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Interactive comment on "Temperature variability of the Iberian Range since 1602 inferred from tree-ring records" *by* E. Tejedor et al.

Anonymous Referee #1

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In this study, a set of tree-ring chronologies from the Iberian Range is used to develop a maximum temperature reconstruction spanning the period 1602-2012. This topic is potentially very interesting since the temperature reconstructions in this region are rare. However, I see relevant issues that raise a number of (serious) concerns related to the composite chronology used for the reconstruction, the climate variable reconstructed and, particularly to the statistics of the calibration-verification. Considering these concerns, I unfortunately cannot recommend this manuscript for publication, and I think that addressing these concerns would entail the preparation of a whole new manuscript.

I will just focus on those main issues starting from the statistics of the calibration-verification. In addition, the manuscript would also require a careful editing since there are spelling errors, repetitions and inaccuracies, particularly related to the definition of

correlations (r) and coefficient of determination (r2).

I loathe to be so critical but Figure 8 and Table 2 give the impression that the numbers provided do not match with the series shown in the Figure and likely something went wrong when calculating and interpreting some statistics.

1. As an example, Figure 8 shows that the r2 of the later period is 0.54 (or a correlation of 0.73). This value does not seem to match the poor interannual synchrony between the series that can visually be seen in the figure. It seems to me that either the correlation is spurious and largely inflated by the similar trend; or a correlation of 0.54 was mistakenly labelled as r2. Please note that correlation (r) and coefficient of determination (r2) are used in the manuscript and figures in both upper and low case letters and sometimes mixed (i.e., in Figure 11. R2 is defined as adjusted correlation; and text between lines 27-31 in page 8 mention correlations but show values labelled as r2) and I wonder if this could has been a potential source of confusion.

2. The validation statistics seem also too high. The reduction of error (RE) value of 0.99 is just hard to believe. A RE value of 0.99 (considering that the theoretical maximum value is 1) would basically mean that trees are recording temperature with the precision of a thermometer and, unfortunately, this is not realistic. It is very likely that something went wrong in the calculation, and this would need to be re-checked and re-interpreted.

3. The reconstructed climate variable is the mean temperature over 21 months. This variable will presumably have a strong autocorrelation. It is not clear to me whether and how the authors statistically addressed the calculation of the significant levels considering the reduction in the degrees of freedom associated to a high autocorrelation. On the other hand, the authors stated on the manuscript that the chronology used for the reconstruction (BasPois) displays a first-order autocorrelation of 0 which implies that the proxy record does not mimic the autocorrelation of the temperature series used for calibration. Hence, there is a clear mismatch in the statistical properties of the predictor and the predictand. At this point, I am missing an analysis of the residuals from the

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regression modelling (trend, autocorrelation, etc) that will provide critical information on the adequacy of the predictand included in the model. I would expect that the residuals derived from such a regression will show a strong autocorrelation which will question the estimations of the uncertainties and statistical significance.

4. In view of the clear visual mismatch in high frequency between the tree-ring chronology and the instrumental temperature record, I would recommend to do a comparison of both series at different time scales to make sure that the correlation observed in the calibrations are due to synchrony in both, low and high-frequency domains, and it is not a spurious correlation due to similar long-term trends. This would definitely help to know if the climate variable chosen for the calibration is the correct one.

In the background there are a couple of other issues that are not as relevant as the one with the statistics of the calibration-verification, but also critical in the general context of the paper.

5. The authors combined data from different chronologies into a single sort of regional chronology using different methods, which is always an interesting exercise. However, having a look to Figure 3, I wonder why all chronologies have been included into the final regional composite instead of discarding the chronologies that clearly showed poor correlations (i.e., s047). According to the information currently available on the paper, a reader cannot be sure whether chronologies encoding different climate signals have been merged into a final composite. To answer this question and reinforce the methodological decision adopted, I would suggest to check whether all chronologies encode the same climate signal before building regional chronologies, particularly if some chronologies clearly show a limited agreement with the rest. In this way, potential doubts on the quality and regional representativeness of the composite regional chronology will be minimized.

6. The link between the climate variable reconstructed in the paper and the proxy record lacks a consistent physiological explanation. The explanation given between

lines 13-16 of page 11, though correct in general terms, is too general and seems insufficient to explain the selection of a climate season that is quite unusual in the context of tree-ring based climate reconstructions. In fact, the explanation given could be applied to any lagged climate season. However, and independently of the physiological explanation, calibrating with a 21 cumulative monthly mean of temperature when the chronology shows a first order autocorrelation of 0, seems totally contradictory to me. I do not doubt that the authors have a consistent reason for all the decisions adopted in the paper. However, the present version of the manuscript gives the impression that the selection of the composite chronology and the climate season used for the reconstruction were purely based on the highest correlation obtained, and all other considerations and potential implications were somehow overlooked.