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Biogeosciences Discussions

Interactive comment on "Sensitivity of Holocene atmospheric CO_2 and the modern carbon budget to early human land use: analyses with a process-based model" *by* B. Stocker et al.

B. Stocker et al.

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Dear Editor and reviewers

We would like to thank you for providing us the time to carefully address all comments. This process took longer than usual as the first author was away from office for six months until August 1.

We thank all three reviewers for their comments and advise that helped to improve the presentation of our results. We now describe and discuss the results of an "overshoot scenario" as requested by the reviewers. Results of this additional scenario are

included in figures and tables. The reviewers asked for the evaluation of simulated carbon inventories. In the new figures 5 and 6 modeled carbon inventories in soils and vegetation are compared with observation-based estimates from Batjes, 2008 and Luyssart et al., 2007. These comparisons are discussed in the main text (section 4.1). The reviewers asked for an extended discussion on several issues such as the impact of shifting cultivation on land use emissions. We have slightly expanded the discussion section to cover the requests and added subsection headings to the discussion section to improve readability.

In addition to the changes requested by the reviewers, harvest on croplands is now explicitly taken into account in the standard model setup. All model runs have been repeated with the harvesting routing active. Figures and tables have been revised accordingly. The impacts of harvesting on terrestrial carbon storage and atmospheric CO_2 is assessed by simulations with and without harvesting on croplands (Table 2).

Finally, an (unidentified) error in the labelling of the y-axis in Figure 1, top (global land use area over time) is corrected (range is 0-50$\cdot 10^6$ km^2 instead of 0-5$\cdot 10^6$ km^2).

Conclusions remain unchanged from the submitted manuscript.

Yours sincerely
B. Stocker, K. Strassmann, F. Joos

General comments

1. *"1) The paper appears to be a very valuable study that very likely will generate considerable interest among the readers of biogeosciences. The authors take up the criticism that has been voiced at past attempts at quantifying anthropogenic land use and its effects on the global carbon cycle and analyse the consequences of anthropogenic land use scenarios that do not rely on fixed land use per person. I recommend*

accepting the paper with minor revisions. In fact most of my criticism refers to minor textual changes, the substance of the study seems very solid."
Thank you.

2. *"2) The authors investigate the influence of four land-use scenarios for the time 10000BP to present day on the atmospheric CO_2 concentration. These scenarios consist of a "standard" scenario with fixed LAP (land area per person), and in addition to this kind of scenario three others are used that contain LAP that varies in time. Since the use of fixed LAP has been criticized strongly, the present study provides a very welcome reply to this criticism. This is the first publication that actually uses land use scenarios with variable LAP to estimate the atmospheric CO_2 concentration, as opposed to just determining the additional area converted. The scenarios are based on the HYDE database and necessarily stylized, ranging from a doubling of agricultural area before 1700 AD in the H2 scenario to a linear interpolation of agricultural area between 10000 years BP and present day in the extreme X2 scenario. While such an extreme scenario appears rather implausible, it certainly serves to illustrate the effects of these extreme assumptions. Unfortunately the authors refrain from an in depth assessment of the plausibility of their scenarios. While it is perfectly clear that neither X1 or X2 can be regarded as plausible, the H2 scenario is described as plausible by the authors, but it seems doubtful whether a simple doubling of land area used would be plausible in all regions. I would therefore recommend that the authors spend a little time on evaluating the plausibility of the H2 scenario, not just for Europe, where it seems to agree with the Kaplan 2009 study, but also for other world regions."*
See reply no. 9 to reviewer 1.
We describe the scenarios as idealized (section 4.2): "We address the uncertainty of Holocene ALCC reconstructions using four additional idealized scenarios with larger preindustrial ALCC than in HY."
We also point to the lack of a global data set that prevents a sound assessment and the limitations inherent in using global scaling factors in section 4.2: "Applying global

scaling factors neglects the spatially differentiated evolution of agricultural practices. However, no global dataset exists that differentiates such practices throughout the Holocene. This leads to an according uncertainty in regional carbon fluxes from ALCC, as they depend strongly on the location of land conversion and the simulated carbon stocks in the respective grid cell."

3. *"3) In addition, one type of scenario that might be quite interesting has not been investigated by the authors. Ruddiman and Ellis also mention a "convex" scenario, leading to agricultural area that would shrink during the last few centuries of the time frame considered. Such an overshoot scenario might also lead to interesting results, though I certainly won't fault the authors if it should prove impossible to run additional scenarios."*
We added an "overshoot" or "convex" scenario (OS) that has been tailored to the proposed scenario by Ruddimann and Ellis. The OS scenario is described in section 2.1: "The overshoot scenario (OS) represents the same spatial distribution of land use areas as in HY, but area fractions before 1700 AD are scaled with a time-varying factor that relates population density and the area fraction of agricultural areas in a gridcell, as defined by Eq. 1 in Kaplan et al. (2009) (see Stocker (2009)). The scaling factor is 1 for all years after 1700 AD. This relation is suggested to represent the effect of technological change on land productivity and hence on the mean LAP. As a result, global total ALCC areas decline sharply after Medieval times because the relative population growth is smaller than the relative decline in LAP." Results of the overshoot scenario are included in figures and tables. Our conclusions remain unchanged and changes in atmospheric CO_2 remain very small also for the overshoot scenario.

4. *"4) In the abstract it is mentioned that CO_2 changes due to land use change only exceed natural interannual variability after 1000 AD. This may well be true, but this statement only appears in the abstract and is not substantiated in the main text. The latter is missing any estimate of the natural variability, either as shown by the BernCC*

model, or as shown by other models. While the point is an important one to make, some substantiation within the main text would appear warranted."
See reply no.5 to reviewer 1.

5. *"5) The model description is short, but sufficient for the reader to get an understanding of the setup used in order to investigate the questions. Unfortunately there are two points that should be addressed in a revised version of the paper: Some methodological issues in land use change unfortunately aren't covered by the text.It is unclear whether cropland and / or pastures are introduced by reducing all natural vegetation, or whether the grassland fraction is used first before any trees are removed. In addition, the model description doesn't mention the fate of soil carbon explicitly. Both of these points can of course be resolved if the reader looks into the Strassmann (2008) paper, but they are rather important for the current paper, since the emitted amount of CO_2 would be quite sensitive to such details. Therefore they should be mentioned."*
For the issue whether grasslands are preferentially converted, see reply no.12 to reviewer 1. Regarding the fate of soil carbon after land conversion, see added text for the description of how harvest effects are simulated (Section 2.2):
"Here, annual harvest on cropland is implemented following the approach of Olofsson and Hickler (2008). These authors reduce the fraction of the litter decomposition flux that is transferred to the soils from 30% to 20% . Here, we further reduce this fraction to 17% and thus increase the impact of harvest on soils and land use emissions to better reproduce the observed impact of cultivation on cropland soils (**?**)."

6. *"Finally, the text mentions that LPJ is driven with constant boundary conditions and 1950 orbital forcing, but it takes the reader a while to realise that this also means that the climate that drives LPJ also isn't changing. An additional sentence making this clear would help the reader who just skims the text without checking all the details."*
Added text in section 2.1:
"Here, for simplicity of interpretation, we keep all boundary conditions other than ALCC

constant. In other words, climate is kept constant throughout the simulations."

7. *"In addition, the authors mention that they updated some of the PFT specific parameters in LPJ [...] some evaluation of the effects of the new parameterisation would seem warranted, as well as some evaluation of the effects of the new climate data."*
We added a comparison of modeled versus observation based carbon stock estimates (Section 4.1 and Figures 5 and 6). Concerning the updated climatology: The respective effects are discussed in Stocker, 2009 (Master's Thesis, p. 54-59, download here: http://www.climate.unibe.ch/?L1=people&L2=personal&L3=beni&PUB=yes).

8. *"The authors use rather strong language when it comes to Ruddiman's various hypotheses. While I agree that the present study adds a further piece of evidence that the Holocene didn't quite develop the way Bill Ruddiman envisioned, it isn't the only or the first paper addressing these issues. The authors certainly show that larger anthropogenic land use than considered previously does not increase atmospheric CO_2 by the amounts proposed by Ruddiman, but whether humankind has prevented glaciation has been addressed before. See for example Calov et al., CP, 2009 for a discussion of conditions required for glaciation. Therefore the strong language used by the authors implies that the paper is more than it actually is, and my suggestion is to dampen it down a little."*
This critique was considered when rewriting the article for final submission. In particular, the last sentence in the abstract ("We falsify..") was replaced by "Our results show that even extreme assumptions for preindustrial land conversion and high per-capita land use do not result in CO_2 emissions that would be sufficient to explain the late Holocene CO_2 increase."

9. *"Unfortunately the text contains a number of "Germanisms", i.e., instances where the German way of composing a sentence was used instead of the English way. While this does not detract from the scientific merit of the paper, I would recommend involving*

a native speaker when composing the final draft. One example: Page 933, line 10: "The residual sink flux remains also negligible..." would usually be written as "... also remains negligible..."".
A native speaker has revised the text.

10. *"Finally, it is a great pity that the authors did not try to better quantify the impact due to slash and burn agriculture and wood harvests."*
See replies no.2 and no.8 to reviewer 1.

11. *"One more thing on Figure 2: I'd suggest adding the present day distribution as opposed to just describing it."*
Done.

Interactive comment on Biogeosciences Discuss., 7, 921, 2010.