Earth Syst. Dynam. Discuss.,
doi:10.5194/esd-2016-11-RC1, 2016

Earth System Dynamics Discussions

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

Anonymous Referee #1

Received and published: 9 May 2016

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.

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

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.

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?

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.

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

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

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?

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.

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

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.)

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

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

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?

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.

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.

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.

References:

Davies-Barnard, T., Valdes, P. J., Singarayer, J. S. and Jones, C. D.: Climatic impacts of land-use change due to crop yield increases and a universal carbon tax from a scenario

model, J. Clim., 27(4), 1413–1424, doi:10.1175/JCLI-D-13-00154.1, 2014.

Davin, E. L. and de Noblet-Ducoudré, N.: Climatic Impact of Global-Scale Deforestation: Radiative versus Nonradiative Processes, J. Clim., 23(1), 97–112, doi:10.1175/2009JCLI3102.1, 2010.

Jones, A. D., Collins, W. D., Edmonds, J., Torn, M. S., Janetos, A., Calvin, K. V., Thomson, A., Chini, L. P., Mao, J., Shi, X., Thornton, P., Hurtt, G. C. and Wise, M.: Greenhouse Gas Policy Influences Climate via Direct Effects of Land-Use Change, J. Clim., 26(11), 3657–3670, doi:10.1175/JCLI-D-12-00377.1, 2012.

Matthews, H. D., Weaver, A. J., Meissner, K. J., Gillett, N. P. and Eby, M.: Natural and anthropogenic climate change: incorporating historical land cover change, vegetation dynamics and the global carbon cycle, Clim. Dyn., 22(5), 461–479, doi:10.1007/s00382-004-0392-2, 2004.

Pongratz, J., Reick, C. H., Raddatz, T. and Claussen, M.: Biogeophysical versus biogeochemical climate response to historical anthropogenic land cover change, Geophys. Res. Lett., 37(8), L08702, doi:10.1029/2010GL043010, 2010.

Interactive comment on Earth Syst. Dynam. Discuss., doi:10.5194/esd-2016-11, 2016.