

Interactive comment on “Tree growth and its climate signal along latitudinal and altitudinal gradients: comparison of tree rings between Finland and Tibetan Plateau” by Lixin Lyu et al.

Anonymous Referee #2

Received and published: 10 April 2017

The manuscript, “Tree growth and its climate signal along latitudinal and altitudinal gradients: comparison of tree rings between Finland and Tibetan Plateau” has good potential to reveal some insightful ways in which trees and tree populations respond to climate over gradients. I enjoy the potential in the data and some of the findings. It is written well enough for most audiences; some places could use some clarification. While many of the results generally follow prior work, the examination of the sub-monthly response is particularly novel, especially at such large scales.

One major concern I have is how do the authors control for differences in sunlight or day length in comparing to two regions? There are significant differences between the two and can affect the results. How do they ensure that some of the differences they

[Printer-friendly version](#)

[Discussion paper](#)



see, perhaps with the response to precipitation, is not related to day length?

The second smaller concern is that because the authors go to sub-monthly climate responses, Figures 5 or 6 (and much of the discussion related to those figures) are almost unnecessary. The findings are not too unexpected, but the sub-monthly analysis gives more insight. If choosing one, Figure 5 might be preferable over Figure 6. It is easier to read and provides more information.

Suggestion: as this is an international paper, perhaps use only Latin names throughout the manuscript.

There are some concerns with the current status of the work. They are detailed below.

Page 2, lines 1-2: it is likely better to emphasize these are potential natural laboratories. Space for time doesn't really equal time, especially given that less than 100 years are analyzed here. Environmental variability increases with greater periods of time. So removing that from the introduction would be ok. Along these lines, the authors do not fully come back to that concept.

Page 2, Line 7: perhaps "or" instead of "and" between reproduction and survival. Also, here and throughout: the Oxford comma will aid clarity in the manuscript.

Page 2, Lines 8-9: There are updates to the Loehle reference in the region with greater replication at a larger spatial scale. It also indicates the same concept in this sentence. Related: explain here how a negative correlation to temperature and a positive response to precipitation together equals drought.

Page 2, Lines 26-27: (Kim and Siccama 1986) is a great forerunner of this idea and deserves recognition. It was well ahead of its time.

Materials and Methods:

Page 3, Lines 16-28: how might sampling in belts alter the climatic response? Does differing densities impact the observed climatic response? See, for instance, (Sánchez-

[Printer-friendly version](#)[Discussion paper](#)

Salguero et al. 2013, Sohn et al. 2013, Aldea et al. 2017).

Page 4, Line 4 etcetera: the proper convention is crossdate, crossdating, crossdated. Please use this convention.

Page 4, Lines 20-24: it is not likely a serious issue, but how might the inferred temperature with elevation impact the results?

Page 4, Lines 26-29: although long in use, mean sensitivity is not a useful for comparing tree-ring records. See (Bunn et al. 2013). Please remove the MS analysis and comparison from the manuscript.

Suggestion: an interesting comparison might be the coherence within each population over space. Because many of these samples were collected in plots or were aimed to be representative of the forest, perhaps a box plot or something similar expressing the strength of interseries correlation would be compelling and insightful instead of MS.

Page 5, Lines 4-6: why 31 day windows? Did the authors experiment with narrower windows?

Page 5, Lines 8-11: given the submonthly work, it is not clear the use or need for these analyses. Why focus on seasons?

Page 5, Lines 16-19: suggest removing the MS results.

Page 5, Lines 20-24, Figure 4: Did the authors conduct cluster analysis within each region? Might the analysis on all populations force artificial grouping within each region? The analyses in Figures 7 & 8 somewhat supersede the scale of analysis in Figure 4, correct?

Suggestion for Figure 7 and most figures: consider choosing a consistent color scheme for Finland and the Tibetan Plateau and use it throughout the paper. In Figure 4 the TP is black, but by Figure 6, Finland is black. Maintaining the same colors for regions will make it easier on the reader.

BGD

Interactive
comment

Printer-friendly version

Discussion paper



Page 5, Lines 25-30, Figure 5: the authors write about growing season, but their information here is more specific. Suggest that in the Results the authors should be more specific. The authors make much of a negative correlation to temperature in early summer, but it appears this response is much stronger in February and March in northern Finland? Am I interpreting this incorrectly? If so, I apologize. If not, consider re-emphasizing these results. They do not seem as similar as suggested in the text. There appears to be negative correlations in northern Finland in May & June, but they are much weaker compared to earlier in the year.

To make Figure 5 easier to interpret, suggest putting the months on the top of the top 2 plots. Also, perhaps make the symbols in Figure 5 smaller or replace with lines so a clearer interpretation can be made.

Page 6, Lines 10-14: why might the Finland plots have larger variability?

Discussion:

Page 6, Line 20: “ring-widths are lower” than what?

Page 6, Lines 21-23: remove MS discussion per above

Page 6, Lines 25-29: samples were collected in plots. Density, diversity, and their impact could presumably be investigated here instead of suggesting they might be at work in the results.

Page 7, Lines 15-19: there is a growing and now somewhat large body work finding or examining the relation between winter temperatures and tree growth. A review of this work would help contextualize the findings here. It might help signify the importance or the continuing line of evidence created by the findings in this study.

Page 8, Lines 9-16: this is one of the most novel aspects of the study here and should be emphasized earlier and more prominently. Interesting findings.

Page 8, Lines 21-23: why might this be? Is there literature that could help account for

[Printer-friendly version](#)[Discussion paper](#)

this finding?

Page 8, Lines 24-28: Masting comes out from nowhere in this manuscript. Do the species study mast? If so, how regularly? If there cannot be a better tie between the species studied and masting, it is suggested that this section be dropped.

References: Aldea, J., F. Bravo, A. Bravo-Oviedo, R. Ruiz-Peinado, F. Rodríguez, and M. del Río. 2017. Thinning enhances the species-specific radial increment response to drought in Mediterranean pine-oak stands. *Agricultural and Forest Meteorology* 237:371-383.

Bunn, A. G., E. Jansma, M. Korpela, R. D. Westfall, and J. Baldwin. 2013. Using simulations and data to evaluate mean sensitivity (ζ) as a useful statistic in dendrochronology. *Dendrochronologia* 31:250-254.

Kim, E., and T. G. Siccama. 1986. The influence of temperature and soil moisture on the radial growth of northern hardwood tree species at Hubbard Brook Experimental Forest, New Hampshire, USA.

Sánchez-Salguero, R., J. J. Camarero, M. Dobbertin, Á. Fernández-Cancio, A. Vilà-Cabrera, R. D. Manzanedo, M. A. Zavala, and R. M. Navarro-Cerrillo. 2013. Contrasting vulnerability and resilience to drought-induced decline of densely planted vs. natural rear-edge *Pinus nigra* forests. *Forest Ecology and Management* 310:956-967.

Sohn, J. A., T. Gebhardt, C. Ammer, J. Bauhus, K.-H. Häberle, R. Matyssek, and T. E. Grams. 2013. Mitigation of drought by thinning: short-term and long-term effects on growth and physiological performance of Norway spruce (*Picea abies*). *Forest Ecology and Management* 308:188-197.

Interactive comment on Biogeosciences Discuss., doi:10.5194/bg-2016-559, 2017.

BGD

Interactive
comment

Printer-friendly version

Discussion paper

