

Interactive comment on “Holocene climatic changes in the Westerly-Indian Monsoon realm and its anthropogenic impact” by Nicole Burdanowitz et al.

Anonymous Referee #1

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Review of manuscript “Holocene climatic changes in the Westerly-Indian Monsoon realm and its anthropogenic impact” by Burdanowitz et al.. The main thrust of this manuscript is to use high-resolution proxy records from the NE-Arabian Sea reflecting various aspects of Holocene monsoonal changes in the region. The main new data series include Uk37 based SST estimates, alongside a number of proxy time series (Ti/Al, endmember modelled aeolian input and lithogenics mass accumulation rates) reflecting lithogenic input in the region with the latter being of central importance. Based on these data the authors conclude that the Arabian Sea region around 4.6-3kaBP became more sensitive to changes in the tropical westerly jet controlling climate in the region. Overall there may well be something interesting in this manuscript, but in the

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current state it is not clear what this finding/story actually is. These are just a few problems. In my judgement, the biggest issue is the combination of results and discussion. In the absence of a dedicated results chapter the text rather selectively describes certain findings, whilst (largely) ignoring others. As an example, the authors place emphasis on relationship of the early Holocene rises in the LitMAR record (ignoring at this stage that there is a gap in the record near the time period being discussed) and Bond event 5. Later in the manuscript, again, it is being emphasised that there is a relation between the final Bond events and variability in the LitMAR record. What about the Bond cycles between 7.5. and 3.5kaBP. There is no relation in my view between these cycles and the LitMAR record, which in this interval has no obvious signal. What is this mismatch driven by, the alleged climatological connection between the Bond events and sedimentation in the Arabian Sea, or is the LitMAR proxy not sufficiently sensitive? With regard to the wavelet power spectrum, I am not convinced of the usefulness in this case, the main problem being the lack of a clearly visible signal in the LitMAR record between 7.5-3.5 kaBP. How does that affect the overall analysis? Also, it would help to inform the reader of the main findings based on this analysis (in the main text) rather than just alluding to change in frequency. There are quite a few statements that lack clarity regarding the implied change in the monsoon system and therefore appear contradictory. As an example in lines 154/5 there is this statement “This warm period encompasses the Mid-Holocene climate optimum period and is characterized by low LIT MAR and increasing fluvial input (Figure 2)..” Would increasing fluvial input not entail higher lithogenic sedimentation rates? If so, how does this compare the overall low LitMAR record? Similar inconsistencies regarding the general state of the monsoon circulation can be found elsewhere in the manuscript. With regard to the LIG approach there is not sufficient justification provided why the chosen gradients are the most appropriate. There have been other approaches (on different time scales) such as by Reichart who has used a different gradient. There should be a better explanation as to the reasons for choosing the LIG's. Overall, there may well be something interesting in this paper. Currently, however, it lacks maturity and requires a substantial rewrite.

There, should be a better separation between results (all) and the interpretation. In addition, the discussion should be “closer” to the actual data. Large parts of the text read like and general discussion with a loose relation to the actual observations. More could be said.

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