

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

A. Sluijs (Editor)

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Dear Dr. Reuter,

Please accept my apologies on behalf of the Editorial Board for the delay in the processing of your CPD paper. It took too long to find an appropriate editor and then I was unfortunate in the selection of reviewers. I requested no less than 16 researchers to review the paper. Of those, 13 either declined or did not reply to my request at all within two weeks. This is why it took far too long to establish the desired three reviewer reports. This is certainly abnormal for Climate of the Past as we work hard to make the Discussion phase as concise as possible. That certainly did not happen for your paper

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and therefore I can unfortunately not offer anything else apart from my apologies.

Three specialists in the various fields that your paper touches on have now reviewed your paper. Thank you for actively contributing to the online discussion by replies and clarifications to their comments. It is clear that the reviewers and your clarifications have identified several points that require further explanation and discussion in a revised manuscript. I would consider the required modifications to be moderate.

Please refer to the online discussion to revise your manuscript. I would appreciate a point-by-point list of the reviewer's comments and how you have handled these points in the revision. This facilitates editorial handling significantly. Allow me to bring up one point that you raised in your reply to Reviewer 2. You write that "The comparison of our results with the oxygen isotope curve of Zachos et al. (2008) implies that the strength of the Late Oligocene monsoon over northern India was not mainly controlled by the sea surface temperature since the storm activity at 24 Ma was lower than at 26 Ma despite of a globally warmer climate (Fig. 5)." Please note, however, that the Zachos et al. compilation is a compilation of deep sea benthic foraminifer isotope data that do not necessarily reflect trends of sea surface temperature in any particular region. I suggest different or adapted argumentation of this aspect in the revised manuscript.

As you are revising the manuscript, a more elaborate introduction and discussion on connections to cyclone activity and monsoon dynamics would be appropriate, as suggested by reviewer #2. In the discussion this will likely automatically happen as you incorporate the discussion on reconciling effects of vertical shear from sea surface temperatures in controlling possible cyclone variability. Also please take note of the advise of reviewer #3 regarding the stratigraphic framework and the accuracy of correlations to eustatic and isotope curves.

I look forward to receiving your revised manuscript.

Sincerely,

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