(Reviewer 1 responses)

Dear Reviewer,

Thank you very much for reading the manuscript carefully and for your valuable comments and suggestions for improving it. Please, find below our answers to your comments.

1. The authors state in the abstract (line 19-20): "Our reconstructions have 3, 15 and 60-year periods, which suggests the influence of the El Niño-Southern Oscillation and Pacific Decadal Oscillation on the region's climate and relevant processes"
This sentence slightly contradicts the results presented lines 259-260: "However, we did not find any significant correlation between precipitation reconstructions and other climatic indices (NINO3, NINO4, NINO3.4, SOI, PDO and AO)"
I would encourage the author to rephrase the sentence found in the abstract or even to remove it completely.
Thank you for this comment. However, each of these sentences is relevant for different analyses, hence, these are not contradictory. Specifically, the first sentence is relevant for the Figure 7, i.e. wavelet analysis. This analysis is investigating the periodicity in the reconstruction. On the contrary, the correlation analysis (second sentence) was directly looking at the relationship between oscillation indices and our reconstruction. Although not significant, it does not mean that the results of wavelet analysis are not relevant. However, on the base of your suggestion we made slight changes in Result and Method sections to increase the clarity in the way that these results referred to different analyses.

2. The authors correlated instrumental data from their study area with ENSO, PDO and AO indices. The correlation values presented in the manuscript appear to be statistically significant, yet the values are not very high…
So my question is can we really conclude that PDO, AO, and ENSO have a discernible influence on Sikhote-Alin Mountain Range merely looking at correlations values?
I would encourage the authors to be really careful and to state whenever possible in the manuscript that further analysis would be required to be able to draw definitive conclusions on this matter.
Investigating the influence of modes of variability on tree-growth is now very trendy amongst dendroclimatologists. Yet in my opinion you don't necessarily need to prove that your tree-ring reconstructions reflect past ENSO/PDO/AO variability to publish a very interesting paper.
Thank you for this suggestion. We agree that the further research is needed to be able to draw more precise conclusions regarding the impact of large-scale oscillations on climate and tree-growth. Hence, we added at the end of Discussion the following sentence: "However, the further research is needed to be able to fully understand the mechanisms behind the effect of large-scale oscillations on climate and tree growth."

3. Lines 71-72: «The study area is located in northeast Asia and includes three points located in the southern, central and north-western parts of the Sikhote-Alin Mountain Range, Southeastern Russia"

The author frequently employ the term "point" in the manuscript when they refer to study sites. Wouldn't it be better to use the word "site" instead?
Thanks for your suggestion, we have corrected the term "point" on "site" throughout the text

4. The author state lines 60: (3) "to analyze the periodicity of 60climatic events and their driving forces"

What about: (3) to investigate the influence of modes of variability such as ENSO, PDO on tree-growth (or on the Sikhote-Alin Mountain Range)?
Thank you for your suggestion, indeed, in this form, this aim sounds better. We have made a correction to the text.

5. Lines 110-110 […] "the COFECHA program (Holmes, 1983) was used to check the accuracy of the cross-dated measurements".

Maybe the authors could consider adding the COFECHA output to the supplementary material.
We have added COFECHA outputs for three sites in the Supplementary.

6. Lines 136-137: Low-frequency time series variations in reconstructed precipitation were summarized with moving averages (5-year)

The use of word "summarized" sounds odd in this context… Obtained or an equivalent word maybe?
Indeed, in this context, the word "obtained" would be more appropriate. We have made the correction to the text.

7. I am not sure that a 5 year running mean really allows you to obtain a low frequency time-series…
We agree with you that when we say "low-frequency" we usually mean much lower frequency changes. We corrected this to "multi-annual" as further referred in the Results.

8. Lines 158-159: The statistical characteristics of the chronologies are listed in Table 2.

Shouldn't it be Table 1?
Here we are talking about calibration and verification statistics of the reconstruction equations. We have corrected the sentence with reference to the table so that it is clear that we are talking about Table 2.

9. Lines 201-203: The correlation between the precipitation reconstructions was significant at all three points yet varied as follows: 35% in the case of CSA-NSA, 22% in the case of NSA-SSA and 44% in the case of CSA-SSA.

I am not sure that I understand this sentence…
Here we mean the correlation between reconstructions. Thank you for noticing the oddities in this phrase, we have corrected the values of the correlation coefficient, which for some unknown reason were expressed as a percentage.

10. Lines 222-224: A 5-year moving average of the reconstruction demonstrates multi-annual to decadal variation in April-June precipitation and suggests prolonged wet and dry events, most of them were in 17th and 18th centuries

Is a five 5 year running relevant to analyze decadal variations?
We meant that this average shows variations in precipitation on a scale from several years to a

decade (maximum). In order not to mislead the reader, we have removed the word "decadal" from the sentence.

11. Line 333: draughts

Shouldn't it be droughts?
Of course, these are the "droughts". Thank!

12. Conclusion - Line 406: Thus, our results enable better understanding of future climatic trajectories in Northeast Asia.

I am not sure that I understand why… Could the authors provide more details?
Here we meant that our results could suggest how the amount of spring-early summer precipitation may change in the near future. But we agree with you, this is too general phrase, so we replaced it with the one you suggested (next comment).

13. It may have been interesting to add somewhere in the conclusion that the authors' tree-ring chronologies could be used by the PAGES Hydro2k consortium and/or to update the MADA PDSI dataset developed by Cook et al. (2010).
Thank you for your suggestion! We added this sentence at the end of the discussion.

(Reviewer 2 responses)

Dear Matthew Therrell,

Thank you very much for your careful reading of our manuscript and your valuable comments and suggestions! Please find answers to your comments below.

1. While the manuscript is certainly readable in its current form, there are places where improvement to the written English could be made if possible. This is not necessary, only suggested.
Thank you for this comment. We checked the text again and tried to make small improvements.

2) The authors consistently refer to the three tree-ring sites in order from south to north. This isn't a problem except that this makes Figure 2 appear visually inconsistent with Figure 1. That is the southernmost site is at the bottom of Figure 1 but top of Figure 2. I suggest reversing the placement of panels (a) and (b) in Figure 2. I also suggest including the tree-ring site codes next to the Met. station names e.g., "Krasniy Yar (NSA)".
Thanks for your suggestion! We rearranged the panels in Figure 2 2 and added tree-ring site codes.

3) In section 2.2. "Tree-ring chronology development" the authors state on line 104 that "Two increment cores were extracted from living trees (then we used the one with the highest number of tree rings)…" Using only one of two collected cores is very unusual. I can't imagine why the authors would take this approach and any dendrochronologist who reads this will think the same thing. I don't expect the authors to completely redo their analysis but I think they should explain this choice and I would also encourage them to date and measure all the remaining material before contributing the chronologies to the ITRDB.
Indeed, in the manuscript it sounded like we got two good cores from each tree and then only used one simply just because it had more rings. But in fact, there were quite a few cases (more than 50%) when one of the two cores contained rotten wood, and not necessarily at the end of the core, but in the middle. In some cases, the incremental borer passed through the knot. There were also cases when one of the cores was damaged during transportation. All such cores creates problems for cross-dating, especially if they are cores from trees in the forest, where biotic relationships cause additional "noise". That is why we decided to use one, the highest quality, well-preserved and longest core from each tree. However, as you suggest, we will use all the remaining good quality material in order to obtain the chronologies before sending them to the ITRDB. We also made minor corrections to the sentence to better explain why we used only one of two cores.

4) Also they stated that 136 cores were collected from 136 trees. This does not match the previous statement (two cores per tree). So did they collect 272 cores from 136 trees?
Thank you for this comment. Indeed, we made an inaccuracy in the text. We used 136 cores, so initially we had twice as many cores (as mentioned in the previous sentence). We have made a correction to the sentence to make it clear.

5) In Table 1 the authors appear to indicate that all trees sampled from each site were used in the chronology development. Is that true? It would be somewhat surprising if every tree sampled (longest core only) actually cross dated well enough to use in the chronology (see point 6)

Indeed, Table 1 indicated the number of wood samples before cross-dating. Of course, not all of them have been successfully cross-dated and used to create chronologies. We have corrected the numbers in the table, now there is only the number of cores that was used. We also found and fixed ae error in the number of cores for the NSA.

6) Reviewer # 3 (point 2)) asked whether all the collected material could be crossdated. The authors response focused on whether material from one site could be crossdated against other sites and noted that it was "rather difficult". I am a little bit confused as to why this was the case when in Figure 6 (and associated discussion) they show the common wet/dry years across the three sites. One would imagine that these common signals would allow crossdating between sites. Also I do not think that was actually Reviewer # 3's question. I think it was more about how much of the collected material could not be crossdated at all. As I noted it seems unusual for all the material to reliably crossdate.
It seems that we really misunderstood the Reviewer's question. Hope we have now given an answer to it (answer to your previous comment).

7) I would encourage the authors to show "spaghetti plots" of the raw and detrended tree-ring widths in the Supplementary Materials.
Thank you for this suggestion! We have added "spaghetti plots" to the Supplement. We've also added COFECHA reports as suggested by another reviewer.

9) In the Data Availability statement the authors should add the web address of the ITRDB for those readers who may be unfamiliar.
We have added a full database link to the Data Availability statement.

10) I include below a reference that I was surprised was not included.
We have added this reference, thanks!

List of relevant changes made in the manuscript

Lines 11-13 (Abstract). We added two new sentences at the very beginning: «Climate reconstructions provides important insight to past climate variability and help us to understand the large-scale climate drivers and impact of climate change. However, our knowledge about long-term year-to-year climate variability is still limited due to lack of high-resolution reconstructions. »

Lines 73-74. Objective 3 has been changed to «(3) to investigate the influence of modes of variability such as ENSO, PDO on tree-growth (or on the Sikhote-Alin Mountain Range).»

Lines 93-94. Figure 2 caption changed to «Figure 2: Monthly total precipitation, minimum and maximum temperature at (a) Krasniy Yar (1940-2013), (b) Melnichnoe (1941-2009), and (c) Chuguevka (1936-2004) meteorological stations.» according to the new panels order.

Table 1. We have corrected the number of live and dead trees used for the chronologies development.

Lines 116-118. The sentence «Two increment cores were extracted from living trees (then we used the one with the highest number of tree rings) and one core from dead trees at the breast height. In addition, in SSA discs from dead trees were collected (one disc per tree)» is changed to «Two increment cores were extracted from living trees (then we used for the measurement only one core without rotten parts and other damages) and one core from dead trees at the breast height. In addition, in SSA discs from dead trees were collected (one disc per tree).»

Line 118-119. The sentence «Together we collected 156 wood samples (136 cores and 20 discs) from 156 trees.» is changed to «Together we used 169 wood samples (149 cores and 20 discs) from 169 trees: 45 for SSA, 77 for CSA and 47 for NSA.»

Line 149. The sentence «Low-frequency time series variations in reconstructed precipitation were summarized with moving averages (5-year).» is changed to «Multi-annual time series variations in reconstructed precipitation were obtained with moving averages (5-year)».

Line 158. We have added a link to the new table in the Supplement (Table S1).

Line 172-173. The sentence «The statistical characteristics of the chronologies are listed in Table 2.» is changed to «The calibration and verification statistics of the reconstruction equations are listed in Table 2.».

Line 215-216. The sentence «The correlation between the precipitation reconstructions was significant at all three points yet varied as follows: 35% in the case of CSA-NSA, 22% in the case of NSA-SSA and 44% in the case of CSA-SSA.» is changed to «The correlation between the precipitation reconstructions was significant at all three sites yet varied as follows: 0.35 in the case of CSA-NSA, 0.22 in the case of NSA-SSA and 0.44 in the case of CSA-SSA.».

Lines 272-273. The sentence «However, we did not find any significant correlation between precipitation reconstructions and other climatic indices (NINO3, NINO4, NINO3.4, SOI, PDO and

AO).» is changed to «No significant correlation was found between precipitation reconstructions and large-scale oscillation indices.».

Line 405. The sentence «However, the main contribution to the precipitation is still made by the impact of the Pacific Ocean.» is changed to «Our results suggest that the main contribution to the precipitation is still made by the impact of the Pacific Ocean. However, the further research is needed to be able to fully understand the mechanisms behind the effect of large-scale oscillations on climate and tree growth.»

Lines 416. The sentence «Thus, our results enable better understanding of future climatic trajectories in Northeast Asia.» is changed to «Our tree-ring chronologies also could be used by the PAGES Hydro2k consortium and/or to update the MADA PDSI dataset developed by Cook et al. (2010).».

Line 417. We have added the full ITRDB address «(https://www.ncdc.noaa.gov/data-access/paleoclimatology-data/datasets/tree-ring)».

References. We've added two new references:
Janda, P., Ukhvatkina, O.N., Vozmishcheva, A.S., et al., 2021. Tree canopy accession strategy changes along the latitudinal gradient of temperate Northeast Asia. Global Ecol Biogeogr. 30, 738– 748. https://doi.org/10.1111/geb.13259
Zhu, H.F., Fang, X.Q., Shao, X.M. and Yin, Z.Y., 2009. Tree ring-based February–April temperature reconstruction for Changbai Mountain in Northeast China and its implication for East Asian winter monsoon. Climate of the Past, 5(4), 661-666.