

Interactive comment on “Reconstruction of drip-water $\delta^{18}\text{O}$ based on calcite oxygen and clumped isotopes of speleothems from Bunker Cave (Germany)” by T. Kluge et al.

Anonymous Referee #1

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In their paper "Reconstruction of drip-water $\delta^{18}\text{O}$ based on calcite oxygen and clumped isotopes of speleothems from Bunker Cave(Germany)", Kluge et al present a new method for estimating past drip water $\delta^{18}\text{O}$ values using combined classical $\delta^{18}\text{O}$ and clumped isotope (D47) analyses. The method is based on the assumption that both $\delta^{18}\text{O}$ and D47 are affected by kinetic processes, and that the slope between the two parameters due to this kinetic fractionation is constant, the "co-variance" method. The authors use this technique to estimate past $\delta^{18}\text{O}$ values fro drip water in the cave, using temperature estimates from other sources (pollen, Moberg's temperature reconstruction, noble gas temperatures).

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Overall the paper was well written and shows the results of a potentially powerful tool (the co-variance technique) to estimate past changes in drip water $\delta^{18}\text{O}$ (or temperature). The results are broadly consistent with estimates for other European paleoclimate data (Lake Ammersee and Greenland ice cores). The biggest assumption is that the co-variance slope is constant over time. Although plausible variations in this slope appear to have only a minor effect on the calculated drip water $\delta^{18}\text{O}$ values based on the sensitivity analysis, the basis of this assumption should be explained in some more detail. In fact, if one plots just the BU1 and BU4 data, the slopes differ from the one used for the calculation. Although this may be due to the small number of samples ($n=9$ for each) and the assumptions, it does raise the possibility that the co-variance slope differs from sample to sample or over time.

Given the proliferation of recent fractionation equations for oxygen isotopes in calcite that in some cases differ substantially from one another (e.g., Coplen's 2007 Devils Hole calibration and the Kim and O'Neil calibration used in the Kluge study), there is good evidence that our understanding of oxygen isotopic equilibrium is only poorly known. I also wonder how 'correct' the original Ghosh et al. Temperature-D47 calibration curve is? What data constrain that relationship?

The authors also have an opportunity to look into some broader issues of the D47 technique: if D47 is affected by kinetic fractionation in modern speleothems, could such effects be important for stratigraphic studies from marine sediments?

Figure 4: Although the three-point smoothing of the calculated $\delta^{18}\text{O}_w$ is justified, it is important to note that the point-to-point variability is equal to or exceeds the Holocene-long trend in values (noise = or > signal). This 'noisy' pattern may be real variability that would be further elucidated with higher resolution sampling; or it could reflect uncertainties in the technique. Also, the base of the figure appears to be chopped off. Y-axes should be expanded to show the Ammersee and Greenland time series. Putting the Greenland and reconstructed $\delta^{18}\text{O}_w$ y-axis on the same scale would also be helpful in comparing the trends.

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BU1 data
Equation $Y = -0.0006174897104 * X - 0.04338099691$
Number of data points used = 9
R-squared = 0.000469098

BU4 data
Equation $Y = -0.01524089671 * X - 0.02926283085$
Number of data points used = 9
R-squared = 0.260947

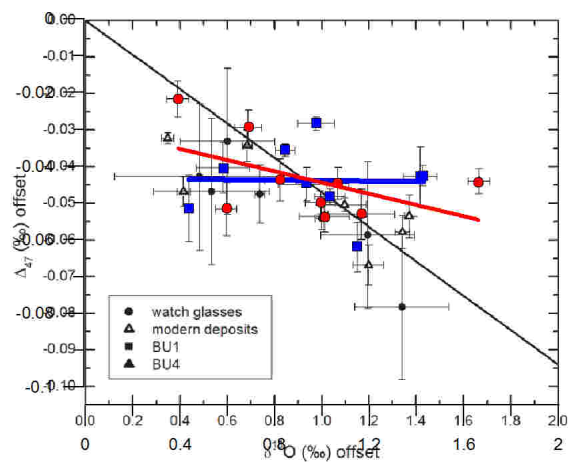


Fig. 2. Δ_{47} versus $\delta^{18}O$ offsets for Holocene stalagmites BU1 and BU4 as well as modern speleothems. The straight line is the slope obtained from a linear fit of modern calcite precipitates (Fig. 1).

Fig. 1.

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