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Interactive comment on "Smaller global and regional carbon emissions from gross land use change when considering sub-grid secondary land cohorts in a global dynamic vegetation model" *by* Chao Yue et al.

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Yue and co-authors do in this paper demonstrate how inclusion of differently aged forests in the ORCHIDEE DGVM leads to reduced global carbon emissions (CE) from land use changes (LUC) during the period 1501-2005. This reduction is mainly attributed to the part of the CE which stems from shifting cultivation in the tropics (which they also included as a new feature in ORCHIDEE)). The authors systematically quantify the contribution of different processes (net LUC, shifting cultivation and wood harvest) to the total CE from LUC (ELUC). The study is thus an important contribution to

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Discussion paper

quantifying the ELUC which clearly demonstrates the importance of the inclusion of many aspects of vegetation dynamics and LUC to obtain accurate estimates of ELUC.

The paper is in general clearly written (though the authors at some places tend to repeat themselves), well structured and easy to read.

The main part of the description of the model development has been put in an accompanying paper "Representing anthropogenic gross land use change, wood harvest and forest age dynamics in a global vegetation model ORCHIDEE-MICT (r4259)", Global Model Development Discussions, 2017-118 (hereinafter GMD118), where the model functionality is demonstrated in an idealized site study and a regional study in South Africa. Since these two papers are closely related, some of my comments (including the main comment on the setup on the S-experiments) below also apply to GMD118 (unfortunately I missed the discussion deadline for GMD118).

The idea to separate the work in a development and an application part seems nice, but the separation between the two papers is not very clear: A lot of the model description is repeated in the present paper, and the analysis methods and results are very similar for the "South Africa" study and the global study which suggest to replace the results of the "South Africa" study with those of the global one in GMD118.

Though the papers (present and GMD118) represent a valuable contribution to the quantification of ELUC and its originating processes, there are a number of issues to be addressed at different level of severity:

Major:

Though qualitatively the major conclusion of the paper (effect of introducing age classes on gross transitions LUC) is obvious, unfortunately the experimental setup is not optimal for supporting this conclusion quantitatively. The authors use an "additional process approach" by starting with a model without any LUC (their S0), then adding net transitions (S1), gross transitions (also called "turnover", S2) and finally wood harvest

(S3). Such an approach only delivers a best guess for the last step - i.e. the wood harvest. However the main conclusion is about the turnover and the result does thus ignore the differences in the effects of wood harvest between the different experiments, which are clearly present (e.g. their increase in ELUC_harvest from ageless to age). To provide a best guess on the effect of turnover, an additional experiment (I call it S4), including net transitions and wood harvest but ignoring turnover, would be needed. The turnover effects are then calculated from the difference between S4 and S3 instead of between S2 and S1. This could either be used to throw out S2 (the S2 setup is - to my knowledge - not used by any model, and thus is only usable to provide good estimates on the effects of wood harvest, not for model intercomparison) or to turn the general experimental structure into a "subtractive process approach", based on the "best guess" experiment (S3) and analyzing the effects of the different processes by removing them individually (turnover by comparing to S4, harvest by comparing to S2). In the first case the quality of the ELUC from wood harvest will be degraded, in the latter, some structural changes are needed to the paper.

I don't see the added value of the "South Africa" study in GMD118 in addition to the idealized site level study (also in GMD118) and the global study presented in this paper. The description of the "South Africa" sub-study in GMD118 is very short and hardly complete (e.g. which initial vegetation distribution was used?, were the LUH1 data backcast as in the present global study?)

A reasoning and discussion of the validity and influence of the priority rules for turnover and wood harvest is absent in this paper though some discussion is included in GMD118. This needs to be added or at least referenced and could also advantageously be extended.

The authors several times mention "inconsistencies between LUH1 and ESA-CCI-LC", but these problems may as well - at least in parts - stem from the choice of priority rules and the assumptions by Hurtt et al. (2011) for creating the global LUH1 data set. At least some comments attempting to disentangleing these effects should be made.

See e.g. the discussion in Arneth et al. (2017) and references therein.

l. 507-543: Upscaling the ELUC based on scaling the total carbon to the TRENDY intermodel mean is very speculative and does - though it seems so - not add any quantitative information - specially not since the main focus of the paper is on the effects of including (or excluding) certain processes and not on the absolute ELUC numbers. I suggest to put the entire paragraph together to (essentially, not literally): "We have low absolute ELUC, relating to a low absolute carbon stock. These two quantities seems to be linearly related (Li et al. 2017)". This let the readers do the upscaling themselves being aware that this extrapolation is only qualitatively valid. This leaves Fig. S8, Table S2 and perhaps Table S1 (the main message can also be extracted from Table 3) obsolete.

The presentation let the model development seem entirely new, though Reich et al. (2013) contains a similar introduction of gross transitions and Shevliakova et al. (2009) introduced both vegetation with different age and gross transitions. These two studies must be taken into account in the description of the model development.

Minor:

Are S0-S2 and the Spinup entirely without wood harvest or do they use a fixed pre-industrial (1500) wood harvest? If no harvest has been used, S3 will be subject to a "carbon chock" at the beginning of the transient run stemming from starting from a wrong equilibrium state and the absolute ELUC numbers - specially from S3 - are likely overestimated (S0 contains too much carbon).

Figure 6 needs to be introduced in paragraph 2.2 (likely with a lower number), since it actually do not show the results of the work of the authors but is rather a part of the description of the LUH1 data set. The figure is, however, absolutely necessary for the understanding of the results.

The numbers in Line 544-551 should also be introduced when introducing the LUH1

data set (paragraph 2.2). It is rather important for evaluating the results to know that substantial fractions of some of the transitions in the LUH1 data set are ignored.

Was "apparent gross transitions" arising from the aggregation of LUH1 (which only contains gross transitions in the tropics) over multiple grid cells actively suppressed outside the tropics? If yes: Why? This seems to be an unnecessary loss of information.

The division of herbaceous vegetation into two age cohorts based on the soil carbon (SOC) is either insufficiently explained or only representative for a certain type of LUC. In line 53-54 of GMD118 the authors state: "SOC decreases when a forest is converted to cropland; SOC increases when a cropland is converted to pasture" indicating that young herbaceous vegetation can have SOC both higher and lower SOC than the previous vegetation. Furthermore it seems that the division ignores that the main part of the changes in SOC do not take place instantaneously at the time of LUC.

Technical:

It should be made clear earlier in the paper that the terms "shifting cultivation" and "turnover" are used interchangeably.

Please repeat the main quantitative findings of the study in the conclusions.

In some cases letters are swapped in the subscripts.

Figs. 4-6 and S7: Please swap the order of the sub-panels from column-wise to row-wise. This is used in Fig. 2 and is much more intuitive.

Figs. 5, 6 and S7: The order of the geographical regions seems totally random. Please introduce some "around-the-globe"-ordering as in e.g. v.d.Werf et al. (2010). I am not saying, that the authors should adopt the regions from v.d.Werf - just the systematic ordering principle.

Figs. 3d-3f, 6, S3, S4 and S6: The unit Mkmˆ2 is not a valid SI unit (double prefix). Please use "Mill. kmˆ2", "10ˆ6 kmˆ2", "10ˆ12 mˆ2" or rescale to e.g. "MHa" (which

would fit the numbers in Figs. 3 and 6 quite well).

Fig. 5 vs. 6: It is confusing that Fig. 5 starts in 1900 which Fig. 6 starts in 1800. The only thing mentioned in the paper before 1900 is - as far as I see - the peaks in North America. Does that need to be displayed?

Table 2: The main point of this table is the threshold fractions of Bmax used - the ages used for the determination are only relevant for the development stage and thus these are the numbers which should show up in brackets. Please either leave out "x Bmax" (described in the table caption) or add it everywhere - the mixture leaves the table rather confusing. The PFT-numbers are only of model internal relevance and should be removed.

In GMD118 l.477 and l.688 the LUH1 data set seems attributed to Hurtt et al. (2006) while the actual description of the data are in Hurtt et al. (2011).

The initial nomenclature is in my opinion more confusing (through unnecessary abstraction of rather simple expressions) than helpful and could be removed.

My personal opinion is that supplemental material should be kept at a minimum. For this paper this implies that the description of the backcast of the LUH1 data should rather be an appendix to the paper - or to GMD118 if the method was also applied here. Raw figure data should rather be "available upon request" than put in the supplement.

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