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Comment

***Interactive comment on* “Sensitivity of Holocene atmospheric CO₂ and the modern carbon budget to early human land use: analyses with a process-based model” by B. Stocker et al.**

Anonymous Referee #2

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Review of “Sensitivity of Holocene atmospheric CO₂ and the modern carbon budget to early human land use: analyses with a process-based model” by B. Stocker, K. Strassmann, and F. Joos.

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,

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the substance of the study seems very solid.

The authors investigate the influence of four land-use scenarios for the time 10000BP to present day on the atmospheric CO₂ 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 CO₂ 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. 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.

In the abstract it is mentioned that CO₂ 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

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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.

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 CO₂ would be quite sensitive to such details. Therefore they should be mentioned.

In addition, the authors mention that they updated some of the PFT specific parameters in LPJ, and then point out that the new PFT parameterisation in combination with a different baseline climatology lead to a reduction in LUC related C emissions by about 30%. Unfortunately, as far as I can gather from her thesis, Wania herself limits the evaluation of her PFT parameterisation to the two new PFTs she has introduced. The consequences of her other changes to PFT parameters have, to my knowledge, never been published. Since a reduction in emissions by 30% is quite substantial, 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. 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.

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

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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 CO₂ 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.

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..."

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. While this would have expanded the scope of the present study considerably, possibly making it impossible to handle in the time available, it would also have added to the value of the present study. Right now the authors seem to assume that these anthropogenic activities are also (implicitly) covered by their scenarios. This would appear to be unwarranted, since the carbon emissions are quite different from the ones caused by permanent agriculture.

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

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