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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 reviewer for their contribution. They have raised some interesting points which certainly require discussion, and in many cases, the addition of more detail and more nuance to our manuscript, which we feel has improved the manuscript.

"There are still a lot of discussions surrounding the infamous 4.2-kyr event as its timing, nature and spatial extent remain uncertain, not to mention the lack of evident forcing mechanisms (e.g. solar, volcanic, AMOC. . .). New precisely dated records are therefore needed to reduce the current uncertainties, particularly from the tropics. The new stalagmite record from Anjohikely Cave in northern Madagascar shows a hiatus be-

tween 4.32 and 3.83 kyr BP and thus confirms a hiatus in a stalagmite from Anjohibe Cave approx. km away from Anjohikely. However, the hiatus in stalagmite ANJ94-5 from Anjohibe Cave lasted from 4.2 to 4.0 kyr BP which is most likely related to slightly different age models, but not really covered in the manuscript. In my opinion, the complete lack of an adequate discussion of the age models and the resultant timing of the 4.2 kyr event is one of the main weaknesses of this manuscript and major revisions are required to address this crucial aspect. There is quite a lengthy discussion on the isotope profiles which seems to be slightly disconnected from the main aspect (the climate-induced hiatus) of the manuscript. The authors should highlight the positive shifts at the onset of the 4.2 kyr event more effectively in order to document the abruptness of the 4.2 kyr event in greater detail. While we are here, it is quite interesting that stalagmite ANJ94-5 (Wang et al., 2019) is showing a positive shift in del18O whereas such a comparable shift is missing in the AK1 profile (Fig. 5). What is the reason for this mismatch? Is there a possibility to increase the sampling resolution of the AK1 isotope profile to identify such a comparable isotopic towards the 4.2 kyr event? As mentioned above, a detailed discussion about the onset of the 4.2 kyr event should be a central aspect of the paper, which would include a detailed discussion about the uncertainties of the stalagmite age models (AK1 and ANJ94-5) which are based on different approaches to develop an age model: the age model for stalagmite AK1 is based on OxCal and the one for ANJ94-5 is based on StalAge. I would recommend to use the same age modelling approach and to cite the 2-sigma uncertainties for the ages of the hiatus throughout the manuscript."

As suggested by the reviewer, the offset age between the onset of the hiatus in AK1 and ANJ94-5 can certainly be treated in more detail, and we are happy to provide more information on what is the key conclusion of our manuscript, and we have made several changes to section 5.2.

We believe the offset in the hiatus starting point is likely a combination of two factors: age model discrepancies and drip hydrology. The last stable isotope datapoint of

ANJ94-5 is 4199 yrs at 520mm. Age uncertainties are not supplied with the ANJ94-5 data but are likely of the order of +-30yrs, which is the 2s uncertainty at the nearest age at 535mm. StalAge has a constant growth-rate bias, and the ANJ94-5 age model is 40 years too young at the nearest age tie point, outside the 2sigma uncertainty. This suggests the ANJ94-5 age model may run too young at the hiatus also by around forty years. In comparison, the last stable isotope datapoint of AK1 is 4307yrs at 707.25mm. The OxCal age uncertainty is +-60 years at the data-point, but is likely comparable to the ANJ94-5 uncertainty given the nearest U-Th tie point at 710.5mm is 4325yrs +-34. OxCal has its own mean growth rate bias when extrapolating beyond tie-points to hiatuses. We calculate the effect of this on the age of the last stable isotope point to be 5 years (too old). Therefore, a supposed 90 year difference in hiatus may only be half that.

However, the age model discrepancies may be irrelevant when considering that the two stalagmites have different drip hydrologies, and therefore are likely to dry out at different rates. (This is why re-running both stalagmite age models with the same software would not necessarily be informative). The exact timing of a hiatus is unique to each stalagmite. The temporal resolution of AK1 is 4-5x that of ANJ94-5, and the sampling resolution is 6x so an higher resolution pre-excursion transect of AK1 would likely not include any additional information. We are happy to clarify this point in the manuscript too as it contributes to our discussion on drip hydology differences.

If the hiatus is idosyncratic, then perhaps instead we should be looking at the drying before the hiatus. For example, we can look at the last low d18O value before the hiatus. In ANJ94-5 the last low d18O value below -5 per mill before the dramatic drying trend is at 4295yrs at 536mm. This is just 12 years different to the hiatus of AK1, which given a possible young bias in the ANJ94-5 age model and a sampling resolution of 23 years could be considered simultaneous. Alternatively, we could choose the last d18O minima in both stalagmites. In ANJ94-5 the last d18O value below -6 per mill, which appears to be a more natural peak, occurs at 4337 yrs (543mm). In AK1, the d18O

minima prior to the hiatus occurs at 4321 +-46 years at 708.75mm. Again, this can be considered simultaneous. Can a unified age be established here? 4320 yr BP +-50 years seems reasonable. But the issue here is whether the exact choice of location is subjective, and due to the proximity of the hiatus, a statisitical fit is likely to fail without sufficient post change data points.

Instead, it might be better to look at broader centennial scale trends rather than isolate individual stable isotope points. Both stalagmites show wetting signals beween 4.7 and 4.6 kyr BP, and a drying from 4.5 kyr onwards.

All of this analysis is indeed possible. The question we also have to ask is whether this analysis is robust and within the bounds of reasonable interpretation of the data. We could easily imagine presenting this analysis and a different reviewer telling us that we were overreaching the data, given the dating resolution, the sampling resolution and the idiosyncrasies of drip hydrology. We hope we have struck the right balance in our revisions.

“Furthermore, a better documentation (thin section or macro images) of the hiatus would make the manuscript much stronger.”

In light of the comment from Dr. Voarintsoa, and the review from anonymous reviewer 1, we think this is a good suggestion for providing extra information on the hiatus.

“The authors should also add a more detailed discussion of the chronologies of other hydroclimate records from southeast Africa (e.g., Lake Malawi, Lake Masoko) as the timing of the onset of drought conditions is crucial. The onset of drought conditions at Lake Masoko began at 4.5 kyr, at 4.4 kyr at Lake Malawi, 4.3 at Anjohibe and 4.5 kyr (see chapter 5.2). The authors comment on this in only one (incomplete) sentence “The age errors for most records are around ±600 years (2-sigma) for the stalagmite records and ±200 years (2ï ËŻA Âÿs) for most other records”. Their conclusion 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.Chapter 5.3

"Timing of the middle to late Holocene climate shifts in the SEAfM" must be therefore expanded to document the chronologies of the key-records in much greater detail. Overall, a proper evaluation of the chronologies of the different records is required."

We agree with the reviewer comment that calling these records synchronous with the 4.2 kyr event is optimistic. This was our aim, as we discuss in section 5.4. However, it is clear that we did not make this as obvious as it could/should be. Therefore, we agree that to make this clearer, additional discussion is needed. Firstly, we have removed reference to the 4.2kyr event from section 5.3, and the first paragraph of the conclusion, to discuss what the records show without the bias of interpretation. Coincidence with the 4.2 kyr event is discussed exclusively in section 5.4.

Discussion of the age errors of each individual record is complicated. Most studies do not discuss age model error, or in many cases even calculate them. 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. Nether-the-less, we agree it is important to state these age errors in the manuscript. The large errors make correlating events difficult or easy depending on your proclivity. They are mostly within 2sigma range of each other, and mostly (but not completely) in range of the 2sigma 4.26 kyr event. Alternatively, you could view these errors as being large enough to be unable to reliably prove synchronicity. As 2sigma age errors are not cut-off boundaries of yes/no and are in fact arbitrary markers on a distribution where the mean/median (depending on whether they are Gaussian or not) is the most likely. Determining age model uncertainty resolved synchronicity is beyond the scope of this paper. Indeed, it was part of the deliberate choice in the separation of this paper with its co-submission, which does deal with the highest resolution, precisely dated records in the region, with publicly archived interpolated age uncertainty data. We believe it is more informative here to continue with the mean ages. We hope that the rephrasing of several sentences throughout sections 5.3 and 5.4 help better state this inherent uncertainty in the analysis.

Attached is the new Figure 4

Figure 4 caption: Close-up of the mid- to late- Holocene hiatus in AK1. a) image with no annotation. b) image with annotation. Red shaded areas denote U-Th date sampling locations, blue shaded areas denote stable isotope transect trench, visible laminae in thin black lines, the mid- to late- Holocene hiatus in thick black line, unmarked circular pits are stable isotope drill holes from a low resolution pilot study. c) higher zoom unannotated image of the mid- to late- Holocene hiatus.

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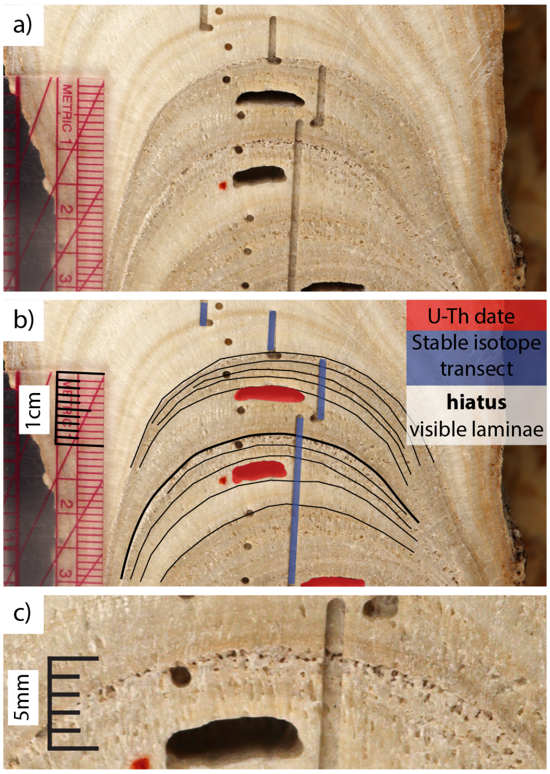

a)
b)
U-Th date
Stable isotope transect
hiatus
visible laminae
1cm
c)
5mm

Fig. 1.