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Interactive comment on "Changes in soil organic carbon storage predicted by Earth system models during the 21st century" *by* K. E. O. Todd-Brown et al.

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Reviewer comments are in italic and our replies are not.

Throughout, but especially in the introduction I feel like more careful citation of recent literature would provide more robust support for claims made in the paper. The citations I suggest below are just that, and a modest amount of effort help cast the framework for this paper in a contemporary light. For example discussion of: Terrestrial C-cycle uncertainty (P 18972 L1-10); 2. SOC results in FACE studies (P 18972 L 21-20); and Processes missing from ESMs (P 18973 L 1-11 P 18987-18988) Could include: 1. Other CMIP5 results (eg Arora et al 2013) 2. Broader survey of FACE results (eg

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Phillips et al 2011; Hungate et al 2009) 3. Highlight lack of permafrost dynamics in cmip5 models (Koven et al 2011; 2013).

We extended the citations for the FACE and CO2 elevation studies and added a new paragraph tying the results from Todd-Brown et al 2013 to potentially under-represented mechanisms including permafrost. Thank you for the citation suggestions.

Please clarify how decomposition rates and turnover times (and their changes over the 21st century) were determined in each model and in different analysis? Was this consistently done using the reduced complexity model (eq 4,6, 7), or in some analysis estimated using pools/fluxes (ie SOC/NPP)? This comes up in section 2.3; 3.1 Fig. 1 (Turnover time); and 3.4 Fig 4 ('constant turnover time', and 'turnover time evolves').

In all analyses the decomposition rates and turnover times were calculated from the heterotrophic respiration and soil carbon stocks per Section 2.1 3rd paragraph. I believe the confusion arises from the use of an intrinsic decomposition rate term in the reduced complexity model. The intrinsic decomposition rate parameter has been renamed (κ) instead of (k0) to reduce the confusion, and we've added a clarifying statement after Eq. 7 that dk is not directly dependent on κ.

I really like the analysis presented in Fig 4 and section 3.4 that isolates NPP and temperature effects on SOC pools across models. It's curios however that this is the only analysis presented that considers it's historical period. I realize that Todd-Brown has another paper focusing on SOC dynamics over this historical period, but I wonder if results from that paper could inform the analysis here? For example, are drivers in changes in SOC turnover similar over this historical period similar to those projected over the 21st century (eq. 1 2; Figs 5 6).

We've extended discussion of the benchmark results from Todd-Brown et al 2013 in the introduction. We also note in the methods and discussion that most of the change in SOC and change in the drivers occurs in the 21st century.

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Thumbnail plots are nearly impossible to read in the current draft. In the final published version, these will look slightly better, but I feel taking some time to make plots larger by removing redundant axis labels between plots would be helpful.

We've reworked Figure 1 to reduce white space and removed the redundant axis on figure 4.

I'm not a huge fan of presenting new analyses late in the discussion of a paper, and feel some background information regarding Fig 7 (really a table?) is warranted in the methods and/or results. I know co-author Randerson (and others) on this paper invested considerable effort in model benchmarking, but throwing these results in at the end of the paper seems too much like an appendix that should either be fully embraced and described in the paper or left out. For what it's worth I think these results are valuable and should be included.

We've added new methods and results sections to introduce this benchmarking and incorporated the justification into the introduction.

Finally, the wealth of information presented here and the clarity with which it is presented are commendable. That being said, I'd encourage the authors to explore implications of their findings in greater detail specifically relating to 1) spatial patters of SOC response and 2) (no) effects of hydrologic change on SOM dynamics. Spatial patters of SOC gains, especially high soil C gains in high latitude systems is shocking to me. This is discussed in section 4.3, but this disquieting result should be highlighted throughout (especially in the abstract!). To me, this rings huge alarm bells that C-cycle climate projections from cmip5 models likely present an unrealistically optimistic view of future terrestrial C dynamics. I appreciate the authors' restraint in interpreting their findings, but feel stronger language could be used in the discussion of results (and would be justified). Second, lack of soil moisture response is consistent with Todd-Brown's previous work, but contradicts recent publications that stress the importance of soil moisture in experimental literature (loads of references) and in global SOM sim-

ulations (e.g. Ise Moorcroft 2006, Falloon et al. 2011, Exbrayat et al. 2013). Is it worth discussing the lack of a soil moisture effect across models?

We added two sentences about the spatial patterns in the abstract to point out the accumulations in the high latitudes and how they are inconsistent with empirical data. These sentences reinforce the paragraph on these results at the end of the discussion. We also extended the discussion of the lack of moisture effect in both the introduction and discussion.

Minor comments / suggestions follow: P 18980 L 4: Add SOC to "..most of the global SOC gain . . ."

Done

P 18982 L 22-23: please clarify. What was 2-50 times greater? Is this the range in "constant turnover" "constant inputs" is 2-50 times greater than projected SOM changes?

This statement is confusing and redundant with earlier statements in the paragraph so we deleted it.

P 18983 L 1-12 (and Fig 5): Please reorder figures and/ or text so they are organized in the order in which they are presented. Also do any of these results refer to Fig 5d?

We've reordered figures. Fig 5d is referred to in section 3.4.

P 18986 L 29: How much of this terrestrial C loss was from soil fractions?

This terrestrial carbon loss was not separated into soil vs vegetation fractions.

Table 1: I'm surprised tropical rainforests are consider "mid-latitude biomes". Could another description be used? "mid and low-latitude biomes" or "temperate tropical biomes"

Excellent point. We've changed 'mid-latitude biomes' to 'other biomes'.

Fig 5: Choice of red/green lines will present unnecessary challenges for some readers.

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Why not red/blue? Also grey line is impossible to see (maybe better online?)

We've made the red solid line a red dotted line, the grid line to a green dashed line, and the black dashed line to a black solid line.

Fig 6: There seems to be a mismatch between figure layout and figure captions (6b 6c)?

Thank you; this has been corrected.

Fig 7: Isn't this a table?

We will discuss with the copy editor on whether we can have a color dependent table.

Interactive comment on Biogeosciences Discuss., 10, 18969, 2013.