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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 #1

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This paper evaluates the sensitivity of the terrestrial carbon sink to differing representations of soil microbial decomposition. Two sources of uncertainty are addressed. First is the effect of including nutrient (N and P) limitation, second is the effect of soil temperature and moisture functions on respiration. Both of these affect the soil carbon pools, and the total land carbon (vegetation+litter+soil) due to differing rates of nutrient availability and their impacts on plant productivity. The different respiration functions are shown to result in large spread in net accumulated carbon over the historical period (1850-2005) within a model. As this study uses respiration functions representative of the current state-of-the-art ecosystem models, it makes an important statement on the range of carbon uptake simulated by these models. Additionally it shows impacts of

including N and P limitation on the land carbon sink, which is an important contribution. I recommend for publication with some minor revisions. Below are general comments and questions from each section, followed by specific comments.

Abstract

- In general, I would limit the use of acronyms in the abstract, and particularly STRF and SMRF (see next comment).

Introduction

- I find it easier to read fT and fW than STRF and SMRF. This comes down to personal preference but I would recommend using fT and fW throughout the paper.

Methods

- Is the vegetation cover prescribed for these simulations? If so, I would expect only very small differences in the climate between the simulations. If not, the simulated climates could be very different due to biophysical feedbacks. Please address this, so the reader can know how much of the differences discussed through the rest of the text could come from different climates.

Results

Comparing these results to previous estimates is worthwhile, but a little more context for these studies and what is comparable to the current study should be included. For example,

- Both the Sitch et al. and Canadell et al. studies relied on models that did not have N or P limitation on NPP. So they are actually most comparable to the C-only simulations of CASA, and it is probably safe to assume that using N/P limitation would have reduced the land sink in Sitch & Canadell's studies. In that case, the fact that they are similar to the CN results means that the CN results might be simulating a too-strong C sink.

- Some context should be given for the statement on LUC at Line 10 (Pg 10238). The

Arora and Boer paper estimated LUC emissions that were about half of those from Houghton. Presumably this study used the Houghton et al. land use data as well, and so the implication is that the LUC emissions are too high in this study. I would think that using lower LUC emissions would increase the land C sink (not lower as stated in the text). However, the overall effect of a different LUC emissions scenario on land C storage would not be entirely straight-forward since carbon sequestration by the vegetation and soils depends on the type of land use/natural vegetation. This statement is confusing.

- Please specify the land use data set used in this study

Discussion

- Can you explain the large shift in NEE/NPP during $\sim$1960? It is a very sudden change so I would assume it's related to a sudden change in one of the forcings (is it climate? Atmospheric CO2? Land use?)

- Given the large spread seen in Figure 12, is it possible to constrain the soil C density? Do observations tell us anything about which of these might be right (or at least which are wrong)? The point in the paper that the soil C is a result of the model equilibrium state is true but can observed soil C maps help at all? A couple sentences on this would be helpful.

- How large is the variation in k (from Eq. 1) between models? Presumably it is not nearly as large as the variations in fT, fW, and Cs but it would be helpful to know the contribution of this term to the CMIP5 model uncertainty.

Specific comments

Page 10230, Line 18-20: This sentence is confusing. To me it makes more sense like this: "Further, although the absolute uncertainty in global C uptake is reduced when N and P limitations are added, the uncertainty due to the temperature and moisture functions grows relative to the interannual variability in net uptake."

Page 10230, Line 20: "soil C depends" not "soil C also depend"

Page 10232, Line 13: Introduce this abbreviation (ESM)

Page 10237; Line 12: Say what the fraction of emitted CO2 taken up by land is from Le Quere et al. (2009), it seems to vary around 30% over the last half century according to the Supplementary Information of that article.

Tables 1 and 2: The abbreviations SMRT/STRF are used in the captions but symbols fT and fW are used in the formulas. Define fT and fW in the caption.

Page 10237; Line 23-25: This sentence is basically repeated in the next paragraph, I think it fits better there as that paragraph addresses the variability in more detail.

Page 10240, Line 10: Remove "very"

Page 10240, Line 15-16: This sentence (beginning "Removing N-limitation ...") is repetitive of the sentence at Line 10. Perhaps remove this sentence and add the 1-2 kg/m2 to the sentence at Line 10.

Figure 3: In the Canadell study, please double check that the period is through 2005 and not 2006. Also I do not see an estimate of the land C sink in Canadell for the 80's. Where does the number in Fig. 3c come from? What is the shading around the Sitch et al. numbers? And finally, specify that the 1958-2002 period from the Sitch et al. data is in panel 3a only.

Figure 8: Why are there some regions with higher NEA in the CNP model?

Page 10243, Line 7: Change the beginning of this sentence to "The lower panels in Figure 3 indicate that ..."

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