

Interactive comment on “Comparison of Cenozoic surface uplift and glacial-interglacial cycles on Himalaya-Tibet paleo-climate: Insights from a regional climate model” by Heiko Paeth et al.

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We thank the reviewer for the competent and helpful comments. We have discussed these comments among all co-authors and suggest the following revisions in the manuscript:

1. The relation to the Prell and Kutzbach paper has also been criticized by Reviewer 1. We admit that we have to rephrase and clarify our key hypotheses and classify MH and LGM as special rather than typical sub-periods of glacials and interglacials.
2. The experimental design was state-of-the-art when the experiments were carried

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out. We admit that new geological evidence has been gained in the meantime. We will include a thorough and extensive discussion of alternative interpretations of uplift phases across Asia. We will also re-phrase the temporal assignment of the uplift period in Asia.

3. We agree with the reviewer that our manuscript lacks a direct indication of the added value of regional downscaling. We haven't shown the patterns related to the driving ECHAM model because this is partly shown in another published paper and in order to reduce the number of figures. However, according to the reviewer's suggestion we will include the respective climatological and anomaly patterns in our manuscript and discuss the differences between REMO and ECHAM more explicitly.

4. We must indeed compare our results more extensively to other studies using different experimental designs and other global climate models in order to highlight the new insights gained from our experiments.

5. We will collect additional available and appropriate proxy data sets for the region and refer more extensively to other papers dedicated to such reconstructions.

6. The only noticeable and interpretable response in atmospheric circulation is the change in the extratropical stationary wavetrain over Eurasia during the uplift period. This is displayed by Fig. 7. Indeed, we have not plotted wind vectors (which are quite dispersed and confusing) but the 500 hPa geopotential which is large-scale and a direct indicator of the geostrophic wind as the major flow component in approximately 5,500 m height. We admit that this is not made clear in the manuscript. By the way, changes in surface wind from a regional climate model should not be overinterpreted because the resolution of topography is still too low. In another paper under review we have compared 10 m wind from REMO with observed wind from several stations across the Tibetan Plateau: there is hardly any consistency because the observed surface wind is driven by local topographic effects. We will address the aspect of atmospheric circulation more explicitly in the revised manuscript.

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Minor comments: We can easily deal with all changes and corrections suggested by the Reviewer..

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