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

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This MS presents the findings of a spatially explicit implementation of a new IPCC Tier II approach to soil carbon stock changes. The authors use this model to calculate how much SOC has been lost due to agriculture but then go on to run the model annually for the period 1975 – 2010 to produce a dynamic picture of SOC recovery over this modern era of farming. The major takeaway message is that while agriculture has incurred a large SOC debt, recent agronomic improvements have led to 4 Pg C of SOC sequestration over this period of time. This detailed picture of SOC in croplands over the past several decades is of incredible importance to policy makers and as such I believe that paper can be an important contribution to the literature; however, I do have several major concerns that may or may not be addressable with revisions.

Methodological concerns.

I had to read the methods section twice and spend an hour with Calvo Buendia et al. 2019 to fully understand what the authors have done. I'm still not 100% confident that I fully understand the methodology. I suggest adding an illustrative example or two graphically demonstrating how the process works. Perhaps starting with a simple case of one lu transition and then showing a more complex case of multiple lu transitions within a pixel.

I have not been convinced that this sort of "dynamic" implementation of a steady state modeling approach is appropriate. I understand that the method was developed by the IPCC as a way to add more nuance into the Tier I emission factor approach but I don't think the method was intended to be applied annually. Why not go all the way to Tier III approach using the process-based dynamics that are embedded in this simplified model? It appears you have all the data assembled to do this. My main concern with applying a steady-state model to annual changes is we know that the recent past trajectory of SOC (particularly in the slow and passive pools) will greatly influence the short-term model response to improved management – i.e. the model will take years to decades before SOC stocks start to rebuild if the trajectory was negative prior to the change – but this will be completely missed with the steady state application (stocks will start increasing immediately upon change).

What time frame are the monthly climate data averaged over to get the rate modifiers for a steady state solution? Given the passive pool has an intrinsic decay rate equivalent to >100 year turnover time, it seems that you need to have a 100+ year average climate to come up with the proper rate modifiers.

Transfer between lu types is not clear. I do not understand how a "respective share of the SOC is reallocated." My concern is that the per area SOC stock for long-term cultivated land will be much different than the per area SOC stock for newly converted cropland, so I don't see how you can suddenly bin these separate areas into one model

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component. Perhaps my request for a visual guide will help me (and other readers) understand that I have misunderstood this part of the methods.

Why was 1901 chosen to spin up to steady state? We know that this was a time of rapid agricultural expansion in several major regions of the world and thus a time of rapid soil carbon loss.

LULUC data – are these data all provided as area within each grid cell (or percent of a grid)? I think so, but please indicate. A supplemental table with the cross-walk between the LUH2 and the 17 crop groups used in this study should be included.

Please include units for eq 9 – I don't follow the AGR calculation – it sounds like you are adding biomass and area together. Additionally, HI is usually defined as CP divided by total aboveground biomass, so CP x HI is a meaningless number.

Lack of validation.

There appears to be no attempt to validate the model or the input data used to drive the model. In general, there is a lack of quantitative evaluation throughout. There are just two qualitative quality assessments – a table comparing calculated stock change factors to Tier I estimates and a discussion on how the map looks similar to other SOC maps.

The model itself was developed recently as part of the 2019 Refinements to IPCC Guidelines and those updated guidelines discuss how the model was calibrated to a set of long-term trial sites but do not report any model performance metrics. As pointed out by the authors some areas of central EU and the UK more than double SOC under current agriculture than under native vegetation. This is certainly indicative that there should be some checks against real data (see detailed elaboration on this point further down in this review). It could be argued that point-based validation for a model run at 0.5 degree resolution is meaningless but it would be an interesting exercise to see how the model reproduces trends with known SOC histories.

Issues with residue C return.

Given that the major takeaway from this paper is that the SOC is being sequestered due to improved yield leading to increased residue return, is there any empirical evidence that C inputs to the mineral soil have nearly doubled (Fig 3)? I think the method for calculating residue return to the soil is potentially flawed leading to this large apparent increase. The authors have assumed that both harvest index (HI) and root-to-shoot (RS) ratios have been constant through time. However, yield improvements over the last century, and in particular the last 50 years, are a result of improvements in genetics and nutrition. Breeding has resulted in the ability to plant most crops at much higher densities and selection towards more photosynthate being allocated to harvestable organs. Both of these improvements have altered HI and RS ratios. Additionally, there are strong interactions between N fertilization rate and root density. There is a huge literature on crop breeding that support the non-stationarity of these important parameters.

Other specific comments.

Units – Gt and Mt are not SI units, please use Pg and Tg

L13 (and elsewhere) – "we also find that SOC is very sensitive. . ." – this is in reference to an unvalidated model result. I'd suggest rewording these sorts of phrases to, "Our model results suggest that SOC is very sensitive. . ."

L279 – "we provide the first world map" – no, you did not. All of the global maps that have been developed using a statistical environmental-covariate modeling approach (i.e. soilgrids and similar) implicitly include all historic land management.

It is great that all the data are provided but I found the Karstens 2020a repository to be confusing. Can you have a description for each file in the repository? The naming convention is not clear. I did not want to download 9 Gb of data to figure it out.

Fig 1 – perhaps it is just the spatial scale of these small maps (and I haven't down-

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loaded the results to explore in more detail) but it looks like there is zero intact forest in the Congo Basin and very little intact forest in the Amazon. Also, I would have liked to see a map showing the trend in SOC spatially – how are the 4 Pg C that has been sequestered been spread across the globe? Is it all in Central EU and UK?

Fig 2 – this is a really interesting way of summarizing the model results.

L313-314 – This sentence ("global SOC would still increase") is confusing as the graph shows a loss in the constResidue scenario.

Fig 4 – the finding presented here is very counterintuitive to me. Why is the SOC debt halved when the model is initialized with natural vegetation? Shouldn't the 1975 SOC debt be much greater if the 1901 starting point was natural vegetation instead of actual land use? Perhaps I am just misunderstanding this sensitivity analysis.

Discussion section – in general, there is very little discussion of how these results fit into the large literature on SOC. There are many places were a reference or two would great increase the credibility of the statements that are being made.

Section 4.2 – I think this section should come right before the conclusions especially as you refer to analysis that is only presented for the first time in section 4.3

L358-360 – the finding that northern temperate zones (particularly in EU and UK) now have SOC levels up to twice that of native state yet tropical soils have lost 40-70% of their SOC is problematic and, as the authors point out in relation to the EU example, likely points to issues with getting C input to soil correct. The EU has the perfect data set to test this model finding – the EU JRC LUCAS survey was a balanced sampling design between forested and agricultural land uses. In the tropics, it has been fairly well documented that already infertile tropical soils do not lose nearly as much SOC as their fertile temperate zone soil counterparts. While there are issues and large scale generalizations in the IPCC Tier I default factors, they do represent the consensus literature on the topic. The updated meta-analysis between the 2006 and 2019 IPCC

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guidelines when this emission factor for the tropics changed dramatically (see Table 4 in this MS) points to this new knowledge.

L396 – how is this validation? It is just a comparison.

L400-406 – there is a large literature that can be drawn upon to support some of the claims made in this section.

Section 4.4 – I do not think this is a valid comparison because SoilGrids explicitly tried to capture high carbon density soils well while your model explicitly excludes organic soils. I suggest applying an agriculture mask to all of these datasets and then redo the analysis. Additionally, ISRIC released an update to SoilGrids >6 months ago that focuses primarily on mineral soil carbon stocks. This update is probably a better comparison.

L425-428 – this seems out of place.

L453 – comparison to 4p1000 is not really fair because your model is really just the business-as-usual scenario with SOC gains simply because yields are improving globally. 4p1000 is about intentional management shifts to increase SOC.

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