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Climate of the Past
Discussions

Interactive comment on "Speleothem Evidence for Megadroughts in the SW Indian Ocean during the Late Holocene" *by* Hanying Li et al.

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Anonymous Reviewer #2:

We would like to thank this reviewer for her/his comments on the manuscript. Please find our detailed answers to each comment below.

Climate of the Past Submission cp-2018-100, "Speleothem Evidence for Megadroughts in the SW Indian Ocean during the Late Holocene" by Li et al., presents evidence from two stalagmites from Rodrigues Island in the southwestern Indian Ocean and makes inferences about climate from 4500 to 3000 years BP. The most profound inference is of a "multicentennial period of drought (i.e., megadrought) that lasted continuously from $\sim$ 3.9 to 3.5 ka BP". The fundamentals of this kind of research are (A) the stalagmite(s)

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studied, (B) the radiometric ages, (C) the age model resulting from A and B, (D) the data placed in time-series using C, and (E) the reasoning employed to interpret the data in D. I will therefore proceed through this list and then consider the broader implications of the manuscript. This leads to six enumerated suggestions for improvement of the manuscript. A figure on the second page illustrates some of the points made. A. The stalagmites studied The study draws on two stalagmites, LAVI-4 and PATA-1, from two caves on Rodrigues Island. I infer from Figure 4B that the project does not draw on the entirety of Stalagmite PATA-1. As I said, I had to infer this, and I do not see it stated in the text. I think it should be, to save readers confusion, and hence my first suggestion: Suggestion 1. The manuscript and its figures should make explicit what portions of the two stalagmites were analyzed for this project.

Answer- We agree. We will add explicit descriptions about the portions of the two stalagmite that we used in this study. We will clearly mark the portions of the two samples using blue bars in the related figure.

The images of the stalagmites provided in Figure 4 suggest that significant layerbounding surfaces (Railsback et al., 2013) may be present, but no mention of layer-bounding surfaces is made in the text, leaving the reader to wonder if the authors infer none or, alternately, did not look for them. This leads me to Suggestion 2. Suggestion 2: The manuscript should report the layer-bounding surfaces seen in the stalagmite or state explicitly that there are none. Both of these suggestions will matter considerably later.

Answer- Thanks for the suggestion. Railsback et al. (2013) identified two types of layer-bounding surfaces in their stalagmite: Type E, formed under wet conditions and Type L reflecting dry conditions. Upon a closer petrographic inspection, type L surface likely occurred at ~124 mm depth in stalagmite LAVI-4.

B. Radiometric ages The manuscript focuses on the period from 4500 to 3000 years BP (ostensibly around 4.0 ka for the "4.2 ka BP event", but the big result comes later in the 3000s years BP). The number of ages reported is as follows: Stalagmite Number

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of ages between 4500 and 3000 BP LAVI-4 16 PATA-1 1 LAVI-4 is clearly thoroughly dated, but PATA-1 has only one age in the time interval of interest. This will be important in Part C and will lead to Suggestion 3.

Answer- In order to improve the age model for sample PATA-1 we have obtained two additional 230Th dates between 4 and 4.5 ka BP. The new subsamples of PATA-1 were drilled at 20 mm (4284 $\pm$87 years BP) and 22.2 mm (4494 $\pm$138 years BP), respectively. The reconstructed age model with new additional dates is consistent with the previous one, but more robust.

C. Age models C1: The PATA-1 age model Through the time interval of interest, the age model for PATA-1 in Figure 4B is a straight line, and the very quantitative algorithms used to generate the age models give relatively small uncertainties. However, the age of material from 18 to 24 mm from the top is unconstrained because there are no radiometric ages in that interval. Application of growth rates derived from earlier parts of the stalagmite (my dashed lines on Figure 4B) suggests that the material at 22 mm from the top could be anywhere from 4600 to 3900 years old (and a hiatus could make the range even greater). To summarize Section B and the previous paragraph, because the PATA-1 record from 4.6 to 3.6 ka has only one U-series date, age is largely unconstrained in that interval. Thus PATA-1 provides an isotopic record correlative with that of LAVI-4, but PATA-1 is of no help with chronology. This leads me to Suggestion 3. Suggestion 3. The statements that the manuscript presents "chronologically well-constrained speleothem oxygen and carbon isotopes records of hydroclimate" (Lines 23 and 24) and "two precisely dated speleothem oxygen (δ18O) and carbon (δ13C) isotope records" (Line 67) should be changed to the singular "record", and the word "two" should be deleted, because the manuscript in fact presents only one chronologically well-constrained record of the interval of interest, not two. The plural claim "records have tight age control" is likewise invalidated.

Answer- We agree with the reviewer. As we noted in the previous response, we have added two additional dates for PATA-1. We will refine the statements in the revised version.

C2: The LAVI-4 age model The LAVI-4 age model in Figure 4A presents the results of many radiometric analyses, which is good. Figure 4A suggests a relatively constant rate of growth, with one exception about which three points can be made: a) Growth is relatively constant except at 123 mm, where the growth rate is much less, suggestive of a hiatus. b) The image of stalagmite nested in Figure 4A (the only image provided) is not particularly clear, and the indexing scheme is not shown, but my attempt to reconstruct the indexing to which readers are not privy suggests that there may be a layer-bounding surface (and thus a possible hiatus) at about 123 mm. c) In the stable isotope data from about 123 mm, there is a shift in δ13C of about 6.2‰ between two successive stable isotope samples. The manuscript says that resolution of the data is about 4 years, so that the data imply a change in δ13C of about 6.2‰ over about 4 years. That implies a major ecosystem shift and shift in soil carbon (which has decades-to-centuries residence times) in just four years. A more likely explanation is a hiatus in which the unrecorded time allowed the shift in soil ecology at feasible rates. Points a, b, and c lead to Suggestion 4. Suggestion 4. Either the manuscript should be revised to use an age model including the hiatus evident at about 123 mm, or the manuscript should explicitly explain why it rejects the hiatus that will be evident to many readers. Clearly the statement in Lines 120 to 121 that "both samples grew continuously between 3.5 and 6.0 ka BP interval without any visible hiatuses" should be reconsidered. As an aside, I would add that the problem here is a common result of generation of age models using computer programs that are not written to include hiatuses and that do not consider evidence beyond the radiometric dates. Use of these programs seems very quantitative and objective, so it is attractive, but it also leads to non-recognition of hiatuses and thus flawed age models. It is far better to recognize a hiatus, to generate a better age model, and to interpret the cause of the hiatus - and in this case the hiatus is very convincing evidence of extremely dry conditions.

Answer- Good point again. We will add a figure to show two possibilities for the sample

LAVI-4 at a depth of ∼124 mm, a hiatus (∼100 years) as suggested by the reviewer, or a portion of the sample with a very slow growth rate. Either way, the major hydroclimatic patterns between 6 and 3 ka BP inferred from our records, thus our conclusions, remain similar.

D. Stable isotope data The manuscript reports both δ18O and δ13C data, which is good - some researchers oddly do not report their δ13C data, despite its usefulness. The data reported seem quite normal: range of δ13C is greater than that of δ18O, both are in the negative single-digit values (relative to VPDB) typical of stalagmites, etc. The two co-vary, which is typical of settings in which rainfall limits the extent of vegetation. LAVI-4 has greater ranges of both δ13C and δ18O than PATA-1. One notable omission is that the stable isotope data from PATA-1 stop at 15 mm below the top of the stalagmite, during the most extreme part of the "megadrought", and do not record the return to the less extreme conditions. This is like reading a novel only to find the last few pages have been torn out. Definition of the time and duration of the megadrought would seemingly require continuation of the PATA-1 series of stable isotope data later above 15 mm from the top of the stalagmite. Note that the unexplained absence of data from above 15 mm in PATA-1 invalidates the abstract's claim to "present high-resolution and chronologically well-constrained speleothem oxygen and carbon isotopes records of hydroclimate variability between ∼6 and 3 ka ago from Rodrigues Island": PATA-1 was not analyzed to give a record after 3.5 ka. Suggestion 5: The PATA-1 series of stable isotope data should be extended higher/later than its present extent, or the manuscript should explain the omission to readers who wonder why it was terminated in mid-event. If the omission persists, the abstract's claim to "records [plural] of hydroclimate variability between ∼6 and 3 ka ago" should be modified, because only one record goes to 3 ka.

Answer- Sample PATA-1 shows a major hiatus at 15 mm. Based on our dating results, growth re-commenced after ∼630 years at ∼2740 yr BP. Thus, the record of the top 15 mm may not be helpful for the issues we addressed here. We will add this information in the revised manuscript.

E. Reasoning employed to interpret the data Lines 105 to 109 lay out the mindset used to interpret the oxygen isotope data, which hinges entirely on the amount effect giving an inverse relationship between δ18O and rainfall. I know little about the Indian Ocean but find no problem with that general assumption, but there is a growing literature suggesting that post-rainfall effects like evaporation are important too. No literature is cited in the manuscript. McDermott (2004) and Lachniet (2009) commonly are cited with regard to the amount effect and Cuthbert et al. (2014), Markowska et al. (2016), and Treble et al. (2017) are examples of the newer literature. The rationalization of the carbon isotope data appears much later, in Lines 190 to 195, and similarly seems sound.

Answer- According to this suggestion, we will add a paragraph to discuss the effect of prior evaporation process in changing the composition of drip-water δ18O in the shallow soil zone and epikarst. We will also cite the relevant papers, including those suggested by the reviewer.

Broader considerations The 4.2 ka BP event is prominent in the abstract and introduction, but it hardly gets a mention thereafter. The former, rather than the latter, seems strange, because the present manuscript is mostly concerned with a major dry event that happened later, at 3.9 to 3.5 ka. Lines 209 to 211 in fact disavow any recognition of the 4.2 ka BP event, stating the "the interval corresponding to the ’4.2 ka event’, typically considered between 4.2 and 3.9 ka BP (e.g., Weiss et al., 2016), in the LAVI-4 records does not however, stand out as ’pulse-like’ event as evident in many other proxy records". One thus has to wonder why all the mention of the 4.2 ka BP event in the abstract and introduction. With that said, one can return to the LAVI-4 data in which age is well constrained and see two negative/wet spikes in δ18O in the interval from 4.15 to 3.93 ka (see my mark-up figure). Railsback et al. (2018) concluded that the so-called 4.2 ka BP event took place from 4.15 to 3.93 ka, commonly is recognized as two pulses, and in Namibia can be recognized as two moderately wet pulses. That’s exactly what can be seen in LAVI-4.This leads to Suggestion 6: Suggestion 6: Either the manuscript's presently incongruous early focus on the so-called 4.2 ka BP event should be de-emphasized, most notably in the abstract, or the manuscript should discuss the project's data about the 4.2 ka BP event, which suggest a pair of wet pulses congruent with other published data from the Southern Hemisphere's zone of summer rainfall.

Answer- We addressed this comment by keeping the focus on the 4.2 ka event and using the record before and after the event to provide a climatic context for the event. First, we agree with the reviewer. In the revised manuscript, we will characterize the wet and dry events during the 4.2 ka event (4.2 to 3.9 ka BP), and discuss its correlation with other well-dated records (e.g., the Dante cave record (Railsback et al., 2018)). Then we will point out that the multi-decadal fluctuations during the 4.2 ka event are similar to those in the time period from 6 to 4.2 ka BP with a mean state of our entire record between 6 and 3 ka BP. Third, we'll characterize the aridity between 3.9 and 3.5 ka BP as a 'post-event' megadrought. Thus, our data provide new insights not only into the climatic variability during the 4.2 ka event, but also broader background information surrounding the event.

Minor things a) In Line 177, PATA1 should be PATA-1 b) de Boer et al. (2013, 2014, 2015) are listed in the references as "Boer", E.J.D., 2013 . . .", which left this reader scrambling. c) Edwards et al. (1987) is between the Cs and Ds in the references.

Answer- We agree. We will fix these in the revised manuscript.

Reference:

Railsback, L.B., Akers, P.D., Wang, L., Holdridge, G.A., Voarintsoa, N., 2013. Layer-bounding surfaces in stalagmites as keys to better paleoclimatological histories and chronologies. Int. J. Speleol. 42, 167–180.

Railsback, L.B., Liang, F., Brook, G.A., Voarintsoa, N.R.G., Sletten, H.R., Marais, E., Hardt, B., Cheng, H., Edwards, R.L., 2018. The timing, two-pulsed nature, and variable climatic expression of the 4.2 ka event: A review and new high-resolution stalagmite data from Namibia, Quaternary Science Reviews, 186, 78-90.

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