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.

Anonymous Referee #1

Received and published: 1 August 2017

General Comment:

This study touches on the issue of the representation of shifting cultivation in the dynamic vegetation model. The new model features including a better description on PFTs (plant function types) demography, wood harvest and shifting cultivation at a sub-grid scale. The behavior of the enhanced model was tested both at a small scale and at a regional scale over an old growth forest (Miombo/dry woodlands) in South Africa. The model result shows that the new development has a robust representation of shifting cultivation during a long-term simulation period and the carbon emission due to the land use change has been underestimated without the consideration of gross land use change (including shifting cultivation, age class PFT and wood harvest). The most important term for this net emission is contributed from the biomass burning due to shifting cultivation activities (the F_{Inst} term in Eq. (3)). The manuscript was written in a good shape with a detail model description and its experimental design, and the new model feature opens the opportunities for the scientific community to study the research issue such as the effects of shifting cultivation between different biomes on the climate from different soil types and climate zones.

We appreciate the reviewer’s efforts to review our paper. Please see our point-to-point response as below. All the revised texts in response to the reviewer’s request are tracked in the updated manuscript.

Specific Comment:

I suggest the authors to provide a more detailed description and adequate reference of each term in the Eq. (3), which are the crucial parts of mathematical representation for the biophysical/chemical processes. For example, the “F_{HR}” term is often parameterised as function of surface temperature, and it also could be parameterised as function both of surface temperature and soil moisture (Chang et al. 2008). In the view of result presented by the authors, "F_{Inst}” term is the major source of the net CO2 emission from the shifting cultivation between forests and croplands. I would also like to understand the sensitivity of this term to the state variables, such as soil temperature, soil carbon stock and ect. in the model.

Reference:Chang, S.-C., K.-H. Tseng, Y.-J. Hsia, C.-P. Wang, and J.-T. Wu. 2008. Soil respiration in a subtropical montane cloud forest in Taiwan. Agric. Forest Meteorol. 148: 788-798

All the terms in Eq. (3) are now explained in more details. Further references are provided when necessary. F_{Inst} represents the instant carbon fluxes to the atmosphere in forest clearing and is determined in the model on an annual time scale. It depends only on the wood mass of the forests being cleared and not directly on soil status including the temperature and moisture. This is now explained clearly in the revised texts. We added the following texts in Sect. 2.1.5: "*Carbon in the two wood product pools is then released into the atmosphere according to their respective turnover times, and this flux contributes to the overall land carbon balance as a source term (see the next section).*", "*Agricultural harvest and associated fluxes to the atmosphere through food consumption or livestock feeding are assumed to happen locally in the model, without considering spatial relocation by international trade*.". We added the following texts in Sect. 2.2.1: "*F_{Inst} and F_{Wood} are both fluxes on an annual time scale that depend only on wood mass at the time of forest clearing and the respective wood product degradation rates (see Sect. 2.1.5). FHR is simulated at a time step of 30 minutes and depend on soil temperature and moisture. F_{Fire} is simulated with a prognostic fire module SPITFIRE (Yue et al., 2015).*"

In this paragraph (P8L241-L245), I was confused about the description of the recruitment in a forest. Does the natural recruitment in a forest increase the original forest cover fraction (Diluted the carbon stock)? Or, the forest cover fraction is always fixed and the recruitment only increases the carbon stock.

We apologize for this confusion in the original text. The focus here is to describe how forest cover fractions are handled in the process of natural mortality and recruitment, as our paper focuses on land cover change representation in the model. Natural recruitment from regeneration in a forest does not increase the original forest cover fraction. It does not either dilute the existing carbon stock (here, the original texts are inaccurate in its description). Instead, recruitment increases individual density and renews part of leaves (by updating leaf age composition in the model). The recruited sapling biomass is incorporated into the existing biomass only when the latter is virtually zero while a larger-than-zero ground fraction is prescribed. We revised the relevant texts in the paper as below and hope it is clearer (Sect. 2.1.3):

Natural forest mortality in ORCHIDEE could be either prescribed as a constant rate or dynamically simulated, but mortality takes effects by reducing the amount of existing biomass only, with the coverage of the concerned forest patch being unchanged. Likewise, recruitment increases forest individual density and update leaf age composition and other relevant variables, but again, forest coverage remains unchanged. These features are necessary, as the original ORCHIDEE model does not take into account forest demography. As explained in Krinner et al. (2015, page 8), recruitment sapling biomass is only incorporated when the existing biomasses is virtually zero while a larger-than-zero ground coverage is prescribed. These features remain the same in the case of with sub-grid cohorts, i.e., forest mortality or natural recruitment does not modify forest cohort ground coverage. In addition, forest mortality and subsequent regeneration due to forest fires are handled in a similar manner.

The author choose a dry woodland as an example to demonstrate the model behavior of shifting cultivation at a dry and warm climate zone. Regarding to the design of the land surface model (ORCHIDEE) is for a large scale study, I think it would be able to apply this new feature for a tropical peat land forest and the model behavior should be also welcome and interesting for the readers in the Earth System Modeling community.

We agree with the reviewer that forest clearing in tropical peat land forest can be an interesting case to apply our model. In a companion paper (https://www.biogeosciences-discuss.net/bg-2017-329/) where we apply our model to investigate historical land-use change carbon emissions from shifting cultivation, there are some shifting cultivation activities in tropical Asia being included. However, the hydrological impacts on carbon due to land use change on peat-land forest must have not been adequately represented mainly because peat-land-related hydrological process and soil processes are not represented in the model version used here. There is a parallel model development in ORCHIDEE aiming for including peat land process (https://www.geosci-model-dev-discuss.net/gmd-2017-155/). In the future, these developments could be integrated for a more sensible representation of peat land-related land use change.

Technical Comment:

P2L59: the definition of “M” 10^6(million) or10^9(mega)?

It means 10^6 (million). This is indicated in the revised manuscript.

P2L65: reference of “Hansis et al. 2015” is missing the reference list

Done.

P4L110: Some recent developments. . ., please cite more references

In response to the comments by other reviewers as well, we have added an overview table of DGVMs having implemented gross land use change, and more references are added in the introduction section.

P5L158: ...”Fig 1d”... to ...”Fig. 1d”...

Done.

P8L239: . . .are properly defined. Please explain how to define the criteria for the cohort thresholds.

This has been explained in detail in Sect. 2.2.3 in the original manuscript. To not increase the manuscript length by making repeats, the section 2.2.3 is now cited in the Sect. 2.1.3.

P9L279: the index i, j have been already used. It should be replaced by another indices, such as k, l.

We argue that it is convenient and an implicitly agreed practice to denote an element of a matrix M as M_{ij}. In our case we suppose readers can easily distinguish that here the indices i,j are different from the ones used before in Sect. 2.1.3. So this notation is maintained.

P13L395, L404: The description of F_{Fire} for Eq. (3) is missed.

It is explained in the revised texts, in response as well to the first specific comment by the reviewer.

P13L414: ...”simulations and Le Quere et al. (2016)”... I suggest to rephrase it to . . .”simulations and the existing global carbon budget dataset (Le Quere et al., 2016)”.

Done.

P15L473-L474: six CFTs but only five ages (3, 9, 15, 30, 50) in the text

The last cohort ($Cohort_6$) corresponds to the mature or primary forest and therefore its age (i.e., years) is not given as an exact number. To remove the potential confusion, we denote the age of $Cohort_6$ as >50 years in the revised manuscript.

P15L481: the reason for choosing 65%.

This value here is chosen tentatively and more for a demonstration purpose. 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 demonstrating purpose rather than having solid scientific significance. This is primarily due to the fact that soil moisture is simulated in the model on the basis of water columns, and soil temperature over the whole grid cell rather than cohorts, as explained in the text (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 and parameterization has to be improved in the future model application. We inserted at the end of Sect. 2.2.3 the following sentences to clarify this: “*Overall, this feature of separating herbaceous MTCs into multiple cohorts is coded more as a “place holder” for the current stage of model development rather than having solid scientific significance. To fully track soil carbon stocks of different vegetation types and their transient changes following land use change, a much larger number of cohorts are needed. But for a global application, this is limited by the computation efficiency*.”

P19L599: The Fig. 9 sub-index for “b” can’t find the Figure 9. Please revise it for the consistence between the context and figure.

Fig. 9 is now revised.

P21L667-L669: Please give an example for the possible missing process in the land use change.

The example is given in the original text, e.g., gross land use change.

P22L702: The citation of “Hurtt et al. 2016” is not in the reference list.

This is corrected.

P22L711: Typo: . . .O”R”CHIDEE-MICT. . .

Thanks for pointing this out. We apologize for this typo. It has been corrected.

P22L723: . . .”is need to streamline land use”. . . This is a bad English structure. I would recommend to rephrase it as . . .”is needed to streamlining to land use”. . .

We change this sentence to: *But in general and over a long term, land use or land management decisions need to be integrated directly into DGVMs.*

P23L734-L736: It is a sentence with a bad English structure. Please rephrase it.

We modified these two sentences to make them more concrete: *These developments also make it possible to verify modeled global and regional forest age distribution using independent age information from either forest inventory or remote sensing. The model version used here has incorporated the developments in pasture and cropland modules (Chang et al., 2015; Wang et al., 2017). On a regional scale such as Europe, where the comprehensive forcing data are available, it is possible to go beyond the carbon emissions only by LUC activities, but also to include LUC-induced changes in emissions of other greenhouse gases such as methane and nitrogen oxide.*

P33L989: Add a line for “S_{age}” simulation. I was confused about the zero cover fraction for both Cohort4 and Cohort5. For a 100 year simulation, the Cohort4 and Cohort5 supposed to have dynamic changes in the cover fraction. Pease explain the zero cover fraction for Cohort4 and Cohort5 in the main text.

The S_{age} simulation is shown as each individual cohort from $Cohort_1$ to $Cohort_6$. This is now explained more clearly in the revised figure caption. As this figure shows a simulation of an annual forest-cropland turnover of 5% of grid cell area and the clearing of forest targets primarily on $Cohort_3$, this cohort has been converted to cropland before having the chance to move to $Cohort_4$. This explains the zero fractions of $Cohort_4$ and $Cohort_5$. This point is also explained in the revised text.

P36L1014: Please check the label of the Fig.9. sub-label “b” is missed.

This has been corrected.