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Climate of the Past Discussions

Interactive comment on "Quantitative reconstruction of precipitation changes in the Iberian Peninsula during the Late Pleistocene and the Holocene" *by* Liisa Ilvonen et al.

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The discussion paper by Ilvonen et al. entitled "Quantitative reconstruction of precipitation changes in the Iberian Peninsula during the Late Pleistocene and the Holocene" presents new pollen-based precipitation reconstructions from the Iberian Peninsula which are relevant for the wider study of palaeoclimate of the western Mediterranean region. Given the importance of moisture balance for vegetation in the region, this paper addresses an interesting and valuable subject. Also, in light of the strong environmental gradients and climatic diversity of the Iberian Peninsula, it is surprising that there are relatively few studies to date exploring climate reconstruction with this spe-

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cific geographical focus. The chosen methods are appropriate for the study with use of two transfer function reconstruction approaches (WA-PLS and Bayesian-based). The paper is concise and clearly presented. The study presents some valuable insights into general trends in reconstructed precipitation levels and engages with current debates in Holocene palaeoclimatology regarding the main climatic signal in Holocene vegetation changes. The approach and findings should be of interest to the community of scientists researching past climatic change in the western Mediterranean region. I would encourage the authors to consider the general observations and comments below, as well as the more specific comments listed, if they seek to enhance the impact of the study in a revised paper for publication in Climate of the Past.

The paper sets out an aim of testing “contrasting interpretations” (p2, line 34) about Holocene climatic conditions, citing the works of Mauri et al., 2015 and Samartin et al., 2017. As far as I understand it, the discrepancy between these cited studies relates to the timing of maximum summer warmth during the Holocene and contrasts between previous pollen- and chironomid- reconstructions for the (western and central) Mediterranean region. I think this discrepancy should be spelled out more clearly in the introduction because some readers may not be familiar with that debate. Going further, the potential implications of overlooking the role of precipitation (in terms of conflating humid- and cool- conditions during the mid-Holocene) could be also made more explicit. Towards the end of the paper, the authors suggest that their findings “do not contribute directly to this debate” – but I suspect that this might not be so clear cut, and the authors might wish to consider that statement (and the implications of their findings) further.

The authors have chosen to focus on precipitation as the target for reconstruction and they present some good reasons why precipitation should be a strong determinant of vegetation cover across the Iberian Peninsula. However, in order to advance the aforementioned debate, it would be helpful to demonstrate that precipitation indeed does a better job than other climatic variables such as summer and winter temperature at explaining the variance in the modern pollen data (variance partitioning). There are some strong patterns in the residuals shown in Figure 3 which would benefit from further analysis and explanation. I'd also be interested to know whether the authors considered a drought severity index (e.g. scPDSI), aridity index (e.g. PANN/PET) or soil moisture index (alpha) (see e.g. Dai, 2011; Cramer and Prentice 1988). Ultimately, temperature and precipitation should interact to determine moisture stress for plant life, most notably during the drought season where the precipitation input is lowest and the evaporative demand is greatest. Conceptually at least, a moisture index might perform better than precipitation alone as a predictor for vegetation composition. In practical terms, precipitation may nevertheless be the best available predictor since the site-specific evaporative component of water stress may be poorly constrained (limited documentation of wind speeds, soil moisture capacity, etc). In some places in the text, the interpretations do not appear to take into account these real-world interactions, and the summary might be too simplistic, e.g. "the predominant driver of vegetation patterns in the Mediterranean region is water availability and not summer temperature, and this is a fact which must be borne in mind when assessing any feature in pollen-based temperature reconstructions" (p11).

I would be interested to read a little more about the training set used. Were the samples collected and analysed specifically for this study, or compiled from previous studies? Were the samples analysed by one person / one research group, or if not, how did the authors deal with harmonisation of pollen taxonomy? Are they confident that critical identifications such as evergreen vs deciduous Quercus have been made and recorded consistently? The methodology described for "averaging" across multiple moss polsters at each site is quite distinctive and I wonder whether the signal derived in this way has been evaluated in any particular previous work? This could be cited if so. I am familiar with the study of Adam and Mehringer (1975) advocating a similar averaging approach for surface soils, but not for polsters. Then, the authors cite the European Modern Pollen Database as a source of more details on the samples, but it isn't clear if data has been derived from that repository or not. Overall, these details are important for evaluating the quality of the training set. It is good that the authors acknowledge the limitation of different sample types between the training set and the fossil material. My own previous research has been strongly critiqued in this regard, although the pragmatic necessity was similar (critique of Fletcher et al., 2010 in Birks et al., 2010). The authors here are correct that developing ideal training sets in this region remains a challenge. Finally, it would be worth commenting on why a regional training set, rather than a continental-scale training set (cf. Wei et al., 2019) was chosen to address the particular objectives of this study.

My overall impression from the seven reconstructions is one of heterogeneity in the results – and I do not find all of the synthesis statements and descriptions of the findings entirely convincing. For example, the “synchronous rise of Pann values” around 11000 cal yr BP is not so clearly evident. I think that the paper could benefit from some further approaches/efforts to illustrate the common patterns (and site specific differences) better. The use of coloured bars indicating formal stratigraphical division of the Pleistocene, Early, Mid and Late Holocene in figures 4 and 5 doesn’t help the reader to visualise where each record reaches maxima, minima, etc. It might be more helpful to colour code each record into above and below average sections, $\pm$1/2 standard deviations around the mean, for example. Going further, it might be valuable to support the written statements, for example about the declining general trend of Pann over the last 5000 years, with numerical summaries or boxplots for the different intervals to illustrate these patterns and help convince the reader that they are robustly expressed and significantly different from other intervals.

Related to this previous point, it seems that the discussion is quite rigidly organised around chronostratigraphical subdivisions, even where there is limited relevant data, e.g. one reconstruction only pertaining to the Lateglacial (cf. section 3.3.1), no signatures of the 8.2 event (cf. section 3.3.3). The authors might seek to emphasise the new contribution resulting from the new reconstructions, focusing more on both commonalities and individual features with respect to the range of values, timing of maxima and minima, patterns of variability, etc.

Finally, I would be interested to know why the authors chose these seven sites for this study and not a wider set. While they represent a fair spatial transect across the Iberian Peninsula, the addition of further sites might certainly help discriminate the common trends and patterns (cf. comments in previous paragraphs). Due to the length of the records and chronological issues discussed, only half the records are relevant for the discussion of Lateglacial and/or earliest Holocene intervals of interest. Futhermore, only four of seven sites are located inside the Mediterranean bioclimatic zone (and only two in water-stressed settings, as per Figure 1) where precipitation is most critical for plant growth. Finally, the sites are located at different elevations from 0 to 1500 m a.s.l., which adds further complexity for the interpretation. The paper seeks to test ideas developed in works adopting a more extensive sampling approach (e.g. Mauri et al., 2015) – so it would be useful to discuss further whether the choice of seven sites here is due to a very strict set of selection criteria. Ultimately, the messages about common Holocene trends could be strengthened by additional reconstructions from other sites.

Specific comments

1. Page 1, line 19 "100% higher" – does this mean "double"?

2. Page 1, line 21-24. "In general, our results suggest that. . ." – the manuscript does not really return to this overarching parallel between warm high latitude and humid Iberian conditions explicitly in the discussion or conclusions. It would be good to develop this further, and consider a possible climatological mechanism for the connection.

3. Page 2, line 1 – I'm not sure the Mediterranean can itself be the transition area from Atlantic to Mediterranean – needs some rewording

4. Page 2, line 29 – give select references for the type of climate variability or events implied here

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5. Page 2, line 33 – expand on “contrasting interpretations” to clarify what this study is seeking to test

6. Page 3, line 4 – can reconstructions be “fragmentary”? perhaps better “rare” or “sparse”?

7. Page 3, line 12 – the paper drifts in usage between Iberian Peninsula and Spain – better to stick to one or the other, probably IP as the geographical entity. Here, for example, the surface area of Spain as a country seems irrelevant.

8. Page 3, line19 – “south and east”?

9. Page 4, Line 19 – in Figure 1 indicate where the Eurosiberian and Mediterranean regions are.

10. Page 5, Lines 14-15 “Given that our seven pollen records. . .” – the reads as though the decision to reconstruct precipitation was a function of the availability of records, but surely the scientific aim was to reconstruct precipitation and the sites were selected accordingly? Reword according to the intended meaning.

11. Page 6 , Section 3.1 There are quite strong linear patterns in the residuals shown in Figure 3, which are not discussed in the paper – are these linked to temperature, site elevation, etc? The authors should comment on this and the implications for the reconstructions.

12. Page 7, line 13. “summer temperature records” indicate from where (geographically). . .

13. Section 3.3.1. This section is rather long relative to the amount of new contribution from the one site – can it be made more concise? The relevance of the section on Fagus on Page 8 is not clear, for example.

14. Page 8, lines 13 and 18 – what is the difference between “steppe vegetation” and “open vegetation” with respect to the implied contrast “By contrast. . .”?

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15. Page 9, lines 8-13. I don't really follow how a "synchronous rise in Pann" is shown at Quintanar, San Rafael and Navarrés-3 at 11000 cal yr BP when the latter two records begin around that time and don't show any rise as such – need to clarify the key finding here

16. Section 3.3.3. It's not entirely clear to me that this section is merited. The 8.2 ka event has not been introduced in the manuscript as a particular focus of interest, and it would require careful justification in any case with respect to the sampling resolution and age uncertainties of the selected records to demonstrate that this impact could really be tested or detected with the available records. In the end there is no substantial contribution of this study in relation to this event. Regarding the point that "accurately dated high-resolution pollen records are needed" – the authors could make reference here to works by Combourieu Nebout et al. (2009) and Fletcher et al. (2010) which do identify vegetation changes in forest cover for SE Iberia around the 8.2 ka event and also give quantitative estimates for PANN anomalies associated with this events.

17. Page 10, Line 27. Given that the main finding of the Renssen et al. (2009) work cited was spatial and temporal complexity and variability in the Holocene Thermal Maximum, I think the simple equivalence of the high Pann interval in Iberia and the high latitude HTM isn't immediately comprehensible here and should be more precisely described and discussed.

18. Page 11, lines 22-28, the authors discuss human impact during the last 500 years, but then evidence agricultural activity since 7500 cal yr BP – it doesn't seem quite related in terms of the timescale or cultural setting; please clarify/prioritise whether this section is about the long record of human activity or the intensification of disturbance in recent centuries

19. Conclusions – the authors emphasise "strong spatial and latitudinal gradient during last 15 thousand years" – this gradient may be implicit but hasn't really been discussed or illustrated – this should be developed in the discussion to justify it as a conclusion.

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20. Conclusions – for the finding that the "Late Pleistocene is characterized by rapid shifts in Pann values" the authors should indicate that this is based on one site only

21. Figure 1 – it would be helpful to indicate the site locations for other data presented in the paper, such as the lake records shown in Figures 6 and 7

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