

Response to Reviewer 1

The reviewed manuscript presents a new oxygen stable isotope chronology measured in cellulose extracted from annual growth-rings of oak trees, which originated from the area of Letea Forest, Romania. This chronology is over 200 years old and was used to reconstruct the August Standardised Precipitation Evapotranspiration Index (for an accumulation period of 9-months - SPEI9) in the area of central and eastern Europe. The paper also discusses the interannual and decadal variability evident in the obtained palaeoclimatic reconstruction, resulting from the occurrence of dry and wet periods associated with large-scale atmospheric and oceanic circulation patterns. The new stable oxygen isotope chronology proved to be an effective tool for reconstructing past hydroclimatic variability in the Letea Forest of the Danube River delta and in central and eastern Europe. This makes the reviewed article an important contribution to the issues of past climate research.

The manuscript has an appropriate text structure, is written in clearly formulated language and the individual chapters present the subsequent topics in sufficient detail. The accompanying table and Figures enable a better understanding of the descriptions. The authors also refer to previous studies of a similar type in the neighbouring areas, discussing and comparing the results obtained. The article is of high scientific value and I recommend it for publication.

We want to thank the reviewer for the appreciation/suggestions/comments/feedback that helped us improve our manuscript, and for taking the time to read and review our paper. Moreover, we are pleased that the reviewer clearly emphasizes the scientific importance of our manuscript.

Some 'specific comments' and 'technical corrections' are contained in the attached version of the manuscript.

We carefully revised all the suggestions and the different technical annotations from the attached file, we agree with the reviewer and will implement them in the revised version of the manuscript by improving our manuscript according to the reviewer's suggestions.

We confirm that all the technical annotations indicated by the reviewer were implemented in the revised version of the manuscript, while for the comments and suggestions please see below a point-by-point response.

Minor comments:

sp.

Was it possible to determine the species of tree during sampling?

We added the oak species information in the revised version of the manuscript. Please see the revised manuscript.

Sodium hydroxide is usually used to extract alpha-cellulose, while organic solvents are usually applied to remove the resins and extractives. However, "findings by Rinne et al. (2005) suggest

that the 2-step extraction with NaClO₂ and NaOH can make an extra solvent extraction step unnecessary" (Helle et al. 2022).

Thank you for the suggestion. We improved this section in the revised version of the manuscript.

It would be good to describe the chemical preparation in more detail and to specify which fraction of cellulose was extracted (alpha-cellulose, holocellulose).

Thank you for the suggestion. The reviewer is correct. We have added details of the extraction and changed the sentence accordingly.

Poorly readable map symbols (Danube River, Danube Basin, Country borders, Elevation), too low resolution of the lower map, lack of a and b symbols on maps.

We improved the resolution of Figure 1 in the revised version of the manuscript.

Please add the formula by which the calculations were carried out.

We added more information about how the calculations were made and cited the proper papers.

Are these values negative?

No, $\delta^{18}\text{O}$ in tree ring cellulose are positive values.

For which months or seasons?

Thank you for the suggestion. In the revised version of the manuscript, we indicated the months and seasons, and improved this section.

The beginning of the sentence does not justify the conclusion reached: "which means that the $\delta^{18}\text{O}$ variability in oak tree-ring celluloses in the studied area depends primarily on the moisture conditions".

Thank you for the suggestion. In the revised version of the manuscript, we improved this section according to the reviewer's suggestion.

No letter designations (a-e) in the Figure.

Thank you for the suggestion. We added the letters in the revised version of the manuscript.

It would be good to replace the word validation with the word verification in all descriptions in the Figure.

Thank you for the suggestion. We improved the figure according to the reviewer's suggestion.

No letter designations (a-b) in the Figure.

Thank you for the suggestion. We modified the figure accordingly.

Wrongly phrased sentence.

Thank you for the suggestion. In the revised version of the manuscript, we improved this sentence according to the reviewer's suggestion.

Are the measurements of individual trees or other values shown at the top of the figure (together with the master chronology)?

Thank you for the suggestion. In the revised version of the manuscript, we included more information about what is represented in the figure. Please the revised version of the manuscript.

Response to Reviewer 2

The authors present a new drought reconstruction for Eastern Europe in this manuscript, based on $\delta^{18}\text{O}$ tree-ring measurements. Collectively, the authors possess strong knowledge and expertise in the methodological aspects of the manuscript (lab and statistical work). Therefore, I have only a few minor suggestions regarding the methods and analyses performed. However, my main concern relates to the relevance of the study, which I believe the authors might have overlooked. What new insights does their study provide? What knowledge gap are they aiming to fill? Why is this important in the context of what we already know? What does your reconstruction tell us? The manuscript would greatly benefit from better contextualization of the results and a less methodological approach.

We want to thank the reviewer for the appreciation/suggestions/comments/feedback that will help us improve our manuscript, and for taking the time to read and review our paper. We appreciate the constructive comments and in the revised version of the manuscript, we are confident that we can improve our manuscript according to the reviewer's concerns.

The title “A long-term drought reconstruction based on oxygen isotope tree-ring data” exemplifies my concerns. Unfortunately, it is not an appealing title; it sounds like just another reconstruction study. While it may indeed be another reconstruction, 1) it also focuses on a region (Romania, Eastern Europe) that has been largely overlooked and underrepresented, and 2) it shows no strong trend in recent decades, which could be further discussed in the manuscript.

Thank you for the comment. In the revised version of the manuscript, we improved the title of the paper to be more appropriate and more precise. Regarding to the second point, we partially agree that the observational data for August SPEI9 shows no significant trend overall, but our reconstruction does capture the extremely dry period recorder after 2000, which is in agreement with a recent report published about the state of climate in Romania (<https://www.infoclima.ro/starea-climei-ro-2024>). We have added this information in the revised version of the manuscript.

The authors also present very advanced (and nice) analyses, including the link between the $\delta^{18}\text{O}$ chronology and atmospheric circulation, but the manuscript lacks some explanation and contextualization (beyond their own studies). In this regard, I feel that the introduction could also be reformulated to incorporate more of these atmospheric drivers of $\delta^{18}\text{O}$.

In the revised version of the manuscript, we improved the introduction according to the reviewer's suggestion and we included explanations of atmospheric drivers of $\delta^{18}\text{O}$.

Finally, in the comparison with other reconstructions, I agree that the high-frequency domains align, but I am not as certain about the low frequencies, trends, and values. Brázdil et al. (2016) show low SPEI values (<0) since the 1950s, which aligns with most studies indicating a drying trend since the mid-20th century. The present study shows low SPEI values only since 2005, implying that the latter part of the 20th century was relatively humid. Indeed, the reconstruction

shows quite high SPEI values compared to the observed values for the period 1980–2000. Perhaps some comments and reflections on this discrepancy would be interesting.

Thank you for the comment. In the revised version of the manuscript, the section with the comparison with other reconstructions was improved, and more explanations about the differences because of variations in the specific methodologies employed, proxies utilized, and the seasonal focus of each study were indicated.

Also, we want to indicate the observed low-frequency differences among the analyzed reconstructions may be attributed to their varying temporal scales. Our study employed a longer time frame (SPEI9), while the others utilized shorter periods (SPEI 6 or SPEI 3), therefore the extremes from the summer season that are more accentuated in other reconstitutions are less evident in the long-term reconstitution.

Minor comments.

Line 41: "Complex climatic dynamics" — Provide more detail for readers who may not be familiar with the region's climatic dynamics.

In the revised version of the manuscript, more details have been provided. Please see the revised manuscript.

Line 65: It might be worth adding that $\delta^{18}\text{O}$ records capture significant climatic information, even when collected from sites that are not classically considered “climatically” limited, as in this case.

We added the suggested information in the revised version of the manuscript. Please see the revised manuscript.

Line 97: Which oak species are you analyzing here? This is not mentioned anywhere in the manuscript.

We added the oak species information in the revised version of the manuscript. Please see the revised manuscript.

Line 104: "Latewood" — The spelling appears to be incorrect.

The misspell was corrected in the revised version of the manuscript.

Figure 1: The resolution is very low, particularly for the climatograph. Is it possible to improve the quality?

We improved the resolution of Figure 1 in the revised version of the manuscript.

Line 130: Why not use VPD? It could be even better than SPEI due to its direct link with leaf-to-atmosphere pressure.

Unfortunately, the VPD time series are very short for the analyzed area, and it is not possible to build a robust reconstruction model, which can be also split into calibration and verification to evaluate the model, thus we decided to reconstruct a drought index.

Supplementary Figures: They are usually ordered according to their appearance in the main text, but here they are not. The chronology is labeled as S3, while the atmospheric pattern (cited later in the text) is S1. Please reorder them.

Thank you for the suggestion. In the revised version of the manuscript, we reordered the supplementary figures, accordingly.

Line 250: While you cite the paper that explains this methodology, this manuscript should stand on its own. It would be better if you could briefly describe the method behind the stability map.

In the revised version of the manuscript, we add a short description of the stability map method. Please see the revised manuscript.

Figure S3 (Chronology): Why are there not consistently 7 cores, but rather sometimes 7, then 6, and then 7 again? Are these missing rings? Please explain.

Some of the tree rings were so narrow that there wasn't sufficient material for analyses, we added this explanation in the revised version of the manuscript. Please see the supplementary file.

Line 223: Please reference Figure 4 at the end of the sentence.

We modified the text accordingly in the revised version of the manuscript.

Figure 3: This may not be the most appropriate figure to show here. The focus should be on the last rows—correlations between $\delta^{18}\text{O}$ and monthly SPEI for the five different windows. The rows above, showing correlations between September SPEI with January SPEI (which, of course, is positive), August with January, and so on, may not be needed. Please consider revising this figure.

Thank you for the suggestion. In the revised version of the manuscript, we replaced Figure 3 with a new Figure according to the reviewer's suggestion and moved the old version of the Figure to the supplementary file.

Section 3.4: This section is not described in the methods but is explained here. I suggest moving the method explanation up and leaving the results and discussion in this section.

Thank you for the suggestion. In the revised version of the manuscript, we moved the description of the method to the Method section and improved section 3.4.