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Interactive comment on “A study of the role of wetlands in defining spatial patterns of near-surface (top 1 m) soil carbon in the Northern Latitudes” by E. M. Blyth et al.

E. M. Blyth et al.

emb@ceh.ac.uk

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I think this reviewer has misunderstood the background and purpose of the work we undertook and reported in this paper. And that is my fault for not being clear. I would like to take this opportunity to explain it, and explain therefore why their criticisms of the paper are not relevant. The essence of the paper is to show that current climate models (which work on the time-frame of 100 years) mis-calculate the impact that saturated regions have on the near-surface soil carbon balance. The paper first shows that the top 1m soil carbon is higher in saturated regions from the data. It does NOT aim to model where those regions are – the organic peat-y soils are specified in the

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simulations, not grown. This is because the model in question (JULES) is used in climate (100 year) simulations, not a peat-growing model such as LPX which might be used in much longer time-spans. The discussion about the longer time-spans was included to show the logic of why we only look at the top 1m of soil which relates to the 100 year time span, not longer. This logic is necessary so that we can rationalise the focus on the role of wetlands on soil carbon, not the other way round (i.e. growing the peat-lands). The paper then demonstrates that the model does not suppress the soil respiration enough in saturated conditions so that the top 1m soil carbon is not as high as that indicated by the data. The point of the paper was to demonstrate that and quantify it using new spatial analysis methods. I am sorry the reviewer doesn't think the writing is clear, but maybe they were looking for something to do with longer time spans and peat-growing etc. Perhaps a clearer steer in the abstract that the paper is looking at the 100 year time frame would help readers.

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