Earth System
Dynamics
Discussions

Interactive comment on "Historical and future carbon emissions from croplands" *by* S. J. Smith

Anonymous Referee #1

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This paper addresses the question of historical and future cropland C emissions from management. To assess this question the paper uses a simple model of global cropland area and cropland C dynamics. An important component of this model is to include a representation of non-harvested cropland, relying on a newly developed dataset of 'other arable land' that is not harvested. The analysis includes a comparison of residue removal and no-till practices on modelled cropland C emissions and concludes with suggestions about the most efficient combination of these practices for climate mitigation.

While the paper examines a very relevant and interesting question and proposes a potentially useful framework for analysing this question by using a simple empirical model of global cropland driven by historical data about cropland area as well as crop yields, the lack of detail about the model and methods used leads to unsupported conclusions that are not legitimized by the analysis presented. Given this limited description of the

methods involved, the results of the analysis are thus unfortunately not very meaningful and do not provide any considerable new addition to the scientific literature.

General comments:

A model is only useful to us if we know what the underlying assumptions and equations are and how processes are represented in the model. If the results of a model analysis shows for example that conservation tillage has a substantial influence on cropland C emissions, then we need to know how conservation tillage is implemented in this model, as the result could also simply arise from an over- (or mis-) representation of tillage effects on C flows in the model. I am not doubting here that no-till methods actually could have a substantial influence on cropland C emissions, but the current description of the analysis conducted does not allow for the type of conclusions drawn in this paper and limit the usefulness of the analysis. I would suggest to (1) describe the methods and the model used in much more detail, and (2) discuss the model results in view of the model structure, i.e. – "Given the key XY assumptions underlying the model, the results suggest YZ".

Given the simplicity of the model, the simplified incorporation of processes like no-till agriculture (i.e. by simply increasing the turnover time for the slow C pool by 50% in no-till land), and the lack of validation with real world data, as well as the fact that the model does not appear to be based on many established model representations of processes (as there are almost no references cited in the methods section), it is especially important to describe model components and assumptions as clearly as possible.

Some important questions that I do not find answered in the article include: - What is the spatial and temporal resolution of the analysis? - As I understand, the C emissions from land use conversion are modelled but not discussed in this paper. Why not? - What type of input data is used to describe soil C content before cropland conversion? - Is CO_2 fertilization accounted for in the model? - How are different crops

represented in the model? Different crops have widely differing NPPs, yields (as well as yield trends), residue management, tillage practices etc., and consequently widely differing C dynamics. It appears problematic if different crops like alfalfa, wheat, vegetables, strawberries, rapeseed, groundnut, or maize are treated the same way.

Some suggestions about how the description and discussion of the model could be improved:

1. It would be useful to conduct a more systematic sensitivity analysis that examines which one of all the parameters included in the model influences modeled C emissions the most. The sensitivity analysis conducted by the author is very limited, as it only varies a few parameters and often only varies these selected parameters in one or two ways. Instead it would be useful to conduct a full-range sensitivity analysis of all parameters, allowing each parameters to vary in many different steps.

2. Include a table of all input data sources used.

3. Include a table describing all parameters (and the references for choosing these parameters) used in the model.

4. Include a comparison with other model results about historical and future cropland C emissions (e.g. in Fig. 1 or 2) as well as (if possible) validation of model results with real world data.

I would suggest to also limit the paper to a historical analysis, as the model used for future scenarios is not described at all, and the addition of this scenario analysis does thus not add anything to the paper. It is impossible for the reader to interpret the future scenario results given the lack of knowledge about the models and scenarios used.

Another problematic part of the paper is, in my view, the estimation of non-harvested land used by the author. Due to widely varying definitions of e.g. cropland, arable land, harvested area, temporary and permanent crops in different data sources and different agricultural statistics, I am not convinced that what the author quantifies as 'other arable

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land' (i.e. the difference between HYDE cropland estimates and FAO harvested areas) actually represents fallow land. I rather believe that this difference often arises simply from differences in definitions.

The incorporation and accounting of non-harvested cropland represents one of the potentially most interesting new contributions of this paper. I am, however, very doubtful as to the accuracy of the current analysis. A better estimation of this 'fallow land' over historical time periods would for example require a more detailed analysis of reasons for the differences between cropland area and total harvested area in different countries, as well as a more detailed comparisons of definitions used in different sources.

Specific comments:

In p.3, line 6 the author states that emissions from 'land use conversion' are not considered. But then later on he writes that the model used can account for C flows under 'land use change' (p. 3, line 11 & line 21). Please be more clear from the beginning what C flows and C emissions are accounted for and which ones are not. It would be helpful to include a brief discussion in the intro explaining the different sources of C emissions from croplands (i.e. from conversion vs from management, as well as what type of different management C emissions exist) and followed by an explanation about which ones of these are part of the model and thus part of the present analysis and paper. A definition of key terms used (e.g. land use conversion vs land use change vs land management) would also be useful.

Section 2.1: What are these 12 different ecosystems/land use types? Is there more than 1 cropland class? Does the model include pasture or permanent crops? Does the model distinguish between different crop types (e.g. soybean vs rice vs tomato vs wheat)?

p. 4, representation of soil C pools in model:

It is OK to refer to other papers for details about the model used. But whatever part

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of the model is relevant for the present analysis and thus described here, should be explained clearly. Why does the author choose a turnover of 1.5 years for the fast C pool? Cite a reference or explain. How exactly are these values scaled for other regions? Does the author scale the turnover time for the fast C pool according to regional grassland mean residence times as well scale the slow + passive soil C contents following Thomson et al. (2008)? What exactly is the mean residence time? Is it the mean residence time of all C pools together? Or did the author use parameters for the mean residence time for each individual C pool? Please explain this section better.

All variables in the equations 1-4 should be explained. As is, I cant understand these equations as I don't know what the variables represent.

Please discuss what these different SOC pools represent in the real world.

How are C inputs to croplands through animal manure accounted for in the model?

Section 2.2:

Please describe the key components of how NPPeff is derived from historical census yield data in the text. It is OK to refer to other papers or the supplement for details but the most important components and assumptions of the model should be summarized here. Does NPPeff increase proportionally with yield increases? I.e. does the model assume that the fraction of crop productivity that is left behind as crop residue remains the same with increasing productivity? Where does data on the harvest index come from? If possible, include the key equations used to estimate NPPeff from historical census yield data.

It could be useful to include a table describing the different datasets used in different parts of the model, as well as the references to these datasets. Knowing where the data used comes from is especially important for the reader, as the compilation of the dataset on 'other arable' land is one of the key new contributions of the present paper.

p. 5, line 22: GCAM RCP4.5 scenario using what model? Much more detail needed

here.

p. 5, lines 25ff: should this better be included in the results section? Table 1 and Table 2 are part of the results and the tables as well as their discussion should probably be moved to the results section.

p. 6, line 15: cite a reference for the HYDE dataset

Do I understand this correctly and does the author estimate 'other arable' land as follows:

Harvested area = FAO Other arable = cropland (HYDE) – harvested area

What exactly is the FAO data the author is using to estimate actual harvested area? Is it the sum of all crop harvested areas? Or is it the 'total harvested area' category presented in FAOSTAT? Please include a table showing the sources of different cropland and harvested area datasets.

The author uses the term 'other arable' to describe non-harvested cropland area. I would suggest to more consistently be using the term 'fallow land' instead, as this term is a widely used term to describe non-harvested cropland area and this is (what I believe) the author is trying to quantify.

Note that harvested area can also be larger than cropland area in the case of multicropping (which would especially be expected for tropical regions). The maximum value in Table 2 is, however, a ratio of 1. Did the author cap values higher than 1 at 1?

Also note that the HYDE cropland data is also based (at least partly) on FAO statistics. In addition, the type of data included by datasets like HYDE in the cropland estimates is often a combination of different types of agricultural area definitions (e.g. arable area, harvested area, temporary crop harvested area or similar).

The estimates of fallow land in Table 2 appear very high. This (in addition to the knowledge about how widely definitions of cropland used in different datasets vary) supports

my doubt about whether the approach used here actually allows quantifying fallow land (see earlier comment).

p. 7, line 22-23: please be specific, i.e. 'net carbon emissions'

p. 8, line 22: if the assessment of conservation tillage is a key topic of the paper then the way this process is implemented in the model needs to be explained.

p. 9, line 17: Please explain more why the model assumes cropland soil C approaches the same equilibrium C content regardless of the land-use conversion process. And also discuss whether this assumption makes sense from a real-world perspective. Does this imply that a peatland converted to cropland will have the same soil C content at equilibrium as a dryland converted to cropland? Is this a realistic assumption? Or does this assumption imply that a natural forest converted gradually over managed forest, shrubland and grassland to cropland will have the same equilibrium C content (and thus the same total C emission) as a forest converted directly to cropland through e.g. slash and burn? Please clarify.

p. 9, line 25f: 'cropland NPP is much less than that of most natural ecosystems', please provide a reference. 'croplands could be a net source of emissions to the atmosphere for long after the transition' – also, please provide references for this statement.

p. 10, line 5ff: move this section (& discussion of Fig. 1) into results.

p. 11, line 25: Increased NPP in 21st century: Why do we see this marked increase in cropland NPP and thus a transition of croplands from a C source to a C sink? Yield increases due to the green revolution already started manifesting themselves in the 1960s and I would therefore rather expect this transition to happen earlier. And recently we have actually seen reductions in yield growth or even yield stagnation in many regions of the world.

Caption of Fig. 2 is wrong, as the graph does not represent C emissions by region.

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Interactive comment on Earth Syst. Dynam. Discuss., 5, 1, 2014.