Response to Referees

Manuscript Title: Simulating Bark Beetle Outbreak Dynamics and their Influence on Carbon Balance Estimates with ORCHIDEE

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We're grateful to Referee #1 and Referee #2 for their thorough and insightful feedback. Based on their suggestions, we've revised nearly 60% of our paper. One of the key comments affected the equations in our model, of which some are now quite different from those in the original study by Temperli et al. 2013. With an increase in the number of the changes and the nature of the changes, it is becoming less straightforward and less meaningful to present a side-by-side comparison of our model with the one Temperli and their team created. So, it was decided to shift the presentation of the model from side-by-side comparisons towards explaining how our work builds on Temperli's ideas from 2013. We hope that these updates make it clear how our research adds new insights and directions to the field, while still recognizing the foundation laid by Temperli and their colleagues.

Because of the numerous revisions including changes in structure, analyses, and figures, we kept the cover letter short rather than copy/pasting extensive parts of the revised manuscript. Below, we summarize our responses to each of the major and minor comments.

Although the code was updated in preparation of the revised manuscript, the version number and doi were not. The reason is solely technical. We would like to wait for the referee comments to decide whether we can finalize the code changes. Once the code is finalized, changes will be committed on the svn server, the revision number will be updated in the manuscript, and the code will be tagged with a new DOI.

Response to Referee #1

Major Comments:

Comment on Evaluation:

My main concern about this manuscript and study is the evaluation. Stating that the model has been evaluated (qualitatively or quantitatively) means to me that subsequent studies can claim that the model is ready for use in assessing beetle impacts on the carbon cycle. So the words matter, in my opinion. The analysis that is presented consists of running the model across a number of sites and for various windthrown intensities, then evaluating cumulative wood volume and NEP and time series of NPP. Furthermore, the comparison with other studies (Table 5) is flawed. This study is a first step, but does not rise to the level of an evaluation. Rather, this is a model sensitivity study, in my opinion. At a minimum, to use the term "evaluation", I think the following additional steps are needed: a) presentation and analysis of the variables and metrics associated with bark beetle outbreaks (e.g., the important variables of Equations 1-14) for the model runs, including time series (similar to Figure 2, but more detailed); b) comparison of time series of drivers (climate, windthrow, substrate, etc.) and outbreak variables to assess how the drivers are affecting the outbreak variables; c) at least one run, and ideally multiple runs, for locations with observed outbreaks, and a comparison of model results with observations (or perhaps results from previous modeling studies?); d) improved selection of other studies for comparison in Table 5, deleting studies that are not relevant or address beetle species whose behavior is different and adding other modeling studies. Unless the authors include these steps, I don't think the authors should use the term "evaluation", and text throughout the manuscript should be changed to avoid implying an evaluation. See below for detailed comments.

Response:

To address this concern, we have:

- Changed "quantitative evaluation" to "sensitivity experiments" to more accurately describe the scope of our analysis.
- Conducted and added a sensitivity analysis of model parameters and climatic conditions.
- Extended the analysis with time series analysis illustrating how different drivers influence outbreak variables.
- Removed Table 5 and enhanced the comparison with other studies in the revised Table 3 and Section 5.1 of the Discussion to provide a more accurate context for our model's applicability and specificity.

Comment on Bark Beetle Species Specificity:

The manuscript is too vague about which bark beetle species is simulated. I'm assuming it is Ips typographus. Some aspects of bark beetle biology and ecology are generalizable across species, but others are not (including what triggers and influences

outbreaks). The manuscript needs to state the species of interest explicitly in the Abstract, Introduction, and Methods, and add some discussion about generalizability to other bark beetle species in the Discussion.

Related to this, because of differences in ecology among beetle species, the model implementation described in this manuscript is not necessarily applicable to other locations/systems. For instance, mountain pine beetle, the major beetle species in North America, is not triggered by windthrow events, has multiple climate influences, and has one, not multiple, generations per year. And the beetle phenology model used in this study, while maybe appropriate for Ips typographus, is not appropriate for other beetle species. Differences and lack of direct applicability should be included in the Discussion.

Response:
We have clarified the species in the revised manuscript. It now explicitly mentions that our model simulates *Ips typographus* in the Abstract, Introduction, and Methods sections. Furthermore, we have added a section in the Discussion (lines 792-796) to discuss the model's potential extension to simulate other bark beetle species, acknowledging the unique aspects of bark beetle ecology and behavior.

Minor Comments:

Introduction:

Comment: In the Introduction, please include more description of past studies that modeled effects of bark beetles on ecosystem properties. Please distinguish studies that prescribed outbreaks from those that modeled outbreaks. Briefly describe the system (beetle and host) and results. Suggested studies include Temperli et al. (already cited, but what was the study about); Jonsson et al., AgForMet, 2012; Seidl and Rammer, Landscape Ecol., 2017. Please do a literature search to identify others.

Response: We have enriched the Introduction with a more comprehensive review of past studies on the effects of bark beetles on ecosystem properties, distinguishing between studies that prescribed outbreaks and those that modeled them. This includes a discussion on Temperli et al., Jonsson et al. (AgForMet, 2012), and Seidl and Rammer (Landscape Ecol., 2017) in lines 92-108.

Comment: In the Introduction, please provide more information about Ips typographus outbreaks, including what fraction of a 25 x 25 km ORCHIDEE grid cell might be affected. What have been observed outbreak areas or volumes, and what are these values relative to a grid cell? (I realize in this study, the model is run in aspatial mode.)

Response: : Additional information on Ips typographus outbreaks, including potential effects on ORCHIDEE grid cells, has been incorporated to provide a clearer context for our simulations in lines 92-108.

Methods:

Comment: Winter beetle mortality from very low temperatures has been modeled previously for Ips typographus (Jonsson et al.) and other beetle species. Why wasn't this included here? Add to Discussion as missing processes.

Response: Acknowledging the importance of winter beetle mortality as highlighted in previous studies, such as those by Jonsson et al. 2012, we've now incorporated a variable dedicated to beetle winter survival in our model. This addition, detailed in lines 348-354 of the manuscript, specifically addresses the critical role of extreme low-temperature conditions on beetle population dynamics.

Throughout the description of equations: it would be helpful to interpret for the reader the equation, including what processes are represented and why they are represented the way they are. The authors do some of this, but for other equations, I am confused. L 225-240: Both G and rDD are described as the number of generations in a year. Please refine the description of one of the variables.

Response: We've managed to enhance the comprehensibility of the equations within the manuscript, ensuring a cohesive presentation that clearly delineates the ecological and process-based rationale underpinning each equation. This revision aims to alleviate any confusion regarding the representation and purpose of these mathematical formulations.

Comment: Please describe the rationale and ecology/biology behind the apparent averaging of G and Act. What is the rationale and ecology/biology behind multiplying the different factors?

Response: To clarify the ecological and biological reasoning behind the averaging of variables G (beetle generations) and Act (beetle activity), we have extensively revised

the pertinent sections (lines 320-344). This revision describes the underlying rationale and significance of these calculations within the context of beetle population dynamics.

Comment: Assuming that Si (Eq 1, 5) is the same as SI (L 265), this variable should not be in the calculation of RI twice. It makes more sense to have susceptibility separate from beetle pressure, so remove from Eq 1 and clarify L 255? Maybe I'm confused.

Response: Upon review, we agreed with the observation regarding the redundancy of the susceptibility index (Si) in certain calculations. The manuscript has been adjusted to rectify this, ensuring a clear distinction between indices representing tree host characteristics and those pertaining to bark beetle behavior. This adjustment fosters a more logical and scientifically sound representation of the model's components.

Comment: Section 2.2.3 opens with a comparison between tree mortality from mass attacks and tree mortality from, presumably, beetles in the endemic population phase. So I'm confused about if the killed biomass is applicable to both situations, or just one?

Response: To dispel any ambiguity surrounding the application of tree mortality rates from mass attacks versus endemic population phases, we have refined the descriptions of equations 6a' and 14. Additionally, the introduction of the conceptual scheme (Figure 1) serves to guide the reader through the intricate relationships between various model indices, enhancing overall understanding.

Comment: Please add units where appropriate. Examples include Binf, Bt, Bdb. There may be other instances.

Response: All necessary units have been meticulously added to Table 1, ensuring comprehensive and clear communication of the model's quantitative aspects.

Comment: It might be easier to understand if Section 2.2.5 were moved before the equations that represent the processes.

Response: In response to suggestions for improved readability and comprehension, we have merged Section 2.2.5 to with the detailed equations section that delineate model processes. This restructured presentation is intended to facilitate a more intuitive grasp of the model's operational framework.

Results:

Comment: I think the Results section should add a presentation about the simulated outbreaks. Please add figures that illustrate time series of outbreak metrics (BPI, wood volume killed by beetles) for each site, similar to Figure 4, and discuss.

Response: You suggested adding more details about the simulated beetle outbreaks, including time series data for each site. We've updated Figures 5, 6, and 7 to show these time series, giving a clearer picture of how the outbreaks unfold over time.

Comment: Also, please move Section 3.2, which describes outbreaks and comparisons with other studies, before the sections that discuss how outbreaks vary across climate and windthrow gradients. Seems more logical in this order.

Response: Based on your feedback, we've moved the section that describes the outbreaks and how they compare to other studies to an earlier part of the Results. This rearrangement makes the flow of information more logical, first establishing the basics of our findings before delving into the specifics of how different factors like climate and windthrow affect the outbreaks. You can retrieve it in section 3.4 and 4.1 and 4.2.

Comment: Figure 5: Would be helpful to separate the NPB effect of windthrow from that from bark beetles. I realize that may be tricky, and may require two figures, but it will provide readers with a better sense of the contribution of outbreaks to the carbon dynamics.

Response: We decide to remove figure 5 and replace it by figure 7 but Figure 8 (updated from the original Figure 6) focusing on the contribution of outbreaks to carbon dynamics implies that the figures and possibly associated text have been revised to make the role of outbreaks in the model clearer, separate from the effects of windthrow events. Finally, we rewrite section 3.6 aimed at clarifying how the model accounts for mortality and its impact on carbon dynamics.

Discussion:

Comment: Throughout this table: the Edburg et al., 2012 study is not in the reference list, but if that's the Frontiers article, that was a conceptual paper and should not be used for model evaluation. Please select something specific to Ips typographus. Similarly, the Hlásny et al. 2021 article is a review; please replace that with observational studies that support the statements in the table.Also, many metrics are specific to a particular (and different) beetle species, which may have different dynamics and drivers.The comparisons will be strongest when studies of Ips typographus are

included. First row of "Red or epidemic stage":Please demonstrate this idea that high populations can kill healthy trees by plotting time series of the various variables that represent tree health (susceptibility to drought), BPI, etc. The recovery part of the Pfeifer et al. study was only modeling.Modify the text (not observational). And comparing results from different models is okay but weaker than observations.

Response: You pointed out the need for more relevant and specific references in Table 5, particularly emphasizing the importance of focusing on Ips typographus. In response, we have decided to remove Table 5 and instead integrate its key points into Section 5.1, where we now discuss our findings in the context of Ips typographus-specific studies. This change allows for a more focused and relevant comparison, aligning with your suggestion to rely on observational studies and specific metrics relevant to Ips typographus. We've ensured that the references now more accurately support our statements, moving away from conceptual papers and reviews to more empirical and species-specific literature.

Comment: Please compare your modeling to previously published studies (see my comments on the Introduction for some studies, but not all?), noting similarities and differences in model formulation, applications, and results.

Response: While we acknowledge the importance of comparing our model to previously published studies, space limitations have constrained our ability to conduct a full review within the manuscript. However, we made an effort to clarify the methodological distinctions and similarities where possible, aiming for transparency and ease of comparison for the reader.

Response to Referee #2

Major Comments:
Comment 1: Many symbols are missing from Table 1 (which should be moved to an Appendix). I've listed some but not all of them in the "minor comments/corrections" section, but please do a thorough review and add any others you find.

Response: We have thoroughly revised Table 1 to ensure all symbols are included and updated. To maintain the flow and readability of the main text, we decided to keep Table 1 within the main text rather than moving it to an Appendix but we let the editor decide the location of table 1.

Comment 2: The references to beetle "generations" are hard to understand. They don't seem to fit with how beetle dynamics are ultimately realized in the model, but even in context here it's confusing. The sentence at lines 240-241 says, "the index G reaches its maximum value of one when 2.5 or more generations occur in a single growing season." But isn't G "the number of
beetle generations… that could occur in the current year"? Then the next sentence is confusing because of "of the first generation." So far we've only been learning about beetle pressure index, not what actually triggers a generation.

Response: We recognized the confusion in our description of beetle generations and have taken care to rewrite this section for better clarity. The revised text more accurately explains how the concept of generations integrates into our model, ensuring a clearer understanding of beetle dynamics. Lines 330 to 340.

Comment 3: It's really important to include something like Sect. 2.2.4 (differences from beetles in LandClim; lines 365-374), but it's currently too vague. The "calculation of the susceptibility" differences are documented (although not explained, per se) in Table S1, but that's not referenced in the text here. As far as I can tell, the other mentioned differences are not
explained at all. A model descriptive paper in GMD, as outlined in the Manuscript Types webpage, "should be sufficiently detailed to in principle allow for the re-implementation of the model by others, so all technical details which could substantially affect the numerical output should be described." If such technical details need to go in an Appendix and/or a Supplement, That's fine, but they must be included somewhere. Note also that it doesn't need to be every line of code or even every equation—if you can describe what you did clearly enough using words, that's fine.

Response: Based on your suggestions, we've made major updates to nearly 60% of our paper. One of the key changes involves the equations we use in our model, which are now quite different from those in the original study by Temperli et al. 2013. These changes mean it's now tricky to directly compare our model with the one Temperli and their team created. So, we've decided to focus less on making side-by-side comparisons and more on explaining how our work builds on Temperli's ideas from 2013. We've used their study as a starting point but have developed our own unique approach from there. We hope these updates make it clearer how our research adds new insights and directions to the field, while still recognizing the important foundation laid by Temperli and their colleagues.

Comment 4: Although I have plenty of suggestions for Sect. 2.2.5 as you'll see below, I think it's great overall. For the most part, you do a good job of explaining how the model works in plain language. I would encourage you to consider, actually, moving Sects. 2.2.2-2.2.4 to an Appendix—they are highly technical and make more sense after reading Sect. 2.2.5. This may require a bit of reworking on Sect. 2.2.5, but with sufficient references to locations and equations in the Appendix I'm sure you can do it well.

Response: we opted to merge sections 2.2.2 to 2.2.5 to streamline the narrative and improve comprehension. This restructuring allowed us to maintain technical rigor while enhancing the accessibility of the model description.

Comment 5: Lines 399-403: What about the simulation results caused you to choose these thresholds? The second threshold's explanation seems circular. It is especially important to explain the reasoning here because the time spent in different stages is the crux of the comparison of your model against the literature.

Response: The revised manuscript provides a clearer rationale for the thresholds, ensuring they are grounded in empirical observations and model sensitivity analyses. The presentation of the results was restructured by removing the post-processing into outbreak phases which allowed us to reduce the number of thresholds used in the manuscript.

Comment 6: Various metrics used in analyses need definition. Lines 506-508: How do you recover time from Fig. 3? Presumably looking at the areas of some of the wedges, but which? … Or are you looking at Fig. 4? If so, mention that at the top of this paragraph, and still—define "recovery." Is it "time to return to Year

Response: In line with both referees' suggestions, we have reframed the manuscript to focus on a sensitivity analysis of model parameters and climate conditions, moving away from the term "qualitative evaluation". This adjustment better reflects the nature of our analysis and its contribution to understanding beetle dynamics.

Comment 7: Sect. 2.4: I think it sells this paper short to call it a "qualitative evaluation." There is a fair amount of quan5ta5ve evaluation happening here too, especially with regard to amount of time spent in different outbreak stages and time to recovery. I think what you're getting at here—and this is clarified at lines 567-571, which as you'll see I think should be moved here—is that you're not actually testing whether specific real-world beetle outbreaks can be reproduced. Right?

Response:
You pointed out that describing our study as a "qualitative evaluation" might not fully capture the depth of our analysis, which includes significant quantitative aspects, such as the time spent in various outbreak stages and the recovery period. We appreciate this insight and agree that our initial terminology might not have adequately reflected the comprehensive nature of our evaluation.
In light of your comment, along with feedback from another referee, we have revised our approach to emphasize the sensitivity analysis of model parameters and climate forcing effects. This adjustment more accurately represents the rigorous, quantitative analysis we've conducted, moving beyond a mere qualitative assessment.

By clarifying our focus on sensitivity analysis, we aim to better highlight the model's capabilities in simulating outbreak dynamics under various scenarios, without the immediate goal of replicating specific, real-world beetle outbreaks. This nuanced approach allows us to explore the model's robustness and predictive power within a controlled, theoretical framework, setting the stage for future studies that may aim for direct real-world outbreak simulations.

Minor Comments:

Comment: Is "total woody biomass" just wood in living plants, or does it also include (some?all?) Deadwood?
Response: Clarified to exclude deadwood, with additional text revisions for transparency.

Comment: Lines 294-301: This explanation should come before "ORCHIDEE formalizes this dependency" at line 282?
Response: Adjusted the sequence of paragraphs to ensure a logical flow of information, particularly in the methods section.

Comment: Line 319: How is RDI calculated?
Response: Included the equations and a detailed explanation for the calculation of the Relative Density Index (RDI).

Comment: Lines 376-377: Referring to these as "bark beetle outbreak development stages" would avoid confusion with "development" in the sense of physiological growth from larva to adult, as well as improve consistency with Table 2.
Response: Ensured consistent use of terms related to bark beetle outbreak stages, enhancing clarity and avoiding confusion with physiological development stages.

Comment: Lines 378-379: Does this refer to the hysteresis described in lines 264-278? If so, refer to Sect. 2.2.2 here. If not, please clarify.
Response: Revised the text to better convey the continuous process simulation of bark beetle dynamics, avoiding the misunderstanding that the model differs for different outbreak stages.

Comment: Line 407: Act is confusing here, because it's referring to activity both in the current year (previously in the text) and the next year (second part of this sentence). I suggest just removing it, or at least deleting the subscript.
Response: Removed potentially confusing subscripts and clarified the usage of the activity index in the model.

Comment: Line 499: Fig. 3 shows that these sites never left the endemic stage, but it's my understanding that trees can still be killed by bark beetles during that stage. If my

understanding is correct, please change "remained unaffected by bark beetles" to "never left the endemic stage" (unless you have other data, not shown, indicating that biomass loss to beetles was actually zero). If my understanding is incorrect, Sect. 2.2.5 should be improved.

Response: Improved the description to accurately reflect the impact of beetles during the endemic stage, based on the simulation results.

Comment: Fig. 6: How can the "background mortality only" treatment (i.e., no windthrow) be "after the windthrow event"?

Response: The term "background mortality" was replaced with "continuous mortality." This change is likely aimed at better reflecting the nature of the model conditions, where mortality is considered a constant factor. The mention of Figure 8 (updated from the original Figure 6) focusing on the contribution of outbreaks to carbon dynamics implies that the figures and possibly associated text have been revised to make the role of outbreaks in the model clearer, separate from the effects of windthrow events. Finally, we rewrite section 3.6 aimed at clarifying how the model accounts for mortality and its impact on carbon dynamics.

Comment: Sect. 4.2: Lines 567-571 should be moved to Sect. 2.4, because it provides great justification for what initially seemed a questionable choice. Then the remaining text doesn't really warrant a Discussion section; it's more Conclusion.

Response: Moved pertinent sentences to appropriate sections to enhance coherence and logical flow, particularly in relation to the justification for evaluation methods.

Comment: Lines 613-614: How does the study show anything about the importance of initial conditions? Where was that tested?

Response: We agree and We rewrite it to present this notion as an ancillary concept, albeit not substantiated by our research.

Comment: Conclusion: Also mention plans for more quantitative comparison against observed bark beetle events.

Response: included a statement in the conclusion highlighting future plans for a more quantitative comparison against observed bark beetle events, underscoring the ongoing development of the model.

We hope these revisions address the concerns and suggestions raised by Referee #2, as we believe that they significantly enhanced the manuscript's clarity, coherence, and scientific rigor.

Sincerely,

Guillaume Marie