

Revisions on egusphere-2025-4494.

The manuscript by Sanmiguel-Vallelado and colleagues “Unlocking the potential of pollarded oaks: A 375–year hydroclimate reconstruction from north central Spain” aims to presents a 375-year hydroclimate reconstruction based on pollarded deciduous oaks from communal dehesas in north-central Spain. The study is original and potentially important, showing that traditionally managed pollarded trees can retain a strong precipitation signal at stand level and can be used to reconstruct November–June hydroclimatic variability in a region where long lowland records are scarce (as some locations of Spain seem to be). The chronology is long, the latewood signal is robust, and the comparison with documentary drought evidence adds value. The overall premise is therefore compelling and timely.

However, at the same time, the manuscript would benefit from major revision before being considered for publication. The main concerns do not relate to novelty, but to some aspects of the reconstruction design and interpretation that require clearer justification (i.e. methodology). In particular, the pooling of two species and two sites into a single chronology, the calibration/validation framework based on different instrumental products, the use of quantile mapping, and the interpretation of extremes and pollarding effects all need more careful treatment.

Overall, the manuscript addresses an important gap in Mediterranean dendroclimatology by focusing on lowland dehesa systems and by highlighting pollarded oaks as a potentially valuable archive for hydroclimate reconstruction. Chronology statistics seem to be strong enough, with long RWI and LWI series and high EPS values, and the latewood index shows a clear relationship with November–June precipitation. The study also explores whether pollarding weakens the climate signal at the tree level and whether this propagates to the stand chronology. This is an interesting and useful contribution. The comparison with large-scale hydroclimate products and with roagation records further increases the relevance of the work.

However, several methodological choices underlying the reconstruction need stronger support.

Major comments:

Pooling of two species and two sites (only). The study combines *Quercus faginea* and *Q. pyrenaica* from two different woodlands into a single chronology. Although the sites are geographically close, they differ in species composition, and it is not fully demonstrated that pooling them does not blur the climate signal. Because the reconstruction relies on the pooled latewood chronology, separate site-level and species-level chronologies, together with their intercorrelations and climate responses, should be presented. Some references, if any, would strong these findings.

The reconstruction is calibrated using FIC precipitation and validated against CRU. While understandable, using different datasets for calibration and validation makes it difficult to interpret model skill. A more standard approach would strengthen the results, such as split-period calibration/verification using a single dataset, parallel tests with both datasets, or cross-validation (e.g. leave-one-out).

Quantile mapping is applied to correct distributional biases and better capture extremes. Since the manuscript emphasizes extreme years and prolonged wet/dry periods, this step is important and should be better justified. It should be clarified whether the analysis of extremes is based on the raw or bias-corrected series, and how sensitive the results are to this correction.

The manuscript concludes that pollarding does not affect the stand-level signal. This is plausible, but the evidence is limited (again, some additional references would strong the findings of the MS). The argument

appears to rely on asynchronous pollarding among trees, yet the temporal distribution of pollarding events is not quantified. Providing a summary of the proportion of trees affected through time would strengthen this conclusion.

In addition, extreme years and prolonged wet/dry periods are defined using fixed thresholds. Because these results are central to the manuscript, a short sensitivity analysis using alternative thresholds would be useful.

Rogation records come from surrounding locations rather than from the study sites. The comparison is useful, but the interpretation should be framed more cautiously as regional coherence rather than direct local validation.

The manuscript indicates that data and code will be available upon request. Public archiving of chronologies, reconstruction data, and code would improve reproducibility and long-term usefulness.

Some minor comments:

Section numbering in the Methods appears inconsistent (Section 2.4 missing).

Several language issues and typos should be corrected.

The final regression equation should be reported explicitly.

Since only one core per tree was extracted, potential implications for within-tree variability should be discussed more clearly.

The discussion of latewood performance could be linked more directly to the empirical results.