

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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Received and published: 19 September 2017

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.

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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 unsatisfying 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 addressed in the next revision round. Below I'm listing these two major points and a few (a bit more) minor ones.

Major

- The point that the presentation and discussion of results in the context of the available literature is unsatisfying echoes critique raised in the reviews of Yue et al. (2017), available 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-agricultural land and how many cohorts are distinguished. An overview table would help. Au-

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thors 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.

- 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 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 ask-

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ing 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.

Minor

- Results of (residual) land sink (l.324-331) are confusing if not misleading. Authors 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 question 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

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well. This point should not be repeated here.

- It should be discussed that decisions with respect to priority of forest age cohorts used for conversion are unknown at the global scale.
- “Age classes for forest PFTs are distinguished in terms of woody biomass, while those for herbaceous PFTs are defined using soil carbon stock” (I.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.
- “the land turnover resulting from the upscaling of 0.5° to 2° is not included” (I.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?
- “Following LUH1 (Hurtt et al., 2011), we assume that no land use change occurs during the model spin-up.” (I.249). See my comment in the reviews of Yue et al. (2017), available 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?

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- Eq. 1 (I.256): Why is this decomposition defined here but no results for separated components are shown. Is Eq. 1 really necessary?
- I.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 I.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.
- I.542-543: Mention here how these compare to the un-corrected values.
- I.611: What does “down-estimate” mean?
- I. 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 Arneeth et al. (2017) concluded a likely higher-than-expected residual land sink.

Interactive comment on Biogeosciences Discuss., <https://doi.org/10.5194/bg-2017-329>, 2017.

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