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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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We would like to thank Dr. Giesche, Professor Weiss and an anonymous reviewer for their inputs to this paper. The excellent questions have forced us to refine our paper and provide better evidence for our claims – be it on variability of the winter monsoon or a more thorough examination of the spatial relationships of the principal component analysis. We believe that the double drought hypothesis is far stronger with these inputs, our arguments have been made more robust with additional evidence, and that the manuscript is more precise and fairer in its language. In this final report we provide

a summary of the main discussion points raised by the reviewers, either individually or together. Individual detailed responses, and responses to minor comments have already been submitted as part of the discussion phase.

One of the major points raised by reviewer 1 was a desire to see more evidence for changes in the source region and transport route of the westerly winter monsoon. Our paper had originally focused on the summer monsoon, as this is where we provide new evidence. However, in order to establish the double drought hypothesis, we agree that both 'monsoons' need discussion. We are therefore happy to discuss rainfall changes along the path of westerly disturbances. Unfortunately, there are not sufficient high resolution, high dating precision records to conduct a similar PCA analysis as we have done for the Indian Ocean region. Therefore, our analysis was limited to a literature review. We first focused on records from the Middle East which give data about the winter season (e.g., a pollen record from Tell Tweini (Kaniewski et al., 2008), coral from the Gulf of Oman (Watanabe et al., 2019) and the speleothem d18O from Gol-E-Zard (Carolin et al., 2019)). These confirm the intuitive reasoning and previous studies (the Kaniewski et al., 2018 review in Climate of the Past) that the 4.2-3.9 kyr BP drought was likely a winter drought in the Middle East. We then tracked the drought through non-seasonally resolved records across the Middle East towards the Indus Valley. While not every record showed a drought (e.g., Mirabad Lake, (Stevens et al., 2006), Maharlou Lake (Djamali et al., 2009)) (it would be quite unusual if all records did agree), a substantial number of lake records did show a drying from 4.2-3.9 kyr BP (e.g. Neor lake (Sharifi et al., 2015), Lake Hamoun, (Hamzeh et al., 2016). This also helps put into context the archaeobotanical data determined from Harappan crops - the carbon isotopes of rice grains, a summer crop, show drought only after 4.0kyr BP in both Gujarat and Indus valley locations (Kaushal et al., 2019). Overall, we have made a substantial addition to Section 4.3 (Regional Climate Drivers) to explain this, and feel the double drought hypothesis is strengthened and the paper substantially improved by these changes.

The suggestion of a winter drought (and therefore two droughts) is not new. As Dr. Giesche pointed out, the idea was raised previously in their 2019 Climate of the Past paper. It was our error not to include it. Our analysis oversimplified the relationship between the individual proxies of their paper (G. ruber and N. dutertrei) as summer and winter rainfall proxies, whereas it was the relationship between the two (and G. sacculifer) that was interpreted as the winter rainfall proxy. We are happy to correct our error and give credit where it is due, making changes in several places in the manuscript. We feel there are plenty of similarities between the two records, and they are not necessarily at odds with each other: e.g. a secular trend of decreasing summer rainfall, an increase in winter rainfall at 4.6-4.5 kyr BP, a winter rainfall drought around the 4.2 kyr event, an increase in rainfall between 3.5 and 3.0 kyr BP are all consistent. There remain some differences between the two records. Giesche et al., hypothesise concurrent winter and summer droughts. We hypothesize consecutive winter then summer droughts. We believe this difference might be due to the Giesche et al winter and summer rainfall interpretations both relying on the G. ruber proxy (although the difference between N dutertrei and G. sacculifer support their G.ruber based interpretations). They therefore might not be truly independent records.

Reviewer 1 also asked us to provide further discussion on the spatial component of the MC-PCA, particularly PC2. We agree that the spatial analysis was not discussed as thoroughly as it might. The spatial pattern of loadings, particularly PCA-3 PC2, help provide key evidence in our discussion as to why the 4.2-3.9 kyr drought is unlikely to be caused by changes in summer rainfall. Namely that both the Oman and Sahiya cave records plot oppositely to the Indus fan records. We are happy to follow the advice of reviewer two and include maps (a new figure 5) and an enhanced discussion of the spatial characteristics of PC1 and PC2 (second paragraph of section 4.3).

Reviewer one asked us to provide technical points on the Monte-Carlo procedure used and to share the data and code from this work:

Re: technical issues with code: We have used the Deininger et al. (Climate Dynam-

ics 2017) code, with only minor adjustments in consultation with Michael Deininger to facilitate changes to the input files (the time period was previously hard coded at 30 years) and output files for easier plotting. As raised by Reviewer 1, truncation of the records is part of the Deininger software. The production of 1000 independent age models from each record results in individual realisations being stretched and compressed between randomly sampled ages. Windows without data-points are dropped. Therefore, there is a compromise between window size and the ability to generate one data point in each and every time window for all 10,000 age models (10 records x 1000 realisations). Slightly longer windows did not result in substantially longer principal component time-series.

Re: data availability. We thoroughly agree with the idea that data should be shared. We are submitting both the new Madagascar record (in companion paper cp-2020-137) to SISAL and the NOAA paleoclimatology database, and will submit the principal component time series and loadings to the NOAA paleoclimatology database also. The majority of records used in this study are publicly available. However, we do not believe it is our place to share the data of others so will refrain from publishing their data for all the records used in this study.

Re: code availability. We feel similarly about the Deininger MC-PCA code used in this study. It is not our place to share other people's code without their express permission. Reviewer one asks for both code to be available (to facilitate replication) but also changes to the code (to facilitate non-independent age models in the Indus fan sediment cores). These are unfortunately incompatible. We have decided to stick as closely as possible to the Deininger code, for the sake of replication, rather than adjust the code for modest gains in performance and make our code unreplicable as we cannot republish what is 95% someone else's work.

Professor Weiss detailed four significant comments in response to our manuscript. The comments, and our detailed response total over 7600 words. Here we provide a summary of the main issues:

The first comment took issue with our interpretation of KM-A stalagmite from Mawmluh Cave, which Professor Weiss argues is an Indian Summer Monsoon record only. A recent drip monitoring study by Ronay et al. (Scientific Reports 2019) casts doubt on this idea, instead suggesting that stalagmite proxies (both stable isotopes and trace metals) may have a strong or even dominant winter component. There is therefore doubt as to whether KM-A is a 100% Indian Summer Monsoon record. The KM-A record is still useful and under the interpretation of the double drought hypothesis, does show convincing evidence for all three drying events. We would therefore argue that KM-A likely records both summer and winter rainfall variability.

Professor Weiss then argues that the KM-A record replicates with stalagmites ML.1 and ML.2 "within standard deviations". We disagree. While ML.1 nor ML.2 may contain minor changes to variability or mean state around the 4.2 kyr event, neither contain substantial step-change deviations of the kind seen in KM-A at either 4.3 or 4.1 kyr BP. Certainly neither ML.1 nor ML.2 contains the dramatic reversal at 3.9 kyr BP seen in KM-A. ML.1 and KM-A are plotted together in figure 2 of the manuscript. This is followed by an objection to one of our final statements that KM-A is unsuitable as a golden spike for the mid- to late- Holocene transition. We retract our comment that the record is of low resolution, it is not. But we stand by our comments that the stalagmite has low dating frequency, is not replicable within its own cave, ambiguously defines a climate event, and is not significant across its climate domain (i.e. the Indian Summer Monsoon). In our direct response we provide detailed evidence for our stance on each of these clauses.

The second comment takes issue with the records used in the Principal Component Analysis. Our entire analysis was designed to investigate only the highest resolution records available so that the common centennial scale variability could be determined taking into account age model uncertainty. Including low-resolution and poorly dated records would not provide statistically significant insight into the 4.2 kyr event given the age uncertainty. The records chosen for our PCA were chosen on the basis of

resolution and length of record and were selected objectively based on these criteria. Professor Weiss criticized a lack of discussion on global low-resolution records generally, and East African records in particular. In our manuscript we chose to discuss low resolution records in two sub-regions only: the south-west Indian Ocean in the companion manuscript cp-2020-137, and the Indian subcontinent in this manuscript. The East African records were not included as we felt that a discussion of the impacts of the 4.2kyr event on the East African double wet season (not part of the Indian Summer Monsoon domain) was beyond the scope of this Indian Summer Monsoon and Indian Winter Monsoon paper. Nevertheless, we conducted a literature review of low resolution East African records and could find no compelling evidence of a 4.2 kyr event in summer rainfall that could cast significant doubt on our basin wide analysis (full details of that review are provided in the response). On a similar note, the impact of the 4.2kyr event on the collapse of the Old Kingdom in Egypt was out of scope.

The third comment questioned 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". We agree that this phrase is likely an overstatement, and as the discussion of every single global record covering the 4.2kyr BP event is beyond the scope of this paper, we are happy to rephrase this statement. Instead, we highlight that while numerous climatic anomalies at the mid- to late- Holocene transition have been attributed to the 4.2kyr event, the global picture remains insufficient to attribute climate mechanisms.

The fourth comment discusses the radiocarbon data from the Indus Valley. Professor Weiss argues simultaneously that: "When, and at what rate, the Harappan urban abandonments occurred during this 700 year gross ceramic definition period is yet uncertain" While arguing later in his response that: "the archaeological evidence for the synchronous collapse of Egyptian, Mediterranean, West Asian, and Indus settlement systems at 4.2 ka BP appears increasingly robust." These two statements are contradictory. Moreover, we make no changes to the interpretation of the radiocarbon data.

The radiocarbon data compilation used in our study is taken from the Sengupta et al., 2020 compilation which argues for 4.2-3.9kyr collapse at Dholivara. The idea that abandonment was most severe in the Indus Valley, and did not occur in simultaneously in Gujurat is not a new idea either (e.g. Giesche et al., 2019). The archaeological evidence cannot simultaneously be sufficiently robust to allow for civilization collapse from a 4.2-3.9 kyr BP summer monsoon drought, while at the same be insufficiently robust to allow for civilization collapse from a 4.2-3.9 kyr winter monsoon drought. Our data does not call into question a 4.2-3.9 kyr decline in the Harappan civilization and abandonment of the more northerly Indus valley sites. Our data provides evidence that a winter drought occurred at this time, and not a summer drought. We agree that there is remaining uncertainty in the radiocarbon ages, both in their inherent age uncertainty, but also in the archaeological context in which they are found. However, if the uncertainties are sufficiently concerning that the chronology of civilization decline and collapse cannot be attributed to the 4.2 kyr event, then there are dozens of previously published studies across numerous disciplines that are unsubstantiated.

Our manuscript provides perhaps the first compelling climate mechanism to extend the influence of the 4.2 kyr event outside of the Middle East and Mediterranean (both climatically and culturally). We argue that the 4.2kyr event that contributed to the collapse of the Akkadian Empire in the Middle East is likely to influence the Harappan by the same mechanism: reduced moisture from the Mediterranean transported by upper-level winter storms in the westerly jet. This data fits well with the regional climate dynamics – the 4.2kyr event is a known winter drought in the Middle East, and the Middle East is the moisture source and transport region for the westerly disturbances that feed the Indian Winter Monsoon. A winter drought is in agreement with the archaeobotanical data and the temporal pattern of settlement abandonment. Our analysis expands, rather than limits, the reach of 4.2 kyr event.

Finally, In our final report, the editor asked us to comment on the recently published Lilaur Lake record from the Ganga Basin (Singh et al., 2021). Singh et al interpret

coarse silt and clay fractions as wet/dry indicators with increased coarser fractions representing wetter conditions. Under this interpretation they infer a dry event from 4.20 (possibly 4.25) to 4.05 kyr BP, wet or very wet conditions from 4.05 to 3.8 kyr BP and then a gradual drying towards 3.0kyr BP. Therefore, at first viewing, it appears to match the PC1 pattern of our analysis but with the opposite loading to the other records.

The Lilaur Lake record is good quality and has excellent sampling resolution. However, the dating of this section of core is not ideal. There is one radiocarbon date at 3.1 kyr BP +-35 and one OSL date of 3.7 kyr BP +-300 covering the entire Holocene. The 4.2 kyr event is interpolated between the 3.7 kyr BP age and a radiocarbon date at 17.4 kyr BP. Therefore, the precise location of a 200 year long dry interval is difficult to interpret.

Despite this uncertainty in precision, it is still possible to interpret the record in light of our results under the assumption the mean age model is accurate. As suggested by Singh et al., 2021, the Lilaur Lake record does look remarkably similar to the Tso Moriri Lake EM3 record of Dutt et al. (2018), which has more precise bracketing dates at 4.4kyr BP +-120yr and 3.7kyr BP +-50yr. The Tso Moriri element record shows an abrupt drying around 4.35 kyr BP but it is unidirectional in nature, lasting until at least 3.4 kyr BP. Tso Moriri Lake has significant winter rainfall under modern climatology. It is therefore reasonably intuitive to suggest that the Tso Moriri Lake likely experienced consecutive winter and then summer droughts. It is a little harder to interpret the Lilaur Lake record in this way, as the modern climatology at Lilaur Lake is dominated by the summer monsoon. However, if the Giosan et al. (2018) hypothesis (supported by Giesche et al., 2019 and our analysis) of increased westerly disturbance rainfall is correct, then Lilaur Lake is not too far east that winter rainfall may have penetrated as far as Lilaur Lake and influenced proxy variability. The sediment core itself is a wet/dry indicator with no seasonality indicators.

What about the wet event from 4.05 to 3.80kyr BP? A coring gap (not a hiatus) between 4.7 and 4.25 kyr BP makes it difficult to evaluate the 4.2 kyr event in the context of the prior climatic regime, particularly the 4.6-4.3 kyr BP wet period (Giosan et al., 2018, Giesche et al., 2019, this study). Therefore, it is impossible to know if the 4.6-4.3 kyr period was wetter than the 3.9-3.6 kyr BP period. The coarse silt percentage indicates that 4.05 to 3.80kyr BP was the wettest period on record. However, the clay percentage suggests that 4.05 to 3.80kyr BP was wet, but not unusually so – being comparable or perhaps slightly drier than 5.1 to 4.8 kyr BP. (Prior to 5.1 kyr BP the lake was likely a river so direct comparison is more difficult). The strength of the 4.05 to 3.80kyr BP wet signal at Lilaur Lake is unclear.

Our discussion of the Lilaur Lake record is emblematic of many hydrological records in the region, and could be repeated for any and all Indian subcontinent hydroclimate record. Most of the records are interpreted entirely as Indian Summer Monsoon records, as the modern climatology is dominated by summer rainfall. If Giosan, Giesche and ourselves are correct in interpreting a stronger winter monsoon at the end of the mid-Holocene then it is entirely possible that many paleoclimate records were recorders of winter rainfall variability or mixed winter and summer (i.e. annual) rainfall variability. The Double Drought hypothesis provides a new framework with which to view anomalies and trends in the Indian Ocean hydroclimate system of the mid to late Holocene, made up of three major components either in isolation or as a mixture. They are: 1) gradual drying over millennia, 2) abrupt drying at 4.25kyr BP with recovery at 3.9 kyr BP and 3) abrupt drying at 4.0 kyr BP. Some records will fit more naturally than others, and there will be differences between different proxies from the same location depending on proxy sensitivity. In the case of Tso Moriri the Dutt et al., 2018 elemental record supports abrupt drying at 4.3 kyr BP and no recovery until 3.4 kyr BP, which fits nicely with both #2 and #3 – recording winter and then summer drought. The Leipe et al. (2014) pollen record shows both 4.4 kyr BP and 4.0 kyr BP drying, interpretable as #2 and #3, but with a slight recovery in between. In the case of Lilaur Lake, the clay percentage record shows a gradual drying trend, decrease at 4.2 kyr BP and only a slight recovery from 3.9 kyr BP onwards: interpretable as showing #1 and #2 clearly, but #3 less obviously but plausible. The Lilaur Lake record coarse silt percentage shows a

gradual drying trend, decrease at 4.2 kyr BP but no drying at 3.9 kyr BP, indeed a full recovery to wet conditions: #1, #2, but definitely not #3. Such similarities but subtle differences between records are a feature of all records which measure more than one hydrological proxy in the same archive. Spatial heterogeneity will still exist in the climate system and uncertainty will still exist in the interpretation of hydrological proxies and their sensitivity to the varying and variable components of the hydrological cycle (winter rainfall, summer rainfall, annual rainfall, evaporation, storminess, river flooding etc.). There is still much to learn.

Singh, S., Gupta, A.K., Rawat, S., Bhaumik, A.K., Kumar, P., Raj, S.K.: Paleomonsoonal shifts during âĹij13700 to 3100âĂŕyr BP in the central Ganga Basin, India with a severe arid phase atâĹij4.2 ka, Quaternary International, in press, https://doi.org/10.1016/j.quaint.2021.01.015.

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