Reviewer 1

Dear Reviewer,

We would like to thank you for your interest in our work, and for providing valuable comments that will significantly improve the quality of our publication. Below we provide a point-by-point reply to your comments, formatted in *italic*. Additions to the manuscript are formatted in ***bold italic***.

Best regards,

On behalf of all authors,

Jette Elena Stoebke and David Wårlind

This manuscript develops a new canopy structure scheme within the LPJ-GUESS model. The new scheme represents spatially explicit canopy (SEC) and is expected to better simulate vertical and horizontal light environment. The manuscript also compares how different canopy structure schemes influence model simulation results.

Horizontal/vertical heterogeneity is a long-term challenge in modeling canopy structure and its impact on ecosystem dynamics, especially with the adoption of explicit vegetation demography. Fisher et al. 2018 has listed different flavors of canopy structure representations. To me this study tries to mix spatially explicit gap models with spatially implicit dynamic global vegetation models. I am glad to see LPJ-GUESS, being an important global model, to advance in this realm. Please see below for my main concerns.

First of all, using spatially explicit structure generally requires running an ensemble of simulations/stands to reduce the impacts of spatial stochasticity, especially for stands/grids with no spatially explicit census data.

*REPLY: LPJ-GUESS is structured to run with multiple stands (land use types) and patches (replicates of the same land use types). Depending on the scientific question the number of stands and patches will differ. In this study we only simulate a natural vegetation stand type with 100 patches, as stated on line 204 in section 2.2 Model description "All simulations use 100 replicate patches". The use of 100 patches is to limit the influence of stochasticity in the simulations. To make it clearer that we use multiple patches in our simulations the following sentence has been added to the introduction (line 61) "**To account for the stochastic nature of ecological processes and the inherent heterogeneity of the landscape, LPJ-GUESS simulates multiple replicate patches per grid cell, each evolving independently to capture variability in processes such as establishment, mortality, and disturbance**."*

It is unclear to me how the 'fixed positions' (line 145-155) were decided and whether/how ensemble was created.

REPLY: On line 166 we try and explain this "Each new PFT cohort's position was randomly selected from a weighted probability distribution based on the light conditions of each forest floor section that exceeded PARest,min.". Hence, each new cohort position is determined depending on the heterogenic light condition of the forest floor and favours positions with the highest annual forest floor PAR.

In addition, in this case, 'cohort' is not appropriate because it means the average state of trees with similar size/functional type by definition and should be spatially explicit. The scheme is basically modeling individuals (like the SEIB model).

REPLY: Here we agree with the reviewer. Ideally, we would have switched from the standard cohort approach in LPJ-GUESS to the individual-based mode. However, early in the process, this option was ruled out due to the significant increase in simulation runtime it would entail. In particular, calculating the vertical canopy sections (see Fig. 1b) becomes much more computationally demanding in individual mode. Tests showed that the runtime of the SEC scheme increases by a factor of five when switching from cohorts to individuals, as both the number of vertical canopy sections and the number of simulated trees (cohorts or individuals) increase accordingly. Hence, moving from the standard cohort-based LPJ canopy scheme to the SEC scheme with individuals would lead to a more than tenfold increase in total runtime. Additionally, memory usage, which is already quite high when running LPJ-GUESS in parallel mode, would also increase substantially. This is why we haven't considered individual mode for the new canopy structure development of LPJ-GUESS.

I would expect a model test to show simulations of a single stand with the same initial demography but different spatial assignments of 'cohort' positions.

REPLY: This would require modifying the way new establishment positions of cohorts are selected, replacing the current method based on the weighted probability distribution described on line 166. We're actually exploring this approach as part of an ongoing master's thesis project, and we plan to present the results in a future publication.

Second, in addition to comparison in model dynamics, a critical comparison to me is the equilibrium vertical and spatial structure of simulated by different canopy structure assumptions (e.g. vertical LAI profile at stand-level, distribution of biomass/LAI across patches under the same disturbance regime). This can help us to understand how canopy structure assumptions modify long-term model simulations.

REPLY: Figure 3 provides an indirect view of vertical stand structure by presenting the distribution of aboveground woody biomass across DBH size classes. Since DBH is generally correlated with tree height, and given our parameterization (bole height at 50% of total height, with the canopy extending from the bole to the tree top), this distribution gives a qualitative impression of the stand's vertical structure. We acknowledge, however, that a direct comparison of equilibrium vertical and spatial structure, such as vertical LAI profiles at the stand level and the distribution of biomass or LAI across patches under the same disturbance regime, would offer further valuable insights. Such analyses were beyond the scope of this study but will be considered in future work.

Third, 'functional co-existence' seems to be a key motivation of this study. However, the definition or target of simulated functional co-existence is not clear to me. Do you just mean stable coexistence of two PFTs under equilibrium? Are there data to benchmark the degree of coexistence?

REPLY: Our main goal is to make functional co-existence possible in the model, both when it comes to tree sizes and species (see key area 2 line 108). Thus, we focus on enabling structural co-existence rather than matching a specific empirical target. In this paper, functional co-existence refers to the persistence of multiple PFTs (shade-tolerant and shade-intolerant) within a single patch. While standard LPJ can represent such co-existence, it does not always arise as an emergent property of the model dynamics (see Fig. 6 and 7). In contrast, the PPA and SEC schemes overcome this limitation, primarily by increasing light heterogeneity, which in turn promotes the natural emergence of functional co-existence.

Some technical comments:

Line 10 "more realistic simulation of forest floor light conditions" --> I did not find any results/benchmarking on floor light conditions

*REPLY: "...enabling more realistic simulation of forest floor light conditions, especially…" has been replaced by "...**enabling simulation of forest floor light conditions that better capture spatial variation, especially**…"*

Line 88-90, one other related process is light-driven trait plasticity. See recent studies in ELM-FATES and ED2 (Needham et al. 2025; Ma et al. 2025). Does LPJ-GUESS has trait plasticity within a PFT?

Needham, Jessica F., et al. "Vertical canopy gradients of respiration drive plant carbon budgets and leaf area index." New Phytologist 246.1 (2025): 144-157.

Ma, Yixin, et al. "Constraining light-driven plasticity in leaf traits with observations improves the prediction of tropical forest demography, structure, and biomass dynamics." Journal of Geophysical Research: Biogeosciences 130.6 (2025): e2025JG008814.

REPLY: This version of LPJ-GUESS does not include trait plasticity within PFTs, as the current development is a direct extension of the main trunk of LPJ-GUESS. However, work on incorporating trait plasticity is ongoing within the LPJ-GUESS community and is being explored in separate development branches (see Dantas de Paula et al. https://doi.org/10.1111/nph.17600, https://doi.org/10.5194/bg-22-2707-2025).

Line 99-100, this assumption might not be too biased as long as the patch size is assumed to be similar to crown size (say 25m by 25m for tropical forests) and patch dynamics was done correctly. See the ED modeling literature (e.g. Moorcroft et al. 2001 and the Fisher et al. 2016 review).
REPLY: We acknowledge that this assumption may be less problematic for fully grown trees, however it is limiting for small saplings, whose crown sizes are much smaller than typical patch sizes. Accordingly, we have revised the corresponding sentence in the manuscript to clarify this distinction.

*"The mentioned shortcomings point to the need for improvements, particularly..." has been replaced by "**The mentioned shortcomings might be less relevant for large trees but become important for small saplings with much smaller crowns than the patch size, particularly during forest regeneration, and highlighting the need for further improvements, particularly**..."*

Line 145, this equation seems to be confusing highly empirical to me. So a cohort with CA = half of CA_max would have a mort_self of 1 per year?

REPLY: Eqn 2 is only applicable if CA_i is larger than CA_max. The equation has been updated to make this clearer.

Line 150, what does z mean here? distance to the center? I am not sure what does 'border issues' mean here.

*REPLY: z refers to canopy depth (see line 121) and is not related to the spatial placement of the cohort. The sentence has been corrected accordingly: "...defined in a two-dimensional space (Θ, z), where Θ ..." has been replaced by "...**defined in a one-dimensional space (Θ), where Θ** ...".*
*The border issue refers to what to do with the growing canopy if it exceeds the boundary of the patch. By defining the space as a ring, the exceeding canopy will appear in the other edge of the canopy as seen in the green and purple cohorts in figure 1b. We have revised the sentence to explain this better. "...border issues, as each cohort seamlessly …" has been replaced by "**...border issues, such as when growing cohorts extend beyond their patch boundaries, as each cohort seamlessly …**"*

Line 190, this is related to my abovementioned comments on the definition of "cohort". Once going spatially explicit, the model is simulating individuals not cohorts.

REPLY: See reply to first general comment.

Line 193-194, does the new SEC scheme consider solar angle? It would be a pity not to since this is one of the most important advantage of spatially explicit canopy structure.

REPLY: No, all solar radiation comes directly from zenith as mentioned on line 193. Considering solar angle will be addressed in future work.

Figure 3, I feel all site average of absolute biomass/growth is hard to interpret. I would suggest use a 3 by 3 figure show results for each biome and the appendix includes result for each site.

REPLY: The figure has been updated as suggested.

Figure 5, what does the green line mean in panel a and b mean?

REPLY: Explanation of the green line is given within the upper legend ("Productivity").

Table 3, there are reported observed values from global scale to biome-specific scales right? (e.g. work by Brian Enquist etc.) It would be helpful to put the model simulation results into the context of observed self-thinning scaling.

*REPLY: We have added biome specific slope values with CI from the study of Yu et al. 2024 to table 3 and the following text to the results "**Observations for the same temperature regions reported by Yu et al. (2024) show a distinct increase in slope with warmer climates. A similar pattern is observed for LPJ and SEC during the equilibrium phase.**" and to the discussion "**LPJ and SEC also showed an increase in slope with warmer climate regions, consistent with the findings of Yu et al. (2024). All schemes exhibited a slope that was too steep at equilibrium in boreal and temperate regions, whereas in tropical regions, all schemes fell within the confidence interval of the observations reported by Yu et al. (2024).**"*

Figure 6, again, a biome-specific comparison would be better.

REPLY: The figure has been updated as suggested.

Line 384, what is the key "observational data" to show the advantage of SEC? If only the RSME shown in Figure 8 and 9, SEC seems to perform similar or even slightly worse and PPL.

REPLY: The RMSE values in Fig. 9 are primarily included to illustrate how the model results change as the number of simulated patches increases. The absolute values themselves are not the focus, rather we are interested in how quickly the results converge. The key figure for assessing the improvement is Fig. 3, which shows the distribution of biomass, productivity, and mortality across DBH size classes.

Reviewer 2

Dear Reviewer,

We would like to thank you for your interest in our work, and for providing valuable comments that will significantly improve the quality of our publication. Below we provide a point-by-point reply to your comments, formatted in *italic*. Additions to the manuscript are formatted in ***bold italic***.

Best regards,

On behalf of all authors,

Jette Elena Stoebke and David Wårlind

This paper descripts a new crown organization scheme in LPJ-GUESS, spatially-explicit canopy (SEC), and tested its performance in comparison with its original scheme and PPA (Perfect Plasticity Approximation). This scheme is well-designed and worth to try, especially when the community is struggling in the ED (crown stretching) and PPA (crown sorting) schemes. The paper is well-written and fully tested the performance of this scheme. I just have some minor questions to ask the authors to clarify.

Since it is “spatial explicit”, is there any shading effect among individuals/cohorts? With changes in the solar angles, the shades of trees can shade other trees and the calculation is pretty complicated. The crown stretching (original LPJ-GUESS and ED models) and crown sorting (PPA) schemes were used to skip these calculations.

REPLY: There is no shading within individual trees of a cohort, but canopies of different cohorts within the same patch can overlap (see Fig. 1b). In such cases, vertical canopy sections containing multiple cohorts are created, allowing them to shade each other. Since all incoming solar radiation is assumed to come directly from the zenith (as mentioned on line 193), the calculation of these vertical sections is simplified, keeping the computational demands at a reasonable level.

Co-existence mechanism is mentioned in this paper. However, I didn’t see the explanation of the detailed mechanisms. Why and how spatial explicit scheme can increase co-existence?

REPLY: The spatially explicit canopy scheme increases co-existence by creating gaps and varying light conditions within the forest, allowing both shade-tolerant and shade-intolerant plant types to establish and survive in different parts of the patch. This spatial heterogeneity enables more diverse plant communities than in schemes with uniform canopy structure. This is mentioned briefly in the Methods section (lines 146–148) and discussed in more detail in the Discussion section (lines 416–454).

Other comments:

Line 45: “simulating forest demography”: I’d prefer to use “demographic modeling”.

REPLY: Updated accordingly.

Lines 48~49, for the sentence “In recent years, various approaches have been developed to model forest dynamics, striving to balance model fidelity with its complexity.”, it would be better to cite the specific model papers, such as ED, PPA, and forest gap models developed by HH Shugart’s group, instead of just “(Fisher et al., 2018).” For the next sentence, it is ok to use “(Fisher et al., 2018)”

*REPLY: The citation has been updated. “(Fisher et al. 2018)” has been replaced by “**(Moorcroft et al., 2001; Strigul et al., 2008; Shugart, 1984)**”*

Lines 115~117: I suggest to rephrase these sentences to make it clearer. For me, it seems the leaves are well-mixed, instead they are sorted vertically.

REPLY: Yes, you understood it correctly. The leaves are well-mixed or "distributed uniformly" as we describe it, within the two-dimensional space defined by the horizontal crown extent and the vertical range between bole height and canopy height.

Line 168, “a perfect plasticity-like (PPL) approximation”. I didn’t get why the authors call it “PPL”. To me, it is exactly PPA. Why don’t call it “PPA”?

REPLY: After reading Strigul et al. 2008 again it makes sense to call our approach PPA, as we have used the original concept of PPA when we have implemented it in LPJ-GUESS. We have updated PPL to PPA in the manuscript and updated the section describing the PPA model and its implementation in LPJ-GUESS (line 168-189).

Line 169, as for the citation, please add “Strigul et al., 2008” (doi: 10.1890/08-0082.1), which is the paper describes the details of PPA. (SCALING FROM TREES TO FORESTS: TRACTABLE MACROSCOPIC EQUATIONS FOR FOREST DYNAMICS - Strigul - 2008 - Ecological Monographs - Wiley Online Library)

REPLY: The citation has been added.

Line 176, about the reference of PPA, please use “Strigul et al., 2008”.

REPLY: The citation has been added.

Lines 181~182, “the PPA scheme (Fisher et al., 2018) introduced a gap fraction (η) that represented small gaps between trees within a cohort”, remove “(Fisher et al., 2018)”.
By the way, the “gap fraction” is not a feature of PPA. We added it just for allowing more light in the understory. It’s a tweak.

REPLY: The model description section on PPA has been revised to clarify that it is not a perfect plasticity-like (PPL) approximation, but is instead based on the original perfect

plasticity approximation (PPA) model proposed by Strigul et al. (2008) (line 168-189). The "gap fraction" is mentioned as a tweak to the original PPA model.

Line 193 "assuming instead that all solar radiation came directly from the zenith.", it is not the case for LM3-PPA (and in the current version of LM4.2). We inherited the radiative transfer scheme in LM3, which considers incidence angle changes during a day.

*REPLY: To make it clearer that we here only address the implementation in LPJ-GUESS we have updated the initial sentence of the paragraph on line 190. "...the schemes was that tree individuals ..." has been replaced by "...**the schemes implemented in LPJ-GUESS was that tree individuals** ...".*

Line 270, as for "self-thinning", in PPA, the density-size relationship is determined by allometry equation and survival rate of understory layer trees.

*REPLY: In the updated model description section on PPA we have added this sentence on how self-thinning is done in our PPA implementation **"Self-thinning in our PPA implementation follows the same approach as in SEC."***

Additional updates

Verb tense has been harmonized throughout the manuscript.

“Co-existence” has been updated to “coexistence” throughout the manuscript.

A duplicated section in the Introduction has been removed (lines 90–94 in the latexdiff document).

Text throughout the manuscript has been revised to improve clarity and flow for the reader. As a result, some line references in the reviewer replies may no longer be perfectly accurate.