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Interactive Comment

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

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The clear reply to my initial review warrants the following acknowledgements:

Storm bed sedimentology. The authors expose my personal bias to the common preservation of silliciclastic storm beds where relatively coarse sediment derived from relatively proximal locations is seen to be transported away from the coast during large storm events (Cheel, 1991; Goff et al., 2010; Xu et al., 2004). The authors supply published evidence for the reverse occurring, and their point about the relevance of deviations of foraminifera tests with respect to silliclastic sand-sized grains is interesting. Where the two modes of directional storm transport are open, the context should

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provide evidence, and the authors reason that the organisms observed to encrust the storm beds indicate very shallow water depths. Indeed, I had not fully recognised the relevance of this paleontological evidence.

Ferruginous crusts. I was slightly surprised to see in the reply that the authors understood I insisted that ferruginous crusts were always caused by flooding and sediment starvation. Upon re-reading of my review, it must be due to wording: “Without excluding the formation of such compounds where ironrich groundwater comes into contact with the atmosphere, we know such iron compounds are also commonly formed in the sea.” The first part of this sentence was meant as an acknowledgement of formation of iron compounds by sub-aerial emergence, but admittedly, it may read cryptically, and it does not cover the lateritic interpretation of the authors. It is certainly not up to me to re-interpret the author’s data, but my review does challenge them to argue for emergence and against flooding&starvation in more detail, as this is a pivotal point underpinning their significant conclusion, and these alternatives should not be dismissed implicitly by not addressing them. Again, the point here is: in the presence of two diametrically opposed models for Iron compound formation (emergence and flooding), can the authors supply evidence for their interpretation? In their reply they specify their reasoning with the observation that ferriclasts “typically represent nodules with pedorelics, and indicate lateritic origin”. This may adequately address my comment, but it would be useful to the reader if the authors could supply facies descriptions, perhaps with photographic evidence to support their claim. Photographic evidence of mammalian bones is wasted on me because I certainly cannot distinguish marine vs. non-marine mammalian bones from photographs, but perhaps the authors can establish this matter with other experts.

My 3rd main point concerned the application of Global Sequence Stratigraphic correlations to the present section, without establishing the influence of tectonics and sediment budgets on the relative sea level. The authors acknowledge the point and show clear understanding of the problem I raise. I have two remaining issues with their reply. A) In general, with their implicit logic: The fact that something has been vigorously debated does not solve the debate, and the GTS 2012 not ruling out eustatic sealevel as a control does not establish that eustatic sea level is the forcing control behind the authors' data. B) Specifically: The authors acknowledge that biostratigraphy is an inadequate tool in this section, yet use it as the justification for their correlation. In absence of the possibility to undertake an integrated stratigraphical study of the section, at the very least the authors should supply an error estimate related to the biostratigraphy they use in Section 5.2, to allow the reader to make up his mind about the strength of the correlation.

The authors have replied clearly to my request to specify their ideas on scales associated with shell beds where I offered a possible parasequence interpretation. They see these beds as related to single storm events. In my perspective on stratigraphy, variations in lithology over dm-scale associated with 100 kyr timespans are related to 4th order relative sea-level fluctuations and climatic cycles, i.e. to changing conditions over the entire basin, rather than to single storm events. The "mass occurrence of irregular echinoids" seems to argue against cannibalisation by exceptional storm beds over 10's to 100's of kyrs as much as a parasequence origin. Regardless of whether we can accurately determine the recurrence interval of the events, approximately 20 beds in approximately 2 Myrs gives a $\sim$100 kyr frequency for bed formation. The discrepancy between the timescale of a storm (2 days), the recurrence intervals of seasonal, decadal, and even centennial storm intensities, and this 100 kyr frequency gives me a sense that a leap-of-faith is needed to link the deposit composition to variations in storm wave base depth of single storms.

It is now clear to me that the paper does not aim to reconstruct wave base evolution through a climatic cycle, but over the peaks of three successive cycles. This improved understanding of the authors' intentions brings about a small number of new detailed questions:

-Upon re-reading, I now understand the meaning of the first sentence of Section 5.5.

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Perhaps this statement should be expanded to clarify that the intention is to compare the paleo-wave-base at comparable relative positions within sealevel cycles.

-The data that must convince the reader of the author's point is in the associations of fossils in Fig. 2. I suggest that the fossils are grouped and color-coded according to the depth-representative assemblages of the model in Figure 4. This grouping should indicate a clear upward trend in presence of deepening storm assemblages over the three high stand periods. At present, the presentation of the order fossils is guided by a different convention, which makes that the visual representation of the data does not support the authors' claim yet.

-What is the intention of the sentence "Vertical changes in these skeletal associations give evidence of gradually increasing tropical cyclone intensity in line with third-order sea level rise." [Abstract l17-19] Perhaps this should be rephrased.

Cheel (1991) Grain fabrics in Hummocky cross-stratified storm beds: genetic implications. Journal of Sedimentary Petrology, v.61, 102-110.

Goff et al. (2010) Offshore transport of sediment during cyclonic storms: Hurricane Ike (2008), Texas Gulf Coast, USA. Geology, v.38, 351-354.

Xu, et al. (2004) In-situ measurement of velocity structure within turbidity currents. Geophysical Research Letters, v.31, L09311.

Interactive comment on Clim. Past Discuss., 9, 583, 2013.