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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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"This manuscript is interesting but, in my opinion, is lacking some fundamental aspects to fully comprehend the stable oxygen isotope proxies that the author used to reconstruct paleoclimate in Anjohikely. Among these are the description of the cave and its microclimate, where in the cave was it collected, and importantly what are the potential factors that drive oxygen isotope variability inside that cave."

We agree with this statement in general terms. The recent increase over the last five years on stalagmites from the area have now produced d18O records covering the vast majority of the Holocene. None of those studies have yet laid out a detailed explanation on the controls of d18O in the region.

Given the highly seasonal nature of rainfall at the site, the single source of precipitation, and its proximity to the ocean it is highly likely that the amount effect dominates. However other factors such as evaporative enrichment in the karst (eg. Markowska et al., 2020) and disequilibrium effects in the cave are likely to play some role – though their roles have not yet been quantified. The exact control of d18O at Anhjohibe and Anjohikely is becoming the biggest unanswered question in stalagmite records from the region.

This is not an easy question to answer, there is no local IAEA site for precip d18O, the nearest weather station does not produce reliable enough data for climatological studies, and access to the site is limited by its remoteness, particularly during the wet season, which hinders cave monitoring. Effort is being made by multiple research groups. The commenter themselves is working on studies on both the d18O of precipitation, and the microclimate of Anjohibe. The lead author of this study is currently working on high-resolution records covering the observational era etc. Over the course of the next few years, the publication of these studies will shed new light on the interpretation and nuance of d18O in caves in northwest Madagascar.

As the top of AK1 does not extend into the observational era, this manuscript cannot shed more light onto the d18O mechanism without speculation. Section 5.1 (to be 5.2 in the revised manuscript) contains a discussion on d18O mechanisms with regards to the comparison of the similarities and differences with ANJ94-5, the coeval stalagmite. We believe this to be the limit of new information that can be gained from our data without speculation.

"About the cave: In an article published in 1997, Burney and colleagues described (p. 756) that Anjohikely is linked to Anjohibe with a subterranean passage (while citing Laumans et al., 1991). In addition to this, Burney et al. (1997) noted that "most passages in Anjohikely are relatively small in diameter, with somewhat limited development of speleothems (stalactites, stalagmites, and other dripstone formations). This makes me wonder about this sample from Anjohikely, particularly that its size is relatively big if considering the former description and speleothem investigation by Burney et al."

We disagree with some aspects of the description provided by Burney et al on Anjohikely and stand-by our description of the cave in the third paragraph of section 5.1 (to be 5.2 in the revised version). Anjohibe certainly has a large number of very large stalagmites and flowstones, but in many places is reasonably bare of decoration. Anjohikely has far fewer large specimens but has a much greater percentage coverage of calcite. Burney et al 1997 is correct in stating that Anjohikely has tighter passages.

We have not been able to trace the Laumanns 1991 reference. We can only find Laumanns and Gebauer 1993. If the commenter has a copy, we would appreciate them sending it on.

"It may be helpful if the authors provide, at least, a skecth or map of the cave, with some illustrative figures of the cave entrance and the chamber from where the speleothems were extracted."

We have not seen a full survey map of Anjohikely. We believe that publishing an inaccurate sketch would not be informative.

"It would also be helpful to have a brief note about the overall microclimatic condition inside the cave (what is the air temperature inside, how about pCO2 and relative humidity [RH])? Somewhere in the discussion (L. 224-229) that the authors discuss about kinetic fractionation as processes affecting stalagmites collected from Anjohibe (Wang et al., 20190) versus stalagmites from Anjohikely (their work). If the cave atmospheric exchange with its exterior atmosphere sounds important, information about cave microclimate (T, pCO2, RH) could be crucial and discussed with more details. Also, I am far from believing that stalagmite growing in Anjohikely was precipitating in equilibrium with the cave drip water, as several studies have proven that almost none of the terrestrial carbonates precipitate in isotopic equilibrium with their drip water (Mickler et al., 2006; Tremaine et al., 2011; Day and Henderson, 2011; Deininger et al., 2012, Daëron et al., 2019)."

At no point in the manuscript do we suggest that our stalagmite was precipitated 100% in equilibrium, no stalagmite is. However, we have added a clause to explicitly state that the stalagmite did not grow under equilibrium conditions.

The comment is correct in stating that almost no terrestrial carbonates precipitate in isotopic equilibrium. The question is always ‘how much disequilibrium fractionation is occurring’, and ‘is the magnitude of the disequilibrium effects on the stalagmite isotopes larger or smaller than the effects derived from climatic changes in d18O of precipitation’. Given the number and size of entrances of Anjohibe, Anjohikely and indeed all the caves in the region it is almost certain that AK1 and all regional stalagmites formed in some form of disequilibrium. The relative amount of which could be influenced by temperature, pCO2, and relatively humidity. There are numerous other potential influences on stalagmite d18O outside of disequilibrium vs equilibrium effects too, relating to storage, mixing, in-karst evaporation (e.g., Markowska 2020) and seasonal growth. As described above, there is a definite need for more detailed understanding of modern isotopic behavior in the region.

We regret that the sporadic nature of our visits to northwest Madagascar and the isolation of the area have prevented us from carrying out a full investigation into the cave microclimate. We did on, our last trip, install T and RH data-loggers at certain key sites, but do not yet have a record of significant length.

We consider these comments to be valid criticisms of this stalagmite, but also of the eight other stalagmite records published from the area. In our study, we clearly lay out which sections of the stalagmite (e.g., above 99mm) where we suspect disequilibrium effects may dominate the stable isotope record. We also provide in-depth discussion of how our stalagmite results compare to coeval stalagmite records and the likely causes of agreement and disagreement. We agree that there is always more that can be done in any stalagmite study to probe the proxy system. But, we believe our paper provides a reasonable discussion of various potential influences on stalagmite d18O, and the areas where we believe climatic processes dominate cave processes and vice-versa.

In contrast, other recently published stalagmites from the area show evidence of disequilibrium fractionation and a lack of replication without mention or discussion of the nuances of stalagmite d18O interpretation. For example: stalagmite ABC-1 (Li et al, Science Advances 2020), shows a dramatic, non-replicated, isotopic trend in a stalagmite from Anjohibe, which cannot be reasonably attributed to climatic change, and must be due to disequilibrium and drip hydrology effects. This was passed off as a climatic signal with no explanation of potential confounding effects. We have confidence that our stalagmite records climatic variability far more reliably than several recently published stalagmites from the area.

"About some statements in the paper: I disagree with the statement at Line 44 that "the impact of the 4.2 kyr event on the tropics and subtropics is unknown". âAĚŸ c Please ′ consult Railsback et al., 2019, QSR, in which a review of the 4.2 ka event was presented with a new isotopic record from Namibia. As the authors also make a comparison with another stalagmite from Anjohibe about the 4.2 ka (Wang et al., 2019 QSR), this statement "unknown" needs to be revised. Other relevant information from India could be known by reading Kathayat et al. (2018, CP) and from China (Zhang et al., 2019 CP) Some comments in this section may be applicable to the preprint of the same authors (Scroxton, N., Burns, S. J., McGee, D., Godfrey, L. R.,Ranivoharimanana, L., and Faina, P.: Circum-Indian ocean hydroclimate at the mid to late Holocene transition: The Double Drought hypothesis and consequences for the Harappan, Clim. Past Discuss. [preprint], https://doi.org/10.5194/cp-2020-138, in review, 2020)."

We agree this was an overgeneralization. There are a few papers that discuss the 4.2 kyr event, but nowhere near the number that discuss the event at temperate latitudes, see Kaniewski 2018, Bini 2019 and the Climate of the Past Special Issue in 2019. We have changed the sentence to: "In particular, the impact of the 4.2 kyr event on the tropics and subtropics is relatively unknown, particularly in the southern hemisphere, with only a few detailed studies, see Railsback et al., (2018)."

"I am also curious to know the opinion of the authors about the two replicated mid-

Holocene hiatuses reported in Voarintsoa et al. (2017 CP) in this region (one in Anjohibe, and another in Anjokipoty). As Anjohikely is a small cave, it is expected to behave like Anjokipoty (hence, should record the same hiatus of mid-Holocene deposition), and this comes back to my point earlier about the need of more details about the cave."

For the reader: The replicated hiatus in stalagmites ANJB-2 and MAJ-5 presented by Voraintsoa et al 2017 is between 7.8 and 1.6 kyr BP.

Wang et al. 2019 (QSR) has previously shown that the majority of this hiatus contains speleothem growth so there is no reason to suggest a multi-millennial scale drying of sufficient magnitude to stop speleothem growth. The presence of AK1 between 5 and 2 kyr BP confirms this. Of the hiatuses in stalagmite ANJ94-5 presented by Wang et al, the one at 4.2 kyr BP is replicated by AK1 and widely discussed in this paper. The 800 year hiatus around 6kyr BP and the 260 year hiatus around 8 kyr BP are both outside the growth interval of AK1 and therefore we cannot add to this discussion.

Anecdotally (and perhaps speculatively) we note that very few stalagmites from the area grew between the top of this stalagmite at 1.9 and the bottom of several stalagmites (ANJB-2, MAJ-5, AB2, AB3) around 1.7 kyr BP. This could indeed be indicative of a dry period in the region, and would explain the drying/progressive disequilibrium signal seen in AK1 from 2.3 kyr BP onwards. Therefore, our opinion is that dry periods lasting a few centuries may well be a regular feature of Holocene northwest Madagascar climate. Clearly further study is required from the stalagmites that do exist to determine the exact nature of this potential dry period, and other dry periods, in the region.

"Interpretation of the data In Figure 2: the authors indicate a hiatus within the palest color of the oldest generation of stalagmite. I wonder if this hiatus should actually be located at the boundary between the pale-brown color and the whiter color stalagmite (see annotated figure below), and if each of the color change throughout the sample too can indicate other short term growth hiatus? As a matter of fact, only the bottom stalagmite has more age data. In contrast, the upper part of the stalagmite has lesser trenches. Why is that? Has there be any diagenesis at that brown-pale bottom part that allowed loss of U, hence the samples appear older?"

A hiatus at the change in color does indeed seem more likely at first glance and was indeed our working hypothesis for several months. However, 1) the U-Th age at 710mm is below the change in color, 2) there is consistency between growth rates below the hiatus at 707mm and above the hiatus until the U-Th date at 631mm, 3) Ages below the hiatus do not show consistently lower uranium concentrations than those above suggesting little or no uranium loss. There is no evidence of diagenesis. A higher position of the hiatus combined with the necessary drastic changes in growth rate is speculative and not supported by the data.

The presence of more U-Th ages below the hiatus occurred because we aim to achieve a consistent sampling of ages with respect to time, rather than with depth.

"At L. 150: Growth hiatus, what are/is the rational for saying "there is a growth hiatus"? I wonder what are the rational for saying that it indicates dry or wet conditions? While looking at the time series in Figure 3, it appears that the isotopic value bracketing the so-called hiatus are showing more negative values. Wouldn't this hiatus represent a wet condition? (May be an evaluation of the petrography (e.g., Railsback et al., 2013) would be useful here."

The rational for a growth hiatus is explained in the opening paragraph of section 5.1 (updated to section 5.2 in the revised version). Briefly, 1) AK1 shows a positive excursion into the hiatus not negative as suggested (although we do accept there is a negative excursion in the preceding few decades). 2) the hiatus is replicated in ANJ94-5 from nearby Anjohibe, which suggests the hiatus is driven by climate rather than a drip specific or cave specific change, 3) ANJ94-5 also shows a positive excursion into the hiatus, indicative of drying.

This comment is very similar to one made by Reviewer #1. We respect both of the commenters opinion that additional evidence is needed for a dry hiatus:

Upon reinspection of AK1 images. We can confirm that there are no truncated layers, a slight thinning on the stalagmite, an increase in d18O into the hiatus and a contraction crack (likely formerly aragonite) with little detrital material. We therefore believe this to be a Type L layer bounding surface, one caused by decreased precipitation. We have added this description to our results section and included the Railsback et al., 2013 reference. We believe that our manuscript is more robust and thank both the reviewer and Dr. Voarintsoa.

The new paragraph now reads: Between 4.30 and 3.84 kyr there is a growth hiatus. The layer bounding surface has no truncated layers, a slight thinning on the stalagmite, an increase in ïĄď18O into the hiatus and a contraction crack (likely formed by the conversion of aragonite to calcite) with little detrital material. We interpret the layer bounding surface as Type L, one caused by decreased precipitation (Railsback et al., 2013). Further, the hiatus is replicated in stalagmite ANJ-94 from Anjohibe at (4.20–3.99) (Wang et al., 2019b), also with a positive isotope excursion just prior, ruling out cave or drip specific drying. The replicated hiatus likely indicates dry conditions and potentially the driest conditions of the mid/late Holocene. The 4.2 kyr event therefore appears at least locally remarkable in northwest Madagascar. A dry anomaly is the opposite to the wet conditions recorded at 8.2 kyr BP (Voarintsoa et al., 2019), a Holocene climatic anomaly often viewed as a greater magnitude version of the 4.2 kyr event (Bond et al., 2001; Wang et al., 2013)

In addition, we are also returning to the stalagmite to see if a more detailed review of this layer bounding surface is necessary. We will report back as part of the formal response to the reviewer, but wanted to give this response more quickly, as part of the discussion phase.

"Presentation of the manuscript: I feel that the authors should clearly write a results section and not combine results with discussion, or vice versa. Some short interpretations of the results are acceptable, if these are meant to emphasize the findings, but results should report results. With that said, In section 4.1 they should elaborate on the isotopic range, if there are any periodicity, highlight the extreme positive/negative excursion, and provide evidence of the hiatus. The author should also discuss about the growth rate. By looking at their Figure 3a, it seems that growth rate of the bottom part of the sample is slow vs. the upper part of the sample. I also feel that some information presented in the discussion belong to the results section (if not mentioning paragraph at L 230, L 235, and L. 242)"

This is a stylistic discussion. Our results section is organized in a result-interpretation format. Each paragraph begins with a result, and is followed by its interpretation: result 1, interpretation 1, result 2, interpretation 2, result 3, interpretation 3. We find this format to be less dry, more readable and less repetitive than: result1, result2, result3, interpretation1, interpretation2, interpretation3.

We agree that a description of the growth rate would be beneficial here and have included an extra sentence.

"The section about the regional variability in the African monsoon (Section 4.2) does not seem to belong into the results section."

Section 4.2 does not contain new results and therefore could be considered discussion. However, Section 4.2 is purely descriptive, and at no point interprets the data, and therefore could be considered results. Which section it belongs in is purely a stylistic distinction, but we have no issue with making this section 5.1 instead of 4.2 and have made the suggested change.

"Other detailed comments: At L 145: Can you please elaborate, or be specific, on the statement "change in drip hydrology" and "change in cave ventilation regime"?"

We have changed this paragraph to give more detail from the literature as to potential

specific causes of changes in drip hydrology.

"Figure 3: Can you please replace "Speleothem depth" with "distance from the top of the speleothem? In my understanding, depth is most commonly applied to sediments that are dig underground." Changed as suggested

"The authors mention in passing the diameter, the shape, and location of the drip axis (e.g., L. 245), it would be better to apply the layer-bounding surfaces approach (such approach was used in Wang et al., 2019 QSR) to quantify such changes. About the shape of the stalagmite again, I think there is quite a number of literature that they could use to back up their statement." We hope that the increased detail in the results section negates the need to alter this paragraph. We are not convinced that layer bounding surfaces approach constitutes quantification.

"Minor editorial errors: For some reasons, several of the in-text citations are replicated, if not only mentioning some at Lines 31, 36, 40, and 73). I guess some attention from the authors to avoid such replication is appreciated." Thank-you for pointing those errors out. Likely a result of the referencing software used. We have made the appropriate changes.

"If you use aragonitic, then calcitic seems to be parallel. However, it may be better to use aragonite and calcite (e.g., aragonite section..)" We agree, and have changed all aragonitic to aragonite

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