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Geoscientific Model Development Discussions

Interactive comment on "Ecosystem age-class dynamics and distribution in the LPJ-wsl v2.0 global ecosystem model" *by* Leonardo Calle and Benjamin Poulter

Anonymous Referee #1

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General comments

Disturbance history and disturbance regime are important drivers of terrestrial biosphere dynamics and ecosystem function, but they are rarely represented in dynamic global vegetation models. Here Calle and Poulter describe their age-class implementation in the LPJ model (LPJ-wsl v2.0), and present a series of simulations seeking to highlight the effects of disturbance history on vegetation structure and the carbon cycle, as well as the global patterns of ecosystem age when accounting for fire and land cover and land use disturbances. This work provides an important model development and can become an important contribution to the modelling community, once

some issues, which I describe below, are addressed by the authors.

The current model description provides an overview of the age structure in LPJ-wsl and includes some examples on how this module works (Figures 1 and 2). However, some mechanisms are not sufficiently described and deserve attention, especially in a journal like GMD. For example, in section 2.2.1, I could not tell how each within age-class element ($f_{i,j}$) is represented in the model: are they treated as "independent" components (i.e., available soil water and light computed independently for each within age-class element), or do all the elements in the same age class share the resources? Also, how do the age-width transitions work in the case of unequal age classes, considering that the age class transitions occur once a year? Does that mean that young age classes have fewer elements, or are multiple elements allowed to transition to another age class at the annual time step? These are mostly points for clarification and should be straightforward to address in a revised version.

The authors compare the effect of some model settings (e.g., enabling vs. disabling the age structure module), but no benchmarking is provided other than the comparison of the predicted forest structures with FIA plots. Consequently, several processes were not truly evaluated against observations or at least reported values in the literature. For example, when the authors compare the simulations with and without age-class dynamics (Figures 5, 6 and text referring to them), it is implied that the age-structure simulations are more reasonable, but the authors do not provide any reference to observations. Although the simulations are idealized, some values from literature could at least indicate whether the time scale for recovery is at least in the right order of magnitude at different biomes.

Finally, the fire disturbance is presented as the critical determinant of forest age distribution, but no assessment of the fire module is provided. I understand and agree with the authors that fire datasets such as GFED will include fire types currently not represented in LPJ-wsl and a comparison of carbon emissions is not possible due to the risk of double counting, but they could be still useful for verifying whether spatial distribution and the inter-annual variability of fire disturbance predicted by LPJ-wsl is reasonable or not.

Specific comments

L58. Re-write this sentence, so it is clear that some models do account for demographic effects, including a few that were cited in the previous sentence.

L94. The authors mention permafrost and wetland methane but these features are not described anywhere. Considering that these are features in the new code, shouldn't they be described somewhere?

L132. This is a good and clear explanation, but I wonder if the authors could also highlight the consequences of adding age-classes to the representation of the micro-environments in LPJ-wsl (light, water and perhaps nutrient availability). Also, was there any reason why natural disturbances (e.g., tree fall) cannot create new age classes?

L140–155. This is not entirely accurate. In some cohort-based models, a patch represents a collection of gaps with similar forest structure. In such models, fusing patches that have similar structure simply means that the structures of patch A and patch B are sufficiently similar so that the merged patch can represent all gaps in A and B (and thus representative of a larger area). At least for ED2, the patch fusion is not determined by one state variable as implied in the text, but by the vertical LAI profile (Fisher et al. 2018).

Section 2.2.2. I understand that the fire model has been previously described, but more detail would help here, as fires are critical for the results shown later in the paper. Instead of describing the model qualitatively, the authors could provide the basic equations and also a table with the PFT-specific fire resistances (SI text and table would be fine).

L219–221. Presumably the fractional area abandoned/logged goes entirely to the youngest element within the youngest age class ($f_{0,0}$, following your notation in Eq. 4), is this correct? Clarify. Also, does it mean that the model assumes that all recently disturbed areas have similar structure of survivors? This may be fine for abandoned and clear-cut logging, but not very appropriate for fires and selective logging.

Section 3.1. Are there allometric equations that relates carbon stocks, vegetation height and stem number density in LPJ? I wonder if this could explain the consistently lower stem densities, and if the biomass distribution across size would look more/less similar to the plot data.

Section 3.1.2. I may be missing something here, but I cannot see which ecological processes are affected by choosing equal or unequal age classes. It almost reads like the only difference between the two simulations is how results are reported, please clarify the mechanistic differences between the two approaches. Also, as a point for discussion, it would be nice if the authors provided some insight of which approach is recommended.

L436–440. These results are a bit expected because recently disturbed patches are more dynamic, so having finer bins for young age-classes makes sense to me. But it is also unclear is the effect of different binning strategies on the final results.

Section 3.2.2. Is a recovery of NEP in 5–6 years more reasonable than 30 years? I don't see why, this needs some independent evidence from observations. Also, some clarification is needed to explain why Rh is consistently higher in the no-age simulation. Shouldn't the stand-scale mortality (and turnover) be the same in both cases, and the only difference be how mortality (and turnover) are applied?

L518–519. I agree with the authors on the need of more targeted simulation experiments, but if some of the variables mentioned are available from the LPJ-wsl output, then the authors could check the results to see if some hypothesized mechanisms could be ruled out.

L647. This would account for only part of the uncertainty. Parameter and process uncertainty in most models can be quite large.

L688–690. It may be worth mentioning that this size distribution may vary across regions (e.g., Espírito-Santo et al., 2014) and even within region depending on abiotic factors (e.g., Asner et al. 2013 which the authors already cite).

L700. It makes sense to end the text with a paragraph about future developments, but the current one is vague. Which specific features could be implemented and which ones should be priority?

Minor comments

L23. Explicitly say which latitudinal band has the lower age.

L24. Land use change and land management were. . .

L25. Does -21 yr correspond to both temperate and tropical areas? Clarify.

L81. “is” instead of “was”?

L98. This sentence could be dropped, considering that version control software has been around for a very long time.

L125. I don’t see a strong reason to use both patch and age-class throughout the text. It makes sense to keep the explanation here but use a single term thereafter.

Eq. 4. Isn’t the $f_{w,n}(t+1)$ term a form of fusion? I guess this depends on how independent the different elements within age-class are.

L175–187. Is there any reason why some of the fractional areas are fw,n and others are Fw,n? If not, then use a single notation. Also, in Eq. (5), is it correct to say that $F'_{\mathrm{total}_j}(t) = F\mathrm{total}_j(t) - f_{w,j}(t)$?

L202. Rewrite this sentence. Conceptually yes, the approach does seem to avoid dilution, but no example from actual model simulations was provided. Also showing that this approach works in LPJ-wsl is different than saying that the age-class/age-width approach solves the dilution issue. I am not even sure this is an issue with other models or the default LPJ, are there examples of this happening from the literature or in other LPJ simulations that the authors carried out?

L223. “to” instead of “->”

L233–235. This assumption seems counter-intuitive at least in the tropics, where young secondary forests have high deforestation rates (e.g., Nunes et al. 2020; Wang et al. 2020).

L235. At least for me, this seems the opposite of a conservative estimate.

L262. “were” instead of “was”

L275. Because readers may not be familiar with FIA plots, include the total plot area and the minimum DBH measured over the entire plot area. Also add the metric equivalents for all diameter references.

L293. Is the 5% based on any real mechanism?

L306. “Data” instead of “Date”

L375. This seems a software-specific remark, mention and cite the software.

L434. Clarify this text. What is the field-based evidence, and whether the results are consistent with the evidence in a quantitative or qualitative manner (from reading the text it looks like it is the latter).

L477. What are the differences in GPP?

L484. “(?), perhaps not” is confusing.

L489. Isn’t it possible to retrieve the soil moisture as a function of age from the LPJ-wsl output? I had understood that soil moisture was solved independently for each age class.

L493. True, but the apparent large difference for other terms may be just because the scales for most variables do not go to zero in Figure 6. In relative terms they may be comparable to the changes in NEE.

L518. “drier” instead of “dryer”.

L549. The central South America looks as strong as the central USA.

L593. Figure 13 could be described in the Results section, and in more detail.

L610. Including age dynamics is important, but this is not a novel concept, so it would be nice to put this paragraph into perspective with previous efforts.

Fig. 2. In case B, shouldn’t 0.25 be in the 2nd row of the 3rd column, with a zero at the 1st row? Also, can logging be applied to other age-classes or just the last one? If multiple classes can be disturbed, then it may be worth showing such example too (or replacing the single-patch disturbance with a multi-patch disturbance example).

Fig. 4. It would be interesting to compare these trajectories for the two age-class approaches (equal bins, unequal bins).

Fig. 9. These results are a bit surprising given that boreal forests burn frequently. Could this be caused by the zonal averaging, which puts drylands and savannas together with low-disturbance forests in tropical and temperate zones (but not so much in the boreal zone)?

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