

Interactive comment on “Precipitation $\delta^{18}\text{O}$ on the Himalaya-Tibet orogeny and its relationship to surface elevation” by Hong Shen and Christopher J. Poulsen

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The paper provides results from climate simulations with a water-isotope module to study the stability of rainfall isotopic lapse rates across the Tibetan-Himalayan orogen with varying altitude. The potential implications of this study are important, because the stability of these isotopic lapse rates through time and through different stages of uplift is a common assumption made by Tibetan paleoaltimetry studies and has been virtually unverified.

I must first say that I am very sympathetic with the effort made by the authors to investigate the behavior of these lapse rates with varying geography. This study is definitely

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a great contribution to paleoaltimetry – so much has been written on the paleotopography of the Tibetan Plateau without a clear understanding of atmospheric and water isotopic dynamics of South Asia. I am not a climate modeler – I am from the data side and I have been working lately on paleoaltimetry topics; yet the writing is clear and the interpretations are understandable for non-modelers, ensuring that the paper will have a real impact on paleoaltimetry practices.

The manuscript is very well-written; the interpretations are reasonable in the light of the provided climate simulations. I have little to say and I just have two main concerns:

1) First, I am amazed about the amount of misfit between the Control experiment and modern data when it comes to rainfall $d_{18}O$, particularly on the Tibetan Plateau itself. 2-5 permil of misfit in central Tibet is huge, when the variation from the Himalayan foothills to the top of the Plateau is of ~ 10 permil. The source of the mismatch is stated as “unclear” (page 9, line 22), which is not satisfactory as Tibet is the area of interest. Please discuss this in more details and explain what the model could do wrong – explaining the mismatch with the interannual variability in rainfall $d_{18}O$ measurements is not satisfactory neither. Also, compare with other models (is ECHAM the only model to do that? What does Botsuyn et al say about LMDz iso over Tibet?). This should be a main discussion point in section 4.3 (caveats).

2) There is no discussion about monsoonal run-off into the Bengal Bay and the related amplification of the rainfall isotopic depletion, which is known to be a key control on rainfall $d_{18}O$ in South Asia. An essential paper on this topic is missing from the bibliography: Breitenbach et al (2010 EPSL). Briefly, this paper shows how seasonal (monsoonal) run-off of isotopically-depleted water into the Bengal Bay result in water stratification in the Bengal Bay and higher than normal sea water $d_{18}O$, that increase rainfall isotopic depletion during the late monsoon season and explain the lowest rainfall $d_{18}O$ values. This type of seasonal effect would have a huge impact on rainfall $d_{18}O$ over the orogen and could have changed significantly with paleotopography (and monsoonal intensity). It sounds to me that the simulations provided in this manuscript

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do not take into account these game-changing seasonal effects. I would like to hear more about how is set up the lower boundary d18O values (page 4, line 20); it sounds essential. Are the lower boundary d18O values varying through the year, or set as constant for the entire year?

More minor comments:

-page 3 line 2: pedogenic and lacustrine carbonates.

-page 3 line 25: Quade et al (2011, AJS) instead of Bershaw et al (2012). (Quade and coauthors were the first).

-Page 7 line 29, page 8 line 1: "summer precipitation decreases from ..." Where? On the whole Tibet?

-page 8 line 19-21, page 9 line 4-5: Actually, the oldest loess deposits are now dated to the Eocene (see Licht et al., 2014, Nature; 2016, Nature Communications; Li et al., 2018, Nature Communications). Similarly, the onset of the modern EASM in the early Miocene is highly debated, it is likely much older (there is quantity of papers on the topic over the last 4 years). Better to remove these statements (or nuance them).

Overall it is an excellent manuscript and I am looking forward to see it published once my two main comments have been addressed.

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