Clim. Past Discuss.,
https://doi.org/10.5194/cp-2020-49-AC2, 2020

Climate of the Past Discussions
Open Access
EGU

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

Olga Ukhvatkina et al.

ukhvatkina@gmail.com

Received and published: 24 June 2020

Dear Reviewer, We would like to express our gratitude for the careful analysis of the manuscript and valuable comments and suggestions for improving the article. Please, find below our answers to your comments.

"1. Please give a brief explanation to Skew/Kurtosis in Table 1" At the very beginning of each of the three chronologies (where EPS < 0.85, Fig. 3) there are few (2-4) outliers that appeared due to averaging of a small number of tree-ring index values (low sample depth). Since the normal distribution is very sensitive to outliers, even one such outlier significantly influences the value of skew/kurtosis. For example, for SSA chronology these outliers are only two values: 1.556 and 1.429. With these values, the

Printer-friendly version

Discussion paper

skew and kurtosis are 0.402 and 0.537, respectively. If we filter out these two values based on the Z-score, then the skew and kurtosis will be 0.234 and -0.087, respectively, i.e. the distribution of tree-ring index values will become much closer to normal. The same is true for other chronologies. In addition, we can take into account that parts of chronologies where EPS < 0.85 were not used for precipitation reconstruction.

We placed in Table 1 two values of skew/kurtosis for each chronology - before and after filtering outliers and gave explanations in the Supplement, Figures S2 and S3. Note that the skew/kurtosis values before filtering outliers have changed, since the STATISTICA, where we performed additional calculations, uses different formulas than ARSTAN.

"2. Table 1 shows that the MS of the tree-ring width chronologies at all three locations appear not high relative to nearby areas. Can you give some explanations on the MS values." In Fig. 1, we refer to 7 studies with the nearest precipitation reconstructions; 4 of them contain information on the mean sensitivity: Chen et al., 2016, MS = 0.216; Liu et al., 2004, MS = 0.45; Liu et al., 2009, MS = 0.23 and Liu et al., 2010, MS = 0.42. Thus, in the first and third studies, the mean sensitivity is lower than in our work, and in the second and fourth it is higher. The mean sensitivity value in our work was expected result, since we collected cores in a closed-canopy stands, where trees are relatively less sensitive to climate changes. Mean sensitivity higher than 0.3, as far as we know, can be expected when cores were collected from single trees growing close to extreme climatic conditions, for example, near a tree line on mountain peaks. For example, in Liu et al., 2010 we found that “The sampling sites are covered with stunted trees or vegetation and sparse Chinese pine trees (Pinus tabulaeformis Carr.), which grow on thin soil (10–20 cm deep) with poor nutrition. These sites are very open, with 50–200 m distance between individual trees.” We added words “closed-canopy” to line 106: “All samples were collected from old-growth trees in natural closed-canopy Korean pine-broadleaved forests.”

"3. You used residual chronologies for precipitation reconstruction, which is differ-

ent from most other studies using standard chronologies. Can you add some explanations?" Indeed, most studies use standard chronologies, since it preserves much lower frequency signals (Cook and Kairiukstis, 1990). But, on the other hand, residual chronology has had all autocorrelation stripped from the series, making it more suitable chronology for regression analysis (Speer, 2010). In our case, the main reasons why we chose the residual chronology were that a) standard chronologies for all three points had significantly lower mean sensitivity (0.210, 0.192 and 0.196 for SSA, CSA and NSA respectively), b) standard chronologies much weaker correlated with precipitation. We added additional information in section 2.2 and Figure S4 to the Supplement to make this clear.

"4. There are two figures named "Figure 7" in the paper. Please modify." This mistake was corrected after a technical check of the manuscript before it was sent for review. Probably, the old version of the manuscript came to you.

"5. It appears that there are more words after line 265. Please complete it." The same as for the previous comment.

"6. I do not suggest to use the periodicity detected in the tree-ring reconstruction to infer the potential linkages with ENSO and PDO. There are other climate modes having similar periodicities also. In addition, it does not mean the climate is under control by a climate mode even their periodicity is very close." We agree that there can be other climatic modes that may have similar periodicity and also influence precipitation. Therefore, in our study we are talking about the relationship between the periodicity in reconstructions and ENSO and PDO as an assumption, taking into account a large number of studies from this region where ENSO and PDO usually indicated as some of the most significant. We made minor corrections to the sentences where we talk about the effects of PDO and ENSO, to emphasize that this is suggestion.

"7. It is helpful to compare your reconstructions with nearby reconstruction to highlight the common climate anomalies." We agree that it would be very helpful to

make such a comparison with reconstructions from nearby territories. Of course, we tried to find reconstructions with which our results could be compared. But as we wrote (Discussion): “Most of the studies available from China, South Korea or Japan ... were aimed at precipitation reconstructions during the summertime monsoon period and rarely covered the spring-to-early-summer period. Thus, comparing our spring-to-early-summer precipitation reconstruction with generally available summer-time monsoon period (June to August) is not suitable as these two periods featured entirely different weather patterns (Mezentseva and Fedulov, 2017). Hence, we decided to conduct only a qualitative analysis of the wet and dry period coincidences with other reconstructions. We compared the data obtained with the identified wet / dry periods in terms of precipitation from the previous October to the current September, which were studied by Chen et al. (2016) for the southern part of northeast China (Changbai Mt., Qainshan Mt.) and the northern part of South Korea ... ”(Lines 319-333). Thus, we made a comparison with one study, and did not find other studies with which we also could compare our results. And in order to show that our study area is far from other reconstructions, we showed them in Fig. 1.

Please also note the supplement to this comment:
https://cp.copernicus.org/preprints/cp-2020-49/cp-2020-49-AC2-supplement.pdf

---

Interactive comment on Clim. Past Discuss., https://doi.org/10.5194/cp-2020-49, 2020.

Printer-friendly version

Discussion paper