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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.

Anonymous Referee #2

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Review of "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, R. Oliver, and N. Gedney.

The authors investigate the role of wetlands for the accumulation of soil carbon in the high northern latitudes. They start with the observation that recent observation-based compilations of SOM distribution in the high northern latitudes show maxima in wetland areas as defined by the GLWD. They very simplistically modify the SOM decomposition routines in JULES to account for the slower SOM decomposition in wetlands and obtain a slightly improved picture in comparison to the original model formulation.

Overall, the manuscript falls short of the state of the art in high latitude SOM modelling.

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Permafrost is more or less completely neglected in the manuscript, most of the literature on wetlands and SOM has been ignored, and the state of the art in wetland and permafrost soil modelling seems to be unknown to the authors. I therefore recommend rejecting the manuscript.

The authors start with the – perfectly sound, though somewhat trivial – working hypothesis that wetlands and high accumulation of soil organic matter are linked. One would expect then to follow a discussion of the various approaches of modelling peat accumulation (papers by for example Frolking, Clymo, Ingram, Kleinen, Spahni come to mind) or maybe the observations of peat distribution (Yu, Loisel, Charman, Korhola, MacDonald come to mind), but none of that can be found in the paper. Therefore the review of the existing literature on the subject was certainly too shallow. This continues with the analysis of the reasons for the high carbon accumulation in the high northern latitudes: Wetlands are one part of the issue, but permafrost and the related cryoturbation are a vital second part of the issue. An explicit treatment of permafrost would not be required with a suitably defined research question, but a deeper discussion of this factor is essential when discussing the high carbon accumulation in high latitude soils. The discussion of Yedoma on page 17929, by the way, is plain wrong. Here, the authors claim that the soils have been blown in, which is correct for the parent material, but most likely not for the carbon contained therein. Also, most Yedoma deposits are substantially younger than 1 Myr.

With regard to the study itself, I found the description of experimental setup and results superficial and not very informative.

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