

Review of ‘Constraining Holocene hydrological changes in the Carpathian-Balkan region using speleothem $\delta^{18}\text{O}$ and pollen-based temperature reconstructions’ by Drăgusin et al.

General Comments: This is an excellent, well written and well-argued manuscript that contains much valuable new data and thought provoking ideas. The use of pollen-derived seasonal temperature ranges to constrain the magnitude of any possible temperature-driven shift in speleothem $\delta^{18}\text{O}$ between two time intervals of the Holocene is novel and it leads to new insights. One of the key conclusions is that the observed increase in speleothem $\delta^{18}\text{O}$ across the 6 ka to 4 ka interval (actually more like 7 ka to 3 ka because of how this is defined) is too large to ascribe to temperature change alone, and so some other effect must be important in the sites from SW Romania. The implication seems to be that this requires greater inputs from a Mediterranean moisture source, but this conclusion could perhaps be made clearer in the abstract and also in the conclusions section.

One complication seems to be that the Mediterranean surface waters may have had higher $\delta^{18}\text{O}$ during this time period. Another complication that should perhaps be considered, is that temperature changes over time (e.g. 6 ka to 4 ka) may well be accompanied by moisture source changes, if atmospheric circulation changes and associated changes in dominant air masses are responsible at least in part for the temperature changes inferred from pollen assemblages etc. There is at least the possibility that sometimes these changes act in opposing directions with respect to rainfall $\delta^{18}\text{O}$, and sometimes they may reinforce one another, depending on the source and air mass trajectory changes that might drive local temperature changes.

Specific Comments:

- (i) In the abstract, the phrase ‘isotopic enrichment due to changing hydrology’ is a bit ambiguous. I think the authors mean changes in the moisture sources and/or transport trajectories? At present some readers might mistakenly think that this refers to cave site-specific hydrological effects (e.g. changes in water residence time in aquifer, evaporative enrichment in soils etc.). Please make this terminology clearer for the reader.
- (ii) In the Introduction, please clarify that the sentence ‘A pattern of increasing $\delta^{18}\text{O}$ values is documented in speleothem records from south-central Europe and the Eastern Mediterranean’ actually means ‘A pattern of increasing $\delta^{18}\text{O}$ values *with time from the early to mid-Holocene* is documented in speleothem records from south-central Europe and the Eastern Mediterranean’
- (iii) The ‘dashed line’ that is supposed to depict the isotopic values predicted by McDermott et al. (2011) according to the caption to Figure 8 appears to be missing. I have tried to eyeball these, and I think that the new data from Ascuncă follow the broad predicted trend, but are displaced to somewhat lower $\delta^{18}\text{O}$ values for much of the early Holocene. Is this correct? If so, the authors need then to consider the effect of elevation when comparing with the low elevations gradients of the McDermott et al. paper, and maybe think about a site elevation correction before comparing with the McD et al. temporal trends. The Ascuncă cave site is at an elevation of 1050 m.a.s.l., considerably higher than Poleva (390 m.a.s.l.?) and Ursilor (482 m.a.s.l.?). The European sites that contribute to the McDermott et al., European gradients/trends in speleothem $\delta^{18}\text{O}$ are from low elevation sites (typically < 400m). Ascuncă’s higher elevation likely implies more rainout of moisture en route, (and therefore lower rainfall $\delta^{18}\text{O}$) than one

- might predict. This, coupled with a relatively low MAAT (8°C) could lead to lower $\delta^{18}\text{O}$ than one might predict on the basis of its latitude from the broad trends in the McDermott et al. paper.
- (iv) It would be helpful to show the time-series data for Ascuncă, Poleva, Ursilor and V11 on the same diagram (using the same y-axis scaling) to give the reader a clearer picture about how the records agree or deviate from each other. It is difficult for the reader to do this in detail in the current Figure 8. This would also show that the $\delta^{18}\text{O}$ trend at Ursilor is relatively flat by comparison with that at Ascuncă and Poleva. This would link nicely to the argument presented in Figure 9.
 - (v) This was clearly a difficult stalagmite to date because of low U, its young age and the presence of detrital thorium. Overall the authors have done a good job with the chronology. It would be helpful for the reader though to indicate the position of possible growth hiatuses on Figure 5, the age model diagram.
 - (vi) Just a comment - It is interesting that the empirical equation of Tremaine et al. (2011) yields calcite $\delta^{18}\text{O}$ values that are closest to the measured values and that the equations of Kim and O'Neill (1997) and Day and Henderson (2011) give values that are too low. This appears to be a commonly observed phenomenon in cave studies globally.
 - (vii) I was unable to find the location of V11 on the map (Figure 1). Please fix this in the revised manuscript.
 - (viii) On page 395, I think the authors mean to refer to Figure 9, not 8, in the sentence 'Figure 8 shows that measured $\Delta\delta^{18}\text{O}_{6-4\text{ka}}$ at Poleva is similar to that at Ascuncă'.
 - (ix) Page 397 'there is virtually no rainfall during summer' (not rainfalls).
 - (x) The main conclusions could be reorganised and written more clearly. For example, the sentence 'we show that the Atlantic source effect was not the main isotopic driver for this well-define isotopic shift' is a rather ambiguous. Better to state more clearly that you think a shift to a greater influence of Mediterranean-derived moisture between 6 and 4 ka in S.W. Romania (if this is your preferred interpretation).