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Interactive comment on "Speleothem Evidence for Megadroughts in the SW Indian Ocean during the Late Holocene" *by* Hanying Li et al.

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General Comments In this high-quality paper Li et al. produce a much-needed record of monsoonal variability during the middle-late Holocene from the southern hemisphere. It fills an important spatial gap in our understanding of the 4.2 kyr BP event, and an important temporal gap of the middle Holocene from the southern Indian Ocean. Given the recent announcement of the Meghalayan age, this paper is also particularly timely. The fundamental conclusion of an expanded and contracted ITCZ during the middle Holocene rather than north-south translation is well founded and an important advance. It may not provide brand new concepts or methods, but not all papers have to do so to be important, and this paper still makes a substantial contribution to progress given the spatial and temporal gaps it fills. It is certainly a suitable paper for Climate of the

Past. I would rate it as excellent under scientific significance.

The record itself is of high-quality, with good replication from a nearby cave and a precise age model. The interpretation of the proxy is well anchored in modelled and observational data. There are a few inconsistencies within the paper (dealt with in the specific comments), but on the whole discussion of the results is balanced, without overreach. The conclusion reached, that of an expansion and contraction of ITCZ during the middle Holocene rather than north-south translation is well founded in places, but perhaps more could be done demonstrate whether this mechanism is functional throughout the entire 3000 year stalagmite growth phase rather than just a 500 year window.

However, I do not believe that one of the conclusions reached is supported by the data and believe there is a temporal mismatch between the interpretation, and what is going on. The authors correctly identify a drying trend that begins at 4.0 or 3.9 kyr and goes on to 3.5 kyr BP. They identify a regional coherency with other speleothem records at Liang Luar, Sahiya and possibly Tangga, and they suggest a plausible mechanism of expansion and contraction of the ITCZ range. However, they erroneously attribute this to the 4.2 kyr event (4.2 kyr BP-3.9 kyr BP) ignoring a small negative isotope excursion (wetter conditions) that exists in the new record and in the Liang Luar and Sahiya records. The regional replication of the signal and quality of the dating on this record and others is sufficient that this offset is unlikely to be due to age model errors. At this time, I would only rate the scientific quality as fair, but I do not believe much work is necessary for it to be excellent.

The paper is well presented and compact, focusing only on what's necessary, with few needless additions. It is well-written in understandable English. Altogether the writing is compact, although there may be one or two too many figures. Of recent papers I have reviewed, this one was the most enjoyable to read. I would rate it as excellent under presentation quality.

Specific Comments Where does the drying start? 4.2 kyr BP as the onset of the drying trend: Line 212 you say that the 4.2kyr event marks the onset of the drying trend. You also use other records as evidence for this around line 236 4.1 kyr BP as the onset of the drying trend: figure 9 deriving from an early d18O low 4.0 kyr BP as the onset of the drying trend: Lines 28, 204, 233, deriving from a late d18O low. Figure 8 3.9 kyr BP as the onset of the drying trend: Lines 27, 214, deriving from the change in mean state

When using 4.1 or 4.0 you measure from the lowest low d18O value (149.2mm or 145.2mm) of one of your two stalagmites. But that doesn't necessarily mean that's the point at which the climate changes. To say drying begins at the wettest part of a wet excursion is technically correct, but misleading. That's just the wettest point of a wet period. I'd be more convinced by the point at which the mean state shifts. Tools such as Rampfit or Bayesian Change Point Analysis would identify this quantitatively, but it qualitatively looks like 3.9 kyr BP to me. In the data there is abrupt shift from d18O values between 3and 4 per mill to d18O values between 2 and 3 per mill at 140.6mm (3887 kyr BP COPRA or 3934 kyr BP ISCAM) with the d13C changing 1mm earlier at 141.6mm (3904/3964 kyr BP). This is also the point at which the mean d18O switches to dry conditions (positive z-score) in figure 6. To me. 3.9 is a more convincing point to start taking about dry periods and drought (especially as the title is about Megadrought).

Is this the 4.2 kyr event or not? Given the age uncertainties on both the event as recorded elsewhere (maybe not great in individual records but I think the community is now pretty satisfied with 4.2 given the array of evidence) and in the stalagmite itself (excellent as expected) then 3.9-3.5 spell of dry conditions is not the 4.2-3.9kyr event. Even if you disagree with my argument above and take 4.0 as the onset of drying, I wouldn't necessarily call that coherence with 4.2 either. As you state in the abstract, the inferred hydroclimatic state over the length of the record is not distinguishable from the region's mean hydroclimate between 4.2 and 4.0 kyr BP.

When you then compare the new record with others, there's a regional coherency to this weak wet and strong dry phasing. Rodrigues has a small wet excursion (likely insignificant as stated in the abstract) between 4.2 and 4.0 kyr BP. You show this explicitly in figure 8. Figure 8 also shows the Liang Luar Record with a wet excursion during this period and drying after. Tangga Cave also possibly shows this pattern too (though its more ambiguous and maybe not quite robust enough to call 'wet'). In Figure 9 you also have Sahiya cave showing a wet excursion between 4.2 and 4.0/3.9 kyr BP, and then drying after*. Three or four precisely dated speleothem records, east and west of the Indian Ocean, north and south of the equator. All showing the same thing. Slightly, but not abnormally, wet conditions (4.2-4.0 kyr BP), dry conditions (4.0-3.5 kyr BP). In which case, does this paper show how unimportant the 4.2 kyr event is in this part of the world. I think you've got a great story here, and it's not being told.

* Kathyat et al., 2017 make the same interpretation here too, analaysing the onset of peak wet conditions as the start of a drying trend, instead of interpreting a wet period followed by a dry period.

What about other parts of the record? Section 4.3 is title about patterns of hydroclimate variability between 6 and 3 kyr BP. Yet the discussion focuses only on a seven-hundred-year period (4.2-3.5 kyr BP). While the proposed mechanism is plausible for this period, is it also plausible for the other 2300 years of speleothem record? I feel this deserves more discussion.

Additional Comments Line 134: Any justification for choosing the standard crustal value for the initial 230/232 ratio? Stratigraphic constraints, isochrons etc.

Line 144: Please state which age model do you end up using as your data?

Line 208: Could gradual positive trends and abrupt terminations be related to drip dynamics rather than climate? A gradually drying karst storage component with rapid fill would produce this kind of shape.

Line 217: On line 217 you state that the Lake Malawi record shows a weak dry excursion 4.1 – 3.5 kyr BP. On Line 254 you state that the Lake Malawi record shows virtually unchanged hydroclimate conditions. Figure 7 looks like very little change in the Lake Malawi record to me. You should probably delete the Line 217 sentence.

Line 258: I understand the desire to include brand new records in a paper, and that these sometimes have to be added last minute. But you introduce an entirely new concept (Southern Hemisphere Westerly winds and their control of restricting the southern range of the ITCZ) in the last sentence of the paper. You either need to introduce this concept much earlier or remove this record and discussion.

Various: Please be consistent with how you label and refer to different records between figures and text. It makes it much harder to follow your argument when you cannot easily switch between the two. Non-speleothem experts who do not know which country relates to which specific cave name and which specific stalagmite in that cave will find it difficult to keep track Line 215: Tatos Basin in text, Mauritius in figure 7 Line 220: Sumatra in text, Tangga cave in figure 8 Line 220: Northwest Australia in text, KNI-51 in figure 8

Figures: 9 figures is a lot for a short paper such as this. Could figures 8 and 9 be combined given they essentially show the same thing – i.e. regional coherency.

Technical Corrections Line 119: No need to use respectively twice in consecutive sentences Line 142: Spell out ISCAM at first usage. Line 172: No need to spell out ISCAM at second usage. Line 166: The Dorale and Liu test is even more convincing when consistent between two nearby caves, you should state this more explicitly that "speleothems from the same cave" Line 173: In line 70, you state PATA1 stops growing at 3.5 kyr BP. In line 173 you have overlap between the two stalagmites up to 3.4 kyr BP. Line 317: Th in superscript Line 349: Blank line? Figure 1: Coastlines should be in a darker color to make them clearer Figure 1: You should show the isohyet on the scale bar Line 497: 199?

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