Clim. Past Discuss., 9, C168–C172, 2013
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Climate of the Past Discussions

# *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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Received and published: 15 March 2013

The paper under consideration draws on paleontology, climatology, and sedimentology to assert that stormbeds and their fossil assemblages in a stratigraphic section in India can be used as a proxy for cyclone intensity and monsoon variability along the NW Indian coast during the Oligocene. I restrict my review to the sedimentological aspects of the work, which lead me to express grave reservations about the line of evidence the authors present for their interpretation of (trends in) water depth on a number of scales.

As always, I am open to be corrected by sound sedimentological reasoning. . .



Process sedimentology of storm beds. Storm bed deposits are formed during and immediately after large storm events. The authors draw an analogy between their 0.1-0.5 m thick, fossil rich, coarse grained beds and deposits from large storm systems such as we know them from present day settings such as the Gulf of Mexico. The state of my sedimentological knowledge about such events is as follows: Coastal water levels are elevated due to a combination of: a) surface shear by onshore directed winds; b) net mass transport of water towards the shore by asymmetry in the wave field approaching the shore; c) low barometric pressure in coastal areas and under storm systems relative to distant ocean pressures ("law of communicating vessels"). At the same time, high energy waves entrain very large amounts of sediment at the shoreline (sand), and the shallowest parts of the sea, with silt and clay-aggregate particles being entrained down to a level referred to as Storm Wave Base. On-shore directed mass transport along the sea surface is balanced by shore-parallel and off-shore directed return flows along the bottom of the sea. The off-shore component of the bottom currents is enlarged by or entirely derived from the negative buoyancy supplied to the bottom water by the entrained sediments. Since the return-flows reach velocities of 1-2 m/s, they in turn are capable of entraining extra sediment and enhance storm wave induced sediment entrainment from the seafloor. Essentially, storms create seaward directed density flows along the seafloor. The authors model summarised in Figure 4, in which storms act to transport sediment upwards along the bottom of the sea runs 180 degrees against the grain of what I understand of storm events, without the authors presenting any justification for such a mechanism. Onshore directed transport seems to be more in line with tsunamite deposition, as surge models for on- and off-shore directed transport associated with large storms has been invalidated in modern settings, but this seems to bring us into the debate of distinguishing tsunamites from tempestites. As becomes clear later in my review, I am not tempted to interpret these beds in event-terms anyway. This is a critical aspect of the present work, as the model for storm deposit generation outlined above places the assemblages with most complex shallow-deep mixed biota in the deepest environment and invalidates the reasoning at the start of Section 5.5 that assemblage composition reflects shifting storm wave base rather than water depth.

Ferruginous hard-grounds: Depositional Environment (Section 5.1). The authors seem to carry the assumption that iron-(oxohydro-)oxides with red appearances indicate atmospheric exposure. Without excluding the formation of such compounds where iron-rich groundwater comes into contact with the atmosphere, we know such iron compounds are also commonly formed in the sea. Ferruginous hard-grounds in (shallow-)marine sections are routinely interpreted to represent marine drowning under sediment starvation, not as surfaces related to emergence and atmospheric exposure.

Ferruginous hard-grounds: Sequence Stratigraphy. In a sequence stratigraphic context, this standard interpretation places the ferruginous crusts at ~2.6 m and 7.5 m in Figure 2 immediately below Maximum Flooding Surfaces, 180 degrees out of phase with the authors' interpretation of relative sealevel lowstand in Figure 3.

Global Sequence Stratigraphic correlations. The concept of sequence stratigraphy in essence puts equal importance on the trinity of forcing by absolute sealevel, regional tectonics, and regional climate on the resulting balance between regional accommodation and regional sediment supply. The concept of global sequence stratigraphic correlation relies heavily on dominance of sequence stratigraphic architecture by a global eustatic sea-level signal. Decades after the bloom of global sequence stratigraphic correlations, the burden of proof has shifted towards workers who want to proof that such correlations are correct for their region of interest. Recent work in the geodynamic community has shown that regional vertical motions of 100's of meters over 2nd and 3rd order timescales can be related to topographic expressions of mantle convection, a concept referred to as "dynamic topography". In fact, we learn in Section 5.3 that the study area is located on the flank of an uplift that experienced significant tectonic movements during the time period under investigation. Also, coupling between long term climatic fluctuations in catchment areas and resulting sediment supply cycles are gravely understudied, but must be assumed to play an important role in settings where



monsoon intensity is regulated on millennial to million year timescales. Therefore, the present climatic and tectonic setting requires independent dating of the section and the interpreted sea-level fluctuations it contains to validate correlations of local expressions of the interplay between relative sealevel and sediment supply to the global eustatic correlations of Figure 3. In this figure, the authors claim 3 ~Myr-duration hiatuses to be present in their section. Do they have evidence for these that is not based on the sequence stratigraphic interpretation of their section?

Occurrence intervals of "Storm deposits". One of the hardest aspects of sedimentology is getting a grip on superposed timescales in a depositional section. A storm and its aftermath last on the order of days, during which a storm bed upto 0.5m can indeed easily be formed. In Figure 3, we learn that the authors think that ~50% of the time between 27.5 and 23.5 Ma was characterised by marine deposition, so a cumulative period of 2 Myr. In their section I count 21 intervals interpreted as storm related deposits, yielding an occurrence rate of 1 per 100 kyr. Do the authors think that we are discussing amalgamated storm beds, or that the storm beds were only deposited by storms with 100 kyr recurrence intervals, i.e. rare events in the geological sense. Because the authors want to link the sedimentology of these deposits with trends in atmospheric conditions, the burden of evidence is again on them to supply a sound reasoning to explain the discrepancies between timescales involved in weather (storm), climate (cyclone variability over decadal, centennial and millennial timescales), and the recurrence of different storm assemblages over ~100 kyr. I note that the 100 kyr time interval is in a broadly accepted range for parasequence duration, which opens the interpretation of the dm-0.5 m beds in a stratigraphic rather than an event context.

Non-record of the peaks. In the authors' interpretation in Figure 3 we learn that no depositional record has recorded the climatic conditions during the lowstands. This seems to have severe implications for the impacts of the current study, as the authors cannot establish or claim a record of monsoon variability climate proxies for the climate conditions that are associated with "lowstands". This means the present section cannot be used to constrain storm activity and wave base depth over maxima and minima of a climatic cycle, but at best between maxima of subsequent cycles.

In conclusion:

Because the authors want to establish a climatological story of signifficant impact, they need an impeccable sedimentological suite of observations, with flawless interpretation. I feel that they have not been able to deliver on this sedimentological interpretation, and this touches the hart of their case.

On these points, I am open to discussion where my knowledge is limited or the authors have more evidence available.

Yours Sincerely,

Joris Eggenhuisen.

Please also note the supplement to this comment:
http://www.clim-past-discuss.net/9/C168/2013/cpd-9-C168-2013-supplement.pdf

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