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Interactive comment on "Causes of variation in soil carbon predictions from CMIP5 Earth system models and comparison with observations" *by* K. E. O. Todd-Brown et al.

C.D. Jones (Referee)

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Review of Todd-Brown et al, on Causes of variation in soil carbon predictions from CMIP5 ESMs and comparison with observations.

This is a well written and well explained manuscript dealing with an important aspect of the carbon cycle simulated by state-of-the-art CMIP5 GCMs. As representation of the carbon cycle in these mainstream climate models becomes more common it is increasingly important to evaluate their performance. The ability to simulate the right amount and distribution of the world's major carbon stores is an obvious and important quantity to evaluate, but so far this has not been done in depth. This study uses

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observationally-based datasets to look at the variation in soil carbon storage between CMIP5 models.

As an area of research there is clearly much more that could be done - but beyond the scope of this paper. Below are some ideas where the community could build on this initial study - the authors might want to include some discussion around future applications in their text. Also below some specific points. Overall i recommend publication after these minor revisions.

Chris Jones

I think the real goal of data-based evaluations like this are two-fold. Firstly as a use for model development they can identify model deficiencies and help to show when they have been improved. They can also be used, though, as a direct constraint on the model behaviour, if the quantity being observed can be linked to future changes being projected by the models. Hence I would like to see some discussion of two points here:

1. you show that the models differ, but not yet why. Are differences due to different soil-C model structure or simply different climate and NPP simulations by the rest of the model? You should be careful not to imply that any errors in the soil carbon simulated here are ONLY due to the soil carbon processes modelled. The climate in the models may also be wrong - e.g. if it is too hot/cold/dry/wet in an area then the soil carbon will be wrong - even for a perfect soil carbon model. So I'm nervous about statements regarding the ability to model soil carbon per se - for these you would be better to run the land-surface models offline driven with observed climate data. You say somewhere that you have evaluated the models ability to simulate soil carbon "due to spatial differences in temperature and moisture" - but this isn't really true - you've looked at the spatial distribution of soil carbon - but not evaluated how well the climate itself matches the spatial patterns of the real world. In the fully coupled models you can only evaluate the fully coupled system - but you don't actually know where any errors originate...

2. can you discuss if there is a link (or not) between the model's initial state and it's

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projection of future changes. e.g. I'd expect the MPI model with 3000 PgC to have a much greater ability to lose carbon under climate change than CESM... Is there an obvious relationship between the initial pool size and the sensitivity to climate change? If so, then your evaluation is at least part way towards becoming a useful constraint. You have shown your simple model can reproduce much of the spatial information in the soil carbon fields, but can it also predict their time-changes under changing climate? If you forced the redocued-complexity model with the 21st century changes in T, moisture and NPP form the models you could compare the predicted with actual soil carbon changes. One of two things would happen:

a) your simple model would predict ESM changes well - this would imply that using observations to constrain the present day distribution also constrains the future proejctions. This would be a hugely important result

b) (perhaps more likely) your simple model does not capture transient changes as well as spatial patterns. This indicates that processes controlling future changes are different from those controlling the spatial distribution. Internal carbon-pool dynamics has been shown before to make big differences to transient rates-of-change without affecting long-term sensitivity (Jones et al., 2005, GCB). This doesn't mean that getting the spatial distribution isn't important, but that there are other factors to get which need evaluating with other data.

[Jones et al., Global climate change and soil carbon stocks; predictions from two contrasting models for the turnover of organic carbon in soil, Global Change Biology (2004) 10, 1-13, doi:10.1111/j.1365-2486.2004.00885.x]

You stop short of actually defining a metric (i.e. a single number to summarise a model's skill) and hence ranking the models - have you thought about doing this?

I would also like to see the skill-scores for soil carbon put in some context of skill scores for other quantities. You make statements about whether the models perform well or poorly - but how do we know what score represents "well" or "poor" in this

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respect? What are equivalent scores for global distributions of T or precip? I'd niaively expect higher scores for temperature, but precip is harder to simulate. How much better or worse are models at simulating carbon than climate? Overall I was actually fairly impressed at some of the models distributions (the correlations against biomes make quite a few models look good). My personal take would be to reverse you conclusion - don't start off saying howpoor the models are at grid level, but start off saying that quite a few of them do a good job at global and biome scale, but errors get bigger (as do uncertainties in the datasets) at very fine scales. I think this is a fairer representation of the situation.

Minor points:

- on first reading the title the word "predictions" made me expect an analysis of future changes in soil carbon. Whilst not wrong as such, perhaps "simulations" would be a better word in the title.

- page 14442, line 21. I don't agree nearly half the models have Nitrogen interactions. CESM and NorESM do, but only because they use the same land-surface model so are not independent in this respect. I don't know much about the BCC model - can you clarify if this includes the nitrogen cycle or not? I didn't realise it did. If not, then this really only leaves 1 model with N included.

Sec. 2.2 on datasets:

- I think you could discuss more firmly that none of these models really try to simulate organic-rich peat soils. So the comparison with NCSCD is perhaps not like-for-like. The HWSD dataset is more like what the models should be aiming for - I would then discuss that omission of peat and permafrost organic soils is a model gap - rather than a model "error". It's certainly important to do it - but I don't think we'd expect the models to be able to get the right answer right now.

- when you estimate uncertainty in the data it looks like you underestimate it - HWSD

is WELL outside your 95% confidence limits for NCSCD. Can you explain why? either the confidence limits are too narrow, or they represent different things. i.e. why does the HWSD bar in figure S6 not get into the shaded region?

- sec 2.4 reduced complexity model. You assume here a balance in soil carbon (NPP = R). But this isn't true for 1990s. Can you quantify the error term this introduces? NPP and R are both available for the CMIP5 models.

- can you speculate why this reduced complexity model doesn't pick up the models' dependence on soil moisture. We know the models include a dependence on moisture in them so why do their results not allow this to be identified? Falloon et al, 2011 (GBC) show how different soil moisture curves in a model affect the distribution and future changes in soil carbon.

(Falloon et al, "Direct soil moisture controls of future global soil carbon changes: An important source of uncertainty", GLOBAL BIOGEOCHEMICAL CYCLES, VOL. 25, GB3010, doi:10.1029/2010GB003938, 2011)

- How do you define biomes in the models? I assume you define these from the observed climatology and keep a constant map for the models. But some models have vegetation dynamics and others specify the land-cover - e.g. HadGEM2-ES might have grasses simulated in areas you class as forest. So you should at least mention that for models with this extra degree of freedom it is harder to get the right answer. Maybe an extra column in your table of model properties?

- figure 2. Can you explain in the text how "turnover time" is calculated here? At equilibrium it would simply be Cs/NPP, but the models are not in equilibrium in the 1990s. Have you used Cs/R? or is it the diagnosed 1/k from your simple model?

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