Clim. Past Discuss.,
https://doi.org/10.5194/cp-2017-98-AC3, 2017

Climate of the Past Discussions

Interactive comment on "Autumn – winter minimum temperature changes in the southern Sikhote-Alin mountain range of northeast Asia since 1509 AD" *by* Olga N. Ukhvatkina et al.

Olga N. Ukhvatkina et al.

ukhvatkina@gmail.com

Received and published: 17 November 2017

Dear Referee, Thank you very much for your attention to our research. We closely analyzed manuscript in accordance with your comments and hope that our answers will be satisfactory to you. Some your comments the same with comments of Ref.#1 and Ref.#2 and we ventured repeat our answers to these comments again. We express to you our deep appreciation for your help, which has greatly improved our manuscript. Corrections in the manuscript (new ver.) according to your comments highlighted in green color.

With kind wishes, Olga Ukhvatkina and co-authors.

Response to general comments:

1. Comment: The paper initially discusses potential climatic parameters as the dominant mechanism controlling tree-ring growth, then – in one sentence – concludes that "the most stable correlation appears between the growth and the minimum monthly temperature of August- December of the previous year at MP7, on which we base our subsequent reconstructions". This aspect is critical, because this is where the meaning of the climate reconstruction is defined, and the treatment of this aspect is too superficial and not sufficiently robust. It is unclear what "stable correlations" refer to, and it should be discussed how much of the variance is actually explained by this parameter – that turns out (in the Conclusion section) to be quite a small fraction. Based on Fig. 4, it seems that several of the other climatic parameters correlate with the tree-ring growth almost as well as the August-December temperature. This should be explored – and discussed - in more detail. Also, would it make sense to use principal component analysis and combine some of the climatic parameters to see if it is possible to explain more of the variance in the data – although this will not make it possible to reconstruct more climate parameters back in time, it may still prove helpful for our understanding of the climate parameters driving tree-ring growth.

The authors' response: Comment accepted, "stable correlation" changed to "significant correlation". Analysis of correlation between climatic parameters and tree-ring weight was conduct in specific package for dendroclimatic studies "treeclim" in R (Zang, Biondi, 2014) (reference in main text: Lines 155-156). This is citation of package authors (Zang, Biondi, 2014): Numerically, treeclim uses the algorithm implemented in DENDROCLIM2002 to calculate response and correlation functions; format of input data is the same as for DENDROCLIM2002 and bootRes. In the case of response functions, the design matrix is orthogonalized so that the regression is performed against principal components of the design matrix, retained according to the PVP criterion (Guiot 1991), which corresponds to the determinant of a correlation matrix of uncorrelated variables. Estimated regression coefficients are then transformed back into the

original parameter space (Zang and Biondi 2013). Correlation function analysis uses Pearson's linear correlation computed between the response variable and each subvector of the climate design matrix. Bootstrap resampling (1000 iterations) is used to test for significant correlations. This citation is showed that significance of revealed correlations is corroborating by bootstrap resampling analysis (1000 iterations). These methods of analysis are common and "classical" in tree-ring studies. The relatively low value of the explained variance was also noted by Ref.#1 and Ref.#2. According to these comments we improved our reconstruction (see new ver. of manuscript and response to Ref.#1). In additional the R2 value now indicated not only in Conclusion, but also in the section in 3.4 (line 188) and in Table 2. Indeed, the principal component analysis could increase the explained variance, but it is practically not used in such studies, because, as you mentioned, it will not possible to reconstruct climatic parameters.

2. Comment: It is also unclear how the bootstrapping method was used for the verification – vital details are missing as this is not explained in the text. It just states that: "The idea that this method is based on indicates that the available data already include all the necessary information for describing the empirical probability for all statistics of interest". It is unclear what this actually means, and it should be explained in more detail how the verification is done.

The authors' response: Comment accepted. Bootstrap method is the one of the most well-known methods of short data analysis in the tree-ring based studies. Since this method is widely used it is well described in the literature. In main text of manuscript (line 196) we added references, so readers can study the features of the method. In Table. 2 it is indicated that 199 iterations have been carried out for the verification, and in the methodology (Section 2.4) there is a reference to the STATISTICA software we used for the analysis.

2. Comment: Defining warm and cold periods The occurrence of warm and cold periods in the new record is defined as when the temperature deviates more than half

the standard deviation from the mean. However, it is unclear if this refers to the 21-yr smoothed record, or the annual data – my guess is the annual data, but the text seems to suggests the 21-yr averaged data, and it is impossible to tell from the figure. The problem with the definition and the figure (Fig. 5) is that it is hard to make them match, i.e. the 21-yr smoothed record rarely increase/decrease above/below the dashed lines (or is that because it is the standard deviation on Fig. 5 and not half the standard deviation?), whereas the annual data show more variability, briefly extending beyond the standard deviation on many occasions – but in this case the defined periods seem very arbitrary and could as well have been longer or shorter. Also, looking at Fig. 5b, the four warmest years do not occur during the years cited in the text (although it is unclear if this is based on the annual or the 21-yr smoothed data, but neither seem to fit the description in the text).

The authors' response: Comment accepted. It's our omission that we didn't describe the process of defining of cold and warm periods. In order to clarify this in the main text the following explanation was inserted: "If the reconstructed minimum temperatures were above or below the average value by >0.5 SD for three or more years, then we considered this deviation as warm or cold period, respectively. Also, if two warm (or cold) periods were separated by one year, when the temperature sharply decreased (or increased), then such periods merged into one." (lines 211-214).

3. Comment: First of all, it is a bit difficult to follow the discussion of regional climate changes without a map, where the location of some of the other records are indicated (e.g. Fig. 7). Secondly, the discussion is somewhat unclear, because it is concluded that "...these results characterize regional climate variations and provide reliable data for large-scale reconstructions for the northeastern portion of Eurasia". At the same time, there are clearly differences between the record presented in this study and the nearby records shown in Fig. 8c and 8d. The differences between the new climate record presented in this study and those from nearby areas are briefly discussed in lines 308-316, and are attributed to differences in the reconstructed tem-

perature parameters – and the asymmetry between medium, minimum, and maximum temperatures. This is a really important aspect, as the different records reconstruct the temperature during different parts of the year, as it is therefore a little like comparing apples and oranges. I think this aspect deserves much more attention, particularly if we are to understand the regional climate variability. It also raises the question as to how and the extent to which we should understand the new record as representing regional climate variability.

The authors' response: As we understood this comment may be divided on two parts. First of all, it is necessary to understand where the study areas for which reconstructions being compared. For this we added locations points on the Fig. 7. The next part of the comment concerns the irrelevance of comparing of reconstructions for different seasons. As we answered to Ref.#2 in part this is a fair comment, but we had to use such different reconstructions (see response to com. 4 to Ref.#2) and it is make sense. In addition, we improved the Fig. 7 and now it shows that our reconstruction is representative to the territory of all three reconstructions (for minimum temperature of August - December). Also, despite the fact that the temperature was reconstructed for different seasons, the general trend (cold and warm periods) coincide.

4. Comment: First of all, there is no description of the methods underlying the spectral analysis. The paper just states that "The MTM analysis over the full length...", which means that it is impossible to reproduce the spectral results presented in this paper. The Methods section should provide sufficient details of the method used to enable other people to reproduce the results.

The authors' response: Comment accepted. We added links to the authors of the method and information about used software (lines 156-159).

5. Comment: Secondly, in the Results and Discussion sections a myriad of significant 2-3 year cycles (2.3, 2.5, 2.9, 3.0, 3.3, and 3.7) are reported and discussed. While these periodicities may be real – and potentially reflect the ENSO or quasi-biennial

oscillation – they are very close to the Nyquist frequency. With a Nyquist period of 2 years, it is hard to interpret the 2-3 years as direct evidence for climatic oscillations on this time scale. It is thus likely that these high- frequency periods reflect year-to-year scatter, but this aspect is not discussed as all.

The authors' response: We used the additional analysis method (SSA) to confirm the significance of the detected cycles. As a result, we obtained that all 2-3-year cycles are joined in one 3-year cycle. Traditionally, such short-period fluctuations in the region are associated with ENSO or quasi-biennial oscillation and we indicate this in the text. But additional analysis using the KNMI Climate Explorer (http://climexp.knmi.nl) did not reveal a significant correlation between the ENSO indexes and the reconstructed temperatures, but showed a significant correlation with the North Pacific temperature. Therefore, we assume that the Pacific Decadal Oscillation is more important for climate variations. According to the comment and the new results obtained, we made corrections to the main text of the article (lines 139-142, 156-159, 224-233, 363-367, 370-375).

6. Comment: Thirdly, the Abstract and Conclusion mention an 11-year cycle, but the 11-yr cycle is not visible in the power spectrum (Fig. 6), and the Results section only mentions the 8.9-yr cycle, whereas the Discussion section mentions a 8.9-11.5-yr cycle. But where did the 11- or 11.5-yr cycle come from? There is no mention of this and this a confusing.

The authors' response: Comment accepted. We made changes to the manuscript in accordance with this comment, comment #5 and new results obtained. According to an earlier study (Zhu et al., 2016), the 11-year cycle of solar activity in tree-ring reconstructions can be detected as a 8.5-11.5-year.

7. Comment: Finally, a more general criticism of this aspect concerns the discussion of the origin of the periodicities. The main problem is that the periodicities, in particular the 8.9- and 189-yr cycles, uncritically are taken as direct evidence for a strong solar

influence on climate on these time scales. While the Sun may have driven climate change on these time scales in the study area, it is simply not enough to infer this based on periodicities that resemble those of the Sun (which on average are 11 and 210 yeas, respectively). In such a record, there will almost always be periodicities that resemble those of the Sun and it therefore takes more to infer causality. In the authors want to establish that the Sun influenced climate in the area, they should engage in much more detailed analysis of the new tree-ring climate record and records of solar activity and calculate correlations, lads, and compare those to red-noise models. It would also be interesting to establish if the cold period indeed corresponds to solar minima – as stated in the abstract – but such an analysis is completely missing.

The authors' response: Comment accepted. We agree that the identification of similar cycles cannot be a direct evidence of the influence of solar activity on the tree growth. Also, the correlation of solar activity indicators with reconstructed temperatures is also not a direct evidence of this. For a full answer to this question, more in-depth studies are needed that, to our opinion, go beyond the scope of this article. However, studies by other authors (e.g., Raspopov et al., 2008) indicate that both short-period and long-period solar activity cycles are directly tracked in tree-ring records and we base our research on these studies. As for comparison of the reconstructed temperatures with the solar activity minimums, we performed an analysis of relationship between our reconstruction and TSI using the KNMI Climate Explorer (http://climexp.knmi.nl). As a result, we obtained a significant correlation with this indicator. In addition, there is analysis of individual cold periods at the end of the 17th century and historical records for neighboring regions in the main text of the manuscript (lines 294-298).

Minor comments L. 19. Abstract: It is unclear what you mean by "de-Vier quasi-200 quazi-200 solar activity cycle." Presumably this refers to the de Vries (or Suess) 210-year solar cycle. The word "year" is also missing. The authors' response: Comment accepted.

L. 47. "It is well known that warming of the climate is correlated with solar activity".

This sentence and the following sentence suggest that solar activity is the only source of warming, including global warming. You need to be much more precise with respect to what you mean here. Also, solar activity is a driver of climate change, but it is not strong driver a temperature changes compared to changes in greenhouse gases. The authors' response: Comment accepted. Line 49.

L. 128-129. Maybe spell out what is meant by "...it matches a minimum sample depth of 3 trees in this segment". The authors' response: This is common expression that mean a number of samples in this part of tree-ring chronology. Usually this expression doesn't need explanation.

L. 133. Where is the Chuguevka meteorological station relative to the sample site? This is really an import aspect. The authors' response: Comment accepted. Line 135.

L. 175. There is no "Y" in the equation – guess this refers to "VUSr"? The authors' response: Comment accepted. Line 188.

L. 297. "The period of landscape formation.....during the transition". This is unclear, as the landscape formation occurred long before the Little Ice Age – do you refer to vegetation changes? The authors' response: Comment accepted. Line 330.

L. 326-327. This sentence makes no sense to me – how is this related to the sentences above (which it refers to)? The authors' response: Comment accepted. We rewrote the sentence (L380-382).

Figures Figure 3 It is unclear what the sample depth refers to. Is it he number of tree records? The authors' response: Indeed, this is the common designation of the number of samples.

Figure 7 It is the correlation coefficient that is plotted here – this is not clear to me? It is also unclear if the signifance refers to all colours, so that for white areas there is no correlation at the 10% signifaince level? It would be very helpful if the geographical position of the record from this study (Fig. 8a) and the two nearby records in Figs. 8c

and 8d could be indicated in this plot. The authors' response: Comment accepted. We added locations on the Fig 7. As indicated in the caption to this figure, it shows the significant value of the correlation coefficient between our data (instrumental observations - Fig. 7a, reconstruction - Fig. 7b) and model calculated temperatures of the earth's surface (CRU TS 4.00).

Please also note the supplement to this comment:
https://www.clim-past-discuss.net/cp-2017-98/cp-2017-98-AC3-supplement.pdf

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