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Discussions

Interactive comment on "Management induced changes of soil organic carbon on global croplands" *by* Kristine Karstens et al.

Anonymous Referee #2

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The authors have conducted a study evaluating the influence of management on soil organic carbon in global croplands. This is an important topic for consideration of greenhouse gas mitigation with natural solutions for climate change policy and programs such as the 4 per mille initiative. As the authors mention, there are few studies that have evaluated cropland management effects on soil organic carbon, and possibly none that have addressed the influence at a global scale. The result that increased residue return to soils is the leading driver of carbon changes over the past few decades in croplands is an important finding. As the authors note, the 4 Gt C increase in carbon is less than the goals of the 4 per mille initiative, which some have argued is not realistic. I have a few concerns about the study after review of the IPCC documentation on the method that the authors selected for this analysis. I would suggest that the authors make revisions before the manuscript is accepted.

Specific comments:

1) The Tier 2 method is in a croplands chapter of the IPCC report. The documentation in the report states that the model would need to be parameterized for other land uses. Did the authors parameterize the model for other land uses that would be considered natural vegetation? If not, the estimation of soil organic carbon for natural vegetation may not be valid. The authors seem to suggest that this is a possibility in Section 4.4 when stating the soil organic carbon and debt from land use change have to be interpreted with caution. If the model has not been parameterized for natural vegetation, I would suggest that the authors focus on cropland model results, and remove the carbon debt results. The results for the cropland alone are important, and deserve publication even if the natural vegetation estimates are not valid with this model.

2) Is it possible to estimate uncertainty with this method? IPCC methods often have large uncertainty but does this method have less uncertainty because it is a Tier 2 method. If it is not possible to estimate uncertainty could the authors speculate on the level of uncertainty in the predictions. Knowing something about uncertainty would be helpful in comparisons to the modeled results from other studies that are shown in the manuscript.

3) The authors state that a sensitivity analysis presented in Figure 4 shows that management impact is robust to the initialization of the soil organic carbon stocks at the beginning of the spin-up phase. But, the stocks and change in stocks almost halves the values if the initialization is done with natural vegetation. The initialization does make a difference, and needs further explanation.

4) Good to see that the authors have made a comparison to another approach to confirm the Tier 2 results. The Tier 1 method provided by the IPCC has been used for this purpose. In section 2.2, the authors present a method estimating stock change factors instead of soil organic carbon changes. But, the results in Table 4 for the stock change

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factors are not convincing that the methods are consistent, and the text seems unclear with discussion about larger differences with the IPCC2019 values, which were updated by the authors and should be more accurate – I would think. Why not estimate the change in soil organic carbon for a direct comparison with the Tier 2 method instead of the stock change factors? Also, the placement of these results after the discussion seems odd, and conventionally would be presented in the results section before discussion.

5) Figure 3 shows results from making certain practices constant from 1975 to 2010. The authors state around line 315 that the effect of no-till has been strong since 1990, but the effect seems minor and may not differ statistically from the histManagement with uncertainty. The conclusion about the importance of residue seems most important here.

6) The authors suggest that there needs to be a circular flow with food supply chain back to soils. They assumed that none of the waste from supply chains are returned to soils (near line 300) but this seems incorrect. Municipal waste and materials are amended to soils in many regions of the world although maybe there are no data on these amendments? If this is the issue, the authors could mention that they are making a conservative assumption due to lack of data.

7) The authors evaluate the sensitivity of the Tier 2 model for tree litter with methods in Section 2.4.3. The Tier 2 model divides litter into metabolic and structural components, and the authors have averaged lignin to nitrogen across tree components as input to the model. But forest also include deadwood and should be separated from other forest litter to model decomposition. Did the authors add a deadwood pool? I question if this model is appropriate for forest if deadwood is not modeled separately.

8) Recommend that the authors provide more explanation for Equation 9, which determines the residue amount of C, and is a key driver of the carbon change. Harvest index is the proportion of plant biomass that is harvested, but the authors are multiplying the

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harvested crop product by the harvest index. But the conventional approach is 'harvested crop production divided by the harvest index' to determine the total biomass and then subtract the harvested amount to estimate the residue. The authors are accounting for double harvesting and fallow in this calculation, which I agree is important, but some further explanation is needed about the calculation to understand how residue carbon is estimate from crop production, harvest index and area.

9) For the Tier 1 method, IPCC divides the reference carbon stocks by climate and soil types. Did the authors also divide the grid cells by climate and soil because only climate is mentioned in the text? And, I found a diagram in Figure 5.1 in the IPCC report that divides low, medium and high input categories. Did the authors use this diagram to classify the input? It is not clear if the authors use the diagram or developed their own. If they developed their own, is it consistent with the IPCC factors?

10) What is 'resp' is 'area reduction resp' on line 110? This sentence should be revised to improve readability. I also found other sentences that were difficult to read or missing words in some cases, but did not make a list during my review. Suggest a careful review before final publication.

11) I found the Tier 2 method in Chapter 5 of Volume 4 of the 2019 IPCC report, and would suggest that the authors cite this chapter rather than the entire 2019 IPCC report, which has 5 volumes. This would make it easier for others interested in the study to find the method in the IPCC report.

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