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Biogeosciences Discussions

Interactive comment on "Global climate response to idealized deforestation in CMIP6 models" *by* Lena Boysen et al.

Anonymous Referee #3

Received and published: 20 August 2020

Review of bg-2020-29:

This study reports on the multi-model LUMIP deforestation experiment. The authors show that controlled, large-scale, global deforestation may contribute global geophysical cooling of near-surface temperatures and global geochemical warming. The geophysical effects vary latitudinally and by model, generally with warming in the tropics and cooling elsewhere, while the geochemical effects are estimated offline as warming everywhere. The geochemical effects generally are greater than the geophysical effects, leading to net warming, although a potential CO2-enhanced land sink is not included here. Land carbon losses are driven by vegetation loss. Some novel metrics for assessing and potentially estimating the effects of deforestation are also presented.

I appreciate the tremendous effort the authors have put into this study to advance our

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understanding of the effects of land cover change on the earth system. I have a few main comments, followed by some brief detailed comments.

1) What are the take home messages? There are a lot of different analyses, and only one aspect is highlighted in the abstract. The abstract includes some key numbers, and at least the potential net warming statement of large scale deforestation. While this in itself is a key finding (with the associated caveat of constant CO_2), there are a few other notable results to highlight. Missing are the policy/scenario implications related to time/fraction of emergence for climate vs carbon. And the potential for rough estimates of response via the sensitivity metrics (which are analogous to climate sensitivity of models). I understand that there are some limitations to the sensitivity metrics and the time of emergence estimates, but based on S13 and S18 (plus the rest of the carbon figures), it seems safe to say that the climate signals have relatively long time frames while carbon signals have relatively short time frames. However, the climate signal emergence is further complicated by observations that show large, immediate meteorological distinctions between forest and grassland. The sensitivity metrics support the temperature and carbon results, and are potentially useful to the community.

2) In relation to comment (1) above, switching some of the regular and supplemental figures would make the paper stronger. For example, figures S22 (carbon sensitivity to deforestation) and S18 (ToE and FoE) are more relevant to the carbon points than figures 9-10, which are explanatory. Also, figure S3 is much clearer and easier to understand than figure 3, and follows the text better (you can add Tas to fig S3).

3) The descriptions of Tsurf-model and Tas are not complete, which makes it difficult to properly assess the temperature comparisons. While Tsurf is clearly a radiative temperature, is Tsurf-model a radiative temperature for all the models? Tsurf in some models is a canopy air temperature, at a height dictated by the displacement height and aerodynamic roughness. The 2m air temperature is often the air temperature 2m above this “Tsurf.” It is important to be clear as to what and where these temperatures actually are, as shown by some of your references.

4) MIROC does not seem to meet the deforestation harmonization requirements, and its plots don't seem to add to the understanding of the issue. In fact, a lot of extra text is dedicated to explaining why MIROC is different from the others. It would be cleaner if it were not included.

5) Some supplemental figures are cited out of order.

With respect to the effects of differences in initial forest cover on the implementation of deforestation (lines 229-227), the author's may be interested in this recently published paper:

A.V. Di Vittorio, X. Shi, B. Bond-Lamberty, K. Calvin, A. Jones, 2020, "Initial land use/cover distribution substantially affects global carbon and local temperature projections in the integrated Earth system model", Global Biogeochemical Cycles. doi: 10.1029/2019GB006383.

Specific comments:

lines 326-327: this isn't clear from fig 3. fig s4 is more appropriate here.

line 465: which figures show these regional effects?

line 505: relate toe and implications to observation of immediate temperature differences between forest and grassland, and perceived differences

line 536: this paragraph is out of place - it doesn't relate to the rest of the section

line 644-646: fig S19 shows declines in GPP for CESM throughout the deforestation area, so it isn't clear how CESM has increases in GPP where the other model have decreases.

lines 680-684: based on fig S20, it doesn't appear the miroc can have the highest sensitiviy. most of its coverage has the smallest change in c per fraction of deforestation.

lines 710-713: this is unclear - you have separated your total range in two, arbitrarily,

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and included veg only change models in one group with other total land c change models.

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