Our comments are written *in italic red font*. Revised text is written in plain red text, for which we give reference to the beginning line number ***(LXXX)*** of the revised annotated version. Also, note that we have used EndNote for handling references and that changes to those do not always show up as revised.

Topic editor decision: Reconsider after major revisions

by Roslyn Henry

Public justification (visible to the public if the article is accepted and published):

The revisions have improved the paper however further revisions are required to adequately address previous reviewer comments with regards to clarity.

Additional private note (visible to authors and reviewers only):

The revisions required are not substantial but important nonetheless.

Improving the clarity of the paper following reviewer comments and addressing reviewer #1 comment regarding the discussion ("My previous criticism of the discussion, which contained general information about bark beetles rather than proper discussion of results, was not addressed. The overall revision of Discussion was minor/formal though it required rather thorough update.") is essential with this round of revisions.

Both myself and the reviewers find great value in this work and a more careful addressing of comments will see this work publishable.

Thanks for giving us the opportunity to revise and improve the manuscript, we have tried to put a good effort in properly discussing our results and generally clarifying the text.

Anonymous referee #1 report

1) Scientific significance: Does the manuscript represent a substantial contribution to modelling science within the scope of this journal (substantial new concepts, ideas, or methods)? **Good**

2) Scientific quality: Are the scientific approach and applied methods valid? Are the results discussed in an appropriate and balanced way (consideration of related work, including appropriate references)? Do the models, technical advances and/or experiments described have the potential to perform calculations leading to significant scientific results? **Good**

3) Scientific reproducibility: To what extent is the modelling science reproducible? Is the description sufficiently complete and precise to allow reproduction of the science by fellow scientists (traceability of results)? **Fair**

4) Presentation quality: Are the scientific results and conclusions presented in a clear, concise, and well structured way (number and quality of figures/tables, appropriate use of English language)? **Poor**

For final publication, the manuscript should be - **reconsidered after major revisions**

Were a revised manuscript to be sent for another round of reviews: **I would be willing to review the revised manuscript**.

Suggestions for revision or reasons for rejection

(visible to the public if the article is accepted and published)

Dear Editor, Dear Authors,

I read the revised paper several times and I am afraid I still cannot recommend it for publication. In my opinion, revisions made in response to my first review were not adequate. Parts of the manuscript remain overly technical, sometimes written in a tone of developer`s guidelines rather than scientific text. Conceptually, the logic of defining different process complexity across the model still remain unclear (see also me previous comments), without justifying this approach. I do not feel that the current response and the revision addressed this comment (L110, …. adopting empirical elements wherever possible). *We have completely rewritten this last part of the introduction to make it clearer what we aim for (**L117**):* To achieve this, a semi-empirical SBB damage model, with components of negative feedback from a dense SBB population, amplification of damage after storm felling, where warm and dry weather can trigger and contribute to sustained outbreaks and SSC functionality was developed. The model was calibrated based on storm and SBB damage statistics from four countries in Europe. This modelling concept represents key aspects of the interaction of SBB with climate and forest state, creating a tool for addressing climate change and forest management scenarios.

Most of my comments from the previous review round are still valid (I will justify this through a few cases below). Most of the revisions are formal (adding a sentence) rather than trying to solve the commented problem. *Sorry for this. We have now tried to take a more thorough evaluation of all comments.*

My previous criticism of the discussion, which contained general information about bark beetles rather than proper discussion of results, was not addressed. The overall revision of Discussion was minor/formal though it required rather thorough update. *We have gone through the discussion, rewritten sections of it, removed some more general material and made sure that all sections have references to our results. We noted that especially the beginning of the discussion tended towards being overly general and have especially focused our revisions there. We believe the later paragraphs are generally already closely tied to the results of our modelling.*

Here I indicate just a few cases:

- Revision made in L55 „… To further complicate the situation ….” uses inappropriate terminology (regeneration should probably be reproduction, infection -> infestation, etc.), which I already comment on in the first review. *We have gone through the entire manuscript and corrected the terms (**L61**, **L62**, **L494**, **L617**).* It also lacks any references to literature. Generally, scientific evidence that SSC can shorten and prevent the outbreak are rather unconvincing. *We have added several references regarding the impact of SSC throughout the manuscript, and have also tried to give a wider view of the uncertainty of the effect in the introduction (**L58**):* To further complicate the situation, counter measures such as salvage and sanitary cutting (SSC), which can be effective in preventing and shortening outbreaks of SBB (Jönsson et al., 2012; Stadelmann et al., 2013), have a high variation in intensity over time and space. Salvage logging of storm felled trees reduce the material where SBB can have very efficient reproduction and can even reduce the SBB population if the harvest is done between infestation and emergence of the new generation. Sanitary cutting of infested living trees will take away the new SBB generation if it is done at the first signs of infestation in summer. This can contribute to ending an outbreak situation. Sanitary cutting is less efficient when done in autumn or winter as only a fraction of the new beetles is still present (Singh et al., 2024). *In M&M (**L302**):* As the numbers for the SSC setting vary over time, regions and countries (Jönsson et al., 2012; Stadelmann et al., 2013; Wichmann and Ravn, 2001), we do not claim that we have found the most representative numbers for all of Europe but they rather should be seen as a starting point for evaluating SSC dependency in this modelling concept. *And in the discussion (**L616**):* In managed forest, however, the occurrence and timing of counter measures such as salvage logging of storm-felled trees, sanitary cutting of infested trees and insect traps can have a big impact on the outcome (Jönsson et al., 2012), factors that vary greatly in time and by region over Europe, as well as at finer scale (Wichmann and Ravn, 2001). Stadelmann et al. (2013), e.g., reports that higher fractions of wind damage were associated with lower intensity of salvage logging after 1999 storm Lothar in Switzerland, while after the Gudrun 2005 storm in Sweden extra resources were brought in from other parts of the country, as well as from abroad, to increase the rate of salvage cutting (Fridh, 2006). By timely salvage logging, a large fraction of the beetles can be removed from the forest, substantially dampening the damage done (Jönsson et al., 2012; Stadelmann et al., 2013). An indirect indicator of the effect of counter measures is that the risk for SBB infestations are higher with nature reserves in the landscape where no SSC is done (Kärvemo et al., 2023).

- Revision made in L110 justifying the philosophy of development. As I wrote above, this does not really helped to clarify my comment from my previous review. *In all honesty we are struggling with this one. Throughout paragraphs 2 (**L42**) and 3 (**L58**) of the introduction (revised version – previously one paragraph) we explain the important characteristics governing bark beetle outbreaks. In Paragraph 4 we outline previous process-based and empirical efforts to model bark beetle impacts and their strengths and weaknesses. Then in Paragraph 5 (**L97**) we outline the characteristics we are looking for in a model, including a list of what it should do. Clearly a model with uniform process-complexity would appear elegant but would not take account of simple realities; there are some things we know relatively well and can constrain at relatively high complexity – e.g. beetle phenology – and some things we consider conceptually very important to be able to explore scenarios, whilst other things we know less well and consider less conceptually important. We have made a change to the start of Paragraph 5 to put more upfront what the SBB model is intended to be used for and matched this with a new sentence at the end of the paragraph. We have also added references in the model description to why we have gone for a particular level of complexity for different components as we introduce them (see e.g. Paragraph 2 of Section 2.3; **L193**):* This additive model concept captures the dynamics of SBB outbreaks based on a tractable set of process-linked predictors that are empirically-supported at large scales. We therefore found this to be a strong basis for our approach and implemented an additive model drawing on this concept for bark beetle damage in LPJ-GUESS. In this implementation we took advantage of the existing LPJ-GUESS formulations for water stress and mortality of trees (L, stem litter from spruces larger than a limit (d_{lim}) killed by other agents than bark beetles last year) as inputs, as well as the available and proven bark-beetle phenology sub-model of Jönsson et al. (2007). *We hope that these steps make the reasoning for the choices clearer.*

- My previous comments:

(i) „Does this approach have any justification? To enable that an outbreak can also be sustained at the highest population levels … ;

(ii) „I have a similar concern as in the previous point concerning the implementation of salvage logging …….".

I do not consider the way these comments were addressed adequate. Concerning the former comment, there was just a single word change in this sentence (feedback to impact, L205). *We have revised the sentence to make it more general and added a reference (**L228**):* To enable that an outbreak can be sustained also during the epidemic phase with the highest population levels (Hlásny et al., 2021), the lowest possible total negative feedback from population size was set to be just below the highest possible positive impact of water stress and phenology. *In response to referee #2 we have also made clarifications to the entire section and Figure 1 (see specific referee#2 comment 2.2 below).* Concerning the latter comment, it concerned the lack of justification on real dampening effect of salvage logging on outbreak in the presented implementation. In the revision, the comments was addressed just by referring to the Swedish Forestry Act, which cannot be considered as a relevant scientific source. *As written above, we have now added several references regarding the impact of SSC throughout the manuscript and have also tried to give a wider view of the uncertainty of the effect (see comment above with reference to **L58**, **L302** and **L616**).*

- My previous comment: .. It is really extremely difficult to understand the logic of such statement: „As storms in Europe mainly occur in autumn and winter … I did not find the revision helpful, the sentence still remains extremely complicated. *We have divided the sentence to make the message clearer. We also changed from "storm season" to "storm year" and are referring to the well-established "water year" term that has a similar usage (**L324**):* Storms in Europe occur mainly in autumn and winter, and the modelled vegetation and bark beetle effect, e.g., should be the same for a storm event in October or in February the next year. The storm damage statistics for a specific year were, therefore compiled for a storm year (c.f. the commonly used "water year" term, Johnstone and Cross, 1949) of 12 months from July until June the next year when building the dataset used for the calibration.

- My previous comment "As far as I understand this part, the authors fixed a poor match of simulated wind damage with observations by introducing a correction by latitude, which correlated with productivity in Sweden (unpublished data?), and because there is higher damage in more productive sites, it should help simulate wind damage better. If this is correct, it looks like rather artificial solution for improving model performance. If the wind module is coupled with the vegetation model, should not the productivity and subsequently wind susceptibility be simulated as an emergent property? Without a need to imprint there this pattern externally" was not addressed by the current response. The point was why productivity gradient needs to be imprinted into the simulations through latitudinal gradient, if vegetation dynamics model is part of the implementation. The current response just says that productivity is highly correlated with latitude, based on the Swedish forest statistics. *We recognise that the discussion of latitude as a proxy of productivity gave the impression that we intended this calibration as something more than simply adjusting wind damage to the observed level in order to focus on the bark beetle calibration. We have thus removed that text and added a sentence to clarify this (**L355**):* As we wished to use the existing wind module's capability to distribute wind damage among cohorts' in different patches depending on their sensitivity (Eq. 1), the model could not be driven directly by the available observations of DF_{storm}. Therefore, instead of calculating WL from the cubed exceedance of the 99.5 percentile of daily wind speed accumulated over storm season as in Lagergren et al. (2012), a calibration was done to adjust WL so that modelled damage followed the observed DF_{storm} (denoted WL_{stat}). *We have also made some additional revision to the section for clarity (**L373**):* As a common linear scaling was used to go from DF_{storm} to WL_{stat}, the exact DF_{storm} time series will not be reproduced by this approach, but the pattern and level should be reasonable well captured. The WL_{stat} time series were then used as external input to the model runs. This procedure was followed purely to provide an observationally consistent wind damage level for the calibration of the SBB model and is not intended as a European-scale parameterisation of the Lagergren et al. (2012) wind module, which will be carried out in a separate study.

Moreover, language revision is highly needed, many sentences does not read well or are inaccurate. *We have checked the language for the entire manuscript.* In abstract, for example:

- L10 Tree mortality is an important function in such models and, especially for needle leaved forest in the temperate and boreal zones, bark beetles are important for the mortality pattern. (quite difficult to read). *The first three sentences have been revised (**L9**):* For exploring forest performance in the future, dynamic vegetation models are important tools. Tree mortality is a crucial process in these models, but explicit representations of major agents of mortality have often been relatively underdeveloped. In needle-leaved forest in the temperate and boreal zones, bark beetles are often important for the mortality pattern.

- L15 Drought can contribute to increased damage and prolonged outbreaks by lowering the defence of the trees, but has been the main driver of some of the European forest damage in the last decade. (I cannot understand what this sentence communicates). *We have made clarifications to the sentence (**L15**):* Drought can contribute to increased damage and prolonged outbreaks by lowering the defence of the trees, but it has also been the main ultimate driver of some of the European forest damage by bark beetles in the last decade.

- L20 An index of the SBB population size that changed over time driven by phenology, drought, storm felled spruce trees and density of the beetle population, was used to scale modelled damage. (I also needed to read the sentence repeatedly. Why not to write directly … The modelled damage was driven by bark beetle phenology …). *We have revised accordingly (**L22**):* The model was driven by SBB phenology

- … but there were discrepancies in levels, which partly can be related to salvage logging of storm felled forest and sanitary cutting of infested trees. (I did not find in the paper any proof that the discrepancy was caused by salvage and sanitation logging; the reason can also be limitations in the implementation or in general understanding of processes, etc.). *In Figure 5 we show that the model results are very sensitive to the setting of SSC. And with the subtler presentation of the SSC uncertainty (see comments above), we have only made a minor modification to this sentence (**L23**):* The model was able to catch the start and duration of outbreaks triggered by storm damage reasonably well, but with discrepancies in levels which can be at least partly related to salvage logging of storm felled forest and sanitary cutting of infested trees.

- …. which may suggest that the drought stress response of spruce in LPJ-GUESS is underestimated (models generally suffer from limited connection of tree stress physiology and biotic agents, which is more complex that just stress underestimation, see e.g., https://doi.org/10.1016/j.tree.2021.02.001). *We removed the word "stress" here in the abstract (**L27**) and now mention this complexity, using the suggested reference, in the discussion (**L659**):* It is also clear that physiological drought modelling is a challenge (Trugman et al., 2021).

- L50 – not true, thinning, on the contrary, reduces stand density and increases resistance to beetles. *Yes that is right, but the thinning effect is much more complicated than that, which we now explain (**L53**):* Thinning may increase the trees' resistance by reducing competition (Singh et al., 2024) but, on the other hand, it can cause direct damage by the logging operation (Hwang et al., 2018), introduce root rot (Vollbrecht and Agestam, 1995) and make the stand more prone to wind damage (Nielsen, 1995). It also increases the growth rate of the remaining trees, making them accessible to SBB at a younger age.

Also later in the text, many sentences are very difficult to read, are overly long and complex, often using technical jargon. E.g.:

- L257: The dependency of ASP instead of, e.g., the number of generations per year was chosen as ASP is a continuous variable which better catch the average when there is a variability in the climate, such in mountainous regions, then a discrete variable. (impossible to understand). *The sentence has been revised (**L284**):* The use of ASP instead of a discrete variable such as the number of generations per year, was chosen as ASP is a continuous variable which better catch the average conditions within a climate grid cell when there is a high variability in temperature, such as in mountainous regions. Furthermore, a well-defined model with constrained parameters already existed for ASP (Jönsson et al., 2011).

In my opinion, the manuscript contains a substantial amount of good work and valuable results. However, the reproducibility and clarity of this work for a broader readership remains limited. Therefore, I cannot recommend its publication in the current form.

Anonymous referee #2 report

1) Scientific significance: Does the manuscript represent a substantial contribution to modelling science within the scope of this journal (substantial new concepts, ideas, or methods)? **Good**

2) Scientific quality: Are the scientific approach and applied methods valid? Are the results discussed in an appropriate and balanced way (consideration of related work, including appropriate references)? Do the models, technical advances and/or experiments described have the potential to perform calculations leading to significant scientific results? **Excellent**

3) Scientific reproducibility: To what extent is the modelling science reproducible? Is the description sufficiently complete and precise to allow reproduction of the science by fellow scientists (traceability of results)? **Excellent**

4) Presentation quality: Are the scientific results and conclusions presented in a clear, concise, and well structured way (number and quality of figures/tables, appropriate use of English language)? **Good**

For final publication, the manuscript should be - **accepted subject to minor revisions**

Were a revised manuscript to be sent for another round of reviews: **I would be willing to review the revised manuscript**.

Suggestions for revision or reasons for rejection

(visible to the public if the article is accepted and published)

I was reviewer 2 on the initial manuscript; the authors have done a good job responding to my initial comments. I have some more, but I recommend this be published subject to what I think are minor revisions. The only reason I think I would need to review again would be if there still seems to be confusion about my comment 13 on my original review (which I'll refer to as my comment 1.13; see comment 2.6 below).

All line numbers refer to the "authors' tracked changes" version of the manuscript.

Comments on replies to my initial review (mostly):

2.1) Re: my comment 1.6: I think I'm satisfied with disturbance being turned o in this experiment, given the likely low rate in this region. *Good.*

2.2) L194-201: "The fraction of the different age and management classes, from the landcover functionality in LPJ-GUESS, were used to calculate Pgridcell weighted over the classes. With all variables in the (Marini et al., 2017) model at +/2 standard deviation from the mean, R has a range of -4.66 – 3.36, but interactions between variables prevent it to reach higher numbers. R calculated from the observation data used in the present study (see 2.4 below) has a range of -2.2 – 2.9, but initial high numbers in the start of an outbreak were often missing as inventories only began when already under an outbreak situation. The total range of R (Fig. 1a) in the presented model was set to -

3.8 – 6.0, where the possible outcome range of the different parts of the model (Eq. 4, Fig. 1a) were given weights of the similar magnitude as in the (Marini et al., 2017) model."

- Refer to Sect. 2.6 where you calculate the -3.8 number. *The -3.8 number is actually not calculated in 2.6 but is motivated and based on the reasoning on **L215-224**, which we have tried to clarify further:* In the Marini et al. (2017) model with all variables set at +/- 2 standard deviation from the mean, *R* has a range of -4.66 – 3.36. Interactions between variables prevent R from reaching higher numbers. *R* calculated in this way from the observation data used in the present study (see 2.4 below) has a range of -2.2 – 2.9. It should be considered, however, that in both cases initial high numbers in the start of an outbreak were often missing as inventories only began when already under an outbreak situation. In the present application, *R* is applied at patch level while observed data have been aggregated over large regions or countries, a minimum number closer to the Marini et al. (2017) value is therefore motivated. At the other end of the scale, the maximum *R* can also be translated to an extreme case of population increase rate with two successful generations in a year with 20 female offspring per mother (e^6 = 20.12). *We have also clarified the base for this setting in the next sentence (**L224**):* Based on this, the total possible range of *R* (Fig. 1a) in the presented model was set to -3.8 – 6.0, where the possible outcome range of the different parts of the model (Eq. 4, Fig. 1a) were given weights (relative contribution to the total range) of the similar magnitude as in the Marini et al. (2017) model.
- What are the "weights" here that you are "giv[ing]" the model components? I don't see any weights in Eq. 4, and the only weights previously mentioned have to do with the different age/management classes. *We now explain what we mean by "weights" in this context (**L226**):* (relative contribution to the total range)
- Is the "The total range of R" another setting you applied, or is it just the emergent result of the "weights" you gave? *We have made some clarifications to this sentence (**L224**):* Based on this, the total possible range of *R* (Fig. 1a) in the presented model was set to -3.8 – 6.0, where the possible outcome range of the different parts of the model (Eq. 4, Fig. 1a) were given weights (relative contribution to the total range) of the similar magnitude as in the Marini et al. (2017) model. *And also to the Figure 1 text (**L235**):* Figure 1: The different components of the increase rate of the bark beetle population index (R). Depending on the state of the model and climate, R and its components take a value within the possible ranges. (a) Possible ranges of the components and the total (Eq. 4). For $f(P_{gridcell})$ and $f(P_{patch} / L)$, the light shaded areas show the part of the ranges that were varied in the parameter optimization, where the sum of the minimum of $f(P_{gridcell})$ and $f(P_{patch} / L)$ was kept constant to have the possible total negative feedback from the population index constant (illustrated with the dotted lines, see section 2.6). (b-e) Shape of the functions for the components of the increase rate of the bark beetle population index (*R*), b Eq. 7, c Eq. 8, d Eq. 10, e Eq. 12. The default parameter setting (Table 7) is shown by thick grey lines (b-d). The functions are also shown in colour for the min and max value of parameters included in the calibration and sensitivity analysis (section 2.6), using the default setting for the other parameters. For *f*(phenology), (e), no parameters were tested but the response depends on the grid-cell's 30-year running mean of the length of the autumn swarming period (ASP, ASP30) and function are shown for ASP30 from 2 to 75 days. *We also added the word "possible" to the upper bar in figure 1a.*

2.3) Fig. 1a: Are the light parts the only ranges you used for optimization, or did you also include the dark part? And what is the dark part—the final allowed range? *If k_{gc_min} is at its max value then k_{p_min} is at its min value and vice versa, we have tried to make this clearer by connecting those points by dotted lines in Figure 1a, and it is further explained in the figure text (**L238**):* the sum of the minimum of $f(P_{gridcell})$ and $f(P_{patch} / L)$ was kept constant to have the possible total negative feedback from the population index constant (illustrated with the dotted lines, see section 2.6) *and is later further explained (**L408**):* To further reduce the number of calculations and to keep the total weight of the population size dependency constant, the sum of the minimum of the ranges $f(P_{gridcell})$ and $f(P_{patch} / L)$ was kept constant ($k_{gc\_min} + k_{p\_min} = -3.8$).

2.4) Fig. 1b: Was k2 min intentionally changed from pink to orange? If so, why? *Thanks for being observant, this was a mistake, k_2 min is now pink.*

2.5) Before showing Equations 7 and 8, remind the reader in words what those terms are supposed to represent (respectively: effect of landscape-scale and substrate-scale competition [or the relief thereof, at low densities]). *We now do this (**L247**):* The representation of landscape scale and substrate scale competition (or relief of competition at low densities) was formulated in two equations (Eq. 7 and 8), respectively.

2.6) Re: my comment 1.13: I don't think I was clear. Looking at Eq. 9, and the text directly after (as the authors mentioned in their reply), I'm only seeing change in L due to tree mortality: "Lmort is C mass of stem mortality of spruce trees above a diameter threshold (dlim) for previous year caused by other reasons than bark beetles (including storm)." That's not an "absolute amount" as the authors said in their reply; the way I read that sentence, Lmort is one influx to L. That's because mortality is a flux variable, not a state variable. Why would L only include one year's influx rather than the total amount of stem litter? Do beetles not eat wood that's been dead for more than a year? *From this comment it is clear that we need to explain how the bark beetles live in the bark. The beetles do not eat wood and they can not breed in trees that has been dead for more than 1-2 years, which we now clarify. By this it was also natural to define what a "gallery" is (comment 2.7 below) (**L259**):* The beetle larvae feed from the phloem in galleries under the bark (Six and Wingfield, 2011). The amount of phloem depends on thickness and area of the bark, and is closely related to stem biomass. The trees are normally no longer suitable for breeding one or two seasons after tree death (Göthlin et al., 2000; Louis et al., 2014), which is why L_{mort} is based only on the mortality of the previous year.

2.7) Re: my comment 1.24: The authors reply that they "think that most readers are familiar with the gallery term," but least 50% of the readers so far (i.e., me) weren't. I'm a vegetation modeler with no entomological background, which I suspect describes many of this article's potential readers. It should be defined. *We now explain this in M&M (see comment 2.6 above).*

2.8) Re: my comment 1.27: I guess what I was saying is that I don't understand how this bit relates to the rest of the paragraph. It would be good for the authors to draw that connection. *We have now tried to make a better flow of the last part of the paragraph connecting the sentences (**L641**):* The outcome after a trigger also depends on the initial level of the SBB population, but many SBB models require an initial population or damage level, which means that they only need to work in relative terms (Marini et al., 2017; Soukhovolsky et al., 2022). A dynamic forest model, on the other hand, needs to operate with absolute damage levels making the modelling more challenging.

2.9) Re: my comment 1.28: Still, "but" seems wrong. The part of the sentence after the comma seems to be supporting the part of the sentence before, rather than contradicting it. *We get the point and have moved some parts after the comma to beginning (**L659**):* Warm weather accompanied by drought is often seen as a factor contributing to sustained outbreaks (e.g. Bakke, 1983), but in recent year it has also triggered outbreaks of SBB in Europe (Nardi et al., 2023; Trubin et al., 2022).

Other comments

2.10) L75: "but has **only** been evaluated". *Fixed (**L84**).*

2.11) L87: "are" should be "is". *Fixed (**L100**).*

2.12) L157: "BLAZE" should be "SIMFIRE-BLAZE": BLAZE is just the fire impacts module, with SIMFIRE giving burned area. There should be a citation here of the first LPJ-GUESS SIMFIRE paper: Knorr et al. (2016, Nat. Clim. Chg., doi: 10.1038/nclimate2999). BLAZE unfortunately hasn't been published; the most complete description is as part of Rabin et al. (2017, GMD, doi: 10.5194/gmd-10 1175-2017). *References added (**L172**):* Fire disturbance was simulated with the SIMFIRE-BLAZE module (Knorr et al., 2016; Rabin et al., 2017), and it was also turned off for managed patches.

2.13) L186: "R for M (Eq. 5)" should be "M on R (Eq. 6)". *What is actually right is* "M on P_{patch} (Eq. 6)"*, which we now write (**L205**):*

2.14) L362: "where" should be "were". *Fixed (**L405**).*

2.15) L483: "spruce'" should be "spruce's". *Fixed (**L528**).*

2.16) L490: Delete "of a". *Fixed (**L532**).*

2.17) L567-569: This new sentence is too vague, or maybe it's hard to understand why it's included. It needs to be tied in more clearly with the story being told in this paragraph. *We have edited the section to make it clearer how the reference fits in the context (**L670**):* This may be a failing of the model parameterization for water stress or of the input climate forcing dataset (Steinkamp and Hickler, 2015). Similar results were also found for the most recent application of SBB damage with the ORCHIDEE vegetation model, concluding a shortcoming linked to high damage levels associated with extreme drought (Marie et al., 2024).

2.18) L575: "has" should be "have". *Fixed (**L680**).*