Comments on "Holocene climates of the Iberian Peninsula: pollen-based reconstructions of changes in 1 the west-east gradient of temperature and moisture"

The manuscript provides new insights in the field of paleoclimate studies, and in particular, in quantitative paleoclimate reconstructions. The authors have used a recently developed technique (fxTWA-PLS method) for reconstructing three climatic variables (MTCO, MTWA and plant-available moisture) in the Iberian Peninsula during the last 11.5 ka.

Main points

One of the main goal of the paper is to use the recently developed fxTWA-PLS method (Liu et al., 2020). Using this method provides new knowledge in the area of quantitative paleoclimate reconstructions, and therefore, it is one of the main points why the paper is worth publishing. However, since this is a new method and very few pollen records have used this methodology (as far as I know: Liu et al., 2020; Wei et al., 2020; Prentice et al., 2022), more information should be included. For example, the authors mention that the WAPLS suffers from the tendency to compress the reconstructions towards the central part of the sampled climate range (Line 87-88) but they do not mention that the WAPLS is robust to spatial autocorrelation (Telford et al., 2005). Has this method been demonstrated to be robust to spatial autocorrelation?

With respect to fossil pollen records, the authors did a great work compiling a large number of sites, using different data sources such as the European Pollen Database (EPD), Pangaea and even contacting directly with the authors from the original studies ("Author" in the excel file "Iberia_pollen_records_v3_0307.xlsx"). However, the authors should have also checked the relatively new and opened ACER database (Sánchez-Goñi et al., 2017), which provides high-resolution global-scale fossil pollen data. Did the authors checked the high-resolution Spanish pollen records from this database? In addition, other new high-resolution Holocene Iberian pollen records should have also been included in the paper, such as lake Medina or Padul (both in southern Spain), especially when the number of fossil pollen records in the southern Iberian Peninsula is not as good as in the north, which could lead to uncertainties in the reconstructions and interpretations (as you mentioned in Lines 350-354).

I really appreciate the work done on the re-calibration of the age models. The authors have calibrated the age-models of the fossil records based on the newest IntCal20 calibration curve (Reimer et al., 2020). This work along with the removal of the samples with large uncertainties (stnd error >100yr) make the paper reliable from the point of view of the age-model and age uncertainties.

As we know, the pollen-based quantitative reconstructions are controlled by the modern pollen training dataset, and thus by the modern pollen vegetation. Although the MTCO reconstructions appear to be consistent with climate model simulations and consequence of insolation forcing (Lines 233-237), I wonder whether the MTCO reconstructions are completely reliable. Looking at the statistical performance of the MTCO (R^2 0.75 for Component 4, Table 1), the MTCO reconstructions should be reliable. However, the reconstructed MTCO anomalies at individual sites (Fig. 3) do not show a clear E-W trend (neither at low nor high altitude). This seems to be a consequence of the recent MTCO variability over a NW-SE (or E-W) transect (Figure 2), which shows no clear E-W changes in temperature. Actually, the MTCO in Figure 2 seems to be related

with elevation changes in Iberia, with the lowest temperatures in Pyrenees, Central and Iberian mountain chains, etc. Although the winter temperature can influence the vegetation at high-altitudes, at low elevations it is well known (you have also pointed it out in the Discussion) that the vegetation in the Iberian Peninsula is strongly influenced and controlled by the precipitation and moisture availability, and not by the winter temperature. The authors have pointed out a similar problem in the Discussion with the MTWA (Lines 260-268). Therefore, taking into account the issues related with the influence of the MTCO (and even MTWA) in the Iberian vegetation, at least at low-altitudes, the authors should consider whether including the MTCO is correct from a scientific point of view.

The authors have briefly compared their moisture reconstruction with speleothem records from the Iberian region (Lines 310-320). Since the study is based on pollen records, it would also be interesting to compare and discuss similarities/differences with other Iberian pollen records showing humidity changes throughout the Holocene period. In particular, comparing the results with other recent pollen-based quantitative moisture index or precipitation reconstructions from the Iberian Peninsula (e.g., Ilvonen et al., 2022) would show a more complete picture of the quantitative precipitation/humidity changes in the region and would definitely improve the Discussion section.

The authors have included a paragraph about the impact of CO_2 on plant physiological processes and how this affects the reconstructions. However, as the paper deals with Holocene records, they have not included any reconstruction that takes into account the effect of the CO_2. I agree with that, but then: what is the purpose of including this paragraph? The authors should avoid taking about a methodology that has not been used in this paper. I suggest removing this paragraph or, they could briefly explain that the variability of CO_2 during the Holocene is very low, and therefore, the effect of the CO_2 has not been taken into account for the moisture reconstructions (in contrast to other reconstructions, such as Wei et al., 2021).

I strongly recommend including the CCA plots (not only the numerical results as in the Table 2) in order to observe the relationship between the climatic variables and the pollen data.

The Variance Inflation Factor (VIF) analysis is used to demonstrate that the climatic variables are independent from each other. Your VIF results suggest that the collinearity between variables is not high, and therefore, variables seem to be independent from each other. However, the authors should better explained the VIF analysis. There is no single mention about the VIF analysis in Methods. In the current version of the paper, the methodology should be improved and these issues/questions should be clarified in the main text. Since the current version of the article is short, the authors could further clarify the methodology.

Minor points

Line 49: Add other references about new Iberian Holocene records related with changes in moisture conditions (e.g., Schröder et al., 2020; Ramos-Roman et al., 2018).

Lines 113-118: You should specify which taxa have been removed.

Line 131: “a modified code from SPLASH…”

Line 140: Remove the Doi number. Include the citation: Harrison et al. (2022)

Line 173: “The variance inflation factor (VIF) scores…”

Lines 173-174: Add reference.

Figure 2: Figure caption: use acronyms for “m above sea level”, for example “m a.s.l.”.

Figure 4: You must include the references for the insolation values. Where are insolation values taken from?

Table 2: The methodology related to this table needs to be better explained. For example, include in Methods the reason for using the VIF analysis.

REFERENCES

Harrison et al. (2022). Pollen data and charcoal data of the Iberian Peninsula (version 3). University of Reading. Dataset.

Ilvonen et al. (2022). Spatial and temporal patterns of Holocene precipitation change in the Iberian Peninsula. Boreas.

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Reimer et al. (2020). The IntCal20 Northern Hemisphere radiocarbon age calibration curve (0-55 cal kBP). Radiocarbon.

Sánchez-Goñi et al. (2017). The ACER pollen and charcoal database: a global resource to document vegetation and fire response to abrupt climate changes during the last glacial period. Earth System Science Data.

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