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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 #2

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General comments

I was happy to see that my general (as well as specific) comments overlap to a very large extent with the comments of reviewer#1. I will therefore in this review focus on those aspects that the authors have not addressed appropriately in their response, as well as a number of additional comments.

I agree with reviewer#1 that the 800-year long d13C record described in this paper could provide a very relevant contribution to the field of paleoclimatology due to its high resolution and its geographic location, but that it's relevance is undermined by a poor description of the methods applied and a lack of nuance in the discussion of the results. The authors have addressed some, but not all, of the methodological issues raised by reviewer#1 in their response, but not all responses were adequate in my opinion. Furthermore, I would like to see the high-versus low-frequency character of their reconstruction addressed in a more detailed and complete fashion in a revised version of this manuscript. Also, I believe that a climate-growth analysis of the tree-ring width data (rather than only the isotope data) should be provided in this paper. I provide more details below in the specific comments section.

The authors raise an interesting hypothesis in their discussion, that suggests a displacement of the maximum precipitation band during the negative NAO phase of the Maunder Minimum beyond the extent of its excursions over the instrumental period. However, in their discussion, the authors fail to address the issue of seasonality, which, in my opinion, is not minor in this case. The authors present a SUMMER precipitation record, whereas the influence of the NAO on precipitation on the Iberian Peninsula occurs predominantly in winter (and correlations with summer precipitation are very weak). Furthermore, the Morocco PDSI reconstruction that the authors use for comparison and that is used for the Trouet et al. NAO reconstruction, reflects spring (Feb-June) drought conditions and is thus not directly comparable with the summer precipitation patterns found for Casorla. If nothing else, I suggest that the authors at least include a discussion of the importance of seasonality as described above in their discussion.

Last but not least, the use of the English language needs to be improved dramatically before this manuscript is ready for publication in Climate of the Past. I suggest that the authors approach a native speaker for revision of their manuscript.

Specific comments

L61-62: Another useful reference here is: Raible, C.C., Yoshimori, M., Stocker, T.F., Casty, C., 2007. Extreme midlatitude cyclones 753 and their implications for precipitation and wind speed extremes in simulations 754 of the Maunder Minimum versus present day conditions. Climate Dynamics 28 755(4). doi:10.1007/s00382–006–0188–7.

L75: what do you mean by ‘solar cycle 23’?

L72-73: It can be seen that Within its range of variations, the maximum of precipitation maximum anomalies shift by about approximately 25 degrees latitude, from 62N to 38N. Please provide a reference for this statement

L82: . . .continuous hydrological proxy records. . .

L85: would be is

L127-132: I would be very interested in

1. Seeing a figure of this TRW record;

2. Seeing the details of this TRW record (RBAR, EPS etc.);

3. Seeing a climate-growth relationship figure for this record. The reason for this last request is that it is unclear to me why the authors go through all of the efforts of developing a d13C record, when TRW is also a sensitive climate parameter, especially considering that the correlations between d13C and precipitation are rather weak. I am assuming that the climatic signal in the TRW is even weaker, but it would be good to show this in a graph. Or if this is shown in another paper (?) a reference to this paper should be provided

L132-133: I agree with reviewer#1 that it is very unclear from the methods and figure 2 how many trees/samples you used for your reconstruction and which time periods they cover. Your response to reviewer#1 does not clarify your methodology enough. Did you use 4 trees (text) or 8 trees (Fig. 2)? If I understand it correctly, different trees were used for the periods pre- and post-1600. If this is the case, does this not increase the risk of a methodology-induced low-frequency variability? Especially because post-1600 is when you see the very low values in your reconstruction. The potential risks

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as well as the methodology itself need to be described/discussed in the text.

L134-135: according to Fig. 2, this statement is not correct: only the four youngest trees were pooled, not the 4 older ones. I find the combination of pre- and post-1600, pooled and individual, and annual vs. 5-year very confusing and the authors need to explain in much more detail what their reasoning was behind this combination of samples.

L149-150 and L168-170: the authors have not addressed the issue of how they calculated confidence intervals/uncertainties in their response to reviewer#1. This issue is crucial in my opinion.

L152-153: I think that the stability of the precipitation (and/or temperature) signal is very record-specific and thus needs to be analysed for this record specifically for instance through a running correlation analysis. Providing references for other studies does not suffice in this case.

L163: split-period procedure: what periods did you use?

L166-167: Fig. 3c only shows correlations in the annual domain not in different frequencies. It would be very interesting to see how the P and d13C correlate at lower frequencies (e.g. through correlation of average values over sequential 5, 10,. . . year periods) and I suggest the authors include such an analysis. Furthermore, fig. 3c shows that the amplitude of the d13C record is considerably lower than for the precipitation record. Why did you not scale the d13C record? The lack of amplitude and the consequences for reconstruction need to be discussed in the text.

L162-163: I agree with reviewer#1 that there needs to be a more extended clarification of why June-September precipitation was chosen for reconstruction. Precipitation in June is not significantly correlated with the d13C record (Fig. 3a) and I wonder if r-values with July-September precipitation would not be higher? I suggest the authors also include r-values for different combinations of months in Fig. 3a.

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L174-175: the authors here describe their reasoning for comparing summer P records with annual P records, but I'm not convinced that this is sufficient evidence. Summer P contributes greatly to annual P and it thus is logical that the two correlate strongly. It would be more interesting to see how summer P correlates with winter P and how the d13c record correlates with winter P.

L183-184: Fig. 4 does not show inter-annual variability, so this statement does not make much sense.

Fig. 4: why do you smooth the record with a 21-yr running mean? It is crucial that the reader sees the original, annual-resolution reconstruction that you have developed. Also, why does your solar variability record stop around 1940? Surely data are available over the most recent period? What is the temporal resolution of the solar record?

L232-233: 34.7% of what? Precipitation? Temperature? Winter? Annual?

L252: negative index of the AO/NAO-like

L267: Raible et al. 2007 (see above) and Trouet et al. 2012 are also good references for this statement. Trouet V, Scourse JD, Raible CC (2012) North Atlantic storminess and Atlantic Meridional Overturning Circulation in the last Millennium: reconciling contradictory proxy records of NAO variability. Global and Planetary Change 10.1016/j.gloplacha.2011.10.003

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