

Interactive comment on “Tree-ring based spring precipitation reconstruction in the Sikhote-Alin Mountain Range” by Olga Ukhvatkina et al.

Anonymous Referee #3

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Olga Ukhvatkina and colleagues present three precipitation reconstructions based on tree rings from *Pinus koraiensis* (Korean pine) from the Sikhote-Alin Mountain Range, a region where no other dendroclimatic has ever been conducted.

Developing new hydroclimatic records in poorly documented areas of the Northern and Southern Hemisphere is a challenging but really important task to better understand past climate variability and in this respect the work performed by the authors should be commended

This is an interesting contribution that fits well with the scope of Climate of the Past and that will certainly be of interest for the readers of the journal. The paper is rather well written and structured.

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Yet at this stage, I have a few concerns preventing me to accept the manuscript as it is. I would recommend acceptance after major revisions.

Comments:

1) The authors state that they sampled trees in an area where almost no anthropogenic activity occurred over the last 300 to 500 years, this is really interesting. Do the authors think that it could be possible to extend back in time the existing records? I would discuss somewhere in the discussion whether it would be possible to extend the chronology back in time using living, dead and/or subfossil materials.

I think that most of the reader never had the chance to go the Sikhote-Alin Mountain Range. Would it be possible to add to figure 1 a picture of the study site and possibly a picture of one disc collected by the authors?

2) Could all the samples collected be crossdated?

3) This concern has already been raised by other referees, but it would really useful to have more details about the detrending method used by the authors. Age-dependent spline smoothing is a very general description. The author should keep in mind that Science should always be reproducible and in this respect providing sufficient details for the reader to understand how the analyses were performed is really important.

Could the authors let us know the reasons that led them choose this particular method over other methods such the negative exponential method for instance?

4) How did you aggregate the detrended series together? Did you use the Tukey's Robust Mean or simply averaged the detrended series together?

5) Did the authors account for variance changes resulting from changing sampled replication?

6) Overall I think that the section "Tree-Ring Chronology development" could be expanded slightly and should contain more details.

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7) Lines 132-1322, the author state: “A correlation analysis was used to evaluate the relationships between the ring-width index and observed monthly climate records from the previous June to the current September”

Did the authors used bootstrapped correlations functions? Again, additional details would be most welcome.

8) I concur with the other referee that, using the residual chronology to perform climate reconstruction is a little bit unusual. . . Have the author at least tried to perform the precipitation reconstructions using the standard chronologies? Do the reconstructions have some predictive skills? One compromise could be to present the “residual reconstructions” in the main manuscript and to present the “standard reconstructions” in the supplementary material.

9) Unless I missed something, I was not able to locate the error bars in the plots displaying the reconstructions. The authors should keep in mind that trees are not perfect rain gauges. The method used to reconstruct precipitation variability also comes with limitations. Therefore paleoclimatic reconstruction should always come with uncertainty estimates. I would also invite the author to describe in the method section how they computed the uncertainties of the reconstructions.

10) Figure 3: I would not reconstruct precipitation for the sections of the chronologies having an EPS below 0.85.

11) Figure 3, 5, and 6: Whenever possible I would encourage the authors to use the exact same scale for the Y axis.

12) There is something odd in the Table 2. RE and CE are replaced by E and E.

13) I do also have a few concerns about the authors’ conclusions regarding the linkages with ENSO and PDO...

The author didn’t find any significant relationships with the NINO3, NINO4, NINO3.4 and SOI indexes, yet they hypothesize that the periodicities detected by the wavelet

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analyses are related to ENSO. . . How can the authors be sure that the 3 years cycle is related to ENSO? It could be something completely different. I am not sure that the evidence currently presented by the authors support their conclusions.

Providing more details regarding the influence of ENSO on Far East Russia would be also be welcome. If I am not mistaken, so far the authors only cited one reference (Byshev et al., 2014). Does it mean that no other study attempted to investigate the influence of ENSO on Far East Russia's climate?

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