

Interactive comment on “Comparing the influence of net and gross anthropogenic land use and land cover changes on the carbon cycle in the MPI-ESM” by S. Wilkenskjeld et al.

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Received and published: 27 June 2014

article

Answer to anonymous review #2

We thank the reviewer for his or her helpful comments. We are pleased, that the reviewer found our paper well-written and an important topic for Earth system modeling and thus worth publishing in Biogeosciences.

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Specific comments

1. ***Throughout the paper the authors refer to the land-use datasets used by their model as the “Harmonized Land-use Protocol”, or simply the “Harmonized Protocol”. However, the official name for this dataset is the “Land-use Harmonization Dataset”.***

We have changed our terminology accordingly.

2. ***In the abstract and section 1 the authors state that gross land-use changes do not affect the net vegetation distribution. However, it should also be noted that although the net areas of natural vegetation, cropland, and pasture might not change, the underlying natural vegetation could be quite different (e.g. a mature forest could become a very young regenerating forest). Under a changing climate the re-growing vegetation could also potentially be different from the original natural vegetation within a grid-cell.***

In response to questions by the other reviewers (pt. 8 of reviewer #1 and pt. 2 of reviewer #3) it has turned out that though in JSBACH/CBALANCE the biogeophysical properties and NPP are only dependent on the vegetation distribution and not on the (e.g. maturity) state on the vegetation, other effects than LCE derived from the differences between net and gross transitions (different carbon stocks and minor differences in the vegetation distribution) are more important than previously anticipated. Therefore the discussion of such derived effects also has become more important and we have thus refined the abstract, paragraph 3 of section 1 and paragraph 8 of section 5 in the revised manuscript to put more emphasis on the existence of these effects. Though our study does not address the topic, we now also mention that in reality the two transition types would lead to different changes in the biogeophysical surface properties.

Although biogeophysical feedbacks of land use change on LCE could potentially be important at the local scale, our analysis has shown that accounting for

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biogeophysical effects is a second-order effect (see our response to pt. 7 by reviewer #1; here, we used the forcing applied by Brovkin et al, 2013, which includes biogeophysical effects, to compare to our setup that neglects them). This finding is also consistent with Pongratz et al. (Ph.D. Thesis, 2009) (http://www.mpimet.mpg.de/fileadmin/publikationen/Reports/WEB_BzE_68.pdf), who showed that global land use emissions are hardly altered by biogeophysical feedbacks.

3. ***In section 2.1 the authors state that the off-diagonal matrix elements are obtained from an external datasets, without stating what that dataset is. I assume they are referring to the Land-use Harmonization dataset discussed elsewhere in the paper?***

Though the LULCC data set used for our study is “the Land-use Harmonization Dataset”, the model is capable of handling any LULCC data set providing values for the off-diagonal elements of the matrix in eq. 1. In our opinion it would be misleading to include the name of the used data set in the technical model description since this is independent of the choice of data. Thus the used data set is not named until the description of the experimental setup where it is of course appropriately acknowledged.

4. ***Why do the authors not include RCP6.0 in their set of experiments?***

We performed our experiment using JSBACH/CBALANCE which is an off-line model which needs to be driven by atmospheric input data. Many of the CMIP5 models did not perform experiments for all of the proposed scenarios, and at MPI-M, the RCP6.0 was left out. Therefore no appropriate forcing data for RCP6.0 has been available for our study. We have clarified this in the description of the used scenarios: First sentence of sect. 3, paragraph 3 has been replaced with:

“Four of the CMIP5-scenarios (Taylor et al., 2012) were redone: the historical (1850–2005) and the three of the RCPs (2006–2100) (van Vuuren et al., 2011a)

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scenarios for which forcing data from the MPI-ESM were available: rcp2.6 (van Vuuren et al., 2011b), rcp4.5 (Thomson et al., 2011) and rcp8.5 (Riahi et al., 2011).”

5. ***I would also like the authors to mention in the Discussion section that gross land-use changes can also impact the biophysical properties of the Earth System (e.g. surface roughness, albedo, etc) as well as ecological impacts (e.g. young regenerating forest instead of old-growth forests, reduction in habitat for biodiversity, etc).***

See our response to pt. 2 above.

6. ***In section 5 the authors state that Hurtt et al. 2006 made a simple assumption that shifting cultivation occurs in “the tropics”. However, it should be noted that in Hurtt et al. 2011 (the paper that the Land-Use Harmonization datasets are based upon) this assumed area of shifting cultivation was improved and based upon the map of Butler (1980).***

Our explanation has been oversimplified, and we have changed it accordingly in the 2nd paragraph of section 5 by replacing the sentence describing Hurtt's assumptions by:

“...and thus Hurtt et al. (2011) applied a rather coarse and static map of the location of shifting cultivation (mostly in the tropics) and assumed a fixed period for which agricultural land is cultivated before it is again abandoned (15 years, corresponding to an abandonment rate of 6.7%).”

7. ***The authors should state that not all Earth System models are currently able to model gross land-use changes, and that this should be an area for further model development over the next few years.***

In the revised manuscript we explicitly stress this point (in paragraph 2 of section 5) to explicitly encourage further development of the non-gross-LULCC models:

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The sentence *“One might thus be tempted...”* in the 2nd paragraph of section 5 has been replaced by: *“Since gross transitions includes a more complete description of the LULCC, this should be the preferred method for modeling LULCC; however, presently only few models are capable of doing so.”*

8. ***Some minor grammatical and typographical corrections***

These corrections have been included in the revisited manuscript.