

Interactive comment on “Tree-ring reconstruction of seasonal mean minimum temperature at Mt. Yaoshan, China, since 1873 and its relevance to 20th-century warming” by Y. Liu et al.

Anonymous Referee #4

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general comments This manuscript presents a reconstruction of December–June minimum temperatures for a site in the Central Plains of China, based on tree-ring data. There are several issues that the authors need to address before it may be considered to be published in CP.

Specific comments First of all, I do wonder about the choice of target season (December–June). I do not buy the authors claim that this is “logical and easy to understand” (p. 869, line 3). From the correlation analyses (Fig 4), it is evident that there are strong correlations with T in February– April and June, but not in the other months. I would also assume that there would be distinct differences in weather patterns and between the winter monsoon period, spring and the onset of the summer monsoon

C649

(June), which may not at all be linearly related? I would advise examining this (i.e. redoing the correlation analysis) by removing the trend in the data.

I would like to see some rationale for reconstructing minimum temperatures.

Why do the authors choose to standardize the data using RCS? This is a method that is used to preserve low-frequency variability and more suitable for long chronologies (multi century). The authors may be familiar with the potential pitfalls when using this method, such as spurious end effects, and personally I would not use it for such short records as presented here.

I also question the usage of individual cores rather than tree-averages when calculating the EPS values. This clearly will boost the numbers, and from Fig. 2, I have a feeling that the “reliability” of the chronology weakens considerably prior to the early 1900s.

Also, why is the sample depth dropping after the 1970s? What is the possible effect on the chronology of this?

Another striking thing is the overall lack of correspondence between reconstructed and observed D–J T_{min} (Fig. 6 & 7). While there is quite a strong positive trend in the observations, there much less trend in the tree-ring data. Also, the inter-annual agreement is quite weak. This suggests that the reconstruction neither captures the recent trend, nor the more extreme years. Could this be an indication of the choice of target season not being optimal? Assuming that the warming trend is larger in winter, and that the trees contain more of a spring signal, it may be wise to choose the latter as the target.

I know that it is more or less standard practice to do spectral analysis on tree-ring data, but when it is just briefly (and not very convincingly) discussed, I feel that it is redundant.

Basically the discussion is weak and needs to be improved, especially page 871.

The language needs to be checked throughout (especially the abstract and introduction

C650

Out of curiosity: the authors suggest that the historical documents are biased. Can this be quantified and put in relation to the large uncertainty of the tree-ring based reconstruction?

Interactive comment on Clim. Past Discuss., 10, 859, 2014.

C651