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Climate of the Past Discussions

Interactive comment on "Reconstruction of drip-water δ^{18}O based on calcite oxygen and clumped isotopes of speleothems from Bunker Cave (Germany)" *by* T. Kluge et al.

Anonymous Referee #2

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This manuscript presents an approach to reconstructing past dripwater δ18O variations in stalagmites that are characterized by appreciable kinetic fractionation. By combining paired δ18Oc and D47 measurements from the same speleothem calcite, and computing the kinetic offset in D47 using known paleo-temperature estimates, the authors derive paleo-d18Ow estimates. The approach is robust, and it is neat to see it applied to a mid-latitude sample so successfully. It represents a promising way forward in speleothem paleoclimate research, and demonstrates the maturity of the authors' investigations into the controls on speleothem D47. With the goal of improving the presentation of a viable road map for making use of combined δ18Oc and D47 measurements in speleothems, however, I have a number of suggestions that are designed

to improve the readability of the manuscript.

My most significant comment concerns the reliance on noble gas temperatures from speleothem fluid inclusions on paleo-δ18Ow calculations in several specific intervals of time. If these are to be used, then a) they should not be used exclusively for these intervals, and b) they should be used with the delivery of appropriate caveats. There are a couple layers of issues here that warrant discussion and, where necessary, revisions to the manuscript: 1) I was puzzled to see the large offsets inferred in the NGT estimates, which were treated with a uniform, time-invariant correction. I am no expert in making these measurements, nor in their interpretation, but how justified is this approach? Wouldn't whatever environmental influence caused the offsets be subject to change through time? Especially going back through the last glacial cycle? I am particularly concerned with the early Holocene NGT estimates, which show a roughly 5C cooling at $\sim$10-11kybp. This directly leads to anomalously light d18Ow estimates at this time. Are such coolings possible at this time period? They seem far too large, given LGM temperature changes of $\sim$9C quoted in this manuscript. Perhaps the authors should consider alterative paleo-T estimates. How is it possible that the authors pull from 4-5 Stage 5e temperature estimates, but rely on corrected NGT estimates to derive their largest δ18Ow anomalies? 2) In the manuscript, the authors cite multiple sources of paleo-T estimates that are used for the various time periods, but it would be much more effective to consider a range of paleo-T estimates for each time period. As it stands, the sole reliance on a single number for each time period (with an arbitrary error assignment of +/- 1C) doesn't give an accurate reflection of the full uncertainty likely associated with the paleo-δ18Ow estimates.

Minor comments: 1) Section 5.2: It seems a bit of a stretch to discuss temporal changes in the d18Ow-T relationship with the current dataset, with such appreciable (and inter-related) uncertainties in both the numerator and denominator. The δ18Ow estimates use the paleo-T estimates, so this strikes me as problematic to calculate their ratio through time. I strongly recommend deleting this section, which contributes

little to the manuscript.

2) Fig 2: Can the authors assign more distinctive symbols to the various calcite datasets represented here? It is hard to tell, but I think that the Holocene stalagmite datapoints define a δ18Oc-D47 slope that is quite different than the watch glass and modern calcite samples. Is this true?

3) Fig 5: If δ18Oc in the stalagmites = d18Ow + T + kinetic fractionation, then can't you provide some constraints for the d18Oc-related kinetic fractionation in the stalagmite through time? More to the point, such a timeseries might be useful to compare against stalagmite d13C timeseries from the same sample, which presumably is also affected by kinetic isotope fractionation (a la Hendy). This would be of broad interest to the speleothem paleoclimate community, which has long struggled to interpret d13C with respect to climate versus kinetic causes. The authors already have the d13C calcite record, presumably, so it's only a matter of thinking carefully about how kinetic δ18O and d13C fractionation might be represented using the new constraints presented in the manuscript.

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