nature portfolio

Peer Review File

Droughts preceding tree mortality events have increased in duration and intensity, especially in dry biomes

Corresponding Author: Dr Antonio Gazol

This file contains all reviewer reports in order by version, followed by all author rebuttals in order by version.

Version 0:

Reviewer comments:

Reviewer #1

(Remarks to the Author)
This paper provides an extensive overview of tree/forest mortality reports in the literature and drought characteristics that cause tree mortality in forest types and biomes. This paper will be interesting for a large audience, and already the update on mortality events (building on previous papers of some of the authors) is extremely important. The paper's main findings are that in dry vs. wet biomes as wells as in conifer vs. deciduous tree-dominated forests, different characteristics (e.g., duration, intensity) of drought events are causing mortality. The authors also show that "warming-amplified" aridity (hot droughts) is getting more critical.
The methods applied are sound and well-justified, and it is mainly the extensive database the authors base their analysis on that is impressive. The results support the conclusions and allow for generalization (biomes, forest types), which is the central point made by the authors. It is a solid piece of science that might not provide "sexy novel insights", but the manuscript will be an essential basis for further analysis and inspire the scientific community to improve forest mortality monitoring on a global scale.
Given the solid methodological basis, the comprehensive data sets, the novel way to characterize drought features that affect particular biomes and forest types, and the general importance of the topic justify, in my opinion, the publication of this work as it is.

Arthur Gessler

Reviewer #2

(Remarks to the Author)
Review of Warming-induced longer droughts accelerate tree mortality globally, especially in more arid regions

This study uses data from a climate database and a published meta-analysis of mortality events (updated) to link patterns of estimates of drought and temperature stress to patterns of published accounts of forest mortality.

This is an important topic, and therefore faces a high bar in how it is executed and related. It is possible that a majority of the issues I have with the study emerge from challenges in explained what are generally proxies for mechanisms mapped against each other with fairly complex metrics and filters. This is a difficult area of study, so I understand these challenges, but again, the bar is high. The title implies that this study will link climate warming to droughts, which then accelerate tree mortality, globally. (Especially in more arid regions). This is a huge claim, with strong implications of causal links and important impacts, and therefore sets a high standard for review.

This study would be better served with a more humble title, and a more focused (and narrower) scope. What the study does best is explore how droughts might occur in some parts of the world over time. What it fails to do is clearly explain many of the decisions that go into these analyses--there is no sensitivity analysis as to what constitutes a ‘severe drought’ or a ‘mortality event’, and uncertainty is not discussed explicitly. Note: because of the sample sizes, a frequentist approach to confidence intervals does not serve as an uncertainty analysis, as these just refer to point estimate precision. Standard deviations of the data would be better (along with plots of the data, distributions, etc.

I'll focus on the larger themes that this study fails to meet and then share more specific comments.

Drought patterns

Because the methods of this paper have been developed to translate continuous time-series of climate to 'events', it might be helpful to have a conceptual diagram so that the reader clearly understands everything that this translation involves. Show the density of SPEI and how a 10% decile is appropriately drawn from it so that 'droughts' are then labeled. A discussion of why 10% should be the cutoff (and the data distribution should be included) and perhaps a sensitivity analysis would support this justification (re-run some of the analysis with 5%, 15% and 25% as a cutoff to show robustness to the 10% decision). This would also offer some sense of uncertainty in linking these patterns to inferred processes. I found some of the supplemental figures helpful, and some part of them (with data distributions and decisions) might find their way into such a concept figure.

There is a significant amount of text (and analysis) about 'drought patterns'. This might be distilled into a more cohesive point. All of these are exercises at discretizing the continuous, inferring something fundamental from something vague, and so would benefit from prioritizing the points of the drought patterns and their ecological or biological potential meaning.

Mortality is not quantified in this study, merely recounted

This study relies on Hammond et al. 2022 for data on mortality (with new data collected in a similar manner). But the actual quantitative definition of a mortality event is not obvious (in either study). The only definition that is apparent is the final sentence of the first paragraph: "This is represented by pulses of elevated mortality which exceed background rates of mortality by far" with Hartmann et al. 2018 and 2022 cited, note 2018 is not in the bibliography). Tree mortality is, to me, the critical, and most difficult, component of the study. I will not re-review Hammond et al. 2022, as it is peer reviewed and has value as a marker of studies that claim forest die off. However: 1) these methods are so critical to this study that they should not be merely cited, but re-introduced as methods for this paper, 2) Hammond et al is a meta-analysis of studies that claim mortality events. I struggled to find (but I shouldn't have to search) any real data or metric for data that determines what a mortality event actually is? What does 'by far' mean? Is it 7% death of trees over 20 cm dbh in a 5 year census interval? This could be adjusted forever, and I don't expect a single metric (though that would help), but there is no clear protocol that I can find easily, and it should be up front and center in this study (again, see the title), that quantifies what exactly constitutes a mortality event.

This lack of clarity emerges also in the text. In a single sentence or paragraph, 'tree mortality', 'forest mortality', 'forest die-offs' and other similar terms are used seemingly interchangeably or added together. This betrays the lack of mortality data used in this study. There is so much recent work on the physiology and ecology of tree death, with very important implications for how we assess, monitor, and predict future forest dynamics, that such a pattern-based approach with a bold title needs to be tightened up significantly, and present what it can present very openly and with a great deal of numbers and sensitivity analyses and caveats and potential biases. Although there are caveats later in the text, I felt that the manuscript as written relied too heavily on cited text and terms that were not defined.

The lack of quantitative rigor is important, because it's hard to trace tree observations from a meta-analysis performed years ago based on publications from even longer ago. The onus is on the authors to ensure that this study demonstrates a clear sense of how patterns are interpreted, translated into comparable data, uncertainty is characterized, and inference couched in the context of all of those decisions.

Other comments:

38: Outside of what is mentioned above, the 'triggering forest die-off and tree mortality' seems causally confusing? Can forests 'die off' without tree mortality? Is tree mortality an event or a demographic process or rate? As above, these terms need to be very clearly defined and used consistently.

40: Hartmann 2022 is in the Bibliography, but 2018 is not.

42: Is there a more recent review that can be cited to indicate the 'accumulated evidence' of the last decade?

57-58: I was confused by this sentence. How droughts translate into forest mortality is not an extremely stochastic process (n.b. on 'forest mortality', again, forests don't die, only trees) is actually fairly well known, and not stochastic, but merely difficult to definitively identify (see Zuleta et al. 2021 on the complexities of 'cause'). Mortality certainly seems stochastic, and this could be an important point of discussion for this study's approach to finding meaning from large patterns of drought and mortality.

67: again, I recommend glancing at Zuleta et al. 2021 for this.

72: Need to formally define 'drought legacies'.

74-84: This is maybe an oversimplified review of trait-demographic rate-drought interactions. The field is really much more advanced than SLA and wood density. The trade-off axes are also a bit antiquated (yes, there are collections of traits that fit canonical functional groups, but there is a great deal of complexity, as this study encounters, especially when ignoring physiological traits more directly related to drought strategies.

146: It is important to discuss clearly the difference between biomes as they may exist globally and how they may be represented in this study. E.g., ENSO events affect the Amazon and some parts of SE Asia, but not moist tropical forests in Africa, where other climate events are more important.

156: Not clear on these designations for 'growing season'. These windows mean little in the tropics, and change significantly in the non-tropics as latitude increases. Surely there are data on green-up periods (maybe based on MODUS) that can give a first approximation of the growing season at any site?

191-193: Is this somewhat circular (a concept figure might help me understand better). Because SPEI takes into account past water balance values, don't low SPEI values have to be associated (or are very likely to be associated) with longer droughts?

227-228: Height did not load on the second axis, and so there is no contrast between SLA and height.

Figure 5. These time series of the PCA axes are odd. First, the data should be plotted. Second, if they were plotted, many points would be excluded as the values for both axes are between negative and positive 0.2, but the data plotted in panel a) range between -0.45 and +0.45 on Axis 1 and similar on axis 2. Finally, there is no inherent reason why these proxies for variables should have a linear relationship to time, and so understanding what might be driving a temporary appearance of a linear relationship might help.

Zuleta et al. 2021. Individual tree damage dominates mortality risk factors across six tropical forests. New Phytolgoist.

Version 1:

Reviewer comments:

Reviewer #1

(Remarks to the Author)
I have now carefully read the last rebuttal letter and the latest comments from Referee 2.
This is a really important discussion and it shows in my opinion how important manuscripts as the one from Gazol and colleagues are.
Mortality is so difficult to define quantitatively (as we often do not have good values for background mortality in different ecosystems). This is the main point of this discussion, and I feel the manuscript would benefit from mentioning this issue (which is also a methodological one) a bit more explicitely, even though I fully understand that the authors cannot provide more quantitative rigour in defining a mortality event. Pointing to this difficulty of a clear quantitative definition and discussing that clearly in the text could reconcile the problems Referee 2 has and the points the authors raise. In my opinion, this would then show the framework within which the morality events are set, the problems that originate from different methodologies of the single studies to define a quantitatively homogenous definition of mortality and could be phrased as a plea for defining common protocols for future studies. This would then even increase the impact of this - in my opinion, already now - valuable study. The representativeness globally: This is a general problem when it comes to ground-based observations - I feel that the study already includes many tropical and subtropical cases - but they might mention potential limitations in their conclusions for these biomes more explicitly.
In summary, I think that including my suggestion would increase the strength of the paper, which I think is a very important one and it would be important to have this study published in Nature Comm.

(Remarks on code availability)

Reviewer #2

(Remarks to the Author)
I thank the reviewers for their responses in the manuscript. I think that aspects of this study are interesting and potentially valuable. I think the gap between potential value and current status is one of inference and scope. I appreciate the author's reluctance to too narrowly quantify a mortality event, but feel that my concerns regarding that definition and some of the inference that falls from that, especially in this study's attempt to present a global analysis, requires a bit more careful explaining on my part.

As I mentioned in my previous review, the professed scope of this study is ambitious and of current interest across fields in forest ecology. In order to deliver on that ambition however, analyses must hold up to serious scrutiny by the field. I'm concerned that if published as currently presented, there would arise very serious criticism of the clarity of the methods, the extent of the data, and the inferential claims. I will walk through what I mean by this in the hopes of communicating with a bit

more clarity about my concerns.

What is a mortality event?

The authors responded to my interest in having a 'mortality event' (the central unit of observation that underpins the entire project) defined. I'm a bit surprised that across the mortality event database, the publications linked to the database, and this manuscript, which extends the database, there is not even a qualitative description of methods for identifying events that could be reproduced in the introduction and methods of this manuscript?

In the authors' revision, there remains no methodological description of what a 'mortality event' is. In their rebuttal, the author's argue that this would mark a 'fundamental change in the scope of the manuscript' and that 'putting the determination of thresholds of severe forest mortality as a main topic …" is beyond the scope of the manuscript. I apologize if I failed to explain clearly what I felt was needed. It was not, at all, merely or simply a test of thresholds, but really any description at all as to how this database was built? In the current manuscript, there is no information whatsoever about how the database was compiled, what constituted something to include in the mortality event database or not, or even whether it was based on analyses of plot inventories, literature searches, etc. In the rebuttal, the authors mention 'mainly based on numerous, diverse published studies'. This is at least a reasonable start!

Perhaps have a paragraph in the Methods that identifies at least qualitatively how these data come from. Were they the product of a literature search? If so, what keywords were used? Did the authors augment with their own studies, with studies of colleagues? If the authors are concerned that the eclectic nature in which the database was built might undercut their findings, well then they have to accept that. Reproducibility in science requires details that can be followed.

What does a mortality event observation include?

I wasn't clear on how species were pulled out of mortality events. If a paper claims a significant above background mortality in a forest, how were the species pulled from that study? I'm particularly curious about how this was quantified for the tropics, but of course, temperate forests also have multiple species in a site. So how could a forest plot or site show a mortality increase, but there still be very few species represented in the analysis? As the authors are aware, species differences in drought impact is a critical component of understanding how and when community composition, ecosystem function, etc. might change. Please clarify.

Representativeness in the global context

I think the authors should remove biomes that are not well represented in the study. For example, there's no meaning to discussing anything about tropical forests in this study. If a tropical forest ecologist added a few oaks and a pine from Spain and then claimed a 'global study' the authors would certainly protest. I believe there are around 5 species representing the tropical biome in this study? This would account for 1% of 1 ha of tropical forest diversity. That's right. A single 100m x 100m forest stand in the Amazon can have 500 to 600 species. There are likely 70,000 or more tree species in the tropics. 5 species is not telling us anything at all about drought impacts, responses, trait characteristics, etc. Simplify the study and make the results more robust.

(Remarks on code availability)

Version 2:

Reviewer comments:

Reviewer #2

(Remarks to the Author)
I very much appreciate the significant effort the authors made in response to my comments and concerns. The increase in clarity in the methods, as well as the greater focus on the strengths of the database (over the biomes it poorly represents) make this a much improved manuscript. I look forward to the responses of our community to this framework and its findings.

(Remarks on code availability)

Nature Communications manuscript

REVIEWER COMMENTS

Reviewer #1 (Remarks to the Author):

This paper provides an extensive overview of tree/forest mortality reports in the literature and drought characteristics that cause tree mortality in forest types and biomes. This paper will be interesting for a large audience, and already the update on mortality events (building on previous papers of some of the authors) is extremely important. The paper's main findings are that in dry vs. wet biomes as wells as in conifer vs. deciduous tree-dominated forests, different characteristics (e.g., duration, intensity) of drought events are causing mortality. The authors also show that "warming-amplified" aridity (hot droughts) is getting more critical.

The methods applied are sound and well-justified, and it is mainly the extensive database the authors base their analysis on that is impressive. The results support the conclusions and allow for generalization (biomes, forest types), which is the central point made by the authors. It is a solid piece of science that might not provide "sexy novel insights", but the manuscript will be an essential basis for further analysis and inspire the scientific community to improve forest mortality monitoring on a global scale.

Given the solid methodological basis, the comprehensive data sets, the novel way to characterize drought features that affect particular biomes and forest types, and the general importance of the topic justify, in my opinion, the publication of this work as it is.

Arthur Gessler

Dear Prof. Arthur Gessler,

Thank you very much for the interest in our work and for your positive feedback. As you pointed out, we do not provide novel insights regarding forests mortality since it has been recognized as a worrisome global phenomenon. However, our manuscript deepens into the understanding drought-induced forest mortality by characterizing the intensity, duration, and completeness of droughts preceding forest mortality. Thus, we offer an updated database of forest mortality across biomes and forest types together with a comprehensive explanation of something that was not explored before, namely the duration of droughts preceding forest mortality.

We have revised the manuscript according to the comments of Reviewer #2 and we hope that you will enjoy reading the revised version of the manuscript.

Sincerely,

Antonio Gazol on behalf of the coauthors.

Reviewer #2 (Remarks to the Author):
Review of Warming-induced longer droughts accelerate tree mortality globally, especially in more arid regions

This study uses data from a climate database and a published meta-analysis of mortality events (updated) to link patterns of estimates of drought and temperature stress to patterns of published accounts of forest mortality.

This is an important topic, and therefore faces a high bar in how it is executed and related. It is possible that a majority of the issues I have with the study emerge from challenges in explained what are generally proxies for mechanisms mapped against each other with fairly complex metrics and filters. This is a difficult area of study, so I understand these challenges, but again, the bar is high. The title implies that this study will link climate warming to droughts, which then accelerate tree mortality, globally. (Especially in more arid regions). This is a huge claim, with strong implications of causal links and important impacts, and therefore sets a high standard for review.

Dear Reviewer #2,

Thank you very much for the valuable comments and suggestions. We are pleased that you acknowledge the significance and importance of our manuscript. Drought-induced tree mortality is a global phenomenon that deserves the attention of the scientific community. Thus, we understand the critiques regarding key points of this manuscript including the title, the definition and analysis of drought metrics (patterns), as well as the conclusions of the study. We have made substantial changes in the manuscript directed to clarify this research and the significance of our findings. Following your recommendations, we have changed the title (now it reads "Droughts preceding tree mortality events have increased in duration and intensity especially in dry biomes"), revised the explanation of drought metrics and improved the analyses (including sensitivity analyses using different thresholds and a descriptive figure in the supporting information), and clearly stated the strengths and weaknesses of our approach.

It is also important to note that originally, we consciously did not address one of the challenges you raised: "Mortality is not quantified in this study, merely recounted". As you pointed out, this is a critical point that absolutely deserves further scientific attention, but our view is that defining thresholds for the identification of forest mortality patterns in our unique global dataset is beyond the scope of this manuscript. in significant part because this is an inherent limitation with the currently available global forest-mortality field datasets that we compiled and used. We updated, enlarged and reanalyzed a global database of reported drought-induced mortality events which is mainly based on numerous, diverse published studies. Thus, in these currently available data there is wide variation in the field and analytical methods used to identify forest mortality, and (more importantly) estimates of tree damage or mortality (e.g., percentage or density of dead trees) are not reported routinely or in a standardized way. Thus, given the continued current lack of any globally-organized and standardized tree mortality monitoring program using worldwide forest plot networks, there are some fundamental constraints associated with our global-scale compilation of plot-based tree mortality dataset – which nonetheless remains the only currently available dataset of this sort for global-scale tree mortality analyses; in comparison other studies using uniformly-collected remotely-sensed data (e.g., Senf et al. 2020) or regional- to subcontinentaly-scale forest plot networks (e.g., Neuman et al. 2018) are able to define similar thresholds across regions since these data are collected with consistent methods (but at much less than global scale).

Therefore, we think that putting the determination of thresholds of forest mortality as a main topic in our study represents a fundamental change in the manuscript, a change for which our global tree mortality dataset is not well suited. We prefer to maintain the focus in the characteristics of droughts preceding forest mortality even though we recognize the limitations of doing so without a precise and consistent definition of how forest mortality is estimated.

Thus, we have explained these caveats, potential limitations, and future directions in the new version of the manuscript which maintains the focus in characterizing the droughts preceding severe forest mortality events.

In the following lines, we respond point-by-point to your comments and suggestions.

Sincerely,

Antonio Gazol on behalf of the coauthors.

This study would be better served with a more humble title, and a more focused (and narrower) scope. What the study does best is explore how droughts might occur in some parts of the world over time. What it fails to do is clearly explain many of the decisions that go into these analyses--there is no sensitivity analysis as to what constitutes a 'severe drought' or a 'mortality event', and uncertainty is not discussed explicitly. Note: because of the sample sizes, a frequentist approach to confidence intervals does not serve as an uncertainty analysis, as these just refer to point estimate precision. Standard deviations of the data would be better (along with plots of the data, distributions, etc.

I'll focus on the larger themes that this study fails to meet and then share more specific comments.

Drought patterns
Because the methods of this paper have been developed to translate continuous time-series of climate to 'events', it might be helpful to have a conceptual diagram so that the reader clearly understands everything that this translation involves. Show the density of SPEI and how a 10% decile is appropriately drawn from it so that 'droughts' are then labeled. A discussion of why 10% should be the cutoff (and the data distribution should be included) and perhaps a sensitivity analysis would support this justification (re-run some of the analysis with 5%, 15% and 25% as a cutoff to show robustness to the 10% decision). This would also offer some sense of uncertainty in linking these patterns to inferred processes. I found some of the supplemental figures helpful, and some part of them (with data distributions and decisions) might find their way into such a concept figure.

There is a significant amount of text (and analysis) about 'drought patterns'. This might be distilled into a more cohesive point. All of these are exercises at discretizing the continuous, inferring something fundamental from something vague, and so would benefit from prioritizing the points of the drought patterns and their ecological or biological potential meaning.

We have revised the title following your recommendations. The new title reads: "Droughts preceding tree mortality events have increased in duration and intensity, especially in dry biomes". In addition, we have clarified the methods section and included a figure (as recommended) to clarify the definition of drought events and their significance. We try to show how extreme drought events were detected across scales and months based on their occurrence below a threshold. This figure has been included as an extended data figure in the supporting information (see Fig. S1 in the supporting information).

We have re-run the SPEI analyses using different thresholds (deciles) to define extreme droughts preceding forest mortality. Importantly, we have done sensitivity analyses to show that our choice of 10% is justified We have added two figures in the methods to show that the selection of the percentile influences what is defined as an extreme drought but general patterns in site ordination are not very sensitive to the selection of the threshold. In fact, there is a good correlation in the first and the second axis of the PCA with the climate variables when the 10 and the 25% percentiles are used ((See Fig. S10 and S11 in the supporting information, also added to the letter).

We have made a substantial effort to reduce the explanation of drought patterns and their implication.

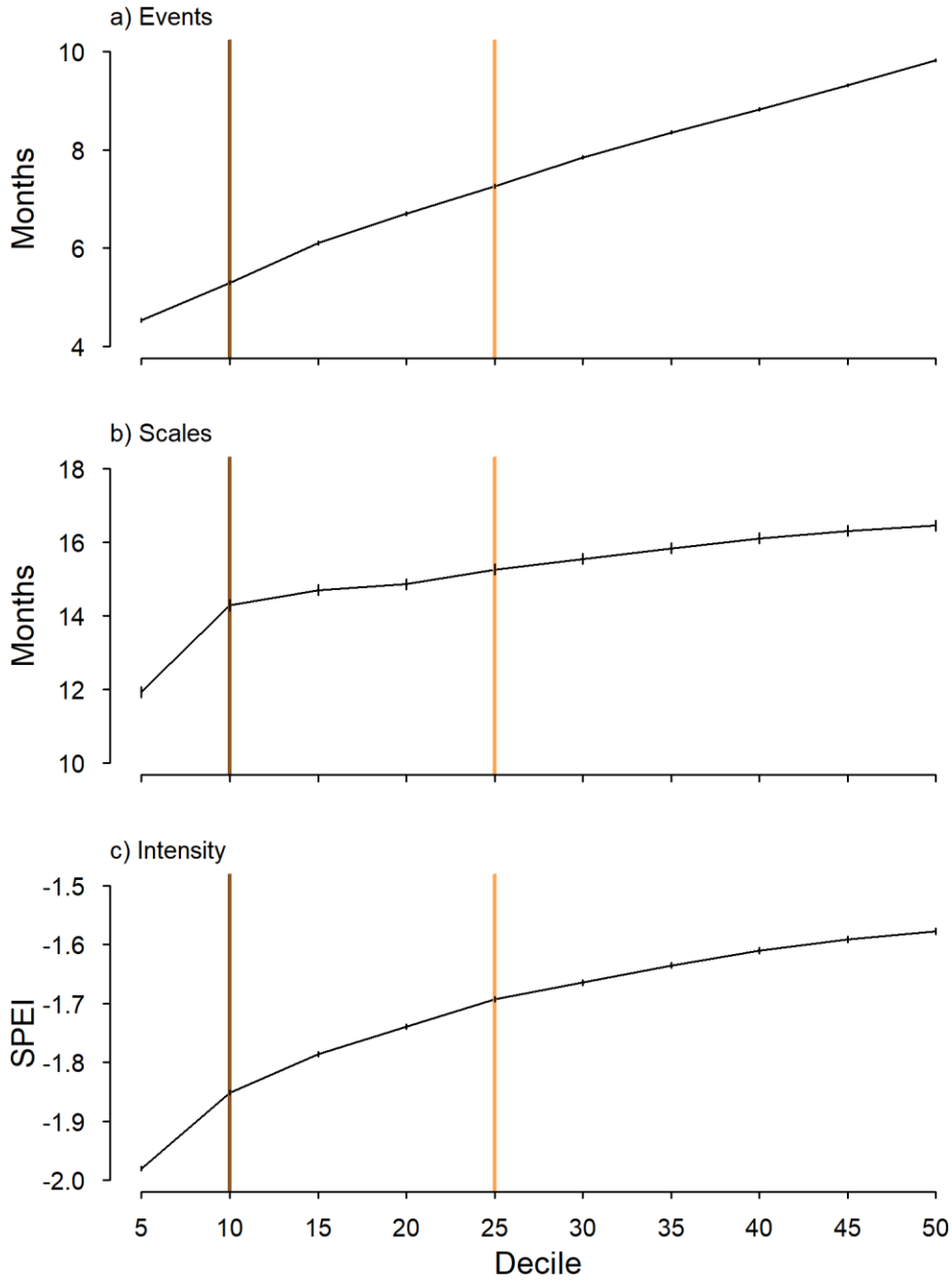


Figure S10. How the number of extreme droughts considered, the average duration in months and the minimum value of the SPEI changes as the value of the percentile chosen to define extreme droughts change. The vertical lines indicate the 10 and 25 percentiles respectively. Lines represent the cases and the vertical segments the standard error for each percentile (from 5 to 50).

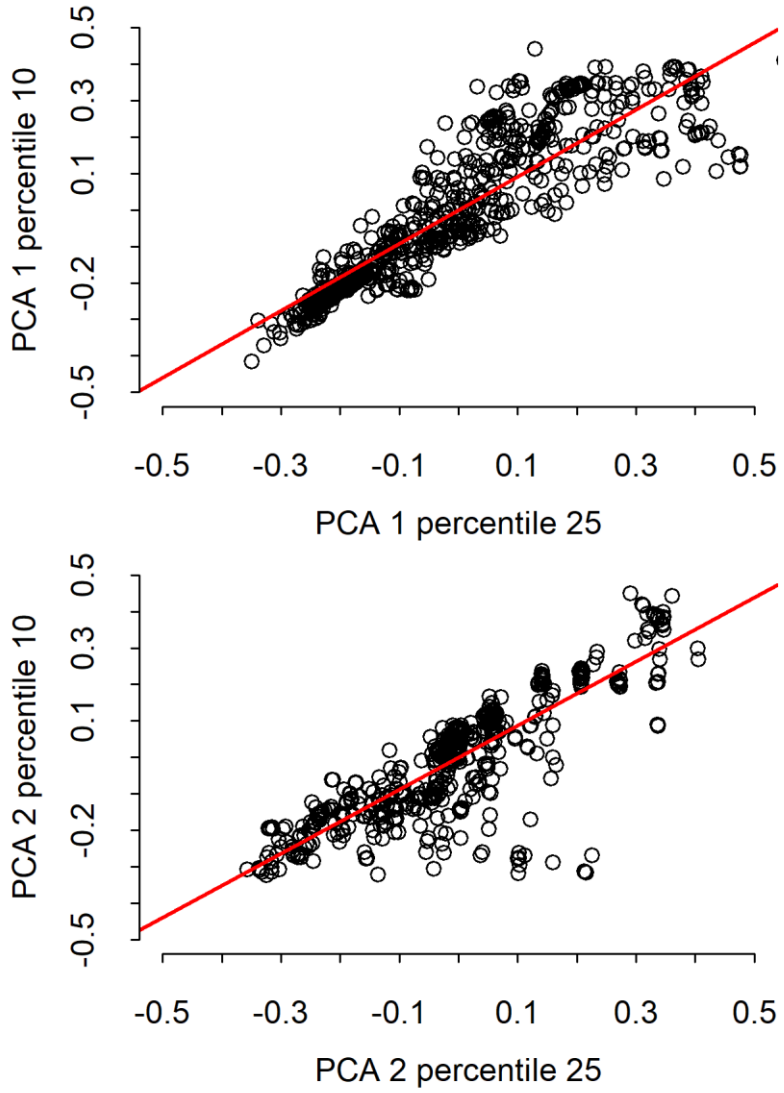


Figure S11. The relationship between the ordination of drought characteristics using the 10 and the 25% percentiles. The first and the second axis show a correlation of 0.88 and 0.83 respectively. Thus, general patterns are maintained.

Mortality is not quantified in this study, merely recounted

This study relies on Hammond et al. 2022 for data on mortality (with new data collected in a similar manner). But the actual quantitative definition of a mortality event is not obvious (in either study). The only definition that is apparent is the final sentence of the first paragraph: "This is represented by pulses of elevated mortality which exceed background rates of mortality by far" with Hartmann et al. 2018 and 2022 cited, note 2018 is not in the bibliography). Tree mortality is, to me, the critical, and most difficult, component of the study. I will not re-review Hammond et al. 2022, as it is peer reviewed and has value as a marker of studies that claim forest die off. However: 1) these methods are so critical to this study that they should not be merely cited, but re-introduced as methods for this paper, 2) Hammond et al is a meta-analysis of studies that claim mortality events. I struggled to find (but I shouldn't have to search) any real data or metric for data that determines what a mortality event actually is? What does 'by far' mean? Is it 7% death of trees over 20 cm dbh in a 5 year census interval? This could be

adjusted forever, and I don't expect a single metric (though that would help), but there is no clear protocol that I can find easily, and it should be up front and center in this study (again, see the title), that quantifies what exactly constitutes a mortality event.

As mentioned before, we think that determining thresholds of severe forest mortality as a main topic in our study represents a fundamental change in the scope of the manuscript. We agree that this and related methodological issues (protocols) are critical points that deserve further attention, but we honestly think that defining thresholds for the identification of forest mortality patterns is beyond the scope of this manuscript. We focused our analyses on an updated, enlarged and reanalyzed database of reported drought-induced mortality events which is mainly based on published studies. On the one hand, there is a wide variation in the methodology used to identify such die-off and severe mortality events. On the other hand, all reported cases have been published and assessed by experts. Thus, it is reasonable to think that the studied cases represent strong mortality events possibly surpassing background mortality rates, but it is also reasonable to think that many events occurring in other regions were not observed and thus reported. We lack this information as we also lack quantifications of tree damages or mortality in many of the studies (consider that monitoring forest plots is challenging and costly in many countries due to economical and logistic difficulties). Thus, defining objective thresholds of high mortality rates without such monitoring is very difficult given that these severe mortality events are rare in space and time. For instance, it has been published that National Forest Inventories often fail to record such severe mortality events occurring after rare extreme droughts despite they are a robust source to estimate background mortality events (see https://onlinelibrary.wiley.com/doi/abs/10.1111/gcb.13428). As an example, we can indicate the comparison of the mortality rates observed in three sites where our research group has monitored tree mortality after extreme drought events (see Camarero et al. 2015; https://doi.org/10.1111/1365-2745.12295). Tree mortality was monitored in these sites from 2012 to 2019 and the mortality rates observed were: 0.122 yr-1 for *P. sylvestris*, 0.053 yr-1 for *P. halepensis* and 0.046 yr-1 for *Abies alba*. In a study using the Spanish National Forest Inventories (https://doi.org/10.1371/journal.pone.0056843), the observed mortality rates for trees in a DBH range similar to those observed in the above reported forests mortality rates were: 0.006-0.008 yr-1 for *P. sylvestris*, 0.009-0.010 yr-1 for *P. halepensis*. No data was available for *Abies alba*. Thus, mortality rates were above or in the upper range of the observed mortality rates across regions. We acknowledge that these are just few examples but they can serve to exemplify what will be the majority of studies included in this database.

Nevertheless, we opted for explaining the limitations of our approach and suggested directions for the future in the manuscript to clearly state that this is an important point to consider and improve in future studies.

This lack of clarity emerges also in the text. In a single sentence or paragraph, 'tree mortality', 'forest mortality', 'forest die-offs' and other similar terms are used seemingly interchangeably or added together. This betrays the lack of mortality data used in this study. There is so much recent work on the physiology and ecology of tree death, with very important implications for how we assess, monitor, and predict future forest dynamics, that such a pattern-based approach with a bold title needs to be tightened up significantly, and present what it can present very openly and with a great deal of numbers and sensitivity analyses and caveats and potential biases. Although there are caveats later in the text, I felt that the manuscript as written relied too heavily on cited text and terms that were not defined.

Thank you very much for the recommendations. We have revised the use of these terms through the text in order to increase the consistency, robustness and clarity of the terms used in the text. Overall, we use the term tree mortality through the manuscript for clarity.

The lack of quantitative rigor is important, because it's hard to trace tree observations from a meta-analysis performed years ago based on publications from even longer ago. The onus is on the authors to ensure that this study demonstrates a clear sense of how patterns are interpreted,

translated into comparable data, uncertainty is characterized, and inference couched in the context of all of those decisions.

We partially agree with your comments. This is a fundamental point which is also linked to the inherent heterogeneous nature of the data and the potential uncertainty related to observations made in the past. Unfortunately, we cannot face all these challenges so we have decided to focus on the ones we can address with our quantitative data at hand. That is, focusing the main message of our manuscript in the duration, intensity and completeness of the droughts also interpreting their temporal evolution in the studied sites. Moreover, it would not be feasible having and re-measuring forest monitoring plots (to achieve robust estimates of tree mortality) in most of the observations gathered in such geographically extensive dataset as the one we analyzed.

Other comments:
38: Outside of what is mentioned above, the 'triggering forest die-off and tree mortality' seems causally confusing? Can forests 'die off' without tree mortality? Is tree mortality an event or a demographic process or rate? As above, these terms need to be very clearly defined and used consistently.

Thank you very much. We have corrected and revised the use of terms for consistency. In particular, we refer to tree mortality through the manuscript.

40: Hartmann 2022 is in the Bibliography, but 2018 is not.

Added. Sorry for the mistake.

42: Is there a more recent review that can be cited to indicate the 'accumulated evidence' of the last decade?

We added Hammond et al. 2022.

57-58: I was confused by this sentence. How droughts translate into forest mortality is not an extremely stochastic process (n.b. on 'forest mortality', again, forests don't die, only trees) is actually fairly well known, and not stochastic, but merely difficult to definitively identify (see Zuleta et al. 2021 on the complexities of 'cause'). Mortality certainly seems stochastic, and this could be an important point of discussion for this study's approach to finding meaning from large patterns of drought and mortality.

Thank you very much for pointing this out. We also thank for the reference. Regarding the term forest mortality, it is widely used in the literature to refer to the process of elevated pulses of tree mortality (https://www.iufro.org/science/task-forces/tree-mortality-patterns/).

Zuleta et al. 2021. Individual tree damage dominates mortality risk factors across six tropical forests. New Phytolgoist.

67: again, I recommend glancing at Zuleta et al. 2021 for this.

Thanks again.

72: Need to formally define 'drought legacies'.

Done.

74-84: This is maybe an oversimplified review of trait-demographic rate-drought interactions. The field is really much more advanced than SLA and wood density. The trade-off axes are also a bit antiquated (yes, there are collections of traits that fit canonical functional groups, but there is a great deal of complexity, as this study encounters, especially when ignoring physiological traits more directly related to drought strategies.

We fully agree with your comments, but it is also true that adding more traits increases the complexity and reduces the number of species with available measures. Besides, "hard" hydraulic traits have been investigated in other articles (e.g., Sanchez-Martinez et al. 2023). We

think that relying on simple and well-replicated "soft" traits allows a better generalization but assume that unexplained variance increases.

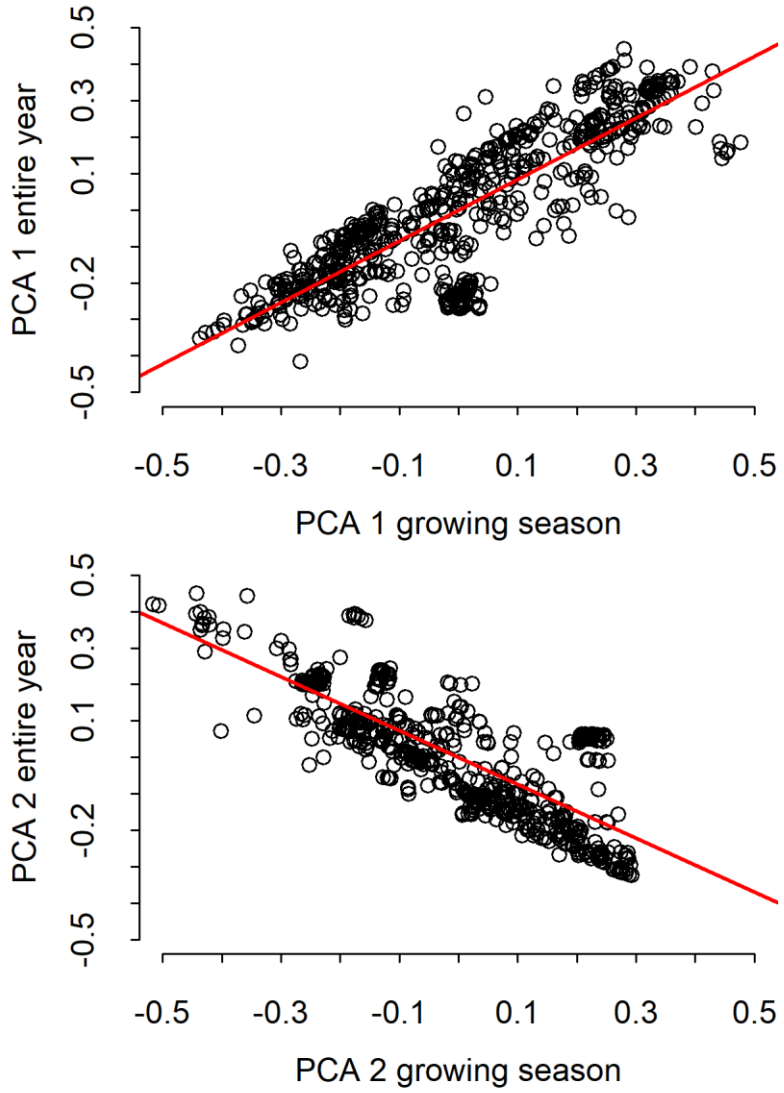
146: It is important to discuss clearly the difference between biomes as they may exist globally and how they may be represented in this study. E.g., ENSO events affect the Amazon and some parts of SE Asia, but not moist tropical forests in Africa, where other climate events are more important.

Thank you very much for the recommendation. We have corrected and revised the paragraph accordingly. In fact, we now discuss that the impacts of the 2015-2016 El Niño, considered an important drought, differently impacted South American (Brazil) and African tropical biomes (Berenguer et al. 2021, https://doi.org/10.1073/pnas.2019377118; Bennet et al. 2021, https://doi.org/10.1073/pnas.2003169118). Moreover, we have expressed our limited capacity to draw strong conclusions in the tropical biome due to the lack of data availability in our database.

156: Not clear on these designations for 'growing season'. These windows mean little in the tropics, and change significantly in the non-tropics as latitude increases. Surely there are data on green-up periods (maybe based on MODUS) that can give a first approximation of the growing season at any site?

We fully agree that growing season length varies between regions as well as it does between years. Moreover, the variation in the start of the growing season interacts with the timing of drought and affects the capacity of vegetation to recover after drought (Li et al. 2023; https://doi.org/10.1038/s41558-022-01584-2). A recent study using field measurements of growth (dendrometers) along aridity gradients also indicates that the start of the growing season varies along aridity gradients, and that it was mainly controlled by temperature rather than photoperiod or latitude (Zhang et al. 2024; https://doi.org/10.1016/j.dendro.2024.126201). Thus, there is a strong variability in the start and end of the growing season both in space and time. Moreover, the term growing season is complex not only to define but also to determine as it can be approached using different proxies related with phenology, growth, primary production or environmental conditions (Körner et al. 2023; https://doi.org/10.1111/ele.14260).

Thus, we acknowledge that the growing season length will be larger in some biomes, particularly tropical ones, and shorter in boreal biomes, thus we have decided to use the entire year instead of a selected set of months. In fact, Körner et al. (2023) suggested that what they define as the "window of opportunity" is suitable for global comparisons irrespective of the conditions during that period excluding extreme environments such as the treeline. The selection of the entire year instead of the growing season period does not alter the results considerably (see the figure below). In fact, the first and second axis of the PCA have a good correlation ($r = 0.844$ $p. < 0.01$ $r = -0.77$ $p. < 0.01$, respectively).



191-193: Is this somewhat circular (a concept figure might help me understand better). Because SPEI takes into account past water balance values, don't low SPEI values have to be associated (or are very likely to be associated) with longer droughts?

You are partially right… In fact, that is the reason why completeness and intensity are related. We explain it better in the revised results pointing to the importance of the multiscalar nature of the SPEI on driving this relationship.

227-228: Height did not load on the second axis, and so there is no contrast between SLA and height.

Corrected. Thank you very much for noting this. We also corrected the interpretation of the results accordingly.

Figure 5. These time series of the PCA axes are odd. First, the data should be plotted. Second, if they were plotted, many points would be excluded as the values for both axes are between negative and positive 0.2, but the data plotted in panel a) range between -0.45 and +0.45 on Axis 1 and similar on axis 2. Finally, there is no inherent reason why these proxies for variables

should have a linear relationship to time, and so understanding what might be driving a temporary appearance of a linear relationship might help.

We revised and implemented the figure. We rearranged the y-axis to fit all data and explained the figure. Note that this is a simplification since the interaction between year and aridity indicates that the effect is not linear. However, for simplicity we show only linear effects across selected aridity values.

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REVIEWER COMMENTS

Reviewer #1 (Remarks to the Author):
I have now carefully read the last rebuttal letter and the latest comments from Referee 2. This is a really important discussion and it shows in my opinion how important manuscripts as the one from Gazol and colleagues are.
Mortality is so difficult to define quantitatively (as we often do not have good values for background mortality in different ecosystems). This is the main point of this discussion, and I feel the manuscript would benefit from mentioning this issue (which is also a methodological one) a bit more explicitely, even though I fully understand that the authors cannot provide more quantitative rigour in defining a mortality event. Pointing to this difficulty of a clear quantitative definition and discussing that clearly in the text could reconcile the problems Referee 2 has and the points the authors raise. In my opinion, this would then show the framework within which the morality events are set, the problems that originate from different methodologies of the single studies to define a quantitatively homogenous definition of mortality and could be phrased as a plea for defining common protocols for future studies. This would then even increase the impact of this - in my opinion, already now - valuable study.
The representativeness globally: This is a general problem when it comes to ground-based observations - I feel that the study already includes many tropical and subtropical cases - but they might mention potential limitations in their conclusions for these biomes more explicitly.
In summary, I think that including my suggestion would increase the strength of the paper, which I think is a very important one and it would be important to have this study published in Nature Comm.

Dear Reviewer #1,

Thanks again for the interest in our work and for your positive feedback. We fully agree with you that defining an elevated pulse of tree mortality is difficult, but it is even more difficult to attribute the causes triggering tree mortality. Climate-induced tree mortality events are spatially and temporally heterogeneous. In most cases, they occur in sites that have not been monitored making difficult to retrospectively quantify background mortality rates and to detect when the excess of mortality occurred in response to the triggering extreme climate event (e.g. hot drought). This is why global databases such as the one analyzed here which are built based on published studies become relevant. It is true that they can be biased towards regions in which there is more concern about the response of forest to climate change and that they lack a quantitative estimation of mortality rates. However, they offer a reasonable starting point to study differences and similitudes between tree mortality events at transcontinental scales.

We have revised the manuscript according to your comments and the comments of Reviewer #2 and we hope that you will enjoy reading the revised version of the manuscript. We have modified the analyses to remove those biomes that are certainly underrepresented in our database. This includes tropical sites but also boreal biomes. Besides, we clearly explain how the database was built and what the limitations of the database are. Finally, we highlight the importance of studying climate-induced tree mortality events in those regions that are underrepresented, mainly the “Global south”.

Sincerely,
Antonio Gazol on behalf of the coauthors.

Reviewer #2 (Remarks to the Author):
I thank the reviewers for their responses in the manuscript. I think that aspects of this study are interesting and potentially valuable. I think the gap between potential value and current status is one of inference and scope. I appreciate the author´s reluctance to too narrowly quantify a mortality event, but feel that my concerns regarding that definition and some of the inference that falls from that, especially in this study´s attempt to present a global analysis, requires a bit more careful explaining on my part.

As I mentioned in my previous review, the professed scope of this study is ambitious and of current interest across fields in forest ecology. In order to deliver on that ambition however, analyses must hold up to serious scrutiny by the field. I´m concerned that if published as currently presented, there would arise very serious criticism of the clarity of the methods, the extent of the data, and the inferential claims. I will walk through what I mean by this in the hopes of communicating with a bit more clarity about my concerns.

Dear Reviewer #2,

We really appreciate your patience and the critical and insightful comments to our manuscript. We fully agree about the importance of reproducibility in science, and for that reason we have improved the explanation of how the database was constructed. Besides, we have revised the analyses following your recommendations. We have removed those biomes that are underrepresented in our study. We have tried to highlight the limitations of our approach and those potential research directions that can help to overcome them in future studies.

We hope that you will enjoy reading the revised version of the manuscript. In the following lines, we provide. Point-by-point responses to your questions and comments.

Sincerely,

Antonio Gazol on behalf of the coauthors.

What is a mortality event?
The authors responded to my interest in having a ´mortality event´ (the central unit of observation that underpins the entire project) defined. I´m a bit surprised that across the mortality event database, the publications linked to the database, and this manuscript, which extends the database, there is not even a qualitative description of methods for identifying events that could be reproduced in the introduction and methods of this manuscript?

In the authors´ revision, there remains no methodological description of what a ´mortality event´ is. In their rebuttal, the author´s argue that this would mark a ´fundamental change in the scope of the manuscript´ and that ´putting the determination of thresholds of severe forest mortality as a main topic ...” is beyond the scope of the manuscript. I apologize if I failed to explain clearly what I felt was needed. It was not, at all, merely or simply a test of thresholds, but really any description at all as to how this database was built? In the current manuscript, there is no information whatsoever about how the database was compiled, what constituted something to include in the mortality event database or not, or even whether it was based on analyses of plot inventories, literature searches, etc. In the rebuttal, the authors mention ´mainly based on numerous, diverse published studies´. This is at least a reasonable start!

Perhaps have a paragraph in the Methods that identifies at least qualitatively how these data come from. Were they the product of a literature search? If so, what keywords were used? Did

the authors augment with their own studies, with studies of colleagues? If the authors are concerned that the eclectic nature in which the database was built might undercut their findings, well then they have to accept that. Reproducibility in science requires details that can be followed.

Thank you very much for the detailed explanation and for the advice to improve the explanation about where the tree mortality data comes from. Basically, we used published databases (Caudullo & Barredo 2019; Hammond et al. 2022) which were based on previously published reviews (e.g. Allen et al. 2010) and enriched with more recent studies. We followed the same approach and harmonized the databases published by Caudullo & Barredo (2019) and Hammond et al. (2022), and added a collection of recently published studies including our own study cases. In the methods section, we provide a clear explanation on how these published databases were created by providing the criteria to include tree mortality events and the keywords used to perform literature reviews. We think that this section explains more clearly the characteristics of the database now.

What does a mortality event observation include?
I wasn´t clear on how species were pulled out of mortality events. If a paper claims a significant above background mortality in a forest, how were the species pulled from that study? I´m particularly curious about how this was quantified for the tropics, but of course, temperate forests also have multiple species in a site. So how could a forest plot or site show a mortality increase, but there still be very few species represented in the analysis? As the authors are aware, species differences in drought impact is a critical component of understanding how and when community composition, ecosystem function, etc. might change. Please clarify.

Sorry for the mistake. You are right. We did not include information on how the dominant (pre-drought) tree species were identified. In the case of Caudullo & Barredo (2019) the species affected by the drought-induced mortality event were collected if published in the studies. In the case of Hammond et al. (2022), they also retained information on the dominant tree species as the main affected species. We excluded from our analyses of functional traits those sites in which the species were not properly identified.

Representativeness in the global context
I think the authors should remove biomes that are not well represented in the study. For example, there´s no meaning to discussing anything about tropical forests in this study. If a tropical forest ecologist added a few oaks and a pine from Spain and then claimed a ´global study´ the authors would certainly protest. I believe there are around 5 species representing the tropical biome in this study? This would account for 1% of 1 ha of tropical forest diversity. That´s right. A single 100m x 100m forest stand in the Amazon can have 500 to 600 species. There are likely 70,000 or more tree species in the tropics. 5 species is not telling us anything at all about drought impacts, responses, trait characteristics, etc. Simplify the study and make the results more robust.

Following your recommendations, we have removed the tropical and boreal biomes from the analyses. These biomes were represented by a few sites. In the case of tropical biomes, they are extraordinarily rich in species preventing to use a single species as an indicator of community response. Besides, we have clearly stated the importance of considering mortality events in tropical regions, as well as focusing on community responses rather than focusing only on a few dominant tree species.