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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 #1

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In this manuscript, the authors describe their observations of strong correlation between soil C density (kg C/m2) and wetland abundance in high northern latitudes and their simulation results using a model (JULES). The main results and conclusions include that high soil C density tend to occur in regions with abundant wetlands (peatlands) and that there are weak correlation between soil C and temperature.

In my opinion, there are many problems with the manuscript in its present form, including the project design and focus, result interpretations, inaccurate and incorrect information, and writing. Here I just list a few examples in each of these categories of problems.

The simple reason for the strong apparent spatial correlation between wetlands (especially peatlands) and soil C density is that peat (organic soil, or histosols) contains much higher organic matter (oftentimes around or greater than 90%, and organic matter contains about 50% C) than mineral soil (a few percent at most, and often <1%). That pattern doesn't necessarily in itself say much about the causes for high C accumulation in peatlands, but I don't think that the lengthy discussion about this correlation is needed, and much less to use this as the focus of the study. Also, more realistic considerations and simulations of peat are essential for "soil carbon in the Northern Latitudes".

Carbon accumulation in soil, in particular in peatlands (organic soils), is the cumulative difference between C input (from litters) and C output (mostly as respiration/decomposition) over the soil development history, oftentimes more than 10,000 years. So the lack of a strong apparent correlation between contemporary temperature and soil C is not surprising and actually should be expected. Also, there are so many other environmental factors that may affect plant and litter production and respiration, in addition to temperature. So it seems to me that the premise of the study or at least this particular hypothesis is not really interesting. Again, considerations of peat in the model and observational data for peat initiation and development histories are essential.

On page 17971, line 20-22: the reference to geological ages are confusing and incorrect: the Holocene = 6000 years old = the date of the Last Glacial Maximum. On page 17971, line 1-2: permafrost is not just relic of the pre-Holocene ice ages, but in fact some (a lot) permafrost were formed during the neoglacial cooling in the late Holocene (last 4000-3000 years), or even during the Little Ice Age (600 year ago).

The writing in general is unclear. As a result, it seems to me that the manuscript is premature for submission.

These problems as listed above are just some examples, and similar issues are present

throughout the manuscript.

Below are some specific comments.

Page 17968: Line 2-8: this first sentence is too long, and should break down to 2 or more sentences.

Line 8-10: It is not surprising to have a stronger relationship between soil C density with wetland abundance than with temperature and vegetation productivity (see comments above). So I think that the authors might have identified a wrong focus for the study.

Page 17969: Line 1: what do you mean by "the size of the wetlands"? the extent or abundance or C stock size? Unclear.

Line 3-6: Not much new is here, as many peatland process and global models have extensively dealt with anaerobic respiration under saturated conditions. For example, recent model development in the framework of LPX has made a great improvement in global-scale peatland simulation over the last 20,000 years (See Spahni et al. 2013; Stocker et al. 2014).

Page 17970: Line 4-5: There are updated database as part of NCSCD with more data points for deep soils (down to 3 m). See Hugelius et al. 2013.

Page 17971: Line 19-20: "with no soil of vegetation, just rock": during the ice retreat there were large proglacial lakes as well. In any case, this is too simple and elementary a description for a scientific manuscript.

Page 17972: Line 9-10: how can soil carbon accumulated at a rate measured in mm per year? Unclear writing.

Line 15-15: It appears that the authors infer the ages of peatlands using the rate of 1 mm per year in the previous sentence, so 2-3 m peat would be equivalent to 2000-3000 years ("a few thousand years"). Nowadays there are abundant data available at regional and global scales for the ages of peatlands. For example, MacDonald et

al. (2006) and Korhola et al. (2010) describe some of these large basal peat age databases, and Yu et al. (2010) describe global peatland carbon accumulation history.

Page 17975: Line 5: better refer to "West Siberia Lowland" as in literature, rather than "Yenisei Wetlands", for this large (the largest) peatland region.

Line 9-15: The discussion is not useful, as the simple fact is that wetlands (peatlands in most cases here) simply contain more carbon (see comment above). The reasons for the presence of abundant peatlands in these regions have something to do with topography and hydrology, and climate (precipitation and temperature).

Page 17985: Line 16: delete "In".

In summary, I do not think this manuscript contributes much new to the literature. Some discussions are elementary, and sometimes incorrect factually. In order to make the manuscript publishable, the authors need to identify different focus, rather than spatial correlation between soil C density and wetlands or attempt to correlate soil C density with temperature or GPP. Also, it needs more writing clarity. Many elementary and superficial discussions need to be removed, and the manuscript should only focus on the new and significant contributions of this study to the literature. The authors need to refer to some pertinent literature on the relevant topics as mentioned above (references below). At the present form, I'd recommend the rejection of the manuscript for publication in Biogeosciences. A suitably revised version of this manuscript would be likely an entirely new manuscript.

References: Hugelius, G., et al. 2013. A new data set for estimating organic carbon storage to 3 m depth in soils of the northern circumpolar permafrost region. Earth Syst. Sci. Data 5: 393-402.

Korhola, A., et al. (2010), The importance of northern peatland expansión to the late-Holocene rise of atmospheric methane, Quaternary Sci. Rev., 29 (5-6), 611–617. MacDonald, G. M., et al. (2006), Rapid early development of circu- marctic peatlands and

atmospheric CH4 and CO2 variations, Science, 314, 285–288. Spahni, R.F., et al. 2013. Transient simulations of the carbon and nitrogen dynamics in northern peatlands: from the Last Glacial Maximum to the 21st century. Climate of the Past, 9: 1287-1308.

Stocker, B., et al. 2014. DYPTOP: a cost-efficient TOPOMODEL implementation to simulate sub-grid spatio-temporal dynamics of global wetlands and peatlands. Geosci. Model Dev. 7: 3089-3110. Yu, Z.C., et al. 2010. Global peatland dynamics since the Last Glacial Maximum. Geophysical Research Letters 37, L13402, doi:10.1029/2010GL043584.

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