Authors' response to reviewers "French summer droughts since 1326 AD: a reconstruction based on tree ring cellulose δ 18O" by I. Labuhn et al.

We thank the reviewers for their thoughtful comments, which helped to improve this manuscript. We have taken their comments into account and made changes and additions to the manuscript accordingly. Notably, according to the recommendations of both reviewers, we have (1) extended the discussion about the quality of the reconstruction, including uncertainties associated with small sample size and offset correction; (2) combined the two site chronologies to a regional drought reconstruction; and (3) included comparisons with early instrumental records and other proxy data. Find below our answers to the reviewers' comments point by point.

Interactive comment on "French summer droughts since 1326 AD: a reconstruction based on tree ring cellulose δ 18O" by I. Labuhn et al.

Anonymous Referee #1

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Isotopes from oak tree-rings increasingly appear to be a viable source of high quality proxy climate information for the mid-latitudes. This manuscript adds to this body of information by extending two previously published records. In general I am in favour of publication but they are a few changes and additions I would like to see made first. The biggest flaw with this manuscript is the relatively small sample size prior to the late C19, this will inevitably limit confidence in the reconstruction and is probably responsible for the various data problems encountered by the authors (offsets and low inter-series correlations).

It is true that the reduction in sample size decreases the confidence in the reconstruction. In the revised ms, we are more careful with the climatic interpretations of this part of the chronology, and address more clearly the uncertainties associated with a small sample size (see comments 19, 20, and 21 below).

1. Since this paper was submitted a new European drought atlas, mainly based upon tree ring widths and density (I think), has just been published (Cook et al 2015). I feel that some discussion of this should now

be included and ideally a comparison with the published data over this region. For example I see no sign of the Cook et al (2015) fiftieth century mega-drought in either of these records.

A comparison of our reconstruction and the drought atlas is now added and discussed (Section 4.3; Figure 11).

The 15th century megadrought identified by Cook et al. (2015) occurs in north-central Europe (Southern Scandinavia, Germany, Poland), whereas the north and west of France show average moisture conditions during this time according to the drought atlas. Note also that the drought atlas uses only two tree-ring chronologies from France and the drought reconstruction we extracted for our study region from the atlas is based on interpolation.

2. Title says "a reconstruction" should read "two reconstructions", however you could (should) combine the two as the distances are not too great and produce one reconstruction. I will come back to this later.

We combined the chronologies to produce a regional reconstruction (see comment 16).

3. In the introduction you should cite and discuss Young et al (2015) Rinne et al (2013), both have used isotopes from oak to reconstruct precipitation not far away in the UK and are therefore highly relevant to your research.

These articles are now referred to in the introduction. In the revised manuscript, we also discuss the precipitation reconstruction of Rinne et al. (2013) in Section 4.3.

4. Page 5117 1st paragraph, should also discuss Loader at al. (2013). A strong common signal (e.g. EPS) and an accurate estimate of the population mean are not the same thing, but both are very important when reconstructing climate especially when using non-detrended proxy series. You probably need to do your level corrections because your sample depth is rather small and not due to any systematic offsets. Please discuss.

In the revised manuscript, we discuss the importance of a strong common signal and an accurate estimation of the population mean, referring to Loader et al. (2013).

5. Introduction. Some discussion of why d18o in oaks may reflect both temperature and precipitation is required.

P.5116 l.6-17 refer to relevant studies which explain the relationships between cellulose d18O and temperature/precipitation. A more detailed explanation of the links between cellulose d18O and drought and the combined influence of temperature and humidity is given in the beginning of our discussion. In the revised manuscript, this part of the discussion has been extended and further references were included (see comment 17).

6. Page 5118, second paragraph and Table 2. Are there longer climate records available with which to verify your proxy data? Why reconstruct SPEI (which I agree is better than PDSI) but I think SPI would be more meaningful as it is based upon a single climate parameter, you must have looked at this what is the correlation with SPI? Are there are regional records available in France equivalent to the UK England and Wales precipitation (EWP) and Central England temperature (CET) records as these might be very helpful in interpreting tour data.

We have tested different drought indices, and the correlations of cellulose d18O with the SPEI were consistently higher than with the SPI. The difference in correlation coefficients was between 0.09 and 0.17 depending on the site and the month(s) considered. Although the SPI can be considered a “simpler” variable, as it is only based on precipitation, the SPEI seems to be more representative of the drought conditions which influence cellulose d18O, as it includes both precipitation and evapotranspiration.

There are historical temperature and precipitation records from Paris (near our site FON), which go back to the 17^{th} century. In the revised discussion, we compare our drought reconstruction with an SPEI calculated from the Paris temperature and precipitation data in Section 4.3 and Figure 11.

7. Page 5118, lines 18 and 19. Why only those two combinations of months? If SPEI it is like SPI you can choose a month and lag it with a decay effect over a number of previous months which often is very effective.

The SPEI can indeed be calculated including a varying number of previous months. We have tested the correlations with different combinations of months and present here April-September, which corresponds to the growing season, and June-August, which yielded the highest correlations of all combinations and was therefore chosen as a target for the reconstruction. We slightly modified the text in Sections 2.1 and 3.4 to explain this more clearly.

8. Can you test your data against GNIP d18o data? Also a comparison with mean summer atmospheric pressure (e.g. 850 hPa) may be interesting. If your data are strongly linked to d18o in precipitation mean summer atmospheric circulation is probably the closest meteorological link.

For FON, the nearest GNIP stations have only 3-4 years of overlap with the cellulose d18O chronology. Near ANG, two precipitation isotope monitoring stations exist (Genty et al., 2014), which are not part of the GNIP network, and which provide 11 years of overlap with cellulose d18O data. A comparison of this precipitation d18O data with the ANG chronology is discussed in Labuhn et al. (2014). They concluded that the inter-annual variability of cellulose d18O is dominated by factors influencing leaf water enrichment, while the source water isotopic signal seems to be smoothed by the mixing of water from different seasons in the soil.

At our sites, we found no significant correlation between cellulose d18O and atmospheric pressure. Even if the atmospheric circulation is one of the controls on local precipitation d18O, and could therefore be linked to the source water d18O, there is no direct influence of atmospheric pressure on the isotopic composition of cellulose.

9. Page 5119, line 16-17. Is this hypothesis supported by the dendro dating?

We added references on the history of Fontainebleau castle which indicate a local origin of the wood used for its construction. This is also supported by dendroprovenancing. We correlated tree ring width of the samples used in this study with different reference chronologies. Correlations are highest with a local reference chronology and decrease with increasing distance from the study site.

10. Page 5119, Line 18, nine is a reasonable sample depth but two (and anything below four or five) is too low to draw any serious inferences with.

We agree that a sample depth of 2 is too low to infer any climatic information. With the limited number of available samples from the historic buildings, it was not always possible to keep a higher sample depth. In the revised manuscript, we mark the periods of sample depth < 4 in Figures 6, 7 and 10. The discussion now addresses the issue of sample depth in more detail (see comment 19, 20, and 21).

11. Page 5120. Line 25. I agree that it is very important to use only latewood from oak.

This is common practice in dendroisotopic studies using oak trees.

12. Page 5122, line 21. This is a very sophisticated approach, but I think simply splitting the data into two equal parts and doing the same statistics may be an equally good (if not better) test, especially if the climate data has a trend in it. Any reason why 2/3 and 1/3 instead of 50/50?

In a reconstruction based on such a calibration, it is supposed that the relationship between the proxy and the target variable did not change over time, therefore the model should be independent in time, and we decided to randomly sample the calibration and verification data sets. 3/2 and 1/3 was chosen to increase the number of data points used in the calibration.

We tested splitting the data set in 2 equal parts instead, as suggested by the reviewer, and the resulting verification statistics fall in the range of values found in our iterations. In any case, the method used here will not change the reconstruction, as all data are included in the final model after verification.

13. Page 5123. You have a big spread in your data and I think quite a low N in each cohort I think (the sample depth of the cohorts should be clearly presented I can't see it). Low sample depth is probably the main reason for your offsets between cohorts I expect. In your case some level correction is probably necessary but the best solution would be to increase N to ≥10 trees and hold this constant throughout your whole reconstruction (including calibration period).

We agree with reviewer #1 that an increased and constant sample depth would be highly desirable, but unfortunately the limited number of samples available from the historic buildings did not always allow for that. We added a recommendation for future studies regarding sample depth in the conclusions and perspectives. Plots of sample depth for each cohort were added on Figure 4.

14. More explanation of your pooling strategy and offset correction is required. Figure 4 is quite confusing. It would be much better if the two graphs were on the same x axis scale so that the reader could make some visual comparison between the chronologies. What does the dotted line mean in the top graph? Please explain. The dotted line in the bottom graph is where one of the series was only analysed at low frequency, were these data included in the mean value? This is also a period where the correlation between the two series is very poor.

The two graphs in Figure 4 are now presented on the same x-axis. The dashed lines in the top graph represent average values for overlap periods. There are three cohorts which overlap here, so two average values were calculated from each cohort for the different overlap periods (one of which was plotted as a dashed line to better distinguish them). The dashed lines were replaced by solid lines for clarity.

The data of cohort TW which was measured every 5^{th} year is included in the mean value of the ANG chronology. The correlation between cohort TW and the overlapping cohort GR is .63, the highest correlation found between cohorts.

15. Page 5124, section 3.3. You need to be careful with low filter correlations and must adjust the significance levels for autocorrelation, this is quite simple but necessary to determine significance.

We added the significance level for the correlations in Figure 7a, taking into account the reduced degrees of freedom due to autocorrelation in the smoothed series.

16. Page 5124, section 3.3. The relationship between the series is very good over the C20th when you have a reasonably high sample depth; I suspect the decline is due to a drop in N rather than any climatic effects. This would lead me to combine the two series to create a single regional reconstruction with a much greater sample depth; this should help to resolve the earlier part of both series with relatively low sample depth.

We added a regional drought reconstruction based on a combination of the two site chronologies. Consequently, a number of modifications were made in the manuscript: Correlations between the regional tree ring isotope series and average climate were added (Table 2, Section 3.4); a new model for reconstruction based on the combined chronology was built and validated (additions to Figures 8 and 9 and Table 3); the regional reconstruction is presented (Figure 10, Section 3.5) and discussed in comparison with other records (Figure 11, Section 4.3).

17. Page 5126, line 1. There should be more discussion, with references, of the links between d18o from oak trees and precipitation. I would avoid the word “drought” as this is not really possible to define with a summer proxy “dry summers” would be better.

A more detailed discussion of the links between precipitation and cellulose d18O, as well as other influencing factors, has been added in Section 4. “Drought was replaced by “dry summers”.

18. Page 5127, line 4. Explain normalisation, is this a z-score?

We replaced “normalisation” by “correction” and refer to Section 3.2 where the correction we applied is explained.

19. Page 5127, section 4.2, lines 26 and 27. I think both of these hypotheses are more unlikely than reduction of sample depth, the earlier high correlation may just be good fortune. If you sample depth were much higher and consistent and the results the same then I would give your two hypotheses more credence, see below.

20. With a pooled series, especially beyond instrumental data range, it is not easy to estimate how well the individual trees match and therefore how strong a common signal they contain: however series variability (or SD) is a good indications. Some trees simply do not respond as well as others to the same environmental conditions and this can occur for a variety of reasons. Generally in a mean or pooled

series high variability = good common signal and low = poor common signal, sample depth is also important here as low sample depth also usually leads high variability. So if you consider Figure 7: in the modern period you have a high N and a moderate SD, a good climate signal, and good common signal between sites, this is great and exactly what one would hope for, so good news for climate science. Then both your N and SD decline sharply, so not only do you have a much smaller sample but it also looks as if your trees are not responding in the same way, this could also mean that some of the timbers are from trees that respond poorly to climate (bad luck). With your data you cannot really say which hypothesis is correct, unless your N was held constant which it is not. As you SD increases so does your common signal between the two sites. So I would say that Figure 7 a and d explain one another fairly well the big difference being the modern part but the reason for reduced SD here is your high (adequate) sample depth. You could maybe do some more stats on the data in figure 4 to and look at the common signal between cohorts from both sites. It would be much better is the two panels in figure 4 were on the same scales and showed all the data from both sites to ease comparison. I do not think that such a major divergence in climate over the two regions is very likely. I would say that a decline below an optimal sample depth is probably the most likely explanation.

Reply to comments 19 and 20:

These comments of reviewer #1 are very important. It is true that the low sample depth during certain time periods may contribute to the disagreement between sites, and our hypotheses are difficult to test without either a constant sample depth or individual tree measurements of d18O. In future studies, it is advisable to take this into account and to question the long-held assumption that 4-5 trees are sufficient to obtain a representative climate signal, as well as the common practice of pooling to reduce time and cost of the analysis. This will facilitate interpretations and produce more robust climate reconstructions.

The sharp decline in inter-site correlation before 1800 could be linked to the decline in sample depth at this time. However, what supports our interpretation is the fact that the changes in correlation strength do not systematically occur with changes in sample depth or with the introduction of new cohorts. Furthermore, even though the SD is partly influenced by the number of trees, it increases and decreases simultaneously in both chronologies, indicating a possible climatic cause of this variation rather than a changing sample depth. Lastly, a comparison with grape harvest dates (indicator of summer temperature) from each sites is also in agreement with our interpretation.

Nevertheless, we agree that we need to be more careful with the climatic interpretation of the chronologies and extended Section 4.1 to discuss in more detail the quality of the reconstruction, including the impact of offset correction and changes in sample depth. We also added a paragraph in Section 4.2 to discuss these uncertainties as another explanation of the observed the (dis-)agreement between sites. We modified Figure 4 to put the two panels on the same time scale.

21. Figure 10. Some estimate of uncertainty should be added to these reconstructions, which should be considerably larger as your sample number drops. If you can improve these chronologies in the future to increase and stabilise the sample depth I think that these could represent two very strong precipitation records. With the data you have at present I think the best record would be derived by combining the two series. But this would have quite high uncertainty prior to about AD1800. Comparisons with any early instrumental data may help verification.

Our confidence interval is based on the differences between the measured and the reconstructed SPEI values during the calibration period (+-2 standard deviations of the differences). As we do not have individual tree d18O measurements during the period where instrumental data is available, we cannot quantify the increasing uncertainty when the sample number decreases. However, we marked the periods of sample depth < 4 on the reconstructions in Figure 10.

We have also included a comparison with early instrumental data from Paris, see comment 6.

Interactive comment on "French summer droughts since 1326 AD: a reconstruction based on tree ring cellulose δ 18O" by I. Labuhn et al.

Anonymous Referee #2

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<u>General comments</u>

The authors have used previously published and new d18O data of Oak latewood treering cellulose to reconstruct summer drought for 2 sites in France over six centuries. The sites are about 300 km apart and share much similarity in climate variability during the 20th century, but chronologies differ somewhat during earlier periods. Relatively wet and dry periods were identified and compared with grape harvest data. The analysis, correction, combination of data and calibration are all carefully done. The outcome of the study is a valuable contribution to understand better past hydroclimate variability. There are some limitations to the study which are partly inherent to reconstruction work, particularly when using historic material, but that should still be better addressed:

- d18O in tree-rings is statistically related to drought, as shown in the analysis, but nevertheless there are clearly more factors that are important. Source water d18O is dependent on large-scale hydrological processes and atmospheric circulation. Temperature is recognized as a major driver of this variation. As many studies have shown that the source water isotope signal is strongly reflected in the tree-rings, it seems a simplification to assume drought as the only factor. Because several climate factors act on d18O in combination, I would not expect that the d18O-drought relationship is stable over time, which makes it challenging to use the calibration function from the 20th in earlier centuries. Such questions need to be addressed in the manuscript.

It is true that the d18O of cellulose is determined by a number of different factors, and we do not assume that drought is the only one. The drought index SPEI was selected as a target variable for the reconstruction, because it yielded high correlations that were consistent between the two studied sites. This statistical relationship does not explain the link between the two variables, but the drought index seems to be a good representation of the atmospheric conditions that act on cellulose d18O, as it combines both temperature and precipitation. The d18O-drought relationship is probably not stable over time, but the same would be true for any meteorological variable that could be reconstructed, so this issue is not specific to drought reconstruction. Our records actually indicate that the relationships established for the 20th century might not have been the same in the past.

In the revised manuscript, a more detailed discussion of the various influences on cellulose d18O is added at the beginning of Section 4. We also discuss the temporal stability of the relationship between drought and cellulose d18O that has been established for the period of instrumental data, which is one of the uncertainties in the reconstruction.

- Due to isotope offsets, different cohorts of material needed to be corrected to be combined into a chronology. While I agree that this might be necessary, I find that the consequences of such adjustment has not been sufficiently analysed and discussed. What information is lost during offset correction? What does it mean for the drought reconstruction that low frequency is underestimated? How much is the correlation between the two chronologies changing (improving) when going from raw data to corrected data? This information could be useful as a general outcome because the combination of different records is still challenging and no established protocol available.

We have added a quantification of the correction by giving the correlation between series before and after the correction (Section 3.2). The correction greatly improves correlations, especially of the low-pass filtered data.

The consequences of the correction for the reconstruction are discussed in more detail in the revised manuscript (Section 4.1). The correction does not strongly affect the identification of dry and extremes (relative to the overall mean) of individual years. However, longer-term drought variability might be underestimated because part of the low-frequency variability in the cellulose d18O chronologies is lost in the offset correction.

- Two explanations are given to explain the divergent signal of the two sites in earlier phase. I think that methodological issue might be more important than indicated in the text. Maybe the authors overestimate the reliability of the reconstructions. Could the site conditions of historic material be different from recent ones? The offset correction affects this earlier phase and may interfere because it is different for the two sites. Would the combination of the records actually result in a more stable regional drought reconstruction?

We modified Sections 4.1 and 4.2 to discuss in more detail the quality of the reconstruction, including the impact sample depth and offset correction might have on the reliability of the reconstruction.

Furthermore, we combined the two site chronologies to a regional drought reconstruction, which is presented in Section 3.5 and Figure 10, and discussed in Section 4.3.

Specific comments

5115, l. 4 “algal booms” should be algal blooms

Corrected.

5115, l. 8 “In response to increased greenhouse gas concentrations, climate projections anticipate a marked increase in heat waves and droughts . . .” Not everywhere, needs to be more specific, otherwise the statement is wrong.

We now specify that the increase in droughts is projected in France.

5115, l. 19 no information on droughts “prior to 1950”. Meteo data go much further back, so there is information on droughts before that year

We changed the sentence to “prior to the instrumental period”.

5117, l. 5-12 References are missing. Discuss approaches in Gagen et al. and Hangartner et al.

The references were added in the introduction. The approaches proposed by Hangartner et al. (2012), which we also applied to our chronologies, are further explained in Section 2.3.

5119, l. 16 "The building wood likely originates from the neighboring forest". This seems important so please expand a bit on this. How likely is it that site conditions are similar to recent site considering the surrounding area?

l. 28 Same question for the Angouleme site. ("but a local origin of the wood can be assumed")

Reply to comments on 5119 l.16 and l.28: We added references about the history of Fontainebleau castle which indicate that wood of local origin has been used for its construction. For the buildings of Angouleme, the origin of the wood is not documented. However, dendroprovenancing supports the assumption of a local origin in both cases. We correlated tree ring width of the samples used in this study with different reference chronologies. Correlations are highest with local reference chronologies (around 0.50) and decrease with increasing distance from the study sites (ca. 0.20 with the a chronology 500 km away).

5123, l. 3 "The confidence interval around the reconstruction was determined based on the differences between the measured and the reconstructed SPEI values" Is this really constant over time?

The confidence interval is likely not constant over time. It depends also on sample depth, and due to a restricted number of timber samples it was not possible to keep the sample depth constant. We do not have individual tree measurements for the 20th century, where SPEI data is available, making it impossible to quantify the influence of sample depth. However, in the revised version we mark the periods of sample depth < 4 in Figure 6, 7 and 10.

5124, 3.3 Is the strong mismatch around 1700 related mainly to one cohort only (PE1 in Figure 4). Any issue with this cohort?

The time period around 1700 corresponds to the previously published part of FON (Etien et al., 2008, 2009) for which all trees had been pooled. For ANG, the period of mismatch consists largely of a single cohort. We therefore cannot say that the mismatch is related to one cohort. There were no issues with the crossdating of the cores for either site.

5124, 3.4 In climate analysis, SPEI is not sticking out as dominant climate factor, but T and P are also important. Did you try combing the records and correlate to averaged climate? This might result in a stronger and more stable relationship.

We combined the records of the FON and ANG sites and correlated them to averaged meteorological variables. The strength of these correlations are in the same order or magnitude as the correlations for each site. The results are added to the previous correlation analysis in table 2, and discussed in Section 3.4.

In Figure 7b, the low-frequency trends in the 2 records appear to be rather similar. It could be interesting to look at splines using higher cutoff than 10 years. A good match in the low-frequency would enhance the credibility of the reconstructions.

The low-frequency trends in the two chronologies are indeed similar. This already becomes evident in Figure 6, which presents different ways of stacking overlapping cohorts and the resulting chronologies. This figure shows that the applied correction seems adequate. While the raw data has opposing long-term trends leading to a divergence prior to 1700, the correction results in coherent low frequency variability between the two sites.

5126, first section: Are the cited papers really on drought reconstructions? I think not many studies really reconstructed drought from d18O. From the complexity of the d18O-source water signal, no simple relationship is expected, and that's actually why not many studies have used it for that purpose.

We cited these articles because they found similar relationships between tree ring proxies and different meteorological variables as our study. We modified the text slightly to be more clear about the fact that the cited studies do not present drought reconstructions.

5126, 4.1. This section is a bit vague and not very quantitative. How does the applied correction method affect the results?

In the revised manuscript, we discuss in more detail the differences between corrected and uncorrected series, as well as the implications of the correction for the reconstruction. We also present a quantification of the effects of correction, both in terms of absolute d18O values and in terms of correlation between the two chronologies for high and low frequency variability (Section 3.2).

5127, 4.2 Possible errors in the reconstruction should be given more discussion

We added a paragraph to this section to discuss the uncertainties of the reconstruction in more detail, see reply to general comments above.

5129, 4.3 Comparison to the grape harvest index is interesting, but it would be useful to consider other published drought reconstructions for comparison

No other high-resolution hydroclimate reconstructions are available in our study area. We included a comparison with historical temperature and precipitation records from Paris and an SPEI calculated from these data in the discussion. As suggested by reviewer #1, we also compare our records to the recently published "Old World Drought Atlas" (Cook et al., 2015).

Other precipitation reconstructions from different places in Europe (e.g. Cooper et al., 2012; Rinne et al., 2013; Wilson et al., 2013, 2005) have been considered, but they do not indicate clear common trends with the drought reconstruction presented here. A more detailed investigation of the differences between European drought/precipitation reconstructions and how they compare to present-day precipitation patterns is beyond the scope of this article, but would be an interesting topic for future studies.

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