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Climate of the Past Discussions

Interactive comment on "Stalagmite water content as a proxy for drip water supply in tropical and subtropical areas" *by* N. Vogel et al.

Anonymous Referee #1

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This is a somewhat speculative paper which encounters some serious analytical difficulties along the way but manages to come up with some significant correlations that support the thesis of the paper. Within the paper lie hidden a few other issues that might bear discussion.

Why is there such a big difference between δ18O of the three stalagmites? Mukalla Cave has lower precipitation which should make its δ18O higher but the opposite is true. And even the neighboring samples on Socotra I. differ by about 2 ‰

The differences in water yield by the two methods are extraordinary. It seems unlikely that all of the water released at 480 °C is coming from the microscopically visible inclusions. There should be mention made of comparisons of yield to other studies, which generally give results comparable to what is obtained by heating to 320°C. It seems as

if there are multiple storage sites for water in speleothem, something which has been suggested before and needs to be further investigated.

The pixel counts gives about 10x more volume than the 320°C yields. This suggests that these are not very accurate estimates of fluid volume. There should be a table of these data, rather than only referring to them in text and figure captions.

The main conclusion of the paper is drawn from Fig. 6. My impression is that there is too much scatter in these data to make a strong argument for a control by precipitation rate. For example, the data for P3 show essentially no correlation except that the last point is significantly higher. Likewise, for D1, there are really two regimes: low δ18O and higher δ18O but the uncertainty in each point is so large that it seems that only linear regression saves the day! Why is no error shown in the X-axis? Given the discussion in the text, this would seem to have a significant error as well. One would have to say that statistics suggests some correlation but it doesn't look like a method one could rely on very strongly. I think that the authors should be more forthright about this problem and discuss the sources of error and the reason for the scatter in the data. This is also important.

One further detail in this regard: the paper repeatedly states that there is correlation between δ18O and precipitation and cites Fleitmann et al., 2007 as the source. It seems that in that paper the "evidence" for this is the well known negative correlation between δ18Oppt and rain volume ("amount effect"). There is not, as far as I could perceive, any independent evidence for this correlation. So this paper should really state from the outset that the correlation is attributable to the amount effect (identified by Dansgaard in 1964). That effect is known to be valid in tropical enviroinments typified by high rainfall, and where average annual temperature is > $\sim$25 °C. I am unaware if it has been demonstrated for desertic environments; has it?

In trying to account for the correlation between δ18O and water % the authors suggest that these effects are also relatable to differences in growth rate. However, for two of the stalagmites the U-Th dates provide direct evidence for growth rate. Does there seem to be any relationship between growth rate and δ18O of calcite.

On p. 2901 the authors state

"We thereby imply that a sample with a high volume fraction of fluid inclusions (i.e., comprising both water- and gas-filled inclusions), is also characterized by a high volume fraction of water-filled inclusions alone and vice versa."

why? What information do you have about gas-filled inclusions. Also: "imply" should perhaps be "infer".

Interactive comment on Clim. Past Discuss., 8, 2893, 2012.