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## ***Interactive comment on “Global soil organic carbon stock projection uncertainties relevant to sensitivity of global mean temperature and precipitation changes” by K. Nishina et al.***

### **Anonymous Referee #1**

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#### General comments:

The study carries out an analysis of the extent to which the predicted magnitude of soil C stock changes during the 21st century differ between a range of biome models. The study also looks in detail at the spatial variability in the location of carbon gains and losses in the different models. The large range in the predicted changes in SOC stocks, as well as the complete lack of agreement in terms of geographical patterns of carbon gain and loss, is a sobering conclusion and one which indicates how far away we are in terms of having confidence in predicting the magnitude of this key carbon cycle feedback to climate change. For this reason, I feel this study provides a valuable and thought provoking contribution to the subject.

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However, the study would be even more valuable if some of the following issues were considered: There is some mention within the results of to the extent to which changes in soil C stocks are related to changes in C input rates from plants versus the direct impacts of temperature on decomposition rates (p1043 lines 1-14). In my opinion, it would be useful to develop this discussion in more detail to allow readers to evaluate whether or not the different parameterisations of the temperature response functions are really the key issue, especially given the similarities between some of the temperature response functions. Maps plotting residence times of SOC and how these change over time would be useful. As this is a paper about uncertainty in future soil C stocks, it would be highly desirable to indicate how much of the uncertainty and differences between models is related to vegetation feedbacks (inputs), versus how much is related to the different parameterisations of SOC dynamics.

The changes in inputs versus decomposition rates also has implications for determining the impact of changes in precipitation. Declining precipitation may reduce C inputs for plants as well as decomposition rates and the fact that both changes operate in the same direction, at least in water limited areas, may explain why the overall effect is smaller than the temperature impacts. Again it would be useful to distinguish whether precipitation changes alter inputs and outputs for the different biomes, but have less effect on stores (see also Schwalm et al. 2010 *Global Change Biology* 16, 657–670).

The study is correct to emphasise that information on C stocks in the soils of different biomes are not as good as we would like them to be. However, there is a lot of information available (e.g. Harmonized World Soil database) and it is unlikely that global databases will suddenly improve dramatically in the near future. A more informative sensitivity analysis would have also evaluated these models against our best estimates of the global patterns of soil C storage. I'm not suggesting that this must be done in detail in this paper, but would it be possible to add an additional line on Figure 5a indicating our best current estimate of the distribution of soil C stocks, similar to the line for plant C stocks in Figure S2a?

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Finally, one major limitation which is not mentioned is the failure to include plant-soil interactions, and linkages between nutrient and carbon cycling in some or all of these models. While it is informative to highlight the differences between the current versions of the models, it is likely that feedbacks will differ strongly if the role of decomposition in supplying nutrients is considered directly. Some consideration should be given to this major limitation when predicting future soil C stocks.

Specific technical comments:

Page 1043 line 19: The reference should be to Table 2.

Page 1047, lines 20-23: The values from the Raich and Zhou studies do not reflect the direct effects of temperature on decomposition rates, and I would recommend removing this part of the discussion.

Page 1049, lines 1-12: It would be useful to provide a fuller evaluation of how seasonally frozen soils are represented in high latitude locations and how the model deals with warming-induced thaw. This is critical to evaluating which models are most likely to be representing high-latitude feedbacks appropriately. Again, this emphasises the need to distinguish between inputs and outputs. Are the losses from high latitude systems in LPJmL related to the inclusion of permafrost dynamics, or reductions in plant productivity (boreal browning).

Tables

Table 1:

More details are required in the figure legend. For example, what does 'compartment' refer to?

Figures:

Figure 5:

The panels do not seem to be in the same order as indicated in the figure legend.

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Is there any way of correcting the values for the different latitudes by surface area. At the moment it appears that the impacts of CO<sub>2</sub> fertilisation on SOC will be greatest at mid to high latitudes (Fig 5c), whereas previous studies suggested a positive relationship between temperature and CO<sub>2</sub> fertilisation, at least for net primary productivity (e.g. Hickler et al. (2008) *Global Change Biology* 14, 1531–1542). Is this analysis suggesting that the response for carbon storage in soils should show a different spatial pattern, and to what extent is the latitudinal pattern controlled by latitudinal differences in land area rather than changes in SOC per unit area?

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