Biogeosciences Discuss.,
doi:10.5194/bg-2016-161-AC2, 2016
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Biogeosciences Discussions
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Interactive comment on "Soil carbon response to land-use change: Evaluation of a global vegetation model using meta-data" *by* Sylvia S. Nyawira et al.

Sylvia S. Nyawira et al.

sylvia.nyawira@mpimet.mpg.de

Received and published: 24 August 2016

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Response to D. Schepaschenko of the paper entitled "Soil carbon response to

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land-use change: Evaluation of a global vegetation model using observational meta-analyses"

Ref.: bg-2016-161

It seems our point by point responses at the quick report stage of the manuscript unfortunately were not passed to the reviewer. The requested changes were accounted for before the paper was published on discussions.

Below are the reviewers suggestions (***bold italic font***) and our responses to each point (normal font) based on our changes prior to publication as discussion paper and additional changes in response to the other reviewers' comments.

This study demonstrates an approach for evaluating perfomance of DGVMs to account soil carbon changes following land-use change. It is important to estimate how far DGVM simulations are from the reality and which model setup is closer to observation. The article has rich discussion section, where most of the questions are covered. The paper is well written. Finally I suggest accepting the paper with minor revisions.

Application of universal function for scaling soil carbon pool to 100 cm (page 3, line 15) could introduce substantial bias. In many cases carbon pool changes in top layer only, but you propagate observed value down to 1m and increase therefor the magnitude. That might be the reason of having higher amplitude in meta-data. I would either suggest use soil map and soil specific equations or make the analysis for the top layer only if no observation for deep soil layers

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available.

The reviewer is correct that the scaling of the meta-data with depth is quite uncertain. We didn't find reliable land-use specific functions for scaling soil carbon densities. Therefore, we have removed the scaling of the meta-analysis and discussed the depth issue as a major challenge in the model-data comparison.

Selection of area/climate for simulation is important. By including extra area (where LUC not going to happen) or excluding potential LCC area, you might bias the overall estimation. Authors suggest three different extends and each is not ideal:

1. ***Entire vegetated area of the land surface (too big. Low chances e.g. for forest on permafrost to be converted to cropland)***
2. ***Area where LUC has taken place historically (too narrow, LUC might came to new places, e.g. tropical deforestation)***
3. ***Where meta-data were available (even more narrow)***

I would suggest to overlay PFT map with climate one and define climatic patterns where one or another PFT can appear. This would cover all current and potential LUC.

This comment from the reviewer indicates that our descripition on how we selected model regions for comparison with the meta-data as described in section 2.4 was not clear. We actually did not suggest that the meta-data can be used to evaluate the entire vegetated land surface as pointed out by the reviewers' approach 1. We have revised this section in the manuscript and added a more detailed explanation on why

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we used the other two approaches for selecting the regions for comparison (Approach 2 and 3). The reviewer is correct that these two approaches may not be representative of regions where LUC has not taken place historically. However, the meta-data on LUC exist only in regions where LUC has taken place historically. The only approach not introducing biases is thus to assess regions of LUC, since future LUC may move to regions where the meta-data are not representative. Our study presents a method to identify suitable models that can also be used to project soil carbon changes due to future LUC. However, such projections are beyond the scope of our study, which focusses on model evaluation.

Minor comments

Page 7, line 8. In fact, forest might have lower NPP compare to cropland, but most of the dead matter come to soil surface where decomposition is slow

We have re-written the sentence to show that we are explaining the reasons for the simulated increase in the model. We have further clarified that the change in soil carbon is driven by on average higher productivity.

Page7, line 10. Here could be different explanation. Soil respiration is higher in cropland compare to grassland because of the tillage. That is why having similar NPP grassland accumulate more carbon.

We agree with the reviewer that tillage leads to more soil carbon losses in croplands compared to grasslands. However, the sentence explains the soil carbon response of our standard model simulation, which does not include tillage. We demonstrate the effects of crop management practices with the simulation accounting for crop harvesting

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and further discuss the implications in section 4.2.1.

Page 9, line 10 "Without accounting for crop harvesting" makes sense only to demonstrate how DGVMs are far from reality while not taking into account such evident things like management and disturbances.

We have rephrased this sentence to indicate that the results are for the sensitivity simulations neglecting burning in our standard model simulation, which does not account for crop harvesting.

Page 9, line 10 "switching off disturbances in grass... leads to the right direction of soil carbon change" I hope we aim is to describe the reality with the model, but not just have a similar estimation. Disturbances exist. If result is better without disturbances, then the model makes mistake in its different part.

We agree with the reviewer that the phrasing of this sentence suggests that we switch off burning to get to the right direction of change. We have rephrased the sentence to clarify that this result represents a sensitivity simulation. In addition, we have included an additional sentence to explain that we aim to show that the choice of the vegetation types affected by disturbances in DGVMs has an influence on the soil carbon response to LUC.

Page 10, Line 1. Grassland and cropland NPP generally larger compare to forest in temperate region also because they allocated on best locations (soil, slope, etc). However if you try to convert existing forest to grassland or cropland you

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might not get increase of NPP.

The meta-data include local-scale measurements that are mainly done using paired plots designs; hence such sub-grid scale heterogeinities are accounted for in the meta-data. Therefore, an assessment of the soil carbon response to LUC associated with such heterogeinities is beyond the scope of our study.

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