

Interactive comment on “Simulated oxygen isotopes in cave drip water and speleothem calcite in European caves” by A. Wackerbarth et al.

A. Wackerbarth et al.

anne.wackerbarth@iup.uni-heidelberg.de

Received and published: 16 October 2012

Reply to Referee1:

We thank the referee for his/her detailed and encouraging review which helped to improve our manuscript. In the following a detailed response to the aspects the referee raised is given.

Referee comment 1: Such modelling exercises are needed to evaluate the spatial variations in d18O at different time periods, to test hypotheses of climate variations at key time intervals, such as the mid-Holocene. The new emphasis on spatial variations in d18O has been emphasized in recent work by McDermott et al.s' 2011 GPC paper, which could be discussed in some more detail in the introduction to provide important

C1922

context for the current study.

Reply: We included this in more details in the introduction.

Referee comment 2: The model results compare reasonably well to observations. Perhaps the most critical comparison is the modelled vs. observed d18O value of speleothem calcite (panel e of Figure 1). The differences in the modelled and observed d18O values are strongly dependent on the model assumptions, most importantly through the equilibrium fractionation equations and climate input (e.g., temperature at the cave site). As with any modelling exercise, the full range of uncertainties in the model assumptions is important for the final output. One key aspect that needs clarification is the use of the "equilibrium" d18O drip to calcite fractionation equation. There are now a multitude of such equations (both empirical and theoretical) which produce very different results. Simply stated, there remains a significant amount of uncertainty as to which equation provides the approximation of the true 'equilibrium' fractionation value. The citation to the specific fractionation equation used in the model should be presented and discussed, as the reader may not have read the previous papers on the ODSM that describe it in more detail. The equation apparently used in the model is the Friedman and O'Neil equation, but that one has largely been superseded by more recent work, in particular the Kim and O'Neil equation. (...) These new studies, and others that are cited in these two publications, suggest that our understanding of "equilibrium" d18O values in calcite is poorly known. It is clearly beyond the scope of the present paper to evaluate all of these details on d18O equilibrium. Nor would adoption of a different fractionation factor in the model explain away the discrepancies between the modelled and observed values. However, a short discussion should be included to point the reader to the respective literature.

Reply: We agree with the reviewer that the fractionation process between drip water and calcite is still a very difficult problem and has to be closely discussed and it must be clearly stated which fractionation factor is applied, since there is not one "true" fractionation factor. As the referee indicates our paper is a rather conceptual paper

C1923

focusing on a new application of different models in a paleoclimate context and it is beyond the scope of our paper to evaluate all the different fractionation factors. As suggested we discussed in more detail this topic in the new manuscript. In addition, we explain why we chose FriedmanO'Neil for equilibrium fraction.

Referee comment 3: Finally, the observations supporting a NAO+ state during the mid-Holocene seem warranted. Indeed, this is one of the major contributions of the current manuscript, and it could be emphasized more heavily in the abstract and conclusions.

Reply: The evaluation of the NAO state is one of the possible applications of our approach. However, this topic should be handled very tentatively, since the NAO system is still a field of many unknowns and uncertainties. Therefore only two small comments were added in the abstract and conclusions.

Referee comment 4: Technical corrections The authors need to clearly reference the VPDB and VSMOW scales in the text. As written, one would think only one reference scale is being used. Also, there is a need to use correct stable isotopic terminology. A good reference is Zach Sharp's textbook "Stable Isotope Geochemistry". For example, being ratios, d18O values can not be enriched or depleted. Water can be enriched or depleted in 18O (or 16O), but ratios can neither be enriched nor depleted. "Higher" and "lower" are preferred adjectives to describe changing d18O values, and "heavier" and "lighter" are also ok.

Reply: We thank the referee for pointing out this technical weakness of the paper and revised the manuscript with regard to a correct use of isotopic terminology.

Referee comment 5: It is interesting to set the time scale of epikarst averaging at four years. What are the data citations to support this length of time averaging? One recent estimate was published for a ca. 9-year averaging in Lachniet et al., 2012, *Geology* 40 (3), pp. 259-262, but that result is empirical not observed.

Reply: We discussed the topic more closely.

C1924

Referee comment 6: Page 2788, Line 20: An added complication regarding ET is that ET estimates are time averages, but precipitation is highly episodic. A heavy precipitation event over a short period would be essentially unaffected by long-term ET. Thus, the 'isotopically effective' infiltration may not correspond to the mean precipitation at any given cave site. This was discussed in some recent papers.

Reply: In these lines we refer to the percentage of evaporation and transpiration to the total amount of evapotranspiration, which are assumed constant and different for winter and summer. The evapotranspiration is calculated in monthly resolution. Although we agree with the referee that precipitation is more episodic than evapotranspiration and this is indeed an uncertainty for the model results, the model setup was established to grasp long term means and therefore the best that we can do is applying monthly mean values for all the input parameters.

Referee comment 7: Page 2795, Line 1: There is some support in the literature for the influence of the Black Sea on the d18O value of precipitation in the region near Poleva Cave: see Badertscher et al *Nature Geoscience*: S Badertscher; D Fleitmann; H Cheng; R L Edwards; O M Göktürk; A Zumbühl ..., *Nature Geoscience*, ISSN 1752-0894, 04/2011, Volume 4, Issue 4, p. 236

We thank the referee for hinting this and included it in the revised version.

Interactive comment on *Clim. Past Discuss.*, 8, 2777, 2012.

C1925