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Interactive comment on "Field-warmed soil carbon changes imply high 21st century modeled uncertainty" *by* Katherine Todd-Brown et al.

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Review of "Field-warmed soil carbon changes imply high 21st century model uncertainty", by K Todd-Brown et al.

This is an interesting and well written paper addressing a subject area of clear importance and relevance to the readership of Biogeosciences.

The authors clearly articulate the issue of uncertainty in future land-carbon storage, how this responds to climate change and the vital role played of soil organic carbon and its sensitivity to temperature. The results lead to a change in a central estimate, across multi-model results, of soil carbon changes by 2100, although interestingly the data constraint does not reduce the overall uncertainty. This highlights the claim made in

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the title of "high uncertainty" and leave this area as an outstanding issue for modellers and process community alike to address.

I recommend publication after addressing a few minor concerns as below.

Chris Jones

The paper continues the work and analysis of these authors from the Crowther et al. 2016 Nature paper. The dataset assembled of soil warming plots and changes in soil carbon is clearly very valuable for understanding this key issue of Earth System Modelling. I much prefer the approach here, by comparing with process-based models and trying to constrain their simulations, than I do the direct extrapolation to global scale as per Crowther et al. So overall I like the approach deployed here and have mainly minor comments. There are clearly assumptions and choices made which affect the results - I don't believe these should prevent publication - the authors appear to have been careful to consider these and discuss their implications.

2 specific examples are:

- the issue of timescales - the approach to diagnose Q10 relies on pseudo-equilibrium of the system. It could therefore be potentially dangerous and misleading to compare the outcomes from a Q10 diagnostic of 2-5 years warming with ESM studies of 100 years. I was pleased to see this tested explicitly and the SI shows detailed reasons to believe that this is not a large confounding issue.

- the issue of using a single-pool simple model to recreate and re-scale the responses. Again, the authors appear to have taken potential concerns seriously and present good arguments why their approach is OK. I suspect that using a two-pool model would give different answers (inevitable for modelling!) - but it's not clear that this simplification is inappropriate. Given that prominent ESMs in the past, and still some at CMIP5, still deploy a single soil carbon pool it is reasonable for a simple-model approach to do so (by definition simple models bring clarity at the expense of detail - I feel it is well argued

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here that the choice is appropriate).

I have one issue with some of your methodological choices

- you sample q10 values down to 0.1, and in your ESM scaling define "typical" as going down to q10=0.5. It seems VERY odd (unphysical even) to take q10 below 1. Given this is not a linear relationship, you are not looking at taking gradients down towards 0 which represent being flat. Rather q10=1 is where you hit "flat" - i.e. no sensitivity at all to temperature. Once you go below q10=1 then you get an inverse relationship which says that respiration increase as you get COLDER. I'm not aware of any way this could be possible. So I would strongly suggest you take q10=1 as your lower boundary.

specific minor comments:

- please be very careful discussing response to "temperature" - it is always important to specify clearly if this means air- or soil-temperature. It has long been known that the apparent q10 is much lower when calculated as a response to air temp (Raich & Potter (1995, GBC) suggest a Q10 value of 2.0 for soil temperature is roughly equivalent to 1.7 for air temperature). Please check especially the Bond-Lamberty paper/comparison - they calculate a low Q10, as you say - but isn't this for air-temp? The comparison is then perhaps misleading

- be careful using cCwd - is this a distinct carbon pool? or is it a sub-component of cLitter? I believe that the total vegetation system is captured by cVeg+cLitter+cSoil (e.g. see Jones et al., 2013, J.Clim). Then, within this, cLitter is split into tier-2 variables of cCwd, cLitterAbove, cLitterBelow - these are intended to allow reporting in greater detail, but are not new pools. So I think you should remove any cCwd data from your study. I appreciate this is not well explained in the CMIP5 data request - we tried to clarify this for CMIP6 - see figure 5 of Jones et al (C4MIP documentation paper, GMD, 2016)

- Although HadGEM2-ES does base it's soil carbon scheme on RothC it is not identical

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- in particular we chose to keep a uniform q10=2 rather than the RothC temperature function (which we found to be not well behaved at very low T). Otherwise your description of HadGEM2 is correct.

- the discussion correctly discusses the possible role of soil moisture as a rate modifier. Also to consider are vegetation cover, soil quality, soil structure and changes in input quality. Some models, like RothC, change their decomposition according to overlying vegetation. They also change their allocation (your "b" matrix in equation 1) according to vegetation type and lability of litter inputs. I don't think these are major factors, but worth mentioning its not just T and moisture which change the respiration.

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