Dear Editor:

We are pleased to submit a revised version of our manuscript, “Joint effects of climate, tree size, and year on annual tree growth derived from tree-ring records of ten globally distributed forests” (GCB-21-1209, for consideration for publication in *Global Change Biology*. We have addressed all points raised by the reviewers, as detailed below. The most significant changes include:

* At the request of Reviewer 1, we have added a summary figure (new Fig. 3) that we believe provides a very helpful overview. Accompanying this, we have done some modest rearrangement of the text to create a Results sub-section entitled “Full model results overview.”
* At the request of Reviewer 2, we have modified the schematic figure illustrating the method (Fig. 1) to show raw data for one example species.

Thank you for considering this revised version, and we look forward to your response.

Sincerely,

Kristina Anderson-Teixeira (on behalf of all coauthors)

# Response to Reviewer(s)’ Comments to Author:

## Reviewer: 1

The research is overall a worthwhile topic regarding joint effects of climate, tree size, and year on annual tree growth derived from tree-ring records of ten globally distributed forests. I found the paper to be well-written and I appreciate the work you have accomplished. I consider the approach combining simultaneously the different factors controlling tree growth (non-linear effects of primary climate drivers, reconstructed tree diameter (DBH), and calendar year) highly appropriate for exploring species and stand responses to global changes in different biomes. The study is well performed and the results and their interpretations are sound. The results are convincing and tree-ring data are nicely discussed in the context of global changes. However, given the complexity of the results, it would be very useful to report a summary picture of the main results by biome/ecoregions and level of naturalness of the studied stand (old-growth, seminatural forest ecosystems or secondary forests, reforestation/afforestation if present).

**We have added a summary figure (new Fig. 3) that we believe provides a very helpful overview. (Thanks for the suggestion!) Accompanying this, we have done some modest rearrangement of the text to create a Results sub-section entitled “Full model results overview.”**

I have a few minor remarks that the authors can consider:

Lines 178-180 please insert citations for this statement. I suggest also consider artefact of sampling biases caused by the absence of old, fast-growing trees (cf. “slow-grower survivorship bias”) and of young, slow-growing trees (cf. “big-tree selection bias”) in the dataset ( see Duchesne, L., Houle, D., Ouimet, R., Caldwell, L., Gloor, M., & Brienen, R. (2019). Large apparent growth increases in boreal forests inferred from tree-rings are an artefact of sampling biases. Scientific reports, 9(1), 1-9.).

**Because the relevant references for this statement overlap significantly with those for the previous sentence, we have merged those two sentences: “Yet, even after correcting for ontogeny, growth trend detection remains subject to various potential sampling and analysis biases, which are fundamentally driven by the limitation that it is not possible to use a contemporary set of tree cores to obtain a representative sample of a species’s population at all time points throughout the history of a dynamically changing stand (Bowman et al., 2013; Brienen et al., 2017, 2012; Cherubini et al., 1998; Duchesne et al., 2019; Hember et al., 2019; Nehrbass-Ahles et al., 2014; Sullivan et al., 2016).” Note that we have added the Duchense reference here.**

**We have also added mention of slow-grower survivorship bias in the discussion: “For instance, older trees, which provide the only records available for the earliest decades, may be competitive winners that had above-average growth rates within their cohorts (Aubry-Kientz et al., 2015), which would upwardly bias average growth rate estimates for early decades ("juvenile selection effect", Groenendijk et al., 2015). In contrast, the oldest age classes being dominated by trees with below-average growth rates (e.g., Piovesan et al., 2019) could downwardly bias average growth rate estimates for early decades ("a slow-grower survivorship bias", Brienen et al., 2012; Duchesne et al., 2019).”**

Lines 180-182 please quote at least one supporting reference.

**We have slightly modified this sentence and added a reference: “Tree growth rates are sensitive to stand dynamics, with competition – the intensity of which tends to increase as forests mature – reducing woody growth rates (e.g., Zhang et al., 2015).”**

Lines 646 -648 in temperate closed old-growth forests an opposite behaviour is often reported for the oldest trees. Slow but increasing long-term growth allows for maximum longevity not only in beech, but also in oaks and pines (see Piovesan, G., Biondi, F., Baliva, M., De Vivo, G., Marchiano, V., Schettino, A., & Di Filippo, A. (2019). Lessons from the wild: slow but increasing long‐term growth allows for maximum longevity in European beech. Ecology, 100(9), e02737 and references therein).

**We have modified this section to provide an example of the opposite behaviour, citing the reference mentioned (and Duchesne et al. (2019), mentioned above): “For instance, older trees, which provide the only records available for the earliest decades, may be competitive winners that had above-average growth rates within their cohorts (Aubry-Kientz et al., 2015), which would upwardly bias average growth rate estimates for early decades ("juvenile selection effect", Groenendijk et al., 2015). In contrast, the oldest age classes being dominated by trees with below-average growth rates (e.g., Piovesan et al., 2019) could downwardly bias average growth rate estimates for early decades ("a slow-grower survivorship bias", Brienen et al., 2012; Duchesne et al., 2019).”**

## Reviewer: 2

This article aims to better understand the relationship between tree-growth from tree-rings and both climate and tree size. While these are standard questions in forest ecology and dendrochronology, they are typically considered either independently or at the stand scale. These questions are inherently related (both climate and tree size influence growth), and this article considers them jointly. These do this by presenting a new approach that: 1) identifies climate variables that are most strongly related to three different growth metrics based on detrended ring-widths; 2) for each site and species combination tests a suite of models that have the raw growth metrics (not detrended) as the response, and some combination of the selected climate variables (from step 1), DBH, and the interaction of DBH and climate; and 3) estimates parameters for the model from step 2 with the lowest AIC. This process results in estimates of the relationship between each of the three growth metrics, and climate, size and DBH. Having a better understanding of these relationships, and their variability among sites and species, will ultimately help with prediction of future forest growth (by for example informing ecosystem models).

I commend the authors for preparing such a well-written document. Of all the articles I’ve reviewed, this one has the fewest typographical and grammatical errors (if any). There are several questions/issues that should be clarified prior to publication. I think these are mostly minor, and likely will not require any further analysis (one comment may).

**Thank you!**

More general comments:

Data: I am not sure why there is no data shown on any of the figures presented. Inclusion of the raw data should help support the results of the analysis. Since they are omitted, it makes me (as a reader) question if they are intentionally not shown as a result of some discrepancy. At the very least, I would like to see data included in a panel of each figure, and a comment as to why it was omitted in the rest.

**In general, data were not presented because (1) there’s inherently a lot of variation in tree-ring (or any tree growth) data, so showing the data makes it tough to visualize the trends; (2) figures are showing partial effects and so showing the raw data would be difficult because in multivariate analyses, any single factor is showing only one part of the variance and so it’s hard to meaningfully show the “raw” data, and (3) we’re packing so much into all the figures that adding data points would make them too busy. To show some example data, we have modified Figure 1 to show raw data with model fits for one site-species combination (replacing an example showing just model fits for all species at one site). This effectively implements the suggestion to show example data for one panel in each figure, as Figs 3,5,and 6 include the panels in this example figure for all site-species combinations. We note also that raw data are visualized in Figs. 2 and S6-S9, and parallel plots for all sites are available in the GitHub repository (**[**https://github.com/EcoClimLab/ForestGEO-tree-rings**](https://github.com/EcoClimLab/ForestGEO-tree-rings)**) and archived in Zenodo.**

Census: I think the article mentions that most of these forest plots have repeat census data. How was this data used in the methods? If there were multiple census DBH measurements, how were these all used to constrain DBH time series?

**We have added a short paragraph to Appendix S2 specifying how DBH measurements were used: “DBH measurements were taken at the time of coring or – for some of the trees cored within ForestGEO plots – obtained from the most recent ForestGEO census prior to coring. For these trees, we applied the outside-in approach to DBH reconstruction (detailed below). For a minority of trees (n=50 at LDW), DBH measurements were not available, requiring use of inside-out approach to DBH reconstruction (detailed below).” We did not use multiple census measurements to constrain the DBH time series (a worthy endeavor, but beyond the scope of this project).**

Page 7-8: RW, BAI, and deltaAGB are not independent, and BAI and delta ABH are not independent of DBH, which makes it somewhat complicated to interpret the difference in each of their relationships with climate and DBH. Further discussion on this interdependence is needed. For example, there is reference to two relationships between RW and DBH. The first is that RW declines with age, and then second is that RW increase with DBH. Then the hypothesis that BAI peaks and then declines as a function of DBH. I don’t think it is possible (mathematically) to have RW increase with DBH be consistent with this BAI hypothesis.

**The key point with the hypotheses referenced is that we hypothesize that “[RW] *initially* increases with DBH for shade-tolerant species.” This hypothesis does not preclude a subsequent decline in RW across larger size classes, which indeed was the most common outcome (described in results, “Variation with DBH” section, paragraph starting with “For RW, DBH was included…”). We have carefully reviewed the language around this issue to ensure that descriptions of an “increasing” trend in RW refer only to the smaller end of the DBH range, and have found all of the language to be accurate. While a more thorough investigation of DBH trends in growth rate and their interrelationships across growth metrics could be interesting and informative, this would be beyond the scope of the current paper.**

**More broadly, we believe that the relationships among results for the various growth metrics are fundamentally consistent. We would expect, and observe, some modest differences in climate variables selected (e.g., because the various growth metrics would effectively place different weight on above- or below-average growth rates for large vs small trees), variables included in the best model, etc. However, the results across growth metrics are broadly consistent.**

L222-224: Dealing with tree and forest mortality is difficult. However, climate change is predicted to affect mortality in many forests. In turn, mortality can impact the growth of the remaining trees depending on the stature and location of the dead tree. This point should be addressed- the article presents the relationship between growth and climate only for the surviving trees. How does excluding growth data from the analysis potentially bias the results?

**We’re not entirely sure how to interpret this comment.**

**If the question is how increasing mortality might impact growth trends with year, increased mortality would result in decreased competition and potential growth release for surviving individuals. Thus, in theory, we might expect increasing mortality rates in many forests around the world (McDowell et al., 2020) to result in trends of increasing growth. However, mortality trends may not be notable enough at our sites to produce this effect, or may even go in the opposite direction (e.g., reduced intermediate disturbances at CB). Moreover, increased mortality is often correlated with decreased growth unless the conditions reverse (so, for example, the trend at SC is towards increased mortality and decreased growth). Overall, in this analysis, negative growth trends predominate, and so we do not believe that growth release from increasing mortality is a dominant factor. However, we have added mention of changing mortality rates in the discussion: “Within the three older forests (ZOF, LT, CB), population-mean growth trends were mixed (Table 1, Fig. 7), probably reflecting some combination of successional changes, *changing mortality rates*, and shifting competitive advantages, perhaps in part driven by changing environmental conditions (Furniss et al., 2017; Vrška et al., 2009) or the lack of intermediate disturbances giving rise to increasing crowding (e.g., Lutz et al., 2009).”**

**If the question is about differences in climate sensitivity between the trees that die and those that survive, this is an interesting question, but remains beyond the scope of this analysis. For one site where we included cores from trees cored dead and live (SCBI), we previously found that trees cored dead displayed similar climate sensitivity to trees cored live (Helcoski et al., 2019). Other studies have found differences in drought sensitivity among trees that survived or died during a drought, but even if that occurred at one or more of our sites, it does not seem like it would fundamentally alter any of our results or conclusions.**

P19-20 (especially L301-303): I think it could be made more clear that detrending was used only in the process of selecting strongly related climate variables, and not in the GLS models. Additionally, a comment on how the variability in the application of detrending method used here (thin plate splines) would impact climate variable selection would be warranted. There is a persistent question in dendrochronology about the amount of climate signal gets removed through the process of detrending.

**In the GLS models, we use a temporal autocorrelation structure on raw data. We have revised the GLS model description to make this more clear: "We next combined the primary climate drivers in temperature and precipitation variable groups (included in all models) and DBH (included in models with DBH and DBH-climate interactions) as candidate variables in linear mixed-effects models (function *lme* in the R package *nlme*, Pinheiro et al., 2021). In all models, we included core identity as a random intercept and year as a continuous time covariate for the within-group correlation structure (function *corCAR1*) to account for temporal autocorrelation (similar to how detrending would). We will refer to this model as a generalized least squares (GLS) model (Fig. 1).Within the GLS models, our response variables were raw, log-transformed growth estimates (as opposed to residuals): log[RW], log[BAI], or log[ AGB].**

**Dentrending method is often a highly contentious topic within the dendro community, especially when reconstructing climate variables over long time scales . In our study, our candidate variables had large interannual variability and the relatively short series length does not extend to the point where detrending method will have a significant influence on the identified climate signal (Bunn et al., 2004). We have compared our approach with more traditional detrending techniques and observed that there were not statistically significant differences among the methodologies (Appendix S4). To clarify this in the manuscript, we have added the following sentence (under *Step 1:…* header): “Our application of the thin-plate regression splines acts similar to more traditional a priori detrending methods using a two-thirds spline commonly used in dendrochronology studies and results in similar predictor variable selection (Appendix 4, Cook & Peters, 1997; Rollinson et al., 2021).”**

L301-302: Motivation as to why one climate variable from each group was selected is missing.

**We believe that the clarification needed here is that they were included as *candidate* variables. We have reworded to clarify this: “We next combined the primary climate drivers in temperature and precipitation variable groups (included in all models) and DBH (included in models with DBH and DBH-climate interactions) as candidate variables in linear mixed-effects models (function *lme* in the R package *nlme*, Pinheiro et al., 2021).”**

L302-303: DBH interactions with climate? Or climate-climate interactions?

**We have clarified this: “..DBH (included in models with DBH and DBH-climate interactions).”**

P20-21: Various recent publications have highlighted the importance of the effect of climate interactions and climate disturbance interactions on tree growth. It is not clear to me why the DBH-climate interactions were included, but climate-climate interactions were not.

**Our method could be used to examine climate-climate interactions, and we believe that would be a valuable thing to do; however, we did not include that here because (1) the scope of the analysis is already very large and challenging to present concisely, and (2) we have limited time and resources to try all of the potentially interesting analyses. We now clarify this in the text: “We did not run models of precipitation temperature, climate DBH + year, or climate year, which would be possible in the GLS model framework but would require additional statistical and conceptual validation beyond the scope of the current analysis.”**

L313: From what I understand, a concave up fit is perfectly reasonable. For example, a standard exponential growth function is concave up, and is biologically possible. Clarify. Also clarify on how they were excluded.

**Because the growth variable is log-transformed in our analysis, an exponential (concave-up) fit would be captured by a linear function. What we seek to exclude is u-shaped responses, which would not make sense biologically. We have clarified this in the text: “For climate response, we allowed concave-down fits, but ignored any concave-up fits on the basis that exponential functions would be captured by a linear fit to the log-transformed growth variables, while u-shaped fits are not expected biologically.”**

L336-337: Is it also true that climate + DBH + climate x DBH + year is included? If not clarify why not.

**Our method could be used to do this, and we believe that would be a valuable thing to do; however, we did not include that here because (1) the scope of the analysis is already very large and challenging to present concisely, and (2) we have limited time and resources to try all of the potentially interesting analyses. We now clarify this in the text: “We did not run models of precipitation temperature, climate DBH + year, or climate year, which would be possible in the GLS model framework but would require additional statistical and conceptual validation beyond the scope of the current analysis.”**

L370: Clarify where the 20 comes from. Maybe mention it earlier after the discussion about what was included versus excluded. I tried to determine where this number came from but am lost.

**We have clarified as follows: “For eight of 20 site-variable group combinations (i.e., water and temperature, each at 10 sites),…”**

Figure 3: There appear to be some concave up fits in here, which is not consistent with the statement that these were excluded.

**These appear because the growth variable is log-transformed in the analysis, but fits are plotted here on a linear scale. This should now be understandable to readers through our clarirication in the text (from comment on L313): “For climate response, we allowed concave-down fits, but ignored any concave-up fits on the basis that exponential functions would be captured by a linear fit to the log-transformed growth variables, while u-shaped fits are not expected biologically.”**

Figure 4: This is really interesting. It looks like there is often little effect of climate on the growth of small trees, which seems expected but I’m not sure has been shown like this before.

**Thank you!**

Line 545-547: Agree that this is useful to help with prediction, but what is missing from the article is a more clear idea of how this could/would be done. What emerges here is that trees and forests are complicated. There seem to be some general patterns emerge, but for each of these there are always exceptions. How do you propose synthesizing all of this to actually improve predictions?

**We agree that forests are complex, and this method both reveals and allows us to start to distill some of that complexity. We have added a summary figure (new Fig. 3) to help to highlight some of the general patterns. There is obviously a lot more work to be done on this theme that is beyond the scope of the current analysis.**

Section 590: It is really interesting to contract the variability between population scale and individual scale. There was a great paper by Jim Clark that constrasted these scales and showed that responses could appear different for each. Can’t remember the reference, but this seems to be the theme of this paper: individual scale can be quite different from population scale.

**Good point. To remind readers that our models refer to population-mean responses, we have inserted phrases such as “mean” or “on average” in appropriate places throughout the results and discussion. We have also added specific mention of the principle highlighted by the Clark paper: “In large part, this discrepancy can be explained by differences between cross-sectional analyses and”longitudinal" patterns of individual trees through time (Forrester, 2021; Sheil et al., 2017), consistent with the principle that individual-scale growth patterns and environmental responses do not necessarily match population- or stand-level average responses (Clark et al., 2011, 2003)."**

L630: I’m not sure “correct” is the right term. Since it is a joint model you can account for these things, but how this impacts the response is dependent on the other terms in the model. Maybe I missed something- is the climate signal removed before GLS modelling? I *think* the raw growth metrics are the response, right?

**We have changed this word to “account.”**

Very minor:

L94: enormous is a subjective word, change

**We have changed this word to “large.”**

L96-100: sentence awkward (not sure how the “the or to predict…” follows from first part of sentence)

**We have reworded as follows: “Yet, collection and analysis of dendrochronological records has traditionally been optimized to detect climate signals rather than to understand variation among trees – including size-related variation in climate sensitivity (e.g., Bennett et al., 2015; McGregor et al., 2021; Rollinson et al., 2021) – or to predict forest productivity, its climate sensitivity, and how it may be changing (Babst et al., 2018; Cherubini et al., 1998; Klesse et al., 2018; Nehrbass-Ahles et al., 2014; Wilmking et al., 2020).”**

L269: Defined ‘most important’

**We have reworded as follows: “We used the *climwin* package in R (van de Pol et al., 2016) to identify the most influential climate variable (*i.e.*, that most strongly correlated with annual growth)…”**

L280: linear and quadratic terms?

**We have changed this sentence to read, “…specifying linear and quadratic terms to allow for potential nonlinearities in the climate response.”**

L308: So models with variables removed may include one climate variable and DBH?

**In theory, yes. In practice, no variables required removing. To make this more clear, we have reworded as follows: “Prior to running the models, we checked for collinearity among the candidate variables using the *vifstep* function (Naimi et al., 2014). Our analysis code was programmed to remove any variable with a variance inflation factor > 3, but none required removal.”**

L311: Clarify “we ran every combination.” This is only for selected climate variable right?

**We have reworded as follows: “For each species independently, we ran GLS models including every possible combination of independent variables (*i.e.*, candidate climate drivers, DBH, and year), including both first- and second-order terms for each.”**

L318: Define “complete data set”

**We have reworded as follows: “…where *x* is a complete data set for one species at one site (all records after excluding cores as described above, and with no missing values).”**

L344: “rarely differed significantly”; I think you mean your estimates when compared to the values obtained from the standard analysis (as opposed to your estimates differing from each other). Clarify.

**We have reworded this to read, “While one-to-one correspondence of estimated slope coefficients describing the response of tree growth to interannual climatic variation was neither expected nor observed, estimates obtained using the two methods were correlated and rarely differed significantly from one another.”**

Figure 2: It’s still not clear to me what “window open” and “window close” refer to. Clarify.

**We have re-worded as follows: “Panels (b-d) give statistics for seasonal windows tested in *climwin*, where window open and close refer to the start- and end-months of the window tested, expressed as months prior to current August, and cells across the lower diagonal indicate single-month tests (akin to panel a).”**

L614: I’m still not sure what “cross-sectional” refers to. Does this mean census?

**We have clarified the definition in the preceding paragraph: “This latter pattern is consistent with the observation that when contemporary growth rates are compared across individuals within a closed-canopy stand (e.g., a”cross-sectional" analysis of census data)…"**

L627: What are you reconstructing? You mean back-calculating DBH? Probably a terminology thing.

**“Reconstructing” refers primarily to back-calculating, although in some cases DBH is projected slightly forward (e.g., at SCBI, where cores were taken a couple years after a census was conducted). For this reason, we prefer to stick with the “reconstructed” term. To make sure this is clear, we now refer to the term “back-calculated” in the methods section when we introduce the concept: " For each year in the tree-ring records, we reconstructed (*i.e.*, back-calculated) DBH, as detailed in Appendix S2."**

L678, 683: Weird formatting issue in ref. Maybe journal software?

**I do not see any problem (on a Mac), including in the draft proof. I have opened a GitHub issue (**[**https://github.com/EcoClimLab/ForestGEO-tree-rings/issues/142**](https://github.com/EcoClimLab/ForestGEO-tree-rings/issues/142)**) to remind myself to check this at the proof stage.**

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