Biogeosciences Discuss., 9, C8030–C8041, 2013
www.biogeosciences-discuss.net/9/C8030/2013/

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.

K. E. O. Todd-Brown et al.

ktoddbro@uci.edu

Received and published: 7 February 2013

We would like to update our initial response to this comment

CD Jones – Review Responses

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 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 speciïňĄc 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 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...

BGD

9, C8030–C8041, 2013

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

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 extensively to try to emphasize this in three subsections: Uncertainties in the comparative datasets, differences in the driving variables (like NPP and soil temperature), and, finally, differences in parameterization and model structure. We've also added a comparison with a MODIS NPP product and CRU air temperature to the manuscript. These results are shown in new columns in Table 3 and new supporting figures S7 and S8. We address the details of this comparison later in this response.

In addition, we added a final figure (Figure 5) that shows our reduced complexity soil carbon model can explain most of the variation in soil carbon totals across the different ESMs (R2 0.93) using the multi-model mean soil temperature, but model-specific soil carbon parameterizations and NPP (Figure 5D). This is an additional line of evidence supporting the conclusion that temperature variability is not a large direct driver of differences across models.

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:

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.

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. Preliminary analysis shows that the change in soil respiration across the 20th and 21st centuries in the ESMs is well explained by a reduced complexity model similar to the one described in this manuscript; however soil moisture is a relevant variable in more models. Both soil respiration and NPP increase across the models, but the total change in soil carbon is highly variable. The change in soil carbon does not appear to be linked to the initial size of the pool. However all analysis are still preliminary.

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 skill 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. These statistics are presented in Table 3 and include Pearson correlation coefficients, Taylor scores, and root mean squared errors.

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 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 0.87 to 0.97) as well as NPP estimates from MODIS (Taylor score 0.70 to 0.87, biome regression R2 0.85 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 0.38 to 0.95). 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 higher confidence in temperature relative to NPP in the ESMs. We've also added a citation for Koven et al. 2012 which looks at soil temperature across the CMIP5 models.

We've also modified the discussion of the paper to stress the match at higher spatial scales of the soil carbon as you suggested. We included this text at the beginning of the conclusions: "Overall, we found that some ESMs simulated soil carbon stocks consistent with empirical estimates at the global and biome scales. However, all of the models had difficulty representing soil carbon at the 1° scale."

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

BGD

9, C8030–C8041, 2013

for the suggestion.

- 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.

Response: According to Ji et al 2008, BCC-CSM1.1 does have a nitrogen component in its decomposition model, from Ji et al 2008: "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 than half of the models have a nitrogen component and that sentence in section 2.1 has been adjusted as follows: "Three ESMs include nitrogen interactions with soil carbon: CCSM4, NorESM1, and BCC-CSM1.1."

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 analyses. 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)."

Additionally we compared the HWSD without the high latitude soils covered by the NCSCD. We did not see a significant improvement in model performance (Taylor scores 0.29 to 0.67, correlation between 0.01 and 0.37).

- when you estimate uncertainty in the data it looks like you underestimate it - HWSD is WELL outside your 95

Response: We've added text to address 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 of CI95 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 performed. Such an analysis requires quantification of uncertainty in both measurement and scaling processes used to construct the spatial distribution of soil carbon. Uncertainty in the measurement of soil properties such as bulk density and carbon concentration must be integrated with errors involved in extrapolating data from individual soil profiles to the regional scale. Detailed analysis of the accuracy of soil maps will likely be essential for quantifying the uncertainty in this extrapolation process."

- 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: "Carbon pools were not expected to be exactly at steady state for 1995-2005, and 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 this period. Thus the ESMs were close to steady state, and we assumed steady state to simplify our analysis."

- can you speculate why this reduced complexity model doesn't pick up the models' de-

BGD

9, C8030–C8041, 2013

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

Response: We've added the following to the discussion: "Soil moisture did not play an important role as a driving variable for soil carbon in our reduced complexity models, indicating that for most models this variable did not strongly control spatial patterns in soil carbon stocks (Table 4) or differences among models (Figure 5). This result was unexpected because soil moisture affects decomposition rates in all ESMs (Table 1). Furthermore, other studies have shown that soil carbon stocks depend on the response of heterotrophic respiration to soil moisture in global models, although NPP and soil temperature were also important drivers of soil carbon (Falloon et al., 2011). It is possible that soil moisture influences soil carbon stocks in ESMs, but our reduced complexity model was unable to statistically distinguish the soil moisture effect from the NPP effect because these two drivers often covary.

Alternatively, the exponential form of the moisture function in our reduced complexity model might have been inappropriate if decomposition rates decline at high soil moisture. Based on empirical data, a substantial fraction of global soil carbon likely resides in areas where poor soil drainage impedes organic matter oxidation (Gorham, 1991). It is likely that the interaction of topographic controls and soil texture with soil moisture is not well represented in the current generation of ESMs. New approaches may be needed to determine which grid cells are poorly drained, and the rate at which organic soils form in these area (Ise et al., 2008). We also recommend that future CMIP archives require soil moisture information for different soil layers to facilitate benchmarking studies on the response of carbon to moisture in the soil profile."

- 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 2.2.3 and in the caption of Figure S2. "To evaluate ESM soil carbon across biomes, we aggregated HWSD estimates and model simulations of soil carbon within biomes. The biome map was based on the MODIS land cover product MCD12C1 (Friedl et al., 2010; NASA LP DAAC, 2008) (Figure S2). We assigned one of 16 land cover types to each 1° ? 1° grid cell by taking the most common land cover from the original underlying 0.05° ? 0.05° data. Each 1° ? 1° grid cell was assigned to one of 9 biomes: tundra, boreal forest, tropical rainforest, temperate forest, desert and shrubland, grasslands and savannas, cropland and urban, snow and ice, or permanent wetland. Details for the biome construction can be found in Figure S2."

- ïňĄgure 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 Cs/NPP. We've added a sentence in the caption to clarify this and clarified the discussion section. The caption states: "Turnover times were calculated as Harmonized World Soil Database (HWSD) carbon divided by MODIS NPP for the observations, and simulated global soil carbon divided by simulated global NPP for the ESMs."

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

BGD

9, C8030–C8041, 2013

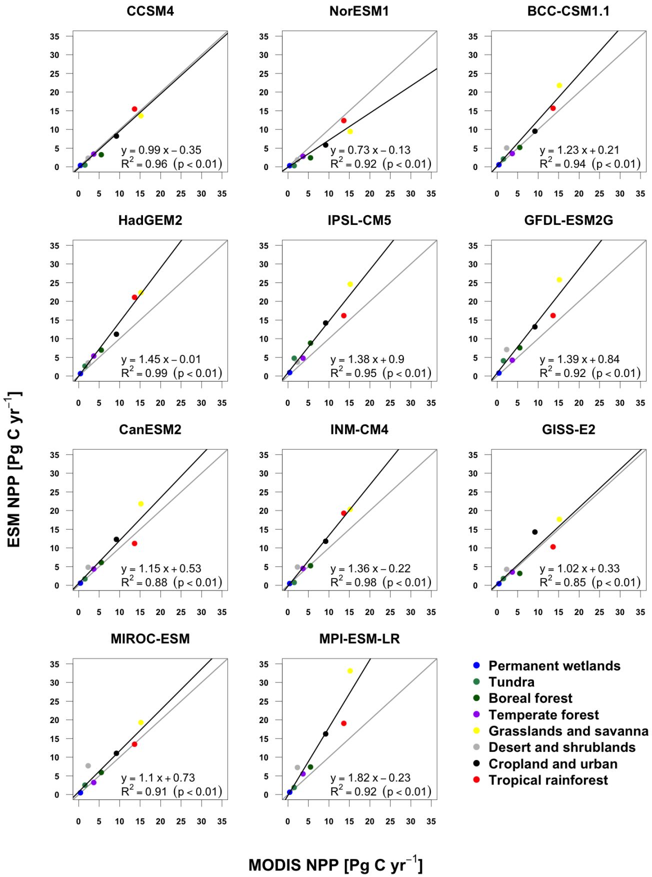

Fig. 1. (Figure S7 in manuscript) Biome comparison between Earth system model (ESM) net primary productivity (NPP) and MODIS NPP.

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

BGD

9, C8030–C8041, 2013

Interactive Comment

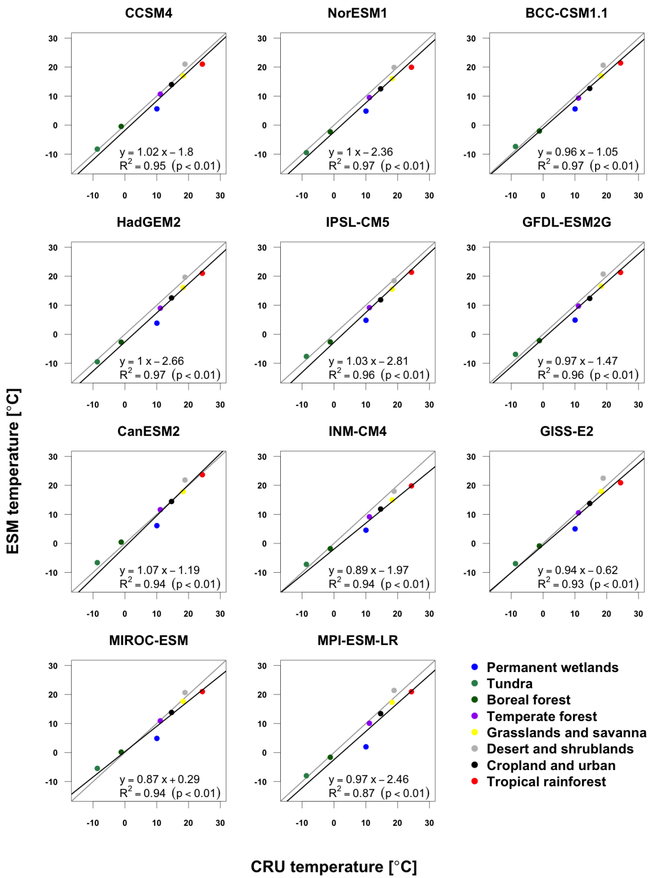

Fig. 2. (Figure S8) Mean surface air temperature (2 m, 1995-2005 mean), versus mean Climate Research Unit (CRU) temperature (2 m, observed 1995-2005) by biome for each Earth system model (ESM).

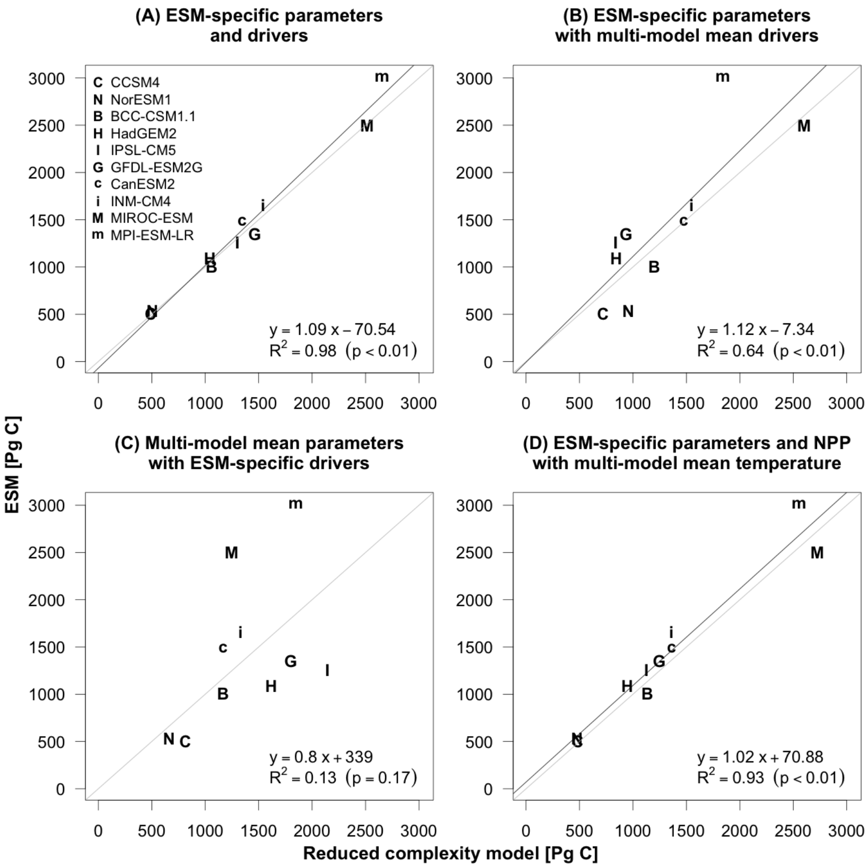

Fig. 3. (Figure 5) Relationship between global soil carbon totals from Earth system models (ESMs) and global soil carbon totals predicted by reduced complexity models.