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Interactive comment on "Circum-Indian ocean hydroclimate at the mid to late Holocene transition: The Double Drought hypothesis and consequences for the Harappan" *by* Nick Scroxton et al.

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In their review, Professor Weiss lists numerous regional and global hydroclimate anomalies that overlap, within the age uncertainty of those records, the 4.2 kyr event, and presents them as evidence of a synchronous climate event regardless of the seasonality of the proxy or the shape of the anomaly. Our aim is not to deny that these hydrological anomalies exist. We dedicate section 4.2 of our manuscript to describing these anomalies as they occur on the Indian subcontinent. Rather our aim is to put them into regional context. The seasonality of any individual paleoclimate proxy

is likely ambiguous. As the paleoclimate community moves towards integrating data from multiple sources ("big data") and accounting for time uncertainty, the original interpretations of individual records may not all hold in light of improved understandings of regional spatial variability.

By studying numerous records across the Indian subcontinent, we are able to separate out these records into three separate groups – 1) gradual drying, 2) 4.2kyr event abrupt drying and reversal at 3.9 kyr BP, and 3) a step-change drying at 3.97 kyr BP. The first of these is a well-recognized millennial scale trend over the entire Holocene. The second is consistent with drying seen across the Middle East (now discussed in more detail thanks to the suggestions of reviewer #1). The third is consistent with regional Indian Ocean dominated records as indicated by our PCA analysis. The low-resolution paleoclimate records are all real records, recording real changes in the hydrological system, and they all fit one or more of the three drying signals. As we argue in our manuscript in the final paragraph of section 4.2, "they cannot all be recording regional variability in the summer monsoon". Therefore, it is impossible to interpret every single record as originally interpreted in the original manuscripts, and impossible to interpret every single record as a proxy for Indian Summer Monsoon rainfall amount. Further, the age uncertainty of many records may make it difficult to distinguish between these different drying events.

The purpose of our PCA analysis is to use only the highest resolution records to tease out the different climate signals contributing to regional hydroclimate variability. The advantage of using high resolution, precisely dated, paleoclimate data, is to provide higher resolution information on past climate, i.e., to separate apart multiple climate signals that may be closely spaced in time and not distinguishable in lower resolution or poorly dated records. Our results do not refute any of these records as real indicators of hydrological change. Simply, our results provide a framework in which lower resolution records can be interpreted in light of the multiple climate anomalies that occur within a short space of time over the mid- to late- Holocene transition.

In comment #4, Professor Weiss claims that we change the chronology and spatial pattern of abandonment of the Harappan civilization. We have not done so, and the chronology and spatial pattern of abandonment follow the literature. The robustness of the radiocarbon data is certainly up for debate, and we recognize that ongoing and future work will surely refine the exact spatial and temporal pattern of the rise, decline and urban abandonment of the Harappan civilization. However, the chronology as presented in the literature cannot be robust enough to allow for a 4.2kyr event summer drought, or a 4.2kyr event combined summer and winter drought as proposed by others, yet simultaneously be insufficiently robust to allow for a 4.2kyr event winter dominated drought. Our data rejects the idea that Harappan decline was forced predominantly by variability in the summer monsoon.

Overall, we find that this review argues that we must be wrong because other records are interpreted differently. The review provides no compelling argument against the methodology, the results, or the idea of three separate dry events in the region, each one derived from climatic changes in different moisture source regions and on different timescales. The arguments presented here against the Double Drought hypothesis rely on specific interpretations of hydrological records. As we explain below in detail, it is not possible to interpret every hydrological record in the circum-Indian Ocean basin as being controlled by the Indian Summer Monsoon. Deconvolving different modes of climatic variability that occur synchronously or within a short space of time is an important step in understanding mechanisms that drive our climate. The Double Drought hypothesis does this. Our interpretation of the spatial pattern of these three separate climate anomalies assigns reasonable climate mechanisms to each climate anomaly. We assign winter rainfall variability to climate dynamics in the source region of winter rainfall, and summer rainfall variability to climate dynamics in the source region of summer rainfall. The Double Drought hypothesis builds on the work of previous paleoclimate studies. It is consistent with regional climate dynamics, regional hydroclimate data, archaeological data, and archaeobotanical data.

FIRST COMMENT: “The IUGS-recognized global boundary stratotype for the 4.2 - 3.9 ka BP event, marking the middle to late Holocene transition to the Meghalayan stage, is the KM-A speleothem δ18O record from Mawmluh Cave, Meghalaya, NW India, that is an Indian Summer Monsoon (ISM) drought record (Berkelhammer et al 2012; Walker et al 2019).” The assumption that Mawmluh Cave KM-A record is a 100% Indian Summer Monsoon record has recently been challenged. A detailed sub-seasonal trace element (Ronay et al., 2019) record from Mawmluh Cave indicated that winter rainfall variability has a significant, if not dominant, influence on proxy variability. The authors explanation is that summer rainfall is so substantial at the cave site that variability in summer rainfall does not alter annual proxy values by a significant amount. Instead, as winter rainfall is much more variable, this variability contributes much more significantly to annual proxy variability. These results were corroborated by the d18O record. We find the work of Ronay et al reasonable and intuitive. We have added the Ronay reference to our manuscript.

Indeed, if the KM-A record is interpreted as responding to both summer and winter rainfall then our results and interpretation match the KM-A record very well. KM-A contains two step changes in d18O which both simultaneously define the 4.2 kyr event of the GSSP. Both step changes in the KM-A record are supported (within a 30-50 year age model adjustment/error) by the two hydrological changes implicated in our study (a winter change at 4.26 and a summer change at 3.97).

“The recent analysis of the Indus delta foraminifera record at core 63KA has identified, as well, the Indian Winter Monsoon drought synchronous with the 4.2 ka BP ISM drought (Giesche et al 2019). The global boundary sub-stratotype is the Mt. Logan Yukon glacial core’s δ18O moisture event (Fisher et al 2008). Scroxton et al present a principal components analysis of seven recent δ18O speleothem records from the Indian Ocean region and the Giesche et al 2019 delta foraminifera analyses (line 110) “to investigate the impacts of the 4.2 kyr event on tropical Indian Ocean basin monsoonal rainfall” and the late third millennium BC Indus urban collapses. Similar to earlier analyses using lake sediment records (e.g., Leipe et al 2014), Scroxton et al note the succession of two gradual centuries long dry periods separated by the 4.2 ka BP aridification event, but from their analysis present four new conclusions."

Our manuscript does not present two gradual centuries long dry periods separated by a 4.2 kyr event. Our manuscript presents two consecutive centuries long dry periods, one of which is the 4.2 kyr event. This misrepresentation probably comes from the Leipe record, which does show two droughts separated by a brief (2 data point) half return to wetter conditions.

"Scroxton et al conclude that the Mawmluh Cave KM-A speleothem is not a useful stratotype because (a) line 370 "The KM-A record replicates neither the other speleothem from Mawmluh Cave (Kathayat et al., 2018). "As previously noted, Mawmluh KM-A is similar within standard deviations to the other Mawmluh speleothems ML.1, 2 in both onset and terminus (Kathayat et al 2018)."

In our opinion, the term "similar within standard deviations" is ambiguous. Is this referring to temporal replication, isotopic values, normalized isotopic values or isotopic change? In figure 2 of our manuscript, we present a series of local and regional hydroclimate records including the Mawmluh Cave KM-A record that support our statements. The Mawmluh cave record is defined principally by positive isotope excursions, one at 4.30 kyr BP, and one at 4.05 kyr BP. The Mawmluh Cave ML.1 record contains neither excursion. Most likely, it contains a step-change around 4.0 kyr BP on a gradual drying trend – the two other regional climate anomalies described in the paper. ML.2 largely replicates ML.1, especially in the gradual trend, though the step change is less obvious. Is there evidence of a 300 year long, abrupt, transient 4.2 kyr BP event that represents a significant climatic departure from normal? In our opinion this event is not obvious. There is minor variability in both records around 4.25 kyr BP – both stalagmites show abrupt isotopic excursions. However, the excursions are of opposite sign between the two stalagmites, and the duration of the excursion is only 20-30 years. We stand by our conclusion that KM-A is not replicated in its own locality.

"nor any regional records (this study)." "It is similar, as well, to the records at the Indus delta (Giesche et al 2019), and to the sampling resolutions of the two recent ISM Madagascar speleothems (Wang et al 2019; Scroxton et al 2020 in review). Not listed here is the Sahiyah Cave, NW India speleothem that certainly does not present an abrupt 4.2 ka BP event, but was "manifest as an interval of declining ISM strength, marked by relatively higher amplitude of del 18O variability and slow speleothem growth"(Kathayat et al 2017). Mawmluh KM-A is also congruent and synchronous with the high resolution speleothem westerlies proxy for the 4.2 ka BP dust/drought event at Gol-e Zard NW Iran (Carolin et al 2019) as shown in Figure 2 top, that is synchronous with the settlement collapse in northern Mesopotamia (Weiss et al 2012) and many regional settlement abandonments across the Mediterranean."

The 4.2 kyr event cannot be all climatic variability between 4.5 and 3.5 kyr BP. In the Mediterranean and Middle East, where the evidence is strongest and most numerous, the 4.2 kyr event is 1) abrupt, 2) transient (an excursion rather than a step-change), 3) occurs between 4.25 and 3.95 kyr BP. We disagree that widely observed step-changes in climate at 4.0 kyr BP are manifestations of the 4.2 kyr event. We disagree that gradual secular trends over 1000 years are manifestations of the 4.2 kyr event. We agree that there are similarities between the KM-A record and other regional records. Indeed, as outline above, we suspect that the KM-A stalagmite may record both winter and summer rainfall variability and therefore both of the drying events. We do not agree that the Sahiya cave replicates an abrupt, transient 4.2kyr event because it shows gradual isotopic variability between 4.5 and 3.5 kyr BP. The Gol-E-Zard record in Iran is not a record of summer monsoonal rainfall, rather it likely records winter rainfall from westerly derived moisture in the d18O record, and regional dustiness in the Mg/Ca record. The two Madagascar records are not records of Indian Summer Monsoon rainfall, numerous paleoclimate studies indicate that southern hemisphere monsoon systems can act in phase, out of phase or without phase to the northern hemisphere under different climatic conditions. Settlement collapse in the Middle East is likely not dependent on summer monsoon rainfall and is certainly not dependent on Indian

Summer Monsoon rainfall.

The purpose of this study and manuscript, and the increasing number of big-data climate papers being produced, is to analyze the results of hydroclimate proxies independent of their original interpretations of seasonality, but rather in the context of regional variability. The similarity of one record to another is not indicative of both being caused by variability in the Indian Summer Monsoon.

We understand how “nor any regional records” might be interpreted as implying a lack of replication with any record, anywhere, rather than the outcome of this study as implied by the phrase “this study”. We should have been more careful with our wording. We have decided to update this section so as to be more precise. However, our interpretation and conclusions have not changed.

“and (b) line 374 “is low resolution”

We agree that KM-A does not have low sampling resolution when compared to other stalagmite records from the region. We have removed this statement.

“low dating frequency,” The KM-A record has three U-Th datapoints between 5100 and 3600 yr BP. An average spacing of 750 years. The ML.1 record has 18 U-Th ages between 3.7 and 4.5 kyr BP (44 year average), and the ML.2 record has 3 (266 years). Between 3.0 and 5.0 kyr BP: the Oman record has 6 U-Th ages (333 years). The Sahiya record 15 (133 years). Our Madagascar record has 7 (285 years). The Rodrigues record has 16 (125 years). The Australian record has 15 (125 years). The Borneo record has 6 (333 years). The sediment core record of Giesche has 7 (285 years). We stand by our statement that the KM-A record has a low dating frequency.

“not replicable within its own locality,” The response to replication at Mawmluh cave is answered above.

“ambiguously defining a climate event” We stand by our statement that the definition of the 4.2 kyr event is ambiguous. We attach a figure showing the KM-A record in detail.

The 4.2 kyr event is defined as being midway between two events: “The first registration of the event in the stable isotope record occurs at ∼4300 yr BP followed by a second marked increase in stable isotope values at ∼4100 yr BP. The abrupt increase in stable isotope values is the primary boundary marker for the GSSP, and hence a date of 4200 yr BP, which effectively marks the mid-point between these two modelled ages” (Walker et al., 2018). The GSSP is therefore defined by a point between two stable isotopes (red dots) during a period of twenty consecutive stable isotope points with less than 1.01 per mill variability. Our analysis demonstrates that the two anomalies are likely caused by separate climate events.

“that is not significant across its climate domain”. We understand that “not significant across its climate domain” may be not be as precisely worded as it should be. However, we have demonstrated above that the KM-A record is not representative of summer monsoon rainfall, its interpreted climate domain.

Overall, we are happy to update our manuscript, to be more precise with our phrasing, and to spell out more clearly our reasoning behind our statements. The updated wording of section 4.5 is:

"The Mawmluh Cave speleothem records which define the 4.2 kyr event (Berkelhammer et al., 2012; Walker et al., 2018) are too short to be included in our PCA analysis. However, the highest resolution replicated record from the cave, ML.1, replicated by ML.2, shows gradual drying over its entire growth period, wetter than normal conditions at 4.1–4.0 kyr BP, and a step-change increase in ïĄď18O at 4.0 kyr BP (Kathayat et al., 2018). These results are consistent with both the secular millennial scale drying trend, and the 4.0 kyr BP summer monsoon drought identified in PC1. The Global Boundary Stratotype Section and Point (GSSP) golden spike is located in stalagmite KM-A (Berkelhammer et al., 2012). Stalagmite KM-A does not replicate ML.1 or ML.2 from the same cave. Our results indicate that KM-A is not representative of hydroclimate variability in the Indian Summer Monsoon domain. Instead, KM-A contains two increases in ïĄď18O (drying events), one at 4.31 kyr BP and one at 4.05 kyr BP. Hydroclimate proxy variability at this site may be significantly influenced by dry season variability {Ronay et al., 2019, #72351}, suggesting KM-A may not be an exclusive record of summer monsoon rainfall. Within reasonable age uncertainty (±30 years at the nearest U-Th date, so likely slightly higher away from the age) the timing of the KM-A dry anomalies beginning 4.31 kyr BP and one at 4.05 kyr BP are consistent with both 4.26 and a 3.97 kyr BP drying events. We hypothesize both a winter and summer influence stalagmite ïĄď18O in KM-A.

There are also notable issues with the stalagmite itself. While the ages of KM-A are very precise, there are only three ages between 5084 and 3654 yr BP and stalagmite growth is very slow. The shape of the stalagmite after 5 kyr BP is not convincing of unaltered equilibrium deposition. Even if the ïĄď18O record of KM-A does record both drying events, the golden spike location is defined as 4.20 kyr BP, part of a run of 40 consecutive ïĄď18O samples between the two drying events, with relatively minor variability (<1‰. The existence of the Northgrippian-Meghalayan GSSP golden spike in a low dating frequency record, not replicable within its own locality, not representative of climate variability across its climatic domain, and ambiguously defined as the mid-point between two different climate events, is problematic at a minimum (Helama and Oinonen, 2019). We recommend that an alternative GSSP golden spike for the mid- to late-Holocene transition be identified."

SECOND COMMENT: "Secondly, Scroxton et al argue that the 4.2 ka BP event was of (line 400) "limited impact on tropical monsoonal rainfall around the circum-Indian Ocean basin" Where we have missed records, we are happy to include them and have incorporated some of the suggested studies into our paper. But as of yet, none of the suggested records dispute the three drying events of our hypothesis: 1) the secular trend, 2) a 4.25 kyr transient event and 3) a 3.97 kyr BP step-change.

However, Scroxton et al do not include the Mawmluh KM-A speleothem record in their principal components analysis (line 364, "too short to be included") Principal component analysis requires records covering the entire duration of the interval of interest. In

not covering the 5-3kyr interval, the KM-A, ML.1 and ML.2 records are too short to be included. We discuss lower resolution records extensively in section 4.2, and dedicate section 4.5 to an in-depth discussion of how the three Mawmluh cave records fit within our new framework. In summary the three Mawmluh cave records show, between them, all three drying events.

"nor the ISM Tibetan plateau speleothem record (Cai et al 2012)," The Cai et al., 2012 record stops growing at 4.15 kyr BP (+- 180 years). It therefore cannot be used in the PCA analysis. A hiatus at this point is indicative of some kind of drought, and is within error of both drying events at 4.26 and 3.97 kyr BP. We have included this record in section 4.2.

"and the possibly anti-phase Southeast African records (Humphries et al 2020)." We discuss both the 2019 and 2020 Humphries records from southern Africa in detail in our companion manuscript. They are not included in this compilation as they are not summer monsoon records deriving moisture from the ITCZ. Instead, they derive the majority of their moisture from the SE Trades, with some influence from tropical temperate troughs, which have their own complex tropical/mid-latitude climatic controls.

"Most problematically, however, Scroxton et al ignore the Horn of Africa, the Ethiopian highlands, as there are no speleothem paleoclimate records. Nevertheless, the Ethiopian highlands are a major component, multiply recorded, of ISM sourced Indian Ocean basin hydroclimate." We have not included the Horn of Africa in our detailed analysis because 1) there are no records of sufficient resolution to include in our PCA, 2) we have reserved discussion of smaller sub-regions to a) the Indian Summer Monsoon region with direct relevance to the Harappan, and b) the South-East African Monsoon region as discussed in the companion manuscript. This is in part because 3) the Horn of Africa double monsoon season is not the same as the Indian Summer Monsoon, having both different wet seasons and a different sensitivity to zonal variability (the Indian Ocean Dipole). 4) The progression of climate change in the Horn of Africa during the Holocene is dominated by the African Humid Period, drying in the mid- to late- Holocene may therefore have little dependence of the 4.2 kyr event. The Horn of Africa is therefore currently out of scope, as is the Old Kingdom of Egypt. This would be an entirely different paper.

Nevertheless, we are happy to discuss individual records here as it is indeed probable that some climatic processes may be similar. We also include a second figure here that demonstrates some of the East African records. We do not believe the discussion below is within the scope of this paper.

"The 1200 mm of highland Ethiopian ISM precipitation collect at Lake Tana, and become the Blue Nile and Atbara Rivers, which together provide 90% per cent of Nile peak flow as measured by air mass back trajectories and 97% of Nile annual sediment load (Williams 2019:28; Woodward et al 2014; Costa 2014). The Nile River extends 4759 kms from Lake Tana to the delta (William 2019: 117), or 2000 kms longer than the Indus, and was the primary physical determinant of ancient Egyptian irrigation agriculture. The sediment core from Lake Tana documents an important, albeit 200 year resolution, low stand at 4.2 ka BP (Marshall et al 2011)" The Marshall et al., 2011 Lake Tana Ti record does indeed show an excursion around 4.2 kyr BP with an age uncertainty of 150 years. However, the lake Tana dD record (Costa et al., 2014) shows wet conditions between 4.6 and 4.0 kyr BP and a single dry data point at 3.7 kyr BP, before returning to dry conditions.

"synchronous with other East African records that include the Lake Mega-Chad large scale dust mobilization (Kröpelin et al 2008; Francus et al 2013)." The Lake Yoa record of Kropelin 2008 shows a step change between 4.2 and 3.9 kyr BP, this is not an abrupt 300 yearlong 4.2kyr event anomaly. The Lake Yoa record of Francus 2013 has a separately defined stratigraphic unit between 4.3 and 3.9 kyr BP but is described as a gradual transition between the units above and below. We interpret neither as showing an unambiguous abrupt anomalous event.

"This ISM/Nile source reduction, known at the Nile delta within numerous and various

sediment core proxies, is recorded at the Nile deep sea fan marine cores. For example, the recent analyses of MD04-2276 include the 4.2 ka BP SST (Jalali et al 2017)" The Jalali et al MD04-2726 record shows a transition in SSTs around 3.8 kyr BP. A longterm of increasing SSTS begins at since 6kyr BP, peaking around 3.9kyr BP before a decrease in SSTs gradually over 400 years to a minimum around 3.5 kyr BP. The paper also describes a more humid interval from 4200-3000 yr BP. We interpret this result as showing two of the three climate anomalies: the long-term secular trend and a step-change sometime around or just after 4.0 kyr BP, but not an abrupt transient 4.2 kyr event. There is a two data point excursion between 4.1 and 3.9 kyr BP. It is of the same magnitude as another excursion at 4.4 and very similar in magnitude (just starting from a different baseline) at 4.55 and 4.9 kyr BP. If this anomaly is the 4.2kyr BP event, it is not of unusual magnitude, and merely represents normal centennial scale variability on top of millennial scale long term change.

"and Mn/Al flux (Mologni et al 2020) events. Although there is interpolation across three radiocarbon dates, Figure 2 middle displays this SST event synchronous and congruent with the Mawmluh KM-A record." The Mologni core describes in detail conditions between 10,200 and 7,200 kyr BP. Above which the core is heavily bioturbated. There are two ages in the bioturbated region of 5.8 and 3.9 kyr BP defining this section. There is a peak in Mn/Al flux and minima in the log(Ti/Ca) in the section. The data is not publicly available to provide quantification here, but the anomalies appear to be roughly 1500 years long.

Other records in the East African region include the Lake Turkana TEX86 and BIT index records of Lake Turkana (Berke et al., 2012), and the Lake Victoria Diatom PCA2record of Stager et 2002, and the Pilkington Bay (also Lake Victoria) Diatom PCA2 record of Stager et al., 2003. None of these records show an abrupt, climatic event at 4.2 kyr BP. Records south of the equator in East Africa are discussed at length in the companion paper to this manuscript.

The 4.2 kyr event is visible in the Gulf of Oman (Cullen et al., 2000) as a winter dust record but could reflect aridity in any season. In the Arabian sea low resolution records without the 4.2 kyr event include the Arabian Sea G. bulloides record of Gupta et al., 2003, the Gulf of Aden Leaf Was dD record of Tierney et al., 2013 and the Arabian Sea sediment lightness record of Schulz et al., 1998. This represents an exhaustive search of all records with at least 7 data points between 5000 and 3000 years with publicly available data in the NOAA repository.

The significance of the 4.2 ka BP Nile event resides in its synchronism with the Old Kingdom collapse and the beginning of the First Intermediate Period (Barta 2019), one feature of which was considerable settlement abandonment at the Nile delta and resettlement in Middle Egypt. While the Old Kingdom collapse was also synchronous with rain-fed settlement abandonment across Mesopotamia and Syria at 2200 BC (Ramsey et al 2010; Weiss et al 2012), Upper Egypt and its Kerma culture, close to the source of Nile flow, did not experience similar Nile flow reductions nor regional settlement abandonments (Woodward et al 2014). The influence on the Old Kingdom of Egypt is not within the scope of this paper. We do not mention it in our manuscript.

THIRD COMMENT: Thirdly, Scroxton et al state, (line 47) that "the areal extent of the 4.2 kyr BP event beyond the data-rich heartland of Mediterranean Europe (Bini et al 2019) and Mesopotamia (sic) (Kaniewski et al 2018) is unclear." But the event records are abundantly available. The 4.2 ka BP event extended to Australia (Deniston et al 2013) and to southern and Northern China, an ISM and East Asian Summer Monsoon (EASM) event, e.g., Hulun Lake (Zhang et al 2020), Dongshiya Cave (Zhang et al 2018), Lake Balikun (An et al 2011). At Lake Wuya in North China (Tan et al 2020) the event is recently described erroneously as gradual when δ18O increased and decreased abruptly at 4200 and 3996 ka BP. The EASM is, of course, ISM sourced (Liu et al 2015; Yang et al 2014), and a North Atlantic wavetrain for 50% of modern ISM drought events has now been identified (Buhar et al 2020).

Along the western Pacific the event is recorded in Japan (Park et al 2019) and in the Kuroshio Current's "Pulleniatina minimum event" (Zhen et al 2016; Zhang et al 2019;

Shuhuan et al 2021) where its northeastern trajectory likely generated the Mount Logan Yukon 4.2 ka BP event (Fisher et al 2008). The Mount Logan 4.2 ka BP sub-stratotype, with 2-3 year resolution, is both synchronous and congruent with the Mawmluh KM-A event (Figure 1, Figure 2 bottom).

Across mid-latitude North America the event is also well documented, from western Idaho to western Massachusetts, "with median moisture levels reaching a minimum from 4.2 to 3.9 ka" (Shuman and Marsicek 2016: 42; Shuman et al 2019) alongside the North American monsoon phase change recorded at Leviathan Cave (Lachniet et al 2020). In the South American Monsoon region, along the western coast of South America, northern sediment cores suggest abrupt wet eastern Cordillera events synchronous with abrupt dry western Cordillera and Altiplano events. Lake Titicaca, for example, experienced an abrupt diatom shift at ca. 4300 BP followed by a drought event from ca. 4200 - 3900 BP with a lake level drop of ca. 70 meters (Weide et al 2017). At the southernmost Andes, "Marcel Arevalo" caves MA 1-3 record a uniformly wet period from ca. 4.5 to 3.5 ka BP interrupted by an abrupt ca. 23% drop in precipitation centered at ca. 4.2 ka BP (Schimpf et al 2011), possibly associated with several volcanic eruptions.

Synchronously, the continental monsoon along Brazil's east coast and the South Atlantic Convergence Zone that crosses Brazil, experienced abrupt and radical alteration. The Lapa Grande speleothem's sharp, decreased spike in δ18O extended from ca. 4.2 – 3.9 ka BP (Stríkis et al 2011). At Chapada do Apodi, Northeastern Brazil, high resolution speleothems, clastic sediments and bat guano analyses display abrupt high δ13C and δ18O and low 87Sr/86Sr values indicating "a massive episode of soil erosion. .the beginning of the Meghalayan chronozone, characterized as the aridification of this region, decline in soil production, drying out of underground drainages" (Utida et al 2020).

Apart from the dense distribution of Mediterranean and Western Asia records, including the Gol-e Zard speleothem congruent with Mawmluh KM-A, Figure 2 top, the 4.2 ka BP event synchronous records extend to Alpine Europe (e.g., Spannagel Cave, Fohlmeister et al 2012) and more than fifty subpolar North Atlantic records (Weiss 2019). The latter are now complemented by the high resolution north Iceland marine core MD99-2275 SPG event dated 4290±40 ka BP (Jalali et al 2019; Figure 1) and the Irminger Current event (McCave and Andrews 2019), which both suggest a 4.2 ka BP AMOC slowdown. This was due, possibly, to the freshwater dosing associated with glacial melt documented synchronously, for example at the Agassiz glacial core (Vinther et al 2009; Fisher et al 2012; Lecavalier et al 2017).

We recognize that the phrase "the areal extent of the 4.2 kyr BP beyond the data-rich heartland of Mediterranean Europe (Bini et al 2019) and Mesopotamia (Kaniewski et al 2018) is unclear" may be considered an overstatement. Through numerous discussions over the past few years of this project we believe it to be an accurate portrayal of the opinion of the paleoclimate community. As there is no systematic, detailed review of every single global record spanning the 4.2 kyr event it is not a citable fact. We are happy to change the phrasing of this sentence so as to stress that our understanding of the 4.2 kyr event and its climate mechanisms in the data-rich heartland is considerably better than elsewhere in the world. Given the density of records in the region, this cannot be disputed. We also delete the last sentence of the paragraph which similarly stated "The global extent of the 4.2 kyr event is uncertain."

The new sentences read: "The climatic impact and mechanisms of the 4.2 kyr BP event are better understood in the data-rich heartland of Mediterranean Europe (Bini et al., 2019) and Middle East (Kaniewski et al., 2018) than elsewhere in the world, although even in the Mediterranean spatial heterogeneity limits a complete mechanistic understanding (Bini et al., 2019). While individual records do report climatic anomalies at 4.2kyr BP, the global picture remains incomplete. This is due to poorer spatial coverage of records, the use of low-resolution records with limited ability to reliably detect a 300-year anomaly, and chronological uncertainties inherent in paleoclimate records. These uncertainties hinder the determination of spatial variability and climate

mechanisms of the 4.2kyr event."

We believe it beyond the scope of this Indian Ocean focused paper to address and review every global record, and beyond the scope of a response to reviewers to comment on all 26 citations provided by the reviewer as potential expressions of the 4.2 kyr event. Other research groups have already taken up the task of systematic regional investigations elsewhere in the world and are likely to publish in the coming years.

FOURTH COMMENT: Our arguments concerning the Harappan relate to the seasonality of the drought, and do not infer any different timing of abandonment to any other previous study. We agree that there is ambiguity in the timing of abandonment between settlements, and in the paper we echo the idea that the precise timing of abandonment is due to a "complex interaction of societal, biogeophysical and geomorphological feedbacks and responses, rather than by climate alone". The archaeological evidence cannot simultaneously be sufficiently robust to allow for civilization collapse from a 4.2-3.9 kyr BP summer monsoon drought as reported widely in the literature, while at the same be insufficiently robust to not allow for civilization collapse from a 4.2-3.9 kyr winter monsoon drought proposed by our study. Professor Weiss argues simultaneously: "the archaeological evidence for the synchronous collapse of Egyptian, Mediterranean, West Asian, and Indus settlement systems at 4.2 ka BP appears increasingly robust." Yet also: "When, and at what rate, the Harappan urban abandonments occurred during this 700 year gross ceramic definition period is yet uncertain"

We believe this misunderstanding was caused by the phrase "The absence of a significant, widespread 4.2 kyr event in tropical Indian Ocean hydroclimate has consequences for the timing and causes of the deurbanization of the Harappan civilization in the Indus valley." As paleoclimatologists "tropical Indian Ocean hydroclimate" already infers summer, but we recognize that this might not be the case for other readers. We add the word summer to this sentence to clarify our point.

Our radiocarbon dates are taken from Sengupta et al., 2020 who state "Note the nearâĂŘsynchronous Harappan decline at all the sites just at the onset of or immediately after the Meghalayan stage." All we did to the data was to sort the sites by latitude. This reveals a clear geographical pattern which has been recognized previously in the literature (e.g. Giesche et al., 2019).

Our interpretation of the archaeological evidence for the timing of abandonment of Harappan sites is no different to that of previous studies. Professor Weiss misrepresents our interpretations by suggesting that we hypothesize widespread abandonment at 4.2kyr BP. We hypothesize drought related climatic stress between 4.2 and 3.9 kyr BP contributed to major city abandonment in the Indus Valley by 3.85 kyr BP (1900BC) via complex biogeophysical and societal feedbacks, with ongoing aridity leading to subsequent transition to a more rural society by 3.0 kyr BP. This hypothesis is already widespread, and we suggest no changes to the timing of any societal change, merely the seasonality of rainfall that contributed to it.

Interactive comment on Clim. Past Discuss., https://doi.org/10.5194/cp-2020-138, 2020.

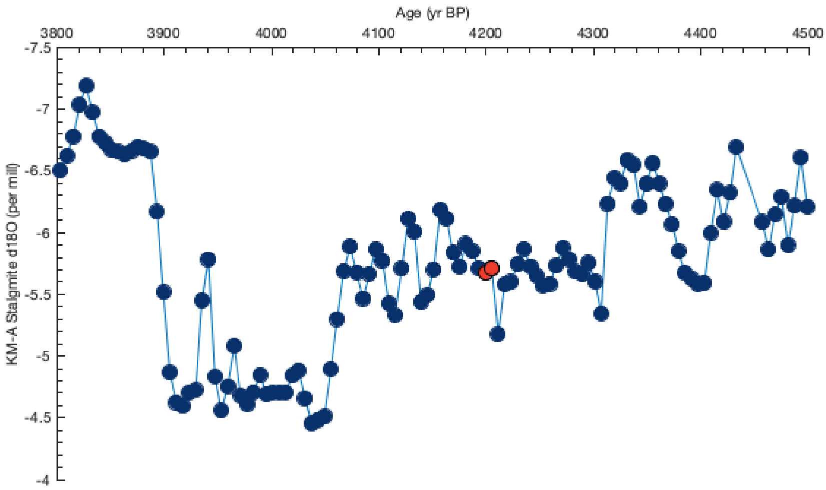

Fig. 1. Isotopic record of stalagmite KM-A from Mawmluh cave. The 4.2 kyr event is defined by the mid-point between the two excursions at 4.20 kyr BP (ie. between the two red circles)

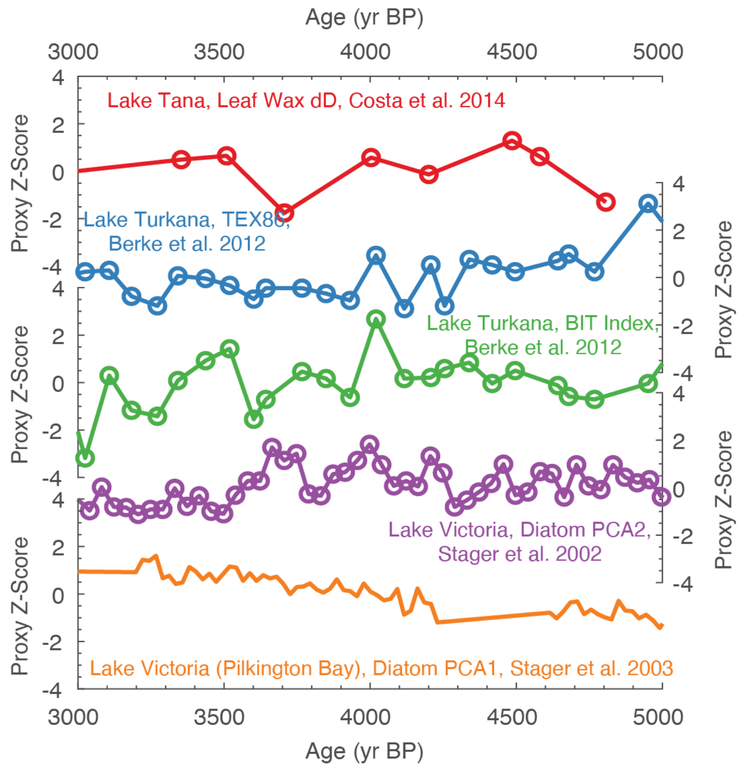

Fig. 2. Hydroclimate records from East Africa northern hemisphere with at least seven data-point between 5000 and 3000 yr BP