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Geoscientific Model Development Discussions

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 dis-

tinguished 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-speciffic 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.

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 (l.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, l.26) and presenting results of the simulation "S_ageless" as representative for "traditional approaches" is thus a bit misleading.

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.

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.)

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.

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?

MINOR:

* l.61: . . . emissions *of CO2* (Houghton et al., 1999) . . .

* l. 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).

* The net-versus-gross LUC question is introduced only on l.73 - in my view too late. Preceeding paragraphs detract attention from the questions at hand here.

* As pointed out on l.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 small-holder 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 Heinimann et al., 2017, PLOSOne: https://doi.org/10.1371/journal.pone.0184479).

* l.105: reference for "dilution approach"?

* l.122 "r3247": Better use SVN tags than SVN version numbers for reference.

* l. 277 (Eq.1):

* 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).

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

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