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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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See attached pdf with the figure

Comment for: Scroxton, N., Burns, S. J., McGee, D., Godfrey, L. R., Ranivoharimanana, L., and Faina, P.: Possible expression of the 4.2âĂĽkyr event in Madagascar and the south-east African monsoon, Clim. Past Discuss. [preprint], https://doi.org/10.5194/cp-2020-137, in review, 2020.

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



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factors that drive oxygen isotope variability inside that cave.

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.

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.

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).

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”. âĂć Please consult Railsback et al., 2019, QSR, in which a review of the 4.2 ka event was pre-

sented with a new isotopic record from Namibia. âĂć 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. âĂć Other relevant information from India could be known by reading Kathayat et al. (2018, CP) and from China (Zhang et al., 2019 CP) âĂć 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) 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.

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?

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.

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, âĂć 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. âĂć 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)

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

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"?

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.

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.

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

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

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Please also note the supplement to this comment:
https://cp.copernicus.org/preprints/cp-2020-137/cp-2020-137-SC1-supplement.pdf

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