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Interactive comment on "Spatial scale-dependent land-atmospheric methane exchange in the northern high latitudes from 1993 to 2004" *by* X. Zhu at al.

Anonymous Referee #2

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General comments:

This paper looks at the importance of spatial scale in determination of water table depth and estimated wetland methane emissions for the high northern latitudes. They use a combination of models. VIC feeds soil hydrological and temperature information to TEM. They use a TOPMODEL parameterization to allow investigation of the influence of a 5 km (VIC-TEM-TOPMODEL) vs. 100 km (VIC-TEM only) representation of water table depth (WTD).

I had a lot of difficulty with this paper. This is partly why I have taken a long time to submit my review. I needed to ruminate on it.

In general, the paper reads well and the language has greatly improved from the quick access initial review. I was glad to see the modelled WTD is now compared to observations, rather than another model. However, I am not convinced on some aspects and, unfortunately, I think they are central to the paper.

My main issue with the paper relates to how well the authors demonstrate an improvement in simulation of wetland methane emissions or water table depth in the 5 km vs. 100 km simulations. It is hardly surprising that they would give different results. That is not interesting, nor really worthy of publication in my view. The interesting story is if the 5 km simulation results in improved results compared to observations. This would give some meaning to statements like, 'This study suggests that previous macro-scale biogeochemical models using grid-cell-mean WTD might have underestimated the regional CH4 budget'. Therefore I feel a lot hinges on whether the 5 km simulations actually improve the simulations.

Here then are my primary complaints:

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.

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 modelled 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).

Overall, I think this paper could still have merit, but major revisions are needed before publication.

Specific comments:

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

p. 18459 l 3 - 'many existing biogeochemistry models' - such as?

I think the Papa/Prigent dataset is formally called GIEMS

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.

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

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? "

Pickett-Heaps, C. A., Jacob, D. J., Wecht, K. J., Kort, E. A., Wofsy, S. C., Diskin, G. S., Worthy, D. E. J., Kaplan, J. O., Bey, I. and Drevet, J.: Magnitude and seasonality of wetland methane emissions from the Hudson Bay Lowlands (Canada), Atmos. Chem. Phys., 11(8), 3773–3779, 2011.

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