Dear Editor,

Please find below our point-by-point response to your comments and the comments of the reviewers (*reviewer*, response). We thank both anonymous referees for the time and effort in reviewing our manuscript (cp-2020-39) especially in these challenging times. The comments, suggestions and feedback raised in the second round of the review process are highly appreciated as they help us to clarify our statements and to improve the quality of our manuscript. In our response, we have highlighted the relevant changes made in the manuscript with line numbers. Furthermore, we have uploaded a marked-up manuscript version.

On behalf of all co-authors, I would like to thank the Editor.

Best regards,

Daniel Balting

Editor

Comments to the Author:

Dear authors,

Thank you for revising your manuscript and for addressing most of the concerns that were originally brought up after the first round of reviews. As you can see from this second round of comments, there are outstanding issues that need further explanation in your final article. I consider those to be relatively minor changes.

In particular, please address the following:

1- make sure to clarify language when describing the EOF and double-check for any typos in the formula

2- further discuss and nuance the potential meteorological/climatological scenarios that can lead to the d18O signatures you obtained (see comments 2 and 4 from reviewer 1)

3- further discuss the issues related to pooling of trees, and also why 4 trees (2 increment cores) is a justifiable/sufficient number of samples for your analysis

4- read through the specific comments made by the reviewers and address them to the best of your ability

Thanks,

Julie Loisel

R: Dear Dr. Loisel, thank you for your helpful comments and recommendations. We have tried to address your above-mentioned points in our revised manuscript and in our response to the reviewers.

1: We have replaced the word "separate analysis" with "corresponding analysis" and corrected the typo in the formula [L164 & L174].

2. Based on the suggestion of reviewer 1, we have added an additional paragraph in the discussion section to emphasize more clearly the climate signal of $\delta^{18}O$ from tree rings[L430-436]. A detailed discussion is given in our response to reviewer 1 (comments 2 and 4).

3. We have added a section about pooling in the discussion chapter [L347-355] and also discuss why four trees are a justifiable number in our response to reviewer 3.

4. Please find our response below to the comments of the reviewers.

We are looking forward to your next evaluation of our revised manuscript.

Anonymous referee #3

The paper "Large-scale climate signals of a European oxygen isotope network from tree-rings" by Balting et al. is a very good example showed how proxy (isotope 18O) data obtained from tree rings can be used to quantify the past climate patterns over western and central Europe for the last 400 years.

I read the revised MS after first round of revision process and noted that the authors carefully followed the most (90-95%) of both reviewers recommendations improving all sections of their MS step-by-step.

The overall impression of the revised paper is very good. The logical structure of the manuscript, an in-deep introduction, a detailed description of the methods, visible connections between results and their discussion are noteworthy.

Taking into account the authors explanation why they used two terms (PCA and EOF) in their answers to the reviewers, theoretically, EOF is a part of PCA.

R: We are glad that the reviewer found our revised manuscript suitable for publication. We are grateful for the helpful comment regarding EOF and PCA. According to the suggestions of both reviewers, we have replaced the word "separate analysis" with "corresponding analysis" [L164].

I understand that the stable isotope measuring in the wood is a time-consuming and an expensive procedure but possibly in further works the authors will explain why four trees (two increment cores) (See section 90) are enough to guarantee the spatial-temporal statistical robustness of obtained time-series for the considered isotope network. Possibly high variation of 18O measurements even for one habitat is one of the reasons to lost a connection between spatial isotope pattern and ENSO signal before 1850.

Nevertheless I suggest the paper can be published as it is.

R: We agree wholeheartedly with the reviewer that measuring stable isotopes in the wood is time-consuming and expensive. In order to be able to ensure the quality and standards in this international project, with many laboratories involved, the methods applied (tree selection, sampling, pooling, isotope analyses) were harmonized and adjusted among the laboratories involved in the establishment of our data set, making it rather homogenous in this regard. How many trees are needed for a pooled chronology cannot be answered, as this depends on the spatial conditions as well as climatological conditions and inter-tree variability. The number of four trees was chosen to introduce a standard for the whole project. This is the only way to establish a tree-ring stable isotope network from more than 20 sites across Europe within a reasonable time frame. This problem can be overcome by comparison with the increasing number of isotope chronologies from other, additional sites and/or tree species being available to the community. Also, future comparison with reconstructions from other proxy archives, relying on various climate-parameter relationships, will help to test and challenge the data set and interpretation presented here. This would be a great topic for a second manuscript!

Anonymous referee #2

The authors improved the manuscript but still some issues remain:

1. EOF Analysis: Please check e.g. https://atmos.washington.edu/~dennis/552_Notes_4.pdf or other publications, text books etc: The empirical orthogonal function (EOF) analysis decomposes a data set in terms of orthogonal basis functions which are determined from the data. The term EOF analysis is also interchangeable with the geographically weighted PCA (principal component analysis). From reading the answer to reviewer 1 I get the impression that the lead author does not fully understand what the EOF (PCA) analysis is. May there are some typos in the formula presented.

R: We thank the reviewer for the comment/recommendation regarding the usage of the terms EOF and PCA. Based on the aforementioned comment, it can be stated that we understand EOF and PCA in the same way as the reviewer. The calculation of EOFs is definitely part of PCA, so we have replaced the word "separate analysis" with "corresponding analysis". Furthermore, we have removed the typo from the formula [L164 & L174].

2. A clear discussion of the former major comment 2 is missing in the manuscript: The authors need to say that a signal recorded by the trees can originate from different source regions during different seasons and that this can have strong implications in the interpretation of the proxies used. To make a simple example. You have a yearly value of delta18O and we just look at one year. The Value is obtained be a mixture of seasonal signals. In Europe the winter circulation and summer circulation deviate dramatically. So you can think of a multitude of combinations between a certain winter circulation (e.g. strong NAO leading to moisture transport from the Atlantic) and a predominant southwesterly flow leading to transport from the Mediterranean. So, your proxy is mixing both signals and I doubt that it is possible with one proxy to say something about this specific circulation of that at least the authors need to discuss the problems related with delta18O from trees.

R: We agree with the reviewer that it is helpful to have a more detailed description of the problems of the mixed signal of different seasons. This can influence the strength and the variability of the signal. In this respect, we have added a new paragraph in the discussion section where we deal with this issue [L430-436].

Regarding the circulation and transport issue please check our answer to comment 4.

3. The authors still not mention the problem related to pooling versus not pooling trees to measure delta18O. Please discuss this issue when introducing the data and also in the discussion section, see Hangartner et al. 2012 Methods to merge overlapping tree-ring isotope series to generate multi-centennial chronologies.

R: To address this issue, we have added a paragraph in the discussion [L347-355].

4. I disagree with the answers made to mayor comment 3. In winter you find as leading mode diploe structures over Europe. Furthermore, the authors state "The ENSO anomalies (either El Niño or La Niña) develop in winter and it needs 3-6 months to see a signal in the European climate. This lagged relationship is typical for many ENSO related teleconnections. This long transition from the tropical Pacific to central North Atlantic affects in turn the large-scale atmospheric circulation and as a consequence the climate over Europe, especially in spring and summer.". Again an effect on the large scale atmospheric circulation is not necessary as with an unchanged atmospheric circulation over the Atlantic the warming induced by ENSO you change the delta18O at the source region and then transport it to Europe. So I suggest that the authors shall be more careful in the interpretation of the data. They only show statistical relations ships (not causal ones) and sometimes they are rather weak.

R: We agree with the reviewer that it is necessary to be careful in the interpretation of the data. However, we do not agree with the explanation that the variability of the $\delta^{18}O$ signal in trees is based purely on changes in temperature of the source region. We think that this statement ignores several points. First, it is important to say that $\delta^{18}O$ in trees does not represent a homogeneous temperature signal. In our previous calibrations, we have achieved much higher local correlation with a drought index (e.g., the Standardized Precipitation Evapotranspiration Index) and VPD (vapor pressure deficit) than with temperature and precipitation. This indicates that $\delta^{18}O$ signal is much more a mix of temperature, precipitation, $\delta^{18}O$ in precipitation, moisture availability and other variables. In addition, the signal and the importance of the variables depends on the habitat of the tree. Therefore, the statement that an increase in temperature leads linearly to a change in $\delta^{18}O$ in the tree might be misleading, as $\delta^{18}O$ in trees depends on many other factors.

Furthermore, the transport does not remain constant. For example, El Nino can lead to a certain change in the transport (e.g., Fraedrich and Müller, 1992; Fraedrich, 1994).

Basically, the water vapor transport leads to a change in the $\delta^{18}O$ signature in the atmosphere. This is based, among other things, on the fact that ^{18}O tends to condense and precipitate first (for more information see Dansgaard 1964). Thus, if transport paths and the duration of transport from the source to the sink changes, this will lead to a change in the $\delta^{18}O$ signal detected by trees. We agree with the reviewer that the $\delta^{18}O$ source signal can also change, but it must also be considered that transport processes have a strong signature on the $\delta^{18}O$ ratio. We have added a paragraph in the discussion about the changes of the source signal [L430-436].

If the consideration were so simple that the $\delta^{18}O$ source value determines the variability of the isotope ratio and transport is negligible, we would get high correlations with the climate variables in the source area. But these do not exist.

5. The 20CR reanalysis is not described. It remains unclear what they use (ensemble mean or individual member, I guess it is the ensemble mean) I strongly recommend to use the individual ensemble members in this analysis so that the authors can assess the uncertainty of their results related to the uncertainty of the reanalysis product.

R: We thank the reviewer for the helpful comment. We have added a paragraph about the used data (ensemble mean) from the 20CR reanalysis (Compo et al., 2011) [L112-117]. Furthermore, we agree with the reviewer that further analysis of the uncertainties of this reanalysis product can be interesting and helpful for our analysis. As we would have to perform detailed analyses of the uncertainties with all the used climate data sets and time series, this is beyond the scope of the study. We can only refer to other studies that have investigated the uncertainties of individual climate products. Nevertheless, it is a great idea for a subsequent manuscript examining the impact of uncertainties in climate data sets on the relationship with $\delta^{18}O$.

6. How does Figure 7 look like for PC2? In section 4.5, you interpret it as a pure summer signal, so the question is if they confirm it with a similar analysis.

R: To answer the question of the reviewer, we have computed the correlation between PC2 and the modelled $\delta^{18}O$ in soil water and precipitation. Based on the figure below, it can be seen that a clear correlation pattern is only visible in the summer season.

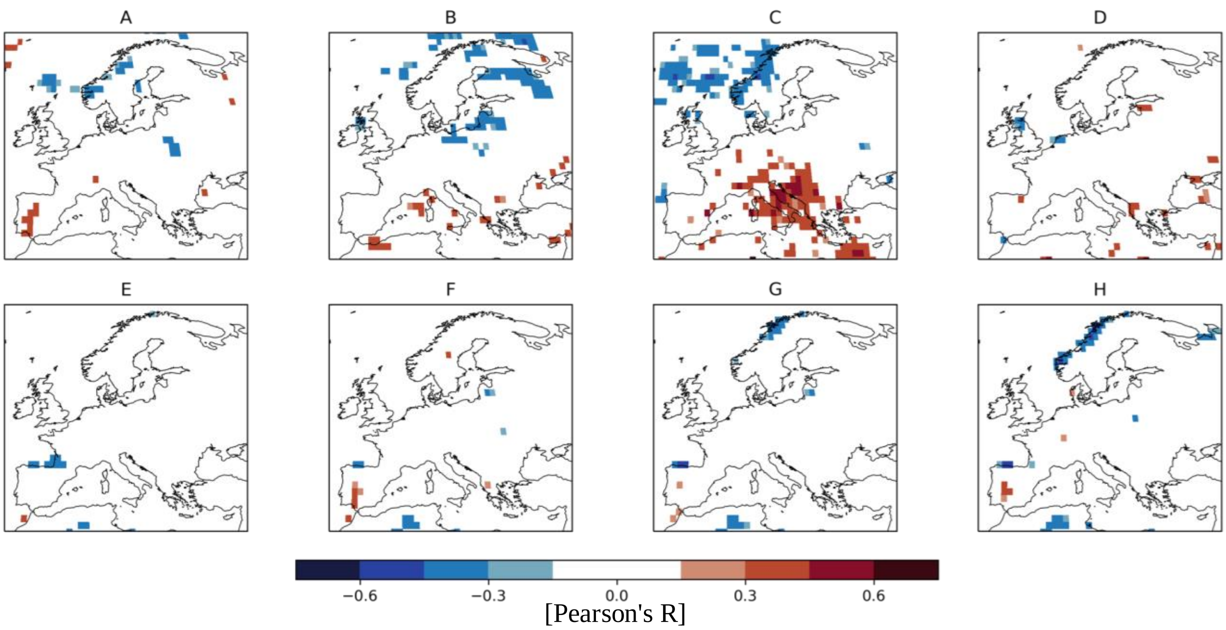

Figure 1: Links between the second $\delta^{18}O$ component and the modelled $\delta^{18}O$ in soil water and precipitation from nudged climate simulations with ECHAM5-wiso (Butzin et al., 2014). The upper row is showing the correlation between PC2 and $\delta^{18}O$ in precipitation for winter (A), spring (B), summer (C) and autumn (D). Panels E, F, G, H are the correlation maps for PC2 and $\delta^{18}O$ in soil water for winter, spring, summer and autumn. In all maps, the significant grid cells are coloured.

7. Extremes: It is just a wording issue but I think the 33% of the data are not an adequate definition to be extreme. (see also my previous comment).

R: We understand the reviewer's problem with the term extremes. As we have clearly defined the term for our study, it should not cause any problems of understanding for the reader [L192-194]. Furthermore, we have distinguished between high and low extremes in our study, each of which accounts for only approximately 16 % of the data.

8. Specific comments:
Around L60: please discuss in 1-2 sentences the nonlinearty of the ENSO response presented in the existing literature.

R: We have added two sentences to describe that the ENSO response is not stable and nonlinear with the corresponding references [L70-72].

L104: If you only use the 12 times series in your analysis – will you get similar results for the period 1850 to 1998 when using all data sets? If yes this would be good if not, e.g., the correlation is as weak as for the period 1600-1850 then the "non-stationarity" of the ENSO signal over time is just due the fact that you include more data after 1850.

R: The same result cannot be achieved with 12 time series which makes sense. The reason for this is that the described and analysed pattern from the $\delta^{18}O$ isotopes of the network requires the availability of the time series, which show a large eigenvector in the EOF plot. Without these time series, the variance that can be explained by the pattern is not available and therefore the pattern cannot be computed. However, since almost all the time series needed to calculate the pattern are available from 1750 onwards, the EOF pattern can be replicated with the available data from 1750 onwards. We are well aware of this uncertainty and this is the reason why we compare the periods 1750 to 1850 and 1870 to 1905 with Event Coincidence Analysis. The detailed discussion of the spatial as well as temporal limiting factors can already be found in the discussion chapter of the revised manuscript.

L110,L114: The wording "we want to" is weird.

R: We agree with the reviewer and have removed the "want to" from the sentences [L118 &. L121].

L121 and elsewhere: The authors mix using different tense - here, they suddenly sue simple perfect. Please correct the entire manuscript.

R: We have improved the used tense in the mentioned sentence and checked the entire for other tense mistakes [L129].

L204. Please remove "Discussion" as this is now a separate section.

R: According to the suggestion of the reviewer, we have removed the word "Discussion" [L212].

Section 3.4 I think this is more a evaluation of the data so should be mentioned earlier. (Just a suggestion)

R: We agree with the reviewer that the sub-section could also be moved further forward. However, our aim with the sub-section is to underline the previously explained results and to highlight them from a different perspective. Therefore, we have decided on this position for the sub-section.

L314: You did not show droughts, so just write dry conditions.

R: We have improved it in the revised version of the manuscript [L321].

Section 4.1 Discuss Hangartner et al 2012 here.

R: To discuss this issue, we have added a paragraph in the suggested subchapter [L347-355].

L340 is influencing –> influences
L357: is getting -> gets

R: We have changed the mentioned points in the revised version of the manuscript [L357 & L374].

L361 Please say how the teleconnection have changed in the publications Rimbu and Felis

R: We have added a better description of the cited studies [L377-381 & L385-390].

L384: "However as discussion above" reads bad.

R: We have removed "as discussion above" from the sentence [L407-408].

Fig 4,5,6: exchange column and row.

R: We think the reviewer means the wrong description in the figure caption. We have improved it in the revised version of the manuscript [Fig. 4,5,6].

References used in our responses:

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Dansgaard, W.: Stable isotopes in precipitation, 16, 436–468, https://doi.org/10.1111/j.2153-3490.1964.tb00181.x, 1964.

Fraedrich, K.: An ENSO impact on Europe?, 46, 541–552, https://doi.org/10.1034/j.1600-0870.1994.00015.x, 1994.

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