Clim. Past Discuss., 7, C2678–C2687, 2012
www.clim-past-discuss.net/7/C2678/2012/

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.

C. Martín-Puertas et al.

celia@gfz-potsdam.de

Received and published: 15 February 2012

We thank the referee2's comments, which will help to improve our manuscript. Probably we wrongly assumed that the chronology technical details were not of interest or excessive information for the audience of Climate of the Past; we tried to condense it and just show the most relevant characteristics of the reconstruction. However, and after the revisions, we become aware that the methodological section may seem incomplete and should be rewritten in order to provide more information about how the chronology and the precipitation reconstruction were developed. We address the referee's comments below:

General comments:

[R2]: 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 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

[AR]: A graph with different frequencies will be provided to better show the high to low frequency character of the reconstruction. An overview of the climate signals recorded in several tree-ring records can be found at Dorado Liñan et al (2011). In this study, the sensitivity of TRW to temperature is shown. In a later study (submitted for its publication), the effect of using different climate data sets (local station versus regional grid data) was also investigated: TRW is in every case sensitive to previous year summer to autumn temperatures. Precipitation or drought signals were not found. From the set of different variables at this site, only stable carbon isotopes were found to be sensitive to precipitation, and this relationship was also found to be stable. TRW shows up as a good proxy for temperature reconstruction but definitely not for past hydroclimatic variations. In Dorado Linan et al (2011 in press), a summary of main characteristics of each developed chronology can be found, including TRW and stable carbon isotopes. Nevertheless, a plot with the TRW series as well as the RBAR and EPS can be provided also as supplementary information if the reviewers consider it appropriated.

[R2]: 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). I suggest that the authors at least include a discussion of the importance of seasonality as described above in their discussion. 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.

[AR]: We reconstruct summer precipitation because the summer soil water balance is the limiting factor for tree growth at this site; however, summer soil water balance depends to a great extent on the water recharge during the wettest seasons since the summer precipitation is very low. We would like to clarify why we resort to the NAO pattern to explain long-term (multi-decadal to centennial) changes in our reconstruction and how we address the issue of seasonality:

1a) Summer precipitation is only the 10.1Following the Reviewer3's suggestions (see in the Open Discussion) we correlated summer precipitation and non-summer precipitation (January-May + October-December). The correlation is r=0.25 at inter-annual level but it is higher with increased filtering (eg., r=0.46, 15-yr filtering and r=0.52, 20-yr filtering) which suggests a synchronize pattern of decadal to inter-decadal variations of the summer and non-summer precipitation. It should be kept in mind that our focus is on medium to long-term hydrological changes rather than the inter-annual variability.

1b) On multi-decadal to centennial timescales, the driest period recorded in our summer precipitation reconstruction occurs from AD 1650 to 1750, coinciding with the MM.

Unfortunately instrumental series are not available for this period; however, a higher number of rogation ceremonies praying for rainfall carried our during the winter, spring and autumn seasons (Dominguez-Castro et al., 2010, 2011) support the MM was dry in the Iberian Peninsula, in good agreement with our finding.

Based on 1a and 1b, we consider the Cazorla summer precipitation reconstruction is able to reflect general humidity conditions for the Iberian Peninsula on inter-decadal to centennial timescales.

2) We do not focus only on the Maunder Minimum, but on the entire LIA (AD 1400-1850). Throughout this period, we found a curious relationship among long-term periods (multi-decadal) of negative precipitation anomalies and cooling episodes and solar minima throughout the LIA, suggesting common mechanisms and forcings behind. We base our NAO-hypothesis on those linkages, since: i) the NAO pattern is the best mode of variability known at present time, ii) the NAO is considered responsible of the European cooling during the LIA and iii) the NAO variability has been related to the 11-yr solar cycles and long-term solar minima such as the Maunder Minimum. We suggest a southern displacement of the storm tracks associated with an extreme negative NAO enhanced by solar minima within the LIA. The hypothesis about higher cyclone intensity during this period by Raible et al., 2007 and Trouet et al., 2010 could be also in good agreement with our hypothesis since it is suggested that the location of the cyclones is connected to the NAO pattern. With our hypothesis we only try to provide a possible atmospheric situation assuming the role of the solar forcing, which explains differences between the hydrological pattern in the Iberian Peninsula and Morocco during the solar minima within the LIA versus present day conditions.

[R2] 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.

[AR]: Although the reviewer's comment is relevant, previous studies have also made the reasonable assumption that at multi-decadal timescales, seasonal precipitation variability is correlated with the annual totals. For instance, Trouet et al. 2009. used the Morocco PDSI reconstruction, which represents the spring season, together with a Scottish speleothem-based precipitation proxy (Proctor et al. 2000) that does not distinguish between seasons and reflects annual precipitation variability. Both were used to reconstruct the variability of the WINTER NAO index during the last millennium (smoothed with a 30-year cubic spline). Both records appear anti-correlated and thus roughly display the well-known sea-saw pattern of winter NAO. Thus, the NAO index reconstructed by Trouet et al. 2009 could be showing the annual NAO index rather than the winter NAO state. In our study, we compared our reconstruction with the 20-yr smoothed Morocco PDSI reconstruction shown by Esper et al. 2007 because we also assume that our reconstruction reflects the moisture conditions of the Iberian Peninsula on multi-decadal to centennial timescale (see comment above).

Specific comments

[R2] L75: what do you mean by 'solar cycle 23'?

[AR]: Solar cycle 23 was the 23rd solar cycle of 11 years since 1755. An 11-yr solar cycle is considered from a state of low activity to the next low state. The solar cycle 23 lasted from 1996 to 2008 with a solar maximum in 2000. The winter 2009/2010 coincided with the onset of the solar cycle 24. So we will mean cycle 24 instead of 23 in line 75.

[R2] 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.

[AR]: We provide Figure 1 based on precipitation data from the NCEP/NCAR meteorological reanalysis in order to support this statement.

[R2] L132-133: I agree with reviewer1 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 reviewer1 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 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.

[AR]: Because of the limitation of the tree-rings thickness (see answer to reviewer 1) we could not use trees covering the whole study period and we were forced to combine different trees in different periods. The approach is similar to that used when combining samples from living trees and sub fossil wood: different samples cover different periods but there is always an overlap between the samples. Thus, all trees do not start or finish at AD1600, but there is always an overlap of least 50 years with annual resolution. This ensures that the transition from one sample to another is not determining the final shape of the chronology and reconstruction. The second challenge we were facing is a well-known issue in dendrochemistry: the problem of the replication and the technique to build the isotope chronology (see Dorado Liñan et al 2011 for more details). In the last 10 years it has been extensively used the pooling technique to build isotope chronologies, but pooling prevents the calculations of confidence intervals around the reconstruction, which is a major disadvantage. A second approach is to measure every sample individually but not for every year (e.g. each fifth year). Using this method the chronology will not be annually resolved but the individual measurements will allow building confident intervals for the final reconstruction. We decided to combine both approaches in order to obtain a final chronology, which is annually resolved and also has individual measurements that allow the calculation of the confidence intervals. After the reviewer's comments, we become aware that this approach could seem confusing and needs further explanations.

[R2] L149-150 and L168-170: the authors have not addressed the issue of how they calculated confidence intervals/uncertainties in their response to reviewer1. This issue is crucial in my opinion.

[AR]: The estimation of uncertainties in the reconstructions follows the standard considerations of regression analysis. There are two main components in the total uncertainty. One stems from the uncertainty in the estimation of the regression slope; the other component stems from the variance unresolved by the linear regression (the magnitude of the regression residuals). Under the assumptions that the error terms in the regression are not serially correlated and normally distributed, an estimation of the uncertain in the reconstruction for particular time steps can be found in many statistics text books (e.g. Storch and Zwiers, 1999). Usually, the first contribution is more important at longer timescales, since the residual variance is filtered out when considering longer averages. When the error term in the regression equation cannot be considered serially independent, for instance if the regression residuals indicate a serial correlation, the estimation of both contribution of the uncertainty has to take into account this serial dependency. One possibility to do this is described in Briffa et al. (2002). In their appendix they provide an upper bound for the uncertainty bound in the presence of residual serial dependence, under the assumption that the error term represents an auto-regressive processes of order one. Here, we have implemented numerically the underlying theoretical equations to estimate exactly, i.e. not an upper bound, the error bars under the same assumptions as in Briffa et al. (2002). As mentioned to the reviewer1, we will give an extended explanation in the revised version of the manuscript.

[R2] 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. [AR]: The stability of the precipitation signal was tested with the split period method. Moreover, we applied the Fisher r-to-z transformation (z) to test if the differences in the correlations of the two periods of calibration were significant, and they were not (results not shown). Thus, we consider that the issue of the stability of the precipitation signal is conveniently addressed on the paper. However, we agree that showing a running correlation graph will further support the climate signal stability test.

[R2]L163: split-period procedure: what periods did you use? [AR]: Periods are specified in Table1, but they will be also specified in the text

[R2] L166-167: Fig. 3c only shows correlations in the annual domain not in different fre- quencies. 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.

[AR]: We agree that scaling method retains more variance than regression methods in proxy-based reconstructions. However, we finally chose regression methods because of it is difficult to interpret the inflated error estimates (RE) when using scaling since RE has no lower boundary. In addition, there are not test of the RE statistical significance. It is true that the amplitude of the climate variations is enhanced when using scaling but the amplitude of the noise is also increased.

[R2] L162-163: I agree with reviewer1 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. [AR]: Actually, July to September displayed very similar values than June to September, but the relation was less stable. The meaning of the relationship between stable carbon isotope and summer precipitation has a clear physiological reasoning (see answers to reviewer1). Obviously, all possible combinations of summer months were tested and studied in detail but, since this manuscript is not focused on methodology, we consider unnecessary to show this information. We also believe that showing only the r-value would not ensure a correct interpretation since a correlation coefficient in this case requires of another information (eg., spaghetti plots and/or test of stability of the climate signal) to be correctly interpreted. Nevertheless, we will figure out a way to provide a summary of the analysis performed.

[R2] 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?

[AR] We will show inter-annual data. The reason of that smoothing was the comparison with the Morocco PDSI reconstruction, which is smoothed with a 20-yr filter.

We show d14C from tree-rings (Intcal curve) as proxy for solar activity. The dataset is smoothed with a 5-year filter (ww.radiocarbon.org/IntCal04

[R2] L232-233: 34.7

[AR] Winter Precipitation

[R2] 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: rec-

onciling 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.

We thank the referee 2 for the recommendation and we will include them in our discussion.

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