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# Reply on RC1

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Author comment on "Quantifying the role of moss in terrestrial ecosystem carbon dynamics in northern high latitudes" by Junrong Zha and Qianlai Zhuang, Biogeosciences Discuss., https://doi.org/10.5194/bg-2021-57-AC1, 2021

General comments

This study updates an existing ecosystems model (TEM 5.0) to account for mosses - including moss photosynthesis and respiration, and the influence of the moss layer on soil temperature, moisture and ecosystem N dynamics. The updated model (TEM-Moss) is then used to simulate future carbon dynamics for northern high latitudes, and by comparing the TEM-Moss simulations to those from TEM 5.0, the authors aim to understand the role of mosses in determining the future carbon balance of the region.

This is an important topic – forecasting northern high latitude C dynamics is critical for understanding global change, and mosses are an important component of northern vegetation. Attempting to understand the role of mosses on such a broad scale is novel; there has been some work incorporating the thermal properties of mosses in land-surface models, but I'm not aware of any similar analyses at this scale. It's an ambitious study and in general the manuscript is well structured and logically presented.

My main criticism is around how the TEM-Model is calibrated and validated, and whether the comparison to TEM 5.0 is valid. It may be that I haven't understood the methods fully, but it seems TEM-Moss is based on ecosystem-level calibrations of the 'moss parameters', but TEM 5.0 is not based on representative ecosystem level calibrations. If this is the case, it doesn't make sense to compare the performance of the two models. It also means that the calibrated 'moss parameters' will be compensating for un-calibrated 'non-moss parameters' i.e. the optimal moss parameters for an ecosystem will likely reflect differences in the properties of the higher plant vegetation which have not been captured by the 'default' version of TEM 5.0.

In conclusion, I think the aims of the study are worthwhile, and the general approach to update TEM 5.0 is valid, but a more robust model analysis is needed.

Specific comments

I've made line by line comments below which I hope will be helpful in revising the paper.

Line 41: Define northern high latitudes and the types of ecosystems that are included in the study.

**Response:** Thanks for your comments and suggestions. We changed the sentence to "Northern high latitude ecosystems, which refers to the land ecosystems (>45 ºN) in northern temperate, boreal, grassland and tundra regions".

Line 43. Add some text to highlight the uncertainty around the 1024 Pg figure.

**Response:** We revised the sentence as "contain as much as 1024 Pg soil organic carbon from 0 to 3 m depth".

Line 44-47. "This large amount of carbon is potentially responsive to ongoing global warming". The references supporting this statement are quite old, please cite some more recent literature (e.g. Burke et al., 2017, Koven et al., 2015, Comyn-Platt et al., 2018)

**Response:** Following suggestions, we updated the references.

Line 154: Please provide more detail on the function f(NA).

**Response:** We added "which is a scalar function that depends on monthly N available for incorporation into plant production of new tissue" to describe f(NA).

Line 238: "higher plants" rather than "higher vegetations".

**Response:** We revised it.

Line 238: Did you use a single set of default parameters for the standard TEM model? I'm not sure I follow the reasoning here. Zha and Zhuang 2018 is an arctic study, yet you are using data from temperate forests and grasslands to calibrate TEM-Moss. Did you use the same set of default parameters across all sites? And did you use any other site-level information – apart from the NEP data – when calibrating the model?

**Response:** For TEM 5.0 simulations, we used different sets of default parameters for each vegetation type. Zha and Zhuang (2018) focused on the same region, but we parameterized that TEM version with site level information. In this study, we used site level data to parameterize TEM_Moss, but use the default parameterization of TEM 5.0 to compare with TEM-Moss simulations. Site-level parameterization was conducted based NEP data in addition to site level vegetation and soil information. Some site level data of NEP were used for model validation. Additionally, soil temperature and moisture at validation sites were also evaluated.

Line 247: I don't fully understand how the posterior parameter distributions were generated. As I understand it, the SCE algorithm provides a point-estimate for each parameter, then you treat the 50 independent point estimates as samples from a posterior parameter distribution? Is this correct? Please provide some clarification on this in the text. Please also update the legend in figure 4 – what probabilities do the boxes and tails represent?

**Response:** Yes, the posterior parameter distribution is just the distribution for the 50 independent point estimates. We added the explanation to boxes and whiskers into the figure caption "Boxes represent the range between the first quartile and the third quartile of the parameter values, the red line within box represents the second quartile or the mean of the values. The bottom and top whiskers represent minimum and maximum parameter values, respectively."

Line 250: Zhuang 2010 is a study from the Tibetan plateau, and Zhuang 2015 is northern high latitude wetlands. How do you justify using (I assume calibrated?) parameters from these studies to model C and N dynamics at temperate forest and grassland sites?

**Response:** The correct citation is Zhuang et al. (2003).

Zhuang, Q., A. D. McGuire, J. M. Melillo, J. S. Clein, R. J. Dargaville, D. W. Kicklighter, R. B. Myneni, J. Dong, V. E. Romanovsky, J. Harden, J. E. Hobbie (2003) Carbon cycling in extratropical terrestrial ecosystems of the Northern Hemisphere during the 20th Century: A modeling analysis of the influences of soil thermal dynamics, Tellus, 55B, 751-776, 2003

Line 266: please explain in more detail how the six site-level calibrations for TEM-Moss are applied to the pixel by pixel simulation. Is this on the basis of vegetation class?

**Response:** Yes. We added a sentence "With six site-level calibrated parameters, TEM-Moss is applied to the region pixel by pixel based on vegetation distribution data.".

Line 289: If I understand correctly, TEM-Moss uses calibrated parameters for the representative ecosystems, but TEM 5.0 uses a single set of default parameters. If this is the case, it is not surprising that TEM-Moss performs better than TEM 5.0 in the validation exercise.

**Response:** TEM 5.0 also used the calibrated parameters for representative ecosystems and extrapolated to the region based on the same set of data of vegetation distribution.

Line 359: The number for RH for TEM 5.0 is not correct, and the figure reference should be figure 11b.

**Response:** We corrected the number. But the figure is figure 11a.

Line 412-414: These figures for the moss percentage contribution to NPP seem very high. 20 % of NPP may be realistic for boreal forest (note the Turetsky study is 20% of aboveground NPP, which is probably < 10 % of total NPP) but your study covers the entire northern latitudes from 45oN. Is a moss contribution of >25 % of 21st century NPP really plausible? I would want to see a much more thorough discussion of this, with references to observed data from a wider range of representative ecosystems.

**Response:** Thanks for the comments. Yes, Turetsky et al. (2010) suggested an average contribution of 20% of aboveground NPP from moss in boreal forests. Frolking et al. (1996) even reported a contribution of 38.4% to total NPP by moss at a boreal forest site. These estimates are for the historical periods and our estimates of 17.6% of NPP in the 20th century is at the lower end of their estimates. Our estimates of 28.8% and 27.6% in the 21st century under the RCP 2.6 410 and RCP 8.5 scenarios, respectively, are still similar to the range of existing estimates for the historical period.

Turetsky et al. (2010) conducted a long-term data analysis through literature synthesis, representing a good knowledge about moss contribution to both wetlands and upland ecosystems in Alaska. They found that mosses contributed 48% and 20% of wetland and upland productivity, respectively. In this revision, we revised the sentence to "This is comparable with the results reported by a synthesis study, indicating an average contribution of 20% of aboveground NPP from moss in upland boreal forests and the contribution is 48% in wetlands ecosystems."

Line 440: Changing vegetation is a key limitation, I recommend adding some more discussion here on the likely changes in moss abundance as climate warms, e.g. with respect to changing temperature/ hydrology/ shading by vascular plants.

**Response:** Thanks for the suggestion. We added a few references to explicitly discuss the potential the impacts of moss distribution and abundance on carbon budget in the region. In this revision, we also added the following to further discuss the impacts of

vegetation including mosses on carbon dynamics in the region. "A long-term warming experiments along natural climatic gradients, ranging from Swedish subarctic birch forest and subarctic/subalpine tundra to Alaskan arctic tussock tundra concluded that both diversity and abundance of mosses are likely to decrease under arctic climate warming (Long et al. 2012). Similarly, total moss cover declined in both heath and mesic meadow under experimental long-term warming (by 1.5–3 °C), driven by general declines in many species (Alatalo et al., 2020). Due to global warming, significant losses in moss diversity are expected in boreal forests and alpine biomes, leading to changes in ecosystem structure and function, nutrient cycling, and carbon balance (He et al., 2015). "