

Clim. Past Discuss., referee comment RC3
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Comment on cp-2021-181

Anonymous Referee #2

Referee comment on "Development of longitudinal dunes under Pangaeen atmospheric circulation" by Hiroki Shozaki and Hitoshi Hasegawa, Clim. Past Discuss., <https://doi.org/10.5194/cp-2021-181-RC3>, 2022

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Development of longitudinal dunes under Pangaeen atmospheric circulation.

I found this an interesting paper dealing with a topic (dunes and climate) with which I'm familiar in a time-frame that I am not (beyond the Quaternary).

In large part the paper is well-written and structured, the evidence appears sound and the main conclusions seem to me to be well founded.

The aspects of the paper which deal with palaeo-wind directions, establishment of the aeolian sands as being deposited by longitudinal dunes, and the palaeogeographic

interpretation I thought were unproblematic.

However, the paper also made some highly speculative forays into the periodicity of past aeolian events, timing and association with orbital forcing and then to reconciliation with orbitally-forced climate simulations which I thought were unsupported. Principally, the authors did not recognise that any sediment sequence (and perhaps particularly sand dunes) is incomplete. They suffer erosion almost synchronously with deposition and for long periods afterwards until deeply buried. This is quite well modelled in papers cited by the authors (Thomas and Bailey, 2017). With only very broad dating available (millions of years) it is impossible to establish if the preserved cross-bed sets had any temporal or causal association with orbital cycles. Some description of the bounding surfaces may have gone part way to answering this, but see Leighton et al, 2013, QSR; 2014 ESR, for cautionary tales. I think that this section, specifically any claims for orbital forcing, should be removed from the paper.

They detract from what is otherwise quite a clear story with a strong conclusion in which the apparent conflict between climate models and field data is resolved. This is worthy of publication. However, some parts should be rewritten (as indicated on PDF) to make clear that the model predictions are indeed being tested.

Another weakness is the current arguments suggesting a role for vegetation in stabilising the dunes and causing sand accumulation seem ill-founded. There is no parallel in the Sahara today between sand thickness and vegetation cover. Indeed, globally, where dunes are vegetated or partly vegetated sand cover is thin.

Appendix A should be elevated to the main paper. It is interesting and valuable support for the interpretation of the dunes as being longitudinal in origin. Conversely, I question the interpretation of some sites as being barchans dune deposits. I think it is impossible today to find a site where barchans dunes form in a thick sediment sequence with preservation potential. It is quite well documented that they occur where sand supply is very low and quite often on hard surfaces which aid sand transport. Furthermore, there is no modern parallel for contemporaneous and nearby barchans and longitudinal dunes to

have divergent orientations (figure 1). I think the explanation for your single slip-face orientations is most likely to be a sampling issue.

Nevertheless, the overall conclusions of the paper regarding palaeogeography and circulation patterns are interesting, well documented and well written.

I have noted minor issues of grammar, spelling and word usage on the manuscript.

Please also note the supplement to this comment:

<https://cp.copernicus.org/preprints/cp-2021-181/cp-2021-181-RC3-supplement.pdf>