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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 #2

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The manuscript by Calle and Poulter investigates age-class dynamics as simulated with a dynamic global vegetation model (DGVM) called LPJ-wsl 2.0, a model developed based on the DGVM LPJ. Some aspects of this model are described in the methods, including those which were newly introduced to work with age-classes. The core of the paper seems to be a set of factorial simulations on different spatial scales used to investigate age-class dynamics together with their effect on the simulated carbon fluxes. In addition, the authors assess the contribution of the two types of modelled disturbances (fire vs land use) on forest age structure and derive a generalised linear model to predict carbon fluxes from temperature, precipitation and age-class. The latter is then used to map the "effective range" of each of the predictors to identify regions

with significant contribution of demography. I find the manuscript interesting and timely, because forest age structures are an important aspect of the (anthropogenically) disturbed terrestrial biosphere, particularly with respect to the role of land use in climate change mitigation scenarios, and since forest age structures are still underrepresented in DGVMs. In my opinion, however, several aspects of the paper need careful revisions. In particular, the main aim of the paper did not become apparent to me (see general comments below).

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General comments:

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1. The main aim of the paper is unclear to me and so is what the new aspects are (i.e. the gap mentioned in the abstract l.15 and in the last paragraph of the introduction ∼l.81). Is the paper supposed to be a) a model development paper, i.e. describing LPJ-wsl v2.0 or describing the implementation of age-classes in LPJ-wsl v2.0? Or is the paper b) the investigation of the simulated demographic effects? While I find detailed descriptions of models / new model development important and a legitimate scientific contribution, in my opinion, there would still be quite a bit information missing if a) would be the purpose of the paper. To me it especially did not get clear, what has been the new development and what was there before (particularly in subsection 2.2.2 – is this all new or are parts from LPJ-wsl v1.0 or even LPJ?). One aspect that could help to clarify this would be a consequent use of "LPJ-wsl v1.0" vs "LPJ-wsl v2.0" (vs LPJ) highlighting the "modifications for integration with age-classes" (l.118). (Furthermore, there are currently several occurrences of only "LPJ" which probably should be called LPJ-wsl 2.0 (e.g. Table 1, Supplementary, results section)). In addition to having clear model version references, some reordering could help, e.g. moving LPJ-wsl v1.0/LPJ aspects to 2.1.2, such as probably most aspects of fire, primary and secondary/managed forest, LUH2 driver, emissions and residues, product pools, etc.

This could, by the way, also solve the sudden occurrence of primary and secondary tiles (l.215) and the unexplained "land use" in Figure 1. From the current structure of the paper I tend to assume that b) is the main purpose / the new aspect. In this case – but to some degree this also holds for case a) – I would expect some form of comparison to observational based data, particularly for the global simulation for which the authors derive the role of demography in the global carbon cycle. On one hand, I would expect some kind of comparison of the global simulation with and without age-classes to e.g. a GPP or better AGB dataset to get a feeling for the relevance of the finding of a 40 PgC increase in turnover, and, on the other hand a comparison to a global age map, especially since one of the authors recently published such a map (Poulter et al., 2018). The comparison to a global age map could particularly be instructive to learn where the model fails to reproduce age-structures from the observational based dataset and to discuss why this might be the case (e.g. missing disturbances vs. issues with the fire algorithm or as I expect also issues with the LUH2 data – could be included e.g. in 3.3.2 and 4.1).

2. I had some problems with the way the matrix notation is presented. In general, I found the matrix description a good idea, since it quite nicely visualizes what happens upon ageing and particularly which fractions are merged into the next age-class. My critique, however, is that this is not what has been done in the code and that it also does not suit any of the two age-class setups applied in the study (Table 1). I would therefore recommend to clearly state that this is the theoretical idea, which neither suits the applied age-class setups (because they both contain unequal age widths) nor is what has been implemented in the code. Furthermore, I would appreciate a paragraph on how the age tracking is actually realised in the code.

3. The authors state that the simulated age structures are an "upper limit of age-class distributions" due to not represented disturbances (e.g. l.38, l.593) and that the study overestimates ecosystem age (l.606). However, couldn't the simulated disturbances (fire, harvest and land-use changes) also be too strong in some places? Es-

pecially with l.233-239 stating that "deforestation always occurs in the ranking of oldest to youngest age-classes... typically resulting in greater land-to-atmosphere fluxes"? Could this lead to too young forests as well as to an overestimation of historical fluxes? For the conclusions drawn in this manuscript, two additional scenarios could maybe be valuable: a) a simulation with deforestation occurring in the ranking from younger to older age-classes and b) a "best guess" simulation using the LUH2 harvest categories "harvest from secondary young forest" and "harvest from secondary mature forest". Another thought: Have the authors considered conducting the fully fledged global simulation also with the unequal bin setup? Would this lead to different results?

4. Literature work: In some of the sections I had the feeling that more references / locating the paper in context of the existing literature would be appropriate.

a) First of all I wondered if LPJ-wsl v2.0 is the model on which the publication of Pugh et al. (2019a) is based? In this case this should in my opinion clearly be stated in the paper.

b) Looking in the literature for occurrences of LPJ-wsl I found several publications that had at least short model descriptions and I wonder why none of these is referred to in the manuscript (e.g. Poulter et al, 2015; Zhang et al. 2017,2018)?

c) In section 2.2.1 l.142 when introducing the VTFT approach, the authors point to the paper by Nabel et al. (2019) having a similar independently conceived approach. Indeed, it seems as if many of the aspects described in 2.2.1 are similar to those described in Nabel et al. (2019), including the tracking of fractions per year and the merging process: merging of disturbed areas into the youngest age-class and merging of aging fractions exceeding the width of the age-class into the next age-class. Each with subsequent area-weighted averaging of carbon with the transitioning fractions. To a certain degree similarities seem to also hold for the applied age-class setups. While I truly believe that this approach has been independently conceived, I would still recommend relating to the existing approach, e.g. pointing out similarities and in

particular also differences.

d) In the discussion section I would highly recommend to conduct a bit more literature work. Several studies have already been conducted with global models that do include age-classes and I would strongly recommend having a look what is out there to compare with and refer to. For example, regarding the statements around l.625 please have a look into Yue et al. (2018) (but also e.g. Shevliakova et al., 2009). Overall, l.55-57 include quite a lot of references to models that account for demography in one way or the other but hardly any of these models occur in the discussion section and particularly e.g. statements in ∼671 are probably true for some of these models.

5. I would recommend clearly stating when simulation output is referred to as opposed to observational based data (e.g. l.20, l.32-33).

6. It did not become clear to me what exactly is compared in 2.3.2 and 3.1: Are these simulation results from a global simulation? From which? Sage? But if from Sage, why are the FIA data with disturbance, stocking or longing excluded?

7. Figure 3 and 4: I would appreciate to also have Figure 4 for the 10-year age-widths, since this is what is used in the global simulations. Also, could for ease of readability maybe all panels with unequal age-widths start with the youngest age-class? Furthermore, it might increase comparability when changing the x-axis to show linearly increasing years instead of the classes and then to place the boxes for the different age-classes at age-class mean ages. This would particularly underline the differences in the NEP dynamics among the different age-class setups. Even more so, if the two age-class setups would be integrated in one plot/panel for each of the depicted variables instead of having separate panels with differing x-axis.

8. Is there a recommendation/conclusion on what age-class setup to use based on the studied simulations? I.e. when would a simulation with unequal bins be preferable, when with equal bins or the like?

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Specific comments/ Technical corrections:

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- Mixed usage of hyphens: grid cell, grid-cell, gridcell; age-widths, age widths; age class, age-class; land use change -> land-use change; land use transitions -> land-use transitions; land-use -> land use, . . .

l.13 “most global ecosystem models” – consider changing to “many” acknowledging the considerable list in l.55-57.

l.15 Could you specify which gap exactly?

l.18 Could you maybe make this sentence a bit more precise? Could it also be fractions of an age-class which experience a stand-clearing disturbance? The simulated stand-clearing disturbance is fire, and the prescribed ones are harvest and abandonment of agricultural area?

l.20 “that patterns of ecosystem function” -> simulated patterns? Patterns resulting in/from model simulations?

l.24 land-use change

l.25 “an additional” –in the sentence before, with regards to fire, only the difference between boreal and tropical lats is given, maybe you could give the absolute effect there, too?

l.25 “-21 years in temperate (23N-50N) and tropical latitudes” are these analysed together or independently but experience both the same decrease in age through land-use change and land management?

l.32-33 please clarify what kind of “Spatial datasets are provided for global ecosystem age” simulated ones? Do these stem from the ‘fully-fledged’ LPJ-wsl v2.0 simulations?

l.35 “40-Pg C” -> “40 Pg C”

l.35-36 A 40 Pg C increase over which time period?

l.38 “upper limit” – what do you mean with upper limit? That the forest will not be younger? Please consider rephrasing/explaining. And couldn’t the modelled disturbances (fire, harvest and land-use changes) also be too strong in some grid-cells leading to forest which is too young (particularly due to the applied old forest first rule; see also general comments)?

l.41-45 this seems to rather be an enumeration than a sentence and pretty long, could it maybe be taken apart and rephrased?

l.49-52 This sentence seems imprecise to me: From which of the publication exactly do the $\sim$60% total sink stem from? Over which time period? What are the time periods for which Pan et al. 2011b and Pugh et al. 2019a report/estimate the specified sinks, respectively? Is this in combination with changes in environmental forcings?

l.50 Pan et al. 2011b not 2011a according to the references?

l.51 really 0.3 to 1.1 PgCyr-1?

l.51 When I understood it correctly than the findings in Pugh et al. 2019a are mainly build on exactly the model being described in this study? In this case I would find the line of argumentation circular, in-transparent and therefore somehow scientifically concerning.

l.54 why is fire listed separately of “disturbances”?

l.60 but have a look at e.g. Zaehle et al. (2006) or Bellassen et al. (2010)

l.64-65: Unfortunately, I cannot find this order in Frolking et al. (2009). In section 3.1 in Frolking et al. (2009) globally disturbed fire area is largest ($\sim$3 $\times$ 106 km2 a$-$1) but only 1 $\times$ 105 km2 a$-$1 in forest – which is equal to that estimated for wind ($\sim$1 $\times$ 105 km2 a$-$1), while global estimates for wood harvest and shifting cultivation are larger

–each $\sim$1–2 $\times$ 105 km2 a$-$1 of forest area.

l.76-79: Please clarify: why forest management here – elsewhere land-use change and land management?

l.81 Could you specify which gap exactly? Else maybe omit this phrase?

l.81-83: Note: Several of the studies listed in l.55-57 have demonstrated that a representation of demography influences ecosystem stocks and/or fluxes.

l.85 is there any more recent reference than Sitch et al. 2003 (maybe Poulter et al, 2015; Zhang et al. 2017,2018)? Or maybe rephrase e.g. “a model building/based on the Lund...”?

l.110 are?

l.115 before and elsewhere in the text I understood that fire is also implemented as a stand replacing disturbance/ burned fraction moves to youngest age-class?

l.130 “unequalbin setup is applied to explore model dynamics at the level of a single grid-cell;” according to Table2 its not a single grid-cell but region, which is also suggested by e.g. Fig.4.

l.127-131: I would appreciate a bit more information on and explanation of the choices that drove the separation in age-classes. Particularly, why is the cut off with 151years in the 10-yr equal bins and why is it with 101years in the unequal bins? Why is the age range of the pre-last class (code 11) in the 10-yr equal bin larger – making it an “unequal bin”, too. Maybe also the motivation for the 2, 5 and 25 year ranges as well as the switches between these ranges could shortly be outlined? If this resulted e.g. from preliminary tests, the experiences of the authors could maybe be instructive to the readers.

l.146 . . . number “of” simulated . . .

l.161 I would recommend to introduce a j on the w to indicate that the age-classes (can)

have different widths.

EQ4 and l.173 personally I find f0,0 an unlucky choice and would prefer an extra term, such as fdis or the like

EQ5 and l.179 why a capital F in F(t)_w,j-1, isn't this just one entry?

l.192-199: is this an enumeration? If so, could it maybe be separated with newlines? Else I would appreciate complete sentences.

l.202 to which age-widths does this refer to, those from the unequal setup or both setups? Is there a specific section of the manuscript where "it is demonstrated" or is this a more general statement as "in this study"?

l.206 and 220 "merged with a youngest" -> the? Or can there be several youngest?

l.213 I do not understand this, why can't the not burning fraction stay in the current age-class/patch and only the burned fraction move to the youngest age-class?

l.215 This is the first time primary and secondary forest are mentioned. Also, the term tile has only been mentioned one time before ("Age-classes are represented as sub-tiles within a grid-cell"). Maybe it would help to already introduce these aspects in 2.1.1?

l.217 Does managed land refers to crop/pasture here (i.e. not forest management)?

l.225-226 mix of singular and plural?

l.224 I assume this is not a only "if the". Consider rephrasing such that it gets apparent that net zero land-use change is just one example?

l.229, l.263, l.527, l.602 consider updating to Hurtt et al. 2020

l.228 lost "and"?

l.229: I do not understand what you mean with modifications 1a (and 2a) seem not to be modified with respect to LUH2?

l.233: LUH2 offers a separation of harvest to mature and young forests. Consider shortly stating why this separation is not used in LPJ-wsl 2.0?

l.233-237: But wouldn't e.g. shifting cultivation rather make use of younger forests?

l.237-238: LUH2 offers both, harvested area and harvested biomass. Here it is stated: "until two conditions are met" and in the next sentence: "until a prescribed harvest mass or harvested area is met". This requires clarification when which of these criteria is applied.

l.244-245: I wonder if this would really be the case, I would assume that the ranking from old to younger age-classes decouples deforestation and abandonment?

l.240-... Is this new in LPJ-wsl 2.0 or is this as it has been done already before? Noticing Earles et al (2012)and McGuire et al. (2001) in 249/251 I wondered if the authors could also give the reference for the % ratios in l.240-247?

l.249-251: could you clarify which numbers are from Earles?

l.251-256: "product pool" is used twice here – with different meanings?

l.253 "dataset described further in Sect 2.3.3" – I cannot find such a description there?

l.265 "managed lands" = agricultural managed lands (since forests can also be managed?)

l.271 "accessed ..." consider moving to references.

l.272 "fuzzed" is this relevant for this study?

l.275 Refer to SM2 here.

l.275 "model-observation comparisons" – isn't the model resolution anyway 0.5° in the compared simulations?

l.310 and regrowth?

l.318 info does not match Table 2.

l.320 it is unclear to me which of the deforestation rules from Section 2.2.2. also applies for the Snoage_event simulation, could you please give a bit more detail?

l.322 NBP so far not introduced (NPP and Rh only in the abstract).

l.330 Table lists 4 objectives/questions.

l.336 Maybe already add here for clarification that Sage = SFireLU.

l.339 “all three simulations” presumably refers to SFire, SLU and SFireLU? But what about Snoage? What was the spin-up procedure for this simulation?

l.339 does the first spin-up also has “land use values” or does it assumes only natural vegetation?

l.341 Could you please specify what you mean by ‘natural conditions’ – fire?

l.342 please clarify “land use values” does that mean managed agricultural land distribution? What about harvest?

l.342 please clarify: was the second spin-up procedure subsequently or alternatively for different simulations? Do all four simulations start from the same values in 1860?

l.356-359 I found this sentence a bit difficult to read since the “By contrast, fire . . .” seems to refer to the “Trends in LULCM are . . . prescribed” – please clarify by e.g. rephrasing.

EQ6 I wonder if the last factor should be written as a sum with age classes as index?

l.393 “age-structure patterns” – maybe “patterns of tree density and height per age”?

l.397 what does stand refers to –patch?

l.404 I do not understand this part of the sentence: “data be taken on every species; although species-level data are available”.

l.417 These survivor trees make me think if a classification as “time since disturbance” would make more sense than a classification as age-classes?

l.417 LPJ? LPJ-wsl 1.0? LPJ-wsl2.0? All of them?

l.442 all “three” U.S. States.

l.452 Figure 5?

l.457 missing t in event.

l.457 LPJ? LPJ-wsl 1.0? LPJ-wsl2.0? All of them?

l.484 “(?)”?

l.487 Snosge -> Snoage

l.498 LPJ? LPJ-wsl 1.0? LPJ-wsl2.0? All of them?

l.508-509 the 23 years are not directly evident from Table 3, nor is it the decrease in zonal ecosystem age, could you help your readers specifying which of the values in Table 3 show these? This also holds for the rest of the paragraph; maybe consider extending Table 3 or adding another table showing integrated values?

l.519 grammar issue?

l.529 also here sum over B3age?

l.530 simulated NPP and Rh.

l.533 consider to delete “slightly”!

l.587 LPJ? LPJ-wsl 1.0? LPJ-wsl2.0? All of them?

l.593: again I would recommend clarifying “upper limit” and again I am not sure if this is correct, due to the oldest age-classes first principle for harvesting and deforestation (l. 233-239).

l.606 same here with the underestimation – given the oldest age-classes first principle I am not fully persuaded that it underestimation is granted.

l.622 But isn't this model dependent? Maybe consider rephrasing, e.g. "suggesting that uncertainty in carbon residence time could potentially be reduced" or the like

l.624-627: I do not agree that this is "the current state of knowledge", nor that "existing models that estimate the global land-use flux... do not include age dynamics". For the former and the latter please e.g. refer to findings of Yue et al. 2018, in addition, for the latter, the authors might have a look into other studies conducted with some of the models listed around l.55-57.

l.631: consider adapting the subsection header since this subsection seems to be more about precipitation than demographic effects?

l.662: is this only the case if using the unequal age-class setup?

l.664: LPJ? LPJ-wsl 1.0? Both?

l.671: I assume this is the case in several of the models listed around l.55-57.

l.675: consider adding "on the same machine" (if this is correct).

l.680: The first 2-3 sentences seem to be incomplete?

l.700: To my understanding JSBACH4 does not represent much vertical heterogeneity. You might want to have a look into e.g. ORCHIDEE-CAN (Naudts et al. 2015) or in individual based models (in addition to ED), e.g. LPJ-Guess (Bayer et al., 2017).

Table 1: LPJ-wsl v2.0?!

Table 2:

* single-cell: included processes might not match the description in 2.3.4, l.318.

* global: Initially I tried to associate each of the four questions with one of the simulations, due to the visual structuring of the rows of the table. Maybe merge cells and

number questions?

Figure 1: please explain what you mean with "land use" in this context

Figure 2:

* Initially I was confused by the last explanation located next to timestep 2 in panel B. Maybe it would help to place this explanation in-between timestep 1 and 2?

* fw in a3 in panel B confused me, maybe this is because a3fw = a3f1 since w=1 for a3?

* I am not sure about using ftotal in the formulas in panel B and C, would this also be correct if at the same timestep a fraction would leave an age-class and another fraction would enter the age-class (which might be the case in a large grid-cell quite often)? Instead of showing two examples for the simple merging please consider replacing example 2 (Panel C) with an example with incoming and outgoing fractions.

Figure 3: explain MI,MN & WI again.

Figure 5: consider increasing visibility by changing the y-axis of the first panel (max of -5/-6 kgCm-2).

Figure 6:

*consider adding simulation names (Sage and Snoage if I understood it correctly).

* could you show the simulation starting from the spin-up, i.e. starting 1860? Is the difference between the simulations due to the spin-up or evolving in the course of the simulation?

Figure 7:

* consider using (a) and (b) instead of left and right.

* "LPJ-wsl simulations" consider adding simulation name from Table 2. Figure 9: Since SFireLU is more complete than SFire, consider using the solid line for this more com-

plete set-up?

Figure 10:

* consider using the same y-axis for better comparability (same SMFig1).

* l.988 model is can -> model can

Figure 11:

* l.993 “black is zero”? On the colour map it is yellow?

* red (-0.3) and pink (0.7) are difficult to distinguish, maybe consider a change in the colour map.

Figure 12: consider labelling panels (a)-(d) instead of using top row, bottom row, top left and bottom left.

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