

Interactive comment on “A multi-proxy perspective on millennium-long climate variability in the Southern Pyrenees” by M. Morellón et al.

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Morellón and co-authors present a useful review of high-resolution climate records from the southern Pyrenees covering the last millennium. The paper synthesises climatic reconstructions derived from on several different archives (e.g. lacustrine, dendrochronological, glaciological/geomorphological). The findings of the review support a warm, arid Medieval Climate Anomaly and cool, humid Little Ice Age, and refine this division to include an intermediate, transitional period of fluctuating conditions. Given the interest in understanding spatial variability in climatic conditions during past climatic episodes such as the Little Ice Age, coupled with the fact that some of the records may not be familiar to the international reader, I consider the paper to be a useful contribution to Climate of the Past which merits publication. Overall, the paper is well-written

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and structured, and the figures and tables are necessary to support the text.

I have some recommendations for the authors to consider in revising the paper, which I appreciate are not all easy to address given the challenges inherent in the synthesis of multiple proxy records:

1. The authors should define the regional focus more precisely, and in doing so explain the wider significance of the choice of study area. Is the region exceptional or unusual in biological or climatic terms, and hence of intrinsic interest? Or do the varied bioclimatic conditions of the region make it a testing ground for understanding Mediterranean montane regions more generally? In part, the regional focus would appear to be specifically due to recent research advances in the area by co-authors on the paper– if this is the case it should also be made clear.
2. In section 3 the authors indicate valid reasons for limiting their review to the selected sites. I think it would strengthen the review and justify the selection of records if there were also a table listing the locations and types of other sites and records within the study area and highlighting the reasons (e.g. low sampling resolution, absence of dating control, etc.) for not including them in the study (expanding upon section 3.1, p3056, lines 18-21).
3. The authors might wish to comment further on the climatological/altitudinal coverage of the region offered by the chosen sites. Although the choice of sites appears to have motivated by coverage of an altitudinal gradient (i.e. the inclusion of Portlligat at 0 m.a.s.l.) the paper does not explore the extent to which climatic signals may be different between high and low altitudes, or deal with differences in dominant climatic forcing (e.g. temperature vs. precipitation) on natural systems which may be significant across the selected sites.
4. The authors might also try to deal more explicitly with the challenges arising from comparison of different types of record. Factors such as proxy sensitivity and seasonal bias, chronological precision and contrasting temporal resolution of the time series

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should arguably be introduced in a review of this nature when assessing the robustness of the identified climatic patterns. For example, similarities might be expected between the BER-SOB and Lake Redon temperature proxies (Figure 3), but it is not clear that an obvious correlation exists between the two, or at what timescale the two records can be usefully compared.

5. The authors' findings on hydrological response to solar forcing within the Little Ice Age disagrees with that proposed for the southwestern Mediterranean in Fletcher and Zielhofer (in press). The authors should comment on whether they consider the contrasting interpretation proposed there to reflect regional differences in climatic patterns, an incorrect interpretation of the available records, or whether uncertainties related to chronologies/temporal resolution mean the question cannot yet be satisfactorily resolved. As a logical statement, the authors' conclusion that "Although most of the reviewed sequences display colder and more humid conditions during the grand solar minima, the timing and intensity of the environmental responses is highly variable" (section 5.1, p3071, lines 17-19, emphasis my own) appears internally inconsistent.

6. The authors invoke the "longer response time of glaciers to abrupt climate fluctuations" (section 5.1, p3071, lines 15-16) as an explanation for an apparent delay in glacier advance compared with other evidence for cooling. Can the authors be more specific about the likely response times and justify their view? I am aware of some evidence to suggest that small, montane Mediterranean glaciers may in fact be extremely responsive to climate change (e.g. Hughes, 2008).

7. Overall, the arguments rely on visual comparison of the records, and I would encourage the authors to be critical about the way in which records are described, and more cautious about some of the statements of good matches between records. For example, high summer-autumn temperatures at Lake Redon are cited for the Medieval Climate Anomaly (Section 4, p3064, line 17), while in fact both some of the highest and lowest summer-autumn temperatures of the entire record are recorded during the interval. Also, at face value, the observation of "a good correlation" between the high-

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frequency Capdella and NAO reconstruction (section 5.2, p 3072, lines 13-14) is not entirely convincing, and it's not clear that the inverse relationship between regional precipitation and NAO phase implied in the conclusions of the paper is evident (presumably the statistical correlation is insignificant?). Perhaps the authors need to be more specific about at which timescale (decadal, multidecadal, centennial) they are highlighting similarities in the two records.

8. Finally, with regards to the conclusions about the role of the NAO, I think it is worthwhile to (a) indicate the extent to which a clear NAO signal is present or not in (recent/historic) regional rainfall records (given the complexity of seasonal rainfall regime, as compared with, for example, with western Iberia), (b) bear in mind that the benchmark used in this case for past NAO variability is itself a reconstruction based on dendrochronological precipitation proxies from Morocco and Scotland, such that detecting the fingerprint of past NAO variability will be hindered by any impacts of climate variability ultimately unrelated to the NAO, and (c) to note that while the multi-centennial-scale contrast between generally dry MCA and wet LIA appear to reflecting different prevailing phases of the NAO which is reflected in many of the records, the suggested "strong relationship" (Section 6, p3074, lines 13-14) between reconstructed NAO and regional hydrology at multi-decadal scale remains more challenging to demonstrate in a satisfactory way.

In sum, I consider the paper to be a useful regional review paper that will help advance the study of climate change during the last millennium in the western Mediterranean, but which can be improved by revision taking into account the above points.

References

Fletcher, W.J., and Zielhofer, C., in press. Fragility of Western Mediterranean landscapes during Holocene Rapid Climate Changes: CATENA, 10.1016/j.catena.2011.05.001

Hughes, P.D., 2008, Response of a Montenegro glacier to extreme summer heatwaves

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in 2003 and 2007: *Geografiska Annaler: Series A, Physical Geography*, v. 90, p. 259-267.

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