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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.

Anonymous Referee #1

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Changes in soil organic carbon storage predicted by Earth system models during the 21st century by Todd-Brown and others provides an insightful, careful analysis of soil C projections made by cmip5 models. The paper is clearly written and well organized. My suggestions are largely to help clarify ideas in the text, and should be easily addressed by the authors.

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 should help cast the framework for this paper in contemporary light. For example discussion of: 1. Terrestrial C-cycle uncertainty (P 18972 L1-10); 2. SOC results in FACE studies (P 18972 L

21-20); and 3. Processes missing from ESMs (P 18973 L 1-11 & P 18987-18988)

Could include: 1. other CMIP5 results (e.g. Arora et al. 2013) 2. Broader survey of FACE results (e.g. Phillips et al. 2011; Hungate et al. 2009) 3. Highlight lack of permafrost dynamics in cmip5 models (Koven et al. 2011; 2013)

Please clarify how decomposition rates and turnover times (and their changes over the 21st century) were determined in each model and in different analyses? Was this consistently done using the reduced complexity model (e.q. 4, 6, & 7), or in some analyses estimated using pools / fluexes (i.e., 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").

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 curious, however, that this is the only analysis presented that considers this historical period. I realize Todd-Brown has another paper focusing on SOC dynamics over this historical period, but I wonder if results from that paper that could inform the analysis here? For example, are drivers in changes in SOC or turnover times similar over this historical period similar to those projected over the 21st century (eq. 1 & 2; Figs 5 & 6).

Thumbnail plots are nearly impossible to read in the current draft. In the finally 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.

I'm not a huge fan of presenting new analyses late in the discussion of a paper, and feel some background information relating to Fig 7 (really a table?) is warranted in the methods and / or results. I know co-author Randerson (and others) on this paper have invested considerable effort in model benchmarking, but throwing these results in at the of the paper seems too much like a 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.

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 simulations (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?

Minor comments / suggestions follow:

P 18980 L 4: Add SOC to "..most of the global SOC gain . . ."

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?

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?

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

Fig 5: Choice of red/green lines will present unnecessary challenges for some readers. Why not red/blue? Also grey line is impossible to see (maybe better online?)

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

Fig 7: Isn't this a table?

Additional references to consider (and mentioned above)

Arora, Vivek K., et al. (2013) Journal of Climate, 26, 5289–5314. Falloon et al. (2011) GLOBAL BIOGEOCHEMICAL CYCLES, 25: GB3010, doi:10.1029/2010GB003938 Hungate, et al. (2009) Glob. Change Biol. 15, 2020–2034 Ise & Moorcroft (2006). Biogeochemistry 80:217–231 Koven, et al. (2013). Biogeosciences 10:7109-7131.

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