nature portfolio

Peer Review File

Widespread slow growth of acquisitive tree species

Corresponding Author: Dr Laurent Augusto and Dr Marie Charru

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

Version 0:

Reviewer comments:

Referee #1

(Remarks to the Author)
The authors demonstrate that relative growth rates of trees planted in monocultures differ from relative growth rates predicted from plant traits. As one example, species with large maximum photosynthetic rates are predicted to grow faster than species with smaller maximum photosynthetic rates; however, the authors demonstrate the opposite outcome when trees are planted in monocultures. This has important policy implications because trees are increasingly planted to sequester carbon, with a focus on species expected to grow rapidly including species with large maximum photosynthetic rates. The authors find similar unexpected results for other plant traits. The authors conclude that species expected to grow fast often do poorly when planted in monoculture and recommend that tree planting should be informed by local knowledge of environmental conditions and species-specific environmental requirements of tree species to be planted.

The analyses hold several surprises. The authors analyze three different data sets with slightly different methods. I will use their EAN data set as an example. Equation 1 (manuscript line 618) defines “site productivity” as the mean of species-specific mean growth rates for each species. Equation 2 (line 654) then defines relative growth rates as the logarithm of the “absolute value of species mean growth rate” divided by “site productivity”. A first question is why “absolute value”? Are there species with negative mean growth rates? What are the implications of making negative mean growth rates into positive mean growth rates by taking absolute values? This seems problematic.

I will set the “absolute value” problem aside for a moment to consider Equation 2. Since “site productivity” is the mean growth rate with species weighted equally, Equation 2 amounts to a ratio of species-specific growth rates to site-specific mean growth rates (were it not for that absolute value). Ratios often have problematic distributions and that appears to be the case here. Equation 2 includes a log transformation of the ratio. The log transformation is insufficient, however, so extremely small values are truncated to -2 (line 659). This also seems problematic.

I recommend that the authors consider standardizing growth rates to become standard normal deviates by subtracting the mean growth rate and dividing by the standard deviation of growth rates for each site. This will place all sites on the same scale with mean 0 and standard deviation 1.

Another problem occurs when the authors test for site-trait interactions in the EAN data set. The authors evaluate a linear model between “site productivity” and the Pearson correlation coefficient between species-specific mean growth rates and trait values (Equation 3, line 700; Figure 3). I have never seen an analysis like this possibly because of a problem discussed by the authors. Their Pearson correlation coefficients apparently vary with sample size across sites, where sample size is the number of species with growth and trait data at each site. The authors solve this problem by constraining this sample size, which also seems problematic. The authors might consider a test for site-trait interactions using linear mixed effects models as follows:
$G_{i,j} = \text{trait}_i * \text{site productivty}_j + (1 + \text{trait}_i \mid \text{site}_j)$.
Here $G_{i,j}$ is the growth rate of species i at site j. The model includes a random intercept for site and a random slope for trait values for each site.

The authors used two different methods to evaluate site-trait interactions for their SBD data set and for sites in their TDN data set with just three to eight species. The different methods are categorical and continuous. When continuous data are available, categorical analyses provide less information and should be avoided. I will focus on the analysis of continuous data. There is another problem at Equations 4 and 5 (lines 741 and 742). Equation 6 defines a site suitability index as the product of a climate index and a soil carbon:nitrogen ratio or C:N ratio index. Equations 4 and 5 define the soil C:N and

climate indices using a minimum function. For example, all soil C:N ratios smaller than 12 are defined to equal 1 while C:N ratios larger than 12 are defined to be 12 divided by the C:N ratio. The authors argue that C:N ratios of 12 separate unfavorable (>12) and favorable (<12) sites. This index combines categorical and continuous values. The index combines a categorical definition of favorable (i.e., C:N < 12 is set to 1) with a continuous definition of unfavorable (i.e., C:N > 12 equals 12 divided by the C:N ratio). This is the first time I have encountered a variable that combines categorical and continuous components. This index is a problem.

Referee #2

(Remarks to the Author)
The manuscript "Generalized slow growth of fast-growing trees" brings a critical view of the performance of so-called slow-growing and fast-growing species by analyzing the association of growth rate and traits in a broad geographical gradient. Associating growth rate and plant trait is not new per se, but using the traits to classify species in terms of growth and evaluating how they perform under various environmental conditions is rather new.
The study is based on a comprehensive collection of three datasets on common garden plots, namely: European Atlantic Network (ENA), TreeDivNet (TDN), and Stand Biomass Dataset (SBD). Out of these three, the EAN and TDN have data on tree height growth and the SBD is mainly a picture of the biomass, with no successive surveys. These plots combine species of angiosperms (99 species) and conifers (53 species). Still, the core of the actual growth data is located in North America and Europe.
The authors employed quite a few approaches for predictor selection based on traits and climate conditions, which is fine. And then, the core of the analyses are overall standard and quite straightforward.
Based on all this significant dataset and I would say appropriate analyses, the authors find some intriguing patterns related to the association between the traits and growth rate. Indeed, these associations change along gradients of climate, soil characteristics, and stand productivity. In the end, an alternative conceptual model is presented.
Although the results are really interesting and potentially disruptive, I have some reservations regarding the dataset, and how the analyses under undertaken. Basically, I wonder how the results may be biased by the limited geographical representativeness in the tropics and by the potential influence of a phylogenetic effect in the data.
Regarding the distribution of the dataset, it is mainly located in temperate areas in North America and Europe. Although the authors correctly point out the role of tropical forests in the carbon cycle, the datasets don't really represent well them. This adds an important bias for striving for global conclusions because tropical and extra-tropical forests face different limitations in terms of soil fertility and climate. Soil fertility is significantly lower in the tropics, while tropical forests may struggle in warmer conditions. Although the dataset from SBD comprises some tropical sites, the data seems to be unbalanced towards cold-limited sites. In addition, it is not clear to me how the growth rate was calculated if it has only one measurement of biomass per site. Maybe that is something that needs additional clarification in the Material and Methods.
Finally, one must be careful while mixing angiosperms and conifer species for the reasons presented before. From my experience, one should analyze them separately because the results are too influenced by the composition of the species.

In Summary, this is a valuable contribution to a broad audience interested in forest growth, carbon gain, and climate change mitigation. However, there are still important gaps that need to be checked and re-evaluated.

Please see below some minor comments and suggestions:

L67: Sounds like a contradicting information. I understand your point here, but I would put it in another way. You can say that forest planting is supported by trait-based decisions, but field studies fail to detect it clearly. growth-trait relationships
L79: I think you are overly careful with your statement, maybe just say "...species have a higher potential...".
L85: The potential to mitigate current rates
L93: Focusing on fast-growing trees is controversial if one convents the fact that there is a live-fast and die-young conundrum. So, at first, having a fast-growing community of trees may accelerate carbon gain, but this carbon will be stored for a very limited time because these trees will not reach old age. The exceptions may only be the long-lived pioneer species.
L116: Please, can you quickly bring this context to this part of the text?
L122: I understand you are bringing the main message forefront. It is a nice strategy, no question, but, not sure it works where you are just defining what you call a fast-growing species.
L135: we categorized
L183: Not sure you can truly generalize it across such a broad geographical gradient.
L186: I believe there is one key thing that is not mentioned here in relation to field experiments, and that is the role of competition. Competition also increases under more favorable conditions. How would it affect or eventually bias your results?
L195: However, based on 139 sites,
L209: I think that it is not really the tree species that drive more soil organic carbon, but it is more the interaction among site characteristics that does.
L211: This is an overall beautiful study, and I think it deserves a better and more focused conclusion. There are some elements that just came out of the blue right in the last sentence. You can of course draw general conclusions but within the scope of your own study. Otherwise, you only shade the real advances of it.
L417: common gardens found across
L464: Please, if the SBD does not provide growth data, it is a picture of the site at a given moment, how it is useful for this study?
L467: After assessing the data quality,
L484: Not sure about this statement. Biomass accumulation may imply a rate. You have the stand biomass only.

L507: moribund trees?
L524: I understand this dataset adds a lot in terms of species, sites, and environmental conditions. But it feels like you are making too many assumptions to use this data in this study. Therefore, the real role of it is not clear.
L605: I am not sure it is the case for temperature in the tropics. And also, this is not the right reference for this context, because it is not about decomposition, but about carbon gain.
L649: What were the second variables chosen by each approach?
L678: I understand that defining these thresholds is tricky, but it is one of the main steps in this study. How the results would change if you shift these thresholds say 10% up and down? As you pointed out, this is an artificial division of the data.
L681: This is a very important statement and in line with a previous comment of mine. Conifers and angiosperms have very different strategies, in terms of physiology, anatomy, morphology, and so on. I would treat them separately. Otherwise, you will be assuming a global pattern for all plants, while it is mainly driven by taxonomic issues.
Extended Data Table 3: Why wood density is higher in fast-growing trees? Is it because you are actually comparing groups composed mostly of conifers or angiosperms?

Referee #3

(Remarks to the Author)
Augusto and co-authors have collected and analysed an impressive dataset of tree growth in common garden experiments across continents, climate zones and forest types; a truly impressive dataset and an analysis at unprecedented scale. They used these data to evaluate whether tree species that are considered to have functional traits allowing fast growth actually also grow faster in a forest stands, compared to species that have traits associated with slow growth. The authors carefully account for differences in productivity and species sets across sites, and take methodological differences across the three datasets into account. The findings are truly original, highly significant for the field of forest ecology, and of interest to practitioners in reforestation and afforestation. The authors find that species with highest potential growth rate (i.e., highest rate of photosynthesis) tend to be the relatively slower growers (i.e., in terms of height or biomass) in real-life practice of growing forest stands. The study also shows that this association seems to be strongest under unfavourable climate or poor soils. The authors explain results by suggesting that species with functional traits that would support fast growth, only can realize fast growth under optimal conditions.

Main comments
1. Species classification. While in the text, authors are careful to explain that the classification of fast and slow growing trees is based on functional traits and not on growth data, it still is strange (and likely incorrect) to call these fast- and slow-growing species, even if preceded by the term “supposedly” or somewhat more hidden as in x-axis label of Fig 5. I stress this because I wonder whether for the 150 spp included in the study the ranking of actual growth rate (in terms of height, biomass or diameter) corresponds to that inferred from functional trait data. Without such a check it would be hard to even call species with high Amax (or SLA or leafN) “supposedly fast growing”. If the authors are not able to establish such an association for a large share of their study species, the link between functional traits and (potential) growth is not proven and it would be hard to maintain terms “fast and slow growers”.
2. Intra-specific differences in traits and growth rates. The sites of the common garden experiments were distributed across wide climatic and environmental gradients. This may induce variation in functional traits. If I understand correctly, one functional trait value is now used per species, taken from databases, or measured by the authors. It is unclear whether the literature values are averages across published values or not, and what the sizes were of the individuals of which trait values were used (relevant because trait values vary with size). In any case, it seems that acclimation of functional traits to climatic or edaphic conditions is not taken into account. For the interpretation of the results, it is necessary to know whether intra-specific trait variation across the environmental gradients could help explaining the observed patterns (e.g., Figs 1, 2, 3).
3. Climatic niches. A possible contribution to explaining the lower (relative) growth rate of (potentially) fast-growing species in common garden experiments is that these species are climatically more restricted than slower growing species. This may be relevant for common garden experiments, in which tree species can also be planted outside their climate (or environmental) envelop. I do not think there is general evidence that fast growing species are climatically restricted, but it would be good to verify to what extent the relative growth rate is positively associated with the spatial extent of species, or whether species located outside their climatic (or edaphic) envelop are growing (relatively) more slowly. A second – and related -- comment on climate effects is that the negative growth-Amax relationships (Fig 2A) for unfavourable climate is based on a general classification of climatic favourability (Sup Fig S4), while a species-specific assessment of the suitability would probably be better. This could be done by verifying whether a site is within or outside climate envelop of a species, or whether it is more favourable or unfavourable climatically compared to the species-optimal climatic conditions.
4. The ecological foundation of the expected (and actual) results of the study can be more extensively explained and introduced earlier in the introduction. Now the hypothesis on the fact that fast-growing (i.e., acquisitive) species only comes after mentioning the main aim of the study. In my view, the introduction would be strengthened, if the fundamental contrast between the goals of forest-based climate solutions (calling for fast-growing tree species), the changing climate and increasing climatic extremes (calling for resilient tree species) and the ecological characteristics of fast-growing species (acquisitive nature, implying low efficiency of water use and probably also nutrient use) would be stressed. This could even be illustrated with a Figure.
5. I find it strange that site productivity for the SBD dataset (which contains biomass growth) is based on environmental drivers instead of a measure of productivity (height growth being used for the other datasets). I would say that for the SBD plots, biomass productivity can be used as a very direct way to estimate site productivity.

Other comments:
- Title: could be changed into a more clear description. The term generalised is quite vague, and perhaps title could reflect

difference between potential growth and realized rates in practice?
- Line 104: replace “question” by “evaluate” as it is strange to “question” something in a scientific study.
- Line 108: nutritive stresses is unclear to me, and I wonder whether this means excess nutrients deposition (as in EU, a.o.) or nutrient scarcity.
- Line 123-132: It was a bit hard to understand the function of this paragraph, especially the start of the text. It seems to deal with (relative) details while the reader probably expects general results/patterns.
- Lines 167-175: Wouldn’t an obvious and simple solution to this problem be to measure functional traits for large tree individuals, in situ? And if so, why weren’t such values used in this study? They are available (and even part has been obtained for missing traits, I read).
- Line 189: I don’t think the word “really” is needed here…
- Line 192: not sure what “biological activity” is. This can also be activity of herbivores, pests species, pathogens ….
- Line 194-196: I think this is probably the most important message and Figure of the paper. Would be good to emphasize this more.
- Line 202-203: I do not get “despite” (L 202). If ‘fast-growing species’ have higher WD this actually may actually partially explain the lower relative growth rate because they require more biomass to realize the same height growth.
- Line 205-208: I find this discussion hard to follow and quite far fetched. Inferences are made of carbon sequestration strategies of fast and slow growers based on comparisons in WD and C density which use different sample sizes. Note also that the median differences are quite small and thus have little meaning ecologically.
- Line 215: two times management in one sentence?
- Lines 215-222. I do not get why sustainable forest management is discussed here at length: it has not been mentioned in the introduction and it’s unclear what share of the species included in the study are valuable (for timber). I would focus and limit this discussion to increasing carbon stocks in (newly planted) forests, and perhaps mention that this is also relevant for the growth of timber volume in planted forests.
- Lines 223-230. I think this last part of the discussion could make more use of the results presented here: concluding that local foresters should best know what species to plant is in general a good idea, but does not do justice to the results of this study. Wouldn’t it be very useful to produce a list of the fastest- and slowest-growing species at each site? Or putting that in a separate dataset? There is a wealth of information in this study that can be used to inform decisions on local forest restoration/planting/afforestation.
- Fig 1&2: Wouldn’t it helpful to combine these in 1? Their axes are the same and Fig 2 is a detailed assessment of the overall patterns in Fig 1.
- Fig 1. Perhaps briefly explain what normalization entailed (that this was done per site)
- Fig 3. This Fig is a quite hard to grasp, also because the caption seems to provide technical details rather than guidance to understand the complex set of graphs. Also, the results in panel A are similar to those in 2A, and the latter are more easy to grasp. Another option would be to show relations like in Fig 2 for some of the traits shown in Fig 3.
- Fig 4: very clear Figure. Perhaps in caption mention that C and D provide support for the revised paradigm. Are C and D based on different datasets? Wouldn’t it be logical to include “height” in y-axis label in C and “biomass” in D?
- Ext Dat Fig 4. Caption is unclear: “Each panel presents… of a given species” is incorrect? Each panel contains values from multiple species, right?
- Ext Dat Fig 7. I think it would be nice to combine this as additional panels to Fig 5. It shows consistent differences across the three datasets. To me this is more important than Fig 2.

Version 1:

Reviewer comments:

Referee #1

(Remarks to the Author)

The authors conclude that species with functional traits associated with efficient resource conservation have greater growth rates than do species with functional traits associated with rapid resource acquisition when grown outdoors in monocultures. This is not surprising. Current understanding is that species with functional traits associated with rapid resource acquisition have larger potential growth rates than do species with functional traits associated with efficient resource conservation. Potential growth rates are only realized when resource supplies are adequate and plant enemies are unimportant. These conditions are met in growing houses when nutrients are added and enemies are excluded (as the authors demonstrate at lines 106 to 109). It is not surprising that realized growth rates do not match current understanding of relationships between potential growth rates and functional traits when plants are grown outdoors without nutrient supplements and in monocultures that attract enemies.
The manuscript uses data from three studies (named EAN, TDN and SBD) and a composite of several tropical studies. Height growth is calculated for trees in the EAN and TDN studies and then standardized before analysis (equation 2). In their rebuttal letter under their subheading “Comment 02”, the authors state that equation 2 from their original submission defines a response ratio. Note, equation 2 is identical in the original and revised submissions. In their rebuttal letter, the authors cite papers by Hedges and Gurevitch for the response ratio. The seminal paper (Hedges et al. 1999. Ecology 80: 1150) defines the response ratio as follows: “The response ratio, the ratio of some measured quantity in experimental and control groups, is commonly used as a measure of experimental effect because it quantifies the proportionate change that results from an experimental manipulation.” In contrast, equation 2 is the ratio of a species-level mean growth rate to site productivity. Equation 2 lacks an experimental group. Equation 2 lacks a control group. Equation 2 is not a response ratio. The authors define site productivity in the denominator of equation 2 to be the mean growth rate over all species present at

each site. Thus, equation 2 standardizes species-level mean rates by dividing by the mean growth rate over all species present (with species weighted equally).
There is a potential problem for the EAN and TDN studies. Initial plant size has a huge effect on growth rates. Tree age varies from 3 to 10 years in EAN and from 8 to 24 years in TDN (see Supplementary Table S1). Height growth was quantified over a time interval of 1 to 9 years, which started more than 2 years after planting (lines 462-473). That is all the reader knows. A potential problem arises because different species may have had different initial sizes at the beginning of the time interval used to calculate height growth. The very large effect of initial size on growth rates could then explain much of the interspecific variation in observed growth rates. The main conclusion of the manuscript is that species with conservative functional traits often grow faster than species with acquisitive functional traits. If these two categories of species had attained different sizes at the beginning of the time interval used to measure height growth, then that alone might explain observed differences in realized growth. Initial plant size could enter the analyses as a covariate, but this has not been done.
There is also a problem with the analysis of the SBD data set. The SBD data set consists of a single observation of stand-level biomass. The authors obtain an estimate of site productivity by dividing stand-level biomass by stand age. Stand age ranges from 11 to 225 years. Stand-level biomass increases rapidly in young stands and reaches an asymptote in mature stands. Thus, the site productivity measure must be large for young stands and small for old stands. Thus, the author's interpretation of stand productivity is problematic for the SBD data set.
Finally, the authors recommend that species be planted in appropriate environmental conditions to maximize carbon sequestration. No one could object. The authors also recommend that this will usually be species with functional traits associated with resource conservation. I believe this recommendation is suspect for reasons described above. If the plan is to plant monocultures that facilitate enemies in nutrient poor soils, then species with functional traits associated with resource conservation should be planted. If the plan is to plant mixtures of species or supplemental planting into natural secondary succession both of which limit enemies, then it is far from clear that species with functional traits associated with resource conservation should be preferred.

Minor comments follow:
Why are there so many missing entries in Extended Data Tables 1 and 2? From the table captions, I understand the entries correspond to an exhaustive list of the possible changes in a combination of Pearson correlation coefficients and changes in mean square error from a second analysis. Should all missing entries be question marks? The table captions should explain why entries are missing.
Line 129 – Avoid "extra tropical". The word extratropical means outside the Tropics. Plus, it is not extra data. The rebuttal letter indicates it is a newly added core data set.
Lines 146 and 147 – Why refer to some trait-growth relationships as "inconsistent" and others as "contradictory" in Extended Data Table 2. I see positive and negative and mostly very weak relationships (and lots of missing entries) for trait-growth relationships identified as "inconsistent" and identified as "contradictory". What distinguishes them?
Lines 377 to 407 = Many criteria used to select species and sites are described, and the descriptions seem incomplete. The authors could (should?) provide a table of all excluded species and provenances and the reasons for excluding them and a second table of all excluded sites and the reasons for excluding them.
Lines 409 to 423 – See previous comment.
Lines 425 to 437 – See two previous comments.
Line 606 – What does "predictive linear models with iterations" mean?

Referee #2

(Remarks to the Author)
Dear authors.

I have read the new version of the manuscript. It reads nicely and, from my point of view, all suggestions and concerns have been addressed. I have no reservations in recommending it for publication.

If the authors are willing to make a small additional effort, I bring two suggestions for the abstract. Please feel free to use it or not:

Line 77:Just to avoid repeating the word species here, "We recommend planting acquisitive tree species in areas where they can realize their fast-growing potential"

Line 79: In other regions, where environmental stress dominates, conservative....

Referee #3

(Remarks to the Author)
Having read the revised version, I still consider this to be a strong and important study. I restate what I mentioned about the original submission (I was Referee #3): "The findings are truly original, highly significant for the field of forest ecology, and of interest to practitioners in reforestation and afforestation. The authors find that species with highest potential growth rate (i.e., highest rate of photosynthesis) tend to be the relatively slower growers (i.e., in terms of height or biomass) in real-life practice of growing forest stands."

I studied the replies and additional analyses performed by the authors in response to the comments from all reviewers.

Overall, the revision is thorough, thoughtful and well conducted.

Asa a tropical forest ecologist, I am particularly pleased by the inclusion of new tropical data, and the discussion of these new results. Yet, I have two comments on the tropical data:

1. Not all studies included were conducted in common garden experiments (defined in L 359-360 of the ms as "In each common garden, characterised by homogenous conditions, several monospecific stands were installed by planting one different tree species by stand."). The Song et al 2023 study was conducted in a permanent sample plot in a natural forest in Panama; the van der Sande et al 2015 study was also conducted in natural forest. Clearly, studies that were not designed as a common garden experiment should be left out (there could be more of those; I did not check all papers).
2. It seems logical that the tropical data are included in as many main Figures as possible (taking limitations due to study setup into account of course). Now they are not in Fig 1 and Table 1, and I'm not sure why not. In any case, panel F in SI Fig S2 can be added to Fig 4: this would directly illustrate the larger similarity between fast and slow species in more productive sites, and helps the discussion.

I am pleased with the answers to my main comments: I value the additional analyses of climatic ranges (my comment #3) and the quantitative assessment of intra- vs inter-specific trait variation (#2). My other comments are also addressed satisfactorily.

As for the comments of other reviewers: my overall view is that these have also been addressed well, and -- while I'm not a user of response ratios -- I concur with the authors that these are often used in ecology and that they seem appropriate in this study. I value the efforts of the authors to calculate, derive and discuss the Z-scores as an alternative, but given explained limitations, I support their choice to continue using response ratios.

Some minor comments:
Abstract and intro: I think it would be nice to mention the term paradigm in here, to summarize (abstract) and prepare (intro) the shift in paradigm suggested from the results in Fig 3.
L 105-106: I like the new setup of the introduction, but would suggest to already specify the knowledge gap here
L 209-2012: complex and long sentence. Also, "other" in L210 is rather vague; would be better to specify.
L 359-360: "one different tree species" is unclear and a bit odd. Not sure what is meant here

Version 2:

Reviewer comments:

Referee #3

(Remarks to the Author)
In my view, the authors produced precise, thorough and extensive answers to the comments of all reviewers, and have adequately and convincingly addressed these. I am happy with a more central inclusion of the tropical data in the main manuscript (I was reviewer #3), and like the new way of presenting results by latitudinal range (rather than a 3-letter abbreviation of the dataset). I also value -- and am convinced by -- the additional analyses done in response to comments 66 and 67 (reviewer #1).

With regard to comment 65 (from reviewer #1, on response ratios vs log growth ratios), I have reflected on this comment and the authors' responses. I think reviewer #1 was correct in stating that the ratios used are not response ratios as employed in meta-analysis, i.e. they do not standardize the response of tree growth to some treatment by their growth under control conditions. This was also not the purpose for which the authors calculated the ratio. The purpose was to standardize species-level growth rates by site productivity, and thus allow comparisons across sites that are (to some/a high degree) independent of differences in environmental conditions across sites. I think naming the used ratios now as log growth ratios avoids the confusion that -- in my interpretation -- was the basis of the comment.

EDITOR:

Your manuscript, "Generalised slow growth of fast growing tree species", has now been seen by 3 referees, whose comments are attached below. While they find your work of potential interest, as do we, they have raised important concerns that in our view need to be addressed before we can consider publication in Nature.

=> Authors' answer:

We appreciate the feedback and we took into account all the comments. A description of the modifications we have done is detailed below.

SOURCE DATA (GRAPHS): to provide, in spreadsheet form, the data underlying the graphical representations used in figures. In the case of all experiments presenting data from animal models, this is a requirement and is not optional. This is in addition to our well-established data-deposition policy for specific types of experiments and large datasets. Online readers of the manuscript will be able to access the graphical source data directly from the figure legend. Spreadsheets must be submitted in .xls, .xlsx or .csv formats. One file per figure is permitted. If there is a multi-panelled figure, the source data for each panel should be clearly labeled in the file; alternatively the source data for a figure can be included in multiple, clearly labeled sheets within an Excel file. File sizes of up to 30 MB are permitted, but it is expected that the vast majority of graphical source data files will be considerably smaller than this. When submitting these files with your manuscript, you should select the "Source Data" file type and use the title field in the file description tab to indicate the figure(s) to which the source data pertain.

=> We provided five xlsx files (one per figure/table), containing the data used for Figures 1-4 and Table 1.

Referee #1

Comment 01:

The authors demonstrate that relative growth rates of trees planted in monocultures differ from relative growth rates predicted from plant traits. As one example, species with large maximum photosynthetic rates are predicted to grow faster than species with smaller maximum photosynthetic rates; however, the authors demonstrate the opposite outcome when trees are planted in monocultures. This has important policy implications because trees are increasingly planted to sequester carbon, with a focus on species expected to grow rapidly including species with large maximum photosynthetic rates. The authors find similar unexpected results for other plant traits. The authors conclude that species expected to grow fast often do poorly when planted in monoculture and recommend that tree planting should be informed by local knowledge of environmental conditions and species-specific environmental requirements of tree species to be planted.

The analyses hold several surprises. The authors analyze three different data sets with slightly different methods. I will use their EAN data set as an example. Equation 1 (manuscript line 618) defines "site productivity" as the mean of species-specific mean growth rates for each species. Equation 2 (line 654) then defines relative growth rates as the logarithm of the "absolute value of species mean growth rate" divided by "site productivity". A first question is why "absolute value"? Are there species with negative mean growth rates? What are the implications of making negative mean growth rates into positive mean growth rates by taking absolute values? This seems problematic.

=> Authors' answer: There was no transformation of growth data using the mathematical absolute function: "absolute" was a qualitative description of the original values as compared with the "relative values" (after standardisation). In practice, the original values were used directly, meaning that there were only positive values (after data quality filters; see lines 418-420{*}). We apologize for being confusing. The text now reads as follows (lines 591-593): "*To remove the prominent influence of site productivity and hence to enable comparisons among species across all sites, we transformed the original values of tree species growth (cm yr-1) to relative values of growth.*".

{*} line numbers refer to the numbering of the file with tracked changes.

Comment 02:

I will set the "absolute value" problem aside for a moment to consider Equation 2. Since "site productivity" is the mean growth rate with species weighted equally, Equation 2 amounts to a ratio of species-specific growth rates to site-specific mean growth rates (were it not for that absolute value). Ratios often have problematic distributions and that appears to be the case here. Equation 2 includes a log transformation of the ratio. The log transformation is insufficient, however, so extremely small values are truncated to -2 (line 659). This also seems problematic.

I recommend that the authors consider standardizing growth rates to become standard normal deviates by subtracting the mean growth rate and dividing by the standard deviation of growth rates for each site. This will place all sites on the same scale with mean 0 and standard deviation 1.

=> The equation 2 (of the former version of the manuscript) is known as the "response ratio", or the "log response ratio" (hereafter referred to as "RR-score"). This metric is widely used in ecology (Gurevitch et al., 2018). This metric is robust and its use is particularly relevant in studies for which the intra-site variance is not known (Hedges et al., 1999), which is a common problem in ecology (Gurevitch & Hedges, 1999). On the other hand, we do recognize that the Z-score (that is the metric recommended in this comment C02) is also a good standardisation method, known for decades (Noy-Meir et al., 1975). We consequently tried to implement the Z-score metric in our study.

After having coded new R scripts, we had a look at the results. Here is an example:

	A	B	C	D	E	F	G	H	I	J
1	SUBSET AS AN EXAMPLE								Formula used in first version	Proposal of Reviewer R1
2									ln (sp / mean)	z-score = (sp - mean) / sd
3	Dataset	Site	Tree species	Species growth (Mg/ha)	Site growth (Mg/ha) - mean	Site growth (Mg/ha) - SD)	Site growth (Mg/ha) - min)	Site growth (Mg/ha) - max)	Relative growth	z-score
4	SBD	GBL_1	Castanopsis kawakamii	312.0	221.0	60.8	187.0	312.0	0.34	1.50
5	SBD	GBL_1	Cunninghamia lanceolata	188.0	221.0	60.8	187.0	312.0	-0.16	-0.54
6	SBD	GBL_1	Fokienia hodginsii	187.0	221.0	60.8	187.0	312.0	-0.17	-0.56
7	SBD	GBL_1	Ormosia xylocarpa	197.0	221.0	60.8	187.0	312.0	-0.11	-0.39
8	SBD	GBL_105	Fagus sylvatica	234.4	267.3	29.6	234.4	292.0	-0.13	-1.11
9	SBD	GBL_105	Picea abies	292.0	267.3	29.6	234.4	292.0	0.09	0.83
10	SBD	GBL_105	Pseudotsuga menziesii	275.4	267.3	29.6	234.4	292.0	0.03	0.27
11	SBD	GBL_117	Fagus sylvatica	152.5	153.2	0.9	152.5	153.9	0.00	-0.71
12	SBD	GBL_117	Picea abies	153.9	153.2	0.9	152.5	153.9	0.00	0.71
13	SBD	GBL_128	Acer saccharum	170.1	172.0	2.6	170.1	173.8	-0.01	-0.71
14	SBD	GBL_128	Pinus resinosa	173.8	172.0	2.6	170.1	173.8	0.01	0.71

While most values looked ok, some others were surprising. Indeed, despite very similar growth values (column D), the species of the sites "GBL_117" and "GBL_128" (lines 11-12 and 13-14; column B) displayed surprisingly high Z-score values (column J). We first believed that we made an error in our scripts but checks were OK. More importantly, we observed that (i) these strange values existed only for sites with two tree species and (ii) the Z-scores were always +0.71 and -0.71. After having validated our scripts, we tried to understand if there was a mathematical explanation for this Z-score behaviour and, indeed, there is. In the Box 1 (see below), there is a mathematical demonstration showing why the Z-score approach always produces values equal to +0.71 and -0.71 (in practice equal to $\pm\sqrt{2}/2$). This pitfall prevents anyone from using the Z-score metric for populations with only two individuals (as in our study).

Despite this drawback, we tried to circumvent the obstacle by developing a proxy of the Z-scores. To do so, we first plotted RR-scores and Z-scores for sites with numerous tree species (here $N \geq 15$). The two metrics were well correlated to each other ($r = +0.86$). We fitted a polynomial regression between the two metrics (see below Figure R1A). Then, we applied the following approach for the whole dataset (Figure R1B): (i) for sites with at least three tree species, the Z-score values were calculated as usual, and (ii) for sites with only two tree species, the Z-score values were estimated based on the RR-score values (using the fitted regression). The panels R1C and R1D present how the problematic values (in red) were corrected (in green).

When evaluating the results, it first appeared that, as for the RR-score, the distribution was a bit skewed and consequently needed a truncation (here at +3; see below Figure R2). Another problem was the final distribution of data: Z-score values showed to be (slightly) less normal than RR-score values (i.e. higher D-value and lower W-value for the Lilliefors normality test and the Shapiro-Wilk normality test, respectively; Figure R3). These tests are relevant for our dataset (Razali & Wah, 2011). This difference is also visually observable using QQ-plots (Figure R4), particularly for the left part of data distribution for which Z-score values were less well-transformed than RR-score values (see notably wood density, leaf P, SLA and SRL).

Based on these investigations, we concluded that the RR-score metric was more suitable to our dataset than the Z-score metric because: (i) both metrics are well-known robust standardisation methods, (ii) these metrics are well correlated to each other, (iii) RR-score is directly usable for all our sites (included sites with only two tree species), (iv) both metrics needed a one-sided truncation, and (v) final values of the RR-score were better distributed than those of the Z-score.
In practice, we consequently used the response ratio method for standardising our data. But we modified the text as follows (lines 594-598):
"*The two metrics were highly correlated to each other (r = +0.86), but the response ratio metric was more suitable for our data because (i) the z-score cannot be calculated for sites with only two tree species (Appendix 4) and (ii) the values transformed as response ratios showed better distributions as evaluated by normality tests (Lilliefors and Shapiro-Wilk tests) and QQ plots.*"

We should mention that we sincerely believe that the Z-score is a good metric, and we honestly tried to implement it in our study. It turned out that it was not the best option in our case, explaining why we finally changed our mind and returned to the RR-score approach. Anyway, these investigations enabled us to strengthen our methods by exploring an alternative approach, and we do thank the reviewer for having raised this point.

References cited:
Gurevitch & Hedges (1999). Statistical issues in ecological meta-analyses. Ecology, 80(4), 1142-1149.
Gurevitch et al. (2018). Meta-analysis and the science of research synthesis. Nature, 555(7695), 175-182.
Hedges et al. (1999). The meta-analysis of response ratios in experimental ecology. Ecology, 80(4), 1150-1156.
Noy-Meir et al. (1975). Data transformations in ecological ordination: II. On the meaning of data standardization. Journal of Ecology, 63(3), 779-800.
Razali & Wah (2011). Power comparisons of shapiro-wilk, kolmogorov-smirnov, lilliefors and anderson-darling tests. Journal of Statistical Modeling and Analytics, 2(1), 21-33.

Box 1: Mathematical demonstration

Step 1

The standard deviation (**SD**) of a sampled population is:

$$SD = \sqrt{\frac{\sum_1^n (x-m)^2}{(n-1)}}$$

With **m** as the mean value and **n** as the population size.

Let's consider a population of 2 values (**a**,**b**).
Hence here n=2 and (n-1)=1
So, for a population of 2 values, the standard deviation is:

$$SD = \sqrt{\sum_1^n (x-m)^2}$$

- $SD = \sqrt{(a-m)^2 + (b-m)^2}$
- $SD = \sqrt{\left(a - \left(\frac{a+b}{2}\right)\right)^2 + (b - \left(\frac{a+b}{2}\right))^2}$
- $SD = \sqrt{\left(\frac{2a}{2} - \left(\frac{a+b}{2}\right)\right)^2 + (\frac{2b}{2} - \left(\frac{a+b}{2}\right))^2}$
- $SD = \sqrt{\left(\frac{a-b}{2}\right)^2 + \left(\frac{b-a}{2}\right)^2}$

Step 2

Let's consider the absolute difference (**d**) between **a** and **b**:

$$d = |a-b| = |b-a|$$

Then:

- $SD = \sqrt{\left(\frac{d}{2}\right)^2 + \left(\frac{d}{2}\right)^2}$
- $SD = \sqrt{\frac{d^2}{4} + \frac{d^2}{4}}$
- $SD = \sqrt{\frac{2d^2}{4}}$
- $SD = \sqrt{\frac{d^2}{2}}$
- $SD = \frac{d}{\sqrt{2}}$

Step 3

Now, let's consider the **z-score** of the **a value** of the population (a,b):

$$Z_a = \frac{(a-m)}{SD}$$

- $Z_a = \frac{\left(a - \left(\frac{a+b}{2}\right)\right)}{SD}$
- $Z_a = \frac{\left(\frac{2a}{2} - \left(\frac{a+b}{2}\right)\right)}{SD}$
- $Z_a = \frac{\left(\frac{a-b}{2}\right)}{SD}$
- $Z_a = \frac{\left(\frac{a-b}{2}\right)}{\left(\frac{d}{\sqrt{2}}\right)}$
- $Z_a = \left(\frac{a-b}{2}\right) \times \left(\frac{\sqrt{2}}{d}\right)$
- $Z_a = \left(\frac{|d|}{2}\right) \times \left(\frac{\sqrt{2}}{d}\right)$
- $|Z_a| = \frac{\sqrt{2}}{2}$
- $|Z_a| = 0.707$

Figure R1 – Correction of Z-score values

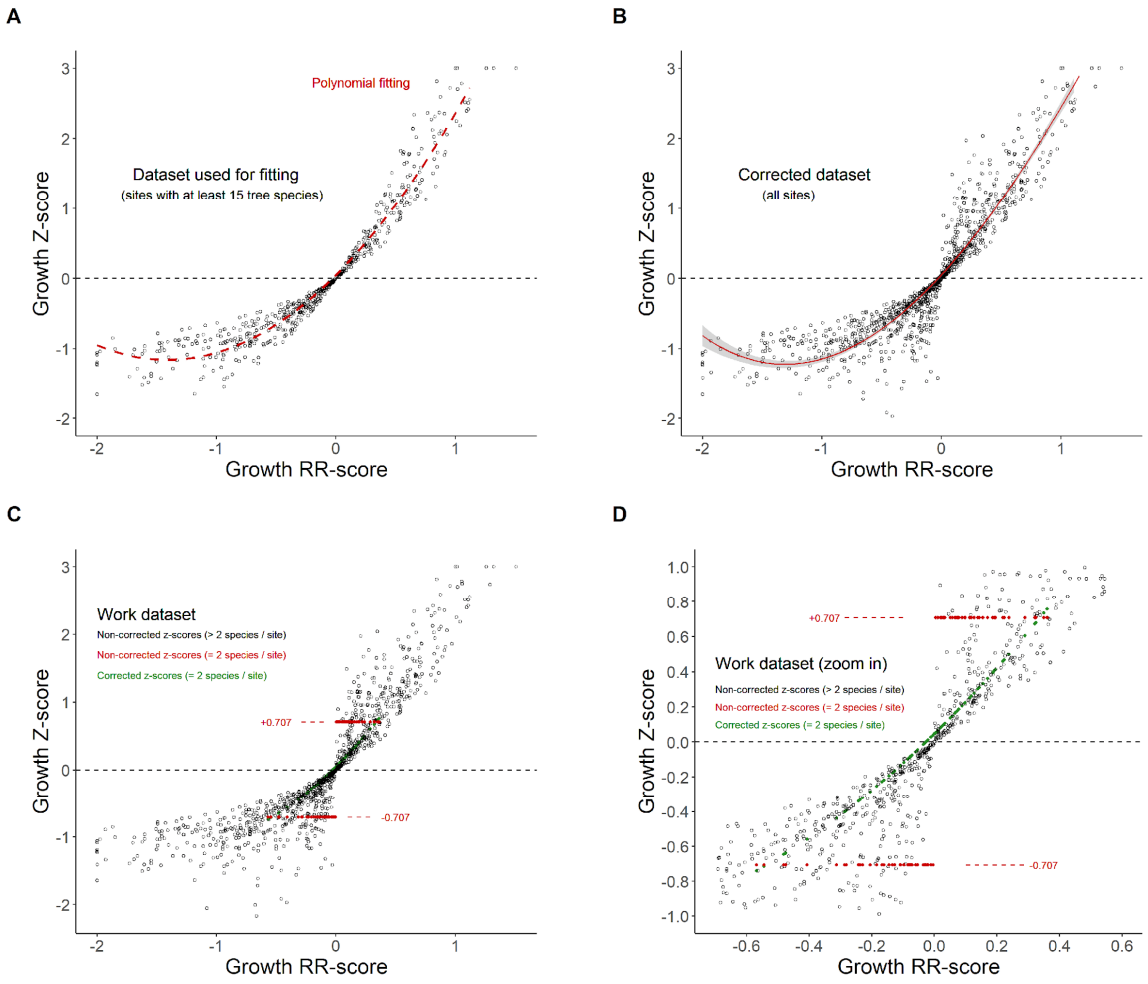


Figure R2 – Skewed distribution of Z-score values and its correction

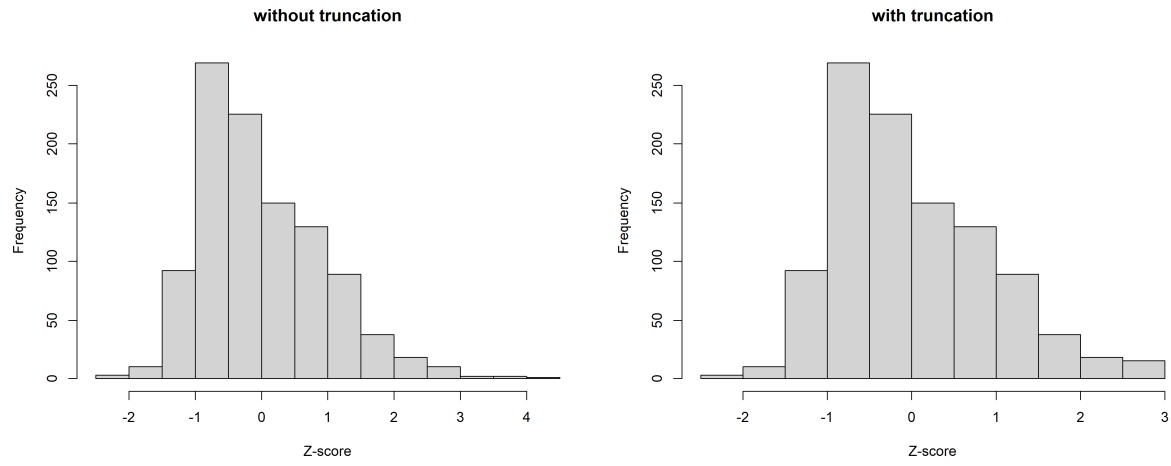


Figure R3 – Tests of normality on data distributions based on Z-score values or RR-score-values

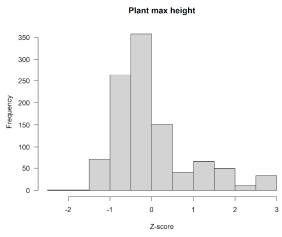
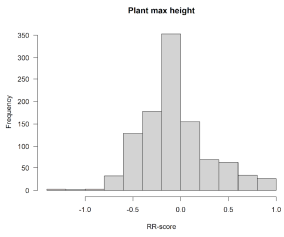
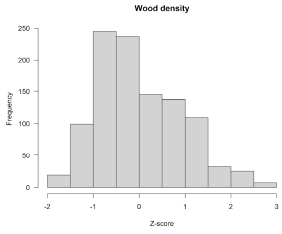
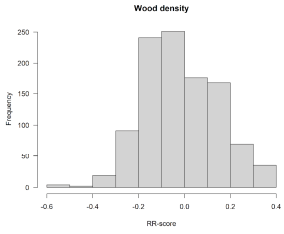
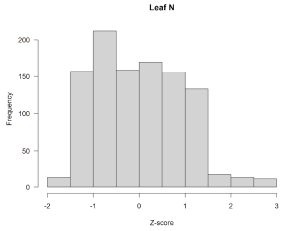
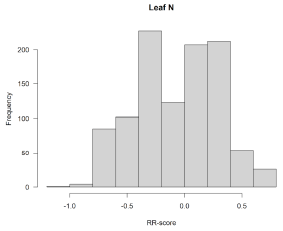
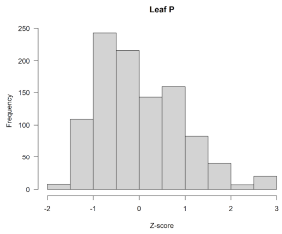
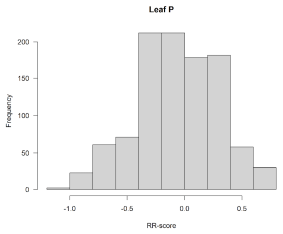
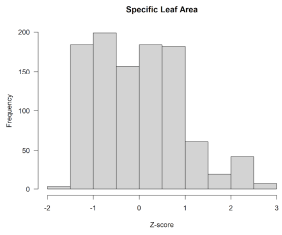
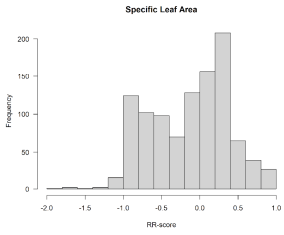
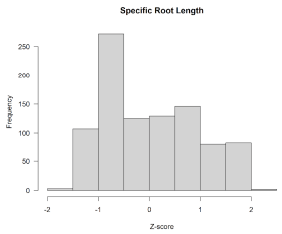
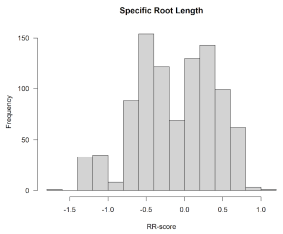
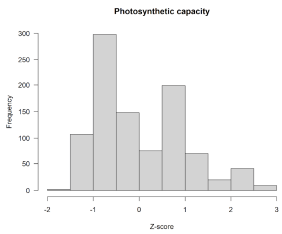
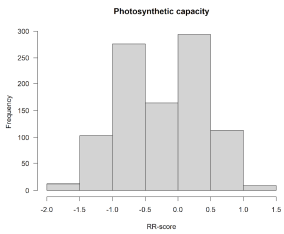
Distributions		**Normality test**
Z-score	RR-score	SW & Lilliefors tests
		Plant max height Z-score: D = 0.166 RR-Score: D = 0.097 Z-score: W = 0.882 RR-Score: W = 0.971
		Wood density Z-score: D = 0.084 RR-Score: D = 0.058 Z-score: W = 0.968 RR-Score: W = 0.990
		Leaf N Z-score: D = 0.125 RR-Score: D = 0.091 Z-score: W = 0.964 RR-Score: W = 0.974
		Leaf P Z-score: D = 0.086 RR-Score: D = 0.052 Z-score: W = 0.962 RR-Score: W = 0.993
		SLA Z-score: D = 0.094 RR-Score: D = 0.090 Z-score: W = 0.948 RR-Score: W = 0.961
		SRL Z-score: D = 0.124 RR-Score: D = 0.069 Z-score: W = 0.945 RR-Score: W = 0.968
		Amass Z-score: D = 0.139 RR-Score: D = 0.105 Z-score: W = 0.923 RR-Score: W = 0.967

Figure R4 – QQ-plots of data distributions based on Z-score values or RR-score-values

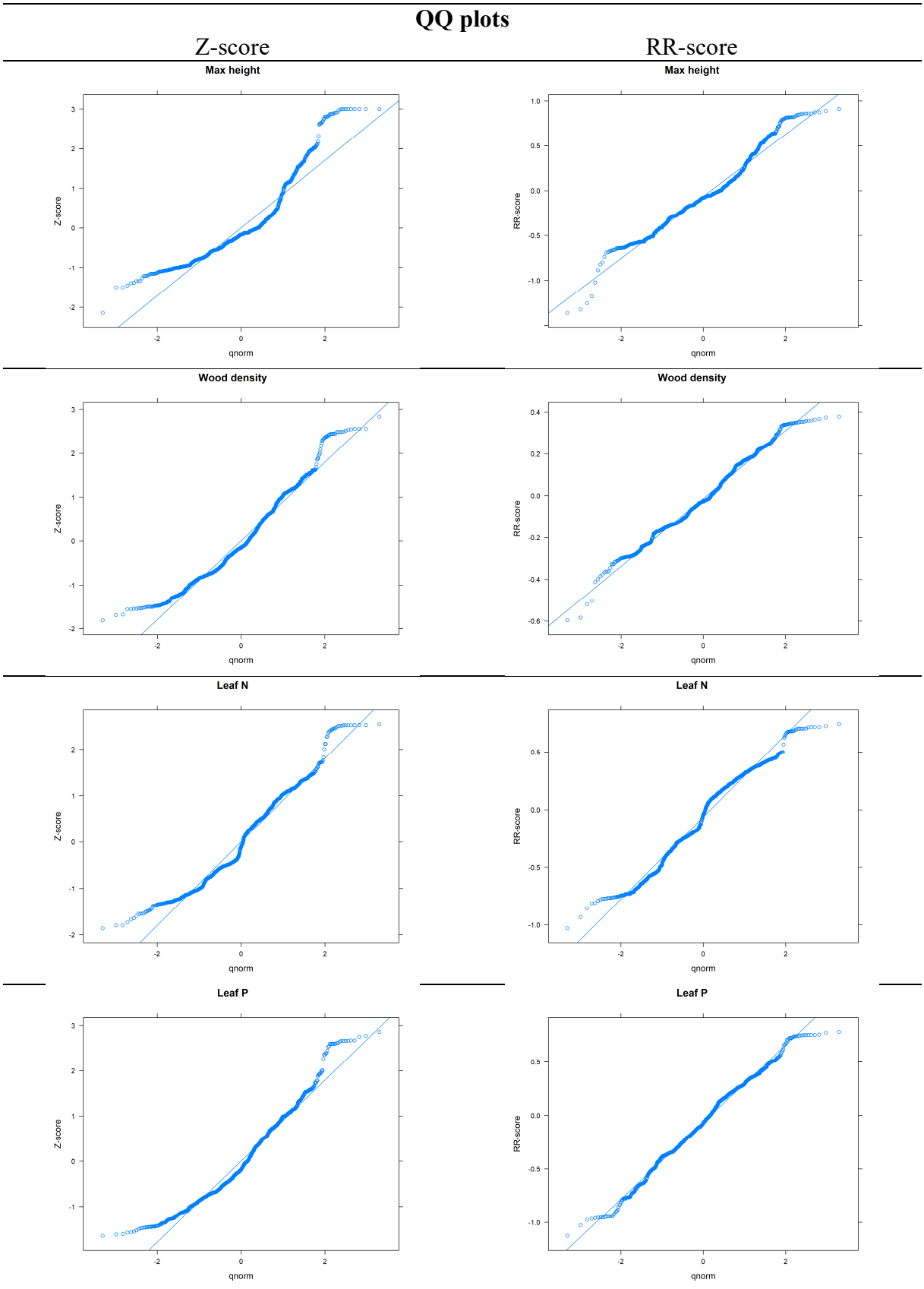
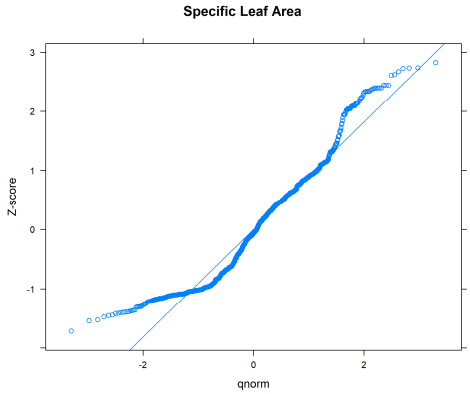
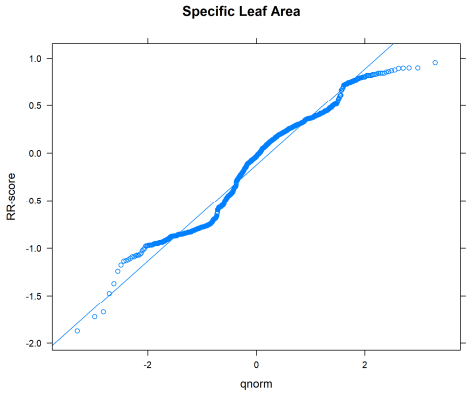
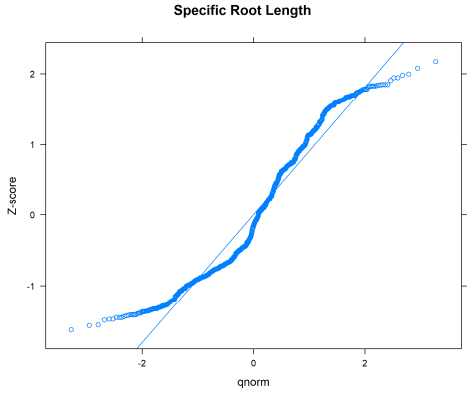
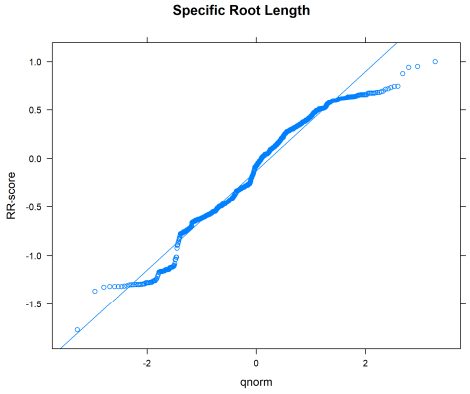
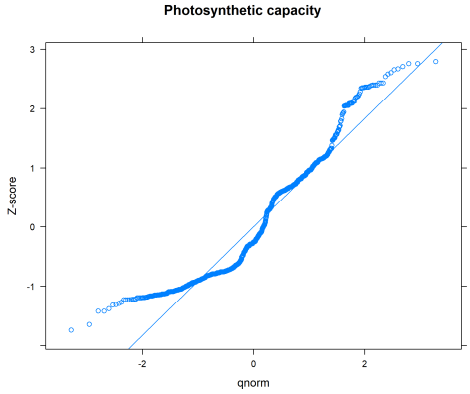
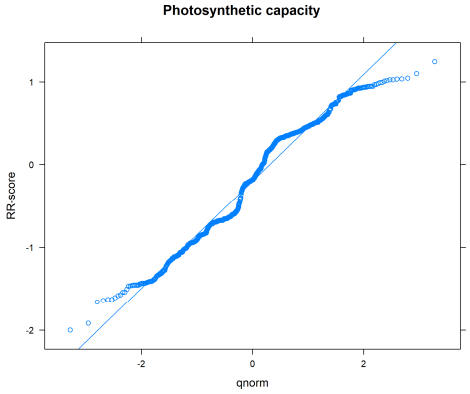


Figure R4 – QQ-plots (continued)

QQ plots

Z-score	RR-score
	
	
	

Comment 03:

Another problem occurs when the authors test for site-trait interactions in the EAN data set. The authors evaluate a linear model between "site productivity" and the Pearson correlation coefficient between species-specific mean growth rates and trait values (Equation 3, line 700; Figure 3). I have never seen an analysis like this possibly because of a problem discussed by the authors. Their Pearson correlation coefficients apparently vary with sample size across sites, where sample size is the number of species with growth and trait data at each site. The authors solve this problem by constraining this sample size, which also seems problematic.

=> We agree that this approach to present results about interactive effects are not very common (see however recent examples in ecology: Figs 1B & 1C in Weemstra et al. (2021); Fig. 3 in Davila-Orozco et al. (2022); Fig. 3B in Feng et al. (2024)). Constraining the sites used for these analyses based on their data size was indeed needed to avoid biasing the results. Therefore, we believe that this approach is reliable. To go beyond any unsupported claim, we took into account the next comment about linear mixed effects models (cf. comment 04). Indeed, in the revised version of the manuscript, interactive effects were at first assessed using mixed models (see Table 1), as proposed by the reviewer. We think that the two approaches are complementary to each other: the mixed models enable to test the interactions in a reliable way, whereas the correlation approach illustrates them. For instance, the mixed models indicated that specific leaf area (SLA) has effects on growth that are both fixed effects and interactive effects (Table 1), and this result is consistent with Figure 3B that shows how correlation values go from negative values to positive values.

In practice, now the revised manuscript clearly states that the assessment of the interactive effects was based on mixed models, while these effects were graphically presented using the correlation approach but without pretending that this approach could be used as a statistical test (lines 648-653).

References cited:

Dávila-Orozco et al. (2022). Multisite species-level roadkill across years reveals challenges and opportunities for monitoring and mitigation. Austral Ecology, 47(2), 341-352.

Feng et al. (2024). Geologically younger ecosystems are more dependent on soil biodiversity for supporting function. Nature Communications, 15(1), 4141.

Weemstra et al. (2021). Tree growth increases through opposing above-ground and below-ground resource strategies. Journal of Ecology, 109(10), 3502-3512.

Comment 04:

The authors might consider a test for site-trait interactions using linear mixed effects models as follows: Gi,j = traiti * site productivtyj + (1 + traiti | sitej).

Here Gi,j is the growth rate of species i at site j. The model includes a random intercept for site and a random slope for trait values for each site.

=> We agree that linear mixed effects models are a good tool to analyse a dataset such as ours. Following reviewer's suggestion and a recent study about the same topic (Qin et al., 2024), we used models as follows (equation 3):

$$G_{i,j} = trait_i * productivity_j + (1|species) + (1|site)$$

Because the response variable (G) was transformed in relative values (in order to remove the productivity effect), the model was modified by keeping only the trait-site interaction, as follows:

$$G_{i,j} = trait_i + (trait_i : productivity_j) + (1|species) + (1|site)$$

The results are presented in Table 1.

Reference cited:

Qin, Y., Wang, C., Zhou, T., Fei, Y., Xu, Y., Qiao, X., & Jiang, M. (2024). Interactions between leaf traits and environmental factors help explain the growth of evergreen and deciduous species in a subtropical forest. Forest Ecology and Management, 560, 121854.

Comment 05:
The authors used two different methods to evaluate site-trait interactions for their SBD data set and for sites in their TDN data set with just three to eight species. The different methods are categorical and continuous. When continuous data are available, categorical analyses provide less information and should be avoided. I will focus on the analysis of continuous data.

=> We followed the recommendation of the reviewer and removed the categorical analyses. In practice, the former Figure 2 was deleted from the revised version. Continuous analyses were kept in priority (see Figures 1, 2, and 3).

Comment 06:
There is another problem at Equations 4 and 5 (lines 741 and 742). Equation 6 defines a site suitability index as the product of a climate index and a soil carbon:nitrogen ratio or C:N ratio index. Equations 4 and 5 define the soil C:N and climate indices using a minimum function. For example, all soil C:N ratios smaller than 12 are defined to equal 1 while C:N ratios larger than 12 are defined to be 12 divided by the C:N ratio. The authors argue that C:N ratios of 12 separate unfavorable (>12) and favorable (<12) sites. This index combines categorical and continuous values. The index combines a categorical definition of favorable (i.e., C:N < 12 is set to 1) with a continuous definition of unfavorable (i.e., C:N > 12 equals 12 divided by the C:N ratio). This is the first time I have encountered a variable that combines categorical and continuous components. This index is a problem.

=> We agree that this approach is a bit complicated. To simplify data interpretation, we calculated an index of site productivity in all cases following the same rationale. For young trees, it is the mean growth rate of the sites (in cm/yr, as in the former version of the manuscript; Fig. 3C). For mature stands, it is also the mean growth rate of the sites, but in Mg/ha/yr (Fig. 3D). This metric was not used in the former version and, following a recommendation of the reviewer 3 (see comment 43), is calculated as the ratio between stand biomass (Mg/ha) and stand age (yrs), and is consequently the mean rate of net biomass accumulation.

Referee #2:
Comment 07:
The manuscript "Generalized slow growth of fast-growing trees" brings a critical view of the performance of so-called slow-growing and fast-growing species by analyzing the association of growth rate and traits in a broad geographical gradient. Associating growth rate and plant trait is not new per se, but using the traits to classify species in terms of growth and evaluating how they perform under various environmental conditions is rather new.

=> Authors' answer:
We thank the reviewer for pointing out the novelty of this study.

Comment 08:
The study is based on a comprehensive collection of three datasets on common garden plots, namely: European Atlantic Network (ENA), TreeDivNet (TDN), and Stand Biomass Dataset (SBD). Out of these three, the EAN and TDN have data on tree height growth and the SBD is mainly a picture of the biomass, with no successive surveys. These plots combine species of angiosperms (99 species) and conifers (53 species). Still, the core of the actual growth data is located in North America and Europe.
The authors employed quite a few approaches for predictor selection based on traits and climate conditions, which is fine. And then, the core of the analyses are overall standard and quite straightforward.

=> We are thankful for this positive feedback.

Comment 09:
Based on all this significant dataset and I would say appropriate analyses, the authors find some intriguing patterns related to the association between the traits and growth rate. Indeed, these associations change along gradients of climate, soil characteristics, and stand productivity. In the end, an alternative conceptual model is presented.

=> Although the reviewer found that the first analyses were appropriate, we tried all the same to improve them as much as possible, following the reviewers' comments (see comments 02-06 and comments below).

Comment 10:
Although the results are really interesting and potentially disruptive, I have some reservations regarding the dataset, and how the analyses under undertaken. Basically, I wonder how the results may be biased by the limited geographical representativeness in the tropics and by the potential influence of a phylogenetic effect in the data.
Regarding the distribution of the dataset, it is mainly located in temperate areas in North America and Europe. Although the authors correctly point out the role of tropical forests in the carbon cycle, the datasets don't really represent well them. This adds an important bias for striving for global conclusions because tropical and extra-tropical forests face different limitations in terms of soil fertility and climate. Soil fertility is significantly lower in the tropics, while tropical forests may struggle in warmer conditions. Although the dataset from SBD comprises some tropical sites, the data seems to be unbalanced towards cold-limited sites.

=> There are two important points in this comment: (i) the possible influence of phylogenetic effect in the data, and (ii) the representativeness of tropical data.

About the possible phylogenetic effect, we added new data and new analyses in our study. In practice, we built a phylogenetic tree for the 23 species of the EAN dataset (lines 782-798{*}). We did so because this network has a factorial design (i.e. all the same tree species in all the studied sites), which simplifies data analysis. Based on this phylogenetic tree, we calculated the phylogenetic distance of all possible pairs of tree species, and then we tested to which extent this distance might explain tree growth and trait values. We found that the phylogenetic distance had an effect for tree species that were close to each other in the phylogenetic tree (i.e. distance < approx 100 Myr; Supplementary Figure S10), but this effect was weak and explained only a very small proportion of the variance (2-10%). Based on these results, we concluded that there was a significant effect of the phylogeny but that was a minor effect. These results are presented and discussed in the revised manuscript (lines 798-804).

We agree that our previous manuscript presented results that were more representative of Mediterranean forests, temperate forests and boreal forests than of tropical forests. To address this comment, we proceeded in three steps:
First, we complemented our dataset with new tropical data. Because tropical data that comply to the selection criteria of our former dataset are scarce, we defined more flexible margins of acceptance for cases in tropical forests, enabling us to build a fourth dataset (TED: tropical extra data). This dataset added new sites (see Supplementary Figure S4) and nearly one hundred new tree species.
In a second step, we made data analyses that were specific to tropical forests. With data from four young common gardens (including sufficient tree species), we tested if the correlation between growth and trait values depends on site productivity. We found that these tropical sites were fairly well in line with our results based on Mediterranean-temperate-boreal sites (Figure 2): when productivity increases, the growth-trait relationships tend to fade. We investigated with all our tropical data if there were general growth-trait relationships and found that, except for wood density and to a lower extent leaf nitrogen, there were no significant relationships (Supplementary Figure S2A;E). As a whole in tropical sites, we did not observe any significant difference of growth rate between acquisitive{**} species and conservative{**} species (Supplementary Figure S2F). We interpreted this last result as the consequence of the high rate of production in the tropics (see Supplementary Figure S3, based on global NPP MODIS data).

Finally, we revised the manuscript to take into account these results. In brief, we concluded that tropical forests follow the same pattern as forests of higher latitudes: growth-trait relationships exist, are strong under unfavourable conditions, but weaken when conditions improve. Because tropical climates are more favourable to plant growth, it explains why the influence of functional traits on growth was weak (see discussion lines 248-258). This result also has practical consequences on the general recommendations we gave (lines 277-278).

{*} line numbers refer to the numbering of the file with tracked changes.
{**} following the comment 42 (reviewer #3), we renamed the tree species groups: "slow-growing$_{[trait-based]}$ species" are now "conservative species", and "fast-growing$_{[trait-based]}$ species" are now "acquisitive species".

Comment 11:
In addition, it is not clear to me how the growth rate was calculated if it has only one measurement of biomass per site. Maybe that is something that needs additional clarification in the Material and Methods.

=> This information was made clearer in the Material and Methods section (lines 400-402).

Comment 12:
Finally, one must be careful while mixing angiosperms and conifer species for the reasons presented before. From my experience, one should analyze them separately because the results are too influenced by the composition of the species.

=> We agree that angiosperms and gymnosperms exhibit clear functional differences that may strongly influence the results. To test this possible effect, we followed the reviewer's suggestion and analysed the growth-trait relationships for each spermatophyte group separately (Supplementary Figure S11A-E). We found that these relationships were often still significant within a given spermatophyte group. In addition, we found that the acquisitive-conservative difference was still significant for angiosperms (Supplementary Figure S11F; not enough data for gymnosperms). On the other hand, we observed that some growth-trait relationships were no longer significant, notably for the gymnosperm group (Supplementary Figure S11E). We interpreted this result as the consequence of narrower ranges of trait values for gymnosperms (lines 811-817). This explanation may also apply to leaf nitrogen (Supplementary Figure S11C) as the data dispersion shows that the overlap between angiosperm data and gymnosperm data is small, suggesting that -for leaf N- the general observed effect is indeed induced by the spermatophyte dichotomy. Conversely, for traits with bigger overlapping, there was no clear spermatophyte effect (e.g. Supplementary Figure S11B,D). We discussed this effect in the revised version of the manuscript (lines 817-830).

Comment 13:
In Summary, this is a valuable contribution to a broad audience interested in forest growth, carbon gain, and climate change mitigation. However, there are still important gaps that need to be checked and re-evaluated.

=> Thanks for the rather positive summary. We did our best to fill the gaps.

Please see below some minor comments and suggestions: Comment 14:
L67: Sounds like a contradicting information. I understand your point here, but I would put it in another way. You can say that forest planting is supported by trait-based decisions, but field studies fail to detect it clearly. growth-trait relationships

=> This passage was rephrased following this way (lines 67-70).

Comment 15:
L79: I think you are overly careful with your statement, maybe just say "...species have a higher potential...".

=> The text was modified following this suggestion (line 83).

Comment 16:
L85: The potential to mitigate current rates

=> modification done (line 88).

Comment 17:
L93: Focusing on fast-growing trees is controversial if one convents the fact that there is a live-fast and die-young conundrum. So, at first, having a fast-growing community of trees may accelerate carbon gain, but this carbon will be stored for a very limited time because these trees will not reach old age. The exceptions may only be the long-lived pioneer species.

=> We agree that the turnover of the biomass carbon pool is important, and we seriously took into account this comment. To do so, we collected data about species longevity (lines 455-456). We found data about approximately one hundred of our studied species (fairly distributed between acquisitive species and conservative species). We found that acquisitive species (i.e. supposedly fast-growing species) have on average significantly shorter lifespan than conservative species (i.e. supposedly slow-growing species; Extended Data Table 3). It means that, acquisitive species indeed die young, which is in line with the cited conundrum (Salguero-Gómez et al., 2016). But we observed that, in nature, conservative species actually grow faster and have longer theoretical lifespan than other species, implying that growth rate and longevity do not have conflicting effect on biomass accumulation, but instead cumulative effects. We modified the manuscript to mention this (lines 243-245) and we mentioned the need for further investigations (lines 245-248). Finally, we remain open to any suggestion about this topic.

Reference cited:
Salguero-Gómez et al. (2016). Fast–slow continuum and reproductive strategies structure plant life-history variation worldwide. Proceedings of the National Academy of Sciences, 113(1), 230-235.

Comment 18:
L116: Please, can you quickly bring this context to this part of the text?

=> The sentence was rephrased to bring more explicit information (lines 135-137)

Comment 19:
L122: I understand you are bringing the main message forefront. It is a nice strategy, no question, but, not sure it works where you are just defining what you call a fast-growing species.

=> In fact, it was not our intent to "spoil" the conclusion; we just wanted to make it clear what fast-growing species are. Anyway, because the tree species groups were renamed (following the comment 42), this passage was no longer needed and was removed.

Comment 20:
L135: we categorized

=> This passage was removed.

Comment 21:
L183: Not sure you can truly generalize it across such a broad geographical gradient.

=> We agree that this passage needed to be nuanced (see lines 219-220).

Comment 22:
L186: I believe there is one key thing that is not mentioned here in relation to field experiments, and that is the role of competition. Competition also increases under more favorable conditions. How would it affect or eventually bias your results?

=> Our study is an investigation of the growth-trait-environment interactions in tree species worldwide. This scope is already quite large and it did not seem possible to us to include other factors, such as

competition. Nevertheless, we fully agree that competition (and some other factors; see below) is important (Michalet et al., 2006). Therefore, we mentioned that the possible effects of competition should be considered too (see lines 245-248).

Reference cited:
Michalet et al. (2006). Do biotic interactions shape both sides of the humped-back model of species richness in plant communities? Ecology Letters, 9(7), 767-773.

Comment 23:
L195: However, based on 139 sites,

=> This passage was modified (lines 232-233).

Comment 24:
L209: I think that it is not really the tree species that drive more soil organic carbon, but it is more the interaction among site characteristics that does.

=> This passage was removed (see comment 53).

Comment 25:
L211: This is an overall beautiful study, and I think it deserves a better and more focused conclusion. There are some elements that just came out of the blue right in the last sentence. You can of course draw general conclusions but within the scope of your own study. Otherwise, you only shade the real advances of it.

=> We understand and agree. Indeed, those additional elements are not directly supported by the results of our study. On the other hand, we believe that it is crucial to not let interpret our study in a simplistic way: tree growth rate is an important criterion but is not the unique criterion to consider when thinking about forest management. That is why we believe that it is important to maintain some general guidelines in the conclusion. But, because we agree that the conclusions should stick to results as much as possible, we shortened the general guidelines (lines 264-274). We remain open to any suggestion about this redaction.

Comment 26:
L417: common gardens found across

=> Modification done (line 316).

Comment 27:
L464: Please, if the SBD does not provide growth data, it is a picture of the site at a given moment, how it is useful for this study?

=> Our initial design of data analysis was based on the rationale that the amount of standing biomass of mature forests provides at the same time information about growth (needed to accumulate this biomass) and about the durability of this carbon pool (growth-survival trade-off). But we agree that this initial rationale was confusing. Following a recommendation of the reviewer 3 (see comment 43), we changed our analysis approach and calculated a growth rate for mature forests. This metric is the ratio between standing biomass (Mg/ha) and forest age (yr), leading to a rate value (Mg/ha/yr). Taking into account that mortality rate was not known, this growth rate should be considered as the mean annual rate of biomass net growth. The methods section and all results were modified accordingly (see lines 434-442 for Methods).

Comment 28:
L467: After assessing the data quality,

=> Modification done (line 366).

Comment 29:
L484: Not sure about this statement. Biomass accumulation may imply a rate. You have the stand biomass only.

=> We agree that using standing biomass directly as a growth metric was confusing. This methodological aspect of the study was changed (see comment 27).

Comment 30:
L507: moribund trees?

=> The Collins Cobuild advanced learner's English dictionary defines "moribund" as follows: "*if you describe something as moribund, you mean that it is in a very bad condition*". Hence, it seems to us that this word is correct. Nevertheless, we concede that this word is not commonly used, and the cited dictionary even mentions that it is formal English. That is why we modified this passage to ease the reading (lines 422-429) and stay open to any suggestion.

Comment 31:
L524: I understand this dataset adds a lot in terms of species, sites, and environmental conditions. But it feels like you are making too many assumptions to use this data in this study. Therefore, the real role of it is not clear.

=> It is correct that the main advantage of the SBD dataset is to enable testing our hypothesis with many tree species present in sites worldwide. That is why this dataset is useful for this study. As mentioned above (see comments 27 and 29), we changed our design for analysing this data in order to present values that are net growth values.

Comment 32:
L605: I am not sure it is the case for temperature in the tropics. And also, this is not the right reference for this context, because it is not about decomposition, but about carbon gain.

=> This climatic index was also used to assess tree growth. We modified the text and added a relevant reference to be more explicit (line 533-536).

Comment 33:
L649: What were the second variables chosen by each approach?

=> We added this information (line 590).

Comment 34:
L678: I understand that defining these thresholds is tricky, but it is one of the main steps in this study. How the results would change if you shift these thresholds say 10% up and down? As you pointed out, this is an artificial division of the data.

=> We fully agree and this possible bias was tested in the former version of the manuscript. But we realise with this comment that it is worth mentioning here that a sensitivity analysis was performed. We added this information (lines 632-633).

Comment 35:
L681: This is a very important statement and in line with a previous comment of mine. Conifers and angiosperms have very different strategies, in terms of physiology, anatomy, morphology, and so on. I would treat them separately. Otherwise, you will be assuming a global pattern for all plants, while it is mainly driven by taxonomic issues.

=> It is correct. Please see our answer to the comment 12.

Comment 36:
Extended Data Table 3: Why wood density is higher in fast-growing trees? Is it because you are actually comparing groups composed mostly of conifers or angiosperms?

=> This was indeed a bit surprising at first glance. But it is logical since trait-based fast-growing species were defined based on functional traits that did not include wood density (i.e. mainly leaf Amax, to a lesser extent SLA and leaf N). However if we consider realised growth rate, wood density is lower in fast-growing trees (Extended data Fig. 3).
Analysing data per group of spermatophytes showed that the effect of wood density was significant even considering only angiosperms or only gymnosperms (Supplementary Figure S11 B). Similarly, the analysis of tropical data confirmed this growth-density relationship (Supplementary Fig. S2A), suggesting that this effect is robust. We added a mention in the manuscript to make this clearer (lines 150-151), but we stay open to all proposals for further text improvements.

Referee #3:

Comment 37:

Augusto and co-authors have collected and analysed an impressive dataset of tree growth in common garden experiments across continents, climate zones and forest types; a truly impressive dataset and an analysis at unprecedented scale. They used these data to evaluate whether tree species that are considered to have functional traits allowing fast growth actually also grow faster in a forest stands, compared to species that have traits associated with slow growth. The authors carefully account for differences in productivity and species sets across sites, and take methodological differences across the three datasets into account. The findings are truly original, highly significant for the field of forest ecology, and of interest to practitioners in reforestation and afforestation. The authors find that species with highest potential growth rate (i.e., highest rate of photosynthesis) tend to be the relatively slower growers (i.e., in terms of height or biomass) in real-life practice of growing forest stands. The study also shows that this association seems to be strongest under unfavourable climate or poor soils. The authors explain results by suggesting that species with functional traits that would support fast growth, only can realize fast growth under optimal conditions.

=> Authors' answer: we thank the reviewer for this positive feedback.

Main comments

Comment 38:

1. Species classification. While in the text, authors are careful to explain that the classification of fast and slow growing trees is based on functional traits and not on growth data, it still is strange (and likely incorrect) to call these fast- and slow-growing species, even if preceded by the term "supposedly" or somewhat more hidden as in x-axis label of Fig 5. I stress this because I wonder whether for the 150 spp included in the study the ranking of actual growth rate (in terms of height, biomass or diameter) corresponds to that inferred from functional trait data. Without such a check it would be hard to even call species with high Amax (or SLA or leafN) "supposedly fast growing". If the authors are not able to establish such an association for a large share of their study species, the link between functional traits and (potential) growth is not proven and it would be hard to maintain terms "fast and slow growers".

=> This is indeed a crucial point for our study. A way to address this comment could have been to cite more references that showed the positive relationships between some functional traits (Amax, SLA, leaf N) and growth of seedlings under optimal conditions. Given the importance of this point, we preferred going beyond this bibliographic approach and we collected data from the literature about this kind of multi-species greenhouse experiments. We found 10 independent publications, testing growth-trait relationships for a total of 263 values, representing 212 distinct species from 157 genera (Extended Data Figure 1A; results are shown only for SLA because it was the only functional trait studied in all the 10 publications we found). These 212 tree species were representative of all forest biomes and forested continents. We would like to highlight that 35 tree species of this "seedling dataset" were also present in our main datasets (i.e. EAN+TDN+SBD+TED). These shared tree species display the same positive growth-trait relationship as the other species (Extended Data Figure 1E; N=34 in this graph because one shared tree species was the only one in a publication, disabling us from standardising the original values as at least two species per publication are needed to do so). In our opinion, this new dataset provides a

solid argument to put forth that acquisitive species (that are species with high SLA, leaf N and hence high Amax) are expected to grow fast in field conditions as they actually do grow fast under controlled, optimal, conditions. We thank the reviewer for identifying this weakness of our previous version of the manuscript, and we stay open for discussion to improve further our study (see lines 100-117).

Comment 39:

2. Intra-specific differences in traits and growth rates. The sites of the common garden experiments were distributed across wide climatic and environmental gradients. This may induce variation in functional traits. If I understand correctly, one functional trait value is now used per species, taken from databases, or measured by the authors. It is unclear whether the literature values are averages across published values or not, and what the sizes were of the individuals of which trait values were used (relevant because trait values vary with size). In any case, it seems that acclimation of functional traits to climatic or edaphic conditions is not taken into account. For the interpretation of the results, it is necessary to know whether intra-specific trait variation across the environmental gradients could help explaining the observed patterns (e.g., Figs 1, 2, 3).

=> It is correct that one trait value is used per tree species. We added the information to be explicit and transparent (lines 465-466{*}). We also agree that trait values display some variation related to ontogenetic development of plants, genetic variation and to environmental conditions. Nevertheless, when the functional dissimilarity of the studied species is large –which is the case in our study– the inter-specific variation is much larger than the intra-specific variation (Kazakou et al., 2014; Treurnicht et al., 2020). This is why retaining only the mean trait value of each plant species is considered as a relevant and reliable approach in large scale studies such as global scale studies (e.g. Ma et al., 2018; Paine et al., 2015; Pietsch et al., 2014; Reich et al., 1997; Treurnicht et al., 2020; Wright et al., 2004), even with partly imputed data{**} (e.g. Bruelheide et al., 2018; Joswig et al., 2022). We agree that intra-specific variation should be taken into account when possible (see lines 723-726). But in our study, it was not needed because of the huge range of functional dissimilarity among tree species. In our database dedicated to interspecific variation, the coefficient of variation (CV) of traits {***} ranged from 41% (leaf N) to 77% (Amax). As a comparison, intraspecific variation of traits was found to range from 9% to 22% of CV for wood density, SLA, leaf N and leaf P (Fajardo, 2018; Li et al., 2018). Based on these results and on the guidelines of Albert et al. (2011), we concluded that our approach was reliable for traits. For growth, the EAN data was useful as the EAN sites contain both different tree species and (for a given tree species) several provenances. The results show that, as for traits, the intra-specific CV of tree growth was 4-fold smaller than inter-specific CV.

Although we consider our approach as well in line with the state-of-art and recognised guidelines, it seems worth to us that these effects should be explicitly mentioned. We added a methodological section (lines 722-745) to make clear that (i) intra-specific variation exists, (ii) intra-specific variation is mainly due to ontogenetic effects, genetic variation among populations, and site effects, and (iii) intra-specific should be considered when comparing a limited number of closely related species.

{*} line numbers refer to the numbering of the file with tracked changes.
{**} there was no imputation of data in our study.
{***} Amax, SLA, SRL, leaf N, leaf P.

References cited:

Albert et al. (2011). When and how should intraspecific variability be considered in trait-based plant ecology?. Perspectives in Plant Ecology, Evolution and Systematics, 13(3), 217-225.
Bruelheide et al. (2018). Global trait–environment relationships of plant communities. Nature Ecology & Evolution, 2(12), 1906-1917.
Fajardo (2018). Insights into intraspecific wood density variation and its relationship to growth, height and elevation in a treeline species. Plant Biology, 20(3), 456-464.
Joswig et al. (2022). Climatic and soil factors explain the two-dimensional spectrum of global plant trait variation. Nature Ecology & Evolution, 6: 36–50.
Kazakou et al. (2014). Are trait-based species rankings consistent across data sets and spatial scales?. Journal of Vegetation Science, 25(1), 235-247.
Li et al. (2018) Intraspecific functional trait variability across different spatial scales: a case study of two dominant trees in Korean pine broadleaved forest. Plant Ecol. 219, 875–886.

Ma et al. (2018). Evolutionary history resolves global organization of root functional traits. Nature, 555(7694), 94-97.
Paine et al. (2015). Globally, functional traits are weak predictors of juvenile tree growth, and we do not know why. Journal of Ecology, 103(4), 978-989.
Pietsch et al. (2014). Global relationship of wood and leaf litter decomposability: the role of functional traits within and across plant organs. Global Ecology and Biogeography, 23(9), 1046-1057.
Reich et al. (1997). From tropics to tundra: global convergence in plant functioning. Proceedings of the National Academy of Sciences, 94(25), 13730-13734.
Treurnicht et al. (2020). Functional traits explain the Hutchinsonian niches of plant species. Global Ecology and Biogeography, 29(3), 534-545.
Wright et al. (2004). The worldwide leaf economics spectrum. Nature, 428(6985), 821-827.

Comment 40:

3. Climatic niches. A possible contribution to explaining the lower (relative) growth rate of (potentially) fast-growing species in common garden experiments is that these species are climatically more restricted than slower growing species. This may be relevant for common garden experiments, in which tree species can also be planted outside their climate (or environmental) envelop. I do not think there is general evidence that fast growing species are climatically restricted, but it would be good to verify to what extent the relative growth rate is positively associated with the spatial extent of species, or whether species located outside their climatic (or edaphic) envelop are growing (relatively) more slowly.

=> Ecological niche effects can indeed be expected. But, when the common gardens of the EAN and TDN were designed, the principal investigators chose tree species not at random but based on their ecological requirements. We added this information in the Methods (lines 752-754). In addition, we a priori excluded introduced tree species with low survival rate based on the rationale that they were probably outside their ecological niche. Nevertheless, because this is an important possible drawback of our study, we decided to go beyond this claim and we assessed it (lines 747-780). In practice, we collected new data about the 23 tree species of the EAN common gardens, which have a factorial design (see comment 10). The collected data were (for MAT, MAP, and soil pH): min value required, max value required, and range (i.e. [max – min]). We also collected data about the surface area of the natural niche of the species. The results of this investigation (Supplementary Figure S9) suggest that: (i) in a majority of cases, tree species were planted in sites where environmental conditions were suitable for them (see percentage values in panels A-D), (ii) trees planted in sites where conditions did not comply to the expected species requirements did not growth differently as compared with other trees (statistical tests in panels A-D), (iii) tree species with large ranges of climatic niche (or soil pH niche) did not growth faster than tree species with narrow niches (panels E-G; but see point five), (iv) tree species with large spatial niche did not perform better than tree species from small regions (panel H), (v) there was a slight, but significant negative effect of the MAP range value on relative tree growth (panel F). The latter result is mainly due to three tree species with large MAP range but, on average, lower relative growth rate than the other tree species. These large MAP range values were not due to low min-MAP values (which were similar to other min values) but to very high max-MAP values. Although this MAP range effect was significant, we assumed that it has a minor consequence on the reliability of our results because (i) it explains less than 1% of the variance (see R2 value in panel F), and (ii) this effect is not significant when considering tree species with max-MAP ≤ 2000 mm/yr (see red line in panel F) which is the most common case for temperate-boreal tree species. These results are presented in the revised version of the manuscript (lines 747-780).

Comment 41:

A second – and related – comment on climate effects is that the negative growth-Amax relationships (Fig 2A) for unfavourable climate is based on a general classification of climatic favourability (Sup Fig S4), while a species-specific assessment of the suitability would probably be better. This could be done by verifying whether a site is within or outside climate envelop of a species, or whether it is more favourable or unfavourable climatically compared to the species-optimal climatic conditions.

=> Please see comment 40 and particularly panels A-D of Supplementary Figure S9.

Comment 42:
4. The ecological foundation of the expected (and actual) results of the study can be more extensively explained and introduced earlier in the introduction. Now the hypothesis on the fact that fast-growing (i.e., acquisitive) species only comes after mentioning the main aim of the study. In my view, the introduction would be strengthened, if the fundamental contrast between the goals of forest-based climate solutions (calling for fast-growing tree species), the changing climate and increasing climatic extremes (calling for resilient tree species) and the ecological characteristics of fast-growing species (acquisitive nature, implying low efficiency of water use and probably also nutrient use) would be stressed. This could even be illustrated with a Figure.

=> We agree that this structure of the introduction section is better than ours (in the initial version). The introduction was modified accordingly (lines 100-117) and we propose a Figure (see Extended Data Fig. 2), as suggested.
In addition, it seemed to us that the concept of acquisitive-conservative would be more explicit than fast-slow, at least for describing the main results of the study. Consequently, we used the terms "acquisitive-conservative" in most passages of the manuscript and restricted the terms "fast-slow" for introducing and concluding about the implications of this study.

Comment 43:
5. I find it strange that site productivity for the SBD dataset (which contains biomass growth) is based on environmental drivers instead of a measure of productivity (height growth being used for the other datasets). I would say that for the SBD plots, biomass productivity can be used as a very direct way to estimate site productivity.

=> We agree that biomass productivity is better than biomass because it is understandable in a direct way. For mature stands, biomass productivity is now calculated as the ratio between stand biomass (Mg/ha) and stand age (yrs), and is consequently the mean rate of net biomass accumulation (Mg/ha/yr).

Other comments: Comment 44:
- Title: could be changed into a more clear description. The term generalised is quite vague, and perhaps title could reflect difference between potential growth and realized rates in practice?

=> We have spent a lot of time thinking about the title of our manuscript. We wrote many other possible titles, but almost none was as concise, informative and clear as the initial title. Nevertheless, we tried to propose another title, which is "*Widespread slow growth of acquisitive tree species*".
Possible alternative titles might be:
- "*An unifying framework for tree growth-traits relationships*"
- "*Conservative tree species achieve higher growth rates across forests*"
- "*Realised tree growth is inversely related to acquisitive strategies*"
In our opinion, the first version remains relevant:
- "*Generalised slow growth of fast growing tree species*"
Anyway, we stay open for further discussions about the title, with the reviewers and the editorial board.

Comment 45:
- Line 104: replace "question" by "evaluate" as it is strange to "question" something in a scientific study.

=> Modification done (line 123).

Comment 46:
- Line 108: nutritive stresses is unclear to me, and I wonder whether this means excess nutrients deposition (as in EU, a.o.) or nutrient scarcity.

=> Indeed, it was unclear. This passage is now "[…] *these conditions are more and more uncommon due to wide-spread nutritive limitations and climatic stresses*" (lines 127-128).

Comment 47:
- Line 123-132: It was a bit hard to understand the function of this paragraph, especially the start of the text. It seems to deal with (relative) details while the reader probably expects general results/patterns.

=> We wrote this paragraph because, before investigating the growth-trait-environment complex interactions, we needed to report some results about the growth-trait relationships (without taking into account the interactions with the environment). Notably, it seems to us necessary to indicate to readers that, if the results about wood density are consistent with the literature, results about acquisitive traits (Amax, SLA and so on) conflict with the theory. This inconsistency can be explained only by taking into account the local environment (which are evaluated in the following paragraph). That being said, we agree that the intent of our initial redaction was unclear, and we rephrased and shortened this paragraph (lines 145-157).

Comment 48:
- Lines 167-175: Wouldn't an obvious and simple solution to this problem be to measure functional traits for large tree individuals, in situ? And if so, why weren't such values used in this study? They are available (and even part has been obtained for missing traits, I read).

=> This solution is conceptually simple and obvious, but in practice quite demanding. Indeed, because ontogenetic effects have to be taken into account, it implies carrying out long-term monitoring of common gardens with functionally contrasted tree species. Because different levels of resource of climatic stress must be applied over long periods on these tree species, it implies having the same design of common garden in many distinct sites (or manipulating resource over long periods, which is technical demanding in forest ecosystems). In practice, such studies are rare, unfortunately (Figure 1 and Appendix S2 in Augusto et al., 2014). We added a short passage in the manuscript that stresses the need for further research (lines 245-248).

Reference cited:
Augusto et al. (2014). The enigma of the rise of angiosperms: can we untie the knot?. Ecology Letters, 17(10), 1326-1338.

Comment 49:
- Line 189: I don't think the word "really" is needed here…

=> This passage is no longer in the manuscript (lines 226-229).

Comment 50:
- Line 192: not sure what "biological activity" is. This can also be activity of herbivores, pests species, pathogens ….

=> Sorry of the cryptic meaning of this sentence. We wanted to say that microbes (and more generally organisms) also require favourable conditions to decompose dead organic matter (and hence enable nutrient cycling). However, this passage was removed (lines 226-229).

Comment 51:
- Line 194-196: I think this is probably the most important message and Figure of the paper. Would be good to emphasize this more.

=> We rephrased the last sentence of the conclusion in order to recall this message (lines 283-286). We remain open to additional suggestion of redaction for this take-home message.

Comment 52:
- Line 202-203: I do not get "despite" (L 202). If 'fast-growing species' have higher WD this actually may actually partially explain the lower relative growth rate because they require more biomass to realize the same height growth.

=> Our results also applies to the SBD data, which is not in height growth but in biomass increment. Consequently, to avoid confusing readers, we removed this text.

Comment 53:
- Line 205-208: I find this discussion hard to follow and quite far fetched. Inferences are made of carbon sequestration strategies of fast and slow growers based on comparisons in WD and C density which use different sample sizes. Note also that the median differences are quite small and thus have little meaning ecologically.

=> This passage was indeed not straightforward and not really needed. We removed it.

Comment 54:
- Line 215: two times management in one sentence?

=> We replaced "forest management" by "silviculture" (line 268).

Comment 55:
- Lines 215-222. I do not get why sustainable forest management is discussed here at length: it has not been mentioned in the introduction and it's unclear what share of the species included in the study are valuable (for timber). I would focus and limit this discussion to increasing carbon stocks in (newly planted) forests, and perhaps mention that this is also relevant for the growth of timber volume in planted forests.

=> This comment is in line with the comment 25, made the reviewer 2. As explained above, we fully agree that our discussion went beyond our results. But it is also important to stress that choosing a tree species is not the only criterion for sustainable silviculture and that our results must not be interpreted in a caricatured way. Nevertheless, these two independent comments make us realise that we went too far. Hence, we shortened this passage as much as possible (lines 264-274).

Comment 56:
- Lines 223-230. I think this last part of the discussion could make more use of the results presented here: concluding that local foresters should best know what species to plant is in general a good idea, but does not do justice to the results of this study. Wouldn't it be very useful to produce a list of the fastest- and slowest-growing species at each site? Or putting that in a separate dataset? There is a wealth of information in this study that can be used to inform decisions on local forest restoration/planting/afforestation.

=> Actually, we already provided this information, since all our original data is available to all readers without restriction (see "Data availability" on lines 1173-1176).

Comment 57:
- Fig 1&2: Wouldn't it helpful to combine these in 1? Their axes are the same and Fig 2 is a detailed assessment of the overall patterns in Fig 1.

=> Following the recommendation of the reviewer 1, the former Figure 2 was removed (see comment 05).

Comment 58:
- Fig 1. Perhaps briefly explain what normalization entailed (that this was done per site)

=> Explanation added.

Comment 59:
- Fig 3. This Fig is a quite hard to grasp, also because the caption seems to provide technical details rather than guidance to understand the complex set of graphs. Also, the results in panel A are similar to those in 2A, and the latter are more easy to grasp. Another option would be to show relations like in Fig 2 for some of the traits shown in Fig 3.

=> Following the recommendation of the reviewer 1, we removed the former Figure 2 (see comment 05). About the caption, in the view of the present comment, we recognise that it was a bit difficult to understand. We modified the figure and its caption in three ways: (i) a clear and concise explanation

was inserted at the beginning of the caption, (ii) the rest of the caption was re-written, and (iii) the y-axis of the panel A received addition text to help readers.

Comment 60:
- Fig 4: very clear Figure. Perhaps in caption mention that C and D provide support for the revised paradigm. Are C and D based on different datasets? Wouldn't it be logical to include "height" in y-axis label in C and "biomass" in D?

=> We revised the caption of this figure to indicate that panels C and D provide support for the revised paradigm (panel B). We also mentioned that the panels C and D used independent data. Conversely, we did not modify the y-axes labels. Indeed, for a preliminary (not submitted) version of the manuscript, we also found logical to change the y-axis label according to the nature of data. It turned out that it caused a lot of confusion and questions from many of the co-authors when they read these labels. Therefore, we changed our mind and followed the suggestion of some other co-authors who proposed to simplify the labels by using constantly the same general description (that is "tree growth"). But, we concede that there is no obvious and perfect solution to this difficulty of display.

Comment 61:
- Ext Dat Fig 4. Caption is unclear: "Each panel presents… of a given species" is incorrect? Each panel contains values from multiple species, right?

=> This is right. We modified the caption as follows: "Each panel presents the relationship between the Specific Leaf Area (SLA) value and the growth rate value of different tree species growing in a given common garden.".

Comment 62:
- Ext Dat Fig 7. I think it would be nice to combine this as additional panels to Fig 5. It shows consistent differences across the three datasets. To me this is more important than Fig 2.

=> We followed this suggestion and merged the two former figures into one main figure (see new Figure 4). The former Figure 2 was removed (see comments 05 and 57).

EDITOR:

Your manuscript, "Widespread slow growth of acquisitive tree species", has now been seen by 3 referees, whose comments are attached below. While they find your work of potential interest, as do we, they have raised important concerns that in our view need to be addressed before we can consider publication in Nature. Should further experiments allow you to address these criticisms, we would be happy to consider a revised manuscript (unless something similar has been accepted at Nature or appeared elsewhere in the meantime).

=> Authors' answer:

We are thankful for having the opportunity to revise our manuscript, and we used our best efforts to address every comment.

In the revised version, we will need you to fully validate your results, especially in response to Referee #1 by adding relevant analyses that can be referred in the text. We also request that you pay particular attention in backing up claims by appropriate statistics, especially when interpreting any lack of significance.

=> We thank referee #1 for his/her valuable comments. By pointing out some unclear or debatable aspects of the manuscript, the referee's comments helped us to improve our work. Hereafter, we have detailed our answers to comments, and how we took them into account. In brief, we added new analyses and we modified the text when needed (see below). We also described the statistics used to address the referee's comments. We took care to test our results with several statistical approaches (see below).

STATISTICS: When revising your manuscript, you should ensure that any statistical analysis used is sound and that it conforms to Nature's guidelines). A collection of articles explaining the basics of statistical analysis and advice on how to best present it can be found here (https://www.nature.com/collections/qghhqm).

=> We carefully checked the methods we used, and how we reported them. In addition, we consulted the Nature collection and downloaded nine articles. Fortunately, the reading of these papers indicated no problem in our data handling. For instance, our boxplot results already presented the meaning of whiskers (see Figure 4), as recommended by Krzywinski & Altman (2014). Similarly, we always produced scatterplots to interpret correlation r values and hence avoid retaining spurious correlations (Altman & Krzywinski, M, 2015). Based on our reading of these publications, we also made a few minor edits at lines 566-570, 636 and 648-649{*} and we went deeper in our investigations regarding data reliability (lines 658-671 and 673-694)

{*} line numbers refer to the numbering of the file with tracked changes.

References cited:

Altman, N., & Krzywinski, M. (2015). Points of Significance: Association, correlation and causation. Nature Methods, 12(10).

Krzywinski, M., & Altman, N. (2014). Points of significance: visualizing samples with box plots. Nature Methods, 11(2).

Voelkl, B., Würbel, H., Krzywinski, M., & Altman, N. (2021). The standardization fallacy. Nature Methods, 18(1), 5-7.

REPRODUCIBILITY: All of the checklists provided with the current submission (Reporting summary, Editorial policy checklist, and Code and software checklist (if applicable)) should be updated to reflect the revisions made and submitted with the revised manuscript.

=> We updated the checklists and provided them with the revised manuscript.

DATA AND CODE AVAILABILITY STATEMENTS: All original research manuscripts published in Nature Portfolio journals must include a Data availability statement. This statement must make the conditions of access to the "minimum dataset" that is necessary to interpret, verify and extend the research in the article, transparent to readers. This minimum dataset may be provided through deposition in public community/discipline-specific repositories, custom proprietary repositories for certain types of datasets, or general repositories like Figshare, Zenodo and Dryad. Providing large datasets in Supplementary Information is strongly discouraged; the preferred approach is to make data available in repositories. More information on Nature Portfolio's reporting standards and preparing your Data availability statement can be found here.

=> We provided information regarding these statements (lines 1170-1179). All the files are already openly available in the following public repository website: https://entrepot.recherche.data.gouv.fr

SOURCE DATA (GRAPHS): To increase transparency, we strongly encourage you to provide, in spreadsheet form, the data underlying the graphical representations used in figures. In the case of all experiments presenting data from animal models, this is a requirement and is not optional. This is in addition to our well-established data-deposition policy for specific types of experiments and large datasets. Online readers of the manuscript will be able to access the graphical source data directly from the figure legend. Spreadsheets must be submitted in .xls, .xlsx or .csv formats. One file per figure is permitted. If there is a multi-panelled figure, the source data for each panel should be clearly labeled in the file; alternatively the source data for a figure can be included in multiple, clearly labeled sheets within an Excel file. File sizes of up to 30 MB are permitted, but it is expected that the vast majority of graphical source data files will be considerably smaller than this. When submitting these files with your manuscript, you should select the "Source Data" file type and use the title field in the file description tab to indicate the figure(s) to which the source data pertain.

=> We provided five xlsx files (one per figure/table), containing the data used for Figures 1-4 and Table 1.

Referee #1

Comment 63 (the numbering continues whose of the previous revision):

The authors conclude that species with functional traits associated with efficient resource conservation have greater growth rates than do species with functional traits associated with rapid resource acquisition when grown outdoors in monocultures. This is not surprising. Current understanding is that species with functional traits associated with rapid resource acquisition have larger potential growth rates than do species with functional traits associated with efficient resource conservation. Potential growth rates are only realized when resource supplies are adequate and plant enemies are unimportant.

=> Authors' answer:

We do not claim to have discovered that conservative species grow faster than acquisitive species when environmental conditions are very unfavourable. This indeed has already been observed (see for instance Chapin et al., 1990). But this understanding is based on local or small scale observations (Ibid.). To our knowledge, there is no publication that studied an acquisitive-conservative performance switch, in situ, and at large scale (see below an example of bibliographic query {*}). Therefore, we believe that our study brings something new, an assessment that is consistent with the previous evaluation of the manuscript in which our study was considered as having "*unexpected results*" and "*important policy implications*" (Referee #1; comment 01), as being "*new*" and "*potentially disruptive*" (Referee #2; comments 07 and 10), and being "*truly original*" and a "*strong and important study*" (Referee #3; comments 37 and 77). We carefully read in this scope the introduction section, and we modified a passage (lines 106-111{**}) to make clear what is new: "*Similarly, species that are more efficient at keeping their internal resources (i.e. nutrients, water and energy) than collecting external resources are defined as conservative species and are commonly assumed to be slow-growing species, except in particularly unfavourable environments. Current knowledge thus suggests that acquisitive tree species*

should be promoted for mitigating climate change through fast biomass growth, but this paradigm is only partly supported by the literature."

In our opinion, the second –and most surprising– novelty of our study is to show that, on average, environmental conditions are unfavourable enough to give conservative species an advantage, while acquisitive species are commonly assumed to achieve fast growth species in most environments (e.g. refs 8,10 at lines 102-106). This result is observed in different regions, and using independent datasets, indicating that it is a robust pattern.

References cited:

Chapin et al. 1990. *The ecology and economics of storage in plants*. Annual Review of Ecology and Systematics, 21, 423-447.

{*} Example of the bibliographic queries we made: "conservative+AND+acquisitive+AND+fast-growing+AND+slow-growing+AND+(tree+OR+forest)+AND+(growth+OR+biomass+OR+RGR)"

{**} line numbers refer to the numbering of the file with tracked changes.

Comment 64

These conditions are met in growing houses when nutrients are added and enemies are excluded (as the authors demonstrate at lines 106 to 109). It is not surprising that realized growth rates do not match current understanding of relationships between potential growth rates and functional traits when plants are grown outdoors without nutrient supplements and in monocultures that attract enemies.

=> We fully agree that this small dataset is not the novelty of the study. Actually, it was used to address the first major concern of the referee #3 (please see comment 38 of the review, and our answer to this comment). We propose to make this clearer by modifying the sentence at lines 112-113:
"*We compiled data from 10 independent greenhouse experiments, involving a total of 212 tree species from all biomes, and confirmed the well-established result that seedlings of acquisitive species* [...]".

Comment 65

The manuscript uses data from three studies (named EAN, TDN and SBD) and a composite of several tropical studies. Height growth is calculated for trees in the EAN and TDN studies and then standardized before analysis (equation 2). In their rebuttal letter under their subheading "Comment 02", the authors state that equation 2 from their original submission defines a response ratio. Note, equation 2 is identical in the original and revised submissions. In their rebuttal letter, the authors cite papers by Hedges and Gurevitch for the response ratio. The seminal paper (Hedges et al. 1999. Ecology 80: 1150) defines the response ratio as follows: "The response ratio, the ratio of some measured quantity in experimental and control groups, is commonly used as a measure of experimental effect because it quantifies the proportionate change that results from an experimental manipulation." In contrast, equation 2 is the ratio of a species-level mean growth rate to site productivity. Equation 2 lacks an experimental group. Equation 2 lacks a control group. Equation 2 is not a response ratio. The authors define site productivity in the denominator of equation 2 to be the mean growth rate over all species present at each site. Thus, equation 2 standardizes species-level mean rates by dividing by the mean growth rate over all species present (with species weighted equally).

=> We agree that, from a strict semantic point of view, our metric should not be called a "*log response ratio*" (LRR) because the latter has a slightly different formula. To avoid confusing readers, we renamed our metric "*log growth ratio*" (LGR).
Nevertheless, beyond the semantic aspect of the comment, we see no fundamental reason to question the LGR metric. The LRR in particular, and other log-ratio approaches in general (see comment 81 from the referee #3), are recognised as robust metrics for standardising many types of data (Hedges et al., 1999; Lorenzen, 1989; Malyjurek et al., 2019). Our LGR metric is notably very similar to the "*centered log ratio*" (CLR; Malyjurek et al., 2019) with the only difference being that LGR uses arithmetic mean values and CLR geometric mean values (see formulas below). In our data, the two metrics showed to be highly consistent to each other ($r = +0.98$; $t = 157.44$; $df = 1,043$; $P < 0.001$), and the LGR metric was also highly correlated with the z-score metric ($r = +0.86$; see comment C02 of the previous evaluation).

To address the referee's comment, we rephrased the section dedicated to data standardisation (lines 553-570) as follows: "*To remove the prominent influence of site productivity and hence to enable comparisons among species across all sites, we standardised the original values of tree species growth (cm yr-1). To do so, we tested two different approaches: the z-score and a log growth ratio (see equation 2). The two metrics were highly correlated to each other (r = +0.86), but the log growth ratio metric was more suitable for our data because (i) the z-score cannot be calculated for sites with only two tree species (Appendix 6) and (ii) the values transformed as log growth ratios showed better distributions as evaluated by normality tests (Lilliefors and Shapiro-Wilk tests) and QQ plots. We consequently standardised the original values using the log growth ratio metric, which consisted in dividing the absolute values of tree species growth by the site productivity value. This ratio was then log-transformed (natural logarithm):*

Eq.2: $$log\ growth\ ratio = log\left(\frac{individual_{value}}{population_{arithmetic.mean}}\right) = log\left(\frac{species\ growth\ rate}{site\ productivity}\right)$$

The log growth ratio metric is very similar to the centered log-ratio metric, the later using the geometric mean instead of the arithmetic mean. We preferred to use the arithmetic mean because (i) the geometric mean might be biased if one single value of the studied population is nil or very close to zero (which happens sometimes when comparing the growth rate of different plant species), and (ii) the arithmetic mean is consistent with the site productivity metric (equation 1)."

Metrics cited:

$$LRR = log\left(\frac{experimental.treatment_{value}}{control.treatment_{value}}\right)$$

$$LGR = log\left(\frac{individual_{value}}{population_{arithmetic.mean}}\right)$$

$$CLR = log\left(\frac{individual_{value}}{population_{geometric.mean}}\right)$$

References cited:

Hedges et al. (1999). The meta-analysis of response ratios in experimental ecology. Ecology, 80, 1150-1156.
Lorenzen, G. (1989). Log-ratios and the logarithmic mean. Statistical Papers, 30, 61-75.
Malyjurek et al. (2019). Working with log-ratios. Analytica Chimica Acta, 1059, 16-27.

Comment 66

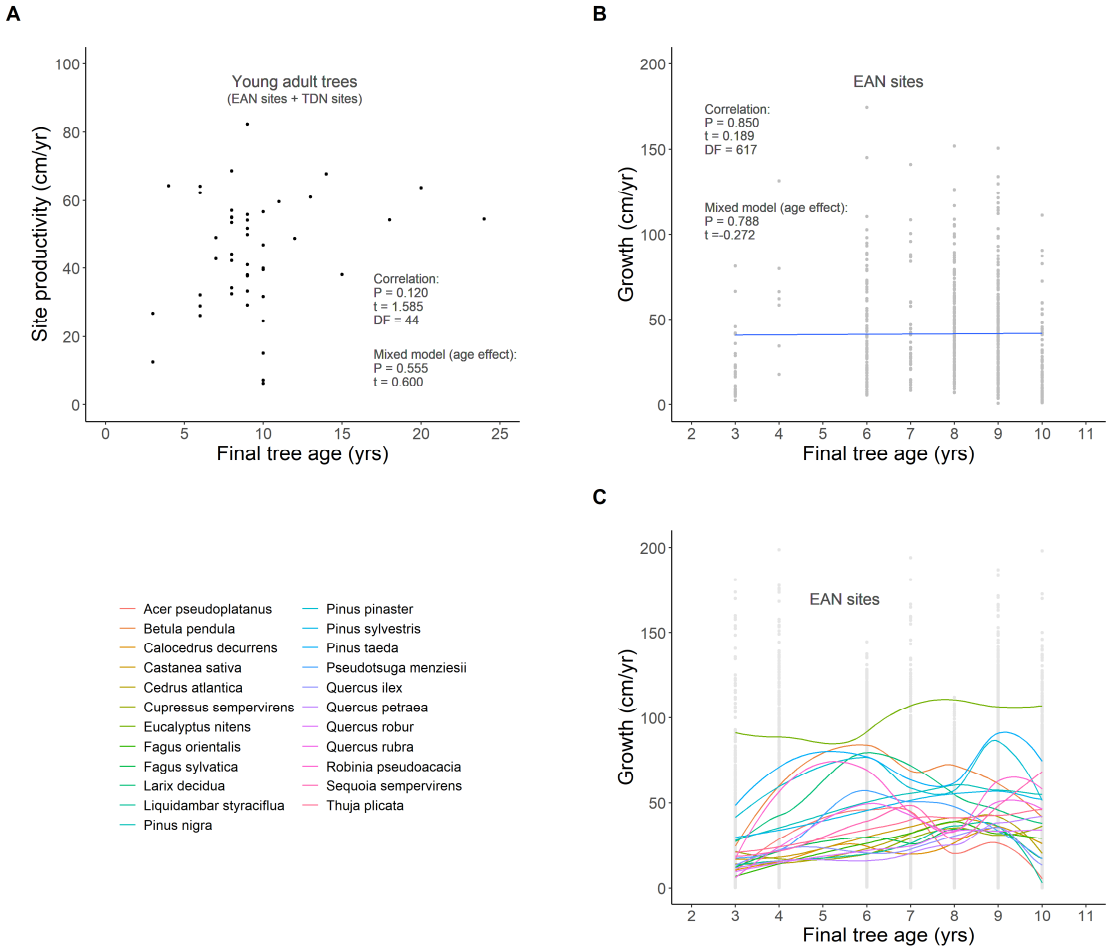
There is a potential problem for the EAN and TDN studies. Initial plant size has a huge effect on growth rates. Tree age varies from 3 to 10 years in EAN and from 8 to 24 years in TDN (see Supplementary Table S1). Height growth was quantified over a time interval of 1 to 9 years, which started more than 2 years after planting (lines 462-473). That is all the reader knows. A potential problem arises because different species may have had different initial sizes at the beginning of the time interval used to calculate height growth. The very large effect of initial size on growth rates could then explain much of the interspecific variation in observed growth rates. The main conclusion of the manuscript is that species with conservative functional traits often grow faster than species with acquisitive functional traits. If these two categories of species had attained different sizes at the beginning of the time interval used to measure height growth, then that alone might explain observed differences in realized growth. Initial plant size could enter the analyses as a covariate, but this has not been done.

=> The comment contains two different aspects: (1) the tree age during the measurement campaigns, and (2) the initial height size of trees at the beginning of the time interval.
Regarding tree age, it is worth remembering that, within a given site, all trees are of the same age because they were planted at the same time. What might be problematic is that the age at which trees were measured differed from site to site because (if we understood correctly the referee's comment) different measurement ages might be representative of different stages of plant development. To test this possible bias, we plotted the site productivity (which is the mean value of all tree species present in the EAN-TDN sites) as a function of tree age (which is also the common garden age). As shown by the panel A (Figure R5; the numbering continues those of the previous revision), there is no significant

influence of the tree age on the growth performance in the EAN and TDN sites (tested together or independently, both using a correlation analysis and a mixed model analysis).

Figure R5{*} – Evaluation of the influence of tree age on growth measurement

{*} the numbering continues those of the previous revision



Using only the EAN data (where tree species and sites are arranged in a factorial design), we also plotted the growth dynamics of sites and species. There was no significant effect of tree age on tree growth of the EAN sites (Figure R5B). Looking at tree species in EAN data (Figure R5C), the variation of tree growth did not show any clear variation with increasing tree age (at tree measurement). Based on these results, we conclude that the development stage did not substantially affect our results. We included these verifications in the manuscript (lines 673-680).

Regarding initial tree size, we fully agree this is potentially important, as numerous studies have shown that initial tree size can influence tree growth. We tested this possible bias in two distinct ways:
In a first approach, we compared our growth metric with another metric commonly used in forest science. As a reminder, our metric is as follows:

Equation R1: $$growth\ rate = \frac{(height_{ty} - height_{tx})}{(age_{ty} - age_{tx})}$$

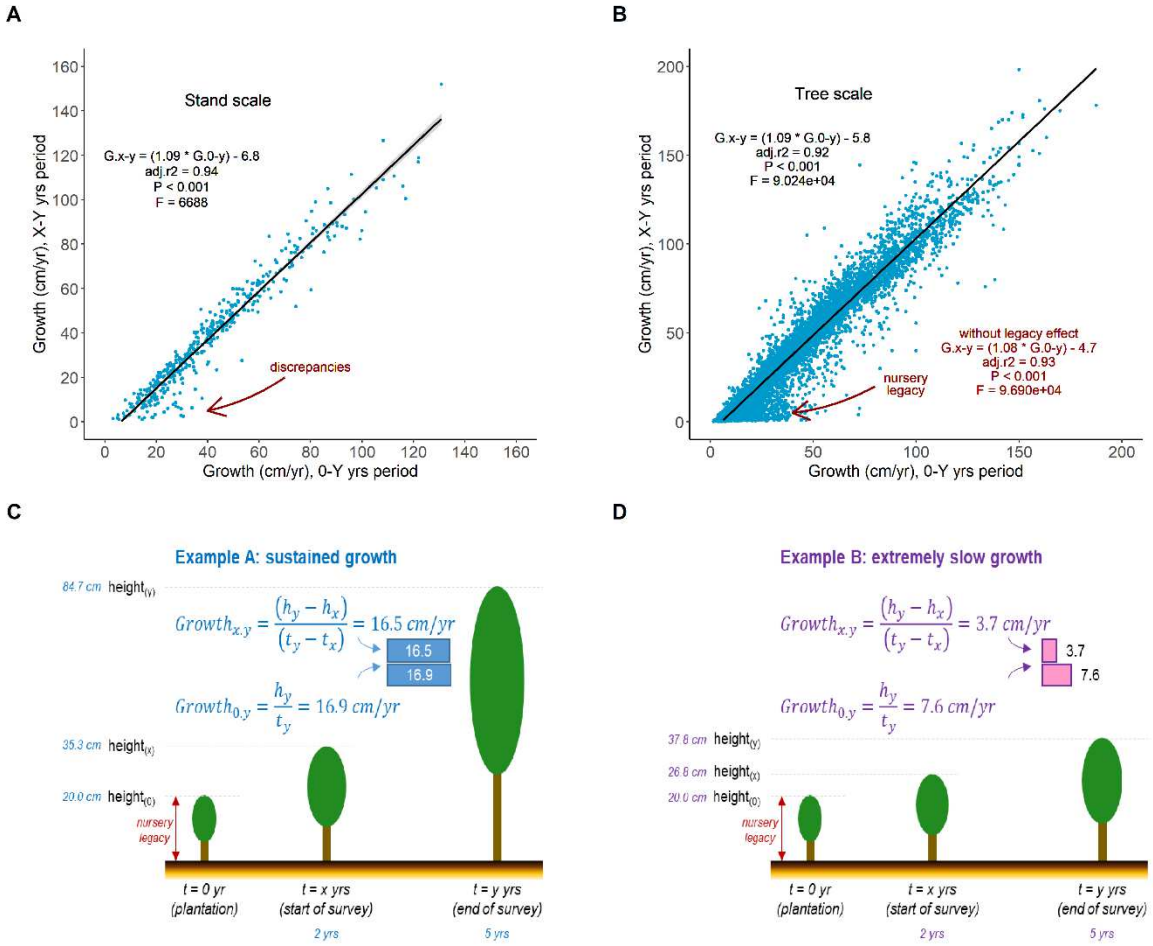
with tx and ty: initial time and final time (for instance, 2 years and 7 years)

The common metric does not consider any "transplant shock period" and hence it fixes tx=0 (and often $height._{tx}$=0) leading to the following simple formula:

Equation R2: $$growth\ rate = \frac{(height_{ty})}{(age_{ty})}$$

We calculated the growth rate of all the EAN trees following the two metrics and the results are presented in Figure R6.

Figure R6 – Evaluation of the influence of the method to quantify tree growth

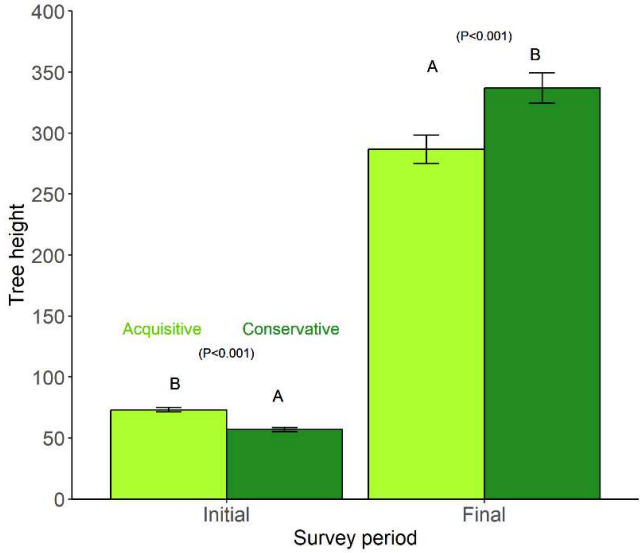


At the stand scale, the two metrics showed an excellent consistency, with an r2 value of 94% (Figure R6A). As a side-discussion, we mention that we explored the reason for having a few points with discrepancies (annotated in red in the panel A). These discrepancies were observable at the tree scale (Panel B) and were identified as a nursery legacy effect. Indeed, because equation R2 does not take into account the seedling size at plantation, this latter height value is improperly included in tree growth. In most cases, when growth rate is substantial and/or the survey period is long enough, this bias of the equation R2 does not affect the final estimates (Figure R6C and ~96% of data in panels AB). Conversely, when growth rate is very low, this bias flaws the growth estimates based on equation R2 (Figure R6D). Nevertheless, beyond the minor bias of the equation R2 (which is not used for young trees in our study), the comparison between the two metrics for calculating tree growth clearly showed that the two metrics are highly comparable, indicating that the suggested concern about initial height measurements is inconsequential with our method (based on equation R1).

In a second approach, we investigated the distribution of initial tree height (i.e. at tx) taking into account if the tree species were classed as "*acquisitive*" (A) or "*conservative*" (C). It turned out that the individuals of the acquisitive species were initially a bit taller than the individuals of the conservative species (Figure R7). This A>C initial difference was significant both using original values at tree scale (Kruskal-Wallis χ^2 =1252.6, df=1, P<0.001), original values at stand scale (χ^2=74.0, P<0.001), and standardised values (χ^2=1359.3, P<0.001). We interpreted this (unexpected) difference as a nutritional advantage given by the nursery conditions for the seedlings of the acquisitive species.
Then, we did the same for the final tree height (i.e. at ty) and found the opposite (which is consistent with the main conclusion in the manuscript): conservative species were taller than the acquisitive species (P<0.001, P=0.027, P<0.001 for the different metrics used for initial height). This C>A final difference

well supports the conclusions of the manuscript because it indicates that the conservative species grew faster than the acquisitive species, despite being initially shorter.

Figure R7 – Evaluation tree height at the beginning and at the end of the survey period



Based on these two verification approaches, we concluded that the results of the manuscript are not biased by a possible effect of initial tree size. We consequently revised the manuscript as follows (lines 377-381): "*This method was compared with a method that estimates tree growth simply as the height:age ratio and found good consistency (r = +0.97). Nevertheless, we preferred to quantify tree growth based on two surveys because it enables excluding the period after plantation (i.e. 1-2 years), which is often problematic for seedlings (the so-called "transplant shock").*"

Comment 67

There is also a problem with the analysis of the SBD data set. The SBD data set consists of a single observation of stand-level biomass. The authors obtain an estimate of site productivity by dividing stand-level biomass by stand age. Stand age ranges from 11 to 225 years. Stand-level biomass increases rapidly in young stands and reaches an asymptote in mature stands. Thus, the site productivity measure must be large for young stands and small for old stands. Thus, the author's interpretation of stand productivity is problematic for the SBD data set.

=> First, it is worth mentioning that, if the total range of stand age is indeed 10-225 years, only two sites are older than 100 years and all our other sites are less than 100 years-old (16-90 years for the percentiles 5% and 95%). Despite this information about the distribution of site age values, we fully agree that stand-level productivity is influenced by stand age (*e.g.* West, 2024), and the referee is right when anticipating that "*the site productivity measure must be large for young stands and small for old stands*" (see panel A of the Figure R8, which is now included in the manuscript as Supplementary Fig. S14). To fix this problem, we proceeded as follows:

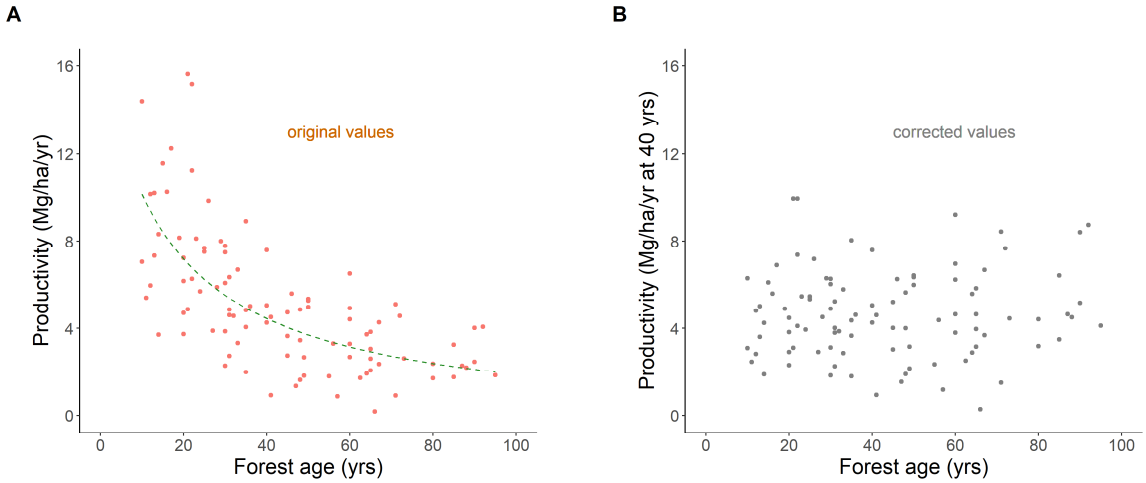
Using almost all our data (i.e. excluding the two very old sites that are outliers), we built a non-linear regression (dashed line in panel A). Then, we calculated a standardised growth rate, using 40 years-old as a reference (which was close to the mean age value of SBD stands = 41.8 yrs):

Eq. (5): $$growth_{.40} = growth_{fitted.40} \times \left(\frac{growth_{original}}{growth_{fitted}}\right)$$

With $growth_{.40}$ = growth estimated at 40 years-old; $growth_{fitted.40}$ = value of the regression at age 40 yrs ($growth_{fitted.40}$ = 4.446); $growth_{original}$ = growth original value; $growth_{fitted}$ = value of the regression at the actual age of the stand.

Doing so, the productivity values were distributed as shown by the panel B.

Figure R8 – Evaluation of the effect of forest age on forest productivity



Then, we tested to which extent this correction might affect our results. It turned out that the outcome remains the same, as shown by the updated results (see Figure 3D and Table 1, in the main text). We made this explicit by including a new section dedicated to this methodological verification and subsequent handling (lines 681-694).

Reference cited:

West, P. W. (2024). A review of the growth behaviour of stands and trees in even-aged, monospecific forest. Annals of Forest Science, 81(1), 1-25.

Comment 68

Finally, the authors recommend that species be planted in appropriate environmental conditions to maximize carbon sequestration. No one could object. The authors also recommend that this will usually be species with functional traits associated with resource conservation. I believe this recommendation is suspect for reasons described above. If the plan is to plant monocultures that facilitate enemies in nutrient poor soils, then species with functional traits associated with resource conservation should be planted. If the plan is to plant mixtures of species or supplemental planting into natural secondary succession both of which limit enemies, then it is far from clear that species with functional traits associated with resource conservation should be preferred.

=> We are sorry if our manuscript left the impression that we promote monocultures, because it is not the case at all. Common garden monocultures were simply needed to test our hypotheses about growth in relation to the plant economics spectrum. In the first version submitted, our manuscript recommended (lines 216-221 of the version submitted in early 2024):

"*We consequently put forth that our results do not question the general guidelines for sustainable forest management that include, amongst others, limiting physical disturbances, lengthening harvest rotations, maintaining a continuous vegetation cover and favouring a high level of biodiversity. The latter recommendation is of particular importance since biodiversity is at the same time an issue for conservation and an efficient lever for increasing carbon sequestration [refs 41,44] and forest resistance to disturbances and stresses [refs 45,46].*"

So, it is clear from the beginning that we have no hidden agenda for promoting monocultures or intensive forestry. It is even the opposite, as we recommend mixing tree species and reducing disturbances.

Nevertheless, the referees #2 and #3 rightfully pointed out that our initial recommendations went beyond the results supported by our data. Because we agree with this view, we consequently strongly shortened the practical recommendations; perhaps we shortened too much. To avoid any misinterpretation of our study (such as "monocultures are encouraged by Augusto et al."), we modified a passage of the conclusion section, as follows (lines 231-236): "*We consequently put forth that our results do not*

question the general guidelines for sustainable forest management that include, amongst many others, favouring a high level of biodiversity that is an issue for conservation. Biodiversity is an even more important issue because mixing tree species in forests is an efficient lever for increasing carbon sequestration and for improving forest resistance to disturbances and stressors.".
We also modified then end of the manuscript (lines 252-254), as follows: "*Furthermore, if low-risk strategies for mitigating climate change are a priority, then dedicated approaches should always consider choosing tree species with caution, regardless the other silvicultural options employed.*".

Finding the best trade-off between conciseness, general recommendations, and data-driven conclusions is not easy. We feel that we have achieved the right balance between these in the present proposal, but we remain open to further suggestions for improving the conclusion section of the manuscript (with the three referees and with the editorial board).

Minor comments follow:

Comment 69

Why are there so many missing entries in Extended Data Tables 1 and 2? From the table captions, I understand the entries correspond to an exhaustive list of the possible changes in a combination of Pearson correlation coefficients and changes in mean square error from a second analysis. Should all missing entries be question marks? The table captions should explain why entries are missing.

=> We are sorry that the tables are unclear. What we intended to explain in the captions is that:
In a first step, Random Forest models were built for all cases (i.e. all species in Table 1, and all sites in Table 2). For each model, we identified the predictors retained by the models (with four levels of confidence). In a second step, we identified the direction of the relationship (between tree growth and the predictors selected by the Random Forest model) using a Spearman correlation analysis.
What we forgot to explain (and which confused the referee's reading) is that no symbol was showed when a predictor was not selected by the Random Forest model. It means that there are no "*missing entries*" but many non-significant contributions of predictors. To fix this problem, we propose to modify the captions as follows (example of the Extended Data Table 1):
"[...] *Data were first analysed using random forest modelling, enabling to identify the site drivers of tree growth with a backward elimination approach (see Methods). The percentage increase of Mean Squared Error (%IncMSE) was used as metric for this identification, with four levels of confidence: low (2% ≤ %IncMSE < 5%; noted as "(↗)" in case of positive relationship; see below), moderate (5% ≤ %IncMSE < 10%; noted as "↗"), high (10% ≤ %IncMSE < 20%; "↗↗"), and very high (%IncMSE ≥ 20%; "↗↗↗"). When the %IncMSE value was less than 2%, the relationship was considered as negligible (noted as "."). In a second step, for the relationships that were selected by the random forest models (i.e. %IncMSE ≥ 2%), the direction of these effects was identified based on Spearman correlation values: the symbols "↗", "?" and "↘" indicate positive (r ≥ +0.20), unclear (|r| < 0.20) and negative (r ≤ -0.20) effects on tree growth, respectively.* [...]".

We hope that the rephrasing of the captions, and the addition of a symbol for negligible effects, will clarify the two tables.

Comment 70

Line 129 – Avoid "extra tropical". The word extratropical means outside the Tropics. Plus, it is not extra data. The rebuttal letter indicates it is a newly added core data set.

=> Thank you for pointing out this misleading phrasing. We modified this description as follows (lines 135-136): "*[...] (iv) a dataset containing additional tropical data (TED).*"

Comment 71

Lines 146 and 147 – Why refer to some trait-growth relationships as "inconsistent" and others as "contradictory" in Extended Data Table 2. I see positive and negative and mostly very weak relationships (and lots of missing entries) for trait-growth relationships identified as "inconsistent" and identified as "contradictory". What distinguishes them?

=> (please see our answer to the comment C69 that explains the apparently missing entries).
Regarding the use of the words "inconsistent" and "contradictory", we meant to say that:
- some traits showed a relationship with tree growth, but this relationship was not observed in all sites. For instance, the SLA-growth relationship was often negative, but "only" for 13 sites (Extended Data Table 2). We named these relationships "inconsistent".
- some other traits showed both positive relationship and negative relationship, depending of the site considered (see for instance Height-max or leaf N; Extended Data Table 2). We named these relationships "contradictory".
Reading the referee's comment, we realise now that our manuscript was not clear enough. We rephrased this passage as follows (lines 154-159): "*In contrast, some traits (e.g. SLA, SRL, leaf N, leaf photosynthetic capacity [Amax]) had inconsistent relationships with tree growth (Extended Data Table 2). We considered that these inconsistent relationships may be due growth-trait-site interactions, and investigated such interactions using mixed linear modelling.*"

Comment 72

Lines 377 to 407 = Many criteria used to select species and sites are described, and the descriptions seem incomplete. The authors could (should?) provide a table of all excluded species and provenances and the reasons for excluding them and a second table of all excluded sites and the reasons for excluding them. Lines 409 to 423 – See previous comment. Lines 425 to 437 – See two previous comments.

=> The selection process could indeed be fully described. We provided two new appendices with the required information (see Appendices 2-3 and their citation at lines 302-303).

Comment 73

Line 606 – What does "predictive linear models with iterations" mean?

=> The used R function (ols_step_forward_aic) is dedicated for building models based on AIC and on an iterative approach. The description of this function is as follows (https://cloud.r-project.org/web/packages/olsrr/olsrr.pdf): "*Build regression model from a set of candidate predictor variables by entering predictors based on akaike information criterion, in a stepwise manner until there is no variable left to enter any more.*".
We are sorry if the description in the manuscript is confusing, and we rephrased as follows (lines 525-526): "*(ii) linear models based on AIC for the selection of the best model (ols_step_forward_aic function of the olsrr package; ref.96)*".

Referee #2

Comment 74

Dear authors.
I have read the new version of the manuscript. It reads nicely and, from my point of view, all suggestions and concerns have been addressed. I have no reservations in recommending it for publication.

=> Authors' answer:
We are very happy for this support, and we thank the referee for having being very constructive during the peer-reviewing process. For sure, the comments helped us to improve the manuscript.

Comment 75
If the authors are willing to make a small additional effort, I bring two suggestions for the abstract. Please feel free to use it or not:
Line 77: Just to avoid repeating the word species here, "We recommend planting acquisitive tree species in areas where they can realize their fast-growing potential"

=> The sentence was modified following this proposal (lines 79-80).

Comment 76
Line 79: In other regions, where environmental stress dominates, conservative....

=> This last summary sentence was also modified accordingly (lines 80-83).

Referee #3
Comment 77
Having read the revised version, I still consider this to be a strong and important study. I restate what I mentioned about the original submission: "The findings are truly original, highly significant for the field of forest ecology, and of interest to practitioners in reforestation and afforestation. The authors find that species with highest potential growth rate (i.e., highest rate of photosynthesis) tend to be the relatively slower growers (i.e., in terms of height or biomass) in real-life practice of growing forest stands." I studied the replies and additional analyses performed by the authors in response to the comments from all reviewers. Overall, the revision is thorough, thoughtful and well conducted.

=> Authors' answer:
Many thanks for this support. As explained above (comment 74), we believe in the peer-reviewing system, and we did appreciate the useful comments we received.

Comment 78
As a tropical forest ecologist, I am particularly pleased by the inclusion of new tropical data, and the discussion of these new results. Yet, I have two comments on the tropical data:
1. Not all studies included were conducted in common garden experiments (defined in L 359-360 of the ms as "In each common garden, characterised by homogenous conditions, several monospecific stands were installed by planting one different tree species by stand."). The Song et al 2023 study was conducted in a permanent sample plot in a natural forest in Panama; the van der Sande et al 2015 study was also conducted in natural forest. Clearly, studies that were not designed as a common garden experiment should be left out (there could be more of those; I did not check all papers).

=> The referee is right when saying that these two studies were different. It was transparent in the previous version of the manuscript (lines 442-444): "*Because field experiments having a common garden design with mature monospecific stands are rare in tropical studies, we used inclusion/exclusion criteria that were more flexible than for our other datasets (i.e. growth metric, tree age, mixed stands)*". But, we agree that applying such a flexibility would probably imply including several other studies. Therefore, we followed the referee's recommendation and we removed the two cited studies. Finally, it is worth mentioning that we found two other studies (see below) that comply to our criteria of selection, and we added them in our TED dataset.

References used:
Lugo, A. E., Wang, D., & Bormann, F. H. (1990). A comparative analysis of biomass production in five tropical tree species. Forest Ecology and Management, 31(3), 153-166.
Rolim, S. G., & Piotto, D. (2024). Diameter growth models and performance of 100 tropical tree species in silvicultural trials in Brazil. Forest Ecology and Management, 569, 122202, 1-9.

Comment 79

2. It seems logical that the tropical data are included in as many main Figures as possible (taking limitations due to study setup into account of course). Now they are not in Fig 1 and Table 1, and I'm not sure why not. In any case, panel F in SI Fig S2 can be added to Fig 4: this would directly illustrate the larger similarity between fast and slow species in more productive sites, and helps the discussion.

=> We agree that the tropical data should be treated, when possible, in the same way as the other data. Nevertheless, the TED dataset is not as representative and as strong as the other datasets (see the new methodological section at lines 658-671). Therefore, it did not seem relevant to us to show the results by dataset (as in the previous version). To circumvent this pitfall, and because we found highly relevant the referee's comment, we present now the results by latitudinal class (see Figures 1 and 4; Extended Data Fig. 3 and 5). Tropical data were already included in the Figure 2, and we included TED data in the Figure 3 and the Table 1.
As a whole, we found that this substantial change improved the manuscript, and we thank the referee for this useful recommendation.

Comment 80

I am pleased with the answers to my main comments: I value the additional analyses of climatic ranges (my comment #3) and the quantitative assessment of intra- vs inter-specific trait variation (#2). My other comments are also addressed satisfactorily.

=> These added results were indeed useful as they address relevant possible critics.

Comment 81

As for the comments of other reviewers: my overall view is that these have also been addressed well, and -- while I'm not a user of response ratios -- I concur with the authors that these are often used in ecology and that they seem appropriate in this study. I value the efforts of the authors to calculate, derive and discuss the Z-scores as an alternative, but given explained limitations, I support their choice to continue using response ratios.

=> We thank the referee for highlighting that response ratios are indeed often used in ecology. We all the same took into account the referee #1's comment (see above), as we concede that the naming might have been misleading.

Some minor comments:

Comment 82

Abstract and intro: I think it would be nice to mention the term paradigm in here, to summarize (abstract) and prepare (intro) the shift in paradigm suggested from the results in Fig 3.

=> This is a good idea. We modified a sentence of the manuscript as follows (lines 72-74): "*This discrepancy between the current paradigm and field observations is explained by the interactions with environmental conditions that influence growth*".

Comment 83

L 105-106: I like the new setup of the introduction, but would suggest to already specify the knowledge gap here

=> We modified this passage as follows (lines 109-111): "*Current knowledge thus suggests that acquisitive tree species should be promoted for mitigating climate change through fast biomass growth, but this paradigm is only partly supported by the literature*".

Comment 84

L 209-212: complex and long sentence. Also, "other" in L210 is rather vague; would be better to specify.

=> We agree that this passage was not as fluent as needed. The paragraph was rephrased to enable an easy reading (lines 220-228).

Comment 85

L 359-360: "one different tree species" is unclear and a bit odd. Not sure what is meant here

=> We rewrote the passage as follows: "*only one tree species by stand*" (lines 264-266).