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Climate of the Past
Discussions

Interactive comment on "Possible expression of the 4.2 kyr event in Madagascar and the south-east African monsoon" *by* Nick Scroxton et al.

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We would like to thank the two anonymous reviewers, and Dr. Voarintsoa for their discussion. We believe the manuscript has been improved via their input. In this final report we provide a summary of the main discussion points raised by the reviewers, either individually or together. Individual detailed responses have already been submitted as part of the discussion phase.

The main issue raised by all three reviewers was that the evidence provided for the hiatus in stalagmite AK1 was insufficient to prove a dry event. In the majority of stalagmite studies we have seen, a hiatus that replicates between two different caves and which shows a positive isotope excursion leading into the event would be considered sufficient evidence of a dry event. And while ‘wet’ hiatuses are possible, they seem very rare in the literature. However, as pointed out, this hiatus is not just part of the record, it is the central result of this manuscript. Therefore, we agree with all three reviewers that the hiatus warrants extra scrutiny. As suggested, we have investigated the stalagmite in light of the layer-bounding surfaces framework of Railsback et al. (2013) and determined the hiatus to be a Type L bounding surface, one likely caused by dry conditions. The evidence for this is an absence of truncated layers, a slight thinning of layers and narrowing of the stalagmite, an increase in d18O into the hiatus, and little detrital material. The third paragraph of section 4.1 is expanded to include this new information. A small fourth paragraph is included to briefly discuss a second Type L bounding surface at 694mm, as suggested by Dr. Voarintsoa. In our response to reviewer 1 we erroneously stated that there might be a potential contraction crack from the recrystallization of former aragonite – we no longer believe this to be the case, unless selective recrystallisation occurred at the hiatus but not the rest of the aragonite stalagmite, which would be highly speculative. We also include a new figure four which shows three close-up images of the hiatus, one annotated.

Reviewer 2 asked us to go further with the description of the hiatus and include a discussion on the relative roles of age model uncertainty and age model choice in the difference in timing of the hiatus between AK1 and ANJ94-5, the replicating stalagmite from nearby Anjohibe. We agree that age model uncertainty plays a role in the difference, as does age model choice. However, likely of equal or greater importance is the drip hydrology of the two stalagmites. The onset of drying (positive d18O excursion) in the two stalagmites are much closer in age than the physical changes (hiatus onset). This suggests that the exact timing of the hiatus onset is determined by the size of the karst water store and drip hydrology. We are happy to include this discussion, but we caution that interpreting age model differences less than the error bounds of the stalagmites could be regarded by other reviewers as over-interpretation. We hope we have found the correct balance between nuanced discussion and avoiding over-interpretation. The first half of section 5.2 has been expanded to include the discussion on this topic. We have, as suggested, placed it ahead of the discussion on the isotopic similarities between the two records.

We disagree with reviewer 2 that the stalagmite age models need to be re-run using the same software. Our choice of age model is based on the U-Th age profile, frequency of dating and known biases of the nine (or so) age modelling software packages available to the stalagmite community. In our opinion it is more prudent to discuss the biases of the age models of the two stalagmites, than it is to run both stalagmites using the same age model and therefore come up with a more similar answer, but one in which the bias is not removed.

Reviewer 2 also asked for more detailed discussion of the chronologies of the other records, saying "that the hydroclimate anomalies in these records is synchronous with the 4.2 kyr event is therefore too optimistic and not really supported by all of the records". We agree with the Reviewer 2 here. Acknowledging that there is doubt over the synchronicity with the 4.2 kyr event was our aim. Therefore, we needed to state this more clearly, and introduce a more thorough discussion of the age model errors of other records. This is not a straightforward task, as most studies no not discuss age model error, or even calculate error at interpolated data-points. Mostly we have to rely on the age determination errors, which are frequently uncalibrated radiocarbon ages, and are of course smaller than age errors on interpolated ages. A full discussion on age model uncertainty resolved synchronicity is beyond the scope of this manuscript. In fact, it is the scope of our companion manuscript cp-2020-138.

We have included a more thorough description of the age uncertainties in the other records at the start of section 5.3 and rephrased several sentences in section 5.4 to add to the discussion over age uncertainty and synchronicity with the 4.2kyr event. We also decided to make a stylistic change to the entire manuscript based on this comment. We have decided to remove "the 4.2 kyr event" as a phrase throughout and restrict its discussion to section 5.4. We have replaced "4.2 kyr event" with "mid- to late- Holocene transition". This avoids the linking of the two events until it is ready to be discussed in full.

Dr. Voarintsoa asked us about the interpretation of the stable oxygen isotope record of AK1, and the potential factors that drive oxygen isotope variability inside the cave, including disequilibrium fractionation. We agree with this line of questioning in general terms but feel it is a more general question that can be asked of all records published so far from the region. As this discussion applies to all stalagmites from the region, we discuss this in section 5.1 rather than 4.1.

Our interpretation follows that of previous studies. The highly seasonal nature of rainfall at the site, the single source area of precipitation and the site's proximity to the ocean suggest that the amount effect likely dominates the d18O of rainfall. Given the aridity of the region and the openness of the cave, processes such as evaporative enrichment in the karst and disequilibrium fractionation during carbonate precipitation all likely play a role in signal modification. We believe we are quite open about these multiple potential processes but disagree with the assertion that we state our stalagmite grew in perfect equilibrium. This is erroneous, we do not make this claim. It is correct that disequilibrium fractionation is likely a component of all stalagmite precipitation, but it is the amount of disequilibrium fractionation that is important, and further, the amount of disequilibrium fractionation is likely driven by climatic processes.

Fully reconciling the different influences on stalagmite d18O at this site is beyond the scope of this paper, but we agree that a better understanding of the exact controls on d18O at this site is fast becoming a significant issue in stalagmite records from the region. We know that it is an area of active research, both by our group, and the work of Dr. Voarintsoa and we look forward to the publication of these studies in the coming years.

We have responded to the more minor comments from the reviewers in the individual responses, and thank them once again for their input.

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