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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.

Anonymous Referee #3

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The authors argue that stable carbon isotope data from tree rings in Spain supports dry conditions in the Iberian Peninsula (IP) during the Little Ice Age (LIA). The dryness was purportedly accentuated during the Maunder Minimum (MM) – the late 1600s to early 1700s. The claim and findings run counter to some accepted ideas about climate anomalies during the LIA, believed to be a period of enhanced negative NAO and consequently southerly displaced storm tracks. These conditions would logically be expected to produce wetness over the Iberian Peninsula and North Africa. The hypothesis offered to reconcile the findings with other paleoclimatic records is a hyper-

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negative NAO during the LIA, such that the storm track was diplaced even further south than during a typical negative NAO episode. The extreme displacement is argued to have left the Iberian Penisula north of the southerly displace storm track, and dry.

The hypothesis of an exceptional southward shift of storm track with negative NAO in the LIA is novel, interesting, and worthy of study. The tree-ring evidence presented in this paper for dry conditions in Spain during the LIA and MM is, however, in my opinion, very weak. The crux of the authors' argument for dry conditions is a negative departure in del-C13 from a single Pinus Nigra tree-ring site. Moreover 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% of the variance of gridpoint summer (JJAS) precipitation in the region of the tree-ring site. These and several other points listed below suggest the conclusions of the paper are poorly supported by the data.

1) [L138] A critical question is whether four trees are sufficient to capture the hypothetical 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, number 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.

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.

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

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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?

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 frequencies. 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% of precipitation variance is explained by del C13. As for tracking at low frequencies, this could be quantified in various ways – for example by giving some correlations between low-pass-filtered time series of reconstructed and observed precipitation.

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?

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

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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?

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).

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 ∼150 yr of series d appears to contradict the high lake levels in series c. 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.

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

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. Likewise, 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.

11) [Fig 2, and others] Non-standard lettering of sub-plots. Why some figures lettered a,b,c,... from top down and others from bottom up?

12) The manuscript is not clearly written, and contains typos (e.g., L488: "per mile")

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