

Interactive comment on “Enhanced Mediterranean water cycle explains increased humidity during MIS 3 in North Africa” by Mike Rogerson et al.

Anonymous Referee #2

Received and published: 10 December 2018

General:

Rogerson and coworkers present an interesting and comparatively large set of speleothem fluid inclusion isotope data from North Africa (Libya). It concerns follow up work on a publication by Hoffmann et al. (2016) who already presented the speleothem calcite $d_{18}O$ and the age model of this stalagmite. The ms is well written, provides sufficient background, and existing work is properly referenced for as far as I can oversee. The new fluid inclusion isotope dataset, that forms the backbone of this study seems technically sound (although unfortunately not presented in a table), but, as the authors describe, is also somewhat problematic because of variable water yields during analysis and relatively poor correlation of fluid inclusion isotope data with existing speleothem calcite $d_{18}O$ data. In any case, there is reasonable agreement

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with rainfall isotope data from the area, lending support to the general accuracy of the dataset. An additional set of Sr isotope values of the speleothem appears to suggest that dust source changes play a minor role over the studied time interval.

Coming back to the fluid inclusion dataset, it indeed is somewhat uncomfortable that there is such poor correlation between fluid inclusion $d_{18}O$ ($d_{18}Ofi$) and calcite $d_{18}O$ ($d_{18}Occ$) and that no clear stratigraphic trends are visible in $d_{18}Ofi$. It may be that this reflects high frequency rainfall sourcing changes or amount effect, but then why is this variability not visible in the $d_{18}Occ$ (which presumably has the higher stratigraphic resolution)? What I would like to see in the ms is an assessment of the extent to which the fluid inclusion isotope data are in isotopic equilibrium with the calcite (which is the assumption underlying the expectation that $d_{18}Ofi$ and $d_{18}Occ$ should be coupled to some extent). While isotope equilibrium is not a given for many speleothems, at least some consistency is to be expected between $d_{18}Occ$ and $d_{18}Ofi$. If there is none or very little, one should think about what the fluid inclusion isotope values really represent. Based on petrographic observations the authors rule out the possibility of diagenetic alteration affecting the oxygen isotope values of the inclusion water, but in doing so, seem to offer no alternative interpretation to explain the absence of isotope equilibrium between speleothem carbonate and inclusion water. Maybe the observation that two phases of fluid inclusions are present in thin section study is of importance, although I can't directly see what kind of effect that would have on your analyses. It would perhaps be useful if the authors discuss that in a bit more detail in the context of their interpretation of the record.

A strong point of this study is that the authors duplicated fluid inclusion isotope data, and show the “duplicable” set in Fig 6. This set is significantly smaller than the original set, attesting to the analytical difficulties the authors describe in the earlier sections of the ms. Some potential flyers in the FI data in fig 4 do not duplicate, suggesting that these are indeed inaccurate data points, which in turn underlines the importance of this reproducibility test. The duplicable set spans a comparatively small range in

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isotope values. Nevertheless, the case can be made that the dataset is influenced by two isotopically distinct atmospheric water sources. I would indeed agree that the FI isotope pattern fits a typical Mediterranean case of mixing between a source that, in deuterium-excess values, is closer to Eastern Mediterranean moisture and one that is closer to more Western sources. Bringing a third water source in, as is suggested in the ms, is not really supported by the data from my perspective. I generally doubt if one can simply take average isotope values of present day rainfall records, that have rather wide and overlapping ranges of isotope values, as distinctive fingerprint for past changes between these moisture sources. One could perhaps argue that slight isotope changes within each of these moisture sources can cause similar isotope patterns?

In summary:

I believe the dataset as presented should be interpreted primarily based on the duplicable dataset. This probably requires the authors to simplify their interpretation to some extent. I believe there still is an interesting story when one does so, as the unusual negative correlation between $d2H$ and $d18O$ in the dataset indeed is best explained by variable deuterium-excess values in the past, and therewith possibly with moisture source changes (presumably between the E and W Mediterranean basins) at the Susah site. Definitely more difficult to understand is the relatively poor correlation of $d18Ofi$ values with corresponding $d18Occ$. Such a poor correlation practically implies that (near) isotopic equilibrium can't be demonstrated, and calls for caution in the interpretation of what the fluid inclusion isotope data (particularly the oxygen isotopes) really capture. Perhaps the same goes for the apparent absence of stratigraphic trends in the fluid inclusion isotope data as presented (with the possible exception of the $d2Hfi$ data). So, while I am somewhat critical about some aspects of the interpretation of the data, I support the publication of this interesting study as it is one of the first examples that I have seen to demonstrate changes in deuterium excess in speleothem fluid inclusion isotope data, in an area where such changes can be quite large and have clear paleoclimatological significance.

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Some further comments and questions:

I'd like to know where your duplicable samples from Fig 6 are located in the stalagmite (stratigraphically). All in one period, or distributed all over? Do you have a better correlation with the $d18O$ values of the carbonate when you consider the duplicable dataset only? Further, I'd like to see if, based on the duplicable set only, one can still observe clear differences between the three wet intervals. If not, the interpretation of interval two as being different from one and three in its fluid inclusion isotope composition may be on thin ice. Not knowing where precisely the duplicable data fit in the stratigraphy, I can't assess this in the present ms.

Figures in general could be improved to make them more accessible for the reader.

Particularly figure 4a and b are not easy to decipher for me as fluid inclusion data points are partly hidden behind other graph elements. In period 2 in this figure, I note 4 $d18Ofi$ and only 2 $d2Hfi$ values (correct?). I assume the data are produced in pairs, so why are some $d2H$ values missing? I'd like to see the data table for the fluid inclusions added to the ms, so that these data are accessible.

The term $d2Hexcess$ (in Fig 10) is to my knowledge not the generally accepted notation for deuterium excess.

Line 191: GNIP does not stand for "Global Natural Isotopes in Precipitation", but for "Global Network of Isotopes in Precipitation".

It could be interesting to know a little bit more about the Sr isotope composition of the host limestone. Are your Sr isotope values merely representing the limestone host rock, or do you have reason to believe there is a Saharan dust siliciclastic component supplying some Sr to the epikarst as well? Could you have any sea spray effect? (not sure how far away from the sea you are)

Towards the end of the discussion, $d13Ccc$ plays an important role. These data, however, are not shown (and they are not shown in any of the figures in Hoffmann et al 2016

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either). It would be good to show the d13Ccc data if you claim that d13Ccc doesn't cycle like d18Occ does (line 371). To follow up on that; the entire argumentation in lines 369-374 is a bit puzzling to me because you don't mention your d18Ofi at all (apart from a "generic" d18O in line 373 with which you mean d18Occ I presume). Shouldn't d18Ofi do the same as your d2Hfi is your claim is correct?

Your statement in line 392 to 394 is not clear to me.

Interactive comment on Clim. Past Discuss., <https://doi.org/10.5194/cp-2018-134>, 2018.