nature portfolio

Peer Review File

Climate change accelerates global forest deadwood dynamics

Corresponding Author: Mr Pascal Edelmann

This file contains all editorial decision letters in order by version, followed by all author rebuttals in order by version.

Version 0:

Decision Letter:

** Please ensure you delete the link to your author home page in this e-mail if you wish to forward it to your coauthors **

Dear Mr Edelmann,

Please allow us to apologize for sending this delayed decision on your manuscript titled "Climate change accelerates global forest deadwood dynamics". It has now been seen by 2 reviewers, whose comments are appended below. You will see that they find your work of some potential interest. However, they have raised substantial concerns that must be addressed. In light of these comments, extensive revisions will be required before we can further consider the manuscript for publication. We would, however, be interested in considering a revised version that fully addresses these serious concerns.

In the revision, please address the editorial thresholds as follows:

* Provide a compelling case that global forest deadwood dynamics intensify under climate change, including full primary factors and clarifying parameter estimations in the model prediction.

* Explicitly address the underlying assumptions of the methods, particularly on effects on deforestation and wildfires.

We hope you will find the reviewers' comments useful as you decide how to proceed. If additional work allows you to either incorporate or refute these criticisms, we will be happy to look at a substantially revised manuscript. If you choose to take up this option, please either highlight all changes in the manuscript text file, or provide a list of the changes to the manuscript with your responses to the reviewers.

When resubmitting, please provide a point-by-point response to the reviewers' comments. Please submit your responses as a separate file, distinct from your cover letter where you can add responses to the Editors' comments that you do not want to be made available to the reviewers. Word files are preferred. We recommend that any figures, tables or graphs that are included in the response to reviewers are also included in the main article or Supplementary Information.

Please bear in mind that we will be reluctant to approach the reviewers again in the absence of substantial revisions.

If the revision process takes significantly longer than three months, we will be happy to reconsider your paper at a later date, as long as nothing similar has been accepted for publication at Communications Earth & Environment or published elsewhere in the meantime.

We are committed to providing a fair and constructive peer-review process. Please do not hesitate to contact us if you wish to discuss the revision in more detail.

Please use the following link to submit your revised manuscript, point-by-point response to the reviewers' comments with a list of your changes to the manuscript text (which should be in a separate document to any cover letter), a tracked-changes version of the manuscript (as a PDF file) and any completed checklist:

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** This url links to your confidential home page and associated information about manuscripts you may have submitted or be reviewing for us. If you wish to forward this email to co-authors, please delete the link to your homepage first **

Please do not hesitate to contact us if you have any questions or would like to discuss the required revisions further. Thank you for the opportunity to review your work.

Best regards,

Lifen Jiang, PhD
Editorial Board Member
Communications Earth & Environment
orcid.org/0000-0002-1546-8189

Mengjie Wang
Associate Editor, Communications Earth & Environment
Consulting Editor, Communications Sustainability
Bluesky: @commsearth.nature.com; @ commssustain.nature.com

EDITORIAL POLICIES AND FORMAT

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You can download the form here:
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REVIEWER COMMENTS:

Reviewer #1 (Remarks to the Author):

Review for COMMSENV-25-2703

Edelmann et al. “Climate change accelerates global forest deadwood dynamics",

The authors estimated future trajectories of tree growth and mortality rates using five dynamic global vegetation models, and deadwood decomposition rates based on a statistical model responsive to temperature and precipitation. They analyzed model responses to climate change projections.
According to their results, deadwood dynamics intensified throughout the 21st century, with both inputs and outputs of global deadwood carbon pools increasing. Because deadwood formation increased about 5.0% more strongly than decomposition, the global deadwood stocks were projected to increase. Considerable geographic variation of the I/O ration was reported (Fig. 3).

The results are instructive and novel, and the manuscript is reader friendly.

I propose the following changes:

Please highlight the role of one DVGM (LPJ-GUESS) in comparison to the four others in obtaining the results and the line of thought. Some models suggest negligible I/O change with prolonged climate change (Fig. 4 of the manuscript).

Please refer to Pan et al (2024; 1) suggesting insignificant change of global dead wood pool from 1990 to 2020, with decreases in tropical forests compensating increases in boreal forests. The conversion of intact forests to other land uses has been a prime reason for dead wood losses in the tropics. Please discuss uncertain projections on global deforestation. The historical net loss of global forests has been estimated at about 200 million ha forests from 1990 to 2020, e.g. (1). Any significant change in this rate makes a difference to the global dead wood pool by the year 2100.

Please present DGVM estimates of the rate of movement of forest biomes (especially the rate of northward shift of the Arctic timberline, in m/yr), as implied in Fig. 3. Is the estimated rate too fast to allow angiosperms and/or gymnosperms to keep pace?

Please elaborate more extensively on combustion emissions in comparison to decomposition emissions. Wildfires prompt deadwood decay, and there are combustion emissions always involved. The refence (2) is anecdotal reporting a relatively small forest fire (0.2 Mha) in a coastal region (Oregon). Fires in continental Canada and Siberia have been much larger (3,4). The rate of increase of the atmospheric CO2 concentration has been record high in 2023 and 2024, probably influenced by exceptional combustion emissions from forest fires in Siberia and Canada (e.g. 5). There can be important mechanisms of wild-fire ignition, which the statistical decay model does not take correctly account.

In summary, the manuscript is a well-written presentation of global dead wood dynamics based on established methodologies. I encourage authors to address more explicitly some of the underlying assumptions of their methods, especially the uncertain roles of deforestation and fire on global lands (see 6). With these notes I recommend accepting the manuscript for publication.

1. Pan, Y., Birdsey, R. A., Phillips, O. L., Houghton, R. A., Fang, J., Kauppi, P. E., ... & Murdiyarso, D. (2024). The enduring world forest carbon sink. Nature, 631(8021), 563-569.
2. Campbell, J. L., Fontaine, J. B., & Donato, D. C. (2016). Carbon emissions from decomposition of fire-killed trees following a large wildfire in Oregon, United States. Journal of Geophysical Research: Biogeosciences, 121(3), 718-730.
3. Byrne, B., Liu, J., Bowman, K. W., Pascolini-Campbell, M., Chatterjee, A., Pandey, S., ... & Sinha, S. (2024). Carbon emissions from the 2023 Canadian wildfires. Nature, 633(8031), 835-839.
4. Delcourt, C. J., Rogers, B. M., Akhmetzyanov, L., Izbicki, B., Scholten, R. C., Shestakova, T. A., ... & Veraverbeke, S. (2025). Carbon emissions from fires in Eastern Siberian larch forests. Global Change Biology, 31(5), e70247.
5. Luo, B., Luo, D., Dai, A., Xiao, C., Simmonds, I., Hanna, E., ... & Gong, T. (2024). Rapid summer Russian Arctic sea-ice loss enhances the risk of recent Eastern Siberian wildfires. Nature Communications, 15(1), 5399.
6. Pyne, S. J. (2022). The Pyrocene: How we created an age of fire, and what happens next. Univ of California Press.

Reviewer #2 (Remarks to the Author):

Forest deadwood greatly affects the global carbon cycle, but its stock and dynamics are difficult to quantify. Edelmann et al. present global maps of forest deadwood stock using a map of current global deadwood stock, DGVMs, and a climate-dependent deadwood decomposition function. While this study represents a substantial effort and addresses a highly relevant topic, the current version has several methodological issues. Please see my detailed comments and questions below.

1. The methods and supplementary materials lack a detailed description of how deadwood decomposition was upscaled from experimental sites to the global scale. I assume the data were derived from Seibold et al. (2021, Nature). However, Seibold et al. showed that insects and their interactions with microorganisms strongly affect deadwood decomposition, especially in tropical areas. This factor is not included in the GAM upscaling, which is only climate-dependent. Additionally, there was no information provided about the decomposition data itself (distribution, spatial coverage, etc.), the data sources of predictors, or the performance of the GAM.

2. The authors scaled DGVM-derived deadwood formation rates to current values to match them with current deadwood stocks and decomposition rates (lines 362–363). However, it is unclear how the scaling factors were derived. Since the current deadwood stock is only available for 2010, there may be equifinality issues in retrieving these factors. If the authors used $dC_{2000\text{-}2010}/dt = \alpha * I_{2000\text{-}2010} - D_{2000\text{-}2010}$ and $dC_{2000\text{-}2010}/dt = \alpha * I_{2000\text{-}2010} - D_{2000\text{-}2010}$ to estimate α as the scaling factor (where I is the default formation rate and D is the estimated decomposition rate), how were the deadwood stocks in 2000 and 2010 calibrated? Please clarify the calculation of scaling factors with explicit equations.

In Figure 4: Since decomposition rates were upscaled based on site results, the temperature effect on decomposition should be consistent across models. Similarly, α is fixed across time and temperature gradients, and the scalar α applied in the previous step does not correct the real deadwood input rate across time. The difference in the temperature–I/O ratio should therefore depend solely on the relationship between temperature and the default tree mortality rate in each model. Therefore, the conclusions drawn from Figure 4 (and Figure 2) are not fully convincing.

Additionally, most DGVMs assume steady state. A large scaling of the formation rate (e.g., multiplying by 13.3 for tropical gymnosperms in SEIB-DGVM) could imply that ecosystem GPP and allocation are much lower than the allocation required for aboveground wood each year. This breaks the overall carbon balance.

3. Fire may contribute more to deadwood consumption than direct decomposition, especially in fire-prone regions. Similarly, harvest in managed forests is not accounted for. As a result, this study may overestimate both deadwood stock and decomposition rates to match the initial carbon stock map.

The age–carbon relationship is another issue, though the authors briefly acknowledge it in the discussion. Neglecting these factors could bias both the deadwood mapping and the I/O estimates. To strengthen the study, I recommend including more measured data to validate the robustness of the derived deadwood stock time series and decomposition rates, ideally, the I/O ratios as well.

Minor:
Figure 2: The panel shows the SD for carbon fluxes and I/O ratios. It would be more informative to show the full spread. Since LPJ-GUESS accounts for more than 50% of ensemble members, the spread and temporal trend are largely driven by this single model. Is there a way to balance model contributions in the final figure presentation?

Title: The title may be slightly misleading, as only climate change is considered. There are no interactive effects among factors (e.g., climate × management, climate × disturbance).

Version 1:

Decision Letter:

** Please ensure you delete the link to your author home page in this e-mail if you wish to forward it to your coauthors **

Dear Mr Edelmann,

Your manuscript titled "Climate change accelerates global forest deadwood dynamics" has now been seen by our reviewers, whose comments appear below. In light of their advice we are delighted to say that we are happy, in principle, to publish a suitably revised version in Communications Earth & Environment.

We therefore invite you to revise your paper one last time to address the remaining concerns of our reviewers. At the same time we ask that you edit your manuscript to comply with our format requirements and to maximise the accessibility and therefore the impact of your work.

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Please outline your response to each request in the right hand column. Please upload the completed table with your manuscript files as a Related Manuscript file.

If you have any questions or concerns about any of our requests, please do not hesitate to contact me.

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We hope to hear from you within two weeks; please let us know if you need more time.

Best regards,

Lifen Jiang, PhD
Editorial Board Member
Communications Earth & Environment
orcid.org/0000-0002-1546-8189

Mengjie Wang
Associate Editor, Communications Earth & Environment
Consulting Editor, Communications Sustainability
Bluesky: @commsearth.nature.com; @commssustain.nature.com

REVIEWERS' COMMENTS:

Reviewer #1 (Remarks to the Author):

Thank you for your interesting piece of work. I appreciate the way you responded to my comments and do not wish to make any further remarks.

Reviewer #2 (Remarks to the Author):

The authors have addressed most of my previous comments, and I really enjoyed reading this version. I have just two minor suggestions/questions for consideration below.

Regarding my previous comments about steady state and lines 384–386: Did the authors apply the scaling factors during the spin-up period for the projection? If not, could the absence of scaling factors affect the “equilibrium” soil conditions? Also, do any soil variables influence wood formation (e.g., through tree mortality or wood density) or decomposition (e.g., via moisture or nitrogen content)? In other words, would it be possible to show that the deadwood stock in 2000–2020, using the adjusted scaling factors for projection, is still close to the initial deadwood stock reported by Seibold et al. (2021)? This would help confirm the consistency of the equilibrium approach.

Regarding the title, my understanding of “climate change” includes changes in both climate variables and climate-induced disturbances (fire, droughts, insect outbreaks). From the title alone, I initially expected that disturbances were considered, and lines 77–80 also highlight their importance. It only becomes clear around lines 117–119 that disturbances are not included. Clarifying this earlier in the text or adjusting the title could help align reader expectations with the study focus.

Reviewer #1 (Remarks to the Author):

Review for COMMSENV-25-2703

Edelmann et al. “Climate change accelerates global forest deadwood dynamics",

The authors estimated future trajectories of tree growth and mortality rates using five dynamic global vegetation models, and deadwood decomposition rates based on a statistical model responsive to temperature and precipitation. They analyzed model responses to climate change projections. According to their results, deadwood dynamics intensified throughout the 21st century, with both inputs and outputs of global deadwood carbon pools increasing. Because deadwood formation increased about 5.0% more strongly than decomposition, the global deadwood stocks were projected to increase. Considerable geographic variation of the I/O ration was reported (Fig. 3).

The results are instructive and novel, and the manuscript is reader friendly.

Response: Thank you for the positive assessment of our work and for the helpful comments on how to improve it further!

I propose the following changes:

Please highlight the role of one DVGM (LPJ-GUESS) in comparison to the four others in obtaining the results and the line of thought. Some models suggest negligible I/O change with prolonged climate change (Fig. 4 of the manuscript).

Response: Thank you for this important suggestion! We agree that substantial model uncertainty is a main finding of our study. We also agree that paying particular attention to the effect of LPJ-GUESS, which has contributed the most data points to our analysis, is important. In response to the suggestion by the Reviewer we now reran our analysis also with a balanced model dataset, i.e. where all models received equal weight in the analysis. While this results in a weaker overall signal, the main finding of a positive I/O ratio for deadwood remains also in this analysis. The newly conducted tests can be found in Supplement Figure S3 and Supplement Figure S4, and we have adapted the text in lines 165-168 and 409-412 to reflect these newly obtained results.

Please refer to Pan et al (2024; 1) suggesting insignificant change of global dead wood pool from 1990 to 2020, with decreases in tropical forests compensating increases in boreal forests. The conversion of intact forests to other land uses has been a prime reason for dead wood losses in the tropics. Please discuss uncertain projections on global deforestation. The historical net loss of global forests has been estimated at about 200 million ha forests from 1990 to 2020, e.g. (1). Any significant change in this rate makes a difference to the global dead wood pool by the year 2100.

Response: Thank you for this suggestion, this is indeed an important aspect that deserves to be addressed explicitly in our text. We have now revised the discussion to explicitly refer to the recent developments as reported by Pan et al. (2024), providing more context for the reader (lines 259-262).

Please present DGVM estimates of the rate of movement of forest biomes (especially the rate of northward shift of the Arctic timberline, in m/yr), as implied in Fig. 3. Is the estimated rate too fast to allow angiosperms and/or gymnosperms to keep pace?

Response: While we agree that changing forest extent (and its effects on ecosystem ecology) is an interesting topic, we here restricted our analysis to areas that were forested in 2010. In other words, we did not explicitly consider the effect of arctic tree line shift the Reviewer refers to in our analysis. This is stated explicitly in the methods section (lines 413-418), and we have now also amended the figure caption of Figure 3 to make this aspect clear for the reader and avoid potential confusion.

Please elaborate more extensively on combustion emissions in comparison to decomposition emissions. Wildfires prompt deadwood decay, and there are combustion emissions always involved. The refence (2) is anecdotal reporting a relatively small forest fire (0.2 Mha) in a coastal region (Oregon). Fires in continental Canada and Siberia have been much larger (3,4). The rate of increase of the atmospheric CO2 concentration has been record high in 2023 and 2024, probably influenced by exceptional combustion emissions from forest fires in Siberia and Canada (e.g. 5). There can be important mechanisms of wild-fire ignition, which the statistical decay model does not take correctly account.

Response: We agree that fire is a globally important mechanism of C release from forests, and that fire can considerably modify decomposition processes. We note, however, that we here do not report C release from decomposition (but rather focus on a change in the mass balance of deadwood, noting that an important share of the C that is lost from deadwood is sequestered in soil organic matter), and hence a direct comparison of the fluxes calculated here to estimated C fluxes from wildfire is not meaningful. That being said we agree that fire deserves to be acknowledged more explicitly, which is why we have added a sentence detailing the role on fire in the discussion (lines 251-252 and 254-256) and have also included the references suggested by the Reviewer.

In summary, the manuscript is a well-written presentation of global dead wood dynamics based on established methodologies. I encourage authors to address more explicitly some of the underlying assumptions of their methods, especially the uncertain roles of deforestation and fire on global lands (see 6). With these notes I recommend accepting the manuscript for publication.

Response: Thank you for your positive evaluation of our work, and for the helpful comments to improve it further! We appreciate the time and effort that went into the review of our manuscript!

1. Pan, Y., Birdsey, R. A., Phillips, O. L., Houghton, R. A., Fang, J., Kauppi, P. E., ... & Murdiyarso, D. (2024). The enduring world forest carbon sink. Nature, 631(8021), 563-569.

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6. Pyne, S. J. (2022). The Pyrocene: How we created an age of fire, and what happens next. Univ of California Press.

Reviewer #2 (Remarks to the Author):

Forest deadwood greatly affects the global carbon cycle, but its stock and dynamics are difficult to quantify. Edelmann et al. present global maps of forest deadwood stock using a map of current global deadwood stock, DGVMs, and a climate-dependent deadwood decomposition function. While this study represents a substantial effort and addresses a highly relevant topic, the current version has several methodological issues. Please see my detailed comments and questions below.

Response: Thank you for your comments on our work and for highlighting the complexity and relevance of the topic! Thank you also for your methodological suggestions and for the helpful comments on how to improve our work further. Our detailed responses to the points raised can be found below.

1. The methods and supplementary materials lack a detailed description of how deadwood decomposition was upscaled from experimental sites to the global scale. I assume the data were derived from Seibold et al. (2021, Nature). However, Seibold et al. showed that insects and their interactions with microorganisms strongly affect deadwood decomposition, especially in tropical areas. This factor is not included in the GAM upscaling, which is only climate-dependent. Additionally, there was no information provided about the decomposition data itself (distribution, spatial coverage, etc.), the data sources of predictors, or the performance of the GAM.

Response: Thank you for highlighting this issue, as it shows that we inadequately described our methods with regard to the decomposition model used. We indeed used the same model as in Seibold et al., and focused on the subset of their data that considers both insect and fungal decomposer communities, i.e. the data that best represent real-world conditions for deadwood decomposition. This information was now explicitly added to the methods (in lines 340-341), and we thank the Reviewer for pointing out this imprecision in our initial text. As for the distribution of the underlying raw data, we agree that this is important information for the reader and have hence added a map showing the data points to the supplement (Supplementary Figure S2). Driver data were the same as for the DGVMs for consistency, which is described in lines 341-343.

2. The authors scaled DGVM-derived deadwood formation rates to current values to match them with current deadwood stocks and decomposition rates (lines 362–363). However, it is unclear how the scaling factors were derived. Since the current deadwood stock is only available for 2010, there may be equifinality issues in retrieving these factors. If the authors used $dC2000\text{-}2010/dt = \alpha * I2000\text{-}2010 – D2000\text{-}2010$ and $dC2000\text{-}2010/dt = \alpha * I2000\text{-}2010 – D2000\text{-}2010$ to estimate α as the scaling factor (where I is the default formation rate and D is the estimated decomposition rate), how were the deadwood stocks in 2000 and 2010 calibrated? Please clarify the calculation of scaling factors with explicit equations.

Response: Thank you for this question and for giving us the opportunity to once more explain and clarify our approach (also for the reader). The key element of our study is that we bring together three different components to answer our research questions: a map of initial deadwood stocks, simulations of future tree growth and mortality (i.e., deadwood formation) from DGVMs, and an empirical function of deadwood decomposition derived from a global decomposition experiment. These different approaches are based on different data, and make different assumptions about land-use. If we were to apply them directly, without scaling, there would be an offset between deadwood input and output already in year 1 of the analysis, resultant from these differences in the approaches. Yet, we are here not interested in this offset, but in how the two components deadwood input and output are responding to climate. Hence, we set this offset to zero for the beginning of the study period by means of a scaling factor, and subsequently only interpret the differential changes in input and output in response to climate, assuming that the scaling factor (which encapsulates the effects of land-use as well as differences in underlying data) remains constant over time. The way we do this mathematically is by assuming that input and output fluxes balance each other out. To derive stable output fluxes we ran the decomposition model for 20 years (2000-2020) around the initial year of our deadwood map (which is for the year 2010), and scaled the input fluxes to this reference value. We have now revised the respective section in the methods and have also included the mathematical notation of our scaling, to clarify this approach for the reader. Thanks for the suggestion!

In Figure 4: Since decomposition rates were upscaled based on site results, the temperature effect on decomposition should be consistent across models. Similarly, α is fixed across time and temperature gradients, and the scalar α applied in the previous step does not correct the real deadwood input rate across time. The difference in the temperature–I/O ratio should therefore depend solely on the relationship between temperature and the default tree mortality rate in each model. Therefore, the conclusions drawn from Figure 4 (and Figure 2) are not fully convincing.

Response: We thank the Reviewer for this comment, but after careful consideration do not agree with the conclusions that the results are not convincing. It is indeed true, that the climate response of decomposition (derived from global experimental data) was similar across DGVMs. However, decomposition is also sensitive to the clade of the deadwood being decomposed (angiosperm or gymnosperm), which is derived from DGVM simulations. If, for instance, one model projects a climate-induced shift from angiosperms to gymnosperms while another model does not, decomposition rates change even if the overall amount of deadwood input were to remain the same. This is to illustrate that the realized I/O rates presented in Figure 4 are the result of multiple, interaction processes. We thus maintain that they are insightful and relevant for the manuscript, which is why we have retained them also in the revised version of the manuscript.

Additionally, most DGVMs assume steady state. A large scaling of the formation rate (e.g., multiplying by 13.3 for tropical gymnosperms in SEIB-DGVM) could imply that ecosystem GPP and allocation are much lower than the allocation required for aboveground wood each year. This breaks the overall carbon balance.

Response: Thank you for this comment. We note that the scaling factors were not embedded in the models but rather applied to model output (see the description in lines 378-386). Hence there is no risk that the model-internal consistency and logic in the carbon balance is affected by our scaling approach. For more details see also our response to the scaling-related question from the Reviewer above.

3. Fire may contribute more to deadwood consumption than direct decomposition, especially in fire-prone regions. Similarly, harvest in managed forests is not accounted for. As a result, this study may overestimate both deadwood stock and decomposition rates to match the initial carbon stock map.

Response: Thank you for these comments, they highlighted for us that the key objective of our work was not yet clearly enough communicated. Particularly, we note that our aim was not to project future deadwood stocks (for which the effects of combustion by wildfire and the effects of land use would have to be accounted for) but rather to contrast the input and output rates to global deadwood pools and investigate direct climate-induced changes to this I/O ratio. We have now revised our objectives statement (lines 115-119) to make more clear for the reader what the objectives of our work are (and what not). That being said, we agree with the Reviewer on the importance of wildfire and harvesting for deadwood in general. Hence, we address both issues extensively in the discussion, where we now considerably extended our consideration of wildfire (lines 251-256), as also suggested by Reviewer #1, and where we explicitly discuss the potential role of land use (lines 257-271). We feel that with this revisions our objectives are much more clear for the reader and the important context of wildfire and management are discussed more comprehensively.

The age–carbon relationship is another issue, though the authors briefly acknowledge it in the discussion. Neglecting these factors could bias both the deadwood mapping and the I/O estimates. To strengthen the study, I recommend including more measured data to validate the robustness of the derived deadwood stock time series and decomposition rates, ideally, the I/O ratios as well.

Response: As already pointed out above, we note that our objective was not to map future deadwood – we do not present future deadwood maps anywhere in the manuscript as this was not the goal of our work. As mentioned by the Reviewer the potential effects of legacies, e.g. in the form of different age distributions of forests, were not considered here. This is explicitly stated for the reader in lines 393-396. As for the suggestion to add more field data: We acknowledge the importance of field data and highlight that with decomposition a key component of our work is based on field data from an hitherto unprecedented global deadwood experiment. That being said, conducting additional field work was beyond the scope of this study, and published field data on future deadwood I/O ratios are not available, as our investigation is – to the best of our knowledge – the first study to assess this. That being said we are happy to report that the global deadwood experiment that forms part of the empirical basis of the current analyses is being continued under the lead of Prof. Seibold (co-author here), and a future integration of this second phase of experimental data with models is currently an active discussion. So while out of our scope here, we hope to fulfil the request made by the Reviewer in future works!

Minor:

Figure 2: The panel shows the SD for carbon fluxes and I/O ratios. It would be more informative to show the full spread. Since LPJ-GUESS accounts for more than 50% of ensemble members, the spread and temporal trend are largely driven by this single model. Is there a way to balance model contributions in the final figure presentation?

Response: Thanks for the suggestion to improve Figure 2 by better showing the variation in the data. Instead of the SD we now show the interquartile range as well as the 5th to 95th percentile range, which gives the reader a better understanding of our data – thanks for the suggestion. With regard to the elevated leverage of LPJ-GUESS: This is indeed a very good point, and one that was also raised by Reviewer #1. While this is partly the result of our experimental design and hence cannot be changed at this point (i.e., the model and climate ensembles studied), we agree that it is important to clearly communicate the effect of this choice to the reader. Hence we reran our analysis with a balanced dataset (i.e., where each model contributes equally to the results), and report the results in Supplementary Figures S3-S4. While the results change numerically and effects weaken, the main finding of increasing I/O ratios with climate change remain also in this analysis.

Title: The title may be slightly misleading, as only climate change is considered. There are no interactive effects among factors (e.g., climate × management, climate × disturbance).

Response: We have carefully considered this remark by the Reviewer, but do not see where the title suggests that interactive effects between climate and management or climate and disturbance are addressed. We have thus decided to retain the title as in the original submission.

Authors Response

We would like to express our sincere gratitude to the reviewers for their insightful feedback and constructive comments throughout the revision process. Your rigorous evaluation has significantly enhanced the clarity and technical depth of our manuscript. We appreciate the opportunity to address these final points and believe the resulting paper is much stronger thanks to your contributions.

Reviewer 2: Regarding my previous comments about steady state and lines 384–386: Did the authors apply the scaling factors during the spin-up period for the projection? If not, could the absence of scaling factors affect the “equilibrium” soil conditions? Also, do any soil variables influence wood formation (e.g., through tree mortality or wood density) or decomposition (e.g., via moisture or nitrogen content)? In other words, would it be possible to show that the deadwood stock in 2000–2020, using the adjusted scaling factors for projection, is still close to the initial deadwood stock reported by Seibold et al. (2021)? This would help confirm the consistency of the equilibrium approach.

Response: We thank the Reviewer for these additional questions. We did not do a spin-up simulation of deadwood stocks here, and the individual DGVMs compared used different approaches to obtain initial vegetation conditions as starting point for the simulation. We refer to the respective references cited in the text for details. With regard to the second question: We here do not make projections of deadwood pools, but rather limit our analyses to the input/ output ratio of deadwood. For this, our scaling factor does exactly what the Reviewer asks, it ensures that deadwood fluxes from the simulation correspond to the values reported by Seibold et al. (2021).

Reviewer 2: Regarding the title, my understanding of "climate change" includes changes in both climate variables and climate-induced disturbances (fire, droughts, insect outbreaks). From the title alone, I initially expected that disturbances were considered, and lines 77–80 also highlight their importance. It only becomes clear around lines 117–119 that disturbances are not included. Clarifying this earlier in the text or adjusting the title could help align reader expectations with the study focus.

Response: Thank you for this comment. Disturbance processes are indeed important for deadwood formation. Fire as the globally most important disturbance process is dynamically simulated in four out of the five DGVMs used here (i.e., all with the exception of CABLE-POP). However, other disturbance agents such as insect outbreaks are not considered explicitly in these models, but are (implicitly) included in the parameterization of the model, e.g. in terms of size- related mortality and/ or random background mortality events. The limitations of global models to simulate disturbances in detail are acknowledged explicitly in the discussion section; as some climate effects on disturbances are indeed included, we -after careful consideration - decided to retain the title as is.