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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The present paper presents an application of the model described in Yue et al. (2017), GMDD, for global simulations covering the period where land use change (LUC) forcing data is available (1501-2005). Simulated cumulative emissions are 118 PgC for net land use plus 27.4 PgC for effects of sub-gridscale bi-directional land turnover (shifting cultivation type agriculture) plus 30.8 PgC for effects of wood harvesting. This amounts to a total of 176 PgC. This is at the lower end of the range of available estimates.

A special focus is put on the value of distinguishing age cohorts of land patches that have been affected by land conversion at different times in the past. The paper shows that not accounting for this effect increases estimates for cumulative LUC emissions. Authors explain that this is due to the generally higher average biomass density of converted land in simulations where no age cohorts are simulated.

Since effects of land turnover (shifting cultivation) and wood harvesting have been introduced into vegetation models, it has remained unclear what effect a distinction of age cohorts would have on simulated land use change emissions. The present paper addresses this knowledge gap and presents results from two simulations - one with age cohorts distinguished (S_{age}) and one without ($S_{ageless}$). The reduction of the land turnover component of total emissions when comparing the two is extremes (S_{age} vs. $S_{ageless}$) is 40.

This is a notable contribution to the existing literature. However, its presentation and discussion in the context of the available literature is unsatifying and some parts misleading. Moreover, the present paper has substantial overlap with Yue et al. (2017), currently under review in GMDD. These aspects should carefully be adressed in the next revision round. Below I'm listing these two major points and a few (a bit more) minor ones.

[R1] We thank the reviewer for the general positive comments and the efforts to review both papers. Please see our point-to-point responses as below. Major revised texts are tracked in the updated manuscript.

Major

- The point that the presentation and discussion of results in the context of the available literature is unsatifying echoes critique raised in the reviews of Yue et al. (2017), availble through https://www.geosci-model-dev-discuss.net/gmd- 2017-118/discussion, in particular the comment by J. Nabel. The same applies to the present paper. Particular attention should be paid to discuss results in the face of findings by Arneth et al. (2017) and to accurately describe which of the previously published models account for age cohorts within non-agricutural land and how many cohorts are distinguished. An overview table would help. Authors describe the S_{age} simulation as reflecting the "traditional approach" (l.181), implying that the age cohort distinction is itself a novelty. However, it is not. Already Shevliakova et al. (2009) distinguished multiple cohorts. Stocker et al. (2014) distinguished two cohorts (primary and secondary land). Only the model described in Reick et al. (2013) and applied by Wilkenskjeld et al. (2014) makes no distinction between age cohorts. The LPJ-GUESS model (Smith et al., 2014) explicitly tracks C pools of land patches (cohorts) subjected to stochastic disturbance. $S_{ageless}$ thus reflects an arguably extreme case and is not reflective of any "traditional approach". Having said that, an improved introduction and discussion will address this concern.

[R2] We thank the reviewer for pointing to these works and this greatly helps to expand the discussion scope of our work. The introduction and discussion sections will be revised to take account into these works. In response to the reviewer's request, an overview table on current implementations of gross land use change in DGVMs is provided in the revised GMD manuscript as we think it's more appropriate there. The overview table will be cited in the revised BG manuscript. We also invite the editor and interested readers to check the discussions of the gmd-2017-118 paper as the reviewer's comments are highly related in these two papers, so are our responses.

- My second major concern concerns the overlap with Yue et al. 2017, where the model applied here is described more extensively. Although authors only refer to their "idealized site-scale simulations" presented in Yue et al. (2017), it should be noted that also regional scale simulations, covering southern Africa, are presented therein and the main conclusion of that paper is identical to the main conclusion of the present paper - namely that accounting for age cohorts reduces the land turnover effect contribution to total LUC emissions. I raised this issue also as a reviewer for the GMDD paper and wrote:

The present paper [GMDD] was submitted on 14 May 2017. On 26 July 2017, Yue, Ciais and Li submitted a paper to Biogeosciences Discussions (https://www.biogeosciences-discuss.net/bg-2017-329/), where the same model is applied to investigate essentially the same questions, but this time at the global scale. The regional focus of the present paper on southern Africa may appear arbitrary at first, but makes sense. Apparently, authors preferred to devote a full paper to model description and evaluation and a second full paper to a global ap- plication. In my view, this is a viable way to go and the large work that went into developing this model warrants two separate papers. However, I find the delineation of their respective scope a bit unsatisfying. Readers will likely be left asking themselves why authors didn't present results from global simulations in the present (GMDD) paper - a relatively small additional step in terms of additional work. Simultaneously, readers of the BGD paper might be left wondering what the additional insight of that paper is after already the GMDD paper concluded that accounting for separate age cohorts reduces the effect of gross versus net LUC emissions.

The same issue applies vice-versa, i.e. to the present (BGD) paper. I further suggested to reinforce the value of the GMDD paper in terms of its model documentation and dissemination aspects. The present paper could for example gain in its value if the age-cohort effect is investigated not only for the two extremes (1 and 6 cohorts) but for additional numbers of cohorts, to establish a functional relationship between the number of cohorts and emissions. This would address also my previous point and would allow for a better comparison with models that distinguish between primary and secondary land (2 cohorts). Of course, this is just a suggestion, but I do encourage that the authors find a solution to finding a better delineation between their parallel submissions currently under review here and in GMDD.

[R3] In view of the reviewer's comments here, and the comments on our parallel gmd-2017-118 paper, we revised both papers to make a clearer delineation in their scopes: (1) Scopes are clearly defined in the introduction of each paper. The gmd-2017-118 paper focuses on model documentation and examination / illustration of model behaviour; the current paper focuses on model application on a global scale since 1850 and comparing simulated land use change emissions with other studies. (2) The figure on the carbon fluxes for Southern Africa in gmd-2017-118 has been removed. Only the Fig. 9 is kept there to illustrate the cohort dynamics with land use change in view of the hierarchical decision rules regarding which cohort to target during LUC in the model. (3) Model documentation is enhanced in the gmd-2017-118 paper, with dissemination aspects being strengthened. In particular, DGVMs having already implemented gross land use change have been referred to and discussed in parallel with our implementation where relevant, in response to several reviewers' comments on this aspect. (4) The reviewer raised the question of sensitivity of simulated land turnover emissions to the number of cohorts represented in the model. We think it is not the number of cohorts that matters per se, but including more than one sub-grid secondary cohorts in the model allows testing the sensitivity of emissions to the biomass (or woody mass) of forests being cleared. We tested such sensitivity for the African continent and a relationship between emissions and cleared forest biomass has been derived and included in the revised discussion section of the BG paper. (5) Following the suggestion by the 2nd review of this paper, we performed an additional S4 simulation, which includes only net land use change and wood harvest. The emissions of land turnover and wood harvest by comparing this simulation with others are discussed in the revised manuscript. This is to investigate the influence of simulations set-up on quantified land use emissions. (6) In the revised manuscript, the implication of our finding, i.e., lower emissions when taking into account age structure, is further discussed in relevance with our model implementation and the work of Arneth et al. (2017).

Minor

- Results of (residual) land sink (l.324-331) are confusing if not misleading. Au- thors find 89.2 PgC for 1959-2005 and compare this to the residual land sink from the global carbon budget (Le Quere et al., 2016). This addresses the ques- tion whether ORCHIDEE can simulate the land C sink as a result of changing environmental conditions, not anthropogenic LUC. This is a different question and out of scope for the present article. I suggest the paragraph l.324-331 to be dropped. Implications of higher LUC emissions simulated by models accounting for gross land use transitions as opposed to models simulating only net land use change are discussed by Arneth et al., 2017, where ORCHIDEE participated as well. This point should not repeated here.

  Following the reviewer's suggestion, the lines of 324-331 are removed.

- It should be discussed that decisions with respect to priority of forest age cohorts used for conversion are unknown at the global scale.

We have added citations of some studies over Europe (McGrath et al., 2015) on the rotational forest ages in forest management. We're not aware of global studies of such data. Following the reviewer's suggestion, this point is discussed in the revised manuscript.

- "Age classes for forest PFTs are distinguished in terms of woody biomass, while those for herbaceous PFTs are defined using soil carbon stock" (l.156): Discuss whether this definition is a problem when biomass and soil C stocks change in response to environmental conditions. I guess the simulated age distribution is therefore not an interpretable modelled quantity.

Indeed, the cohort boundaries defined in terms of woody biomass for forests and soil C for herbaceous vegetation types are static. For forests, biomass growth curves during the spin-up simulation with a stable early 20th century climate and the constant preindustrial CO_2 concentration are used, to delineate forest cohorts corresponding to ages since the start of spin-up. We acknowledge that using such static boundaries cannot ensure exactly the same forest aboveground biomass being cleared in the transient simulation, where environmental conditions have changed in response to anthropogenic perturbations. If we assume land managers always clear forest according to their ages, then the simulated land use emissions might be underestimated. But in general, we think the uncertainties by using static cohort boundaries should be less influential than the uncertainty brought about by the fact that — globally, rotational lengths of land turnover are poorly known and we have assumed constant, biome-specific rotation lengths. These points are discussed in the revised manuscript. Because of these uncertainties, the simulated age distribution from our simulation in this study is more considered for demonstrating the model capability rather than having solid scientific significance. It is for this reason that, even though we can get such a map, it has not been presented in the paper.

- "the land turnover resulting from the upscaling of 0.5° to 2° is not included" (l.240). This can be quite substantial. When transition maps are aggregated to a lower resolution for each transition separately, then this additional land turnover should be automatically included. How come it is not?

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 from spatial upscaling is unfortunately neglected. It can be challenging to rerun all the simulations with updated land turnover matrices due to computation limitation (because using a total number of 65 cohort functional types has tripled the time needed, in comparison 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 rebuilding the turnover matrices by including the 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.

- "Following LUH1 (Hurtt et al., 2011), we assume that no land use change occurs during the model spin-up." (l.249). See my comment in the reviews of Yue et al. (2017), availble through https://www.geosci-model-dev-discuss.net/gmd- 2017-118/discussion, regarding model spin up: *Fig. 6 [in the GMDD paper] shows that if a constant land turnover rate is applied during the transient simulation, but not during spinup, biomass C stocks attain the "wrong" equilibrium. I.e. stocks decline after being subjected to continuous land turnover to a new steady state, reached after around 50 years (under a tropical climate). Soil C stocks likely take longer to attain a new steady state and in cold climates even more so. If simulations are evaluated from the start of the transient simulation, then land-atmosphere C fluxes related to reaching this new steady state confound results. How is this treated when, for example, doing a historical simulation starting in 1850? Shouldn't a continuous land turnover pattern be applied already during spin up in order to avoid these disequilibrium fluxes?*

We agree with the reviewer that ideally, some form of land turnover processes, or shifting cultivation should have been included during the spin-up to mimic the already existing land use activities before the start year of the simulation, which in our case is 1501. Failing to account for this may lead to a spike in generated land use emissions due to a too large initial forest biomass, as pointed by the reviewer and shown in Fig. 2 of Stocker et al. 2014 as well as in the result of Hansis et al. (2015). Surprisingly, in our results of Fig. 3, $E_{LUC\ net}$ and $E_{LUC\ turnover}$ do not show such an initial large value starting from 1501, probably due to a too small implemented LUC area. Such initial large emissions do appear in $E_{LUC\ harvest}$, which results from a distinctly larger-than-zero primary forest harvest by the forcing data, consistent with the results by Stocker et al. 2014 and Hansis et al. 2015. Overall, such an impact of not including pre-spinup land turnover in simulated ELUC is negligible in our results (Fig. 2).

On the other hand, not including net land use changes (not land turnover) prior to the start of spin-up might lead to the omission of their legacy emissions, potentially balancing the effects by omitting pre-spinup land turnover. At last, in Table 3 we made the focus on comparing simulated emissions for the period of 1850–2005, which is expected being little impacted by the absence of pre-spinup land turnover.

All these points are discussed in the revised manuscript.

- Eq. 1 (l.256): Why is this decomposition defined here but no results for separated components are shown. Is Eq. 1 really necessary?

We intend to keep Eq. 1 for a clear definition of NBP in our model. For one reason, NBP can mean different things for different models depending on processes that are included in the model (for example, wood product decomposition or crop harvest). For another reason, this has provided a clear definition for readers who are not familiar with NBP definition in the DGVMs.

- l.363-375: It's important to note that harvest data used here specifies the harvested forest area. LUH alternatively provides harvested wood mass as a forcing dataset. Results presented here are subject to this choice and to the predefined priority rules (which age cohort to harvest first). According to l.172, the same priority rules are specified for land turnover and wood harvest, that is, middle-aged forest is harvested with a priority. Is this plausible? It may at least be equally plausible to assume that the oldest patch is harvested first as it has the highest biomass. In that case, the S_{age} simulation should have higher wood harvest- related emissions and the difference to $S_{ageless}$ should be small.

We agree with the reviewer assuming the oldest forest patch being harvested first will yield higher emissions. But in practice foresters tend to maintain an optimal rotation length to maximize the profit and if we know this age for different regions of the globe, then setting the primary-target cohort with such an age in the model will make sense. This is our major motivation to include the priority decision rules in wood harvest in the model. But unfortunately, the information on the contemporary and historical forest rotation length seems to be scattered in literature and no systematic compilation of such information exists. This point will be discussed in the revised manuscript.

- l.542-543: Mention here how these compare to the un-corrected values.

This will be done in the revised manuscript.

- l.611: What does "down-estimate" mean?

We mean a downward shift in the revision of emissions from shifting cultivation. This is now changed to "the extent to which emissions from shifting cultivation can be revised by a downward shift".

- l. 615 (Conclusions): "This [accounting for cohorts] will lead to a lower-than- assumed so-called residual land CO2 sink on undisturbed land, which is inferred from the net balance of emissions from fossil fuel and land use change, and CO2 sinks in the atmosphere and ocean". This is a change of a change (age cohort effects on top of gross vs. net land use change effect) and the conclusion for a lower than expected residual land sink might appear confusing after Arneth et al. (2017) concluded a likely higher-than-expected residual land sink.

As far as we know at least some models (like JSBACH and the ORCHIDEE version used there) in Arneth et al. (2017) do not account for secondary forests clearing in shifting cultivation or wood harvest. Therefore the emissions from these previously overlooked processes in DGVMs are likely overestimated there, although we agree that directional change (i.e., an upward shift in the revision of total emissions) is without any question. This point will be further clarified in the revised manuscript.