

Response to Reviewer 1:

The article, in reality, consists of two parts: the presentation of a new quantitative reconstruction of Indian monsoon winter precipitation and a discussion of the interlinkages between hydroclimatic changes (e.g. drought) and the collapse of the Harappan civilization. There is no problem in itself with this, although the fact that there are two separate “stories” from time to time makes it slightly more difficult to follow the article. The article is, in general, well written but additional polishing of the text would be preferable prior to publication. The text contains quite a number of typos (especially in the references).

We are thankful for the reviewer’s appreciation and suggestions. Typos are addressed.

Moreover, especially the figures could be clearer and improved. As a minimum, all the graphs should be in colour to make them easier to read.

We adopted a philosophy of minimal use of color to highlight the important points of each figure. However, we made a few changes that address the reviewer’s point and increase readability: (1) we highlighted ENA in color; (2) we increased the visibility of records developed for this study by changing their color to distinguish them from other records used for comparison, and (3) colored some of the archaeological records that otherwise had a high potential to lead to confusions. See modified figures and captions at the end of this response.

The article is clearly suited for publication in *Climate of the Past* but first after a careful revision where the authors can consider my suggestions below. I have no comments regarding the new Indian monsoon winter precipitation reconstruction. It is clearly an important palaeoclimatological contribution that in itself would merit publication in *Climate of the Past*. On the other hand, the general discussion about climatic–societal links in the past can clearly be improved. This field is nowadays large and the references provided are few and rather old. For example, I am missing the works by Carey (2012), McMichael (2012), Brooke (2014); Izdebski et al. (2015), d’Alpoim Guedes et al. (2016), Nelson et al. (2016), Ljungqvist (2017) and Haldon et al. (2018). The methodological and conceptional problems, and interdisciplinary challenges, connected with trying to link climatic changes with societal changes need to be discussed more.

It was not our intention to expand the discussion of climate-society interactions but see no harm in adding a sentence to that regard with the series of references suggested. Indeed, these references that address largely the historical period cover a lot of ground especially due to availability of contemporaneous documents. The situation is a bit different for pre-historical cultures, and especially for the Indus, that do not benefit from written sources.

Modified section now reads: *“Moreover, our knowledge of temporal and spatial climatic patterns remains too restricted, especially deeper in time, to fully address social dynamics. Significant progress in addressing this problem have been made especially for*

historical intervals (e.g., Carey, 2012; McMichael, 2012; Brooke, 2014; Izdebski et al., 2015; d'Alpoim Guedes et al., 2016; Nelson et al., 2016; Ljungqvist, 2017; Haldon et al., 2018). Still, the coalescence of migration phenomena, profound cultural transformations and/or collapse of prehistorical societies regardless of geographical and cultural boundaries during certain time periods characterized by climatic anomalies, events or regime shifts suggests that large scale climate variability may be involved (e.g., Donges et al., 2015 and references therein)."

I would also advise the authors to describe various aspects of the Harappan civilization more in detail on 1–2 pages. Without this information, it is difficult for a non-expert reader to assess if the links to drought that the authors make are plausible or not. I understand that an article of this kind cannot contain a full “handbook text” but more of an introduction to the Harappan civilization would nevertheless be helpful.

We did present the basics for this in the original version and feel that expanding would make the paper much more complex and detract from its goal. There are excellent summaries already available for this topic that are cited in the text and can be accessed by the interested reader. One solution would be, if the editor agrees with that, to write a primer on the “Indus Civilization and Climate” as Text Box (treated similarly to a figure).

Finally, it would be helpful for the reader if the authors added a conclusion/ summary of the new reconstruction at the end of the article. As it is now, the conclusion is mainly devoted to the collapse of the Harappan civilization.

An entire section (5.1.) in the subchapter 5 (“Discussion with Conclusions”) is dedicated to the new reconstruction. The fact that it is followed by section 5.2 dedicated to the Harappan may give the impression communicated by the reviewer. We do not think that restructuring subchapter 5 would change much in the economy of the paper. But if the editor feels that a separate conclusion subchapter is needed we can add that.

Lines 35–36: This sentence is a bit unclear. Do the authors mean that the Little Ice Age only occurred in the extra-tropical Northern Hemisphere? It was indeed global.

LIA appears to have indeed been global, although this is not universally accepted. On the other hand LIA was particularly strong and prolonged in the Northern Hemisphere (NH), which indicates either a cause or a positive feedback in the NH as discussed in references cited. We added in the text that LIA has a global extent and cited appropriate references.

Modified section now reads: *“LNA includes well-known cold events such as the Little Ice Age (LIA), an episode of global reach but stronger and more extensive in the Northern Hemisphere (IPCC, 2103; Mann et al., 2009; Neukom et al., 2014) and the preceding cold during the European Migration Period (Büntgen et al., 2016).”*

Lines 41—42 and elsewhere: I am not entirely happy with the phrase “Early Neoglacial Anomaly” – the Neoglaciation started well before the event in question and it is thus not “early”.

The Neoglacial is not formally defined at a global scale as it is time-transgressive regionally. Instead we used the census approach of Solomina et al. (2015) where the Neoglacial became pervasive in the Northern Hemisphere since 4,500-4,400 years ago. ENA becomes manifest in most records around that time and extends for another ca. 1,500 years, which makes it early Neoglacial rather than late Neoglacial.

Line 43: Likely also in other parts of the world.

Indeed there are some records suggestive of ENA in some Southern Hemisphere (SH) locales where records of appropriate resolution exist and we added a sentence with references in that regard.

New text: *“Whether ENA was manifest in the Southern Hemisphere remains an open question. A south of the Equator record on the Congo Fan (Schefuss et al., 2005) as well as a continental margin reconstruction further south that integrates signals from the Orange River basin (Burdanowitz et al., 2018) both show a period of increased precipitation that is largely coeval with the dry Northern Hemisphere ENA.”*

In the abstract we changed the phrasing to “accompanied by changes in wind and precipitation patterns that are particularly evident across the eastern Northern Hemisphere and Tropics” to leave open the problem to future studies in other regions.

Lines 49–50 and elsewhere: Consider using “Holocene Thermal Maximum” instead of “Holocene Optimum”.

Changed accordingly.

Lines 56–57: Consider revision here. Archaeologists work with inferring societal changes, and their possible causal connections, in societies lacking written sources all the time.

Not clear what needs changing. We agree with the reviewer but that does not mean that such connections are not difficult to prove, especially at the scale of cultures and civilizations.

Lines 59–60: Actually, our knowledge is in many cases rather good today so I would recommend to reformulate this sentence.

We cannot agree with this point. In prehistory we lack the synoptic view afforded by modern or even historic climatic data to make such a claim yet.

Line 313: “Boll” should be “Böll”.

Done.

Line 332: Please, make it clear if this ENA is thought to extend all the way to the present.

It was clearly defined just above that line: "...the Early Neoglacial Anomaly (ENA) between ca. 4,500 and 3,000 years ago..."

Line 335: I would cite IPCC (2013) here rather than Mann et al. (2009).

Added the suggested reference but also kept Mann et al. as it is a well-grounded, dedicated study of the problem.

Lines 336–339: How are these LIALE related to, or the same as, the (controversial) so-called "Bond events" detected for the North Atlantic region and elsewhere? I think this should be discussed here.

This is indeed a controversial issue that would be better discussed at large in a review-type context.

Line 370 onward: I am not entirely convinced that the impacts of solar forcing and volcanic forcing were necessarily smaller in a warmer world with stronger orbital forcing. The mean state of climate was different but not necessarily the centennial- to decadal-scale variations.

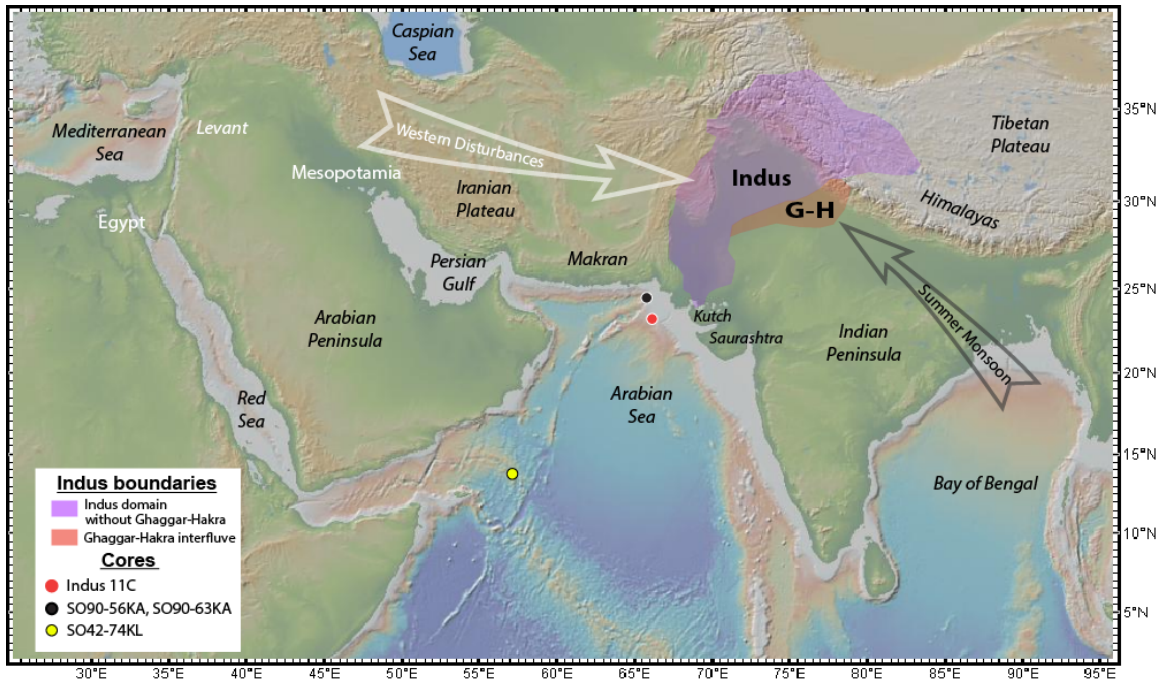
We agree with the reviewer and that is why we limited ourselves to examples based on cited literature. Some (e.g., Wirtz et al.) show increase or decrease in sub-orbital variability that is regionally organized. Testing how our suggested mechanism for ENA can be achieved in future modeling studies and is beyond the scope of our current study.

Lines 373–374: Again, you may cite IPCC (2013) here.

IPCC (2013) added.

Fig. 1: Please, also insert in the legend directly in the figure what the coloured fields mean.

Done.



Figs. 3–5: Please redraw the figures in colour and make them clearer. Now, both the graphs themselves and the text in them are not very distinct.

Some changes made. Please see explanations above and figures below.

