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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 the reviewer for their thought-provoking review. They have raised several excellent points, which we have attempted to answer. In doing so, we feel the manuscript has been improved.

"Major points. 1)The MC-PCA analysis is state-of-the-art, but the spatial component of the results go largely unpresented or discussed. This is especially the case for PC2, given the mix of loadings, but even the spatial patterns of the strength of PC1 could

shed light on the question at hand. I suggest the authors add maps that illustrate the PC1 and 2 loadings, and their uncertainties, and discuss their results. This is particularly important for PC2. The authors describe this as a dipole that represents a fluctuating wet and dry signal. The spatial structure of this dipole, especially as it relates the the Harappan civilization, needs to be presented and interpreted; including a discussion of the age uncertainty, and record selection uncertainties (PC-1,-2,-3)."

We agree that the spatial analysis was 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. We welcome the opportunity to rectify this. We have introduced a new figure (Figure 5) which shows the loadings of PCA-3 PC1 and PC2. In the results section we have changed the description of the loadings section 3 to focus more on the spatial dipole, and (as suggested by the reviewer in a later comment) describing the temporal pattern as areas of wet and dry, rather than describing the whole pattern as wet/dry which was misleading.

The spatial dipole provides compelling additional evidence for a winter drought localized to the Indus valley area. As both the Oman and NW India records plot with the rest of the Indian Ocean rather than the Indus Valley records in PCA-3, PC2, the spatial dipole is unlikely to represent changes in the summer monsoon (which should affect Oman, India and Indus valley together) but rather represents changes in winter rainfall or the degree to which winter rainfall influences the site. We have added to the discussion in section 4.3 to make this argument.

MC-PCA doesn't include its own age uncertainty per se. The age uncertainties of the individual records are accommodated in the y-axis uncertainty of the resulting EOFs. Age uncertainty of individual transitions can be determined independently through a detection algorithm of the 2000 individual principal component analyses, for example using rampfit. We have added a sentence to the methods section to make this clearer.

We have added a sentence to the results to more explicitly state that the similarity in results between the different PCA analyses indicate that record selection is unlikely to play a large role in the inferences drawn.

"2. From the analysis, primarily the gradual decline of the summer monsoon recorded by PC1, the authors propose a "Double-drought" hypothesis, that suggest that the combination of a shorter term (300-yr-long) winter precipitation drought, followed by the monsoon weakening beginning around 3.9 kyr weakening summer precipitation may have driven the collapse. This hypothesis is reasonable, and is supported by the MC-PCA analyses, a synthesis of Harappan archeological sites, and a single speleothem record from Italy (RL4) interpreted to illustrate the decline in winter precipitation in India. The contrast between the first two lines of evidence, and the Mediterranean speleothem is striking, especially given the importance of winter precipitation in the double-drought hypothesis. Unlike the analysis that went into summer precipitation, only a single record (RL4) is shown to draw inference about "Mid-latitude Mediterranean Climate", with a notably different age model than that used in the original (2016) publication. To defend the "Double-Drought" hypothesis, the authors need to better characterize Mediterranean climate, and its relation to winter precipitation on the Indian subcontinent. The paragraph from 359-361 states that this is broadly consistent, but this needs to be robustly established. I recognize that this is a significant request, but it would be ideal to see an analysis, either new or from the literature, of multiple records from Mediterranean (if that is indeed the best way to estimate winter precipitation in India), that handles age uncertainty and disagreement between records. As is, it's difficult to evaluate the hypothesis when this key line of reasoning is so poorly supported."

We agree that this is the obvious next step in the thought process. The argument presented here has a tropical rainfall/Indian Ocean focus. This was by design as we wanted to investigate the tropical climate response to the mid to late Holocene transition rather than get pulled back into mid-latitude climate, which has had numerous

studies on the 4.2 kyr event, including two substantial review papers and a special issue in Climate of the Past in the last three years. However, we recognize that by proposing the Double Drought hypothesis we cannot ignore mid-latitude climate. Our argument perhaps relies too much on proof by elimination: a need to explain the obvious drying in the Indian subcontinent when there is no evidence for summer drought (which is as much as a tropical only study could conclude), rather than firmly establishing the existence of a winter drought outright. We are happy to rectify this. We feel that a detailed study of Mediterranean winter climate is beyond the scope of this paper and would likely not add much beyond the recent Bini et al., 2019 and Kaniewski et al., 2018 review papers. However, a more detailed explanation of the literature is certainly a reasonable request to help characterize Mediterranean winter climate and support our arguments.

We intended RL4 to be illustrative rather than indicative of Mediterranean rainfall. While there are plenty of records from the region showing the 4.2kyr event, there aren't that many that cover 5-3 kyr in the same or better resolution as our PCA-analysis, and therefore able to provide a suitable visual comparison.

A similar MC-PCA analysis to the one conducted in the Indian Ocean, but on the winter rainfall zone across the Middle East would likely not have enough records, mainly because the winter rainfall zone is much smaller than the Indian Ocean, but also because of the challenges of conducting fieldwork in the west of Iran, Afghanistan and Pakistan. A quick review of the stalagmite literature in the Middle East suggests just three or four suitable stalagmite records (including a Borneo style outgroup) and one lake record. Specifically: Jeita cave in Lebanon, which shows a 4.2 in some (Je-1, Je-3) but not all (JeG-stm-1) stalagmites. Sofular cave in Turkey, which shows no obvious signal but is probably too far outside the region and influenced by year-round precipitation. A suitable outgroup record akin to the Borneo record used in our analysis Soreq Cave in Israel is a maybe. Stalagmite 2N terminates growth at 4.4 kyr BP. The multi-stalagmite composite appears to become low resolution at 3.5 kyr BP. Tonnel'naya cave in Uzbekistan is too low resolution and may be more representative of northerly jets to provide a record on the trajectory pathway. Gejkar cave record from Iraq isn't old enough. Mitzpe Shlagim record from Israel/Syria terminates at 4.3 kyr BP. Jerusalem West is too low resolution. Gol-E-Zard record is not long enough. Other non-stalagmite high resolution archives include: The Neor Lake record, which does contain the 4.2 kyr event.

This does not mean there aren't low resolution records that can at least provide an insight as to whether drought propogated across from the Mediterranean, through the Middle East, across Iran and Afghanistan and down through Pakistan to the Indus Valley. And here take the recommendation of the reviewer to provide substantially more evidence. We decided to expand what was formerly one paragraph in section 4.3 on the 'second climate signal' into four paragraphs. The first explains the spatial pattern of the PCA analysis (as suggested in reviewer comment 1), the second on evidence for the 4.2kyr Middle Eastern drought being a winter drought, the third outlines the climate mechanism of Western Disturbances and their importance on winter rainfall variability in north-west India, and the fourth on the paleoclimate evidence for propagation of the winter drought through Iran to the Indus Valley.

We thank the reviewer for this suggestion, as we feel it has made a genuine improvement to our manuscript. As the revised manuscript will likely be submitted later, we include those four paragraphs below to facilitate discussion:

The second climate signal in the Indian subcontinent is an abrupt drying between 4.2 and 3.9 kyr BP with a return to wet conditions afterwards. This signal is observed in our PCA-3 PC2. The spatial dipole of this signal (Figure 5b) with positive loading (dryer between 4.2 and 3.9 kyr) in the three offshore Indus valley records, and negative loading (wetter conditions between 4.2 and 3.9 kyr BP) through most of the rest of the Indian Ocean records. The absence of this signal in PCA-1 PC2 and PCA-2 PC2 suggests the 4.2 and 3.9 kyr BP drying is not a dominant feature of monsoonal rainfall variability in the Indian Ocean basin, although it is locally visible in Madagascar and Australia. Further the Oman and NW India stalagmite records, which are interpreted as proxies

for variability in Indian Summer Monsoon strength via cross Arabian Sea wind strength and integrated moisture rainout respectively (Fleitmann et al., 2007, Kathayat et al., 2017), do not load together to the offshore Indus valley records, as would be expected if the PC2 were a proxy for Indian Summer Monsoon variability. This suggests that PC2, and by extension the 4.2-3.9 kyr BP drying, does not represent Indian Summer Monsoon variability.

Instead, a dry period between 4.2 and 3.9 kyr BP matches the expression of the 4.2 kyr event in the Mediterranean and Middle East as a major drought (Figure 7c). In the Middle East the 4.2kyr event is likely manifested as a reduction in winter rainfall. This is intuitive given the highly seasonal precipitation concentrated in winter months (DJF). Specific seasonally resolved or seasonally sensitive evidence for a reduction in winter rainfall in the region includes a pollen record from Tell Tweini (Kaniewski et al., 2008), a coral record from the Gulf of Oman indicating increased winter shamals and dust storms (Watanabe et al., 2019) and a positive d18O excursion in speleothems from Gol-E-Zard (Carolin et al., 2019). In the modern negative d18Oprecip occurs in Iran during winter months (Mehterian et al., 2017; Carolin et al., 2019).

Winter rainfall over the Middle East is largely sourced from the eastern Mediterranean. The synoptic weather systems continue eastwards in the form of Western Disturbances, with additional moisture from the Caspian and Arabian Seas. Western Disturbances are upper tropospheric cyclonic storms carried by the Subtropical Westerly Jet (Dimri et al., 2015, Midhuna et al. 2020 and references within both), moving across Iran, and Afghanistan into Pakistan and India until they reach the blocking Karakoram and western Himalayas. Western Disturbances are responsible for the majority of the winter rainfall in north-west India and winter snowfall in the western Himalayas (Cannon et al., 2015, Lang and Barros, 2004, Midhuna et al., 2020), and are important moisture sources for growing the winter 'rabi' crops, snowpack and subsequent spring flow of the Indus (Yadav et al., 2012).

Confirming whether a reduction in winter precipitation in the Levant and Mesopotamia at the 4.2 kyr event propagated through Iraq, Afghanistan and Pakistan to the Indus valley requires paleoclimate records from along the moisture trajectory. Insufficient high-resolution records yet exist (Burstyn et al., 2019) to provide a comparable analysis to the Indian Ocean synthesis in this paper but in general, available high-resolution records tend to show a dry event beginning at 4.2 kyr BP while lower resolution records show a mixed response. From west to east the current evidence includes: a multi-proxy record from Mirabad Lake in Iran (33.08°N 47.71°E) which shows no severe drought (Stevens et al., 2006), but may not have the sampling resolution to see such an event. The Neor Lake (37.96°N, 48.55°E) record of aeolian input and hydrological conditions shows increased dustiness (Sharifi et al., 2015). The Gol-E-Zard speleothem d18O records (35.84°N, 52.00°E) (Carolin et al., 2019) indicates a reduction in winter rainfall. Pollen analysis from Maharlou Lake (∼29.45°N, 52.75°E) suggests no major upheaval in the mid-late Holocene transition, with continuous human cultivation (Djamali et al., 2019). A lake sediment magnetic susceptibility record from Lake Hamoun in Iran (30.93°N, 61.25°E) suggests some kind of transient dry event around the middle to late Holocene, but the dating is insufficient to confirm a 4.2 kyr BP timing (Ali Hamzeh et al., 2016). In the Indus valley itself, carbon isotope values of rice grains, a summer crop, show increased drought only after 4.0 kyr BP in both Gujarat and Indus valley locations, indicative of no substantial change to summer rainfall between 4.2 and 3.9 kyr BP (Kaushal et al., 2019).

“3. In new studies that rely on syntheses of multiple records, it's critical that the data (and ideally the code) used to conduct the analyses is available, and replicable. Data that are only available upon request are not publicly accessible, and I strongly encourage the authors to archive the data used in the analysis in a public data repository. If they cannot, the authors need to explain why they cannot follow the best practice recommendation in the “Data availability” section.” We agree 100%. 1) PCA data: as stated in our Data Availability statement: “at the NOAA Paleoclimatology Database: https://www.ncdc.noaa.gov/paleo/study/xxxxx.” with the exact url updated at publication 2) New data: The new stalagmite record from Madagascar will be archived in

the NOAA database and submitted to the SISAL database, as per the data availability statement of the companion submission (cp-2020-137). 3) Previously published data: Supplementary Table 2 lists the SISAL entity ID for the six stalagmite records discussed in the text, along with dois for the marine cores with publicly available data. It is not our place to share data already publicly archived that was not generated by us. 4) The analytical code in this study is written by others and properly cited: namely Deininger 2017 and Mudelsee 2000 and 2013. It would be a breach of copyright to share this code. Our statement on making the data available upon request is an additional courtesy extended to all researchers who might wish to contact the authors to discuss and use the data. We agree entirely that data should also be publicly archived, and it will be, as per the data availability statement.

“4. Why is PC-3 truncated? Looking at the data in figure 2, the three sedimentary records span the full length of PC-1, and PC-2. This decision needs to be explained and justified.” Truncation of the records is part of the Deininger software. First, the software does not extrapolate beyond individual ages to the top and bottom of archives, so the records used will always be shorter than those published. Second, the PCA has to have 2000 full realizations of all included records. Therefore, the younger limits are determined by the 1 sigma older bound of the youngest age of the earliest finishing record. In the case of PCA1 and 2 this is the Rodrigues stalagmite record at 3070 yrs BP. The older limit is determined by the 1 sigma younger bound of the oldest age of the latest starting record. In the case of PCA1 this is around 5600 yrs BP so not an issue which influences the 5000-year cutoff. In the case of PCA2, the Madagascar record provides the limit at 4950 yr BP.

For PCA3 the issue is slightly different. The Deininger software requires not just a mean resolution below the threshold, but a data-point in every bin. For this reason, the Lake Rara Mn/Ti and Flores stalagmite records were not included in the analysis as they contained areas of higher and lower resolution proxy measurements. The Giesche record does contain a data-point in every bin. But only for certain age realizations. The stretching and compression of the age model that occurs in each realization occasionally pulls data points out of a bin, leading to a truncation of the PCA time period. This is particularly important for the radiocarbon ages in the Giesche record, where the error bars at 4600 yr BP are +-100 years, compared to +-40 years at other depths, leading to much larger stretching of the record.

PCA3 is therefore a balance between length of record, and resolution of record. A resolution of 20 years necessitates truncating the record at ~4400 years. The same is true up to 35year resolution. There are slight improvements with decreasing resolution: 4450 at 40 years, 4500 at 45 years, and 4650 at 50 years. This is ultimately a judgement call between length of analysis and resolution of analysis. As this study is designed to use only the highest resolution records, we decided that the 4.5 to 3.3 time period provided by PCA-3 covers the major periods of interest.

We have added a description of this to the methods section so that the decision is at least explained and justified. We have kept the analysis as is, but are willing to change PCA-3 to a 50 year resolution if required.

"Detailed points: 38-39: Awkward to say "increasingly recognized", and then have only references from 2003 and 2004." Oops, yes. We have included a 2011 reference, but there are few non-local overview papers from the last five years, so we also removed the increasingly.

"115: "Between the records"? Do you mean between the ages?" Yes, changed as suggested. Thank-you

"116: Upscaled. Might be better to say "degraded"." We disagree. Upscaled is equally precise, but less emotive. Upscaling and downscaling is also the more commonly used term e.g. Anchukaitis and Tierney 2012, Deininger et al., 2017)

"140-144: Why? It seems easy enough to use identical ensemble members each time through. " We have tried to minimize the number of changes made to the Deininger

code. Changes to the code remove the reproducibility of our work. Using identical ensemble members does not change the outcome of this paper, and our use of different age models gives more conservative error bars rather than less, so we are not overinterpreting the results.

"More importantly, can you justify treating multiple records from the same core as independent climate records?" This is a good question. It seems inevitable that different foram species d18O at the same location have a certain degree of correlation that is independent of the climatic variable attributed to each one individually, and therefore the records are not truly independent. We note that the original Giesche paper interprets winter rainfall as resulting from changes in three foram species via the delat of both sacculifer-ruber and duterrei-ruber proxies while interpreting summer rainfall from one of the foram species (ruber). The two seasons are therefore not completely independent and could explain why Giesche concludes a simultaneous summer and winter drought. Our treatment of the two records (G. ruber and N. dutertrei) rather than the interpretations (Summer and Winter rainfall) removes as much of the interdependence as possible. The addition of the Arabian Sea record does go someway to alleviate the problem by providing a third record from the area, and one that has a similar positive PC2 Loading. Further, the close agreement of PCA3-PC1 which includes all three Arabian Sea records with PCA1-PC1 and PCA2-PC1 which include none suggest that any codependence of two records makes little difference to the overall outcome of the paper.

"172: I suggest you start by describing this as a dipole, and then describe it as fluctuating wet and dry. 178: I don't think you can call this a dry period in PC2, since it's a dipole, it must be dry some places and wet others. In general, the spatial characteristics of PC2 should be fleshed out." We agree with both of these comments. We have restructured the paragraphs to talk about in terms of spatial variability of possible dipoles rather than wet and dry conditions. We also agree that the spatial characteristics of PC2, especially from PCA-3, require additional discussion. While we have added a little here, we feel it is best to interpret the spatial dipole in the discussion. We have added several sentences to the paragraph starting line 260 in section 4.3. Thank-you.

“294: 4.26 to 3.97 kyr BP this is a lot of precision, where do these numbers come from, and what are their uncertainties? This is comment relevant throughout the discussion.” 4.26 and 3.97 in regard to the winter drying both come from Carolin et al. 2019 as cited in their first useage on line 41. We have chosen to report this number to same number of significant figures as the original paper.

3.97 and 3.71 in regard to the summer drying comes from the rampfit analysis from PC1. The standard error on these number are 92 and 95 years. We have added a quick explanation of where these numbers come from to line 296 to make this clearer.

In general, a good rule of thumb for error reporting is two significant digits on the error, and the same number of decimal places for the value. In theory this allows us (just) to report the rampfit derived values to the nearest year. However, given that the quoted errors are one standard error, we can see why this might be viewed as a stretch. Therefore, we have changed all mentions of these values to the nearest ten years (one significant digit of error). We feel this is a reasonable degree of accuracy that does not overstretch these numbers.

At all other points in the manuscript, we quote ages to the nearest 100 years which is comparable to radiocarbon error at this age, with the occasional use of the nearest ten years when defined as such in the original literature (e.g. line 240).

“309: What does this analysis say about whether the mature Harappan occurred during a short term pluvial?” Our data supports this idea. We do have a sentence later in the manuscript that outlines this. However, it is clear this reasoning should be presented earlier. Therefore, we have added a sentence to this paragraph. In particular, PCA3 PC2 shows wet conditions between 4.5 and 4.3 kyr BP. It is also likely that both PC1 and PC2 from PCA1 and 3 show a modest and substantial increases in moisture

between 4.7 and 4.5 kyr BP respectively. But as these analyses say less about rainfall in the Indus valley stating such could be an overinterpretation of the data.

“314: “was likely caused by a reduction in western disturbances that bring rainfall from the Mediterranean and Middle East (Figure 6c). “How do you know this?” This was said with far more certainty than it should have been. The idea of reduced winter rainfall is hypothesized by us to explain a reduction in rainfall that our analysis shows was unlikely to be caused by a reduction in summer rainfall. That this reduction in rainfall was caused by reduced western disturbances is an inference from the idea from Giosan et al., 2018 that the increase in winter rainfall three hundred years previously was caused by an increase in western disturbances. We hope that our new wording of this paragraph makes this clearer. See response to Q2 for more detail.

“Figure 1. Add Harappan sites” Changed as suggested

“Figure 5. Is this the ratio of winter to summer? Or the fraction that winter represents out of total rainfall. “ This is the ratio. ie. winter (DJAF) divided by summer (JJAS). We have amended the figure axis label and caption to make this clearer.

“Also, an overview map showing where this is would be helpful for those less familiar with the region “ Changed as suggested

“Figure 7. The d) label is a little hard to locate. Consider moving it to the top left corner.” Changed as suggested

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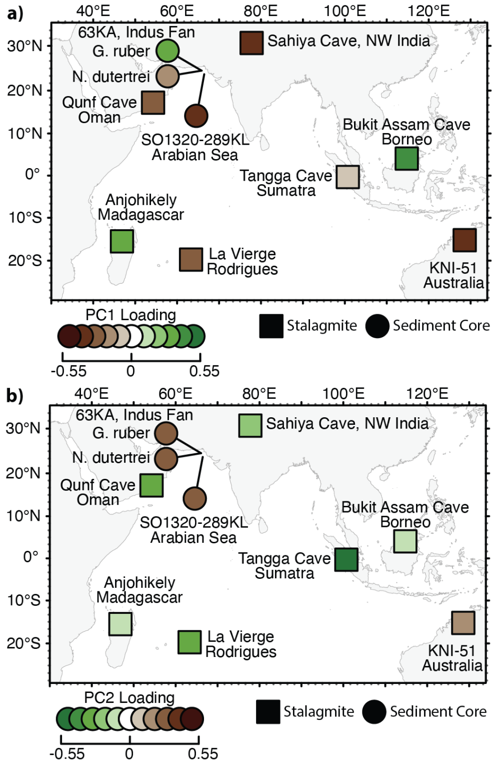
a)
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Arabian Sea
Sahiya Cave, NW India
Bukit Assam Cave
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PC2 Loading
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Fig. 1.