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Biogeosciences Discussions

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.

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Thank you for your comments. We would like to submit the following responses in italic.

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 deïňĄciencies 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 soilC 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 ofïňĆine 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...

Response: You are absolutely correct that errors in the soil carbon are likely due to errors in the simulated environment and parameterization in addition to any model structural shortcomings. We've revised the discussion section to try to emphasize this. We've added comparison with a MODIS NPP product and CRU air temperature (biome comparisons are attached and additional metrics are added to Table 3). It's clear that NPP is more variable between the models than air temperature, which has a high correlation at the biome level to soil temperature.

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

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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 proejc-tions. 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 con-trasting 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]

Response: This an excellent point however we feel it is beyond the scope of this current paper. We have a second paper in the works to address this question.

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

Response: We've chosen not to select a single skills score and instead provide a number of different measures which future studies can select from when evaluating model performance. Which metric is selected will depend on the goal of the study and scale being considered.

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 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

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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 how poor 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 ïňĄne scales. I think this is a fairer representation of the situation.

Response: We've added a comparison of the air surface temperature with the CRU data product (Taylor scores 0.95 to 0.98, biome regression R2 greater then 0.98) as well as NPP estimations from MODIS (Taylor score 0.70 to 0.87, biome regression R2 0.86 to 0.99) to provide some context for the accuracy of the variables driving soil carbon (Taylor scores 0.21 to 0.68, biome regression R2 0.39 to 0.97). The new biome comparisons are attached. These comparisons suggest that the ESMs have relatively similar representations of temperature but moderately different representations of NPP at the global and biome levels and that there may be a higher confidence in the temperature representation in the ESMs then NPP. We've also added a citation for Koven etal 2012 which looks at soil temperature across the CMIP5 models.

Response: We've also modified the discussion of the paper to stress the match at higher spatial scales of the soil carbon as you suggested.

Minor points:

- on ïňĄrst 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.

Response: We have change the references to 'predictions' to 'simulations'. Thank you for the suggestion.

- page 14442, line 21. I don't agree nearly half the models have Nitrogen interactions.

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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.

Response: According to Ji et al 2008, BCC-CSM1.1 does have a nitrogen component in its decomposition model, from the paper: "Soil carbon and nitrogen dynamics module(SOM). This module was newly added to AVIM to constitute a new version AVIM2. [. . .] The transformation and decomposition rates of soil organic carbon are related to temperature, wetness, texture and nitrogen concentration in soil layer." However you are correct that fewer then half of the models have a nitrogen component and that sentence in section 2.1 has been adjusted to reflect this as follows: "Three ESMs include nitrogen interactions with soil carbon."

Sec. 2.2 on datasets:

- I think you could discuss more ïňĄrmly 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.

Response: We do mention that models will likely have problems with high carbon soils and this is one of the reasons we conducted a biome analysis as well as at the grid-by-grid and global levels. Specifically "We expected ESMs to represent high latitude soils poorly because many of the terrestrial decomposition models were developed for mineral soils, as opposed to the organic soils found in many high latitude ecosystems (Neff and Hooper, 2002; Ping et al., 2008; Koven et al., 2011)."

- when you estimate uncertainty in the data it looks like you underestimate it - HWSD is WELL outside your 95% conïňĄdence limits for NCSCD. Can you explain why? Either

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the coniňĄdence limits are too narrow, or they represent different things. i.e. why does the HWSD bar in ïňĄgure S6 not get into the shaded region?

Response: We've added a sentence to addressed this point in the data uncertainties section of the Discussion 4.1. "At high northern latitudes, there was substantial disagreement between the two data sets. NCSCD estimates were between 380 and 620 Pg C, whereas the corresponding HWSD estimate was only 290 Pg C. However, the HWSD did not include regional uncertainty information, meaning that the two estimates may agree once a formal uncertainty analysis has been undertaken. Furthermore, the spatial correlation between the HWSD and NCSCD data sets was only 0.33. This correlation was higher than any model-data correlation for the same region, but clearly indicates that reconciling differences in the empirical estimates is especially important at high northern latitudes. In particular, these efforts should aim to quantify errors and uncertainty associated with mapping the distribution of different soil types with a 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.

Response: In introducing our reduced complexity model in the methods section 2.4 we have added the following justification: "Mean grid differences between NPP and R across the ESMs ranged from 0.01 to 0.12 kg m-2 yr-1 or between 1% and 20% of the mean grid NPP for the 1995 – 2005 period. Thus the ESMs were not exactly at steady state, but we assumed steady state here to simplify our analysis."

- 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 identiïňĄed? 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

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carbon changes: An important source of uncertainty", GLOBAL BIOGEOCHEMICAL CYCLES, VOL. 25, GB3010, doi:10.1029/2010GB003938, 2011)

Response: We've added the following sentence to the discussion: "In contrast, soil moisture did not play an important role as a driving variable for soil C in our reduced complexity models, indicating that for most models this variable did not strongly control spatial patterns of soil carbon accumulation (Table 4). Yet soil moisture is known to serve as fundamental control on decomposition (Falloon et al., 2010), and at a global scale much of the soil carbon pool likely resides in areas where excessive water content impedes organic matter oxidation. One likely possibility is that the interaction of topographic controls on soil texture and soil moisture are not well represented in the current generation of ESMs, and that new approaches are needed for estimating the fraction of grid cells that are poorly drained, and the way that organic soils form in these area (Ise et. al., 2008)."

- How do you deïňĄne biomes in the models? I assume you deïňĄne 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?

Response: The biome mask is constructed from a MODIS vegetation type product. Details can be found in Section 2.2.2 and in the caption of Figure S2. "To evaluate ESM soil carbon simulations across biomes, we aggregated HWSD estimates and model simulations of soil carbon within biomes. The biome map was based on the land cover data product from the MODIS/TERRA-AQUA mission (NASA LP DAAC, 2008) (Figure S2). We assigned one of 16 land cover types to each 1°x1° grid cell by taking the most common land cover from the original underlying 0.05°x0.05° grid. Each 1°x1° grid cell was assigned to one of 9 biomes: tundra, boreal forest, tropical rainforest, temperate forest, desert and scrubland, grasslands and savannas, cropland and urban, snow and *ice, or permanent wetland. Details for the biome construction can be found in Figure S2."*

- ïňAgure 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?

Response: Turnover time in figure 2 is calculated from C/NPP. We've added a sentence in the caption to clarify this and clarified this in the discussion section.

Interactive comment on Biogeosciences Discuss., 9, 14437, 2012.

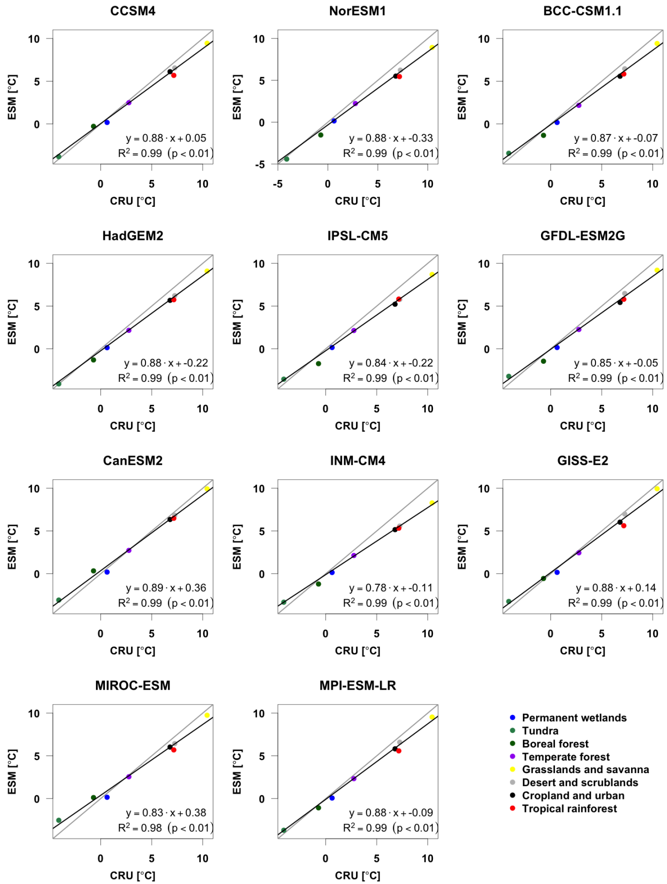

Fig. 1. Mean simulated air temperature (2 m, 1995-2005 mean), versus mean Climate Research Unit (CRU) temperature (2 m, observed 1995-2005) by biome for each Earth system model.

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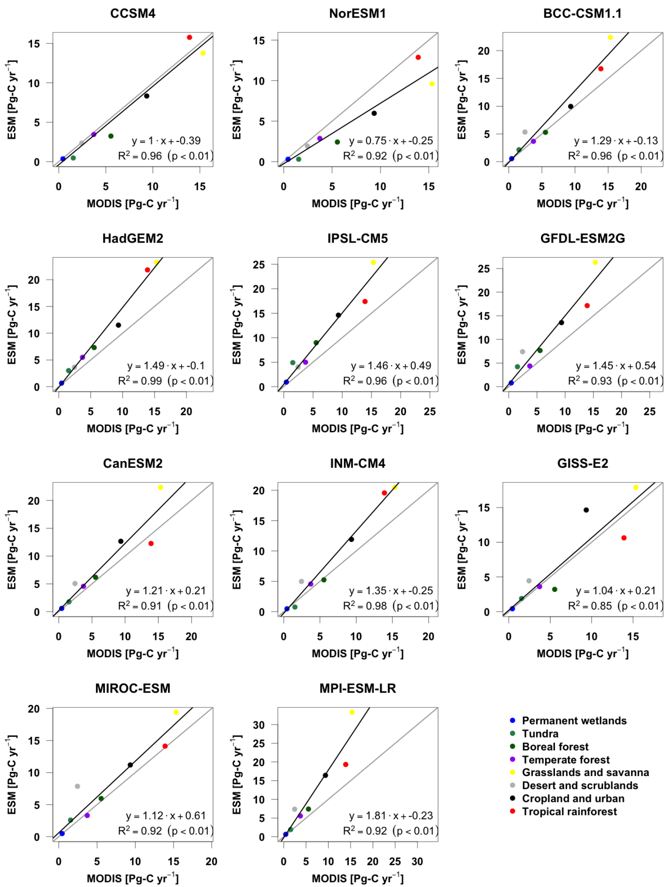

Fig. 2. Biome comparison between Earth system model NPP and MODIS NPP (simulated 1995-2005 mean).

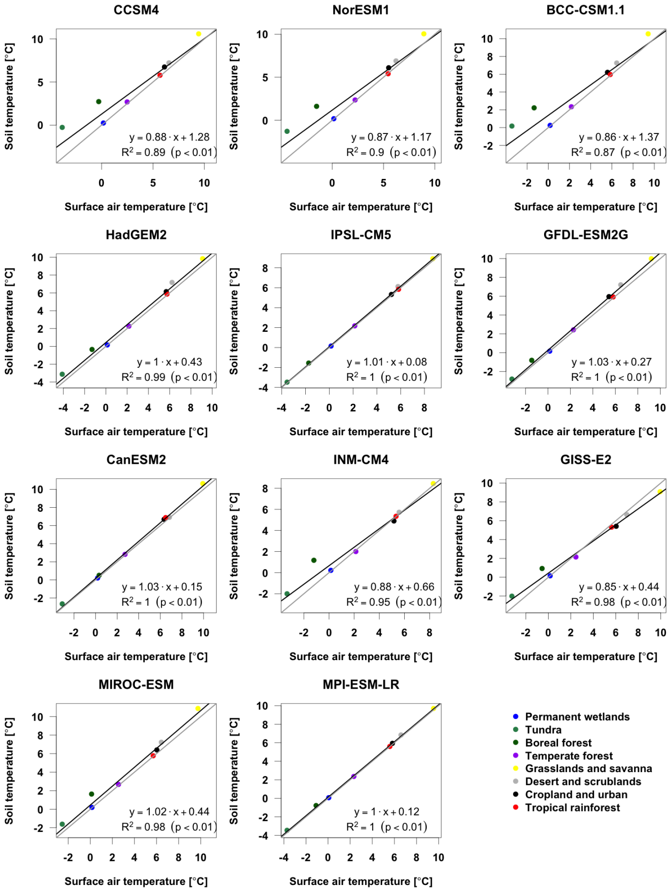

Fig. 3. Mean soil temperature (top 10 cm) versus mean surface air temperature by biome for each Earth system model (simulated 1995-2005 mean).

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