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Climate of the Past Discussions

Interactive comment on "Hydrological evidence for a North Atlantic oscillation during the Little Ice Age outside its range observed since 1850" *by* C. Martín-Puertas et al.

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We thanks referee3 for the comments and suggestions on our manuscript. As it has been mentioned in the answer to referee1 and 2, we will rewrite the methodological chapter in the revised version of the manuscript in order to provide more detailed information about how the chronology and the reconstruction were performed.

We disagree with referee3 on the consideration that our hypothesis is not supported by the results. The Cazorla site is the only forest in the Iberian Peninsula where trees older than 600 years have been found up to now, that's why there is only one single Pinus Nigra tree-ring chronology. However, we contrast our results with a compilation

of paleoclimatic records across the Iberian Peninsula, which describe direr conditions during the LIA and the MM.

We address the referee's comments below:

[R3]: The conclusions are drawn from just four sampled trees at that site. The regression model used for the reconstruction is also extremely weak, explaining only about 20

[AR]: As explained in the answer to reviewer 1, usually the amount of samples to build a reliable isotope chronology is between 4 and 5 trees, which is supported by a large number of publications (e.g., Gagen et al., 2004, 2007; Leavitt, 2010; Leavitt and Long, 1984; Treydte et al., 2001). Actually, one of the main advantages of using stable isotopes is that fewer samples are required to build a reliable chronology since the signal strength between samples tends to be much stronger. However, as we manifested before, we agree on rewriting the methods section including all the chronology details (such as EPS) instead of referring to previous publications. We also want to clarify that the stable carbon isotope chronology from NCZ and the summer precipitation do not only correlate with one grid point. Indeed, the correlation is higher using a regional precipitation average than data from a local station. These results are under revision for its publication (see answer to referee2).

[R3] [L138] A critical question is whether four trees are sufficient to capture the hypotheti- cal and unknown population signal for del C13 at the tree-ring site. This question could be addressed with several additional statistics, computed from the annual time series of del C14 from four different trees: mean between-tree correlation, EPS statistic, num- ber of trees needed for EPS of 0.85. This question cannot be addressed with the EPS computed for the ring-width indices used to develop the master chronology. The EPS must be computed specifically for the del C13 series.

[AR]: As answered to reviewer 1 and 2, all statistics related to the chronology (such as EPS) will be included in the revised version.

[R3] 2) [L513] Can you produce at least one example – a single year – showing a strong negative NAO accompanied by the combination of wet in North Africa and dry in Spain? The paleo-record (Fig 5f vs 5g) is interpreted as multi-decadal persistence of such conditions. This would perhap be more acceptable if at least one year of such conditions occurs in the period covered by instrumental data.

[AR]: Unfortunately we cannot provide any example of such conditions during the instrumental period since this pattern has never observed in the last century. Our idea about an extremely negative NAO is just a hypothesis to explain the Spanish droughts coinciding with solar minima during the LIA. We assume the negative NAO reconstructed for this period, however our hypothesis would explain the anomalous hydrological patter in southern Europe in contrast to the current negative NAO influence. This, we show three possible scenarios concerning climate forcing, which would support our hypothesis (Page 4159 Line 7-15)

[R3]: 3) [Fig 5a] Documentary records of increased “rogation”, or praying for rain, are used as supporting evidence for a dry MM. How stable and homogeneous is the time series of frequency of “rogations”, or praying for rain, as a proxy for episodes of drought and wetness over the centuries? Doesn’t the rogation frequency depend on religious practice, agricultural needs for water, and other factors that are not stable over time? What if the MM period just happened to occur when “rogation” was in vogue?

[AR]: Rogation series have shown a significant potential to study the frequency and intensity of droughts in the pre-instrumental period in different areas of the Iberian Peninsula. We have used the results already published by Dominguez-Castro et al., 2010 and 2011 (CP and CPD) in order to support our finding. In our opinion, whether these data are valid or not should not be addressed by us. We accept these results since they were already revised and published by the scientific community.

[R3]: 4) [L165 Fig 3] To me the time series presented in Figure 3 do not support the claim of a “strong” relationship, nor do they demonstrate tracking at various frequen-

cies. A statistically significant correlation, while indicating that the null hypothesis of no relationwhip can be rejected, does not necessarily mean the relationship is strong. In this case, only 20

[AR]: As mentioned to reviewer 2, we will provide a plot showing the agreement in the different frequencies domains.

[R3]: 5) [pgh begining L171] The second sentence does not follow from the first. These two sentences make two different points. One is that soil moisture carryover from winter to the growing season could mean that del C13 could depend at least partly on the winter precipitation. This is important because NAO is primarily a cool-season phenomenon. The second point is that precipitation in the summer is correlated with precipitation outside the summer. But the second point does not follow from the first, and so “Thus” is incorrect here. Besides, it is confusing to use correlation between summer and annual precipitation to demonstrate correlation of summer precipitation with non-summer precipitation. Some correlaton is expected because the summer component is part of the annual component. Why not directly correlate summer with non-summer precipitation?

[AR]: The correlation between summer and non-summer precipitation is r=0.25 (interannual), r=0.46 (15-yr filtering) and r=0.52 (20-yr filtering). Thus, the correlation coefficient rises as increases the filtering of the series and point out a similar pattern of decadal to inderdecadal variations of the summer and non-summer precipitation in the instrumental period (see answer to referee2)

[R3]: 6) [pgh beginning L144] The methods section is sketchy on details of the regression model used for the reconstruction. First, it unclear exactly which del 13C time series is the predictor for the model. I’m assuming it is an isotope series formed by pooling the wood from the four samples and getting a single del 13C value each year. But that is not stated. Second, how are the confidence intervals in Figure 5f computed. Do they represent 1 SE bars? 2 SE bars? The CI around annual predicted values can

be generated from the regression residuals by assuming a normal distribution. How was the CI around the annual reconstructed values used to construct the CI around the smoothed reconstruction in Figure 5f?

[AR]: See answer to reviewer 2.

[R3]: 7) [same section] Typical practice in regression is an analysis of residuals. Was such an analysis done and does it indicate regression assumptions were not violated (e.g., normality of residuals, non-autocorrelation of residuals, no structural dependence of variance of errors on magnitude of prediced value).

[AR]: Of course, distribution and trends in the residuals of the regression were tested. We did not consider that showing the summary plots would be of interest since they are not usually shown in publications. However, they could be added in a figure or mentioned in the methods setion.

[R3]: 8) [L294] Arguments presented here regarding hydrological time series in Figure 5 (c,d,e) are not supported by the plotted series. First, the lake level and flood series appear inconsistent with one another as indicators of moisture conditions. The period of low flood number in the first âĹij150 yr of series d appears to contradict the high lake levels in series c.

[AR]: Both Taravilla Lake and Guadalentin River have been interpreted as flood records by Moreno et al., 2008 and Benito et al., 2010. We have tried to show a compilation of the hydrological reconstruction carried out in the Iberian Peninsula in order to contrast with our findings. These records are not annual resolved and their chronological models have multi-centennial resolution. The interest in these flood reconstructions is to show that significant floods did not characterize the LIA and the MM in the Iberian Peninsula. On the hand we cannot discuss the differences between both reconstructions, but we might think that likely they can be caused by uncertainties in the chronologies, the type of geological record, local environmental conditions, and/or human impact.

[R3]: Second, the detrital input to Zonar Lake (series e) actually seems to reach a local maximum during the MM. This would seem to oppose the ideas of drought focused on the MM.

[AR]: The slight increase in the Ti influx into the Zoñar Lake during the Maunder Minimum could be due to human impact in the catchment (increased soil erosion by agriculture), which was high during the LIA. In addition, the age-depth model performed for this lake does not allow discussion at multi-decadal resolution and therefore variability within the LIA. We used the Zoñar Lake record to support the decrease in rainfall at AD 1360 (coinciding with 14C date). We interpret the decrease in the detrital input into the lake at AD 1360 as caused by a reduced runoff and, thus, lower precipitation.

[R3]: 9) [Fig 1] Poor line quality in the NAO+ map. The coast boundary is invisible – written over by the color patches.

[AR]: Thanks. We will improve the quality of the figure.

[R3]: 10) [L485 and Fig 4] See comment 6 above on the error bars for the reconstruction. The computation of the error bars should be explained in the methods section. Like- wise, the “21-year filter” used needs to be defined. Was this a Gaussian filter, spline, Butterworth filter? More generally, the methods section should be detailed enough to enable another researcher to replicate the results. In this paper, the methods are too vaguely described.

[AR]: This comment has been addressed in the answer to Referee2. Please see that in the Open Discussion.

The 21-years filtering was performed using a centered moving average.

As mentioned to the referee1 and 2, we will give an extended explanation in the revised version of the manuscript.

Interactive comment on Clim. Past Discuss., 7, 4149, 2011.