

Response to Reviewer Ingalls

1. Line 428 is an example of a common theme in this work in which the authors relay data from a previous study and state what the data imply, but lack additional background necessary for the reader to understand. In this specific example, the authors cite a “positive shift” in oxygen isotopic values and say that these shifts “imply an increased regional aridification and related to enhanced East Asian winter monsoon.” However, nowhere in the work do they explain how oxygen isotopes are related to aridity or how they can be used to make inferences about atmospheric circulation and weather patterns.

We did not have a detailed discussion how their results were obtained for each of our referenced articles. If we did detailed discussion, this review would be very lengthy. Therefore, our review only summarizes common results, and obtains a basic understanding through their current research. We found that the margins of the Tibetan Plateau have three main uplifts and outward-growth, coeval with the climatic drying in Asian inland. Therefore, we concluded that the main uplifts of the margins of the Tibetan play an important role in climatic changes in inland Asia.

In the revised manuscript, we reinterpreted that how oxygen isotopes are related to aridity.

2. Incorrect use of jargon with respect to stable isotopes: I cannot speak to the discussion of paleomagnetism and radiogenic isotope techniques in this work, but I would caution some of the language used with respect to stable isotopes. The authors say “more positive/negative” or “positive/negative shifts” multiple times. A value is positive or negative and cannot be more or less positive or negative. A molecule can have a lighter/heavier isotopic composition w.r.t. a specific isotope/element, or have a lower or higher value. This may seem like a small thing, but will unnecessarily irk some readers.

Thanks for reviewer suggestions, and we revised them in the manuscript.

3. Synthesis: The discussion section mostly summarized everything outlined in Section 2 without much additional discussion of the data or contradicting studies. I think for this work to be useful for the community, it should include a more substantive addition to the discourse rather than just a fairly comprehensive laundry list of recent results. Further, the last paragraph of the Discussion calls into question what has come in light of the authors’ study of the recent literature. Lines 594-596 state that the authors could not draw linkages between the uplift of the TP and evolution of Asia’s climate, which seems to be the motivation of the entire study. If this is true, what has been learned? In the same paragraph, the authors say that climate models do not take into account “detailed topography”, but in addition to other such climate modeling work, the authors cite multiple studies that use topographic boundary conditions to constrain the effect of TP uplift on global and regional climate (as recent as the previous paragraph even). I think the Discussion section would be much improved if this paragraph was removed and replaced with a synthesis of the use of different types of proxies in each of the three tectonic intervals: which proxies seem to agree between the intervals? Which work best and which have greater uncertainty? If the authors believe more studies are needed on topographic boundary conditions, during which intervals and in which sedimentary basins and/or orogenies? These types of questions and answers can help guide the community, which is the ultimate goal of a review paper.

Thanks for reviewer that provided so many suggestions in the Discussion.

In Discussion, we have summarized the factors affecting the drought in Aisa, and concluded that the Tibetan Plateau play an important role in Asian aridification during these three intervals, especially during ~55-35 Ma. We are not going to talk about whose outcome is clearly at odds with the other outcomes. We are just coming up with a basic understanding based on a review of the recent results. But, the contradictions of recent studies are worth studying.

We cannot distinguish effects on Asian aridification between global cooling and the Tibetan Plateau during 15-8 Ma interval because of significant global cooling during this period. Because there is still a vague understanding of the uplift height of the Tibetan Plateau, especially the marginal mountains of the Tibetan, therefore, some models are based only on assumptions with respect to altitude in Tibet, which may not be consistent with reality. As a result, the results of the simulation may be uncertain.

The reviewer provided a grand goal to decipher the uplift effect and evaluate the best and uncertainty of recent results. This may be beyond the scope and subject of this study. But it is worth exploring in future.

4. Figures Figures 1, 2, and 4: It is useful to see geographically and from what tectonic domains the data used in your interpretations of “rejuvenation or initiation of tectonic activities” comes from, but because different proxies were used in each of the studies marked on the maps, and each proxy records a different thermal regime/extension/rotation/magnetism/etc., it’s unclear to me how the different points on the map can be related by the viewer. This ties back into my overall comment that the reader needs more background on the commonly used techniques in many of your cited studies to assess what each proxy actually records under the umbrella of “rejuvenation of tectonic activities”.

We have provided the tables in the manuscript. The tables provided detailed events, ages, methods and references. Therefore, the reader can be obtained the proxy actually records via table 1, 2 and 3.

5. Figure 8: It would be interesting to see some of the additional proxies plotted in Figures 3, 5, and 6 throughout the span of the Cenozoic along with the benthic foraminifera oxygen isotopic composition. The oxygen curve in Figure 8 has been replicated and discussed in numerous studies since its original publication by Zachos, so it would be more intriguing to see how the other proxies change or do not change during the three pulses you attribute climate change and aridification to. Do pedogenic carbonate $\delta^{18}\text{O}$ and wt. % CaCO_3 also follow the benthic foraminifera $\delta^{18}\text{O}$ curve?

We chose additional oxygen isotopes at each interval instead of publication by Zachos (Fig 8) in Figure 3, 5, and 6 because these can provide higher resolution data.

The pedogenic carbonate $\delta^{18}\text{O}$ and wt. % CaCO_3 in Tibetan Basins are not coeval with the changes of the benthic foraminiferal $\delta^{18}\text{O}$ curve. There is a lag time about ~2 Myr. The significant decrease of benthic foraminiferal $\delta^{18}\text{O}$ curve occurred at ~13.9 Ma (Figure 5), but the significant increase or decrease of the pedogenic carbonate $\delta^{18}\text{O}$ and wt. % CaCO_3 , respectively, occurred at about ~12 Ma. This difference may indicate that another factor, such as tectonic uplift of marginal mountains in Tibet, plays a role in climatic changes.