

## ***Interactive comment on “Maximum growing season temperature in Western Europe: multi proxy reconstructions in Fontainebleau from 1596 to 2000” by N. Etien et al.***

**N. Etien et al.**

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Answer to referee3.

1. I find the title misleading: "Maximum growing season temperature" implies that daily maximum temperatures during the growing season are reconstructed. Something like "Annual mean of daily maximum temperatures during the growing season" would better describe what is about.

Initial version: Maximum growing season temperature in Western Europe: multi proxy reconstructions in Fontainebleau from 1596 to 2000. Corrected version: A multi-proxy reconstruction of Fontainebleau (France) growing season temperature from A.D. 1596 to 2000.

2. Abstract: Describe the main new features of the new reconstruction and omit the last paragraph which is too long for an abstract (one third of the whole abstract!). Why does the reconstruction "provide an independent control of the quality of CET data"? What verifies what?

The abstract was rewritten (it is now less than 300 words long). The sentence about the quality of CET was removed.

3. Abstract: Giving the uncertainty of  $0.55^{\circ}\text{C}$  without any comment is misleading as this is just the minimum uncertainty (as stated correctly in the text later). In the abstract the "maximum" (conservative) uncertainty estimate including all quantifiable uncertainty sources is more appropriate.

This comment was taken into account. In the abstract, one can now read:

"The multi-proxy reconstruction of spring-summer maximum temperature spans four centuries (1596-2000). It is associated with an uncertainty of  $\sim 1.1^{\circ}\text{C}$  (1.5 standard deviation), and is expected to provide a reference series for the variability of growing season temperature in Western Europe."

4. 1. Introduction, 3rd last paragraph:  $R^2=0.6$  does not give much information. Correlation coefficients and t-values are extensively used in the text but without thresholds for significance they are of limited value. P-values could be used, as they include the significance of the relationship. This is an important point!

Number of observations and P-values were joint to the correlation coefficients.

5. 1. Introduction, 2nd last paragraph: This is about methods, so move it there. Plus, "approach" is more appropriate here than "originality".

Initial version: The power spectrum of the multiple Fontainebleau tree-ring records will be linked with the power spectrum of the Burgundy historical grape harvest record and the temperature reconstruction derived from the linear regression model described in (Etien et al, submitted). The originality of this analysis is the use of raw tree ring

isotopic data, without any standardisation: we take advantage of the annual resolution of the records and assume limited age effects. This paragraph was removed. In the conclusion something equivalent now appears: Corrected version: "The strength of our approach lies in the quality of the original proxy records and in their strong relationship with growing season maximum temperature, demonstrated for the 20th century."

6. 2.1 Sampling site: Although the historical review is interesting, it has to be shortened for the sake of focusing on the main points. The 6th paragraph is a repetition.

The historical review has been completely removed.

7. 2.4 Sample preparation: This chapter should also be shortened and focus on the information necessary for the following results and discussion. Check also if CP accepts URLs in the text as reference. Providing at least the name of the owner of the URL would improve traceability.

The “ material and method ” section has been completely re-organised and shorten. There is no more reference to any URLs.

8. 2.5 Isotopic analyses : shorten this chapter as well, similar to chapter 2.4

Isotopic analyses is now point 2.2. It is shorten by approx. a half. In particular, most pieces related to d13C analyses are moved to an appendix.

9. 3.2 Calibration : What is the starting year and what the ending year of the calibration ? What test was used when testing the correlation environmental parameter/proxy records? What were the results (p-values)? Type citation () in R to see how R should be cited. Have you tried to validate the calibration equation by using standard calibration/verification exercises (e.g. reserving 1/3 of the time series for verification) or cross-validation? Such analyses are standard and it must be shown that this is successful in order to trust the reconstruction.

The calibration is the subject of a companion paper (which was provided with the paper submitted at CP). This companion paper was accepted for publication in "Climatic

change". Nevertheless, we tried to make things clearer by a new sub-section: 3.2. calibration methodology and uncertainties.

10. 3.2 Calibration: Equation (2) assumes linear relationships. Can the authors show that this basic assumption is correct?

Again, this can be found in the calibration paper. The linear relation between d18O and GHD with Tmax AMJJAS are presented:

For d18O,  $R^2=0.33$ ,  $n=94$ ,  $P\text{-value}=1.3 \cdot 10^{-9}$

For GHD,  $R^2=0.5$ ,  $n=100$ ,  $P\text{-value}=7.11 \cdot 10^{-10}$

11. 3.2 Calibration: d13C turned out to be irrelevant. Hence drop the detailed discussion on d13C and just state its insignificance supported by the two  $R^2$  adjusted.

We agree with this proposition. The discussion on d13C was greatly shortened and moved to an appendix. In the paper, we just mention the irrelevance of using this parameter as a proxy.

Corrected version: In 2.2.: "Similar inter-tree analyses have revealed a much larger dispersion of d13C data (Figure 2a, and Appendix 2). This argument acts against the use of d13C measurements for long-term temperature reconstructions (Etien et al., in press)."

In 3.1.: "By contrast with GHD and cellulose d18O, d13C shows a significant auto correlation ( $R^2=0.22$  with one year lag and 0.20 with two years lag). Figure 3a shows clearly the strong influence of wood sample number and origin on d13C records (see Appendix 2). This dependency and the autocorrelation are the reasons why we have not used d13C data for the Tmax AMJJAS reconstruction."

12. 3.2 Calibration: 3rd paragraph after equation (2): I like to way the authors attempt to address the different sources of uncertainty (although this cannot replace rigorous calibration/verification exercises). However, the term "confidence interval" is not ap-

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appropriate here as confidence intervals are always accompanied by significance levels. What is the probability that the true values lies within  $\pm 0.55^{\circ}\text{C}$ ? How does this uncertainty vary over time? Uncertainties are larger when fewer data are available. How compare the  $0.55^{\circ}\text{C}$  with the overall variability of the time series? "Mean error" would be the more suitable term.

All these comments were taken into consideration in the new calibration section (3.2.)

13. 3.2 Calibration: More details on the applied bootstrap method is needed to fully comprehend how the analyses was done. Therefore, I cannot assess if this method has been applied properly.

The bootstrap method has been described with more details in section 3.2.

Corrected version: "The uncertainty associated with the linear model is estimated using a bootstrap method. Two thirds of the data (calibration samples) are randomly sampled with replacement; the best multiple linear regression is calculated on these data and the quality of the reconstruction is assessed on the last third of the data (verification samples). The uncertainty is obtained using the standard deviation of the verification residuals. After 1000 iterations of this method, we estimated a  $\pm 0.55^{\circ}\text{C}$  uncertainty on the linear model."

14. 3.3 Reconstruction and comparison with other reconstructions, 6th paragraph: It would add much value if the authors could discuss much more in detail how expectional the recent warming is in the context of the new reconstruction. This could be done by comparing recent and historic values along with their uncertainty (expressed as probabilities) or by using the Monte-Carlo approach.

The section was rewritten. We tried to show more clearly how our data show that the 20th century warming is unprecedented (by its duration in particular) and how it compares with the the extreme warm periods of the past.

15. 3.3 Reconstruction and comparison with other reconstructions, 6th paragraph:

How was the uncertainty estimate made of the temperature series of Central England (13.6+/-0.7°C) and the other temperature values? That's not clear enough in the text.

"Instrumental mean growing season temperature series of Central England (Tmean AMJJAS, available from 1659 to 2005) range from 10.9 to 14.9°C with an average level at 13.6 +/- 0.7°C (1 sigma)". 0.7 is the 1 sigma standard deviation of the mean.

16. 3.3 Reconstruction and comparison with other reconstructions, 8th paragraph: Why do the authors analyse the stability of the correlations just over four adjacent 100-year-periods? Running correlations would provide a much more detailed picture.

We do agree with this proposition. We did a 51 years running correlation. The result are presented in figure 5b and commented in 4.

17. 3.3 Reconstruction and comparison with other reconstructions, 8th paragraph: I believe the authors should be more cautious when assessing the quality of reconstructions. I got the impression that they try to assess Central England and De Bilt temperature series when they write "the quality of Central England and De Bilt homogenised data is very good". What verifies what?

This part was rewritten in order to show the differences between CET and the other temperature reconstructions considered in this paper:

Corrected version: "The comparison between our data and temperature estimates for Europe is highlighting. Before 1750, Meier et al.'s, Luterbacher et al.'s and Guiot et al.'s reconstructions are correlated with our reconstruction ( $R^2 > 0.4$ ). This feature is stable over 1600-1750. At the contrary, the correlation coefficients between our reconstructed temperatures and CET show a large decrease from 1750 to 1600 (before 1730,  $R^2$  is even inferior to the significance threshold). The construction of the CET reference "instrumental" record still remains a mystery, and is described by Manley himself as "built up [...] largely by the exercise of judgment on series of observations that are

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formidably open to doubt [...]". The comparison between our reconstruction and Manley mean growing season temperature data provides an independent control on the quality of CET data. We show that (i) our reconstruction preserves more variance back in time, at decadal to centennial scales, probably because it is not distorted by homogenisation or detrending methods; (ii) different reconstruction of growing season temperatures are consistent through time but diverge with the CET reconstruction from 1750 backwards. This comparison provides a strong and independent verification of the quality of the pioneer work of Manley".

In the last sentence of this paragraph the authors refer to "a problem occurring in the interval between World War I and II". This is very vague, has no new information and should be omitted.

We made this point clearer. In France, this time corresponds to reorganisations in the meteorological stations that have some consequences on the quality of the data. We wrote in the new version:

"Correlation coefficients are minima from ~1908 to ~1935. During this time period spanning World War I and II, it cannot be excluded that missing data alter the quality of instrumental record homogenisation. The moving of the meteorological stations from Normal Schools to airports that took place between 1920 and 1950, indeed yielded breaks and gaps in the thermometric series (Météo-France, pers. comm.). As a matter of fact, a warm bias in eastern France meteorological data was observed by comparison with other peri-Alpine temperature data for the 1940s, and was attributed to difficult observation conditions (R. Böhm, pers. comm.)."

18. 3.3 Reconstruction and comparison with other reconstructions, 3rd last paragraph: Why is it an artefact of the data used by Guiot et al. (2005) when the correlation with this reconstruction increases from the present to the past?

This sentence was removed. As suggested by referee 3, a running correlation -instead of 4 century-long ones as it was in the previous version of the paper- was used in

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the revised version. This kind of calculation does not evidence as clearly an increase of the correlation coefficients backwards. Moreover, the significance tests (bootstrap method) conducted on  $R^2$  values show that  $\pm 0.2$  variation of  $R^2$  is not significant. Therefore, the slight increase of  $R^2$  backwards is not meaningful.

19. 3.4.1 Instrumental period, first paragraph: Explain the meaning of AMJJAS when it occurs first, not here.

OK.

20. 3.4.1 Instrumental period 5th paragraph: p-values would be more appropriate (cf. earlier comment)

The p-values were added when needed.

21. 3.4.2 Evolution of power spectra with time: I don't think it's necessary to describe every peak of every record in detail. Moreover, cellulose  $\delta^{13}\text{C}$  is not included in the reconstruction, hence it is pointless to deal with its spectral properties in detail. I recommend to focus on the main features of this analysis that are necessary for the overall conclusions.

The  $\delta^{13}\text{C}$  power spectrum was removed from the figures and, of course, from the comments.

22. Conclusions and perspectives: Again, this chapter is too long and should be reduced to the new(!) points that arise from this study. I reckon this can be done in half of the length of this chapter.

We do not subscribe to this point of view. The conclusion and perspective section was a little modified. But, in agreement with referee n°1, we do not think that it needs to be shorten.

23. Figure 2b: State in the caption which uncertainties are considered, and which uncertainties are \*not\* accounted for.



Figure 2b is now figure 2a. We took into account your remark. The figure caption is now: "Records of tree ring cellulose isotopic composition (d13C and d18O, in permil) measured over pooled samples. The analytical uncertainty (+/-0.1 and +/-0.25 permil for d13C and d18O respectively) is displayed with grey shadowing."

24. Figure 3a: the curve of TmaxAMJJAS is the main result of this study. Why is the variance of the reconstruction lower than the variance of the measurements? Discuss this in the text. It is not sensible to mention all extreme warm years in the caption. Summarise the main conclusions of this data and discuss it in the text.

We took into consideration your remark and we have corrected reconstructed Tmax AMJJAS as follows:

The best correlation is obtained using all the dataset (1900 to 2000). The regression equation is as follows:  $\Delta T_{\max} \text{ AMJJAS} = 0.76 \pm 0.13 \times \Delta d18O - 0.09 \pm 0.01 \times \Delta \text{GHD} (1)$  ( $R^2=0.60$ ,  $n=91$ ,  $p < 2.2 \cdot 10^{-16}$ ) We have adjusted equation (1) in order to get identical variance and mean level for the reconstructed and instrumental Tmax AMJJAS for the period 1950-2000.

The occurrence of extreme warm years is largely dealt with in the new version of the text. Section 3.3 last two paragraphs: "Extreme years (those deviating by more than 1.5 sigma from the centennial average) have been identified on the reconstructed temperature, GHD and d18O records (Figure 3a). The years recognised as extreme are not the same in the d18O and GHD records (except 1686 and 1893). The heat waves do not"

25. Figure 4: This figure can be omitted. Mentionning the significance of the different periods in the text is enough (by giving the p-values).

It has been removed.

26. Figure 5a: I don't see why it is interesting to compare the new reconstruction with the 5 NH temperature reconstructions. Plus, it is not discussed in the text. This should

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be dropped.

We think it is interesting to compare our results with other temperature reconstructions in the same area. Maybe what is meant by referee n°3 is that the comparisons have to be focused on the Fontainebleau area. We have selected 5 proxy-based reconstructions in France or southern Europe in the new version. The comparison with our reconstruction is largely discussed in section 4.

27. Figure 5b: comment on figure 5a applies here as well. Is it the 5% level that is used? I assume the horizontal lines are significance thresholds. If so, please label them.

It was the 5% level indeed. Figure 5b has been redrawn and the significativity threshold at 5% is reported. It is mentioned in the figure caption.

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