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Climate of the Past Discussions

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

L. Lopez-Merino

lolome@hotmail.es

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As the Referee #1, I consider that this paper is really interesting and provides a clear review of some data in the Southern Pyrenees during the last millennium, focusing on the Little Ice Age (LIA), using multi-proxy datasets. But, as Ref#1 points out, the manuscript is too much descriptive, repeating most of the described results in the discussion sections. A more analytical discussion of the data is necessary, as well as a critical review on the differences among the records and the accuracy of the different proxies. Not all the records and proxies respond to environmental change (climate- or human-induced) in the same way.

I also consider that including data from the northern slope of the Pyrenees will improve the research, as the mountain range does not know about countries, as well as the

potential impact of the paper, as a broad audience will be interested on it. In addition, it would be necessary to take into account the information provided by other archives, as peatlands, as they are also good archives of palaeoenvironmental change. In fact, the authors have not considered some peatland records because of the lack of a multi-proxy analysis or their exclusive focus on human activities (page 3056, lines 17-21). On one hand, although these papers may focus on human impact, the palynological data showed could be used in order to compare with data provided by Morellon et al. and understand if there is a regional pattern. For example, in page 188, Bal et al. (2011) say "There is no evidence of landscape changes during the Little Ice Age event because it is a complex period with important climatic oscillations that cannot be reflected in the mature forests". It would be interesting to know why some pollen records where sensitive and other were not, if the resolution is important to detect recent changes or there are other reasons to be or not sensitive to the LIA in very close areas. On the other hand, if the objective of the Morellón et al. manuscript is to use only multi-proxy studies I do not understand the inclusion of the Portlligat record.

With regard to Portlligat sequence, I think that is too complicated to make a comparison with mountainous records. In fact, the palynological content in such record could have two origins: air-borne and water-borne. The water-borne palynomorphs come to the archive through episodic fresh water inputs to the bay from a typical Mediterranean temporary stream that flows into it from its NE shore (Serrano et al. 2011). Then, the palynological spectrum may be reflecting not only the vegetation around the place, but also the plant communities from higher altitudes. Disentangling the provenance or importance of the different bioclimatic belts to compare this assemblage with the mountainous lacustrine palynological information is necessary to establish any similitude. Additionally, there are some aspects I am concern with: i) in Serrano et al. (2011) the age-depth model is different and more precise than in Lopez-Saez et al. (2009). The use of the later model could affect the discussion of the data (at least of the chronology). ii) The authors comment that Type 18 has been used for the reconstruction of humidity (page 3061, lines 2-4), but the reference cited by them (van Geel,

1978) does not record this information. The only study using Type 18 as a humidity index is the work developed by Mighall et al. (2006) on two ombrotrophic bogs from NW Spain. Ombrotrophic bogs are very special ecosystems that receive all of their water and nutrients input from precipitation, rather than from streams or springs. For this reason, if Mighall et al. (2006) found that Type 18 can be an indicator of humidity variations it is probably because of the nature of the archives (i.e. the type of peatland). But Portlligat is not an ombrotrophic system and has other water inputs, so I do not think Type 18 can be used as a direct indicator of humidity in such record. iii) If the LIA was a wetter phase in comparison with the MCA, it is difficult to understand why Quercus suber disappeared but Quercus ilex-coccifera type increased its percentages (Lopez-Saez et al. 2009), taking into account that Quercus suber needs more humid conditions than Quercus ilex spp. ballota, although less than Quercus ilex spp. ilex. But it is true that Q. suber is more sensitive to cold temperatures in winter than Q. ilex (Costa Tenorio et al. 2001). I think the authors should discuss this aspect if the Portlligat record is to be included in this review on mountainous sequences, because the three taxa could be living in this area.

In agreement with Ref#1, I consider that palynological data is the less reliable proxy for climate reconstructions, because during the last millennia the impact of the human activities has been significant. In fact, although Morellon et al. try to find similarities between the different sequences I see some problems. For example, in Lake Arreo Pinus percentages increase during the LIA, while in Estanya they decrease, and in Portlligat they show another pattern (but with the uncertainties of the age-depth model it is not possible to know if this is the real chronology for the LIA), as well as in Montcortes (Rull et at. 2011). In Basa de La Mora, Pinus values are not plotted, and I am sure that Pinus percentages have to be important because nowadays the surrounding vegetation in the area is composed by Pinus uncinata forests. In some of the study areas Pinus sylvestris would be the pine taxon, whereas in other areas it could be Pinus uncinata, or Pinus nigra, depending on location and altitude. If Pinus curves are to be used to infer climate conditions I would recommend to take into account the altitude, the species

ecological ranges and the possible changes in the tree-line due to the changes in temperature and humidity during the LIA.

With regard to La Basa de La Mora: I do not have access to Perez-Sanz (2009), and Moreno et al. (2011) is not published, so the reader does not have information about the chronological control in this record. Is it possible to plot the radiocarbon dates or other dating information? The rest of the sequences have already been published, so there is no problem to find the dating methods and age-depth models.

In page 3065, lines 5-7, Morellon et al. write “The occurrence of predominantly humid conditions during the LIA has been widely detected elsewhere in the Iberian Peninsula”. This is not completely right, as in some records in the NW part of Iberia dry conditions were assigned to the LIA, i.e. Ria de Muros (Lebreiro et al. 2006, Andrade et al. 2011), or Ria de Vigo (Alvarez et al., 2005). This could be because a reverse pattern in the Mediterranean and Atlantic regions of the Iberian Peninsula (also during the MCA). So, the previous sentence needs to be reformulated changing “Iberian Peninsula” to “Mediterranean Region” of the IP. The result of a very small area (Southern Pyrenees) should not be extrapolated to the rest of Iberia.

Page 3067, lines 24-28, Morellon et al. comment that thanks to the NPPs (Type 18?) it is possible to reconstruct an abrupt decrease of the humidity since 1750 AD, and this change was coeval with the decrease of several taxa. They include Quercus suber among those taxa, but its pattern is the opposite, as since ca. 1800 AD this taxon starts having higher values. Again, I don’t think the use of the Type 18 as a humidity indicator in this record is correct, and if it is also necessary to better constrain the age-depth model.

The authors point out in page 3070 (1-3) that “(. . .) the reviewed sequences during the LIA suggest that colder temperatures and at least, more positive water balances and/or increased runoff dominated during periods of diminished solar activity, the so-called grand solar minima (. . .)”. But in Moncortes the conditions are more arid during

this minima (Diatoms C:P ratio), in Estanya the arid phases are not coincident with the reconstructed salinity and not all the solar minima are coincident with moments of higher humidity. In Basa de La Mora there is lower runoff during such minima, and in Capdella the higher moments of reconstructed precipitation are not coeval with these minima too. The uncertainties of the age-depth models, and the sampling resolution could be responsible for that. Other causes are also possible, but the authors need to discuss this topic and be consistent with the data they have.

Finally, I strongly believe that there is a lot of potential in this paper, but I miss more criticism on their own data and discussion and comparison with other nearby records studied by other research teams.

- Please unify: multiproxy or multi-proxy, tree rings or tree-rings. - Page 3054, lines 18 and 20: Quercus ilex instead of Quercus rotundifolia or Quercus ilex-rotundifolia. - Page 3054, line 21: Genisteae and Cistaceae should not be in italics. - Page 3061, line 12 and Page 3062, line 3: Pinus uncinata Mill. or Pinus uncinata R.? I think it is Pinus uncinata Ram. Besides, you did not write such taxonomic information for the rest of the species in the whole manuscript. Be consistent.

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