

Interactive comment on “Representing anthropogenic gross land use change, wood harvest and forest age dynamics in a global vegetation model ORCHIDEE-MICT (r4259)” by Chao Yue et al.

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The paper by Yue et al. describes the implementation of gross land use change within the ORCHIDEE Dynamic Global Vegetation Model. This implementation relies on an explicit and separate treatment of six different age cohorts of land “patches”. C dynamics are simulated separately within each patch and cohort age priority for conversion from forest to agricultural land is specified explicitly. It is shown both at the level of an individual gridcell and at the regional scale (Southern Africa) that this leads to lower LUC-related CO₂ emissions compared to a simulation where age cohorts are not distinguished in a simulation that accounts for gross land use change.

This is a substantial and very complex step in model development and improves the realism of simulations of the anthropogenic land use change. The paper provides a detailed and in some parts rather technical and model-specific description of the implementation. It convincingly shows for a single example gridcell how biomass is simulated to accumulate and transition through cohorts of different age and how it reaches a dynamic steady state under constant gross land use change regime (no expansion, constant land turnover). In that sense, one can conclude that the model works - arguably the most important statement of this paper.

[R1] We appreciate the reviewer’s efforts to review our paper and thanks for the general positive comments. The most model-specific section is probably Sect. 2.1.3, where cohort implementation has been described in detail in ORCHIDEE-MICT. This is necessary for understanding other sections. Furthermore, this could also give insights to other similar DGVMs (e.g., JSBACH, CLM) to implement similar schemes. Sec. 2.1.4, as we can argue, might seem model-specific at the first sight but actually is not — because two key model features, i.e., the necessity to introduce a priority decision rule and allocation of LUC-impacted cohort on different underlying vegetation types, can be needed as well when other DGVMs will try to implement LUC processes with vegetation demography. Thus the development presented here can be potentially useful for other similar DGVMs. We take the chance of addressing the reviewer’s comments in the paragraph below to cite relevant studies and make close comparisons when describing our model development, to make the model descriptions more relevant for other DGVMs.

The authors then go on to investigate the effect of gross versus net land use change and the effect of separating six age cohorts (versus averaging all into a single age cohort) for land use change CO₂ emissions of southern Africa. They conclude that “emissions from bi-directional land turnover alone are 35% lower in Sage than Sageless. (abstract)” and that the effect of gross versus net is to increase emissions by a factor of 2 (for “S_ageless”) and 1.5 (for “S_age”). I have some

concerns regarding the presentation of these conclusions, and regarding the scope (investigating age cohort effects) itself. One more (major) issue is regarding model spin up (see further below).

As stated by the authors (1.87-90), the present paper is not the first one to implement a model for simulating gross land use transitions. Stocker et al. (2014) and Wilkenskjeld et al. (2014) are cited. However, the authors forgot to refer to Shevliakova et al. (2009), GBC, who also implemented multiple age cohorts for simulating gross land use change. It should also be made clear that at least Shevliakova et al. (2009) and Stocker et al. (2014) (not Wilkenskjeld, as far as I am aware) did make a distinction between at least two age cohorts. Referring to “traditional approaches where a single patch is used for a given land cover type” (abstract, 1.26) and presenting results of the simulation “S_ageless” as representative for “traditional approaches” is thus a bit misleading.

[R2] Thanks for the reviewer pointing out the work of Shevliakova et al. (2009) and Stocker et al. (2014). Such expression of “traditional approaches” is now removed in the texts. The model implementations of Shevliakova et al. (2009) and Stocker et al. (2014) are now discussed closely with our implementations in the revised text where relevant.

The present paper 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 application. 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.

[R3] We greatly appreciate the reviewer's efforts to review both our papers and the holistic approach to the reviewing process. The separation of the two papers, and the arrangement of the contents for each of them, are based on several considerations: (1) It will be very lengthy to include both model developments and application in a single paper, so we decide to separate the work into two papers, with one focusing on model development description and exemplifying its application, and the other one focusing on the global application and implications for quantifying historical LUC emissions. We appreciate that the reviewer agreed on this approach. (2) The inclusion of forest demography and related cohorts is a key feature of the current paper. On top of this we implemented in the model a series of priority rules on which forest cohort to be targeted based on different LUC processes (Fig. 5). To exemplify the setting of forest cohort boundaries, and the impact of the implemented priority rule on forest demography dynamics (Fig. 9 in the original manuscript), a concrete example beyond the idealized single grid cell simulation is needed. This is the major motivation to include the southern Africa case study. (3) We argue that whether the results from the Southern Africa simulation belong to “illustrating the model behaviour” or to “scientific results” can be discussed. We tend to include Fig. 9 to show the

model behaviour as it is closely linked to the priority decision rules in LUC as explained in detail in 2.1.4. While in the bg-2017-329 paper, focus is given to the resulting LUC carbon emissions, their spatial and regional patterns and relevant comparisons with other studies. So the results in bg-2017-329 are not just a small step as argued by the reviewer, although the central message of that paper is in line with what's found by the idealized site-scale simulation in the current paper. These arguments/motivations are now included briefly in the revised texts in the introduction section.

A solution for that is to reinforce the value of the present (GMDD) paper in terms of its model documentation and dissemination aspects. Section 2.1.4. is very technical and might be too specific for the ORCHIDEE model, limiting its value for a wider readership. Code is not made publicly accessible (only upon request) and the study is therefore not reproducible. However, authors note the “clearly defined border of the LUC module”. In my view, it would be highly beneficial for the present paper to provide open access, reproducible code along with the paper. The module itself should be able to be decoupled from the rest of ORCHIDEE and some “synthetic” simulations should be possible, where land use transitions and cohorts dynamics are simulated by published parts of code. (I don't understand why this is not strictly required anyway for GMDD.)

[R4] As we described in the response to the reviewers' first comment, Sect. 2.1.4 is necessary to understand the model behaviour and we believe it can also provide insights for other DGVMs. The model codes are in principle open source but according to the policy of the lab, the access is limited to registered users. A username and password will be given upon contact on the corresponding author in order to access the code. On the other hand, the developed module is complex, so interested readers are encouraged to contact the corresponding author to get some navigation through the codes and facilitate their understanding. We are confident that after some adaptation, the codes can be migrated into other models. However, it is challenging to design the codes as fully independent, isolated and pluggable into other DGVMs readily, because to the least extent, the codes are intended to work in the ORCHIDEE code environment.

In any case, I encourage that the authors find a solution to finding a better delineation between their parallel submissions currently under review here and in BGD.

[R5] Based on our responses above (R3), and in view of the reviewer's comments on our parallel bg-2017-329 paper, we revised both papers to make a clearer delineation in their scopes: (1) scopes are clearly defined in each introduction, with the current GMD paper focusing on model documentation and examination / illustration of model behaviour and the BG-paper focusing on model application at global scale with a focus on the 20th Century. (2) Fig. 10 in the original manuscript, along with relevant method sections have been removed in the revised GMD paper, with Fig. 9 being kept focusing on cohort dynamics resulting from historical land use change that are highly relevant to our implemented cohort priority rules in the model. (3) Model documentation is enhanced, 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 the revised manuscript, in response to several reviewers' comments on this aspect. (4) We performed a series of additional simulations to investigate the sensitivity of derived land turnover emissions to the targeted cohort age and biomass in the African continent, to investigate the impact of delineating different cohorts on the

simulated land use emissions. But this result is included in the BG-paper as we think it's more relevant there. (5) In the BG-paper, 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 Arneeth et al. (2017).

Regarding model spin up: Fig. 6 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?

[R6] We agree with the reviewer that ideally some form of land turnover should be included in the spin-up runs. Likewise, natural stand-replacing disturbances should be included as well. However, unfortunately, neither of them is included in our current simulation. This point is acknowledged in the revised BG-paper where LUC emissions are examined in more details. On the other hand, as we start our simulation from Year 1501 in BG paper, LUC emissions from 1850 are unlikely impacted by a lack of land turnover in the spin-up and the estimations can still hold. This latter point is also discussed in the revised BG-paper.

MINOR:

* 1.61: . . . emissions *of CO2* (Houghton et al., 1999) . . .

Done.

* 1. 67 “Given the importance of historical LUC emissions and its large uncertainty, a more realistic representation of LUC processes and land management in DGVMs is desirable”.
Improving realism rarely reduces uncertainty (~model spread).

We agree on this. We changed the original sentence to “*Given the importance of historical LUC emissions and the necessity for their better understanding in order for a better idea of the future land-based mitigation potential, a more realistic representation of LUC processes and land management in DGVMs is desirable.*”

* The net-versus-gross LUC question is introduced only on 1.73 - in my view too late. Preceding paragraphs detract attention from the questions at hand here.

We have shortened the 1st and 2nd paragraph. We believe the lengths of these two paragraphs after revision provide a minimum scientific background of land-use change emissions before diving into the more technical description of gross versus net land use change.

* As pointed out on 1.95, accounting for gross land use change is relevant more generally for appropriately simulating sub-grid scale bi-directional land use transitions and is not only relevant

in shifting cultivation agriculture. The term shifting cultivation refers to a specific form of smallholder agriculture and doesn't encompass all sub-grid scale bi-directional land use transitions. I am aware that the previous literature on modelling these effects used the terms shifting cultivation and gross land use change (or land turnover) more or less interchangeably. Maybe worth stating here (in introduction) what shifting cultivation actually is (see Heinemann et al., 2017, PLOSOne: <https://doi.org/10.1371/journal.pone.0184479>).

We inserted a brief definition of shifting cultivation in this paragraph following the suggestion by the reviewer. Heinemann et al. 2017 is cited as well. The inserted texts are: *“A typical example is shifting cultivation, a form of smallholder subsistence agriculture primarily occurring in tropical regions that involves clearing a forest for a non-permanent cropland, which is often abandoned later. Shifting cultivation was historically important in many tropical regions for the subsistence of indigenous people (Hurt et al., 2006; Lanly, 1985) although more recently it has been in the process of being superseded by more intensified land management (Heinemann et al., 2017).”*

* 1.105: reference for “dilution approach”?*

This term occurs in the ORCHIDEE documentation but cannot be found in literature. Thus it is removed from the revised texts.

1.122 “r3247”: Better use SVN tags than SVN version numbers for reference.

Following the reviewer's suggestion, the code has been tagged as ORCHIDEE-MICT v8.4.2. The title is also changed accordingly.

* 1. 277 (Eq.1):

The reviewer may have some comments here but it seems they did not show up.

* Fig. 6: Very nice plot! Would be very informative to have a curve for total biomass across all cohorts in the simulation with age distinction (to make it comparable to the black curve for $S_{ageless}$).

Biomass carbon stocks for all cohorts with age distinction, and for the single forest patch in the $S_{ageless}$ simulation are all based on per unit area of forest. The different cohorts in S_{age} simulation are spatially distinct or separated so it does not make sense to add them together.

* Fig. 7: Is this figure referenced in the text? Where?

Yes. Fig. 7a and 7b are discussed in section 3.1.2 in the text.