Peer Review File

Manuscript Title: The contribution of insects to global forest deadwood decomposition

Note on referee numbering:

The original referee #2 did not submit a report on the original version of the paper. For the revised version of the paper, the original referee #3 was renumbered as referee #2 and an additional referee (referee #3, replacing the original referee #2) was asked to review the paper.

Reviewer Comments & Author Rebuttals

Reviewer Reports on the Initial Version:

Ref # 1

Deadwood decomposition mediated by insects influences global carbon fluxes
Seibold et al

This MS presents the results from a remarkable experiment. The authors have replicated a deadwood decomposition study at 55 sites across the world, and bring all of those data here to examine a critical, understudied part of the terrestrial carbon cycle. The results as presented are emphatic, and the data can provide sorely needed numbers on carbon emission rates and the proportional contribution that insects make to that rate.
I have nothing but praise for the experiment itself. It is ambitious, thoughtful, considers the key influences on decomposition rate, and walks a pragmatic line between the ideal and feasible. Where the ideal conditions could not be replicated in experimental form – such as using small diameter wood – the authors have conducted a clearly described and nicely considered set of analyses – such as quantifying the decay rate ratio between fine and coarse woody debris – to ensure experimental artefacts do not influence the final take-home messages. Throughout, the statistical analyses are thorough, and their upscaled estimates are accompanied by an analysis of uncertainty.
I do have several major concerns revolving around the magnitude of the effect of decomposers, partly because these effects are exaggerated in the text and stretch the interpretation of the statistical results, and partly because the method used to quantify them comes with a hidden bias. These concerns should, however, be relatively easy to address, through toning down some of the unnecessary claims, providing additional clarity on how this effect has been quantified, and if necessary through relatively straightforward analysis.

Major concerns:
The results around the direct impact of insects are not supported by the analysis. L125 states "Decomposition rates were significantly lower when insects were excluded", and the following sentence (L126) states "the effect of insects was comparable in strength to the effect of temperature" with the reader referred to Extended Data Table 1 to support this statement. But this interpretation of the statistics is not obviously correct as this table does not present any post-hoc tests to determine which pairs of the three treatments differ from each other. This is vital because not all treatment differences can be ascribed to the presence/absence of decomposers. Results in

Table S1-1 show the treatment contrast that can most rigorously test for an insect effect – open vs closed cages – is not significant (p=0.104). With the very large sample sizes involved (n > 1000), p-values in this range should not even be considered “marginally non-significant” and/or “marginally significant” as described in the Supplement (page 7). While there is p < 0.001 for the ‘treatment’ effect in Extended Data Table 1, that treatment effect includes comparisons among all three treatments (closed cages, open cages and no cages), whereas the analysis in Table S1-1 is designed specifically to test for the treatment contrast that specifies insect effects (open vs closed cages). The results from Table S1-1 therefore take precedence when it comes to determining the effect of insects, and they show no significant pattern. The significant effect in Extended Data Table 1 could easily be arising from alternative treatment contrasts that have nothing to do with decomposers; this impression is reinforced by the uncaged vs open cage contrast in Table S1-1 (ie, a test of the procedural control), which has a stronger level of significance (p = 0.058) than the open vs closed cage insect effect. I can see no convincing evidence for a strong, direct effect of insects in these data and analyses.
The rest of this paragraph emphasises the “significant two- and three-way interactions” (L130) of insects with temperature and humidity, and these results are clearly defined and inarguable. (As an aside, Figure 3 is excellent and does a great job of visualising the complex, three-way interactions.) These results show that insects do play a role, but that that role is entirely mediated by climate and appears largely confined to the tropics. This more moderate conclusion is presented nicely in parts of the abstract and conclusion (eg L36, L203), but in other parts of the MS, claims around the direct importance of insects need toning down.

I have two concerns about how the contribution of decomposers to global carbon release rates is calculated (L185). Both issues that I raise introduce bias that will exaggerate the estimate of decomposer importance. First, the estimate comes from a comparison of closed cage vs uncaged treatments (L422-425), but the more stringent comparison should be made between the closed and open cages presented in Table S1-1. No detailed statistics are presented for the closed vs uncaged treatments in Table S1-1 so readers are unable to assess for themselves the significance of this value. Presumably it’s a larger effect than the closed vs open cage, which means the estimate presented should be considered an upper value. The equivalent value estimated from the closed vs open cage comparison should be presented to give an idea of how this choice influences the answer.
Second, the description of how this estimate is calculated isn’t entirely clear, but it appears to have been obtained by comparing upscaled maps of predicted decomposition rates with and without decomposers. Given that the upscaling is conducted with no global information on presence-absence or abundance of decomposers, this method is inappropriate. The authors approach boils down to making two global scenarios – one with decomposers everywhere and one with decomposers nowhere – and comparing total global decomposition under the two scenarios. But both scenarios introduce directional biases. The decomposers everywhere scenario can only overestimate global decomposition rates, and the decomposers nowhere scenario can only underestimate those rates. When combined, this over- and under-estimate necessarily bias the estimate – the difference between the two – to be an over-inflated value. Related to this though, I am confused about the equation in L424-425 showing how the insect contribution is estimated. It looks as though a proportional contribution is being estimated directly, rather than the absolute contribution it is described as calculating (“the amount of carbon released from deadwood by insects” L 422-423). Why does the equation need the ‘one minus’ in it? Isn’t the amount of carbon coming from decomposers simply ‘total carbon release[uncaged] – total carbon release[closed cage]’? Regardless, the estimate of decomposer contribution needs to come from direct analysis of the amount of wood decomposed in open vs closed cages during the experiment rather than the upscaled estimates, as the upscaling approach can only exaggerate the final estimate.

Additional concerns:
Statistics reporting: There are no formal statistics presented in the main text, tables or figures, making it impossible for reviewers to judge for themselves the importance of the results being presented without digging into extended data and supplementary information. These details need

adding into the main body of the MS which is all that 99% of readers will look at.

L67-68: This sentence about the “sensitivity of wood decomposing organisms to climate change…” is much too strong. To my knowledge, there is no direct evidence that decomposers are at particular risk from climate change, and the authors present no literature to back up the assertion. Termites are the key group, and the only termite-specific paper cited in this section (Buczkowski and Bertelsmeier 2017) is an analysis of invasive termites where the main result was that these species will have range increases under climate change. The way this paper is cited gives the impression that is suggests the opposite and that termites are a group under threat from climate change. Most termites spend the majority of their time underground and/or in sealed tunnel systems above-ground that buffer them from external microclimate conditions, meaning much of the generic assumptions about ectothermic species responses to climate change don’t directly apply. They’re also not large-bodied species (specifically referenced in L65). It’s fine to suggest that any loss of decomposers is going to have an impact, but a more balanced perspective on the level of threat they face is needed.

L121: Can you make an estimate of how big that increase is going to be? Given the model coefficients, what percentage increase in decomposition can be expected from a 2°C temperature rise?

L130: How can the effects of insects be negative? This result is referred to a number of times in the MS but no explanation is given (eg L37, L146, L157, L188, L204). My guess is that this is just model uncertainty coming from site-to-site variation in results, hidden slightly because the experiment has such high replication that p-values ascribe certainty to very small ‘effects’. But if there’s a convincing ecological reason to explain this result it needs presenting; otherwise, claims about the implications of this effect should be tempered.

L135: There’s a lot of convincing explanation for the interaction effect with temperature, but none for the interaction with humidity. Can you add a reason for this?

L198: The analyses in this MS do not highlight how deadwood provides habitats for species. This sentence needs re-wording.

L446: It’s more conservative to make no predictions outside of the experimental conditions than it is to enforce what is essentially a flat-line response. A useful statistic to add in here is the proportion of the world’s land surface area falls outside of the experimental conditions.

Extended Data Table 2: I like the addition of the sensitivity analysis a lot. But at the same time, it’s not described in the methods which leaves it largely uninterpretable. The description in the methods boil down to stating the analysis was done but not saying how it was done (L480-487). One item of particular confusion is how uncertainties were combined. For example, in the Data Uncertainty category, the three forms of uncertainty are quantified at ±1.00, ±1.50 and ±0.91 Pg respectively, but the total uncertainty ascribed to Data Uncertainty is only ±2.06. The most detailed description of how these uncertainties is in the table caption, which states “The contribution at the level of uncertainty categories is the sum of the contributing factors” (L572), but the sum of these three factors is ±3.41 Pg. The sums for the remaining two uncertainty types are also much lower than the sum of the individual values, suggesting the total uncertainty around the authors estimates is close to two times greater than currently reported.

Extended Data Figure 5: I didn’t understand what the colours black (legend calls it ‘Present’) and white (not in the legend) signify.

Extended Data Figure 6: Please use different symbols and line styles for the two categories. I found the red and blue very difficult to differentiate.

Ref #2

N/A

Ref #3

Seibold et al. conduct an impressively extensive, standardized, field experiment into the potential contributions of insects to wood decomposition; and use these field data combined with assessments of dead wood decomposition stocks to estimate insect contributions to annual, global carbon release from dead wood in the context of global land carbon emissions. The work has potential to help fill knowledge gaps about the dynamics of a large carbon pool (i.e. dead wood), in particular in relation to how decomposer animals influence these fluxes. That being said, there are numerous details about the experimental design and analysis that are opaque and which need to be clarified (and potentially rethought) if the reader is to have confidence in the use of the experimental data for the global carbon cycle extrapolation. Further, some of the rationale and conclusions seem to extend outside the scope of the study, and so I believe some reframing of how the work contributes to knowledge gaps is required. Overall, I feel it is important to recognize the extensive, coordinated approach that built the dataset on which this paper is built, and the important carbon cycle knowledge gaps it is intended to fill. At the same time, the validity of the carbon flux estimates and hence conclusions cannot be ascertained by the reader because the ambiguity about the details about what was done. I hope the following comments are helpful to the authors for revising the presentation and potentially approach of their work.

1. Please clarify the unit of experimental replication and design as it relates to how you analyzed the study. Much is made of an n of 4,463 observations of wood decomposition. But this n clearly was not used in the statistical models, and there appears to have been multiple occasions where individual observations were aggregated in some manner. This not only reduces the n substantively (which you have to be honest about!) but also discards variation - there appears no consideration for the very real concern that such aggregation can markedly change conclusions about effect sizes (e.g. the impact of insects), which then raises questions about the validity of the carbon flux estimates the authors derived. Further, the authors acknowledge with supporting citations that there is a great deal of within-site heterogeneity in wood decomposition rates, even for the same substrate - and it appears that they have averaged this away without justifying why this is valid. My confusion about what was done refers in particular to the text from line 351 to 387. Here's what I gleaned: there are 9 plots within each of the 55 sites. The 9 plots comprise 3 treatments (cage, uncaged, open cage), each with a replication of 3. And in each plot, there are 3 logs of 3 local tree species and 1 or 2 wooden dowels. But then it gets confusing - on line 384 you say one replicate of each treatment per site was sampled for wood decomposition at 1, 2 or 3 years. But what's a replicate: a log or a plot? What would be ideal would be that you sampled a single log of a local tree species from each of the 9 replicate plots at each site; but it instead appears that you sampled (destructively) a whole plot. Hence, did you actually just collect n=1 per treatment in each year? But what did you then do with the 3 logs per species in each plot? Were they pooled in some way? And is a single dowel then a representative measure? Please clarify exactly what was done and explain how your design captures the within-site variation in wood decomposition rates that you note in your paper. Also, with so much within-site variation in wood decomposition, how did you select the location for the 9 plots within each site to get representative conditions of the location? Overall, it seems that you might have taken a single replicate plot per year, then to get wood decomposition rates, combined the individual observations to fit a slope which you used as the decomposition rate. Then, the analysis was performed on these aggregated data, where much of the measured variation is essentially discarded. That's worrying because small differences here can sum to Pg of C in your extrapolation. So, tell us how strong these fits are, and what the consequences of aggregating are (i.e. why not use the actual observations you collected as an additional/ preferred analysis and compare the numbers you get)? One really obvious thing would help the reader - in all your tables and figures, telling us the number of observations on what the statistical models were based is

expected but not done, which makes it very frustrating for the reader. I suggest you also follow best practice, and share the observed data and also the data you derived to run in your statistical models. The model code you provided was a start, but essentially useless without the actual data to which the model was applied. In conclusion here, as a reviewer I want to be able to follow clearly what was done and to understand all the assumptions made in the data analysis. That requires the authors step me through all of those decisions, which we all have to make in our work, so that I can think about potential caveats or artifacts of the approach. When you do not provide such detail, it raises lots of questions that make it hard to assess the credibility of your analyses.

2. The authors should move beyond reliance on threshold statistics. Extended data table 1 is a good example. In the text, the authors refer to significant main effects and interactions. They give the coefficients and in one place in the text refer to z scores, but in no place on any of the statistical model tables do they tell us if the coefficients are raw or standardized. I have to assume therefore that they are unstandardized coefficients and by z they meant t, and are sticking with threshold statistics despite so many calls in the literature, as well as by the American Statistical Association, to discontinue such poor practice. Use of unstandardized coefficients means comparisons of their size is not meaningful given different units of the predictor (e.g. mm rainfall vs. oC temperature). Further, with use of unstandardized coefficients, it seems the authors are falling into classical interpretation errors. For example, they argue there is no effect of precipitation on decomposition of native trees. But if these are unstandardized coefficients, then the main effects are not reliable given the higher order interactions (i.e. Simpson's Paradox). Note that, given the mix of categorical and continuous predictors, it seems they need to generate standardized coefficients where 2 SDs (not one) are used in the standardization (Gelman 2008). So, I'd like to see standardized coefficients, reporting of the n used in such tables, a move beyond using P values (and t or z) as an indicator of whether an effect is meaningful, and then some consideration of best practice such as the reporting of variance inflation factors. That is, how correlated are the predictors when just the main effects without interactions are run? Are they too correlated to run in a single model? What is the r2 of the fixed effects, and of the random effects? Why/how is the coefficient for binary variable like "host" reported for each (1 or 0) of the levels of this predictor? How can that predictor then just be treated as a single term when interacting with other variables? Why random slopes per site, as opposed to random intercepts (which seems a fairer assumption to cope with the spatial design)? Overall, your use of hierarchical mixed model, regression approaches appears appropriate for your design, but the reliance on threshold statistics and no assessment of model structure (e.g. vifs) raises questions about whether the coefficients reported have any veracity. And, certainly your interpretation of main effects in light of strong interactions is flawed. Further, please note that just because the main effect of precipitation has a small estimate, it plays a role in so many interactions that precipitation is clearly affecting the decomposition rate. Lastly, a small but important point: most of your coefficients are negative. You describe in the text how you measure percentage mass loss of wood. Hence, higher values should mean more decomposition (per Figs 2 and 3!). So, if your coefficient is negative for temperature, for example, higher temperatures cause less decomposition? Please clarify what your response variable is in the models!

3. The framing should better represent the design. First, the paper claims to show the effects of insects on wood decomposition. I question that framing. Is it instead the "additionality of insect effects"? For example, let's assume that in an open cage, termites get to access the wood. When they are excluded, there is no termite vs. fungal competition, so fungi can decompose without being beaten to the food (hence, they substitute for termite effects). Hence, you do not appear to be assessing the absolute impact of insects on wood decomposition, which is likely to be greater than you estimate for, say, termites; but rather you're estimating the additional decomposition that occurs when they are present vs. absent. I feel this is an important distinction. Second, you make a justification for your work, in both the rationale and the conclusions, for understanding how loss of insect biodiversity will impact wood decomposition rates. This appears to stretch far beyond your data. We might be losing saproxylic insects, but your design is not a reduction in

species richness, genetic diversity, or even removal of specific functional groups/ traits. Instead, you remove all animals! Arguing that your experimental work then tells us something about the impacts of biodiversity loss then appears poorly justified.

4. As the authors well know, many temperate forests contain termites; and termites are not uniformly distributed at a site. Hence, they need to clarify what the termite designation means, especially because it is such a central part of the story. Is it that termites were present at the site? Or do they categorize each log decomposition estimate on the basis of whether or not termites had colonized the wood? This is an important point for the reader to assess the termite contributions to wood decomposition. As an aside, many wood-feeding termites are negatively phototropic. Hence, they usually feed within the log. How then do the authors think that the small diameter of the experimental logs used influenced termite colonization and subsequently feeding rates?

5. The claim is made in the conclusions that the authors' findings suggest that rising temperatures should accelerate wood decomposition rates, and they cite numerous mechanisms that might lead to such a consequence. But there is some work showing that such expectations are not realized when soil insects that feed on wood rot fungi are present, and instead warmer temperatures might accelerate feeding rates of such fungivores, which reduces wood decomposition under warmer conditions (e.g. Crowther et al. 2015 PNAS). Such alternate outcomes and associated explanations should likely be cited, especially given the focus of the paper on insect effects.

6. Can the authors discuss any caveats of their design. For example, the logs appeared to sit on a steel mesh. Yet we know that moisture is critical for fungal-mediated wood decomposition. Could the logs wet-up appropriately in the field with the steel mesh barrier between them and the soil surface? Do the authors have fresh masses to substantiate any conclusions here? And, if you do, how variable was moisture within the site and the experimental logs? Minor point - please use SI units. As of now, the mesh is listed as "1000 mesh per square inch". That does not tell the reader the mesh size in mm of the openings, as we would want but forces them to search online. And, in doing so, it seems that that mesh is something like 10- to 15-micron wide apertures. Is that the case? If it is that small, can the hyphae of multi-piece wood-rot fungi penetrate that to access the wood, especially where they are aggregated to minimize fungivory damage?

To conclude: those data collected have the potential to contribute to the examination of an important knowledge gap in the global carbon cycle. However, the data handling and then subsequent statistical interpretation of the experimental data is opaque and, from what I could glean, is not yet rigorous enough to support the extrapolations and the conclusions reached in the paper. Some reframing of the contribution also seems necessary. Hopefully the authors can address these concerns because they have a large number of observations across many sites, and so are potentially sitting on a very valuable dataset.

Author Rebuttals to Initial Comments:

Point-by-point response to the referees´ comments to Seibold et al. "Deadwood decomposition mediated by insects influences global carbon fluxes".

Referee #1 (Remarks to the Author):

Deadwood decomposition mediated by insects influences global carbon fluxes Seibold et al

1. This MS presents the results from a remarkable experiment. The authors have replicated a deadwood decomposition study at 55 sites across the world, and bring all of those data here to examine a critical, understudied part of the terrestrial carbon cycle. The results as presented are emphatic, and the data can provide sorely needed numbers on carbon emission rates and the proportional contribution that insects make to that rate. I have nothing but praise for the experiment itself. It is ambitious, thoughtful, considers the key influences on decomposition rate, and walks a pragmatic line between the ideal and feasible. Where the ideal conditions could not be replicated in experimental form – such as using small diameter wood – the authors have conducted a clearly described and nicely considered set of analyses – such as quantifying the decay rate ratio between fine and coarse woody debris – to ensure experimental artefacts do not influence the final take-home messages. Throughout, the statistical analyses are thorough, and their upscaled estimates are accompanied by an analysis of uncertainty. I do have several major concerns revolving around the magnitude of the effect of decomposers, partly because these effects are exaggerated in the text and stretch the interpretation of the statistical results, and partly because the method used to quantify them comes with a hidden bias. These concerns should, however, be relatively easy to address, through toning down some of the unnecessary claims, providing additional clarity on how this effect has been quantified, and if necessary through relatively straightforward analysis.

Thank you for the positive overall feedback on our study. We addressed all concerns that were raised including the quantification of the effect on insects on wood decomposition by conducting additional analyses (e.g. assessing the probability of insect colonization for the different treatments) and presenting additional discussion on this topic. Furthermore, we adjusted the main text to clarify that insects affect wood decomposition not only directly, but also indirectly via interactions with fungi and we toned down the potential effects of biodiversity loss throughout the manuscript as suggested. For details, please refer to our responses below.

Major concerns:

2. a) The results around the direct impact of insects are not supported by the analysis. L125 states “Decomposition rates were significantly lower when insects were excluded”, and the following sentence (L126) states “the effect of insects was comparable in strength to the effect of temperature” with the reader referred to Extended Data Table 1 to support this statement. But this interpretation of the statistics is not obviously correct as this table does not present any post-hoc tests to determine which pairs of the three treatments differ from each other.

This concern of the reviewer seems to be a result of miscommunication from our side. The original model presented in Extended Data Table 1 (now Table 1) included data only for two treatments, uncaged and closed. Hence, no post-hoc test was necessary. For several reasons supported by our additional analyses

(please see our responses to comment 2b below), we kept this model as the main model, but have now added the assumed full model to the revised version of the manuscript (Supplementary Information Table S1-1) and provide the post-hoc test of treatments as suggested (Extended Data Figure 2). We have rephrased the criticized interpretation of our results, now addressing more clearly that the effect of insects is strongly modulated by climatic conditions.

b) This is vital because not all treatment differences can be ascribed to the presence/absence of decomposers. Results in Table S1-1 show the treatment contrast that can most rigorously test for an insect effect – open vs closed cages – is not significant (p=0.104). With the very large sample sizes involved (n > 1000), p-values in this range should not even be considered “marginally non-significant” and/or “marginally significant” as described in the Supplement (page 7).

We use exp(coef) and corresponding confidence intervals for assessing the size of the relative effect on annual wood mass loss. Although data at the level of individual logs enter our models, the site-specific random effect accounts for the nested design with n = 55 sites as true number of replicates. We have now clarified this in all tables and figures in the revised version of the manuscript.

c) While there is $p < 0.001$ for the ‘treatment’ effect in Extended Data Table 1, that treatment effect includes comparisons among all three treatments (closed cages, open cages and no cages), whereas the analysis in Table S1-1 is designed specifically to test for the treatment contrast that specifies insect effects (open vs closed cages). The results from Table S1-1 therefore take precedence when it comes to determining the effect of insects, and they show no significant pattern. The significant effect in Extended Data Table 1 could easily be arising from alternative treatment contrasts that have nothing to do with decomposers; this impression is reinforced by the uncaged vs open cage contrast in Table S1-1 (ie, a test of the procedural control), which has a stronger level of significance ($p = 0.058$) than the open vs closed cage insect effect. I can see no convincing evidence for a strong, direct effect of insects in these data and analyses.

This comment asking which comparison among the three treatments is the most reliable to describe the effect of insects is highly important, which is why we have conducted new analyses to address the issue. Specifically, we have reorganized our Supplementary Information section 1 and now provide additional analyses, discussion and references. Summing up these new results and arguments, the comparison closed cage vs. uncaged emerges as providing the most realistic quantification of the effect of insects on decomposition for several reasons: First, we find the strongest difference in wood decomposition rates and insect colonization between uncaged and closed cages (Supplementary Information Table S1-1), but no indication for strong microclimatic effects of cages (Supplementary Information Table S1-3). Second, the open cage treatment ranked between closed cage and uncaged treatments

for both decomposition rate and insect colonization (Extended Data Figure 2). The later finding indicates that the open cage treatment reduced access by insects, suggesting that a comparison between closed and open cage to quantify the contribution of insects to wood decomposition would clearly underestimate the true effect of insects. Moreover, previous work by (Stoklosa et al., 2016) **suggests that even this estimate may be conservative if the cages cause faster decay rates relative to uncaged wood. Nevertheless, to quantify the sensitivity of our global estimates to this even more conservative estimate of the contribution of insects, we have considered the comparison for closed cage vs. open cage in our sensitivity analyses.**

d) The rest of this paragraph emphasises the "significant two- and three-way interactions" (L130) of insects with temperature and humidity, and these results are clearly defined and inarguable. (As an aside, Figure 3 is excellent and does a great job of visualising the complex, three-way interactions.) These results show that insects do play a role, but that that role is entirely mediated by climate and appears largely confined to the tropics. This more moderate conclusion is presented nicely in parts of the abstract and conclusion (eg L36, L203), but in other parts of the MS, claims around the direct importance of insects need toning down.

We thank the reviewer for this important comment, which concurs partly with a comment made by Reviewer 3. We have now carefully revised the entire text to clarify that decomposition is a complex process driven by interactions between temperature, precipitation and insects and that main effects, such as the effect of temperature, should not be overemphasized in a model with complex interactions (see also comment 2b by Reviewer 3). We have rephrased the respective sections in the main text and have added Extended Data Figure 4, illustrating the complex interactions from a different perspective. Instead of an overall prediction of how climate change might affect decomposition, this figure allows the reader to quantify how overall decomposition and the effect of insects may change under a given set of conditions if climate changes. Furthermore, in line with your suggestion, we have toned down the direct importance of insects throughout the text and emphasize more the interactions with climate and other decomposers.

e) I have two concerns about how the contribution of decomposers to global carbon release rates is calculated (L185). Both issues that I raise introduce bias that will exaggerate the estimate of decomposer importance. First, the estimate comes from a comparison of closed cage vs uncaged treatments (L422-425), but the more stringent comparison should be made between the closed and open cages presented in Table S1-1. No detailed statistics are presented for the closed vs uncaged treatments in Table S1-1 so readers are unable to assess for themselves the significance of this value. Presumably it's a larger effect than the closed vs open cage, which means the estimate presented should be considered an upper value. The equivalent value

estimated from the closed vs open cage comparison should be presented to give an idea of how this choice influences the answer.

This is partly a misunderstanding as explained in our response to comment 2a. For reasons outlined in response to comment 2c, we kept the comparison of closed cage vs. uncaged as the main result (Table 1). However, we now additionally provide full model outputs for a model comparing all three treatments using post-hoc tests (Supplementary Information Table S1-1), and also report results based on the comparison of closed cage vs. open cage (Extended Data Table 1), which are also included in our uncertainty analysis (Extended Data Table 2).

f) Second, the description of how this estimate is calculated isn't entirely clear, but it appears to have been obtained by comparing upscaled maps of predicted decomposition rates with and without decomposers. Given that the upscaling is conducted with no global information on presence-absence or abundance of decomposers, this method is inappropriate. The authors approach boils down to making two global scenarios – one with decomposers everywhere and one with decomposers nowhere – and comparing total global decomposition under the two scenarios. But both scenarios introduce directional biases. The decomposers everywhere scenario can only overestimate global decomposition rates, and the decomposers nowhere scenario can only underestimate those rates. When combined, this over- and under-estimate necessarily bias the estimate – the difference between the two – to be an over-inflated value.

Our global estimates of carbon loss from deadwood and the contribution of insects are based on predictions from one model (Table 1). For overall decomposition including all decomposers, we used predictions from the uncaged treatment. For the contribution of insects, we use the difference between predictions for uncaged and closed cage. These predictions are directly based on our empirical data, i.e. the presence/absence or activity of insects is indeed considered in our estimates. In other words, we don´t assume the same insect abundance (or rather insect contribution) everywhere, but derive information about the role of insects directly from our empirical data considering interactions of treatment and climate variables. To produce global maps, we then interpolate between sites based on a statistical relationship of decomposition rates with and without insects with climate variables.

g) Related to this though, I am confused about the equation in L424-425 showing how the insect contribution is estimated. It looks as though a proportional contribution is being estimated directly, rather than the absolute contribution it is described as calculating ("the amount of carbon released from deadwood by insects" L 422-423). Why does the equation need the 'one minus' in it? Isn't the amount of carbon coming from decomposers simply 'total carbon release[uncaged] – total carbon release[closed cage]'?

We thank the reviewer for spotting this error in the text! He or she is correct: The amount of carbon released from deadwood by insects is actually just carbon-release_uncaged – carbon-release_caged. The confusion was related to the fact that the model predicts the remaining biomass, instead of mass loss. The error has now been corrected in the manuscript (the actual calculations and the reported results were not affected by this, as it was purely a reporting issue).

h) Regardless, the estimate of decomposer contribution needs to come from direct analysis of the amount of wood decomposed in open vs closed cages during the experiment rather than the upscaled estimates, as the upscaling approach can only exaggerate the final estimate.

This is exactly what we did: we derived functions for the relationships between overall decomposition and the contribution of insects and climate variables and then used these relationships to produce global maps for total carbon release from deadwood and the contribution of insects which were needed to sum up to global numbers. In the revised version of the manuscript we did this analysis twice, once for the more realistic insect assessment from uncaged vs. closed cage and once in a more conservative way from open vs. closed cage.

Additional concerns:

3. Statistics reporting: There are no formal statistics presented in the main text, tables or figures, making it impossible for reviewers to judge for themselves the importance of the results being presented without digging into extended data and supplementary information. These details need adding into the main body of the MS which is all that 99% of readers will look at.

We thank the reviewer for this comment and have now moved the former Extended Data Table 1 to the main part of the manuscript (now Table 1) to make it available to the large audience perusing the printed version of the article. Please note that Extended data items are indeed part of the pdf version of Nature articles.

4. L67-68: This sentence about the “sensitivity of wood decomposing organisms to climate change…” is much too strong. To my knowledge, there is no direct evidence that decomposers are at particular risk from climate change, and the authors present no literature to back up the assertion. Termites are the key group, and the only termite-specific paper cited in this section (Buczkowski and Bertelsmeier 2017) is an analysis of invasive termites where the main result was that these species will have range increases under climate change. The way this paper is cited gives the impression that is suggests the opposite and that termites are a group under threat from climate change. Most termites spend the majority of their time underground and/or in sealed tunnel systems above-ground that buffer them from external microclimate conditions, meaning much of the generic assumptions about ectothermic species responses to climate change don’t directly apply. They’re also not large-bodied species (specifically

referenced in L65). It's fine to suggest that any loss of decomposers is going to have an impact, but a more balanced perspective on the level of threat they face is needed.

Thank you for this comment. We have rephrased this respective section of the revised paper to provide a more general introduction, saying that a better understanding of the role of insects for wood decomposition is needed given that global change is affecting insect communities. It reads now (P. 3, L. 60): *"Terrestrial insect communities are affected by accelerating rates of human-mediated environmental change[8,23,24]. How these changes affect key ecosystem functions such as decomposition remains unclear[8]. As insect decomposers are ectothermic animals, their distribution and level of activity is affected by global climate change[25,26]. Given the sensitivity of insects to climate change[25,26] and the observed declines in insect biodiversity[8,23,24], a better understanding of the interactions between insect decomposers and climate is needed to project the future carbon flux from deadwood decomposition."*

Regarding the specific role of termites, please see also our responses to comments 3b, 4a and 4b of referee 3.

5. L121: Can you make an estimate of how big that increase is going to be? Given the model coefficients, what percentage increase in decomposition can be expected from a 2°C temperature rise?

The effect of temperature on decomposition depends on the level of precipitation. We therefore do not present one number for how big the increase in decomposition is going to be but provide a new Extended Data Figure 4. This figure allows the reader to extract this information for a range of prevailing climatic conditions and climate change scenarios.

6. L130: How can the effects of insects be negative? This result is referred to a number of times in the MS but no explanation is given (eg L37, L146, L157, L188, L204). My guess is that this is just model uncertainty coming from site-to-site variation in results, hidden slightly because the experiment has such high replication that p-values ascribe certainty to very small 'effects'. But if there's a convincing ecological reason to explain this result it needs presenting; otherwise, claims about the implications of this effect should be tempered.

Thank you for this comment. This topic was indeed not addressed sufficiently. Recent studies have shown that insect activity can indeed decrease rates of decomposition by introducing fungi which do not contribute strongly to wood decomposition themselves, while suppressing other principal wood-decaying fungi, thereby lowering overall decomposition rates (Skelton et al., 2019, 2020)**. Extended Data Figure 4 shows that negative insect effects occur in cold regions and are strongest in cold-humid regions where overall decomposition rates are low. We can only speculate here, but it seems possible that under a climate where key decomposers such as termites are absent and where insect activity**

is low (e.g. due to slow larval development (Marshall et al., 2020) **or high pathogen pressure** (Harvell et al., 2002)**), interactions with fungi render the net contribution of insects to wood decomposition negative. Note that over the course of our study, cold-humid regions consistently showed small but negative decomposition rates, which means that dry mass increased slightly and which may be a result of ongoing fungal colonization and nutrient relocation** (Filipiak, 2018)**. We rephrased the respective section (P.7, L.149): *"Fourth, our measure of insect contribution to wood decomposition includes direct consumption of wood as well as indirect effects via interactions with microbes. By modifying communities of wood-decomposing fungi through, for example, vectoring or grazing on fungal mycelia, insects can either increase[36] or decrease wood decomposition[37]. A number of wood-inhabiting insect species introduce fungal species which do not contribute strongly to wood decomposition themselves, while suppressing other principal wood-decaying fungi, thus lowering the overall decomposition rates[37]. In cold and humid regions, such biotic interactions can outweigh the effects of direct consumption, and lead to overall negative effects of insects on wood decomposition."***

7. L135: There's a lot of convincing explanation for the interaction effect with temperature, but none for the interaction with humidity. Can you add a reason for this?

Thank you for this comment. We now address the interplay of both temperature and precipitation more explicitly in the text and have added a new display item to make this interplay more visible for the reader (Extended Data Figure 4). The interaction indicates that precipitation has a positive effect on decomposition by insects at moderate and high temperatures but a negative effect at low temperature. At low temperatures (as mentioned in our response to comment 6), high humidity may negatively affect insects by reducing aeration in wood (Harmon et al., 1986) **and increasing pathogen pressure** (Harvell et al., 2002)**. At high temperatures, low precipitation is usually tied to strong seasonality in precipitation, leading to seasonality of many saproxylic insects with abundance peaks during the wet season** (Berkov, 2018)**. Increasing activity periods may explain why the effect of insects on wood decomposition increased with precipitation and especially with both temperature and precipitation. The respective sections now read (P.6 L,145): "*Third, insects could be negatively impacted by high wood moisture when precipitation is high and evaporation low, as is the case e.g. in humid boreal forests (Extended Data Fig. 4b), due to low aeration or high pathogen pressure[34]. At high temperatures, moisture should be a limiting factor at low precipitation levels, restricting the period of high insect activity to the rainy season[35]."***

8. L198: The analyses in this MS do not highlight how deadwood provides habitats for species. This sentence needs re-wording.

We dropped the statement and rephrased the respective sentence to (P.9, L.215): "*Our quantification of global carbon release from deadwood highlights that deadwood plays an important role in the global carbon cycle.*"

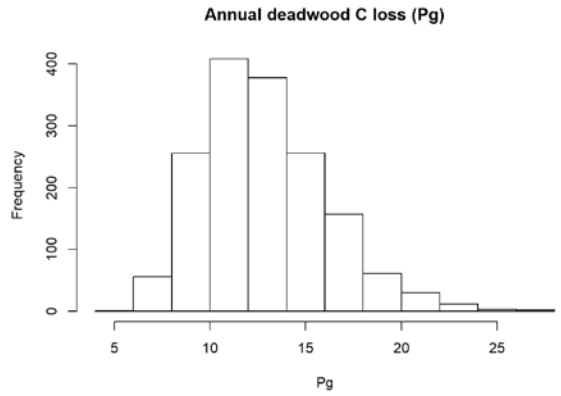
9. L446: It's more conservative to make no predictions outside of the experimental conditions than it is to enforce what is essentially a flat-line response. A useful statistic to add in here is the proportion of the world's land surface area falls outside of the experimental conditions.

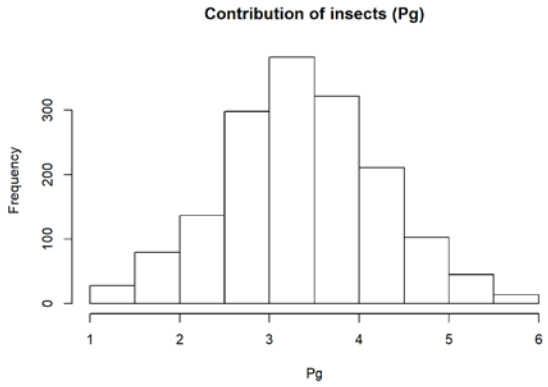
We agree that omitting these areas would result in an even more conservative estimate. However, by not considering a part of the forested area of the globe, we would no longer be able to provide a true global estimate, and therefore we decided to refrain from fundamentally changing our approach. Moreover, our uncertainty analysis (which considers a scenario that makes no predictions outside the climate space covered by our sites) shows that the definition of the climate space of prediction is of little consequence for the global results. We now report the total amount of deadwood (combined for angiosperms and gymnosperms) that is located in areas with climatic conditions beyond the experimental conditions in the caption of Extended Data Figure 6. The land surface area not covered by the experiment is 23.5% for gymnosperm and 17.7% for angiosperm, representing 13.2% of the global C stored in deadwood. The overall uncertainty from the definition of the climate envelope used for prediction is ±0.11 Pg (Extended Data Table 2).

10. Extended Data Table 2: I like the addition of the sensitivity analysis a lot. But at the same time, it's not described in the methods which leaves it largely uninterpretable. The description in the methods boil down to stating the analysis was done but not saying how it was done (L480-487). One item of particular confusion is how uncertainties were combined. For example, in the Data Uncertainty category, the three forms of uncertainty are quantified at ±1.00, ±1.50 and ±0.91 Pg respectively, but the total uncertainty ascribed to Data Uncertainty is only ±2.06. The most detailed description of how these uncertainties is in the table caption, which states "The contribution at the level of uncertainty categories is the sum of the contributing factors" (L572), but the sum of these three factors is ±3.41 Pg. The sums for the remaining two uncertainty types are also much lower than the sum of the individual values, suggesting the total uncertainty around the authors estimates is close to two times greater than currently reported.

We considerably extended the description of the sensitivity analysis in the main text and also clarified calculation steps in the caption of Extended Data Table 2. In short, the uncertainty for total and insect-caused annual deadwood carbon loss per factor was calculated as the standard deviation over all 1620 combinations with all eight other factors fixed to the default value. Similarly, the uncertainty per category was calculated over all combinations with all six

factors from other categories fixed to the default. Therefore, they do not add up. In addition to making these clarifications in the text, we now include the code for the sensitivity analysis (including all data to reproduce the results) as a markdown script in the data repository (during review: https://syncandshare.lrz.de/getlink/fiWgNASKDj5o2Y9YnrLusBxQ/). Below are the distributions for the annual deadwood C loss and contribution of insects over all 1620 estimates calculated in the sensitivity analysis. The SD is 3.2 for annual deadwood C loss, and 0.9 for the contribution of insects. These figures are also included in the Seibold_et_al_2020_sensitivity_analysis.Rmd script and corresponding html output.





11. Extended Data Figure 5: I didn't understand what the colours black (legend calls it 'Present') and white (not in the legend) signify.

Cells are black if the climate is within the climatic range of the model, white means not forested by the respective host species groups. We updated the legend and the description to make this clearer.

12. Extended Data Figure 6: Please use different symbols and line styles for the two categories. I found the red and blue very difficult to differentiate.

Thanks for the suggestion! We updated the figure accordingly.

Referee #3 (Remarks to the Author):

Seibold et al. conduct an impressively extensive, standardized, field experiment into the potential contributions of insects to wood decomposition; and use these field data combined with assessments of dead wood decomposition stocks to estimate insect contributions to annual, global carbon release from dead wood in the context of global land carbon emissions. The work has potential to help fill knowledge gaps about the dynamics of a large carbon pool (i.e. dead wood), in particular in relation to how decomposer animals influence these fluxes. That being said, there are numerous details about the experimental design and analysis that are opaque and which need to be clarified (and potentially rethought) if the reader is to have confidence in the use of the experimental data for the global carbon cycle extrapolation. Further, some of the rationale and conclusions seem to extend outside the scope of the study, and so I believe some reframing of how the work contributes to knowledge gaps is required. Overall, I feel it is important to recognize the extensive, coordinated approach that built the dataset on which this paper is built, and the important carbon cycle knowledge gaps it is intended to fill. At the same time, the validity of the carbon flux estimates and hence conclusions cannot be ascertained by the reader because the ambiguity about the details about what was done. I hope the following comments are helpful to the authors for revising the presentation and potentially approach of their work.

We appreciate very much the positive evaluation of our work and that the relevance of the results is acknowledged. We apologize for being opaque in places regarding the description of our approach, which has led to a couple of misunderstandings. To be fully transparent, we provide a complete annotated R code alongside the data which allows readers to repeat all analyses conducted in our manuscript, including for the figures. Furthermore, we rewrote the respective passages in the text and added further information to the description of all points in addition to conducting new analyses (please see more detailed responses below). As suggested, we rephrased the interpretation of our results, especially with regard to potential effects of biodiversity loss.

1.a) Please clarify the unit of experimental replication and design as it relates to how you analyzed the study. Much is made of an n of 4,463 observations of wood decomposition. But this n clearly was not used in the statistical models, and there appears to have been multiple occasions where individual observations were aggregated in some manner. This not only reduces the n substantively (which you have to be honest about!) but also discards variation - there appears no consideration for the very real concern that such aggregation can markedly change conclusions about effect sizes (e.g. the impact of insects), which then raises questions about the validity of the carbon flux estimates the authors derived.

The raw observations that entered our statistical models of the wood decomposition experiment (Table 1) were the mass of each individual log. We

did not pool or average any data before modelling. While logs collected at the same site and in the same year do not represent independent observations, we accounted for this by using random effects (for details, please see our response to comment 2h). We then used this model (Table 1) to predict relative annual mass loss (separately for the uncaged and closed cage treatment) for each of our 55 sites (true replicates) for angiosperms and 21 sites for gymnosperms (gymnosperm species do not occur at all sites). These values are shown in Figure 2 and Figure 3, and they were used as input data in the additive beta-regression model, which was used to predict annual mass loss globally relative to temperature and precipitation. We now clarify that data at the level of individual logs entered our models in the Methods section (P.22, L.474): "*Dry mass of each individual log at time t served as the response variable and log-transformed initial dry mass (t = 0) was used as an offset term.*"

b) Further, the authors acknowledge with supporting citations that there is a great deal of within-site heterogeneity in wood decomposition rates, even for the same substrate - and it appears that they have averaged this away without justifying why this is valid.

This concern is the result of a miscommunication on our part, as we did not average decomposition rates, but built models based on data of individual logs. We have revised the text to clarify this point.

c) My confusion about what was done refers in particular to the text from line 351 to 387. Here's what I gleaned: there are 9 plots within each of the 55 sites. The 9 plots comprise 3 treatments (cage, uncaged, open cage), each with a replication of 3. And in each plot, there are 3 logs of 3 local tree species and 1 or 2 wooden dowels.

This is correct. Hence, the true replicates in our experiment are the 55 sites, but our models are constructed from data on individual logs accounting for the nested design using random effects.

d) But then it gets confusing - on line 384 you say one replicate of each treatment per site was sampled for wood decomposition at 1, 2 or 3 years. But what's a replicate: a log or a plot? What would be ideal would be that you sampled a single log of a local tree species from each of the 9 replicate plots at each site; but it instead appears that you sampled (destructively) a whole plot. Hence, did you actually just collect n=1 per treatment in each year? But what did you then do with the 3 logs per species in each plot? Were they pooled in some way?

Our terminology followed this structure: site = 55 locations around the globe; each site received 9 installations which comprise 3 different treatments; thus, 3 installations per treatment. Treatments were randomly assigned to the 9 locations within a site. From these three installations per treatment, we selected one at a time after one, two and three years randomly for destructive sampling. We did not use the term plot because it is commonly used for larger spatial units. We rewrote parts of the methods description to clarify these points:

P.20, L.422: "*At each site, the three treatments were applied three times, i.e. three installations per treatment per site, resulting in a total of nine installations per site (Extended Data Fig. 1). The nine installations were arranged in a matrix of 3 x 3 with a spacing of 2 m between installations, resulting in a total size of approx. 15 m x 15 m. Treatments were assigned randomly to each of the nine locations within a site."*

P.21, L.455: *"After approximately one, two and three years, one of the three installations of each treatment per site was randomly selected and collected to measure wood decomposition. In other words, all logs from one uncaged, one closed cage and one open cage treatment were collected per site at the same time."*

e) And is a single dowel then a representative measure?

Indeed, only one dowel per treatment, year and site is obviously not able to capture heterogeneity within a site. The main focus of our experiment was decomposition of fresh branch material cut from native tree species as these more natural substrates are more likely to be colonized by specialized decomposers (Ulyshen and Wagner, 2013)**. In our study, dowels served mainly as control substrate to be able to compare patterns of native species to a standardized substrate.**

f) Please clarify exactly what was done and explain how your design captures the within-site variation in wood decomposition rates that you note in your paper. Also, with so much within-site variation in wood decomposition, how did you select the location for the 9 plots within each site to get representative conditions of the location?

Within-site variation is captured by sampling three logs of the same native tree species per treatment, year and site. These three logs were taken from the same installation and thus from close proximity. They may therefore capture less within-site variability than when three logs of the same tree species and treatment were sampled from different installations. We chose the former and not the later approach for two reasons: 1) The installations were arranged in a comparatively narrow grid (max. distance between installations was 6 m) to have relatively homogenous conditions throughout each site and thus within-site variation was expected to be rather low; 2) We wanted to ensure that the same number of logs could be sampled per treatment and year. When designing the experiment, we did not know if the cages would last for several years, and failure of cages over time would have left us with an unbalanced number of logs for different treatments. The 9 installations per site were arranged in a fixed grid with approx. 2 m between installations. We have now revised the text to make our design (and the rationale behind it) more clear for the reader.

g) Overall, it seems that you might have taken a single replicate plot per year, then to get wood decomposition rates, combined the individual observations to fit a slope

which you used as the decomposition rate. Then, the analysis was performed on these aggregated data, where much of the measured variation is essentially discarded. That's worrying because small differences here can sum to Pg of C in your extrapolation. So, tell us how strong these fits are, and what the consequences of aggregating are (i.e. why not use the actual observations you collected as an additional/ preferred analysis and compare the numbers you get)?

This is clearly the result of a miscommunication on our part. Please see our response to comment 1a.

h) One really obvious thing would help the reader - in all your tables and figures, telling us the number of observations on what the statistical models were based is expected but not done, which makes it very frustrating for the reader.

We thank the reviewer for this comment and have now added the requested information to all tables and figures as suggested.

i) I suggest you also follow best practice, and share the observed data and also the data you derived to run in your statistical models. The model code you provided was a start, but essentially useless without the actual data to which the model was applied. In conclusion here, as a reviewer I want to be able to follow clearly what was done and to understand all the assumptions made in the data analysis. That requires the authors step me through all of those decisions, which we all have to make in our work, so that I can think about potential caveats or artifacts of the approach. When you do not provide such detail, it raises lots of questions that make it hard to assess the credibility of your analyses.

We fully agree that the data and code should be accessible, which we planned on doing after publication so that readers have access to the material. But of course, and we apologize for this, code and data have to be available to reviewers already at this stage. We now provide the raw data and full R code to reproduce the statistical analyses of the experiment, as well as data and the respective R code to reproduce the upscaling approach used to derive global estimates. Both files include code to reproduce figures.

2. a) The authors should move beyond reliance on threshold statistics. Extended data table 1 is a good example. In the text, the authors refer to significant main effects and interactions. They give the coefficients and in one place in the text refer to z scores, but in no place on any of the statistical model tables do they tell us if the coefficients are raw or standardized. I have to assume therefore that they are unstandardized coefficients and by z they meant t, and are sticking with threshold statistics despite so many calls in the literature, as well as by the American Statistical Association, to discontinue such poor practice. Use of unstandardized coefficients means comparisons of their size is not meaningful given different units of the predictor (e.g. mm rainfall vs. oC temperature).

Thank you for these comments regarding better reporting of the modeling results. We updated Table 1 (formerly Extended Data Table 1) and present the information exactly as suggested by the referee. Before we go into details, let us recap the model structure applied here: A Gaussian GLM with log-link explains the expected absolute annual mass loss as an exponentiated function of time, temperature, precipitation, host (angiosperm, gymnosperm), and treatment. The model features an offset term defined as log(initial wood mass), allowing an interpretation of the exponentiated model coefficients as relative reduction in annual wood mass induced by a one-unit increase.

In the initial submission, we reported estimates obtained for standardized and centered temperature and precipitation. Interpretation of main effects referred to changes from the baseline configuration (mean temperature and precipitation, closed cages). We absolutely agree that reporting of scaled effects is confusing, and we now report the effects in the original units of temperature and precipitation. We use 15 °C as reference level for temperature and 13 dm/a as reference level for precipitation and report the raw coefficients, exp(coefficients) as relative effects, and corresponding confidence intervals. The latter intervals allow an interpretation of the magnitude of the effects as a relative annual reduction of wood mass.

b) Further, with use of unstandardized coefficients, it seems the authors are falling into classical interpretation errors. For example, they argue there is no effect of precipitation on decomposition of native trees. But if these are unstandardized coefficients, then the main effects are not reliable given the higher order interactions (i.e. Simpson's Paradox).

The main effects for each variable are interpretable when the remaining variables are fixed at their reference value (mean temperature and precipitation in the initial submission, 15 °C and 13 dm/a in our revision), because the respective interaction effects cancel out. For other configurations of temperature and precipitation, main and interaction effects have to be considered simultaneously, as pointed out in your comment. Of course, interpretation of model coefficients with two- and three-way interactions is challenging. We address this point by adding a new visualization of the relative treatment effect as a function of temperature, precipitation, and host (Extended Data Figure 4). We now also describe in more detail how the results can be interpreted in the caption of Table 1.

c) Note that, given the mix of categorical and continuous predictors, it seems they need to generate standardized coefficients where 2 SDs (not one) are used in the standardization (Gelman 2008). So, I'd like to see standardized coefficients, reporting of the n used in such tables, …

These are presented now in Table 1; raw coefficients and their SDs are also given. All tables and figures showing model results now specify the number of observations.

d) …a move beyond using P values (and t or z) as an indicator of whether an effect is meaningful,

We use exp(coef) and corresponding confidence intervals for assessing the size of the relative effect on annual wood mass loss. We removed asterisks from tables.

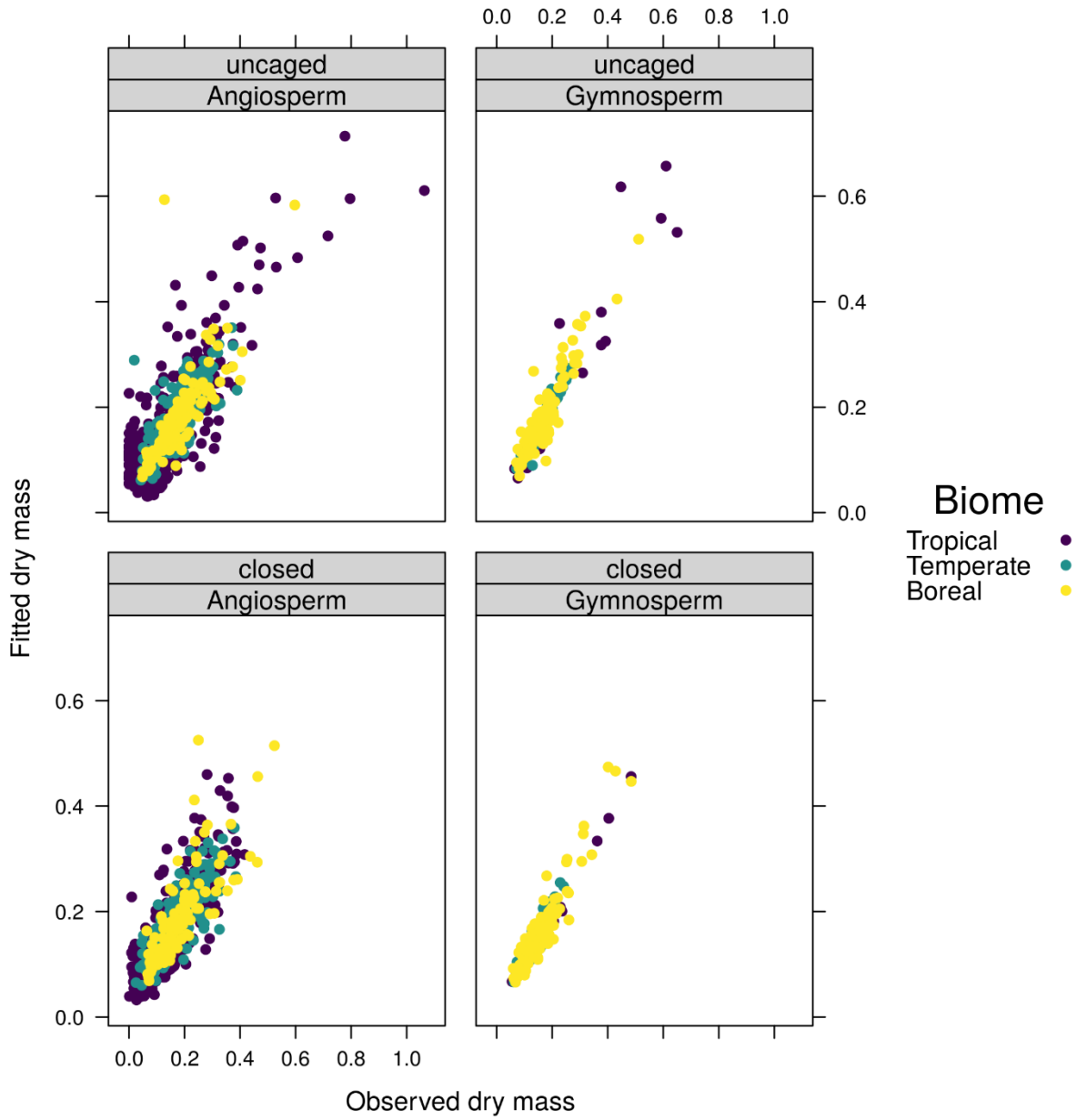
e) … and then some consideration of best practice such as the reporting of variance inflation factors. That is, how correlated are the predictors when just the main effects without interactions are run? Are they too correlated to run in a single model?

The computation for variance inflation factors (VIFs) for a Gaussian GLM with log-link not featuring an intercept term is technically challenging. Using car::vif (which features a merMod method for these type of models) we observe small generalized VIFs (< 1.5) for terms not involving host. The main effect of host has two degrees of freedom and thus a larger generalized VIF (~ 2.8) is expected. The two interaction terms involving host and temperature show a higher generalized VIF (6.4 and 7.2).

These numbers must be interpreted with care. We have been in contact with John Fox, author of car::vif and also the corresponding methodological publication (Fox and Monette, 1992)**. Their formula (10) would need to be adapted to our model lacking an explicit intercept term (X_0, in their notation). The corresponding methodology is currently not available and has to be developed in future statistical research. As a remedy, we report the correlation matrix of the design matrix corresponding to the fixed effects. Interaction terms involving host exhibit stronger correlations (.91 between expoas:TempS:PrecS:hostgymno and expoas:PrecS:hostgymno) but correlations between other terms, and especially treatment (max. abs. correlation is .2), are relatively low. The correlation between temperature and precipitation is .31. We therefore do not expect our model to be affected by multi-colinearity in a severe way.**

f) What is the r2 of the fixed effects, and of the random effects?

The marginal R^2 (integrating out the random effects) is 0.839 and the conditional R^2 (given the random effects) is 0.977. We added the information to the caption of Table 1. We also produced a plot showing observed vs. fitted values, separately for angiosperm vs. gymnosperm, and the two different treatments in three biomes which is embedded in the annotated R script.



g) Why/how is the coefficient for binary variable like “host” reported for each (1 or 0) of the levels of this predictor? How can that predictor then just be treated as a single term when interacting with other variables?

The model does not have an intercept term. Therefore, the main effect of host as a factor at two levels (angiosperm and gymnosperm) requires two parameters. In contrast, interaction effects are defined by the corresponding treatment contrast (host gymnosperm - host angiosperm) because using two instead of one parameter here would induce a rank deficiency in the fixed effects design matrix.

h) Why random slopes per site, as opposed to random intercepts (which seems a fairer assumption to cope with the spatial design)?

All model parameters interact with time, and thus the spatial heterogeneity is captured by one random parameter per site (in fact, a "random intercept" in this terminology) interacting with time (a "random slope", from this perspective). We use the term “random effect” in the main text and explain the parameterisation of the model in detail in the methods part.

i) Overall, your use of hierarchical mixed model, regression approaches appears appropriate for your design, but the reliance on threshold statistics and no assessment of model structure (e.g. vifs) raises questions about whether the coefficients reported have any veracity. And, certainly your interpretation of main effects in light of strong interactions is flawed. Further, please note that just because the main effect of precipitation has a small estimate, it plays a role in so many interactions that precipitation is clearly affecting the decomposition rate.

We thank the referee for the insightful points raised here. The coefficients of model terms involving temperature (15 °C) and precipitation (13 dm / a) are all negative (except the interaction of temperature with host: gymnosperm). This means that increasing temperature and precipitation accelerate the decomposition process in general for sites exceeding 15 °C and 13 dm/a. In colder and drier areas, the effects are smaller and, in extremely cold places, increasing precipitation is even associated with dry mass gain over the course of a year. We hope that the interpretation of the treatment effect is now improved given the new visualization (see 2 b) and the detailed description added to the caption of Table 1.

j) Lastly, a small but important point: most of your coefficients are negative. You describe in the text how you measure percentage mass loss of wood. Hence, higher values should mean more decomposition (per Figs 2 and 3!). So, if your coefficient is negative for temperature, for example, higher temperatures cause less decomposition? Please clarify what your response variable is in the models!

exp(coef * x) with coef < 0 means that a one unit increase in x (one degree of temperature, for example) leads to a relative effect of exp(coef) < 1. See response to comment 2i.

3. a) The framing should better represent the design. First, the paper claims to show the effects of insects on wood decomposition. I question that framing. Is it instead the “additionality of insect effects”? For example, let’s assume that in an open cage, termites get to access the wood. When they are excluded, there is no termite vs. fungal competition, so fungi can decompose without being beaten to the food (hence, they substitute for termite effects). Hence, you do not appear to be assessing the absolute impact of insects on wood decomposition, which is likely to be greater than you estimate for, say, termites; but rather you’re estimating the additional decomposition that occurs when they are present vs. absent. I feel this is an important distinction.

In our view, insects affect wood decomposition via direct and indirect effects. Direct effects are the direct consumption and substrate alteration (e.g. tunneling and fragmentation/shredding), while indirect effects include various interactions with microbes (Ulyshen, 2016)**. These interactions can both increase decomposition (e.g. transport and inoculation of potent decomposers** (Jacobsen et al., 2018)**) and decrease decomposition (e.g. feeding on fungi** (Crowther et al., 2012, 2011)**, transport and inoculation of fungal species which are weak decomposers but outcompete other stronger decomposers** (Skelton et al., 2020, 2019)**). Our experiment measures the sum of these effects, in which indirect effects can either add to direct effects or reduce them. Moreover, even direct consumption of wood by insects is not really independent of microbes since many, probably most, insects rely either on gut symbionts or external fungal mutualists which help to break down lignocellulose** (Birkemoe et al., 2018; Ulyshen, 2016)**. We are thus convinced that direct and indirect effects of insects on decomposition cannot be separated at the scale of a study like ours. We added a statement to clarify this (P.7, L.149): *"Fourth, our measure of insect contribution to wood decomposition includes direct consumption of wood as well as indirect effects via interactions with microbes."***

b) Second, you make a justification for your work, in both the rationale and the conclusions, for understanding how loss of insect biodiversity will impact wood decomposition rates. This appears to stretch far beyond your data. We might be losing saproxylic insects, but your design is not a reduction in species richness, genetic diversity, or even removal of specific functional groups/ traits. Instead, you remove all animals! Arguing that your experimental work then tells us something about the impacts of biodiversity loss then appears poorly justified.

We agree that our design allows us to quantify only the overall contribution of insects to wood decomposition, but not to make predictions about how the loss of certain species, declines in abundance, etc. may affect wood decomposition. We therefore toned down this line of argument throughout the paper as suggested. We retained only statements that biodiversity loss among decomposers is likely to affect wood decomposition and thus carbon cycling, since insects play a particularly important role especially in the tropics. We further clarified that whether insect biomass, abundance, species number, functional diversity or species interactions is a more important determinant of wood decomposition is poorly resolved and that detailed predictions require further studies. Our main statement now reads P. 7, L. 166: "*Our results further indicate that insect biodiversity loss has the potential to affect deadwood decomposition, but the effects may vary regionally. To improve predictions regarding the functional effects of biodiversity loss, more research is needed on how specific components of decomposer communities (i.e., biomass, species number, functional composition, species interactions) affect deadwood decomposition3. If biodiversity loss affects critical components of decomposer*

communities, the strongest effects on deadwood decomposition can be expected for regions with warm and humid climate."

4. a) As the authors well know, many temperate forests contain termites; and termites are not uniformly distributed at a site. Hence, they need to clarify what the termite designation means, especially because it is such a central part of the story. Is it that termites were present at the site? Or do they categorize each log decomposition estimate on the basis of whether or not termites had colonized the wood? This is an important point for the reader to assess the termite contributions to wood decomposition.

It was not the aim of the study to disentangle the effects of termites and other insects. For this, more specific experiments are needed (Ulyshen et al., 2020)**. We added the information whether termites occur in the study region or not to Fig 3 to show that not all temperate and tropical sites had termites. We have now clarified this in the revised version of the manuscript.**

b) As an aside, many wood-feeding termites are negatively phototropic. Hence, they usually feed within the log. How then do the authors think that the small diameter of the experimental logs used influenced termite colonization and subsequently feeding rates?

There are indeed termites which feed exclusively inside larger logs but other species coat smaller items with soil and then feed underneath (Bignell, 2018)**. Termites did colonize wood in our experiment. Whether our functions based on fine wood debris (FWD) allow upscaling of carbon loss at a global scale including also coarse wood debris (CWD) is one of the key issues we tried to assess in our sensitivity analyses and by comparing our model predications against independent data** (Chambers et al., 2000)**. Here is what we discuss on this topic in Supplementary Information section 1 (P. 9) "*Our models of global carbon release from deadwood considered deadwood over the full diameter range (>2 cm). To avoid the problem that our models overestimated global carbon release from deadwood due to differences in decomposition rates of FWD and CWD, we applied a conversion factor relating annual wood mass loss of FWD to CWD. This factor was based on data from eleven peer-reviewed studies including data for both CWD and FWD, and covering tropical, temperate, and boreal forests (Supplementary Table S1-4). As the relationship of CWD loss rate over FWD loss rate was robust across different climates, we used its median value (0.53) in our predictions. To test whether decomposition rates based on our experiment (and corrected for differences between FWD and CWD) matched decomposition rates from the literature, we predicted decomposition rates for sites shown in Fig. 3 of Chambers et al.[6]. This evaluation revealed good levels of correspondence (Extended Data Fig. 7) indicating that our models of global carbon release from deadwood provide reliable estimates despite being based on experimental data with FWD."*

5. The claim is made in the conclusions that the authors' findings suggest that rising temperatures should accelerate wood decomposition rates, and they cite numerous mechanisms that might lead to such a consequence. But there is some work showing that such expectations are not realized when soil insects that feed on wood rot fungi are present, and instead warmer temperatures might accelerate feeding rates of such fungivores, which reduces wood decomposition under warmer conditions (e.g. Crowther et al. 2015 PNAS). Such alternate outcomes and associated explanations should likely be cited, especially given the focus of the paper on insect effects.

The prediction that decomposition rates will likely increase as temperature increases is based on the observed positive effect of temperature on overall decomposition rates and decomposition by insects in our experiment. Acknowledging the complex interactions between temperature and precipitation, we now clarify throughout the paper that the effect of temperature depends on precipitation. As pointed out in our response to comment 3a, our estimate of the effect of insects on decomposition is a net effect of direct and indirect effects. Thus, our prediction includes also indirect effects of insects via interactions with microbes and their changes along climate gradients. We are therefore confident that our predictions in this regard are valid. However, we fully agree that climate change affects species differently and thus, communities and species interactions will likely change. This is something which cannot be addressed by our data and is therefore not considered in our predictions. We had mentioned this in the initial submission, but state it more explicitly now including the suggested reference (P.7, L.161): "*Climate warming could accelerate wood decomposition by increasing microbial activity and insect-mediated wood mass loss, particularly where precipitation is sufficient. However, predicting climate change effects on populations and species distributions is challenging*[37]*, particularly for insects with limited dispersal ability*[38]*. More research is needed to assess how climate change affects decomposer communities and the interactions between insects and microbes*[39,40]*."

We also discuss the role and uncertainty associated with insect-fungal interactions in more detail in Supplementary information section 1 (P.3-4).

6. a) Can the authors discuss any caveats of their design. For example, the logs appeared to sit on a steel mesh. Yet we know that moisture is critical for fungal-mediated wood decomposition. Could the logs wet-up appropriately in the field with the steel mesh barrier between them and the soil surface? Do the authors have fresh masses to substantiate any conclusions here? And, if you do, how variable was moisture within the site and the experimental logs?

Thank you for this comment. We note that water content of wood depends not only on soil contact but also on decomposer activity, decomposition stage, precipitation and evaporation. Insects, for example, may increase water content

as tunnels and cavities may allow higher water uptake (Harmon et al., 1986)**, or by promoting fungi** (Jacobsen et al., 2018) **as fungal tissue has a high water content itself. Unfortunately, our design does not allow separating the different potential sources of water (an additional treatment involving logs suspended above ground would have been needed to quantify water uptake from soil).**

We are, however, confident that all logs and all treatments had sufficient soil contact to allow both water uptake and fungal colonization (see comment 6 b) from the soil. To ensure that all logs had close soil contact, we removed undecayed leaf litter and nailed the cages and the steel mesh underneath tightly to the ground using tent pegs (see photographs). Moreover, both the steel and nylon mesh were thin and flexible enough for the logs to push them to the ground by the logs´ weight.

We clarify this in the methods section (P.20, L.419): ***"The top layer of fresh leaf litter was removed before the installation of treatments. The cages and layers of steel mesh were both fixed tightly to the ground using tent pegs, to ensure that all deployed logs had close contact to the soil and to allow water uptake and fungal colonization from the soil."***

b) Minor point - please use SI units. As of now, the mesh is listed as "1000 mesh per square inch". That does not tell the reader the mesh size in mm of the openings, as we would want but forces them to search online. And, in doing so, it seems that that mesh is something like 10- to 15-micron wide apertures. Is that the case?

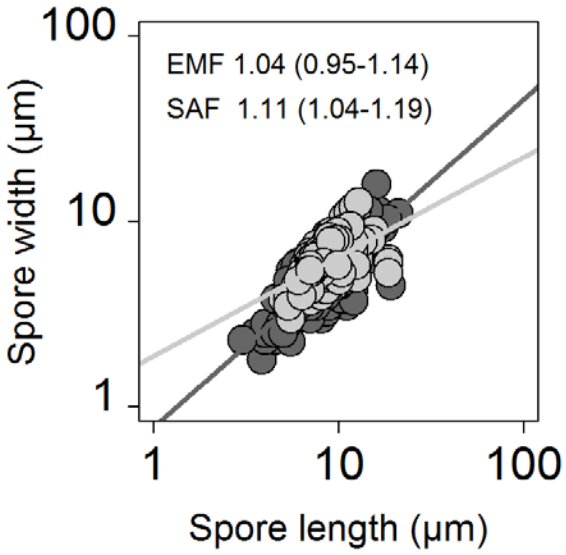
Mesh per square inch is a common unit in the textile industry. But of course you are right that no one knows what this means without searching for it. We measured the mesh width and it was approx. 0.5 mm. We have added this information to the Methods description.

b) If it is that small, can the hyphae of multi-piece wood-rot fungi penetrate that to access the wood, especially where they are aggregated to minimize fungivory damage?

Our mesh size was 0.5 mm (500 µm). The spore sizes of saprotrophic fungi are several orders of magnitude smaller (e.g. Knudsen and Vesterholt, 2012)**. For**

example, spore size of saprotrophic fungal species from a broad range of lineages (>350 species) covering northern Europe are on average 8.9 µm (max 21 µm) length and 5.5 µm (max 16 µm) width (see Figure S2 in Bässler et al., 2015)**. The maximum spore length observed and used within a broad range of wood-inhabiting fungal studies was from an Ascomycete (*Melanamphora spinifera*, 67.5 µm),** (e.g. Bässler et al., 2014)**, which however, seems an exception when compared to the spore size of co-occurring species (particularly basidiomycetes). Note that the fungal lineages used in these studies are distributed across our global data set and the range of spore sizes is therefore most probably comparable. This is supported when focusing on spore sizes within fungal floras and monographs across different biomes of the world** (Bässler et al., 2015; Halbwachs et al., 2015).

Relationships between spore width and spore length for ectomycorrhizal mutualistic fungi (light grey) and saprotrophic fungi (dark grey). Data taken from (Bässler et al., 2015)**:**



The majority of fungi propagate as spores, however, some basidiomycetes can grow out of a resource as mycelium in search of new resources (Boddy et al., 2009)**. Similarly, like spores, single hyphae of wood decaying fungi are far below the size of the mesh width. The width of most hyphae of wood decaying fungi range between 5-20 µm** (Eriksson and Ryvarden, 1987; Ryvarden and Gilbertson, 1994)**. The largest width observed within the fungal kingdom is 100 µm** (Schwantes, 1996)**. However, some species linearly aggregate several hyphae, called mycelial cords or rhizomorphs. Within the tremendously species rich group of wood-decaying fungi, only few form rhizomorphs which can reach the size of our mesh width (e.g., species of the genus *Armillaria*). However, during apical mycelial growth in order to exploit new resources, not the whole organ (rhizomorph) does extend apically; rather, it develops behind a mycelial margin of diffuse hyphae, each of which extends apically** (Boddy et al., 2009; Clemencon,

1997; Moore, 1998)**. That means that each fungus, irrespective whether it produces mycelial cords and rhizomorphs or not, can enter barriers that are larger in diameter than a single hypha, which is the case for our mesh. Note furthermore that fungi can develop the smallest unit (single hyphae) from all organs already developed (e.g. cord, rhizomorph and fruit body)** (Clemencon, 1997; Moore, 1998)

We therefore can safely assume that the colonization of our substrate by wood decaying fungi by spores and hyphae was not limited. We added this information to the Methods section (P. 20, L. 426): *"The mean spore size and hyphae width of saprotrophic fungal species (mean spore length and width: 8.9 µm and 5.5 µm[45]; hyphae width: 5-20 µm[46,47]) is by order of magnitude smaller than the mesh width of our cages. Rhizomorphs, i.e. linear aggregations of several hyphae, can be wider, but during mycelial growth each hypha extends apically rather than the whole rhizomorph[48–50]. Therefore, it is unlikely that the cages hampered fungal colonization."*

To conclude: those data collected have the potential to contribute to the examination of an important knowledge gap in the global carbon cycle. However, the data handling and then subsequent statistical interpretation of the experimental data is opaque and, from what I could glean, is not yet rigorous enough to support the extrapolations and the conclusions reached in the paper. Some reframing of the contribution also seems necessary. Hopefully the authors can address these concerns because they have a large number of observations across many sites, and so are potentially sitting on a very valuable dataset.

Thank you again for this positive statement and the very constructive and elaborate comments. We hope that we could clarify all methodological issues that were unclear, and in doing so rule out future misunderstandings of the reader.

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Reviewer Reports on the First Revision:

Ref # 1

I'm happy with this set of changes. The points I raised have all been dealt with appropriately.

Ref #2

I was reviewer 3 for the original submission of Seibold et al. I very much appreciate the extensive clarification that the authors made around the statistics and experimental design, so that the reader can clearly understand what is meant by insect effects (i.e. their cumulative, additional indirect and direct impacts). Overall, I was once again impressed by the huge amount of work across so many sites that went into producing this dataset, and the findings are certainly of broad interest for our basic understanding of decomposition (which tends to be built only on foliar decomposition rates) and potential consequences for global forest C stocks and flows.

That being said, I view the extrapolation using the experimental data and estimates of deadwood C stocks as being a first pass. I'm fine with this because it gives us some numbers to work with but at the same time there remain for me some concerns that I think the authors need to address to ensure that the results they present are as rigorously presented as is possible. But really my primary concern, now that I understand fully what was done, is whether the uncaged vs. closed comparison reveals the insect effects the authors claim they do (see comments 2 and 3). I also think that if the authors are not upfront and explicit that the numbers they give refer to additionality of insect effects, not insect effects per se, then the numbers will be misused going forward by most in the global C flux community (see comment 6).

1. But I'll start my comments with the deadwood C flux extrapolation. You mention line 533 that the total amount of C stored that you estimate is in standing and downed deadwood. However, standing deadwood generally decomposes much more slowly than downed deadwood, simply because downed wood is much more likely to be in direct soil contact and so will have (higher) moisture values that expedite fungal and insect activity. Notably, your experimental logs were all in ground contact. Therefore, really it seems your global C flux extrapolations should be restricted to downed dead wood, with standing dead wood excluded. I do appreciate that the operational definition for standing vs. downed is typically whether the bole of a dead tree is above or below a 45-degree angle, and so it's possible to have downed wood that is essentially standing, but I think for a first pass estimate such as you have this is not a deal breaker. If you choose to retain standing deadwood stocks in your extrapolation, I am keen to understand how you justify its inclusion.

2. Your statistical clarifications are much appreciated, as is the focus on effect sizes as opposed to solely statistical significance. However, you appear to revert back to a reliance on P values when you discuss whether there were microclimate (temperature and RH) artifacts of the mesh of the cages. In your extended data table 3, although the effects are not significant, there are large enough coefficients for closed vs. uncaged, and open vs. uncaged, to suggest that the cages did have a strong impact on microclimate. Further, it does not seem legitimate to analyze these data for all biomes together - we should actually see data for boreal, temperate and tropical separately, because cage effects can be expected to be greater under warmer and/or more mesic conditions. Overall, then, you have biome variation and relatively few microclimate observations (compared to logs), which should decrease the precision of your estimates of the cages on microclimate artifacts. As such, you would expect to find a non-significant P value, even when the coefficient is quite large (and they appear to be!). What might be more compelling is whether you could report moisture contents of the experimental logs and dowels, so that the reader can at least assess whether log moisture values were altered or not by the cages.

3. The additional microclimate analyses are really critical for placing your results in context of what

they can and cannot tell us. You provide numerous additional analyses to justify the closed cage vs. uncaged difference as the one that is of relevance for quantifying insect effects. However, in an ideal world your open cage decomposition rates would have matched much more closely those of the decomposition rates of the uncaged treatments, or at least been much more similar to the rates of the uncaged treatments than to the rates of the closed cage treatment. Then you would have had a case for minimal cage artifacts. In reality, this did not occur, suggesting that there are strong impacts on decomposition rates of caging. What you try to do is build the case that your open cage controls have much more similar decomposition rates to the closed cages because of impacts on insect activity, as opposed to artifacts through non-insect mechanisms such as altered microclimate. I appreciate the challenges you have here - they are a product of doing such work in the field and trying to develop suitable controls to account for non-target artifacts. But I think you need to explore further potential microclimate effects (comment #2 above) and be upfront in the manuscript that your controls to identify microclimate artifacts were ineffective because they also appeared to decrease insect activity. For example, on lines 82-84 you describe the open cage treatment as a control to correct for artifacts, and I think here you should highlight that your 'artifact control' was ineffective and not then used in estimating insect effects. You do state the comparison you make in the main text, (lines 94-96) but I think you should be more direct about the failure of the open cage controls. Especially because you really have no way of proving your assertion that open cages excluded parts of the wood-decomposing insect community. You acknowledge this in your choice of language - for example, on line 737, you note that it suggests this explanation.

4. A point of confusion that remains for me is that you give data on annual rates of wood decomposition, and yet you collected (when you could) samples at 1 year, 2 year and 3 years of field incubation. With leaf litter decomposition, we expect that decomposition is more rapid at the start as more labile compounds are used, and then decomposition rates progressively slow as more recalcitrant compounds become proportionally more abundant. Please can you explain in the paper whether this expectation holds or not for wood. It seems particularly pertinent, because otherwise why not present the 1-year field incubation data separately to the 2- and 3-year data? In short, I am confused what an annual rate of decomposition actually means in the context of your experiment - is it an average of faster rates in year 1 and slower rates in year 3, for example?

5. You make clear in your rebuttal letter that the sampling design is unusual. That is, you harvested a full cage (or uncaged plot) each year, rather than sampling a log from each cage/ plot in the experimental array. I understand the somewhat unusual sampling strategy here but you do not tell the general reader why. It does feel out-of-sorts to do it the way you did, but thinking about this logically I am not convinced that it would have changed any of your findings because sites were so far from one another, and cages/plots so near to one another within each site, that I do not think I could argue that your replication is flawed because of the way you sampled. But the reader will wonder why you did it the way you did, so I suggest adding the explanation from your rebuttal letter to the text of the script (i.e. you were not convinced the cages would all survive 3 years).

6. Lastly, I remain deeply concerned by the inaccurate wording of your summary and main results/ discussion text. For example, lines 36-37 give an estimate of insect contributions to global deadwood C fluxes. But, as you make clear in your rebuttal letter, these are actually additional contributions over-and-above what would be observed if insects were not present. As such, you're giving an estimate of the additionality of insect effects. I think you need to say in the Summary that you are reporting additionality, so that such a high-profile paper does not mislead the field. For example, if termites consume 20 percent of the wood when present and, when absent, fungi compensate for their loss by consuming 18 of this 20 percent of wood that termites would otherwise have consumed, then the additional insect effect is 2% wood decomposed. That's what you're estimating: the additionality. And not insect contributions per se to dead wood decomposition. But the average reader, without detailed knowledge of animal effects on wood

decomposition, is going to be misled. Maybe you think this is not relevant for global C fluxes, but I would offer a different opinion - presumably fungal vs. termite biomass has a very different fate in ecosystems which will influence C flow through foodwebs and potentially C persistence. As such, I do not think it is fine to bury this additionality in the main text, in a manner that will be opaque to the general reader.

Ref #3

General Comments:
The manuscript by Seibold et al. describes a unique and very interesting study of impacts of insects on decomposition rates comprised of two parts: a three-year field experiment conducted at 55 sites on 6 continents and a model-based analysis of carbon released from global dead wood pools. The first part of the study is very well executed. The authors have addressed the comments of the two reviewers and documented their responses and improvements to the manuscript. As a new reviewer, I am satisfied with these revisions and I have only minor additional concerns with the field study that I think will be easily addressed. I therefore fully support publication of this very important component of the study.
Unfortunately, the second part of the study has some serious flaws that will be harder to address and which were not identified by the previous review. My concern relates to the extrapolation of results from a 3 year field study of decomposition rates of dead wood to global estimates of carbon release from dead wood pools used to estimate the contribution of insects to these decay releases. Wood decomposition models typically do not represent time since death for deadwood pools (i.e. the pool has no age-structure) because the computational overhead to represent decaying wood cohorts with varying time since death is very large. It is therefore common practice to use an approximation (with a deadwood pool that does not track time since death). This approximation works reasonably well, despite recognised shortcomings. The problem with this approach is that while decay of deadwood pools over time can be approximated with an exponential decay function (as most models do), the reality is that the initial decay of wood is much slower than predicted by the exponential model, as colonization of the newly dead wood by fungy, insects and mircobes requires time. The exponential decay function therefore overestimates intial decay and underestimates decay of wood that is in advanced stages of decay. Because deadwood pools contain material with a wide range of time since death the model approximation of the decay for the entire pool is a reasonable approximation.
In this study, the authors used the observed rates of mass loss in the first one to three years of the time since death as affected by insects to derive an estimate of the overall C loss from deadwood pools. In cool (temperate) and cold (boreal) climates mass loss in the first three years since death will be very low (or even negative as observed by the authors in some boreal samples). The three year estimate of decay rates is not an indicator of the mass loss rate over the decay period and therefore, their estimate of rate of loss based only on one to three years of observation cannot be extrapolated to the entire global deadwood pool.
The authors claim that their estimates of decay are in agreement with the literature, but the only study they cite to confirm this is Chambers et al (2000) (#7 in the Supplement). However, that study was conducted in the tropics with high rates of decay even in the first three years and I would therefore expect good agreement between their method and the literature for tropical regions. But in temperate, and even more so in boreal regions, initial wood decay rates (over the three years of observation) will be much slower – or in some cases even negative. The decay rate over the first three years is in no way representative of the half live or average decay rate for the pool and it is therefore incorrect to use observed mass loss over 3 years to estimate C releases from global deadwood pools.
A second issue is that while the authors did recognise and adjust for the contribution of piece size (adjusting their observed decay rates for fine wood to an estimate for all deadwood), they failed to recognise or adjust their estimates for the very large difference in decay rate between standing and downed dead wood. Standing dead wood decomposes much more slowly than downed wood. For example see Fig 1 in Hararuk et al. 2020 (https://doi.org/10.1186/s40663-020-00248-x) which indicates nearly a factor 2 shorter half-life for downed wood compared to standing dead

wood.
It would also be helpful to the reader to provide a clear definition of deadwood – ideally consistent with that of the IPCC's GHG estimation guidelines. It is important for the reader to understand that the estimate of global deadwood pools includes standing dead trees (which is good) but the estimate of decay rates in this study was derived only from downed wood samples (3 cm in diameter), and that is problematic.
The concluding statement at the end of the Supplementary Material, that future studies should also investigate large diameter wood is correct, but I suggest that it is even more important that future studies (in boreal regions) should be conducted over longer periods (or include some wood samples that initially are in advanced stages of decay).
In summary, this novel and unique global experiment demonstrates that insects do affect rates of decomposition in the tropics. However, I suggest that three years was simply not long enough to draw any conclusions of the impacts of insects in the boreal. The authors do show that the impact is small in the first three years, but I would expect this impact to increase (somewhat) with increasing time since death of the deadwood in the boreal (and likely in the temperate) regions. This should be clearly stated in the manuscript.
This is an important study, despite the flawed analysis of the global release of C from deadwood pools. I understand the desire to publish in a high-impact journal such as Nature and, therefore, the need to present results and conclusions that are of global significance. The experimental part of the study is excellent and should be published, but further analyses of the global estimates of C release from deadwood pools will be required before the entire study can be accepted. These estimates must not be based on only the mass loss rates observed over the initial three years after death. I realise that with revised estimates of mass loss that are based on other data sources, the impacts of insects will no longer be quantified. However, failure to correct this prior to publication will results in proliferation of a seriously flawed estimate of C releases from global deadwood pools that greatly underestimates the release in boreal regions, and to a lesser extent also in temperate regions.

Specific Comments:
L32: I suggest rewording this from "we estimate that 11.7 Pg of carbon are lost from deadwood every year." To "we estimate that 11.7 Pg of carbon are released from deadwood every year." The term lost could be misinterpreted to imply that the deadwood C pool decreases by that amount – it does not, because new deadwood is added continuously. The term "loss" is used in other places (e.g. Fig 1 legend and caption). I suggest that the authors consider changing this from 'loss' to 'release' throughout. L98 explains the intended use of the term (which is helpful) but I would still consider using the term carbon release rather than carbon loss.
L37: Suggest revising "Insects accelerate decomposition in tropical forests but have neutral or decelerating effects in temperate and boreal forests" to "Insects accelerate decomposition in tropical forests but have neutral or decelerating effects in temperate and boreal forests in the first three years of wood decay".
L40: It is hardly new or a surprise that "global warming could accelerate deadwood decomposition" – in fact, given the sophistication of the experiment and the richness of data, I am surprised that this is the key message highlighted here. There is so much valuable information in this study that I wish the authors had come up with a conclusion that emphasises their new findings rather than merely reinforcing temperature dependence of wood decay, which is at the core of even the most basic models of deadwood decomposition (and has been for 30 years or more). Moreover, acceleration of deadwood decomposition says nothing about net impacts on the C balance because global warming is likely to affect many other processes, including the productivity of forests, and the rate of disturbances (including insect-mediated tree mortality) – both of which can increase input of biomass to deadwood pools.
L100: "synthesized from empirical and remote-sensing data" – emphasize here already that this was done by the authors (as becomes apparent when reading the methods).
L113: "Increasing precipitation affected decomposition rates negatively at low temperatures but positively at high temperatures." Can the authors please modify this statement to read: "Increasing precipitation affected initial decomposition rates negatively at low temperatures but

positively at high temperatures." Their study was limited to three years – which in boreal environments is really too short for the observation of mass loss in woody materials. I caution against generalisation of this finding. It points in fact at a different (well known) problem – namely that models that use a simple decay function to represent dead wood decomposition C releases tend to overestimate the decay in early years in cold climates.
L123: "The strength of this increase is modulated by current and future levels of precipitation" – this is only partly correct – the main driver is not precipitation but site water balance, which is determined by P and evapotranspiration. Hence, even where P increases, the site may become drier if T increases relatively more. A mention that it is not P directly but its impact (together with changes in T and site and soil characteristics) that will affect decomposition rates may be useful – even if this study only looked at P and not water balance. The wildfire community has sophisticated models to calculate the daily moisture content of dead wood. To address this, the authors could simply state "The strength of this increase is modulated by current and future levels of precipitation and precipitation impacts on site water balance". See Smyth et al 2011. https://doi.org/10.1016/j.ecolmodel.2010.12.005 as an example of an approach to represent water stress impacts on decomposition rates.
L193+: The statement about "high intensity management in the majority of these forests" is wrong. By far, the majority of the boreal forest area is located in Alaska, Canada, and Russia – and these are not intensively managed – huge areas are not managed at all. Yes, boreal forests in Finland, Norway and Sweden are intensively managed and the statement is correct for those systems – but it is wrong for the vast majority of the world's boreal forests.
L199+: "The contribution of insects to wood decomposition is slightly negative in some regions with cool temperatures and high precipitation, such as the Pacific Northwest of North America." I am concerned about the validity of this statement – all the authors have observed in their experiments is the contribution of insects in the first 3 years of decomposition (on average only for 2 years). I suggest that the impact will increase in time as decomposition progresses, and wood becomes softer and therefore more accessible to insects. The authors should highlight the temporal limitation of this study and add a caveat that longer-term impacts of insects outside the tropics could be underestimated.
L206: "were the most important source of uncertainty" – only of those sources of uncertainty that were assessed.
L219: "their effect is currently small in temperate and boreal biomes, where they can even slow decomposition." – as stated above – this conclusion if in part an artefact of the duration of this study. I suggest that the authors rephrase this as – " … where they can even slow decomposition in the first three years" or some other way to indicate the time constraints of this analysis.
L225: If the authors make a general and sweeping comment about how global carbon models need to be improved, they should add a statement about the impacts of disturbances – in particular wildfire – which are not or poorly represented in many global models. Fires, in particular, can contribute to the rate of C input to dead wood pools, and they can accelerate release of C from deadwood pools.

Author Rebuttals to First Revision:

Point-by-point response to the referees´ comments to Seibold et al. "Deadwood decomposition mediated by insects influences global carbon fluxes".

Referee #1 (Remarks to the Author):

I'm happy with this set of changes. The points I raised have all been dealt with appropriately.
Thank you for your positive evaluation.

nature portfolio

Referee #2 (Remarks to the Author):

I was reviewer 3 for the original submission of Seibold et al. I very much appreciate the extensive clarification that the authors made around the statistics and experimental design, so that the reader can clearly understand what is meant by insect effects (i.e. their cumulative, additional indirect and direct impacts). Overall, I was once again impressed by the huge amount of work across so many sites that went into producing this dataset, and the findings are certainly of broad interest for our basic understanding of decomposition (which tends to be built only on foliar decomposition rates) and potential consequences for global forest C stocks and flows.
Thank you for acknowledging the value of our experiment.

0a) That being said, I view the extrapolation using the experimental data and estimates of deadwood C stocks as being a first pass. I'm fine with this because it gives us some numbers to work with but at the same time there remain for me some concerns that I think the authors need to address to ensure that the results they present are as rigorously presented as is possible.
Thank you for this comment. We agree that our global estimates are only a first step in a better quantification of the role of deadwood decomposition in the global carbon cycle. Specifically, we feel that our global estimates contribute in two ways to improve our knowledge: 1) They provide a first estimate which come with some uncertainties but external validation of our experimentally derived decomposition functions in the last version and now updated by a new, much larger validation data set, indicate that our estimates are highly robust (for details, please see comment 4 of reviewer 2). Recent studies, such as Harris et al., 2021, *Nature Climate Change* ("Global maps of twenty-first century forest carbon fluxes"), which contain only basic deadwood carbon models show that such a first estimate of the global deadwood carbon flux is urgently needed. In addition, the forthcoming IPCC report highlights the process of tree mortality (and the subsequent deadwood dynamics) as one of the major areas of uncertainty in the terrestrial carbon budget. 2) Based on our sensitivity analyses and the general results, we can derive some recommendations about how global deadwood carbon models can be further improved, which will help to guide future research. We have now clarified this in the main text in several places:
P. 5, L. 104: " ***To provide a first estimate of the global carbon flux from deadwood decomposition (henceforth referred to as deadwood carbon release for brevity) and to quantify the functional importance of insects for global deadwood carbon, we applied the model derived from our decomposition experiment to a novel global deadwood carbon map (Fig. 1a), which we synthesized from empirical and remote-sensing data. As the global modelling of deadwood remains challenging, we conducted in-depth analyses of uncertainty, evaluating the decomposition function derived from our experiment against independent empirical data34 and quantifying the relative contribution of different sources of uncertainty in a sensitivity analysis (Supplementary Information section 2 and Extended Data Table 2). The sensitivity analysis also highlights how further research can improve the modelling of global carbon fluxes from deadwood."***
P. 9, L. 214: "*Our global estimates are only a first step in a better quantification of the role of deadwood decomposition in the global carbon cycle. Uncertainties related to the underlying data, the statistical models, and other assumptions necessary for upscaling our experimental results were assessed in a global sensitivity analysis. This analysis bounded the uncertainty of global annual carbon release from deadwood and the net effect of insects at approximately ±25% around the mean. Of the various sources of uncertainty that were considered, the underlying data on deadwood carbon stocks contributed most strongly to overall uncertainty (Fig. 3; Extended Data Table 2; Supplementary Information section 2).*

Our results suggest that global deadwood carbon cycle assessments could be improved by more accurately quantifying deadwood stocks in large tropical countries as Brazil or Indonesia."

0b) But really my primary concern, now that I understand fully what was done, is whether the uncaged vs. closed comparison reveals the insect effects the authors claim they do (see comments 2 and 3). I also think that if the authors are not upfront and explicit that the numbers they give refer to additionality of insect effects, not insect effects per se, then the numbers will be misused going forward by most in the global C flux community (see comment 6).
Thank you very much. This issue is important as the results of the paper should not be misunderstood by the readers. Our experiment compares decomposition rates for wood to which insects had access versus where they had no access. We fully agree that this difference is not simply representing the amount of wood consumed by insects, but it is the net effect of insects and other decomposers (mainly fungi). So, e.g. insect exclusion reduces competition between insects and fungi leading to higher decomposition by fungi when insects are absent and thus, as pointed out correctly, fungi may compensate partly for reduced decomposition by insects. The opposite is also possible, that is, insects influence potent fungal decomposers leading to lower decomposition rates when insects are absent. Moreover, although we consider effects of caging on microclimate to be low (temperature and humidity have counteracting effects; please see our response to comment 2), we cannot rule out some effects on decomposition by fungi. We also fully agree that the fate of carbon may differ between decomposition by fungi and insects, but disentangling these different processes is challenging, if not impossible, at least at the scale of our experiment. We do, however, think that the first priority should be to quantify the overall, net effect of insects on wood decomposition and then subsequently, more refined experiments should try to quantify the various underlying processes. We had mentioned that our measure of "insect contribution" is the result of direct consumption and indirect effects via fungi. However, we fully agree that this might be overlooked by some readers. We therefore changed our terminology throughout the paper, now referring to "the net effect of insects". We define the term and the underlying mechanisms upfront in the Introduction (P. 4, L. 80): "*Our estimate of the effect of insects on wood decomposition was quantified as the difference between decomposition rates in the uncaged and closed cage treatments. This measure can be considered the "net effect of insects", consisting of direct consumption of wood by insects and indirect effects via interactions with wood-decomposing microbes. Interactions of insects with microbes include, for example, competition for resources, grazing on fungal mycelia, creation of entry ports or vectoring, and can thus either increase[30] or decrease wood decomposition[31,32]. Consequently, direct consumption by insects could be higher than our net estimate where insect-microbe interactions decrease decomposition rates."*

1. But I'll start my comments with the deadwood C flux extrapolation. You mention line 533 that the total amount of C stored that you estimate is in standing and downed deadwood. However, standing deadwood generally decomposes much more slowly than downed deadwood, simply because downed wood is much more likely to be in direct soil contact and so will have (higher) moisture values that expedite fungal and insect activity. Notably, your experimental logs were all in ground contact. Therefore, really it seems your global C flux extrapolations should be restricted to downed dead wood, with standing dead wood excluded. I do appreciate that the operational definition for standing vs. downed is typically whether the bole of a dead tree is above or below a 45-degree angle, and so it's possible to have downed wood that is essentially standing, but I think for a first pass estimate such as you have this is

not a deal breaker. If you choose to retain standing deadwood stocks in your extrapolation, I am keen to understand how you justify its inclusion.

Thank you for this comment. Decomposition rates of standing and downed deadwood may indeed differ, but the literature shows that this is variable and dependent on climatic conditions: In tropical forests, standing deadwood has similar or sometimes even higher decomposition rates than downed deadwood (Chambers et al., 2000; Hérault et al., 2010; Gora et al., 2019). In boreal and temperate zones, however, lower decomposition rates for standing compared to downed deadwood are most realistic as shown by Hararuk et al. (2020) for *Fagus sylvatica* and *Picea abies* in Switzerland, due to the dryer conditions in standing trees. Differences in decomposition rates between standing and downed deadwood vary substantially depending on tree species, diameter, local climate and site conditions as well as between angiosperms and gymnosperms (Harmon et al., 2011). Since the aim of our study was to provide an overall global estimate for carbon flux from deadwood including all forms of deadwood, we did not restrict our upscaling to downed deadwood, but modified our decomposition function to account for lower decomposition rates of standing deadwood and included standing deadwood in our sensitivity analysis. In specific, we did the following:

1. **First, we derived a conversion factor for decomposition rates of standing versus downed deadwood in temperate and tropical from Harmon et al. (2011). We then took information from large inventories or distributed study sites to estimate the range of the share of deadwood stocks represented by standing deadwood (Richardson et al., 2009; Thünen-Institut für Waldökosysteme, 2014; Shorohova & Kapitsa, 2015; Szymański et al., 2017; Puletti et al., 2019). In our upscaling analyses, we then applied modified decomposition functions (lower decomposition rates) to the part of the deadwood stock represented by standing deadwood. This reduced our global estimate, but the effect was quite small since most carbon is released from deadwood in the tropics.**
2. **Both, conversion factors for decomposition rates and the share of standing deadwood, are associated with uncertainty since data are scarce and variable. We thus included standing deadwood as a factor in our sensitivity analyses to assess its potential relevance. We also want to stress here, that it was not possible to validate our decomposition function including the standing-downed conversion factor against independent data, since even the most comprehensive dataset to date (Harmon et al., 2020) did not include sufficient data for standing deadwood.**

These analyses are now included in the paper and a detailed discussion on this topic was added to the new Supplementary Information Section 2.

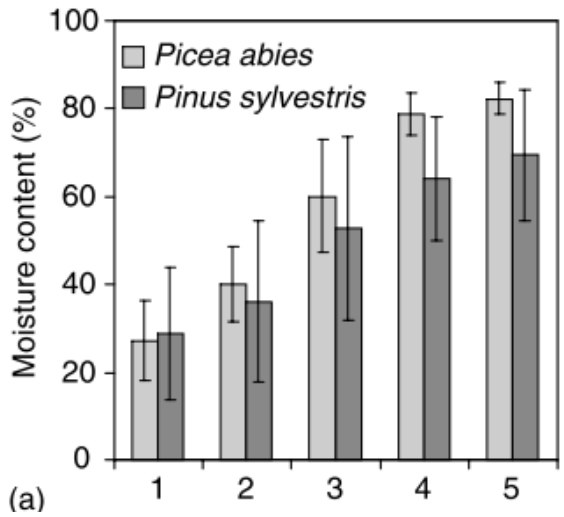
2. Your statistical clarifications are much appreciated, as is the focus on effect sizes as opposed to solely statistical significance. However, you appear to revert back to a reliance on P values when you discuss whether there were microclimate (temperature and RH) artifacts of the mesh of the cages. In your extended data table 3, although the effects are not significant, there are large enough coefficients for closed vs. uncaged, and open vs. uncaged, to suggest that the cages did have a strong impact on microclimate. Further, it does not seem legitimate to analyze these data for all biomes together - we should actually see data for boreal, temperate and tropical separately, because cage effects can be expected to be greater under warmer and/or more mesic conditions. Overall, then, you have biome variation and relatively few microclimate observations (compared to logs), which should decrease the precision of your estimates of the cages on microclimate artifacts. As such, you would expect to find a non-significant P value, even when the coefficient is quite large (and they appear to be!). What

might be more compelling is whether you could report moisture contents of the experimental logs and dowels, so that the reader can at least assess whether log moisture values were altered or not by the cages.

Thank you for this comment. We fully agree that our models of microclimatic data from data loggers have to be interpreted with caution given that the number of sites was limited. As suggested, we added models testing for differences between treatments for each of the three major biomes. We now also discuss results not based on p-values, but compare effect sizes and directions. We now present and discuss this in Supplementary Information Section 1. Briefly, we find a trend towards warmer and less humid conditions inside cages. Effects were overall rather weak, with the strongest effects on temperature in the tropics and on humidity in the temperate biome. Based on the positive effects of temperature and precipitation on wood decomposition (independent of insect activity), we expect that caging could have counteracting effects on decomposition rates (i.e. a positive effect via temperature and a negative effect via humidity). These effects have the potential to influence our quantification of the net effect of insects in different ways: higher temperature and lower humidity inside cages would lead to underestimation and overestimation, respectively, of net insect effects. Since temperature effects were most pronounced in the tropics and humidity effects at temperate sites, our estimate of the net insect effect may thus underestimate the net effect of insects in the tropics and overestimate the effect of insects at temperate sites. This means that if microclimatic effects of cages occurred and affected decomposition rates, our overall pattern of high net insect effects in the tropics and small net insect effects in the temperate zone would be even more pronounced without microclimatic effects of caging. We are not able to quantify the net effect of these processes, nor their interacting effect. Nevertheless, given that the effects seem to be rather weak and counteracting, we are confident that our estimate of the net insect effect is robust. Please note that our function and estimate for total wood decomposition is completely independent of potential microclimatic effects, since they are based only on the uncaged treatment.

Moisture content was not part of our experimental protocol and thus not assessed consistently across sites. However, wood moisture is not a good measure to assess microclimatic effects of caging since wood moisture increases steadily during the decomposition process (Dix, 1985; Sollins et al., 1987; Renvall, 1995) from around 30 to 80% water content (Response Fig. 1). In contrast, the maximum difference in relative humidity recorded by our data loggers was 1.73%. Therefore, we expect treatment to affect moisture content predominantly via differences in decomposition rates and only marginally via changes in relative humidity.

Response Figure 1: Changes in wood moisture content with ongoing wood decomposition (decay stages 1 to 5) after (Stokland et al., 2012 page 124).



3. The additional microclimate analyses are really critical for placing your results in context of what they can and cannot tell us. You provide numerous additional analyses to justify the closed cage vs. uncaged difference as the one that is of relevance for quantifying insect effects. However, in an ideal world your open cage decomposition rates would have matched much more closely those of the decomposition rates of the uncaged treatments, or at least been much more similar to the rates of the uncaged treatments than to the rates of the closed cage treatment. Then you would have had a case for minimal cage artifacts. In reality, this did not occur, suggesting that there are strong impacts on decomposition rates of caging. What you try to do is build the case that your open cage controls have much more similar decomposition rates to the closed cages because of impacts on insect activity, as opposed to artifacts through non-insect mechanisms such as altered microclimate. I appreciate the challenges you have here - they are a product of doing such work in the field and trying to develop suitable controls to account for non-target artifacts. But I think you need to explore further potential microclimate effects (comment #2 above) and be upfront in the manuscript that your controls to identify microclimate artifacts were ineffective because they also appeared to decrease insect activity. For example, on lines 82-84 you describe the open cage treatment as a control to correct for artifacts, and I think here you should highlight that your 'artifact control' was ineffective and not then used in estimating insect effects. You do state the comparison you make in the main text, (lines 94-96) but I think you should be more direct about the failure of the open cage controls. Especially because you really have no way of proving your assertion that open cages excluded parts of the wood-decomposing insect community. You acknowledge this in your choice of language - for example, on line 737, you note that it suggests this explanation.

We share your concerns about the general limitations of exclusion experiments (e.g. decomposer exclusion by cages or mesh bags, exclusion of pollinators from flowers, etc.). Unintended effects of exclusion are typical and most of the time impossible to avoid. On the other hand, in most cases they are the only available option for experimental studies such as the one we report here. As suggested, we changed our wording to be more upfront regarding these limitations. We changed the wording from "to control for effects of caging" to "*To explore effects of caging on microclimatic conditions and decomposition rates,…*" at the end of the Introduction (P. 4, L. 88). We have also added further discussion of this topic to the paragraph on uncertainties (P. 10, L. 223): "*Further uncertainty results from our experimental design: It cannot be ruled out that altered microclimatic conditions in cages affected estimates of the net effect of insects derived from the comparison between closed cage and uncaged treatments. Such a bias would lead to an underestimation of the net insect effect in the tropics and an overestimation in the temperate*

zone (Supplementary Information section 1). When the global annual net effect of insects on deadwood decomposition was derived from the comparison of closed cage and open cage treatments, it still amounted to 1.76 Pg carbon. However, this value underestimates the true effect of insects due to reduced insect colonization in the open cage treatment (Supplementary Information section 1; Extended Data Fig. 2).”

4. A point of confusion that remains for me is that you give data on annual rates of wood decomposition, and yet you collected (when you could) samples at 1 year, 2 year and 3 years of field incubation. With leaf litter decomposition, we expect that decomposition is more rapid at the start as more labile compounds are used, and then decomposition rates progressively slow as more recalcitrant compounds become proportionally more abundant. Please can you explain in the paper whether this expectation holds or not for wood. It seems particularly pertinent, because otherwise why not present the 1-year field incubation data separately to the 2- and 3-year data? In short, I am confused what an annual rate of decomposition actually means in the context of your experiment - is it an average of faster rates in year 1 and slower rates in year 3, for example?

Thank you for this comment. Please see also our response to comment 2 of reviewer 3. For each log, we have only one measurement of decomposition after either 1, 2 or 3 years. In our model, observations of mass loss over 1, 2 or 3 years enter the model individually. For each, the relative mass loss is related to the time of exposure in the model. You are therefore correct that, if decomposition rates decreased over time, relative annual mass loss of a log exposed for 3 years and thus considering the full three year period from year=0 to year=3 would be higher than when only a one year period from year=2 to year=3 was considered. In that case, our function based on observations for year= 0-1, 0-2 and 0-3 would overestimate decomposition rates for wood in later decay stages. Splitting our data in three subsets differing in exposure time and comparing results of separate analyses comes with several problems. First, the number of samples differs between subsets. Second, sites would differ between subsets with boreal and temperate sites being more frequent in the 2- and 3-year subsets due to low decomposition rates (note that some tree species at tropical sites had decomposed completely after only one year). Third, even with a balanced sample distribution, comparing decomposition rates over time in a chronosequence is not replacing real time series. We are therefore convinced that more detailed experiments following individual logs over time are needed if different periods of time are to be compared, but this is hardly possible at the scale of our experiment.

To assess whether the use of our decomposition function could produce biased results in the global upscaling, we compared our decomposition function derived from the experiment to independent empirical data from deadwood surveys (Harmon et al., 2020). This dataset is the most comprehensive global compilation of survey data available and covers a wide range of exposure times. Our decomposition function and the one based on Harmon et al. (2020) match very well (Extended Data Fig. 5). This suggests that using our decomposition function to estimate global carbon release from deadwood considering complete deadwood pools (i.e., with a broad range of exposure times) is providing robust results. We point to this in the Results section (P. 8, L. 186): *“Since our experiment focused on decomposition of small-diameter deadwood over three years, we adjusted decomposition rates to account for slower mass loss of large-diameter deadwood (for details see Methods and Supplementary Information section 2). We evaluated our relationship between decomposition rate and local climate against 157 independent empirical observations from previous deadwood surveys[34], spanning the full range of deadwood diameters > 7 cm, time since tree death and climatic conditions. We obtained a good match of the results from our*

model to these independent data (Extended Data Fig. 5), suggesting our approach is robust for deriving a first quantitative estimate of global carbon fluxes from deadwood decomposition."
Moreover, we added a detailed discussion to the new Supplementary Information Section 2.

5. You make clear in your rebuttal letter that the sampling design is unusual. That is, you harvested a full cage (or uncaged plot) each year, rather than sampling a log from each cage/ plot in the experimental array. I understand the somewhat unusual sampling strategy here but you do not tell the general reader why. It does feel out-of-sorts to do it the way you did, but thinking about this logically I am not convinced that it would have changed any of your findings because sites were so far from one another, and cages/plots so near to one another within each site, that I do not think I could argue that your replication is flawed because of the way you sampled. But the reader will wonder why you did it the way you did, so I suggest adding the explanation from your rebuttal letter to the text of the script (i.e. you were not convinced the cages would all survive 3 years).
Thank you for this comment. As suggested, we added the explanation to the Methods (P. 23, L. 456): *"That is, all logs from one uncaged, one closed cage and one open cage treatment were collected per site at the same time. We chose this approach because the maximum distance between installations was 6 m and thus within-site variation was expected to be rather low. Moreover, we wanted to ensure that the same number of logs could be sampled per treatment and year and failure of cages over time would have resulted in an unbalanced number of logs per treatment."*

6. Lastly, I remain deeply concerned by the inaccurate wording of your summary and main results/ discussion text. For example, lines 36-37 give an estimate of insect contributions to global deadwood C fluxes. But, as you make clear in your rebuttal letter, these are actually additional contributions over-and-above what would be observed if insects were not present. As such, you're giving an estimate of the additionality of insect effects. I think you need to say in the Summary that you are reporting additionality, so that such a high-profile paper does not mislead the field. For example, if termites consume 20 percent of the wood when present and, when absent, fungi compensate for their loss by consuming 18 of this 20 percent of wood that termites would otherwise have consumed, then the additional insect effect is 2% wood decomposed. That's what you're estimating: the additionality. And not insect contributions per se to dead wood decomposition. But the average reader, without detailed knowledge of animal effects on wood decomposition, is going to be misled. Maybe you think this is not relevant for global C fluxes, but I would offer a different opinion - presumably fungal vs. termite biomass has a very different fate in ecosystems which will influence C flow through foodwebs and potentially C persistence. As such, I do not think it is fine to bury this additionality in the main text, in a manner that will be opaque to the general reader.
We fully agree with your critique. Please refer to our response to comment 0b, above. We changed the terminology throughout the paper to "net insect effects" and define it upfront in the Introduction.

Referee #3 (Remarks to the Author):

General Comments:
The manuscript by Seibold et al. describes a unique and very interesting study of impacts of insects on decomposition rates comprised of two parts: a three-year field experiment conducted at 55 sites on 6 continents and a model-based analysis of carbon released from global dead wood pools. The first part of the study is very well executed. The authors have addressed the comments of the two reviewers and documented their responses and improvements to the manuscript. As a new reviewer, I am satisfied with these revisions and I have only minor additional concerns with the field study that I think will be easily addressed.

1. I therefore fully support publication of this very important component of the study. Unfortunately, the second part of the study has some serious flaws that will be harder to address and which were not identified by the previous review.
Thank you for this comment. We changed the weighting of the two parts of our study and now focus more on the experimental part, for example by removing the maps of deadwood carbon release (formerly Fig. 1 b and c) from the main text. Several results presented in the older version of the manuscript based on the upscaling approach can be shown already based on the experimental data and so we have now changed this accordingly. However, we are convinced that our decomposition function derived from the experiment is robust enough to calculate a first estimate of global carbon release from deadwood, especially since recent studies using much simpler deadwood models (e.g. Harris et al. 2021 Nature Climate Change) highlight that such estimates are urgently needed. Nevertheless, it is very important to present uncertainties related to these estimates and we do so by presenting an extended validation of our decomposition function against independent empirical data and by putting more emphasize on our sensitivity analysis. Furthermore, we use the sensitivity analysis to derive recommendations on how global estimates of carbon release from deadwood can be further improved to guide future research.

2. My concern relates to the extrapolation of results from a 3 year field study of decomposition rates of dead wood to global estimates of carbon release from dead wood pools used to estimate the contribution of insects to these decay releases. Wood decomposition models typically do not represent time since death for deadwood pools (i.e. the pool has no age-structure) because the computational overhead to represent decaying wood cohorts with varying time since death is very large. It is therefore common practice to use an approximation (with a deadwood pool that does not track time since death). This approximation works reasonably well, despite recognised shortcomings. The problem with this approach is that while decay of deadwood pools over time can be approximated with an exponential decay function (as most models do), the reality is that the initial decay of wood is much slower than predicted by the exponential model, as colonization of the newly dead wood by fungy, insects and mircobes requires time. The exponential decay function therefore overestimates intial decay and underestimates decay of wood that is in advanced stages of decay. Because deadwood pools contain material with a wide range of time since death the model approximation of the decay for the entire pool is a reasonable approximation. In this study, the authors used the observed rates of mass loss in the first one to three years of the time since death as affected by insects to derive an estimate of the overall C loss from deadwood pools. In cool (temperate) and cold (boreal) climates mass loss in the first three years since death will be very low (or even negative as observed by the authors in some boreal samples). The three year estimate of decay rates is not an indicator of the mass loss rate over the decay period and therefore, their estimate of rate of loss based only

on one to three years of observation cannot be extrapolated to the entire global deadwood pool. The authors claim that their estimates of decay are in agreement with the literature, but the only study they cite to confirm this is Chambers et al (2000) (#7 in the Supplement). However, that study was conducted in the tropics with high rates of decay even in the first three years and I would therefore expect good agreement between their method and the literature for tropical regions. But in temperate, and even more so in boreal regions, initial wood decay rates (over the three years of observation) will be much slower – or in some cases even negative. The decay rate over the first three years is in no way representative of the half live or average decay rate for the pool and it is therefore incorrect to use observed mass loss over 3 years to estimate C releases from global deadwood pools.

Thank you for this important comment. We completely understand this concern. Temporal patterns of decomposition rates are indeed heterogeneous depending on tree species and climate. No change in the k-value over time were reported for example by Chambers et al. (2000) for a mix of tree species in Amazonia and by Müller-Using & Bartsch (2009) for *Fagus sylvatica* in Germany. In contrast, other studies found changes over time, e.g. lower k-value during the first period of decomposition of *Pinus contorta* in Oregon (Busse, 1994). Similarly, studies comparing temporal patterns of relative mass loss between tree species in Central Europe reported exponential patterns for some tree species and linear or sigmoid patterns for other species (Freschet et al., 2012 Edelmann, Seibold, Gossner, Weisser & the BELongDead Consortium, in prep.).

We initially shared this concern, about whether our decomposition function can be used for global upscaling, and thus compared predicted decomposition rates based on our experiment against independent empirical data from survey studies compiled by Chambers et al. (2000) which encompassed deadwood pools with a wide range of time since tree death. We apologize for not being clear enough in the paper that we did not use the data from Amazonia from Chambers et al. (2000), but the meta-analysis published in the same paper which covers the full gradient from boreal to tropical forests. However, during the revision process, a new global compilation of survey data was published (Harmon et al., 2020) which is the to our knowledge most comprehensive dataset to date including all major climate zones, a wide range of time since tree death (<1 year to >300 years), tree species and diameters. We took the opportunity and predicted decomposition rates for the 157 data points from Harmon et al. (2020) based on the climatic conditions at each site and the host tree division (angiosperm or gymnosperm). We then compared predicted decomposition rates against the originally reported ones as well as the relationships between decomposition rates and temperature. Comparing observed to predicted decomposition rates showed that the prediction was unbiased (Supplementary Figure S2-1). Moreover, the relationship between decomposition rates and temperature matched very well (Extended Data Fig. 5). This indicates that our 3-year experiment with small-diameter deadwood provides robust estimates of decomposition rates relative to global climate. We assume that this good match between our experimentally derived function and survey data, despite various examples of changing decomposition rates over time (see above), might be caused by contrasting patterns of different tree species which cancel out when modelled at the scale of mixed-species deadwood pools.

Furthermore, it should be taken into account that for decomposition of branches a period of 3 years is not only early decay. Branches decompose more quickly than large-diameter deadwood (Supplementary Table S2-1) which was one of the main reasons why we used branches for our experiment. By doing so, we could cover a larger part of the decomposition process than by using large-diameter deadwood. This is underlined by the number of branches that had been decomposed almost completely within the 3-

yearperiod of our experiment. Of course, this occurred mainly in the tropics where decomposition rates were highest. However, experimental and survey data from branch decomposition in temperate regions show that residence times of branches are usually less than 10 years under such conditions (Bässler et al., unpubl. data; Müller-Using & Bartsch, 2009; Angst et al., 2018). Decomposition of branches by fungi (measured as ergosterol content) increases quickly over the first two years, peaks between year 3 and 5 and then starts to decrease even under rather cool temperate conditions (Bässler et al., unpubl. data; Angst et al., 2018). This leads to fast destabilization and fragmentation of branches, as shown by an experiment from Central European mountain forest where 87% of 419 gymnosperm and angiosperm branches had fallen apart after 8 years (Bässler et al., unpubl. data). This is also well documented in studies investigating the speed of decomposition in relation to diameter (Supplementary Table S2-1). Considering residence times of large-diameter deadwood of roughly 30-50 years in the same climate zone (Harmon et al., 2020), our three-year experiment with branches should be comparable to a 10-15 year study with large-diameter deadwood.

Taking this into account, we consider that our experiment is covering a large and important part of the decomposition process. We are therefore confident that our upscaling approach is robust enough to provide a first estimate of global deadwood carbon release which is urgently needed (see Harris et al. 2021).
To address this issue, we placed the external validation including a reference to Extended Data Fig. 5 more prominently in the main text and refer also to the issue of time since tree death (P. 8, L. 186): *"Since our experiment focused on decomposition of small-diameter deadwood over three years, we adjusted decomposition rates to account for slower mass loss of large-diameter deadwood (for details see Methods and Supplementary Information section 2). We evaluated our relationship between decomposition rate and local climate against 157 independent empirical observations from previous deadwood surveys[34], spanning the full range of deadwood diameters > 7 cm, time since tree death and climatic conditions. We obtained a good match of the results from our model to these independent data (Extended Data Fig. 5), suggesting our approach is robust for deriving a first quantitative estimate of global carbon fluxes from deadwood decomposition."*
We have added a detailed discussion of this key topic and associated analyses to the new Supplementary Information section 2.

3. A second issue is that while the authors did recognise and adjust for the contribution of piece size (adjusting their observed decay rates for fine wood to an estimate for all deadwood), they failed to recognise or adjust their estimates for the very large difference in decay rate between standing and downed dead wood. Standing dead wood decomposes much more slowly than downed wood. For example see Fig 1 in Hararuk et al. 2020 (https://doi.org/10.1186/s40663-020-00248-x) which indicates nearly a factor 2 shorter half-life for downed wood compared to standing dead wood.
Thank you for this important comment. Reviewer #2 raised a similar point. Please see our comment to comment 1 of reviewer #2. In short, we included now an estimate of share of standing deadwood for the different biomes and a conversion factor derived from global data from Harmon et al. (2011) to account for lower decomposition rates of standing deadwood in temperate and boreal forests in our revised global upscaling approach and included uncertainty regarding this conversion factor and the share of standing deadwood biomass to the sensitivity analysis. The factor derived from Harmon et al. (2011) was very similar to the one reported in Hararuk et al. (2020).

4. It would also be helpful to the reader to provide a clear definition of deadwood – ideally consistent with that of the IPCC's GHG estimation guidelines. It is important for the reader to understand that the estimate of global deadwood pools includes standing dead trees (which is good) but the estimate of decay rates in this study was derived only from downed wood samples (3 cm in diameter), and that is problematic.

We added the definition from (Pan et al., 2011) which is consistent with the definition of the FRA report and the IPCC (P. 26, L. 561): "*The values reported in Pan et al.[1] defined deadwood as "all non-living woody biomass not contained in the litter, either standing, lying on the ground, or in the soil" with a diameter >10 cm."*

For our response regarding standing deadwood, please see comment 1 of reviewer #2. Note that the same line of argument as for the potential effect of time since tree death (comment 2 of reviewer #3) holds also for the potential effect of wood diameter. As discussed in Supplementary Information section 2, FWD (fine woody debris) has higher decomposition rates than CWD (coarse woody debris). We therefore derived conversion factors for CWD decomposition from the literature and applied the modified function to the CWD fraction of deadwood pools in our global upscaling. In the external validation of our prediction against empirical data from Harmon et al. (2020), we used these conversion factors. The resulting match of the relationships (Extended Data Fig. 5) indicated that our decomposition function provided a robust prediction of decomposition rates of CWD. As mentioned above, we placed the external validation (including a reference to Extended data Fig. 5) more prominently in the main text of the revised manuscript and now refer to it upfront with the potential sources of bias and the robustness of our function.

5. The concluding statement at the end of the Supplementary Material, that future studies should also investigate large diameter wood is correct, but I suggest that it is even more important that future studies (in boreal regions) should be conducted over longer periods (or include some wood samples that initially are in advanced stages of decay).

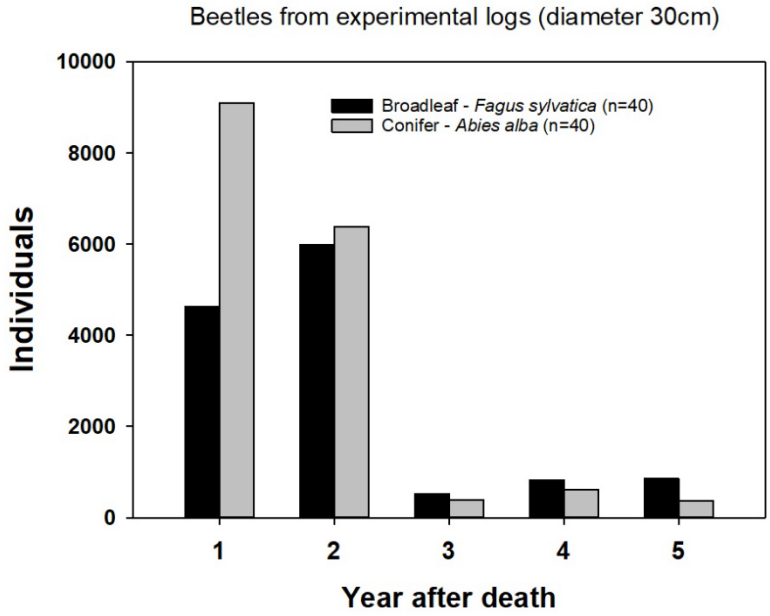
We have added a paragraph to the extensively revised manuscript on the discussion of effects of time since tree death to the Supplementary Information Section 1 and added the suggested statement.

6. In summary, this novel and unique global experiment demonstrates that insects do affect rates of decomposition in the tropics. However, I suggest that three years was simply not long enough to draw any conclusions of the impacts of insects in the boreal. The authors do show that the impact is small in the first three years, but I would expect this impact to increase (somewhat) with increasing time since death of the deadwood in the boreal (and likely in the temperate) regions. This should be clearly stated in the manuscript.

Successional patterns of insect communities are quite well studied in temperate and boreal forests. For coniferous wood, which constitute the main part of the deadwood in the boreal zone, these studies show that the abundance and diversity of saproxylic beetles is highest during the first decay stage (1-2 years) with these measures then decreasing over time (Saint-Germain et al., 2007a; Ulyshen & Hanula, 2010; Stokland et al., 2012; Lee et al., 2014). In angiosperm wood, abundance and diversity trends over time are more variable with later peaks in some tree species (Saint-Germain et al., 2007b, 2007a) and declines over time in others (Grove & Forster, 2011). Late-successional communities consist mainly of mycetophagous and zoophagous species (Grove & Forster, 2011) which do not decompose wood directly. As an example, insect emergence data from a five-year experiment with large-diameter logs of *Fagus sylvatica* and *Abies alba* in Central-European mountain forests found more than a 10-times higher abundance in the first two

years compared to later years (Response Figure 2). Xylophagous species, particularly some of the most potent decomposers, such as cerambycids (Ulyshen, 2018), peak in abundance at the earliest stages of decomposition (Vanderwel et al., 2006; Saint-Germain et al., 2007a; Ulyshen & Hanula, 2010; Lee et al., 2014). Of course, later-successional species also contribute to wood decomposition, such as for example lucanids, but these are usually less frequent than cerambycids (Ulyshen & Hanula, 2010).

Response Figure 2: Abundance of saproxylic beetles recorded from 40 logs of *Fagus sylvatica* and 40 logs of *Abies alba* with 30 cm in diameter recorded one to five years after tree death by stem emergence traps (position shifted each year) in an experiment in mountain forests in Germany (Seibold et al., 2016).



Moreover, as stressed under comment 2, a three year period is not a minor temporal window in the decomposition process of fine woody debris (not for fungi and certainly not for insects). It seems more comparable to studies of 10-15 years for large-diameter deadwood. Therefore, it is very unlikely that we missed important effects of insects in our approach.

We consider that our experiment is not strongly underestimating the net insect effect in boreal forests, although it covered only the first three years of decomposition. This is because major wood-decomposing insect guilds peak very early and then decline over time in coniferous wood (Saint-Germain et al., 2007a; Ulyshen & Hanula, 2010; Stokland et al., 2012; Lee et al., 2014), conifers dominate boreal forests, and that FWD decomposes faster than CWD (Supplementary Material Table S2-1). In temperate forests, where broadleaved tree species are more abundant, it may be more important to also cover later decay stages to capture the full effect of insects. However, decomposition rates are higher in temperate compared to boreal forests and thus our three year experiment covered a larger part of the full decay process in temperate than boreal forest (e.g. residence time of FWD of *Fagus sylvatica* is ~10 years (Müller-Using & Bartsch, 2009)). By contrast, if insects affect wood decomposition in temperate and boreal forests mainly during the first years of decomposition, our study may overestimate rather than underestimate the net effect of insects in temperate and boreal forests when applying our decomposition to global deadwood pools including the full range of decay stages. However, considering the

overall small net insect effect in temperate and boreal forests, this potential bias may be negligible.
In the tropical zone, decomposition in general is much faster than in the temperate zone (Chambers et al., 2000), a result which also was found in our study (with some logs at tropical experimental sites fully decomposed after the first year). Therefore, our data is highly likely to have well captured the net insect effects in the tropics.
Overall, we fully agree that it would be helpful to conduct longer time-series studies and experiments in temperate and boreal forests, but there is a trade-off in the duration of the study and the spatial coverage. More localized studies are needed to cover the long-term decomposition in temperate and boreal sites. Here, we aimed for global coverage, which necessarily limited the possible duration of our experiment. Nevertheless, we believe that our quantification of insect effects is covering the most important stages of the decomposition process in deadwood diameters as used in our approach. We have added some further text on this topic to the new Supplementary Information section 2, where we also discuss the general role of time since tree death.

7. This is an important study, despite the flawed analysis of the global release of C from deadwood pools. I understand the desire to publish in a high-impact journal such as Nature and, therefore, the need to present results and conclusions that are of global significance. The experimental part of the study is excellent and should be published, but further analyses of the global estimates of C release from deadwood pools will be required before the entire study can be accepted. These estimates must not be based on only the mass loss rates observed over the initial three years after death. I realise that with revised estimates of mass loss that are based on other data sources, the impacts of insects will no longer be quantified. However, failure to correct this prior to publication will results in proliferation of a seriously flawed estimate of C releases from global deadwood pools that greatly underestimates the release in boreal regions, and to a lesser extent also in temperate regions.
Thank you for acknowledging the value of the experimental part of our study. We have followed the suggestions of the referee and now focus much more on it, while down-weighting the upscaling approach. We, for example, removed the maps of deadwood carbon release (formerly Fig. 1 b and c) from the main text. We did not remove the upscaling part completely for four reasons as explained in the letter to the editor, in short:

(1) Better functions and estimates of deadwood decomposition are urgently needed considering that high profile papers use much simpler deadwood carbon models than contained in our paper. The publication of our function and global estimates is a first step to close this gap.
(2) The global estimated value depends primarily on forest area, the proportion of deadwood and the function of decomposition in relation to climate. Quite good data on forest area and growing stock exist, but only coarse data are available for deadwood stocks. However, we do not expect to be able to gather much better information in near future due to the challenges in measuring deadwood in managed and unmanaged forests globally. We believe our decomposition function is sufficiently robust as indicated by validation against global survey data, to be presented at the current stage.
(3) Based on your critique and on new global information on carbon fractions in living and dead trees in a recent papers, we have refined our global estimate as well as strengthened the sensitivity analysis.
(4) We now emphasize our sensitivity analyses more strongly and are more transparent about uncertainties associated with different assumptions in the upscaling process.

Based on this and the overall results, we derive recommendations on how global deadwood carbon models could be improved in the future.

Including published data in our global upscaling approach is challenging from a statistical perspective due to the high heterogeneity of data and the fact that often only aggregated but not raw data are available. In addition to these general challenges of meta-analyses, we refrained from employing this approach because it would not allow to address one of the main goals of our study which was to quantify the role of insects in deadwood decomposition.

Specific Comments:

8. L32: I suggest rewording this from "we estimate that 11.7 Pg of carbon are lost from deadwood every year." To "we estimate that 11.7 Pg of carbon are released from deadwood every year." The term lost could be misinterpreted to imply that the deadwood C pool decreases by that amount – it does not, because new deadwood is added continuously. The term "loss" is used in other places (e.g. Fig 1 legend and caption). I suggest that the authors consider changing this from 'loss' to 'release' throughout. L98 explains the intended use of the term (which is helpful) but I would still consider using the term carbon release rather than carbon loss.
We changed the wording to "release".

9. L37: Suggest revising "Insects accelerate decomposition in tropical forests but have neutral or decelerating effects in temperate and boreal forests" to "Insects accelerate decomposition in tropical forests but have neutral or decelerating effects in temperate and boreal forests in the first three years of wood decay".
Please see our response to the next comment (number 10).

10. L40: It is hardly new or a surprise that "global warming could accelerate deadwood decomposition" – in fact, given the sophistication of the experiment and the richness of data, I am surprised that this is the key message highlighted here. There is so much valuable information in this study that I wish the authors had come up with a conclusion that emphasises their new findings rather than merely reinforcing temperature dependence of wood decay, which is at the core of even the most basic models of deadwood decomposition (and has been for 30 years or more). Moreover, acceleration of deadwood decomposition says nothing about net impacts on the C balance because global warming is likely to affect many other processes, including the productivity of forests, and the rate of disturbances (including insect-mediated tree mortality) – both of which can increase input of biomass to deadwood pools.
Thank you for this comment. We have rephrased the conclusion section of the Abstract to (P. 2, L. 37): *"Our results highlight the functional role of insects in deadwood decomposition and the global carbon cycle, and suggest that global warming could accelerate deadwood decomposition where precipitation is sufficient."*

We fully agree that increased decomposition rates do not necessarily mean higher carbon release, depending on changes in stocks and other fluxes. However, our knowledge about tree mortality is considerably more advanced than about wood decomposition and thus, our study aims to close this critical knowledge gap. By implementing our decomposition function into larger forest carbon models which include tree growth and mortality, the carbon balance of forests can be better estimated. We now point to this at the end of the Conclusion section (P. 10, L. 239): *"Our quantification of global carbon release from deadwood highlights that deadwood and wood-decomposing insects play an important role in the global carbon cycle. To robustly project the future of the forest carbon sink[28,29],*

dynamic global vegetation models need to account for changes in deadwood pools and fluxes more explicitly. To this end, both the processes of deadwood production (e.g., via natural disturbances[47]) and deadwood decomposition require increased attention."

11. L100: "synthesized from empirical and remote-sensing data" – emphasize here already that this was done by the authors (as becomes apparent when reading the methods).
We clarified this by saying (P. 5, L. 104): "*To provide a first estimate of the global carbon flux from deadwood decomposition (henceforth referred to as deadwood carbon release for brevity) and to quantify the functional importance of insects for global deadwood carbon, we applied the model derived from our decomposition experiment to a novel global deadwood carbon map (Fig. 1a), which we synthesized from empirical and remote-sensing data.*"

12. L113: "Increasing precipitation affected decomposition rates negatively at low temperatures but positively at high temperatures." Can the authors please modify this statement to read: "Increasing precipitation affected initial decomposition rates negatively at low temperatures but positively at high temperatures." Their study was limited to three years – which in boreal environments is really too short for the observation of mass loss in woody materials. I caution against generalisation of this finding. It points in fact at a different (well known) problem – namely that models that use a simple decay function to represent dead wood decomposition C releases tend to overestimate the decay in early years in cold climates.
Please see our response to comment 4 of reviewer 2 and comment 2 of reviewer 3. We agree that potential effects of time since tree death need to be discussed - which we now do in much more detail in Supplementary Information section 2. We are now more transparent in indicating that our results are based on 3 years of data and hence it is not necessary to add "initial" here. Furthermore, our validation (Extended Data Fig. 5) with external data ranging from <1 year to more than 300 years (Harmon et al., 2020), supports the view that such a restriction is not justified. See also on the comments above (comment 2 and 6) to the discussion point if 3 years in fine woody debris is short, medium or long in terms of the decay process from the perspective of insects and fungi.

13. L123: "The strength of this increase is modulated by current and future levels of precipitation" – this is only partly correct – the main driver is not precipitation but site water balance, which is determined by P and evapotranspiration. Hence, even where P increases, the site may become drier if T increases relatively more. A mention that it is not P directly but its impact (together with changes in T and site and soil characteristics) that will affect decomposition rates may be useful – even if this study only looked at P and not water balance. The wildfire community has sophisticated models to calculate the daily moisture content of dead wood. To address this, the authors could simply state "The strength of this increase is modulated by current and future levels of precipitation and precipitation impacts on site water balance". See Smyth et al 2011 doi.org/10.1016/j.ecolmodel.2010.12.005 as an example of an approach to represent water stress impacts on decomposition rates.
We have now changed the sentence as suggested and added the reference.

14. L193+: The statement about "high intensity management in the majority of these forests" is wrong. By far, the majority of the boreal forest area is located in Alaska, Canada, and Russia – and these are not intensively managed – huge areas are not managed at all. Yes, boreal forests in Finland, Norway and Sweden are intensively managed and the statement is correct for those systems – but it is wrong for the vast majority of the world's boreal forests.
We have rephrased this accordingly to "high-intensity management in parts of these forests".

15. L199+: “The contribution of insects to wood decomposition is slightly negative in some regions with cool temperatures and high precipitation, such as the Pacific Northwest of North America.” I am concerned about the validity of this statement – all the authors have observed in their experiments is the contribution of insects in the first 3 years of decomposition (on average only for 2 years). I suggest that the impact will increase in time as decomposition progresses, and wood becomes softer and therefore more accessible to insects. The authors should highlight the temporal limitation of this study and add a caveat that longer-term impacts of insects outside the tropics could be underestimated.
Please see our response to comment 6 of reviewer 3. The respective sentences were deleted to substantially reduce the focus away from the upscaling approach (and concentrate on the global experimental findings).

16. L206: “were the most important source of uncertainty” – only of those sources of uncertainty that were assessed.
We rephrased this to (P. 9, L. 219): *“Of the various sources of uncertainty that were considered, the underlying data on deadwood carbon stocks contributed most strongly to overall uncertainty (Fig. 3; Extended Data Table 2; Supplementary Information section 2).”*

17. L219: “their effect is currently small in temperate and boreal biomes, where they can even slow decomposition.” – as stated above – this conclusion if in part an artefact of the duration of this study. I suggest that the authors rephrase this as – “ … where they can even slow decomposition in the first three years” or some other way to indicate the time constraints of this analysis.
We have rephrased the Conclusion section here and removed the respective sentence.

18. L225: If the authors make a general and sweeping comment about how global carbon models need to be improved, they should add a statement about the impacts of disturbances – in particular wildfire – which are not or poorly represented in many global models. Fires, in particular, can contribute to the rate of C input to dead wood pools, and they can accelerate release of C from deadwood pools.
We have rephrased the respective sentence to (P. 10, L. 241): *“To robustly project the future of the forest carbon sink[28,29], dynamic global vegetation models need to account for changes in deadwood pools and fluxes more explicitly. To this end, both the processes of deadwood production (e.g., via natural disturbances[47]) and deadwood decomposition require increased attention.”*

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Reviewer Reports on the Second Revision:

Ref # 1

N/A

Ref #2

There are unquestionable challenges associated with the interpretation of the experimental data and in the extrapolation. However, I strongly appreciate the substantial amount of work the authors have done to address these challenges (including in giving full responses through multiple rounds of review). And I applaud the ambitious experimental design and undertaking, not to mention analysis. Overall, I am convinced this will be an influential paper that jumps the field forward in terms of our understanding of carbon fluxes from decomposing dead wood, and the role that animals play in shaping these fluxes. Thank you for entertaining so many questions and your diligence in responding to them. While we may differ on some specifics of the analysis and interpretation, I accept that how you have dealt with the caveats is thorough and defensible.

Ref #3

Review Comments
The revised manuscript has greatly improved. The addition of the comparison of the estimates of wood decay rates against a recently published global data set has increased the credibility of the study (but see detailed comments below about the claim of the lack of bias in their model). The responses to reviewers are detailed and document previously hidden assumptions that now make it easier to understand the manuscript.
The study is comprised of two main components – an experiment of insect exclusion to determine the contribution of insects to wood decay. Overall, that component of the study is well designed, analysed and reported. The second part is an extrapolation of decay rates, applied to global estimates of dead wood C stores, to arrive at a global estimate of C releases from dead wood pools. That part of the study still has some weaknesses: foremost as the authors have shown, a three-year observation period is simply insufficient for a wood decay study in the boreal. Consequently the authors obtained inconclusive results – in fact, they did observe increases in C stocks after three years in some sites. Although the authors continue to argue that 3-years of decay observation of 3 cm diameter branches is sufficient to extrapolate their estimates to the entire boreal forest, I am simply not convinced that this is a justifiable extrapolation. If nothing else, the authors should report the parameters used in their decay function so that this can be compared to other studies.
The authors are appropriately cautious with the interpretation of their results – while I still think that their estimates of C release from deadwood in temperate and boreal forests are substantially too low, the authors do not make any exaggerated claims about the importance of their estimates. The authors could further strengthen the paper by providing a comparison of their estimate (10.9 Pg C) to other global estimates, e.g. from global forest dynamics models.
I recommend considering acceptance of this manuscript after addressing the suggested revisions that may improve the manuscript.

Detailed comments:
Supplementary data Figure S2-1:
The authors state that: The distribution of values over both sides of the 1:1 line indicates that our model is not systematically under- or overestimating decomposition rates.

Sorry – but with no amount of good will can I agree with this statement of the authors about the lack of bias. Sure, a large number of data points at low decay rates is located on either side of the 1:1 line, but the four observed decay rate values greater than 0.2 are underpredicted by a factor of 2 to 3. Clearly, for the data you provided your model is substantially underpredicting decomposition rates if the observed k > 0.2.
Given the temperature dependence of decay rates, k values over 0.2 are expected in tropical and subtropical regions – where your maps show by far the largest amounts of dead wood. Thus, your global estimate of decomposition C emissions is likely to be underestimated.

L 195: "We estimate that 10.9 ± 3.2 Pg carbon is released from deadwood per year globally."
It would be really helpful to the reader if you added a small table to summarize in one place your results by biome (Fluxes are shown in Figure 3a) and added both the deadwood pool size and the annual released amounts of carbon, both in absolute terms and per hectare. This would make it much easier to compare your results to other global and regional studies. Moreover, can you also please calculate and report the turnover time for dead wood carbon in each biome in that additional table? Such data would make it easier for the reader to assess your estimates (e.g. your global residence time for dead wood carbon is only 6.7 years (73/10.9) but what are your estimates for temperate and boreal dead wood systems?

L 208: "Another reason for lower carbon fluxes from deadwood decomposition in temperate and boreal forests is the presence of high-intensity management in parts of these forests, resulting in a reduced amount of deadwood being retained on site."
In my first review comments I challenged your claim of the impacts of intensive management on boreal dead wood pools. I read your response to reviewers but you have not convinced me. The „parts of these boreal forests" that are experiencing high intensity management is a single digit percentage of the total area (the total forest area of Sweden, Finland and Norway combined is less than 5% of the combined area of forests in Russia, Canada and AK). And not all foreststs in Fenno-Scandia are intensively managed. I suggest that the contribution of intensive management on 5% of the boreal forest area is not the reason for lower carbon fluxes. Instead, I suggest that you are missing another, far more important process! In most boreal forests a significant proportion of the C releases from dead wood pools is via fire and not via harvest. In particular because fire is a process that affects a far larger proportion of the boreal forest than intensive management. Please consider replacing your statement about "high intensity management" with a reference to fire, or at least refer to both processes.

Extended data Figure 6c – the X-axis label should read Mg ha-1a-1 – currently missing per year.

Author Rebuttals to Second Revision:

Point-by-point response to the referees´ comments to Seibold et al. "The contribution of insects to global forest deadwood decomposition".

Referee #2 (Remarks to the Author):

There are unquestionable challenges associated with the interpretation of the experimental data and in the extrapolation. However, I strongly appreciate the substantial amount of work the authors have done to address these challenges (including in giving full responses through multiple rounds of review). And I applaud the ambitious experimental design and undertaking, not to mention analysis. Overall, I am convinced this will be an influential paper that jumps the field forward in terms of our understanding of carbon fluxes from decomposing dead wood, and the role that animals play in shaping these fluxes. Thank you for entertaining so many questions and your diligence in responding to them. While we may differ on some specifics of the analysis and interpretation, I accept that how you have dealt with the caveats is thorough and defensible.

We are very thankful for this positive evaluation and the constructive comments which helped to improve the manuscript.

Referee #3 (Remarks to the Author):

The revised manuscript has greatly improved. The addition of the comparison of the estimates of wood decay rates against a recently published global data set has increased the credibility of the study (but see detailed comments below about the claim of the lack of bias in their model). The responses to reviewers are detailed and document previously hidden assumptions that now make it easier to understand the manuscript.

The study is comprised of two main components – an experiment of insect exclusion to determine the contribution of insects to wood decay. Overall, that component of the study is well designed, analysed and reported. The second part is an extrapolation of decay rates, applied to global estimates of dead wood C stores, to arrive at a global estimate of C releases from dead wood pools. That part of the study still has some weaknesses: foremost as the authors have shown, a three-year observation period is simply insufficient for a wood decay study in the boreal. Consequently the authors obtained inconclusive results – in fact, they did observe increases in C stocks after three years in some sites. Although the authors continue to argue that 3-years of decay observation of 3 cm diameter branches is sufficient to extrapolate their estimates to the entire boreal forest, I am simply not convinced that this is a justifiable extrapolation. If nothing else, the authors should report the parameters used in their decay function so that this can be compared to other studies.
Three years of decomposition may appear short for deadwood on a first glance, but as outlined in detail during the last round of revisions referring to literature and data, three years should not be considered short term for fine woody debris as used in our study, particularly in temperate and boreal forests. To correct for differences in decomposition rates between fine and coarse wood debris, we applied a conversion factor. Our validation of the thereby derived decomposition function compared against data from Harmon et al. (2020) shows an excellent match of the functions along the full temperature gradients. Therefore, and since the reviewer is not presenting evidence for his/her statement that "this kind of extrapolation is not justifiable", we remain confident that our approach is indeed justifiable. Nevertheless, different points of view are an important part of scientific debates and thus we accept that we seem not being able to convince the reviewer in every detail. The coefficients of our model are presented in table 1.

The authors are appropriately cautious with the interpretation of their results – while I still think that their estimates of C release from deadwood in temperate

and boreal forests are substantially too low, the authors do not make any exaggerated claims about the importance of their estimates.
Thank you. We were cautious not to overstate the results from the upscaling part of our study.

The authors could further strengthen the paper by providing a comparison of their estimate (10.9 Pg C) to other global estimates, e.g. from global forest dynamics models.
We agree that a comparison to other estimates, e.g. from simulation studies, would be interesting. However, to the best of our collective knowledge, a reliable estimate for the C flux from deadwood from dynamic global vegetation model (DGVM) simulations does not exist. First of all, only a small number of DGVMs consider the population dynamics of trees explicitly, and hence are even in a position to simulate reliable deadwood stocks. See for instance Fisher et al. (2018) for a synthesis of the state-of-the-art. Those models that do consider demography explicitly, such as LPJ-GUESS (which simulates structural information in a comparable manner to forest gap models), have until now used a generic background mortality rate per plant functional type (Bugmann et al., 2019), resulting in an unrealistic input to the deadwood pool (e.g., because disturbances are neglected) and hence an unreliable estimate of global deadwood. More recent work has constrained mortality using satellite data (Pugh et al., 2019), yet no dedicated analyses on deadwood have been performed (and the underlying decomposition functions remain simplistic). Hence, we are not able to make the comparison as suggested by the Reviewer.

I recommend considering acceptance of this manuscript after addressing the suggested revisions that may improve the manuscript.

Detailed comments:
Supplementary data Figure S2-1: The authors state that: The distribution of values over both sides of the 1:1 line indicates that our model is not systematically under- or overestimating decomposition rates. Sorry – but with no amount of good will can I agree with this statement of the authors about the lack of bias. Sure, a large number of data points at low decay rates is located on either side of the 1:1 line, but the four observed decay rate values greater than 0.2 are underpredicted by a factor of 2 to 3. Clearly, for the data you provided your model is substantially underpredicting decomposition rates if the observed $k > 0.2$. Given the temperature dependence of decay rates, k values over 0.2 are expected in tropical and subtropical regions – where your maps show by far the largest amounts of

dead wood. Thus, your global estimate of decomposition C emissions is likely to be underestimated.

The figure shows that 3-4 out of 157 comparisons diverge quite strongly, but it also shows that the rest of the data points are distributed rather equally around the 1:1 line. Differences between single predictions and observed decay rates depend strongly on tree species-specific effects, since our prediction is not at the scale of single tree species but for gymnosperms vs. angiosperms. Two of the mentioned diverging predictions are for tree species with very low decay resistance (i.e. *Bursera simaruba* and *Liriodendron tulipifera*). This underscores our argument that there is no systematic bias, despite the expected individual deviations. Moreover, matching single values is less important than the match of the decomposition-temperature relationship shown in Extended Data Fig. 5 (which is a very close relationship) . This climate-dependent decomposition function is used in our approach to estimate decomposition rates for gymnosperm and angiosperm deadwood, not for single tree species.

L 195: "We estimate that 10.9 ± 3.2 Pg carbon is released from deadwood per year globally." It would be really helpful to the reader if you added a small table to summarize in one place your results by biome (Fluxes are shown in Figure 3a) and added both the deadwood pool size and the annual released amounts of carbon, both in absolute terms and per hectare. This would make it much easier to compare your results to other global and regional studies. Moreover, can you also please calculate and report the turnover time for dead wood carbon in each biome in that additional table? Such data would make it easier for the reader to assess your estimates (e.g. your global residence time for dead wood carbon is only 6.7 years (73/10.9) but what are your estimates for temperate and boreal dead wood systems?

We agree that additional results at a biome level could help readers to assess our estimates. We therefore amended Extended Data Table 3 with a table reporting the annual deadwood carbon release, net effect of insects, and deadwood carbon residence time per biome (in Pg). The low residence times globally are driven by the tropical biome; residence times in temperate and boreal forests are higher (33, and 40, respectively). We refrained from reporting values per hectare, as we do not have the information to reliably estimate the forested area per 5-minute cell. This information would be required to estimate meaningful and serious average values per hectare forest.

L 208: "Another reason for lower carbon fluxes from deadwood decomposition in temperate and boreal forests is the presence of high intensity management in parts of these forests, resulting in a reduced amount of deadwood being retained on site." In my first review comments I challenged your claim of the impacts of

intensive management on boreal dead wood pools. I read your response to reviewers but you have not convinced me. The „parts of these boreal forests“ that are experiencing high intensity management is a single digit percentage of the total area (the total forest area of Sweden, Finland and Norway combined is less than 5% of the combined area of forests in Russia, Canada and AK). And not all foreststs in Fenno-Scandia are intensively managed. I suggest that the contribution of intensive management on 5% of the boreal forest area is not the reason for lower carbon fluxes.

Following this suggestion, we removed the specific sentence regarding forest management effects.

Instead, I suggest that you are missing another, far more important process! In most boreal forests a significant proportion of the C releases from dead wood pools is via fire and not via harvest. In particular because fire is a process that affects a far larger proportion of the boreal forest than intensive management. Please consider replacing your statement about “high intensity management” with a reference to fire, or at least refer to both processes.

We agree that wildfire is an important process that was included only implicitly in our analysis (via the estimates of current deadwood stocks, which integrate data on disturbed and undisturbed ecosystems). However, fire was not explicitly considered in our analysis of C fluxes from deadwood, as none of our experimental sites was burned over the duration of the experiment. We have now added a sentence to the discussion section explicitly alerting the reader to this fact. We, however, also note that fires do not result in a complete loss of deadwood C. Average consumption rates for deadwood pieces of 3 cm diameter are 39%, 65% and 73% in the tropical, temperate, and boreal biome, respectively (Van Leeuwen et al., 2014). However, most deadwood C is stored in large diameter trees, which have considerably lower consumptions rates of 21%, 32%, and 38% in the tropical, temperate, and boreal biome, respectively (Van Leeuwen et al., 2014). Studies across different levels of fire severity furthermore found that consumption rates drop sharply for moderate and low severity fires compared to high severity fires (e.g. from 24% to 8% and 4% for large diameter downed woody debris in Campbell et al. (2007).

Extended data Figure 6c – the X-axis label should read Mg ha-1a-1 – currently missing per year.

This was corrected as suggested.

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Van Leeuwen, T.T., Van Der Werf, G.R., Hoffmann, A.A., et al. (2014) Biomass burning fuel consumption rates: A field measurement database. *Biogeosciences*, **11**, 7305–7329.

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