

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 will improve the title of the paper to be more appropriate and more precise.

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 $\delta^{18}\text{O}$.

Thank you for the suggestion. In the revised version of the manuscript, we will improve the introduction according to the reviewer's suggestion and we will include explanations and contextualization 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 will be improved.

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 will be provided.

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.

Thank you for the suggestion. We will add the suggested information in the revised version of the manuscript.

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

Thank you for the suggestion. We will add the oak species information in the revised version of the manuscript.

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

The misspell will be 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 will improve 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.

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

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 will add this explanation in the revised version of the manuscript.

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

We will modify 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.

In the revised version of the manuscript, we will consider the revising of Figure 3.

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.

In the revised version of the manuscript, we will move the description of the method to the Method section and we will improve section 3.4.