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Interactive comment

Interactive comment on "Impacts of land-use history on the recovery of ecosystems after agricultural abandonment" *by* A. Krause et al.

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We thank the two reviewers for their comments which certainly helped to improve the structure and readability of our manuscript. In the following we answer their comments.

Reviewer #1:

This paper provides an interesting model assessment of the time taken for land to 'recover' after a conversion from agricultural back to 'natural' land. This is a solid, if unsurprising, paper. The results seem robust and the background well researched. Some of the conclusions over-reach a little. It is well worthy of publication. There are a few minor improvements I would recommend.

The introduction is rather long for a paper with this amount of results, and many of the points are repeated in the discussion. Ideally, each point should only be made once, and some rationalization of the amount of general background given would be good too.

Reply: We have rewritten the discussion section reducing the amount of repeated information. We now only repeat information where we feel it to be necessary for clarity.

p 2, line 7. Who is the author of this editorial? The original source/s would be better.

Reply: Unfortunately, no information about the authors is available. We replaced the sentence and the reference so it is more relevant for our study now (p 2, line 7-9 in the revised manuscript with marked changes):

“Based on the HYDE data set, Campbell et al. (2008) estimated that 269 Mha of cropland and 479 Mha of pasture have been abandoned between 1700 and 2000.”

p 4, line 14 and 20. The amount of N fertilization to crops and how much is ’left behind’ seems to be a critical aspect of the story, as N limitation becomes important in the subsequent LUC back to ’natural’ (e.g. the discussion on p 7/8, lines 22 - 4). Some context of the size of this (where did the fertilization value come from?), and discussion of this assumption’s effects on the results are necessary.

Reply: As we were performing stylized simulations, we did not intend to account for the large spatial variability in N application rates. We chose a value of 75 kg N ha-1 yr-1 as a compromise between higher values in intensively managed croplands in parts of Europe and lower values as presently found in large parts of Africa (e.g. Potter et al. 2010). We added the following text (p 4, line 22-24):

“We simulated N fertilization in croplands by applying 75 kg ha-1 yr-1 equally throughout the year to sustain crop productivity with time. This value represents a compromise between higher values presently found in parts of Europe and lower values in most of Africa (e.g. Potter et al., 2010).”

p 5, line 10. I'm a little unsure about the exclusion of desert and tundra. If all grid-cells above 62.5N are excluded from the results, why are they included in the simulations, or the results plots? Why not just have the map end at 62.5N and save the space?

Reply: This is a good suggestion. We have adjusted the maps accordingly.

With regards to the desert, I'm wondering what (if any?) representation of irrigation there is, and whether if there is irrigation, 'desert greening' with irrigated cropland might be an interesting aspect of this study.

Reply: The reviewer raises an interesting point about desert greening via irrigation. We did not consider irrigation in our simulations. Although irrigation in deserts would clearly have a notable effect, the area of irrigated desert is however small globally.

p 6, line 19. Check the sense of this sentence.

Reply: We restructured the sentence, hopefully it is clearer now (p 6, line 26-27):

"In contrast, recovery times are clearly longer (>100 years) in other parts of the temperate forests and in the tropical forests."

p 7, lines 5 - 8. This ought to be in the figure caption, not the main text.

Reply: We have moved the text to the figure caption.

p 9, lines 1-3. This tropical soil carbon response is interesting - can you enlarge on what the physical or model mechanism that causes it is? What causes the change from soil carbon loss to accumulation?

Reply: The two phase aspect of the soil C response results from the differing effect of cropland on individual soil carbon pools. Only the faster-decaying pools are affected by tillage (see Pugh et al., 2015 for a justification), and loss of carbon from these pools dominates the response of the system in the initial period after conversion. Once these faster-decaying pools have come into balance with the influence of tillage on soil respiration rates, then the slow accumulation of carbon in pools not affected by tillage comes to dominate the overall response. We added the following sentence to the text (p 9, line 18-20):

“This occurs because tillage-driven C losses in more labile soil pools, which dominate the system’s response during the first decades, are eventually supplanted as the dominant process by accumulation in more stable pools.”

The discussion is very long, rather dry, and as a reader it is difficult to get a clear overall sense of how well the model results compare to field observations. The simple fix for this would be a table with: observation type (e.g. soil carbon recovery time in pasture); observation value (e.g. 100 years); closest model (e.g. P20); closest model value (e.g. 50 years); model performance (either + (too high), - (too low), or a tick (within the obs. uncertainty)). This would convert about 4 pages of hard-to-digest discussion into 1 page of at-a-glance clear results.

Reply: We agree that the discussion could be more condensed and much of the provided information (which necessarily represent only a subset of all available studies anyway) are not absolutely crucial for the study. We thus added the recommended table (Table 2), although it was often difficult to summarize the main findings and characteristics of the studies and their comparability to our results in just a few words. This, as the reviewer correctly pointed out, allowed us to shorten the original text.

Please could you define acronyms and unusual terms, when they are first used? e.g. swidden, NEE, NBP.

Reply: We removed the sentence containing “swidden” and added the definition to NEE (p 15, line 20-21). NBP was already defined when first used (p 5, line 18-19).

p 13, line 10. I’m not sure it’s accurate to dismiss LUC biogeophysics as irrelevant at a global scale. (For where biogeopysics has a significant impact on the global climate, see for instance, Davies-Barnard et al., 2014; Davin and de Noblet-Ducoudré, 2010; Jones et al., 2012; Matthews et al., 2004; Pongratz et al., 2010.)

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Reply: We did not intend to claim biophysical effects on global climate to be irrelevant and agree that the impacts are still regarded controversial. We have rephrased the sentence to make this clearer (p 14, line 1-5):

"Substantial impacts related to realistic land-use have been found on local-to-regional scales (Alkama and Cescatti, 2016; Peng et al., 2014). Whether or not the locally observed changes translate to a significant global radiative forcing is still debated as the direction of change differs across regions in some climate models, which may cancel when integrated globally (Pielke et al., 2011)."

p 13, line 22. Ill defined is more the case than subjective.

Reply: Corrected.

p 13, line 24. Check the sense of this sentence.

Reply: We shortened the sentence (p 14, line 15-17):

"By our definition we do not capture situations e.g. where the system approaches towards a new equilibrium (as soil C did in some regions in the cropland simulations)."

p 14, lines 7 - 15. Why is +/- 1 sd not the default way of analysis in this study? Wouldn't that make much more sense?

Reply: We indeed considered whether an ecosystem should be designated as "recovered" in cases where the variable's value was initially higher than under reference conditions. However, while recovery from lower or higher levels points towards different mechanisms, one would not easily be able to distinguish both cases. We decided to study recovery from a depletion perspective (which is what people usually link to this term) and thus only used a lower threshold in our default definition as this is the one threshold that is critical from a depletion perspective. In any case, both options result in very similar recovery times in most biomes (Figure A5).

The conclusions need to be more specific, and restricted to results that can be directly evidenced from the results in the paper.

For instance, in #5, BVOCs are the main point being made - but BVOCs aren't included in the model or the paper, and the point is referenced elsewhere. If you have to reference another paper in your take home messages, you really should rethink what *your* take home messages are, because currently, they are someone else's.

Reply: We believe that conclusions should not be restricted to the direct findings of a study but should also consider the "bigger picture". BVOCs are just an example for possible implications of our study on subsystems beyond the land; while we were not able to account for BVOCs in this particular study, it could be principally done in LPJ-GUESS. Still, to avoid misinterpretation we removed the BVOC part from the respective sentence.

Conclusion #3 also particularly suffers from lack of evidence. The results here show that for most variables, an equilibrium simulation of 100 years is plenty to sort out any legacy LUC effects. Soil carbon is an exception, but then there is lots of evidence that soil carbon is very uncertain in both models and observations. Longer simulations would do the same as re-growth dynamics in many cases, and so you need to highlight which variables in which regions under what time-scales, you have shown are affected.

Reply: Vegetation C and composition indeed (nearly) recover within 100 years in most ecosystems, but we don't see how extended simulations would solve the problem. While this approach would indeed reduce land-use legacy effects in equilibrium simulations, it is not an option in more common transient climate simulations where the system is never in an equilibrium state. We added the following to conclusion #3 (p 16, line 11-14):

"Our study suggests that for vegetation and soil C studies, accounting for LUC over the last 100-150 years is sufficient in the tropics, while in the temperate and boreal zone more than 200 years might be necessary; studies restricted to vegetation should not have to account for LUC more than 150 years ago in any major climatic zone."

The color schemes, especially on the maps, are not all that easy on the eye, and would be very difficult for someone who is color-blind to interpret. Could you consider another color scheme? You could look to http://colorbrewer2.org/ for some good, easy color schemes.

Reply: To ease matters for color-blind readers we changed the green-to-red color scheme to a blue-to-red one in Figure 3/A3/A4/A5.

Reviewer's references:

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