

Interactive comment on “Effects of dating errors on nonparametric trend analyses of speleothem time series” by M. Mudelsee et al.

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This manuscript tackles a integration of timescale errors in paleoclimatic time series. This was rather neglected in previous studies. As a method paper, it is welcome and well written. Methods are described with sufficient detail to reproduce the proposed algorithm, and are sound. I have only a minor concern about the discussion of methods. The authors apply two strategies to create an age model, StalAge and iscam, the second assuming good correlation between roughly coeval proxy records. This is however quite a bold assumption, especially where stalagmites from different caves are compared, and introduces an element of circular reasoning when the proxy records are discussed (e.g., the d18O records may have consistent trends in some time intervals, but such trends were already implicitly assumed consistent for age calibration). This

is not discussed in the manuscript but it could be worth to note. It is unclear to me how extensive this problem could be for the general pool of paleoclimatic time series. Sure enough, it needs to be discussed here, where the time coincidence of a $d18O$ oscillation between three stalagmites is taken as proof that a climatic event exists. How relevant this time coincidence could be for the iscam-calibrated $d18O$ series? Probably not much, as the proxy oscillation seems to be there also in the StalAge-calibrates series, but a discussion is necessary.

The manuscript then goes on discussing an interpretation of the proxy records, assuming that calcite $d18O$ in these stalagmites is a proxy for temperature. The possibility that other parameters along with temperature might influence the $d18O$ from stalagmite calcite is discussed too briefly. This is in my view the weakest part of the manuscript. In general, the $d18O$ of speleothem calcite is considered a proxy for precipitations (Fairchild and Baker, 2012). For the specific case of Bunker Cave, monitoring did not get as far as concluding that a paleotemperature could be inferred from the $d18O$ in speleothems (various references, cited in the manuscript). The whole discussion about paleoclimatic implications should be revised in the light of this uncertainty on the meaning of oxygen stable isotopes.

Minor comments:

P1974 L16: not excessively large

Could use a less vague phrasing, e.g.: "are one order of magnitude smaller than other errors"

P1974 L20: Our analyses cannot unequivocally support the conclusion that current regional winter climate is warmer than that during the MWP.

This conclusion should be avoided: the three records show all possible combinations of relationships between the MWP and the Recent, hence, they provide no relevant information at all. Furthermore, the $d18O$ is not a proxy of temperature.

P1975 L5: later part of the Holocene

substitute with "late Holocene"

P1975 L11: Estimates without error bars are useless.

I see your point but this sentence carries no useful information and is substantially false: cases of estimates without error bars that are meaningful could be conceived.

P1981 L23: with high (low) $\delta^{18}\text{O}$ values indicating dry/cold (wet/warm) conditions.

It is unclear here whether P and T are correlated or independent. This makes a lot of a difference, because if, e.g., wet-cold combinations are possible, then oxygen isotopes alone can't be used as a proxy for T as long as another variable is given that is a proxy for P.

P1986 L3: Note that the time series plots (Figs. 2, 4, 5, 6, 7 and 8) show conventionally time t on the horizontal and time series value x on the vertical axis.

Drop this sentence, it's all in the figures and everyone can read.

Figure 1:

If some lines are invisible on a proof, print full page. In any case try to avoid the comment in the caption about invisible features. Please place the name of samples (stalagmites) directly on the plots, e.g., on the bottom right corner.

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