**Atmospheric
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Discussions

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

A. Arneth (Referee)

almut.arneth@kit.edu

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 management has plenty of impact on atmospheric composition, and climate, and hence we need to look beyond "only" CO2 (and more recently: the biophysical 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:

1) LULCC-C fluxes: Julia Pongratz made a nice comparison of how the exact "meaning" 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 closest/identical to your definition of LULCC CO2 flux

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.

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 conversion 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., "theroetical

extreme case" ("TEC") scenario, which would have the implausibility already clearer in the title.

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

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

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