

Reply to Anonymous Referee #1 comments 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.

In the following, we provide answers (in blue) to discussion points raised by referees (in black).

Anonymous Referee #1

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 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.

We are thankful to the Referee #1 for their interest in our work and suggestions to improve the manuscript which we have taken into account in our revisions. We provide answers to their comments below.

Abstract

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

We removed unnecessary acronyms in the abstract.

Introduction

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

We agree with both referees that these notations are redundant and we followed their advice by replacing the terms STRF and SMRF by the more readable f_T and f_W throughout the text.

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.

As the reviewer correctly noted, the vegetation cover was prescribed for all runs in order to minimise differences in the climate simulated by each model version. We thank the reviewer for this comment as this piece of information is required for the reader to understand the implications of our results. We therefore added a sentence to the Methods part 2.3 ll. 175-177

Further, we use a fixed land use map for each model version with dominant plant functional types set following the land cover data for 2005 from Hurtt et al. (2006).

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.

The referee is right in saying that results presented in the Canadell et al. (2007) and Sitch et al. (2008) were obtained without representing N/P limitations. However, these are fully coupled models that integrate processes that were not represented in our modelling system: dynamic vegetation in Sitch et al., ocean-atmosphere fluxes in Canadell et al., land use change in both, etc... Due to the good agreement between Canadell et al. and Sitch et al. in terms of 20th century land carbon sink, we are confident that our CN and CNP simulations are more reliable in terms of simulated response to climate change. That is why we extend the discussion of regional implications to CN simulations in particular. We agree that this needs to be better explained in the text and we added a few words in the results part 3.2 ll. 243-250:

We are aware that data by Canadell et al. (2007) and Sitch et al. (2008) are based on model simulations that do not integrate NP limitation. However, they integrate processes that were not represented in our modelling system for the purpose of simplifying the understanding of our results. Further, the good agreement of these previous studies makes us more confident on the reliability of CN and CNP simulations in terms of response to the historical increase in atmospheric CO₂. Therefore, we will use CN simulations as reference in the remainder of the manuscript, as they were the most similar to these independent estimates of global NEA for 1959 - 2005 (Section 3.1).

- 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.

We thank the referee for this comment and agree that the reference to work by Arora and Boer is out of context here and not relevant to this study. We removed the sentence referring to this paper as we do not simulate land use change in our experiments for the purpose of simplifying the study ll. 175-177:

Further, we use a fixed land use map for each model version with dominant plant functional types set following the land cover data for 2005 from Hurtt et al. (2006).

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

See reply to previous comment.

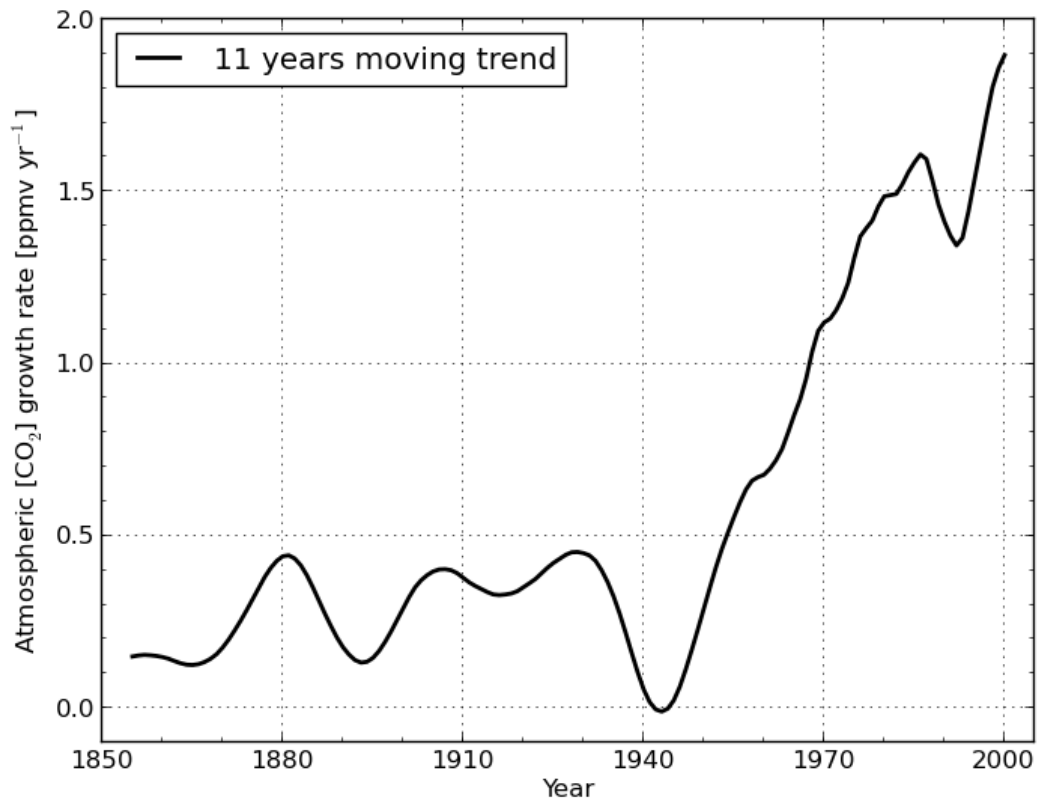
Discussion

- Can you explain the large shift in NEE/NPP during ~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?)

We agree with the referee that this is point that needs clarification. The large step observed in the terrestrial carbon balance around 1960 corresponds to the increase in the anthropogenic emissions shown by the dashed curves on Figure 2 for example. In our model, it is contained in the atmospheric CO₂ forcing. Prior to 1950, the atmospheric CO₂ growth rate was always below 0.5 ppm yr⁻¹. It started to increase exponentially after the 1950s to reach a current level above 1.5 ppm yr⁻¹. We integrated this information in the discussion text ll. 410-414:

As a result, CN and CNP modes (Figures 9b, 9c) do not exhibit the post-1960 step change in NEA that corresponds to a greater carbon sink in the C-only model (Figure 10a) in response to the sudden increase in the growth rate of atmospheric CO₂ concentrations (supplementary Figure S6).

We also added a supplementary figure that shows the growth rate of atmospheric CO₂ since 1850 with an 11 years moving trend. The post-1960 step is obvious on it.



- 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.

We agree with the referee that this is an important point that we previously skipped. A few sentences have been added at the end of the discussion to provide guidelines on how existing datasets could be used to constrain simulated soil carbon, hence the sensitivity of the model response to the initial conditions ll. 500 - 510:

Our results point to a critical need to refine the initialisation of ESMs by spin-up as it controls the sign of change in soil C and NEA, hence the carbon-climate feedback from the land on the atmosphere. We suggest that gridded datasets of current soil carbon content such as those presented by Tarnocai et al. (2009) for high latitudes, or Todd-Brown et al. (2013) globally, could be used as guidelines to constrain soil carbon. Since methods now exist that greatly speed up the spin-up procedure (Xia et al., 2012) – the most expensive part of such simulations – trial and error procedures are now feasible. For example, five of our CN simulations do not fall within the 890 to 1660 Pg C 95% confidence interval of present soil carbon reported by Todd-Brown et al. (2013). Dismissing them leads our projected range of cumulative historical NEA to shrink by about a third from 61 – 175 Pg C to 86 – 161 Pg C.

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

We agree that CMIP5 models are likely to have various pool architectures and values for the reference turnover rate k and that it would be too simplistic to summarise the diversity in these complex modelling systems to variations in two equations. We added a sentence to acknowledge this ll. 364-369:

Of course, CMIP5 models also have a range of pool architectures and are likely to use different values of k as shown by Todd-Brown et al. (2013) with their reduced complexity models. We do not explore this in detail here but we suspect that the similarities between our simulations and CMIP5 results indicate that the formulation of the time and space invariant f_W and f_T is a key source of uncertainty in these models.

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.”

We corrected this sentence accordingly.

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

This was corrected.

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

We now introduce the abbreviation ESM in the first sentence of the manuscript ll. 39-40:

A major step in the transition from Climate System Models to Earth System Models (ESMs) is the inclusion of additional biogeochemical processes.

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.

We rephrased the sentence to add this piece of information and it now reads ll. 212-213

This is more in accordance with Global Carbon Project estimates of uptake that vary around 30% (Le Quéré et al., 2009).

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

Following our reply to earlier comments, we have replaced the SMRF and STRF abbreviations by f_W and f_T throughout the text, respectively.

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.

We thank the reviewer for their comment and deleted the redundant sentence.

Page 10240, Line 10: Remove “very”

This was done.

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.

We rephrased this part of the text that now reads ll. 296-298

NEA in the C-only simulations is generally much higher (by ~1-2 kg C m⁻²) than in the CN-limited simulations

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.

The data presented in Canadell et al. is indeed through to 2006. However, we accessed the annual data on the Global Carbon Project website to calculate mean and standard deviation of NEA for the period 1959-2005 as our modelling experiments stop in 2005. We now better acknowledge this in the text ll. 223-227:

Figure 3 compares our simulation results with estimates using the time period overlapping our simulations from the Global Carbon Project (1959 - 2005; Canadell et al., 2007, data accessible at <http://www.globalcarbonproject.org/carbonrends>) and an intercomparison study of dynamic global vegetation models (1958 - 2002; Sitch et al. 2008).

Following advice from referee #2, we replaced this complicated figure with a simpler one that only presents data for 1959-2005. The shading represents the range of the simulations reported Sitch et al. (2008) and this is now better acknowledged in the figure legend:

Figure 3. Comparison of simulated NEA with previous studies. Boxplots indicate the range in the mean NEA (upper panel) and standard deviation (lower panel)

simulated by all combinations of f_w and f_T in a specific nutrient limitation mode as indicated. Markers represent quartiles. Note data from Canadell et al. (2007) indicate range over the period 1959-2005 and data from Sitch et al. (2008) indicate range over the period 1958-2002.

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

Differences in NEA between the two nutrient-limited modes (now Fig. S2) are well correlated with their differences in soil carbon change (now Fig. 8). We show in this paper that initial carbon pool size controls its change through time under historical forcing, with bigger soil pools likely to release more C than smaller pools. Therefore, as CN simulations equilibrate at a higher level than CNP (Table 3), it is likely that CN model runs will lose more C than CNP runs in regions where the initial pools are large (e.g. southern Africa with the SOILN f_w). Still, this implies a change in the local soil carbon balance only at rare occasions (Fig. 8). This is an interesting feature that we previously failed to address, and we added this analysis in the discussion ll. 484-489:

Further, CN simulations generally equilibrate at a higher soil carbon content than CNP simulations (Table 3). Hence, in regions where large initial soil carbon pools trigger losses during initial simulations (e.g. southern Africa with SOILN f_w in Fig. 4 and Fig. 6), CN runs lose more carbon than the corresponding CNP runs (Fig. 8). Nevertheless, this implies a change in sign of the historical soil carbon balance only on rare occasions as seen in Fig. 8 and Fig. 11.

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

This was done.