

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

Anonymous Referee #3

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General comments

The topic of this paper is relevant and within the scope of Climate of the Past. Basically, a new temperature reconstruction for northern France is presented. The novelty of this approach is the combination of proxy records that have not yet been combined for that purpose to my knowledge. The applied statistical methods are standard though partly incomplete (see specific comments). However, clear statements (especially in the abstract and the conclusions) how this new reconstruction expands existing knowledge of (regional) past temperature variability is missing. Additionally, there is not much analysis on how the recent temperature change compares with this new reconstruction. It

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would add much value to see how this paper differs from other studies in that respect (e.g. Esper et al. 2002). Because of its length and structure, the text is not reader-friendly and I strongly advise the authors to shorten significantly the text and focus on the main new points. Moreover, I find the text poorly organised. There are repetitions, paragraphs not relevant to the actual topic, or at the wrong place (e.g. methods in the introduction). I also advise the authors to use less subtitles and especially to only briefly describe the results but then discuss them in a discussion chapter in more detail. This would help a lot to get the main points quickly.

As this paper has the potential to be a valuable contribution to the literature, I recommend publication after major revisions.

Specific comments

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 it is about.

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?

3. Abstract: Giving the uncertainty of 0.55°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.

4. 1. Introduction, 3rd last paragraph: $R^2=0.66$ 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

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significance of the relationship. This is an important point!

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

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.

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.

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

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.

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

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 R2 adjusted.

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 appropriate here as confidence intervals are always accompanied by significance levels. What is the probability that the true values lie within +/-0.55°C? How does this uncer-

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tainty vary over time? Uncertainties are larger when fewer data are available. How compare the 0.55°C with the overall variability of the time series? "Mean error" would be the more suitable term.

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.

14. 3.3 Reconstruction and comparison with other reconstructions: It would add much value if the authors could discuss much more in detail how exceptional 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.

15. 3.3 Reconstruction and comparison with other reconstructions, 6th paragraph: How was the uncertainty estimate made of the temperature series of Central England ($13.6+/-0.7^{\circ}\text{C}$) and the other temperature values? Thats not clear enough in the text.

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.

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 Cenral England and De Bilt homogenised data is very good". What verifies what? 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.

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

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with this reconstruction increases from the present to the past?

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

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

21. 3.4.2 Evolution of power spectra with time: I don't think it is necessary to describe every peak of every record in detail. Moreover, cellulose ^{13}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.

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.

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

Figure 3a: the curve of T_{max} AMJJAS 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.

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

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 be dropped.

Figure 5b: comment on Figure 5a applies here as well. Is it the 5% level that is used?

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I assume the horizontal lines are significance thresholds. If so, please label them.

I renounce to mention typing errors and English style improvements as large parts of the manuscript should be rewritten. I advise to have the manuscript corrected for English errors before publishing.

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