Answers to M. Braakhekke

Thank you for the comments and suggestions for our manuscript. The manuscript became much clearer to follow in many of the sections based on your suggestions. Below are the questions and suggestions in italic and blue followed by answers and where the changes are made in the manuscript.

- I find the introduction, though interesting, a bit long and repetitive. On the other hand, the part on the current study is relatively short. Since many readers prefer to skip the methods section it is advisable to give a bit more information about what was done.
We have revised introduction section (shortened and restructured) and methods section (adding detail) in response to comments also made by the other reviewer and hope these changes address also your concerns.

*- p 1053, l 9-11: "Moreover, the conversion of N to plant-available forms is reduced in untilled soils and can thus lead to lower crop productivity, which could in the long run decrease the soil's ability to store water and nutrients because the reduced release is partly counterbalanced by a reduced input of new organic material." I find the second part of this sentence (from "which...") a bit strange and speculative, since (to my understanding) no-till farming is applied partially to *improve* water and nutrient retention. I also could not find this in the included reference (Lal, 2004a).*
We have now changed the sentence to read as follows (page 5, line 21-25):
Moreover, the conversion of N to plant-available
forms is reduced in untilled soils and can thus lead to lower crop productivity.
Although no-till farming is applied partially to improve water and nutrient retention,
the reduced crop productivity and thus reduced input of new organic material could also
decrease the soil's organic content in the long run (Lal 2004b).
The Lal reference was wrong, thanks.

- p 1054, l 11-14: It took me several times before I understood this sentence. Please consider revising it.
We decided to split this sentence into two to make it easier to read (page 7, line 2-5).

- Section 2.1.1: please include a brief explanation of the term "developmental stage".
A sentence describing developmental stage is added to the model description on page 8, line 14-17.

- p 1057, l 3: how was the manure application derived from the mineral N fertilizer? By assuring that the total amount of N is the same?
This has been clarified, and now reads (page 10, line 5-7):
The amount of manure is
derived using the mineral N application rate, but applying the
increase in the metabolic and structural SOM pools, rather than the mineral N pool, with a C:N of 30.
This means that 30 units of C are also added for every N.

- p 1058, l 2: this section suggests that the effect of crop residues on soil evaporation is represented in LPJ-GUESS, while to my understanding it is not. Please make this clear.
We clarified this by adding the following sentence to the end of the paragraph (page 11, line 6-7):
While this affects soil C content, the effect of crop residues on soil evaporation and hence soil water content is not represented in the model.

- Section 2.2: 1) this section is somewhat unclear since descriptions for the different simulation experiments are mixed. Please include a brief but clear overview of the simulations that were performed, possibly identified by labels, which can be referred to later. 2) Please indicate briefly why the CMIP5 simulations started from 1850, while the CRU simulations started from 1901. 3) For the future simulations based on CMIP5 it is not clear if the GCM output was used for the complete simulation or for the future part only (with CRU being used for the historic part)

We made some clarifications regarding the different time periods, throughout the section and added a summary table (Table 1) to make the different experimental set-ups easier to compare.

- p 1059, l 27 – p 1060, l 1: This sentence is not completely clear. Do you mean that a longer transition period for land use would reduce the spin up to such an extent that steady state is not reached for the natural vegetation?
This sentence was a bit vague, we have now changed it to read as follows with an additional sentence to clarify the difference in the transition period length (page 12, line 27 to page 13 line 5):
During spin-up, cropland fraction was linearly increased
from an assumed baseline of zero at 1750 to the first historic
value (1901 for CRU and 1850 for CMIP5). The number of years for
this transition (150 years for the CRU-based and
100 years for the CMIP5 simulations) was chosen to ensure
that the soil C and N pools of the natural vegetation fraction of
each grid cell reached steady-state by the beginning of the transition period.
The different period lengths were chosen to make the simulations comparable
in terms of land-use change prior to 1901.

- p 1060, l 10: The WISE dataset comprises both a collection of soil profiles around the globe and a global gridded product derived from this. It seems that here you refer to the latter, while in p 1060 l 14 you refer to the former. Please clarify this.
Clarified in the manuscript on page 13, line 17-18.

- section 2.2.1: 1) this section is somewhat unclear. I think a few introductory sentences about what was done would be helpful. Further: 2) Where results from a single simulation compared to both the the WISE soil carbon data and the data from Stockmann et al.? 3) Also, I do not understand the classification in to climate zones yields 200 cells per zone (so 800 in total?), out of 60,000 grid cells globally.
- p 1060, l 16: what does the "1000" in parentheses refer to? The number of columns per grid cell?
1000 refers to the number of grid cells with soil cores that were taken from croplands and 200 of these were discarded because they were located outside of the simulated grid (e.g. on a small island) or in the boreal zone. The section is updated (13, line 17-22) addressing also the comments under (1) and (2).

- Section 2.2.2: 1) The wording in this section suggest that some sort of optimization procedure was used to determine the management for optimal soil carbon sequestration for each grid cell. However, from what I understand, this is not the case; instead the results from the management experiments where combined by selecting for each grid cell the optimal management for soil C. Please indicate this clearly. 2) Please explain in this section the labels of the simulations as used in Table 1, and elsewhere in the text
#1 clarified in the text (page 14, line 22), #2 the names are expanded in text (page 14, line 14-15) and also in the caption of the table (now table 2).

- Table 1: what does "scenario" (last line) mean?
Clarified in table caption (now table 2).

- Section 3: Unless I misunderstand, the correlation coefficient diagnostic used to eval-uate model fit to observations does not provide information about model bias (i.e. deviation from the 1:1 line). If this is the case please consider complementing it with another metric such as the (normalized) root mean square error.
True, we complemented with RMSE values on page 15, line 5-10.

- Fig 2. This graph is somewhat unclear. There's quite a few lines and the shading overlaps. I would suggest to replace the lines with bar graphs with errorbars for selected years. Also, I personally think an anomaly graph, i.e. the change in soil C relative to a specific year, is more informative than the rate of sequestration. However, I understand that this would

complicate the comparison with the Stockmann et al. data.
We updated the graph, we kept the lines (mean and 2SD) and removed the shading. A bar graph would not give the temporal dynamics, which are important to visualise the decline in C sequestration over time.

- Section 3.2: The (long term) response of C sequestration to management options is much lower than wat is reported by Stockmann et al. However, this is not mentioned in the text nor could I find a discussion on this in section 4.
We added a discussion on this in section 4.1.1 on page 20, line 3-10.

- p 1064, l 5-10: this is a quite remarkable result since croplands generally have lower soil C. I did not see this clearly discussed in the section 4 though. Further, could this also be related to the fact that the land use conversion in the simulations started only on 1750, thereby not giving the soil C in croplands enough time to decrease?
As mentioned in the Results, some of these areas such as Egypt, with large input of both irrigation and fertilisers, the soil C is higher and also that some of the major pasture areas have higher C densities than the PNV that they replaced, consistent with observations (Guo and Gifford, 2002). But also the the reason you mentioned, that the simulated time under agriculture is too short could be a good reason for this discrepancy.
We updated the discussion on page 19, line 10-14.

- Section 4: only three functional types are used to represent the full global spectrum of crops. I understand this was a necessary simplification but it likely adds considerable uncertainty to the results. However, it is not mentioned in the discussion. I'm sure it's possible to say something about the crops and regions for which this may lead to incorrect results (rice comes to mind).
We added the following sentence to the discussion on yields (page 22, line 24 to page 23, line 3):

> The modelling approach taken here to represent all crops globally with three CFTs, introduces an uncertainty in the estimates of global food production and thus also on the carbon cycle.

We expect that this would be most prominent for crops whose growing seasons, water requirements, or physiology differ substantially from the functional types used here, e.g. regions where rice (South East Asia) or tubers (Africa) are are grown over a large portion of harvested area.

- Section 4.1.1: could the low predicted response of C sequestration to management also be caused by the fact that soil C in croplands is over estimated due to the short period of land use conversion?
In some regions this may be the case, but it would require in-depth testing against for instance long term experiments to confirm and evaluate the response since this would be affected also by the available N in the soil. We have now made mention of this in the manuscript.

- p 1066, l 23-25: please consider revising this sentence
We revised the sentence by removing the word global (page 20, line 10).

- p 1067, l 6-8: this sentence is difficult to follow. Please consider revising.
We revised the sentence to read as follows (page 20, line 21-22):
The authors found that CLM without accounting for tillage practices underestimates the emissions caused by agricultural practices.

- Appendix: please include units of the allocation variables, and explain the DS acronymn
We added explanation in the model description (page 8, line 14-17). As DS is without unit, the parameters are also unitless.

Technical corrections
- In many places citations are completely (authors + year) enclosed in parentheses where only the year should be enclosed. I suspect that the authors used latex and wrote "ncitep{}" where "ncitet{}" was intended.
Thanks for spotting this, we now believe that they are correct.

- p 1061, l 13: I assume you mean "Table 1" rather than "Table 2.1.1"
Yes, corrected.
We have also corrected the following:
- p 1063, l 18: consider replacing "over" with "for" in "competition over available N"
- p 1064, l 12: please insert "in" before "1996-2005", or similar modification Table 4, caption: please remove comma in "Also listed are,"
- Fig 2, caption: do you mean "vertical", instead of "horizontal"?
- Fig 5, caption: consider replacing "on" with "of" in "response on"