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Earth System Dynamics Discussions

Interactive comment on "Implications of accounting for land use in simulations of ecosystem services and carbon cycling in Africa" *by* M. Lindeskog et al.

Anonymous Referee #2

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In this paper, the authors use the Dynamic global Vegetation Model (DGVM) LPJ-GUESS, with a new representation of croplands derived from LPJ-mL from Bondeau et al (2007), to study the influence of land-use-change-induced land cover change (LULCC) on the simulation of vegetation variability and carbon cycling in Africa over the 20th century. The model is forced off-line by climate and CO2 concentrations. The authors claim that their results show improved simulation of vegetation seasonal variability, reasonable simulated crop yields (mean and variability), and demonstrate the importance of crop management practices for carbon cycling. The study is straightforward, the article is well-written and the topic within the scope of ESD. However I have some concerns with the paper as it is, which taken together may call for major revision.

My main concern has to do with the relevance and originality of the paper : it essentially looks to me as a regional focus from the global study of Bondeau et al. (2007). Bondeau et al. (2007) used LPJ-mL offline over the 20th century, but on the global scale: like in the present study by Lindeskog et al., they – among other things - compared simulated vegetation seasonal variability with satellite observations (although not over Africa), compared average simulated yields by country to FAO data, analyzed the role of the biosphere in the global carbon cycle (source/sink). The present study by Lindeskog et al. presents essentially the very same elements of analysis, with essentially the same model, only focused over Africa. I would leave it to the editor to decide whether in these conditions the paper potentially warrants publication. At the very least I think the authors should make it clearer how their present study differs from Bondeau et al. (2007): I understand there are a couple of improved crop parameterizations (e.g., relationship between LAI and leaf biomass) in LPJ-Guess-crop compared to LPJ-mL, but I am not sure these justify a new analysis – in any case their implication for the results shown are not discussed. Many 'factorial' climate simulations are performed here, that are not included in Bondeau et al. (2007), but there are only too shortly discussed at the very end of the paper. More generally, I am not sure I understood why LPJ-Guess had to be used, and croplands parameterizations from LPJ-mL included in LPJ-Guess, instead of using LPJ-mL directly ?

Another main comment has to do with what exactly is being compared here: are the authors comparing simulations with and without land-use change, or simulations with land-use change but with either "real croplands" (from LPJ-mL) and "approximated-by-grasslands croplands" (default in LPJ-Guess) ? This question is actually kind of rhetorical, along the text one understands it is the former, not the latter, but I am emphasizing this because it could be more explicit, in particular given the content of the introduction (which stresses that "at first DGVMs accounted for LULCC by simulating grasslands"), and the existing study of Bondeau et al. (2007) (which includes comparison of the default LPJ and LPJ-mL). I do think that comparing "LPJ-Guess-crop" and default LPJ-Guess (both with land-use change) would actually be interesting here, I am wondering why the authors did not do it ? I am slightly uncomfortable with the title of the paper, and how it reflects its content. The authors do analyze the impact of LULCC on carbon cycling in Africa. However, I don't think they analyze its impact on ecosystem services : what they are doing by comparing the seasonal cycle of simulated FPAR and observed NDVI (section 3.1), comparing simulated and observed yields (3.3), simulated and observed interannual vegetation variability (3.1, 3.3), is more of a model validation exercise instead (that is also how these elements are presented in Bondeau et al. 2007). I think maybe the authors should define more clearly what they mean by ecosystem services (what about water supply issues ? runoff, etc..), or reorganize the 'result' section of their manuscript to reflect this aspect of their results. Illustrative of this issue are lines 7-8 in the abstract: the authors claim to analyze "the impact of accounting for land-use on crop production". This sentence is problematic: if there is no land-use, then there is no crop production. I could see this work if the authors were comparing "LPJ-Guess-crop" and default LPJ-Guess, as mentionned above (then it would be "the impact of realistically accounting for land-use on crop production"); but again this is not what they are doing. I think this illustrates a broader issue with the content of the paper, e.g., a confusion between validation results and analysis of ecosystem services.

I think the authors should show somewhere a map of reconstructed land-use change in Africa over the 20th century and discuss the extent, location, etc, of this change – this is expected in this kind of study and would help the reader make sense of other figures and results. In particular the authors also mention cropland abandonment a few times (including in the abstract), I would be somewhat curious to see what area it affects in Africa. Similarly, data on irrigation should be presented and discussed upfront. Right now discussion of these various data is scattered across the result section (for instance, elements of lines 1-8 p.251, or lines 9-10 p.253, should come earlier). In particular, although this is discussed a little bit towards the end of the paper, I think the uncertainties surrounding land-use data in Africa over the last century should be further discussed – how such data is built, etc. (in particular the dataset used here, even if it is described in Bondeau et al. 2007).

Finally, one point I would like to see addressed is the implications of the overestimated yields (by factor 1 to 6) on the continental carbon balance: if yields are so largely overestimated, doesn't it alter the conclusions regarding the simulated impact of LULCC on the carbon cycle in Africa ? In particular, what about crop management practices in this context (I could see that if crop biomass is overestimated, then the impact of crop residue management, for instance, is also likely to be overestimated).

Other comments along the text:

p.242 l.14: at this point PHU is not defined in the text, I believe.

p.243 l.28: "2.0" : where does this number come from, and how constant is it in real life ?

Section 2.2: I assume a weather generator was used. This should be discussed.

p.248. Line 26-27: a important limitation of that improved fit is that because standardized anomalies are used, we can tell it affects variability, but not mean values. This should be explicitly stated (the authors do mention this in section 2.3, but it should be associated again with the results here).

Section 3.1: lines 24-25 p247 + lines 1-2 p248, and lines 3-5 p248, seem contradictory to me: either natural vegetation and croplands have similar phenology, either they don't. Maybe some point-scale plots of crop/natural seasonal cycles would help illustrate the differences.

Section 3.2: Why focus only on the Sahel ? What about the rest of Africa ? The Sahel does not show a particular strong impact of LULCC on simulated interannual variability (actually, virtually none), so I don't understand the regional focus. Why not focus on regions with a higher rate of LULCC ?

Section 3.3: so for wheat, shouldn't the temperature be set higher ? Interannual variability of FAO data can be problematic for many countries and crops (i.e., constant data, abrupt shifts, non-climatic factors contaminate the data, etc...). Were the countries shown on figure 9 selected in any way for the quality of their data ? If yes it should be mentioned.

p.251. Lines20-25: I don't quite agree with the "comparable" word choice (and the "similar" in the abstract). The total effect of LULCC is 8.3PgC, which is several times higher that the effect of cropland management options (that, in addition, offset each other). Even if the direct effect alone of LULCC may be only 4.4 PgC, it is still 2-3 times higher. See also comment above on yield overestimation.

p.253 lines1-3: so is IMAGE a better dataset ? But the authors used Ramankutty and Foley here...

p.253 lines 16-17: I don't understand this statement; -0.5 and +0.09 sound hardly similar. In absolute values, -0.5 and +0.6 are actually closer.

p.253 lines 18-23: this should be discussed in a lot more detail: what processes are involved here, what about radiation effects, etc... There are lots of simulations behind fig.11a, I would expect these results to be discussed further. In addition, as mentioned above, this aspect was not addressed in Bondeau et al. (2007).

Figure 1: what do the dotted and full-line arrows stand for ?

Figure 4: labels should be explained better (what does LPJ-GUESS LU stand for ? It is possible to guess, but still...)

Figure 6: what values are blanked out ?

Text on figures 9 and 11 is a little too small.

Figure 12: what difference is plotted here, explicitly ? The caption could have more information, as the reader is looking at differences of negative fluxes, etc....

Interactive comment on Earth Syst. Dynam. Discuss., 4, 235, 2013.