

Response to comments from Anonymous Referee #2

[Comment] 1. *The modelled WTD is not demonstrated to be superior for 5 k over 100 k. -Fig S7 is a start. How about looking at the difference between the 100 k WTD and the observation then comparing that to the 5 k WTD - observation difference? Does it improve? Since it is already a bit tenuous to downscale WTD (can TOPMODEL even be used in such a low relief area reliably?), I think more evidence that it results in improved WTD is needed. Yes, the well data is going to be influenced by some anthropogenic factors that can't be included in the model, however a subset could be chosen that would be less impacted.*

[Response] As suggested by the reviewer, we compared our simulations of WTD at 100-km and 5-km spatial resolutions to the observed WTD at selected wells. The comparison indicates that WTD is better simulated at a 5-km spatial resolution, while observation difference between 100-km WTD and observation is much larger. The comparison is described in the first paragraph of Results section. A new figure (Fig. 3 in the revised manuscript) was added.

[Comment] 2. *The CH₄ emissions simulated are not shown to be improved for 5 k over 100 k. -Again the authors have made a start, but I feel more needs to be done here. It is rather unconvincing to compare the range in CH₄ observations vs. range in modeled CH₄ emissions. Table S1 has locations of point estimates. If the 5k results at those sites are compared to the 100 k estimate CH₄ emissions, do they improve? In my quick access review, I also suggested to look at the Hudson's Bay lowlands for further evidence that the CH₄ emissions are reasonable. I still think it would be a good idea. Point measurements from WSL and a regional-scale investigation from HBL would be appropriate (like using Pickett-Heaps et al. 2011).*

[Response] In this revised manuscript, we extracted the simulated CH₄ emissions at corresponding geographic locations of point measurements. The CH₄ fluxes at both 100-km and 5-km spatial resolutions are now compared to the observed CH₄ fluxes at 22 field sites (Fig. 3d). The 5-km simulations perform better than the 100-km simulation based on the comparison. The comparison of regional total CH₄ emissions between our simulations

and other studies is also conducted based on previous airborne-based regional-scale investigations from the HBL. See the second paragraph in Results section and Fig. 3d for the description of the comparison.

[Comment] *Fig 3 - lines vs. dashes look the same at the scale of the BGD paper. Try and make them easier to distinguish*

[Response] The quality of the figure is improved in the revised manuscript (Fig. 4 in this revision).

[Comment] *p. 18459 l 3 - 'many existing biogeochemistry models' - such as?*

[Response] We have listed several such models and their references in this revision.

[Comment] *I think the Papa/Prigent dataset is formally called GIEMS*

[Response] In this revision, we use GIEMS data to represent the Papa/Prigent dataset throughout the text.

[Comment] *Fig S7 - Are those histograms of the modelled WTD for the entire region in the simulation panel of Fig S7 or just the same grid cells as the observations? If all the region, replot as only the same gridcells as in the observations.*

[Response] The histograms of the modeled WTD in original Fig. S7 are just the same grid cells as the observations. Since we have added a new figure (Fig. 3) to explicitly compare our simulations with observations, original Fig. S7 has been removed in the revised manuscript.

[Comment] *p. 18464 l 22-25 - 'The depth of 0.5 m might have been used' - It might have been used? Confusing.*

[Response] The confusing expression is now corrected.

[Comment] *I include parts of my original comment about oxidation in the water column from my Quick Access review as I think it should be discussed: "Fig 4 – it seems like the principle effect of 5k/100k in this figure is to increase the variance (as mentioned in the*

body text) with a longer tail toward deeper soil water tables. Since the authors set any water table that is negative, i.e. above the surface to zero, the tail on the high water table end of the distribution is now bunched into the first bin of the histogram. By assuming no surface standing water, any oxidation that would otherwise occur in the water column is then not accounted for. It appears from Fig 4 that the importance of this would be greater in the 5k simulations, since there are more gridcells with higher water tables, which should mean more with standing water. Can the authors demonstrate that most of the difference in CH₄ emissions between the 5k/100k simulations is not an artifact of excluding water column oxidation? "

[Response] As suggested by the reviewer, we have added one paragraph in Discussion section (the second paragraph) to explain and discuss this possible error source.