

***Interactive comment on “A 560 yr summer temperature reconstruction for the Western Mediterranean basin based on stable carbon isotopes from *Pinus nigra* ssp. *laricio* (Corsica/France)” by S. Szymczak et al.***

**Anonymous Referee #1**

Received and published: 24 July 2012

This paper presents some very interesting results and some new isotopic data for a site that has high dendroclimatic potential. The dataset is suited for publication as it appears carefully developed, but the analyses and interpretation are incomplete and contain some problems such that the manuscript is not acceptable for publication in its current form and would require some additional modifications. I believe that this additional work would be worthwhile as it would make a stronger submission and more robust resulting climate reconstruction/interpretation. Main points are listed below, with smaller edits listed at the end of the document. Correction for CO<sub>2</sub> concentration

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



Interactive  
Comment

changes. The authors state that the McCarroll et al. 2009 correction cannot be used on pooled series. This is not correct, whilst it is right to say that each tree has its own response (making the assignment of a single uniform incremental adjustment potentially incorrect), the approach can be attempted on pooled records. The authors can test this with their own data – first correcting the site records individually and then calculating the average, then correcting the raw average curve. The results should be very similar. The description of the correction is also incorrect and needs attention – it is not just removal of low frequency trends using a loess regression. The authors conduct an optimisation of the correlation with meteorological data using a range of incremental corrections. This is, as the authors point out, effectively “tuning” their data to obtain the best correlation. The authors identify this problem but then proceed with a non-independent calibration. Unfortunately, some of the instrumental data have a trend in them too and so the increase in correlation will be reflected in stronger correlations with the instrumental data and data subjected to the larger corrections. This affects the resulting regression equation (see below) and the data presented in the tables. Having tuned the data it is therefore incorrect to develop a regression model and use this to reconstruct past climates in this manner with limited instrumental data. A solution is to correct the data using McCarroll et al. (2009) as it does not rely upon optimising the correlation. If you contact the authors I am sure that they would be happy to assist with the correction. Linked to this correction, the resulting interpretation that a reliable reconstruction can only be derived from a carbon isotope chronology corrected with a factor of at least 0.012‰ppm-1 is incorrect for the reasons outlined above. Please edit this section. Section 4.3 describes the mean difference calculated between series corrected with the different correction factors as being equivalent to 0.8C in AD1497. This is unclear and requires a little further clarification. Is this the same series corrected with a range of incremental corrections? To this end, p2123 describes this correction effect through the increase in the correlation (and hence the resulting regression equation), so it is an important consideration. Table 1 showing the mean  $^{13}\text{C}$  for both corrected datasets requires clarification in the legend. Is this value calculated just over the in-

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

Interactive  
Comment

strumental period, the full record or the industrial period of correction? – without this information these 2 columns are meaningless. If the intention is to show little difference between the corrected records then it makes most sense to use the period of instrumental correction – even if the effect of incremental corrections is only large during the last c. 50 years. If the intention is to identify differences between the series then use a common period and discuss the differences in terms of site characteristics, altitude, pCO<sub>2</sub> etc.

Climate reconstruction: It is not clear if both the mean record and the site record for temperature reconstruction are corrected in the same manner (ie: same magnitude of correction). Please clarify this in the text what method was used for both. I am uneasy about using both a mean record of 4 sites to reconstruct cloud and a constituent single site being used to reconstruct temperature – you are using the same data to reconstruct two things within the same study. This point links also to the interpretation of the “antidromic” behaviour of the cloud and temperature records – if 25% of your signal is the same as your temperature signal then there is a high likelihood that at some point in your linear reconstructions that the two records will appear to diverge. It also links to your interpretation of decoupling of Corsican climate. Please reconsider recalculating using the three independent sites (excluding the temperature reconstruction from Asinao) and then reinterpreting. It would also be useful to calculate the expressed population signal on the 4 sites (pre-industrial period and corrected full record?) to determine the common signal. Rarely are 4 long replicated isotope series developed in a manner that enable this to be done and it would be interesting to assess the common signal before combining to a mean.

Correction of individual series: Please check the correction of your individual site series for <sup>13</sup>C and CO<sub>2</sub> changes – Looking at figure 1, the Ballone site shows a correction c.1850 when the other sites do not. Atmospheric <sup>13</sup>C does not change a lot at this time nor does CO<sub>2</sub> concentration, it looks like an error to me, so please check and recalculate as my suspicion is that the trendline should sit in the middle of the grey

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

region, as for the other three sites.

Minor points: P2113, Consider addition of Young et al. 2012 DOI: 10.1029/2010GB003913 as this provides a clear account of age-related trends. P2114 Please include the dates over which these means were calculated. You also use the dry lapse rate is this appropriate? P2115 Suggested edit: granite blocks and thin soils with low water-holding capacity. P2115 Delete “remarkably” P2116 Are these methods standard? If so, cite the source publication. This may seem a minor point, but in the reference cited (Szymczak et al. 2012) there is an error: “Following Boettger et al. (2007), a 17% instead of a 5% sodium chlorite solution was used to extract  $\alpha$ -cellulose instead of holocellulose”. This should actually be a 17% sodium hydroxide (NaOH) wash to dissolve hemi-celluloses. For correctness, the authors should clarify if this is the method used or if it was the method of Boettger et al. 2007. P2116 – Include the number of measures used in calculating the standard deviation. P2116 The period for calibration is short – two periods of 25 years is at the limit of many dendroclimate studies. P2116 “identical climate responses” – were they really identical – if not – please modify the text. P2117 Description of PIN correction (see notes above) P2117 See notes on use of Asinao for both temperature and cloud reconstruction. P2118 Please consider a little more discussion on the inter-site differences and altitude effects. P2118 L14 – The downward trend remains after correction for changing isotopic composition, not CO<sub>2</sub> concentration (physiological response), please correct/edit text. P2118 Please include r<sup>2</sup> and EPS data etc. when describing the similarities in the datasets. P2119 – “Correlations with precipitation are less influenced by the correction factor” – this indicates that the trend in the data may be making a contribution to the correlations or that precipitation may not be a dominant driver of isotope fractionation? P2119 The statement L23 relating to the high correlation of the mean is not well supported by the fig2 which shows Capannelle and to a lesser extent Asco routinely lower correlated than the other two sites. L2120 – see notes on terminology re: correction and correlation above – “a reliable reconstruction...” (L13, L18) P2120 L25, specify that for this site/study it appears that this is the case. I do not think this is so for all

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



Interactive  
Comment

sites. P2121 L24 1970 (delete th) P2121 How close are the reconstructions to the pith, is there a risk of juvenile timbers being included in these pooled records? P2121 L6 frequency P2122 and elsewhere; Consider use of an alternative word to “antidromic”, it does not seem 100% appropriate in this case. P2122 L12 reconstructions P2122 L20 – Do you mean correction for atmospheric  $\delta^{13}\text{C}$  rather than  $\text{pCO}_2$  here? P2125 L15 Delete “remarkable” P2126 What is the causal link here between the Maunder minimum and the climate of the Mediterranean? Is there firm evidence for a strong solar control throughout the record? P2126 L13 occurred P2127 Comparison of temperature and cloud – please see notes on comparing non-independent reconstructions above. P2128 L1 further P2128 and elsewhere; Consider use of ITCZ rather than ITC? Table 3 Caption : August Figure 1 – See note above re: c.1850 correction of Ballone. Figure 5 – Is the cloud reconstruction corrected for  $^{13}\text{C}$  and  $\text{CO}_2$  in the same manner as for the temperature reconstruction? – please amend figure caption as appropriate. Figure 6 – units for y-axis required. General: Check “plantphysiological” – correct to plant physiological etc.

---

Interactive comment on Clim. Past Discuss., 8, 2111, 2012.

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)