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 har- vest) to the total CE from LUC (ELUC). The study is thus an important contribution to 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.

We thank the reviewer for the efforts to review our paper and the general comments. Please find out point-to-point response below each comment.

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.

The other reviewer of this paper has raised similar comments regarding the likely overlap between the two manuscripts. We appreciated the suggestion and have restructured both papers to make a clearer separation between them. Please refer to our response [R3] to the other reviewer’s comments on this issue.

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.

We thank the reviewer for this thoughtful comment. We make it clear in the revised manuscript that separating the overall LUC activities into these three processes is to examine their individual contributions from a theoretical, modeling perspective, in particular given that land turnover or gross land use change has been overlooked by modeling practices (in view of Arneth et al. 2017). In reality, however, these three activities might never be clearly separated, for example, a fallow forest following agricultural abandonment in land turnover process might later be maintained for wood harvest, or vice versa. Here we followed the approach of Stocker et al. (2014) to run additive factorial simulations to quantify the effect of each process.

We followed the reviewer's suggestion to add an additional S4 simulation, which includes net transitions and wood harvest (it is named S2b simulation). Both emissions from land turnover and wood harvest are calculated from an additive and a subtractive approach. For example, for land turnover, emissions to the additive approach are quantified as the difference between S2 and S1 simulation (the original ones in the manuscript), and emissions to the subtractive approach are quantified as the difference between S3 and S2b. An overview table as below is now provided in the Supplement Material to show the emissions to different approaches (in unit of PgC for 1501–2005):

	No age dynamics	With age dynamics
ELUC_net	123.7	118.0
ELUC_turnover	45.4	27.3
ELUC_turnover_S2b	39.9	25.1
ELUC_harvest	27.4	30.8
ELUC_harvest_S2b	32.9	33.0

As is shown in the table, different approaches have a larger impact on $E_{LUC\ turnover}$ and $E_{LUC\ harvest}$ by the $S_{ageless}$ simulations than S_{age} simulations, but in general the difference between different approaches is much smaller than the emissions itself. This indicates that the impacts of LUC on carbon emissions simulated by ORCHIDEE are largely a linear system. Overall, we cannot agree with the reviewer that a subtractive approach is necessarily superior to an additive one, even for a nonlinear system. For a quasi-linear system like the case here, we think that using either approach would yield small differences. For a nonlinear system, different approaches can be used depending on the purpose of attribution being performed, sometimes a re-scaling or more complex treatment techniques might be needed (e.g., Ciais et al., 2013; Trudinger and Enting, 2005).

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?)

Following the comments by both reviewers of this paper and the reviewer’s comments on gmd-2017-118, the results for the carbon fluxes of the Southern African study has been removed in the revised GMD paper, with only the results on the forest cohort dynamics (Fig. 9) being kept. This is a model feature from our developments that we would like to present. Please see our responses to the similar question raised by Benjamin Stocker on this paper [R3] and the responses in gmd-2017-118.

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 primary target cohort for turnover and its age setting (15 years) mainly depends on the assumptions used in LUH1 data, as has been explained in the manuscript. We think the systematic, worldwide information on rotation lengths of shifting cultivation or wood harvest is lacking. This partly hinders our work to set a more reasonable, regionally varying target cohorts and their ages in the model. This point and its influences on derived carbon emissions are now discussed in the revised manuscript.

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.

The inconsistencies between LUH1 and ESA-CCI-LC are also partly due to the fact that we used the harvested forest area, rather than the wood volume as the input information. But most of the inconsistencies are because of the spatial inconsistencies between the two land cover maps (LUH1 and ESA-CCI-LC). The choice of priority rules is at a lower hierarchical level than the distributions of, and the transitions among the land cover types. Therefore it would not cause any additional inconsistency. The inconsistencies between LUH forest area and that observed by satellites is also highlighted in Meiyappan and Jain (2012), and the inconsistencies in land use transitions among different data sets have been highlighted in Li et al. (2017).

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 section 4.2 addresses the errors in simulated biomass stock in the current model version and its impact on simulated E_{LUC}. We think this section is necessary. It is also important to show that converging values can be obtained by adjusting for such errors. We have chosen not to put such information in the main text because we share the reviewer's comments that these are not completely valid in a quantitative sense. As these are just materials in the Supplement, we tend to keep them, assuming that they can be potentially useful for readers interested to know more details on the corrections that have been made. However, we stress in the revised manuscript that such extrapolations should be taken with caution and the numbers derived are not fully quantitatively valid.

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.

Following the reviewer's suggestion, the introduction section and model description section will be revised to fully reflect these relevant peer works. We included in the revised gmd-2017-118 an overview table of DGVMs having implemented gross land use change and that table will also be referred to in the revised manuscript of BG paper.

Minor:

Are S0-S2 and the Spinup entirely without wood harvest or do they use a fixed preindustrial (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).

S0 to S2 and spinup runs do not include any wood harvest or land turnover. The "carbon shock" indicated by the reviewer is visible in Fig. 3c for the few beginning years since 1501 but such an initial peak of emissions is small compared with the cumulative emissions over 1501–2005. We agree with the reviewer that omitting wood harvest in the spinup runs and in the simulations of S0 to S2 will lead to overestimation of emissions from wood harvest, but their impacts on emissions after 1850 are expected to be very small due to the fading of legacy effects with time. On the other hand, these runs do not include either the legacy emissions from net land use changes before 1501, which can lead to underestimation of emissions. These points will be discussed in the revised manuscript. Please also refer to our responses to a similar comment raised by the other reviewer of this paper (Page 5 of the responses to the comments by Benjamin Stocker).

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.

We agree with the reviewer on that Fig. 6 is not the result of our work in a very strict sense, although it is in fact an output of reconciling LUH1 data and the ORCHIDEE PFT map derived from the ESA-CCI-LC land cover map. Note that in the original LUH1 data land use transitions are not downscaled to forests or grasslands, it is after such reconciliation that historical LUC areas involving forests have been reconstructed. To put this figure as Fig. 6 allows readers to easily refer to it when going through the results of regional LUC emissions. On the other hand, introducing this figure in the section 2.2 would be a little isolated without presenting it in detail (whose details are presented rather in the section 3.3). For the reconciliation between the LUH1 data and the ORCHIDEE PFT map, all relevant outputs in section 2.2 are provided in the Supplement, which has been referred to in the section 2.2. We believe this can provide useful information if readers are interested on the specific outcomes of the historical LUC data reconstruction.

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.

Following the reviewer's suggestion, we have moved these descriptions from the section 4.2 to the revised section 2.2, which are further referred to in the revised section 4.2.

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.

Such a loss of information is not out of an intentional active suppression. It is unfortunately due to an error in upscaling the data from 0.5° to 2°. Land turnover activities are represented in the model using land transition matrices. These matrices are constructed during the process to reconcile LUH1 historical land use transition data and the current-day PFT map used by ORCHIDEE. Somehow during this process the land turnover resulting upscaling is unfortunately neglected. It can be challenging to rerun all the simulations with updated land turnover matrices because of computation limitation (because including a total number of 65 cohort functional types has tripled the time needed, compared to a default ORCHIDEE-MICT run which is already slow due to many processes included). On the other hand, this will not change the fundamental conclusions of the current manuscript. Based on these considerations, we have re-done the process to build up the turnover matrices by including the gross land use change in spatial upscaling. Then we described the missing LUC areas by ignoring the gross LUC from spatial upscaling, and used this information to correct the simulated emissions.

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.

The key point is to separate agricultural lands (croplands and pastures) into two broad age groups assuming that they have different soil carbon stocks. In general, because changes of soil carbon stock following land use change are spatially highly diverse and depend on many factors including the land cover types before and after the transition, the model feature described here is more for informative purpose rather than having solid scientific significance. This is primarily due to the fact that soil moisture is simulated on the basis of water columns, and soil temperature over the whole grid cell in the model rather than on the cohort level, as is explained in the gmd-2017-118 paper (Sect. 2.2.3, 2nd paragraph). To fully track the soil carbon trajectory after land use change, a much larger number of cohorts for herbaceous vegetation are needed, but this is limited by the computing power when running simulation over the globe. Overall, this feature is more like a “place holder” whose function needs to be explored in the future model application.

Because of the diverse changes in SOC following land use change and the limitation in the number of cohorts, we choose a threshold of 65% of the maximum SOC of all vegetation types as the cohort boundary, hoping this can partly accommodate the large ranges in SOC in different land cover types. It turns out such setting seems not working well, as the cohort dynamics of herbaceous vegetation do not show a reasonable temporal pattern as shown in the case of forests (Fig. 9 in gmd-2017-118). Nonetheless, as the differences in land turnover emissions between the two simulations with and without sub-grid cohorts are mainly driven by sub-grid secondary forest dynamics, the influence of errors in setting herbaceous cohorts is expected to be small. All these points will be discussed in the revised manuscript.

Technical:

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

This is a good point and will be done in the revised manuscript.

Please repeat the main quantitative findings of the study in the conclusions. In some cases letters are swapped in the subscripts.

We repeat the main findings in the conclusions of the revised manuscript and subscripts are double-checked.

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.

Following the reviewer's suggestion, we have revised Fig. 5–6 and Fig. S7. For Fig. 4, we keep the current layout. This figure shows in each row, the simulated E_{LUC} by $S_{ageless}$, the age effect and the LUC area involved for each LUC type. Though not being supported by any serious scientific papers, we think to compare maps in a horizontal layout is more intuitive for human eyes.

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.

Thanks for this good suggestion. The order of regions in these figures will be re-arranged.

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).

Thanks for this good suggestion. The unit of Mkm^2 is changed to $10^6 km^2$.

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?

Indeed, the only reason to start the horizontal axes of Fig. 6 from the year 1800 is to show the strong legacy impact on emissions in North America. Following the reviewer's suggestion, we have changed Fig. 6 to have the same horizontal axis range as Fig. 5. The pre-1900 LUC area in North America is still described but without a figure being shown.

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.

We have adjusted the table to put the age information within the brackets and to put the information of fraction of B_{max} in the table cells, with the meaning of "x B_{max}" being explained in the table caption.

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).

In these two places the description of residence time of shifting cultivation (15 years) is cited from Hurrt et al. (2006). Both Hurrt et al. (2006) and Hurrt et al. (2011) described these and we have cited both in the revised gmd-2018-118 paper.

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

We would like to keep this nomenclature if it is allowed according to the journal policy, with the hope that it can facilitate the reading process for the readers without a specific land use change research background.

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.

We believe the suggestion to put in an appendix the backcasting of historical land cover maps is better than putting them in the Supplement if the journal policy allows. We will check with the editorial staff of the journal on this. Several papers have put the raw data in the Supplement and we followed them, but this might not be compulsory. We will also check with the editor on this.

References:

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