

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

### **Anonymous Referee #2**

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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 between 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

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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  $\delta^{18}O$  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? Furthermore, a better documentation (thin section or macro images) of the hiatus would make the manuscript much stronger. 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. 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  $\pm 600$  years (2-sigma) for the stalagmite records and  $\pm 200$  years (2 $\sigma$ ) 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.

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