG - WTD data and if it is suitable for GHG upscaling. If found suitable, the effect of linear vs Gompertz will be quantified and related uncertainties will be discussed.