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Interactive comment on "Examining soil carbon uncertainty in a global model: response of microbial decomposition to temperature, moisture and nutrient limitation" *by* J.-F. Exbrayat et al.

Anonymous Referee #2

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General comments

Exbrayat and co-authors should be commended for tackling this broadly interesting topic with a novel experimental approach. At best this manuscript can provide a thoughtful examination about how model assumptions regarding nutrient limitation and environmental scalars effect the sign and strength of land C dynamics in a global model. At worst the findings could be seen as different model configurations provide different results. In revising this manuscript care should be given to make the paper more of the former, and less of the later.

In my estimation the take home messages from these results are quite tractable. Global

models need to consider nutrient limitation to get global C response to elevated CO2 correct (sensu Hungate et al. Science 2003; Figs 2 & 3; section 4.1). Ultimately, the fate of additional NPP over the historic period and in future scenarios depends on Rh, and assumptions made about the temperature and water sensitivity of organic matter decomposition in soils (Davidson and Janssens Nature 2006, Friedlingstein et al. J. Clim. 2006; Figs 4 & 5). In the end, a C-only model stores too much carbon, especially in soils at high latitudes using temperature functions that slow organic matter decomposition and have longer turnover times (Fig 6). Finally, results over the historical period (and in future projections) depend on the amount of CO2 fertilization (i.e., nutrient limitation) and initial conditions, which are determined by environmental scalars (Fig 13). If this is the general message, it's muddied at present by an over-abundance of results. If it's not the core message, then the central story is obscured by an over-abundance of results.

There don't seem to be any nutrient x climate interactions, where different temperature functions potentially alter productivity by changing nutrient mineralization rates. Is this a fair assessment?

Results and Discussion seem to be very convoluted, with several interesting results and analyses introduced in the discussion (e.g. Figs. 10 & 13). This approach is distracting, and the two sections should be merged, or care taken to separate them.

Throughout the manuscript would be improved by using consistent language for terms used synonymously to aid in clarity. For example uncertainty, range, and standard deviation seem to be used interchangeably throughout the text. Additional examples are described below.

Finally figure captions should be more descriptive so they stand alone, and agree with text in the main body of the manuscript.

Specific comments

It seems like more information could be included / displayed on Fig 2. If the greatest variation in NEA is driven by ft (Figs 4 & 5) may there be some value is displaying models results with different ft parameterizations with different colors lines in Figure 2?

Fig 3: In a complex paper with lots of multi panel display items can this figure be simplified? It seems like the point of this figure is that the C only model generates a large terrestrial C sink with high variation, compared to previous estimates. Is this accurate? I'm not sure much value is added by showing multiple time periods. Since the temporal results warrant little discussion (section 4.1), can just one temporal period be displayed and discussed? Also, here NEA is used synonymously with average land sink, are they the same? If so, can just one term be used to clarify text? Finally, if the authors feel strongly that a 10-panel figure is warranted, why aren't Sitch et al (2008) results shown in each panel?

Post card maps (Figs 4-9 & 11-12) are of limited usefulness, especially when may of them make the same point (i.e., Ft has a strong effect on global C results). Relevant regional results should be highlighted- and interpreted.

Fig 6-9: What time frame we looking at here? Positive values show... what? Are stipples showing significant differences (calculated somehow?) It seems like the color bar alone shows the sign of change. More broadly can Figs 7 and 9 be removed? It seems like Fig 7 is redundant to Fig 6, and Fig 9 repeats patterns shown in Figure 8. Qualitatively, it seems like most of the difference in NEA are driven by differences in soil C- largely at high latitudes, and it's not surprising that these patterns are magnified at high latitudes where K1995 and PnET temperature scalars are much lower than CASA's temperature function.

Figure 10 and discussion on page 10243: How does the data in this graph related to NEA? It seems like there's a lot of information that's synthesized in this figure, but I can't really put my head around what it means. I'm also confused what's really being shown, for example the black line is the standard deviation of the mean (text) or the

mean (caption). What is the signal and noise and how are they calculated. Qualitatively it looks like there's relatively little signal (trend), and a great deal of noise (variation) in all figures, but that there's less signal and more noise in the CNP simulations, correct? As far as I can tell, this is driven by less signal, not more noise from the choice of environmental response functions- but I may be mistaken? Finally, what causes the step function ca. 1960 in the C-only model?

Fig 11 and discussion on pages 10244-10245. In trying to explain the mechanistic rationale for changes in C storage pointing to a bunch of maps is not very useful or quantitative. Could the authors show results from regression or correlation analyses showing how changes in NPP and or environmental scalars (fw and ft) drive changes in soil C pools and / or the strength of the terrestrial C sink?

In general I think conclusions should be revised. The conclusion that ft (or fw) is of greater importance with nutrient limitation isn't clear to me. How was this determined (I think it relates to Fig. 10)? Can the authors provide more interpretation about the mechanisms involved here?

Technical corrections

P 10232 L 14-17. It seems worth noting that only 3 of the CMIP-5 models include N limitation (and two of them use the same land model CLM). While this has changed, few of the CMIP5 models represent nutrient limitation.

P 10233 L 8-9 fw is used synonymously with SMFR (and M in figures), as are ft and STRF (and T in Figures). One abbreviation for the same functions seems adequate, and would aid in understanding. As they are used in eq. 1, Please use fw and ft throughout (or write out temperature function and soil moisture function.

P 10236, L 17: consider replacing "the near future" with "future analyses" since no future projections are presented in the current work

Caption for Fig 2 does not match text in section 3.1, with terms used interchangeably (e.g. NEA and net ecosystem productivity). Are thin black line individual model runs with different combinations of fw and ft? If so, does the shaded area represent range of results of 9 combinations of fw and ft?

P 10237, L 1: consider rewording “there are very major changes”

P 10237, L 21: remove “In terms of the mean terrestrial sink”. To many clauses in this sentence make it difficult to understand.

P 10238, L 27: remove “average land carbon (vegetation + litter + soil), or”; NEA has already been defined.

Fig 4: (and corresponding results) here NEA is convoluted with “land carbon”, which I believe are the same thing? If so, please use a consistent term throughout.

P 10242, L 14-27: While I agree with the sentiment of this discussion, nothing in this analysis makes me think the added complexity of adding P limitation to the model structure is warranted- while CN and CNP results overlap (Fig 2 & 3) if the later was penalized for its added complexity it seems like a “worse” model configuration.

P 10244 L 10-11 where are results showing that NPP is similar between common nutrient simulations (C-only, CN, and CNP)?

Fig 12. Are these 1850 or 2006 results? It seems odd to introduce these results at this stage of the manuscript.

P 10247, L 1-11: while I completely agree this discussion seems outside the scope of the data being presented here.

P 10247, L 21-27: This text should be removed or revised. I would not highlight the unforeseen importance of environmental scalars on calculating equilibrium soil C pools and sensitivity to climate change (see Davidson and Janssens Nature 2006, Xia et al Geosci. Model Dev 2012 and Xia et al. Glob Ch. Bio 2013, and references therin- also Todd-Brown et al. 2013- which should be cited (L 27)).

Interactive comment on Biogeosciences Discuss., 10, 10229, 2013.