We thank the reviewer for the valuable comments and suggestions, and for taking the time to help us improve our manuscript! Please see our answers in bold below each comment.

General comments

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?

Unfortunately, we do not work with large-scale, process-based forest/ecosystem models. As a result, it is difficult for us to know how our findings are relevant to such modelling work. Maybe our results can be used for qualitative comparisons in simulations using process-based forest models (e.g. CLIM4, LPJ-GUESS, LANDIS-II). For example, significant discrepancies in growth trends between our results and models across climate gradients may reveal gaps or misspecifications in process-based models. Nonetheless, we believe that additional feedback from the reviewer is needed for us to answer this question.

I assumed that the methodology is sound (not my expertise) and concluded that the study is sound and the results useful. I found the study interesting, well-presented, and I think it would make a great addition to the forestry literature.

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.

During our revision, we will conduct a thorough proofreading to improve the language.

Specific comments

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 will change the phrasing to read “[…] at risk.”

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

Yes, in our analyses we do not account for mortality, and we mention this in the discussion. We will elaborate further and add the suggested reference as an example that mortality and ecological interactions need to be addressed to fully understand the outcomes of climate change.

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 will expand the section starting at L72 to explain a potential mechanism in the different responses between the species. Namely that a higher root:leaf ratio, previously found in Pinus sylvestris, may provide greater access to water.

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

We will clarify that this is the design of the NFI inventory program.

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

For L109, we will clarify that we excluded trees in wetlands to focus our study on trees growing in productive forest landscapes. Regarding L100, it is not clear to us what 'method' we should provide more information on. Should we expand on the design for data collection throughout Sweden, or should we elaborate on the rationale for using radial growth as a response?

L 112. Reword to: "value magnitudes higher than"

We will change the text in accordance with the comment.

L 115. Define acronym (COFECHA).

We will clarify that COFECHA is a dendrochronological software. However, COFECHA is not an acronym, it's an invented Spanish word for co-date or cross-date, so we will therefore not add any further definition.

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

We will add the period that the climate data represent (1961-2018).

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

We will change the text in accordance with the comment with the slight alteration of "could not end until after August 31st".

L 143. Run-on words.

We will restructure 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." Hopefully this will be more easily read.

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

We will change the sentence to clarify that it indeed indicates the data introduced in the prior sentence. The sentence will instead read “Using the modeled soil moisture data, we calculated a soil moisture index (SMI) based on mean soil moisture values in a buffer of 25 m radius around individual trees using zonal statistics in QGIS (QGIS Association, 2021).”

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

We will clarify that it is increased precipitation that has a generally negative effect.

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 will clarify the range of SPEI. The new sentence will read “SPEI centers around 0, where negative values indicate drier conditions and positive values indicate wetter conditions.”

Results presentations (especially graphs) are excellent! The results data are difficult to interpret, but the presentation helps some and the text descriptions of the meaning of the results help a LOT.

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.

This is a good point. However, we refrain from using “relative MAT” as this can be interpreted as the temperature being a relative measure. Regarding the generality of our results in the context of the globe, we will elaborate on the geographical range of similar findings and add a sentence referring to similar patterns (in terms of increasingly negative growth-responses to temperature along MAT gradients) in tropical and temperate forests. The new sentence will read “Although boreal forests experience relatively low temperature, similar patterns have also been observed in the tropical (Zuidema et al., 2022) and temperate (Charru et al., 2017) biomes.”

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 will add a section on potential mechanisms regarding the negative growth-response to precipitation. The new suggested text reads “A potential mechanism of the negative effects of precipitation may be that higher amounts of snowfall delays the start of the growing season (D’Orangeville et al., 2016). However, the negative response in our study is based on growing season precipitation sums only, thus likely excluding snowfall. Another possible reason as to why

precipitation negatively affects tree growth in the colder regions is that the forests are already near water saturation and excess precipitation causes waterlogging (Laudon et al 2024).”

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

It is true that we excluded plots in wetlands. However, the trees of our study grow in a range of relatively wet to relatively dry conditions. Hence, the pattern seen in previous studies (where trees growing in wet areas (not necessarily wetlands) are less affected by drought conditions than those growing in dry areas) could reasonably be thought to exist in our gradient as well. However, we will add some text to remind readers that wetland trees were excluded. The new section will read “Notably, we excluded trees growing in wetlands in our study. The inclusions of such trees may have revealed a greater effect of soil moisture on the trees’ growth response to drought conditions.”

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 found the minor effect of soil moisture somewhat surprising, and we have done further analyses to validate the results. We have run the analyses on soil moisture indices based on different buffer zones (from 1 to 1000 meter buffer zones around each tree) and found similar results regardless of the buffers used. Furthermore, we have made use of the NFI field estimates of the soil moisture (classified as wet, moist, mesic, dry) and explored RWI correlations to temperature and precipitation, and found no effects of soil moisture. Hence, both modeled and field estimated data suggest that soil moisture has a very limited effect on the tree growth-response to climatic variables.

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

We will change the text in accordance with the comment.

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.

This is a good point and we will expand our caveats for the extreme year analysis by including the following sentence: “It is also important to note that we have studied discrete extreme years, but it may be extended periods of extreme conditions rather than single-year extremes that produce the most severe effects for tree growth (Gustafson et al., 2016).”