Biogeosciences Discuss., referee comment RC1
https://doi.org/10.5194/bg-2021-57-RC1, 2021

Comment on bg-2021-57

Anonymous Referee #1

Referee 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-RC1, 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.

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

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)

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

Line 238: “higher plants” rather than “higher vegetations”

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?

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?

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?

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?

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.

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

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.

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.

References

BURKE, E. J., EKICI, A., HUANG, Y., CHADBURN, S. E., HUNTINGFORD, C., CIAIS, P., FRIEDLINGSTEIN, P., PENG, S. & KRINNER, G. 2017. Quantifying uncertainties of permafrost carbon-climate feedbacks. *Biogeosciences,* 14**,** 3051-3066.

COMYN-PLATT, E., HAYMAN, G., HUNTINGFORD, C., CHADBURN, S. E., BURKE, E. J., HARPER, A. B., COLLINS, W. J., WEBBER, C. P., POWELL, T., COX, P. M., GEDNEY, N. & SITCH, S. 2018. Carbon budgets for 1.5 and 2 °C targets lowered by natural wetland and permafrost feedbacks. *Nature Geoscience,* 11**,** 568-573.

KOVEN, C. D., SCHUUR, E. A. G., SCHÄDEL, C., BOHN, T. J., BURKE, E. J., CHEN, G., CHEN, X., CIAIS, P., GROSSE, G., HARDEN, J. W., HAYES, D. J., HUGELIUS, G., JAFAROV, E. E., KRINNER, G., KUHRY, P., LAWRENCE, D. M., MACDOUGALL, A. H., MARCHENKO, S. S., MCGUIRE, A. D., NATALI, S. M., NICOLSKY, D. J., OLEFELDT, D., PENG, S., ROMANOVSKY, V. E., SCHAEFER, K. M., STRAUSS, J., TREAT, C. C. & TURETSKY, M. 2015. A simplified, data-constrained approach to estimate the permafrost carbon-climate feedback. *Philosophical Transactions of the Royal Society A: Mathematical, Physical and*

Engineering Sciences, 373.