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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.

Anonymous Referee #1

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General comments: Overall, I believe this is a valuable study that is of interest to many of the readers of BG. I see the main scientific progress in testing new scenarios similar to those suggested by Ruddiman&Ellis, 2009 with a carbon cycle model. The conclusion that the effects on CO2 are too small to explain a substantial part of the Holocene CO2 increase will not be surprising to climate/carbon cycle experts based on previous publications, but is important due to its quantification. I see need for revisions though in several aspects (see specific comments for details): (1) The method is straightforward and mostly clearly described, but it has strong limitations that are not sufficiently discussed. In particular no land use apart from permanent agriculture is considered, while it is well possible that shifting cultivation, the use of fire, and wood harvest had a

much larger carbon cycle effect in the early millennia than agriculture. The manuscript needs to make clear that it does not deliver the final conclusion to the Holocene CO2 discussion, but just tests one aspect. (2) The choice of scenarios needs more historical basis. Simple scaling with global factors does not justify calling scenarios plausible or upper bound estimates. If possible, I'd suggest including an additional overshoot scenario, since this has also been suggested by Ruddiman&Ellis and has very different consequences on CO2 evolution. (3) The conclusion is not as novel as suggested, but confirms (admittedly in part less comprehensive) previous studies; this should be discussed more, also to highlight the new and improved aspects of this study.

Specific comments:

Abstract:

- I can't find the comparison to interannual variability in the text and I believe the setup does not allow a statistical analysis of it: A 31-years climatology is applied, but the discrepancy of this annual/decadal variability to the centennial/millennial timescale of the study is striking.

- "Our results falsify…": Previous studies came to the same conclusion – please add that you refer to the newly suggested type of scenarios with much higher per-capita land use in early times.

Introduction:

- P. 924: The notion that Pongratz et al., 2008, rely only on constant per-capita land use values is wrong. Additional datasets that are part of this publication explicitly correct for changes in per-capita land use based on historical literature. p. 925: "Earlier studies have not addressed…" – Pongratz et al. did quantify carbon fluxes under constant vs. changing land use per person. p. 927, l. 3: "higher preindustrial land use than Pongratz" – I don't expect this to be true for all regions for all the Pongratz et al. scenarios.

- p. 925: I'd add that it is due to the uncertainty and unavailability of data (not due to ignorance) that shifting cultivation etc. has not been included by most studies that cover more than the last few centuries. l. 13: Misleading to mention shifting cultivation here, since the present study does not address it either.

Methods:

- (and Introduction): Three critical comments concerning the choice of scenario and land area per person (LAP):

(1) Scaling with one global value neglects the regional differences – a factor of 2 may be "plausible" on global average, but not regionally: e.g. China halved its LAP between the 15th and 18th century alone (Perkins in Grigg, 1974), mainly due to the expansion of wet rice. Similarly, the introduction of New-World cultivars to the Old World as well as technological improvements have likely altered the LAP in certain regions much more than by a factor of 2. I grant that such historical data might not be easily available on the spatial and temporal scale of the study, but I would suggest the authors include some more historical information (as provided e.g. in Grigg, 1974, Vasey, 1992) to determine or justify the applied factor. Since carbon fluxes depend substantially on the location of deforestation, I would not call this scenario a "plausible" one.

(2) Please be specific if you refer to land used per person in an agricultural system or land used per person (globally): Only the first has likely decreased with time due to agrotechnological development, the trend of the second is unclear since early global population has performed agriculture only to a very small fraction, so that smaller land used per person in an agricultural system with time is offset by an increasing fraction of people performing agriculture. The shape of the scenarios strongly depends on the fraction of agrarian population and should be discussed accordingly.

(3) No "overshoot" scenario of global agricultural areas is investigated, although this had been supported as plausible by references in Ruddiman&Ellis, 2009. Due to the slower (if at all) recovery of soil carbon this scenario may yield a larger CO_2 increase

than the others. I refrain from judging a paper based on what it is NOT, but note that including such a scenario would add a very strong point by investigating all suggested scenarios.

- How is agricultural area allocated within each grid cell (is it on forest or other natural vegetation)?

- p. 927, l. 7: Area of shifting cultivation would come on top of the H2 scenario, and can't be seen as part of it.

- p. 927, l. 17: If in X1 areas completely unaffected in HY are now agricultural, how do you then "scale"?

- p. 929, l. 4: Add that "net change" refers to within one pixel.

- p. 929, new parameters: Since carbon fluxes change by 30% compared to previous publication, a more thorough validation is needed here.

- p. 930, l. 14 f: comparison of "net flux" with (primary) "land use emissions" is confusing.

Results:

- p. 931, l. 17: I don't see how Fig. 3 shows the size of the product pools.

- p. 933, l. 8ff, "The small land use emissions…": This sentence seems out of context in a paragraph about the residual sink. Further clarify that you refer to observed, not modeled, CO_2.

Conclusion:

- p. 935, l. 11 f: Shifiting cultivation, wood harvest, or burning should all not be "implicitly" subsumed under the high scenarios of the present study: These processes are of inherently different quality than agricultural expansion, with very different consequences on the carbon fluxes, and these are not addressed by this study.

- p. 936, l. 21: Clarify that cumulative emissions are primary, not net.

- reference to ice age: I don't think the Ruddiman&Ellis, 2009 paper deals with glacial onset.

More Discussion is needed:

- on the disregard of shifting cultivation, wood harvest, anthropogenic fires

- on the disregard of dynamic natural vegetation (I agree that CO_2 changes due to changes in natural vegetation should not be part of the paper, but the CO_2 fluxes caused by agriculture depend on the natural background vegetation that is cleared, and this changes substantially on the millennial timescales of the present study.)

- on how including realistic CO_2 and interactive climate would alter the conclusions drawn concerning the present-day missing sink.

- p. 937, l. 21 ff: compare your 4 ppm to other studies.

- p. 938: That the same cumulative emissions cause a smaller change in late-preindustrial CO_2 if placed early in time is an important point not discussed in the referenced publications. I suggest more emphasis on the respective model quantification.

- p. 938, l. 7: I am surprised that CO_2 fertilization does not alter the results very much. This is in disagreement with previous studies (e.g. Pongratz et al., 2009) – please explain why.

- l. 8: "Holocene changes are primarily driven by natural processes. . ." – this seems too strong a statement here, considering the limitations of the referenced studies (and all studies so far), including the present one that does not investigate shifting cultivation, fire, co-changes in natural vegetation,. . .

- l. 11: Please indicate which equation is used here to relate CO_2 to radiative forcing.

- l. 20 f: Matthews et al, 2004 find a warming in contrast to Brovkin et al, 2004. The sign of the historical land cover forcing is far from being clear.

Tab. 1: Is the number 187 GtC emissions AD 1850-2004 correct (seems high as compared to the 188 GtC in Strassmann et al, 2008 AD 1700-1999)?

Fig. 3: The distribution of emissions to ocean, biosphere, atmosphere differs substantially from Pongratz et al, 2009. Can you explain this by the longer timescale of your study or by model differences?

Technical corrections:

- Introduction & Methods: reference to the HYDE3.1 land cover dataset is incorrect (the referenced publications cover only 1700 to present. Only the population, not land cover data until 10,000 is published so far.) Cite website instead.

- Olofsson-reference: correct author list (it is J. Olofsson and T. Hickler)

- Tab. 2: LAP by Ramankutty et al. – the range is across regions or uncertainty?

- Fig. 3: Since all other figures and tables use 1850 instead of 1700, it would be more consistent to use 1850 here, too.

- Fig. 2: Showing the map for present day would be clearer than discussing it.

- Formally, this study investigates anthropogenic land cover change, not land use change. It would be clearer to stick to the terminology of the land use and cover change community (e.g. Turner II et al., 1995)

References:

D.B. Grigg. The agricultural systems of the world - an evolutionary approach. Cambridge Geographical Studies. Cambridge University Press, New York, 1974.

Turner II BL, Skole DL, Sanderson S, Fischer G, Fresco L and Leemans R (1995) Land-Use and Land Cover Change: Science/Research Plan, IGBP Rep. no. 35. International Geosphere-Biosphere Programme, Stockholm, Sweden, 132 pp.

D.E. Vasey. An ecological history of agriculture: 10,000 B.C. - A.D. 10,000. Iowa State University Press, Ames, Iowa, 1992.

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