

## ***Interactive comment on “Cyclone trends constrain monsoon variability during Late Oligocene sea level highstands (Kachchh Basin, NW India)” by M. Reuter et al.***

**Anonymous Referee #3**

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This manuscript is based on a detailed description of a sedimentary sequence from northwestern India. Such records are rare in the (western) geological literature and as such this manuscript provides a valuable contribution. Before this paper is suitable for publication, however, several issues need some further attention. 1) The time frame presented is not very well presented and not convincing. After a rough biostratigraphic correlation the sequence is “fine tuned” to the Haq global sealevel stack. Subsequently the differences in sea level are use for interpreting the events. This tends towards a circular argument. It would be much better to first establish a detailed stratigraphy and compare this to the sea level and global isotopic records. 2) The sedimentary structures and fossil content have probably been recorded in detail whereas the data

C855

presented in the figures is much more condensed. This is as such not problematic, but to my opinion the interpretation (based on what is shown now) in terms of storm layers is not 100% convincing. This does not imply that the interpretation is not correct, but based on the sequences presented but this cannot be assessed independently. One solution might be to have the detailed logs and photos published on line as supplementary data. As such a detailed sedimentological description does not differ from geochemical or geophysical data. 3) The discussion of temperature effect versus monsoon intensity should be more clearly structured. Currently the Arabian Sea summer monsoon effectively prevents large storms to develop at the time sea surface temperatures are at their maximum. This is partly due to the high wind stress curl over the area and partly to the strong temperature inversion higher in the atmosphere where the north easterlies override the southwesterlies. The storm layers therefore are indicative for a not yet developed Indian monsoon, which is on line with what we currently know about the mechanisms controlling monsoon strength on geological time scales. After the authors have concluded this they start evaluating the data against sea level changes again. This is in my opinion not possible. This could be repaired by: I) a better independent time frame (see point 1) and, II) simplifying the discussion.

Below some minor comments, referring to page and line numbers.

p. 584, l. 1: change order of words: Ongoing climate change has an unknown impact on... l. 5: change “an indicator” in “recorder” l. 7: These are not direct recorders of atmospheric circulation. At best they are indirect recorders of climate phenomena related to atmospheric circulation. l. 19 and 20: The intensity of cyclones is not primarily linked to Indian monsoon strength. Rather the Indian summer monsoon prevents cyclones to develop in the Arabian Sea l. 21-25: Omit, no evidence for this presented in manuscript. p. 585, l. 2: change “hazards” into “weather phenomena”. l. 3: change “associate” into “relate” l. 4: Please explain what kind of “trends” you refer to. l. 21: change “considered” into “hypothesized”. l. 22-24: Not clear, please explain. l. 25: Omit “could”. l. 27: change “herein” into “here”. p. 586 l. 3. Change “the climate

C856

pattern” into “atmospheric circulation”. l. 4-7: strange ending of paragraph. Explain what you want to test here. p. 587 l. 20-21: Explain the methodology how the major components were semi-quantified. E.g. number per m<sup>2</sup> of outcrop, or estimate in relative abundance. P. 589 l. 23: explain “rare nautilids”. p. 591 l. 19-21: This is not necessarily so. Glauconite indicates a low sedimentation rate rather than great water depth. Enhanced Fe-cycling is a prerequisite in combination with enough time to form glauconite. This is in-line with the frequent association with transgressive facies. Please check recent literature on glauconite. l. 26: Indicate what these deeper water indicators are. p. 592 l. 26-29: this is crucial for the interpretation and should be discussed more in-depth. p. 593 l. 9-10: What is a “redox discontinuity potential layer”? p. 594 l. 17: enter “a” or “the” before “coarse” l. 18: enter “had” after “have” p. 595 l. 1: not necessarily so, the glauconite could also indicate a transgressive phase. See comment above. l. 7: change “caused” into “resulted in”. p. 596 l. 11: delete “extremely”. p. 598 l. 7: change “small” into “little”. p. 599 l. 5-9: .which is, hence, in contrast with the results presented here. l. 8: change “Niovet” in “Nivet”. l. 20-24; over interpreted, please omit. p. 600 l. 1: “Asian monsoon decline” is in contrast with p. 599, l. 4: “weak”. Change versus state. l. 10-14: rephrase. Conclusions should be changed to fit the changed manuscript. Figure 1: digital elevation not needed for manuscript. Change for location map. Figure 2: What do the blue bars indicate? Sea level? Figure 3: change into a robust diagram showing the biostratigraphy needed to constrain ages. Figure 5. tentative and not very clear. Might be swapped for correlation and zoom-in of the Zachos curve at the relevant intervals.

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