

Listed below are our changes to the manuscript after considering the reviewers' comments. Note that the indicated lines refer to the complete manuscript (not the track changes version). Apart from the changes suggested by the reviewers, we have also made a change to our statistical approach where we now have used Wald Chi-square tests to test the effects of our models rather than simply summarising the output of the models. We have added a line explaining this in the methods section (L 218) and updated all tables of statistics. Despite slight changes to the statistical output, the general results and our conclusions based on those results remain the same.

Comments from reviewer 1

How are the results relevant for simulation models of tree growth and competition (that is, for projections of climate change effects on forests at landscape and broader scales) especially for those models that account for physiological species traits such as temperature and drought tolerance?

While we have made no changes in relation to the initial comment on models of tree growth and competition (see response to reviewer comment), we have made an effort to discuss how our results may be generalized to broader perspectives in accordance with the reviewer's follow-up comment. For example, see discussion at line 301-306 for a comparison with similar studies in other biomes (tropical and temperate). Further, at line 317-322, we discuss our result in relation to changed precipitation patterns (both in terms of snow during the non-growing season and rain during the growing season). In section 4.2, starting at L 342, where we discuss the tree growth response in relation to local soil moisture, we have expanded the discussion on the potential mechanisms that might explain the surprisingly weak effect of local soil moisture. By this we hope to provide a better context for the important message that climate change adaptation of forest management needs to acknowledge that the importance of soil moisture will differ depending on whether you consider the southern or northern boreal region. Finally we have expanded the conclusion paragraph to broaden our results to perspectives important for forest management (L 407-414).

In general, the English is understandable and quite passable, but it could be improved. I have made some suggestions below, but others could be made. However, the English is very adequate given that English is not likely the native language of any of the authors.

We have gone through the text to improve the language (see the track changes version).

First paragraph. The phrase "at stake" as used here is probably not the best because it is somewhat ambiguous. "At risk" or "threatened" would be more accurate.

We have changed the text in accordance with the comment. See L 33.

L 52. Warming may also affect disturbance rates and intensity, also impacting mortality. See <https://doi.org/10.1016/j.scitotenv.2024.177043>.

We have expanded our introduction of extreme disturbances and added the suggested reference to section of increased frequency and intensity of disturbances and the link to tree mortality. See L 53.

L 93. What is the expected mechanism driving the prediction about species responses in H3? Such mechanisms are important to convey the a priori nature of your hypotheses.

We have expanded on the species-section of the introduction where we provide a potential mechanism for difference between the studied species: that a higher root:leaf ratio, previously found in *Pinus sylvestris*, may provide greater access to water and thereby greater drought resistance. See section starting at L 78.

L 102. Is this describing the design of the inventory or your study? Or both? Not clear.

We have clarified that this is the design of the NFI inventory program. See L 132.

L 109. Give the rationale for excluding these trees. Also provide the rationale for the method described in L 100.

Regarding the first rationale, we have clarified that we excluded trees in wetlands to focus our study on trees growing in productive forest landscapes. See L 139.

Regarding the second rationale, we have not made any changes (see discussion with reviewer 1).

L 112. Reword to: “value magnitudes higher than”

We have changed the text in accordance with the comment. See L 143.

L 115. Define acronym (COFECHA).

We have clarified that COFECHA is a dendrochronological software. See L 147.

L 123. What time period do the climate data represent?

We have clarified that the climate data represent the period 1961-2018. See L 158.

L 138. Reword to: “could not end until after September 1st.”

We have reworded to “could not end until after August 31st”. See L 172.

L 143. Run-on words.

We have reworded the sentence to read: “The modeled data consist of values ranging from 0 to 100 at a 2 m resolution raster grid, where values indicate the probability of being classified as the “wet” category in the NFI inventory field plots.” See L 177.

L 144. Is “mean soil moisture values” the wetness probability described in the prior sentence? Unclear.

We have reworded the sentence to clarify that it is indeed the wetness probability previously described. See L 179.

L 184. Re: “while precipitation and SPEI...” – should this read “reduced precipitation”?

We have clarified that it is increased precipitation that has a generally negative effect. See L 223.

L 188. Re: “The growth response to precipitation and SPEI became increasingly positive with increasing MAT (Fig. A2; Table 1). Has the SPEI acronym been defined? I think the index itself should be defined to ensure that readers are aware that it goes quite negative with severe drought.

At the introduction of SPEI (in the section “Climate and soil moisture data”), we have now clarified the range of SPEI. See L 162.

L 260. Here and throughout, you might consider using “relative MAT” given that in Sweden, your MAT is relatively low compared to elsewhere on the globe. You might somewhere in the discussion discuss the generality of your results in the context of the globe.

We have added a section on site description in the methods where general temperature and precipitation patterns in the study area (Sweden) are introduced. Hopefully this will clarify how our study area compares to global patterns. See L 112.

In the discussion, we have added references from tropical and temperate forest ecosystems to compare our findings with these biomes. See L 301.

L 277. I would have liked some speculation about the mechanism for this result. Is permafrost involved? Is some precipitation in the form of snow that is lost before the growing season?

We have added a section with two potential mechanisms regarding the negative growth-response to precipitation: Growing season delay due to snowfall; and Excess precipitation causing waterlogging. See L 317.

L 299. Didn't you exclude plots on wet sites?

We have added a reminder about the exclusion of wet sites in the discussion, as well as a caveat that their inclusion could change our results. See L 346.

L 321. I would like to see more exploration of this discrepancy with other results. Did this cause you to question your results? On what basis do you trust these results?

We have made no changes based on this comment (see response to reviewer comment).

L 328. “indicators for” seems to be the wrong phrase here. “drivers of?”

We have changed the text in accordance with the comment. See L 381.

L 337. Other studies have suggested that long periods of stress are required to actually kill trees because even one good year can rebuild reserves. For example, see DOI: 10.1002/ecs2.1253.

We have added a caution, coupled with the suggested reference, that our extremes are based on single-year values and that extended periods of extreme conditions may have more severe effects. See L 395.

Tree growth is a fundamental and widely used term in plant science and ecology. The current study aimed to explore and explain tree ring width response to temperature and precipitation gradient over almost 2000 plots across a gradient of climatic conditions in the boreal forests of Sweden. The analysis revealed that in cold environments tree growth responds positively to an increase in the ambient temperature, while in “warm” environments, trees reduce growth due to an increase in ambient temperature. Additionally, soil moisture can mitigate reduction in tree growth in areas of high MAT. Overall, the analysis is well done and has an important value. However, as a whole, this study needs to strengthen and clarify the main message. I found some descriptions are too detailed, whereas some important steps were either ignored or provided with very little information. Therefore, I recommend this to be gone through a major revision. I explain my concerns in more detail below.

Major comments:

1. The ideas in introduction are leaping through one to another without a proper flow. Also, different terms were not consistent across the document, which makes this document very confusing. It begins with a description of the economic implications of tree growth, which is not the central focus of the study. Additionally, there is excessive use of the term “climate change” and occasional use of “global warming.” I recommend define the scope of what aspect of climate change is being investigated. This needs to be clearly defined, as “climate change” on its own is too vague.

Terms like “high temperature” and “high/low soil moisture” are also imprecise and need to be quantified or clearly contextualized—what temperature or soil moisture levels are considered “high”? use the actual value or range of values. That will allow a more application result in other areas. Overall, this section failed to answer/ satisfy "why" it is important? Then, need more specific information about "why" this particular species?

We have made an effort to improve the flow and language of the text (see the track changes version).

We have clarified that it is changes in temperature and precipitation that we study, rather than using the general term “climate change”.

We have added a section on site description in the methods to clarify what “high” and “low” temperature/precipitation refers to in our study. See L 112.

We have added a more detailed description in the section on soil moisture data to clarify what the soil moisture variable indicates in our study. See L 176.

The tree species (the core subject of the paper) are barely introduced. The introduction does not explain why these two species are meaningful. The introduction should describe their geographical distribution, ecological characteristics, and the typical range of temperature, VPD, and soil moisture conditions in which they thrive. It should also reference recent studies related to their responses to environmental stressors that are investigated here.

In the introduction, we have expanded on the description of the tree species used in our study, as well as provided a basis for our hypothesis that the two species will differ in their growth-climate responses. See L 78.

In the new site description section in the methods, we have added a paragraph on the tree species' distributions through our study area. See L 113.

2. All the hypotheses refer to specific regions that are not even mentioned in the introduction, despite playing a central role in the study.

We have reworded the hypotheses to clarify that we indicate regions within our study area (which is presented in the prior sentence). For example, we have added an explanatory sentence to ensure that regions with high MAT correspond to the southern part of our study area. See L 99.

The new site description should further put the study region in a better context compared to other geographical areas. See L 112.

3. There is a clear lack of background on previous research on the physiological responses of conifer trees to temperature and soil moisture. Key studies in the field—such as those by Wagner et al., 2021; Zweifel et al., 2006, Zweifel et al., 2021; Klein et al., 2014; Novick et al., 2024 —are notably absent and should be incorporated to establish the scientific context on trees physiological response to soil and atmospheric conditions.

We have added Zweifel et al 2006 as a reference to the discussion on the minor effects from soil moisture. See L 349.

We have added Klein et al 2014 to the introduction to extreme events linking drought conditions to mortality events. See L 58.

We have added Novick et al 2024 to the introduction regarding the negative effects of VPD and to the discussion on the interaction between rising temperature and soil moisture conditions. See L 43 and L 354.

4. H4 needs to be rewritten and detailed- how each species will respond to an increase/decrease in ambient temperature? How will each species respond to an increase/decrease in soil moisture? And add a paragraph in the introduction that explains why you hypothesize for each species.

We have specified our hypothesis for each species and clarified how we expect them to respond to change along the gradients within our study area. Namely that *P. sylvestris* will have a more positive response to increased temperature in cold areas, and more negative response in warm areas, than *P. abies*. See L 104.

We have also added references to justify our reasoning in the introduction of the species. See L 78.

5. In the method section, a brief sites descriptions was missing, more details, such as what the dominant tree species are, their approximate age, and overall environmental conditions. Snow? Solar radiation?

We have added a site description at the beginning of the methods section, including the dominant tree species, and environmental conditions (temperature, precipitation, snow cover). See L 112.

6. In Figure 1B, the comparison between high and low soil moisture needs to be clearly defined. What thresholds or criteria were used to distinguish “high” from “low” soil moisture? And why not give the actual values of soil moisture (v/v %)?

We have expanded on the figure caption to provide a more detailed explanation of the color gradient, e.g. that yellow or “Low” indicates a 0-20% probability of the site being classified as “wet” based on the modelled data we use in this study. See L 151.

We have expanded the explanation of the soil moisture index that we have used, to clarify that we do not use data on volumetric soil moisture content. Hopefully this will avoid confusion about what the soil moisture values indicate. See L 177.

7. Regarding the calculation of the Standardized Precipitation-Evaporation Index (SPEI), the authors mention using net solar radiation based on latitude. However, cloud cover, which can significantly affect incoming radiation, is not addressed. Was this factor considered or accounted for in the calculations?

We have made no changes based on this comment (see response to reviewer comment).

8. In the results section, I suggest adding a short description of the number of extreme events experienced by the trees during the study period. Were high-temperature extremes or low-precipitation events more frequent? Additionally, it would strengthen the results to quantify the reduction in RWI associated with extreme increases in temperature and extreme reductions in precipitation.

We have made no changes based on this comment (see response to reviewer comment).

9. I recommend expanding the discussion on the threshold between atmospheric demand (VPD) and soil moisture availability. This would provide valuable ecological insight into species-specific sensitivities under climatic stress.

We have expanded the section regarding the interaction between soil moisture and temperature (and VPD), mainly with information provided from the Novick et al 2024 review that the reviewer has suggested. See L 352.