Clim. Past Discuss.,
https://doi.org/10.5194/cp-2018-92-AC1, 2018

Climate of the Past Discussions

Interactive comment on "Timing and Structure of the 4.2 ka BP Event in the Indian Summer Monsoon Domain from an Annually-Resolved Speleothem Record from Northeast India" *by* Gayatri Kathayat et al.

Gayatri Kathayat et al.

kathayatgayatrintl@gmail.com

Received and published: 24 October 2018

We express our gratitude to anonymous Referee #1 for the review of our manuscript. We find the comments supportive for substantially improving the manuscript. In the revised version we will incorporate the reviewer's comments. We respond to all the comments as follows.

Comment #1. Paper structure: -Part of the result are presented in the introduction. Especially, lines 107 to 110 should be moved and combined with the beginning of

section 2. -Part of the discussion is within the method section, i.e. paragraph 2.2 (proxy interpretation) should be moved at the beginning of section 3. -Methods are also very confused. Please make separate paragraphs: one for sampling for stable isotope analyses, where the adopted sampling resolution should be stated (i.e. combine lines 167-171 with section 2.6); another for the U/Th dating (paragraph 2.4 is fine) and one for the age modelling procedure (paragraph 2.5 is a mix between method and results, e.g lines 193-196 are not method, they're results). – The same for Results: I suggest a first paragraph (Chronology or similar) where periods of growth, resolution of the dating and temporal resolution of the stable isotope record are clearly stated. This information is now part in the intro, part in the methods and part in the results. Another paragraph should describe properly the new _18O record. Part now is in line 239-242 and part in section 3.2, but there is mixed with description of the previously published KM-A record, in my opinion this comparison should be moved later. It is fine to have it on a separate paragraph as it is now, but new stable isotope results, and the comparison between the two new record, should be described before. Also please remove discussion about replication from captions of figure 4. Captions should only describe the figures, they cannot contain part of the discussion. I suggest also to insert in this new paragraph a brief discussion about deposition occurring or not close to equilibrium condition. The replication of the same _18O pattern in the two new samples is a strong evidence for the "goodness" of the samples, but it needs to be clearly stated and it should be accompanied by some consideration about the petrographical features (see e.g. Frisia et al., 2002 or 2010). To this end, I think that also a description of the petrography of the samples (which is the dominant fabric? could it be interpreted as related to equilibrium condition?) is needed, maybe alongside their macroscopic appearance, which now is only briefly mentioned in lines 165-116 and 171-172).

Answer #1. We thank the reviewer for providing these suggestions. We will revise the manuscript structure incorporating the reviewer's comments. Following the reviewer's suggestion, we will improve the discussion to clearly describe that why replication test can be considered as a strong evidence for isotopic equilibrium conditions at the time

of speleothem formation. The detail petrography is beyond the scope of this paper but the basic petrographical examination of the sample indicates that there are no known petrographical features present in our samples that can be ascribed to disequilibrium precipitation of the speleothem samples.

Comment #2. Replication. I think that the use of ISCAM, Figure 4b and part of section 2.7 are not needed. The output of this method changes the final isotope values and this is, in my opinion, a little bit an artefact. I think the similarity between the ML1 and ML2 isotope curve is clear and convincing. And it can be better highlighted by some modification in Figure 4, i.e. by plotting ML1 and ML2 results on separate axes. In this way the readers can evaluate similarities and differences by themselves. And I would do the same also in Fig. 5. line 241-242: Not clear what authors exactly mean with “karstrelated differences”. Do they refer to different altitudes of recharge for the drips feeding the different stalagmites? Are there information about the rainfall isotopic altitudinal gradient? In some settings, differences of few hundreds meters in the main altitude of recharge can easily explain differences up to 0.5‰ in different speleothem oxygen records, even from the same cave chamber. Also, partitioning of the plumbing system, with different compartment having different mixing and residence time may account for these small differences. Please explain more clearly.

Answer #2. We will replace Figure 4 with raw data plot and moved the ISCAM figure to the Supplementary Figure S1. We will further improve the explanation in the revised version.

Comment #3. Comparison with KM-A: lines 279-281: I think that authors are right and that the abrupt end of the 4.2 event in KM-A is likely to be related to dissolution features occurring near the top of the sample. However, I do not understand why the presence of aragonite should add support to this hypothesis: is it because aragonite is usually indicative of drier conditions (e.g. Frisia et al., 2002)?. Or because the top mm of KM-A are not primary calcite but diagenetic calcite resulting from aragonite transformation? (but in this case, values should be anomalously enriched, and not depleted, see e.g.

Zhang et al., 2014). Please explain this more clearly.

Answer #3. We will further improve the explanation in the revised version. However, based on previous studies and our observation inside the Mawmluh Cave, the aragonite coating is also quite widespread in several chambers of Mawmluh Cave and the local guides reported its appearance since the 1960s with the advent of intense mining activity above the cave (Biswas et al., 2009). The most depleted δ18O values in KM-A record defining the termination of the 4.2 ka event are recorded in the first 1-2 mm of calcite portion of KM-A sample just below the aragonite layer implying a possibility that the structure of the ‘4.2 ka event’ in the KM-A record could have been altered to some extent.

Comment #4. Discussion (section 3.3) There is no an indication on how the z-score was constructed and on what it means precisely, some explanation on this must be added. Also, I would enlarge the comparison with the other quoted records by creating a figure where all the _18O records are reported. Fig. 6 is, in my opinion, a nice synthesis, but it prevents the reader from evaluating independently the degree of coherence/ dissimilarity between the different records, the different temporal resolutions and so on. So I would add a figure with all the records to be put before the synthesis represented by Fig. 6. Finally, (but very important in my opinion!) the discussion is totally lacking some considerations about the potential causes and forcing for the observed ISM variability at time of the 4.2 event. There are several hypotheses about that, which were reported in some of the works that the author quote for comparison (e.g. solar variability, Staubwasser et al., 2003; feedbacks with mid-latitude westerlies, Berkelhammer et al., 2012; changes in large-scale tropical ocean-atmosphere dynamics, like in the Indian Ocean Dipole (IOD) and El Niño Southern Oscillation (ENSO), Dixit et al., 2014, just to quote some..). These hypotheses need to be briefly presented and discussed on the light of the new results. This would add “scientific thickness” to the new record and would greatly improve the interest of this new study.

Answer #4. The Z-score was calculated by using the mean and standard deviation of

the entire ML.1 δ18O record. We will add this explanation in the figure caption. We will add a new proxy-syntheses figure (using selected proxy records from the Indian Monsoon region) in the supplementary section. The reviewer is right, we also think that the discussion section was not impressive in the manuscript. In the revised version we will improve the discussion section, incorporating the reviewer's suggestions.

Technical corrections:

Technical Remarks #1. Table 1 must be moved into the main text. As one of the strengths of this work, and of speleothem works in general, is the accuracy and quality of the U/Th chronology, the readers should have information about the dating fully available.

Correction #1. We will make the correct corrections in the revised version.

Technical Remarks #2. 230Th dating is used throughout the text to indicate the Uranium-Thorium method and dating. I suggest replacing it with "U/Th dating", as it is the more common and correct form to indicate this method.

Correction #2. Thank you for the suggestions. However, U/Th dates are also expressed as 230Th (see Table 1), and the publications from our group have used the same terminology therefore for being consistent we would prefer using 230Th.

Technical Remarks #3. line 17: I suggest to change "less clear" with "unclear" Correction #3. We will make the correct corrections in the revised version.

Technical Remarks #4. line 43: climatic anomalies is a very vague term. It can indicate almost every climatic state, from very wet and warm to very cold and dry. The global expression of the event (which is almost everywhere characterized by dryness) needs to be better explained, at least in the introduction. Correction #4. We will make the correct corrections in the revised version.

Technical Remarks #5. line 61: remove "a" before "two centuries.." Correction #5. We will make the correct corrections in the revised version.

Technical Remarks #6. line 65: add "previous" or similar before "speleothem record" Correction #6. We will make the correct corrections in the revised version.

Technical Remarks #7. line 65: Quote Fig. 1 after "Northeast India", the same in line 89 line 88: add "expression of" before "the 4.2 event" line 91: remove "event" after "record" line 95: "only" is repeated twice in this sentence, remove one line 129: is the value of 11000 mm correct? line 265: change "manifest" with "manifests" or with "appears" line 270: change "margin of age uncertainties" with "combined age uncertainties" lines 287-288: Fig. 6 is quoted double, remove one line 299: there is a typo in "notably" Figure 5: U/Th ages are reported in Fig. 4, there is no need to report them also here.

Correction #7. Thank you for highlighting the mistakes. We will correct the mistakes in the revised version. The annual precipitation in this region is indeed approximately 11000 mm. The Mawmluh Cave is located near the town of Cherrapunji, which is one of the wettest locations on the planet. We provided 230Th dates in Figure 5 to further illustrate our chronologic constraints for the drought events discussed in the text.

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