Atmos. Chem. Phys. Discuss., 14, C8152–C8156, 2014
www.atmos-chem-phys-discuss.net/14/C8152/2014/

Atmospheric Chemistry and Physics
Discussions

Interactive comment on "Potential climate forcing of land use and land cover change" *by* D. S. Ward et al.

D. S. Ward et al.

dsw25@cornell.edu

Received and published: 16 October 2014

REVIEWER: This is a v. nice paper that attempts to provide estimates for a range of climate effects arising from LULCC. By doing so, the authors rightly emphasise that human land man- agement has plenty of impact on atmospheric composition, and climate, and hence we need to look beyond "only" CO2 (and more recently: the bio-physical aspects of LULCC). As it seems, quite an amount of work has gone into the paper; it combines a range of simulation experiments done with a suite of global models with estimates that are more of a review -based nature, using previously published work. The individual numbers are thus the products of different levels of complexity, which reflects to some degree our current state of modelling, but should be a little

more openly acknowledged (see also the review by Thomas). I have a number of fairly minor comments, mostly related to the methods:

RESPONSE: Thank you for your thoughtful comments, we have addressed them with revisions to the text and in our responses included below. The manuscript has been rearranged and several parts have been re-written in response to the first reviewer. Our responses are preceded by "RESPONSE". We have included a copy of the main text of the paper with added text, and highly revised text highlighted in red, as a supplement to this response.

REVIEWER: 1) LULCC-C fluxes: Julia Pongratz made a nice comparison of how the exact "mean- ing" of this term varies between studies (depending on what fluxes are included), see her 2013 ESD paper. For clarity, could you specify, which of Julia's cases is clos- est/identical to your definition of LULCC CO2 flux

RESPONSE: We have done this, referencing Pongratz et al. (2014) several times in the new Sect. 2.2.4 (CO2 emissions) and noting that our method falls into the "D3" category from her paper and that our adjustment for the CO2-fertilization feedback is an attempt to emulate an "E2" model setup.

REVIEWER: 2) Have I overlooked something – but could you clarify whether in 1850 you used natural land cover, or applied a constant anthropogenic cover fraction for 1850 in the spin up – and/or accounted in the spin-up already for C lost due to LULCC before that period ? (see e.g., Sentman et al, Earth Int., 2011). Using natural cover for spin-up would be incorrect, because of large vegetation removals before that period, legacy effects, the different turnover times of harvested C and so forth.

RESPONSE: We apply a constant 1850 land cover during spinup but were not explicit about this in the text. We have added reference to this now in the new Sect. 2.2, first sentence of the second paragraph:

"Spin-up of CLM is carried out with year 1850 land cover, which includes some anthropogenic changes."

We now also reference Pongratz and Caldiera (2012) in which they note that almost 10% of historical anthropogenic global temperature change may be due to preindustrial LULCC which we do not capture in this study.

REVIEWER: 3) "Worst case scenario": While being rather academic, I actually quite like the idea of exploring a system's response to an extreme scenario case, especially since e.g., the most recent RCP LUC scenarios are limited in terms of their assumptions (and in the CMIP5 simulations were realised individually only by one IAM). The authors have put quite some effort into preparing this scenario. However, I had to ponder a bit as to why the "WCS" made me feel a little uneasy. And perhaps this is semantics, but to me the term "worst case" implies a scenario that is really, really bad – but not necessarily implausible. Yet I would argue that the total con- version of aerable land into crop and pasture indeed is implausible (hence disagreeing with your statement on page 12201, line 5) – based on the hypothesis that well before such a conversion was complete humanity would run into serious issues with local and regional hydrology (floods vs. water shortage), water pollution through fertilisers, desertification and related dust pollution, etc. Social, economic and political pressure would not allow this worst case to be reached. So I like the idea of a (as the authors call as well) theoretical case. But I would like to see it re-labelled, e.g., "theoretical extreme case" ("TEC") scenario, which would have the implausibility already clearer in the title.

RESPONSE: This makes sense to us, and we have rephrased the sentence on pg. 12201 to no longer include the word "plausible". We have changed the WCS to TEC in the text, tables, and figures.

REVIEWER: And: perhaps I have missed an important point, but possibly the authors would have saved themselves quite a bit of work by using the projections of the agro-ecological zones from FAO. I am sure there are differences in the methodology, but the same principle applies, namely to assess crop potentials based on soil and climate

to yield potential crop areas for present-day and future. How different are the areas identified suitable in the AEZs from the areas used in your paper?

RESPONSE: We did consider using the FAO AEZ dataset but our impression was that it would be difficult to extract estimates for pasture expansion from this dataset, even though it is much more detailed and comprehensive with crop potential, including a set of different crops and yields. Also the simplicity of the Ramankutty approach was appealing with the results of the analysis being the fraction of the gridcell supporting crops/pasture regardless of yield, which was ideal for creating the dynamic PFT time-series.

The difference in crop area between the AEZs and our estimates for total arable land appears to be small in most regions. They can be compared by looking at panel 'b' in new Figure A2 in our manuscript and comparing to the FAO panel for suitability for rain-fed crops. This is from the 2002 dataset: http://webarchive.iiasa.ac.at/Research/LUC/SAEZ/plates/gif/plate46.gif

The Ramankutty approach seems to underestimate crop area in northern Europe and in central Africa in comparison but the quantities being compared are not exactly the same.

REVIEWER: And just for curiosity: what's the main reason for your parameter values in (2) - (5) being different from Ramankutty?

RESPONSE: Good question - we follow the same procedure as Ramankutty et al. (2002) but use updated datasets. So where they use 1970-2000 climate data, we were using 1980-2010, and we were using updated soil and crop area data as well.

REVIEWER: Finally: a very recent review of existing potentially available cropland estimates also can help to place the "WCS" into context (accepted manuscript, online): Eitelberg et al., GCB, 10.1111/gcb.12733

RESPONSE: Thank you for pointing us to this paper, we now cite this in our discussion of our estimate of the potential crop area (4180 Mha) and note that it is near the high end of the range of published estimates, but not the highest. This part of the text is now in Appendix A.

Please also note the supplement to this comment:
http://www.atmos-chem-phys-discuss.net/14/C8152/2014/acpd-14-C8152-2014-supplement.pdf

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 12167, 2014.