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Interactive comment on "Precipitation δ^{18}O on the Himalaya-Tibet orogeny and its relationship to surface elevation" *by* Hong Shen and Christopher J. Poulsen

Anonymous Referee #2

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This manuscript uses a water-isotope enabled AGCM to explore the relationship between Himalaya-Tibet elevation and delta-18O in rainfall. Such studies are critical since stable isotope-based paleoaltimetry has been widely used in the last years to constrain the uplift history of orogens. The ms is clearly structured, mostly well-written and, despite some rare confusions & ambiguities, quite convincing. I think the ms could be published with some minor revisions that will improve the overall discussion.

The authors make use of sensitivity experiments to Tibet/Himalaya height to understand processes driving rainfall d18O. In that respect, this ms has a lot in common with a previous paper authors refer to, Botsyun et al. 2016. Results bring also a similar

message, i.e. that there are many processes determining the ultimate O18 composition of precipitation waters that stable-isotope-based paleoaltimetry do not account for. Despite these similarities, I think this ms is a very important contribution for the geosciences community. First, because comparable results obtained with different GCMs reinforce the previous findings by -obviously- making the overall message less model-dependent. Second because authors have added a 1-D model that helps explore the mechanisms sequentially, in a kind of complementary way.

Still the ms would benefit from a deeper discussion of the differences/similarities between these 2 studies, especially regarding 3 points : The role of relative humidity values for re-evaporation processes, the role of the "amount effect" and the sensitivity (or not) of the results to convective/large-scale precipitation partitioning in the models.

This latter point is the only "grey area" of the ms in my opinion, for which I think some clarifications are required. Specifically, in section 3.6, after quantifying the different sources of 18O mass fluxes, authors argue that the decrease in large-scale/convective rainfall ratio leads to Rayleigh distillation weakening in low-elevation scenarios (page 12). I agree with this statement, but I think the implications of convection regime on the isotopes should be better explained. This interpretation and subsequent discussion would actually benefit from clarifications about (i) how convection influence water isotopes in general, (ii) how ECHAMiso convective scheme deal with these processes, and (iii) how one can link that to the amount effect. I'd suggest these clarifications to be made as soon as the introduction. Some useful references for that could be Bony et al. (2008) and Risi et al. (2008) (JGR Atmospheres).

I also think the ms could be improved with a small sketch depicting the sequence of processes tested between sections 3.4 and 3.7.

A sort discussion about the use of fixed SSTs and the expected changes in results if a fully-coupled GCM was used (dynamic coupling between changes in elevation, SST responses and advection of moisture towards the area for example), although putative, would be interesting to inspire future studies.

Lastly, this subject is very active, and new papers have been out in the meantime of this ms submission. For example, I think the discussion would benefit from the recent synthesis published by Rugenstein Caves & Chamberlain earlier this month (!) in Earth Science Reviews.

I summarize in the following a few remarks and minor concerns I had with the ms, trying to follow its chronology.

Introduction. Line 23: Regarding the impact of mountains on biogeography, there's a recent contribtion by Antonelli et al. in Nature Geosciences that would be worth citing (Nature Geosciencevolume 11, pages718–725 (2018)). Line 25: Raymo et al. (1988) is kind of outdated to explain uplift/CO2 links, especially since the studies showing the impacts of organic carbon burial in this drawdown (see for exemple Galy et al., Nature volume 450, pages 407–410, 2007 and/or Maffre et al. EPSL, 2018.

Methods. Line 16: I am a bit puzzled: If a slab ocean is used, then SSTs are not prescribed, but should be calculated via atmospheric heat fluxes and prescribed ocean heat fluxes. Or am I missing something ?

Page 6. Line 14. I think this use slopes ratio to decipher the relative effects of altitude/latitude in low-elevation simulations is unclear. Please reformulate.

Results. General remark. Authors should b cautious about the way they present results : It is not always clear if one deal with JJA results or annual-mean.

I think section 3.3, i.e. ECHAM-iso validation, should come first in the results section. The map of simulated O18 and actual datapoints should be moved from the Supplemental Material back to the main text.

Section 3.1 needs rewriting: First of all, authors need to homogenize the units used for discussion of precipitation rates. Sometimes it's mm/d, in some other places it's mm/y. Example : Figure 3. It deals with JJA rainfall, and units are in mm/yr, which

is quite confusing. I recommend to switch all rainfall amount results to mm/day in the entire ms. Authors also use Fig. 3 to discuss wind reversals and changes in latitudinal rainfall patterns in lowered topography scenarios, but the way 850 hPa streamlines are designed + the poor choice of the colorbar (white threshold at 1800 mm/yr) make it impossible for the reader to check authors statements. Two suggestions to deal with this issue : 1/ Refocus the region over the region of interest, change to mm/d and rainfall colorbar/threshold. 2/ Define some lat/lon sections to show the actual wind reversals, or just add the zero-line of zonal/meridional wind component on each map. I am not convinced by Fig. 4 either. IM decreases from 28.5 m.s-1 to 23, which is a 20% decrease. With an improved Fig 3. & a sentence stating that there's this 20% decrease in IM, the message will be clearer and authors will save space for a figure.

The discussion about the Himalayas impact on IM dynamics (3.2, page 8-9) and the long-standing debate about air-mass isolations versus thermal contrasts initiated by Boos and colleagues is interesting, but might be more relevant in the discussion part.

Moisture source influence. Page 9 : lines 28-30 should be moved back to model validation section. Page 10 line 9 to 14. Figure 7-8 do not show that d18O values of RDM and ECHAM are "close in all elevations scenarios." Actually, the slopes are similar but d18O are systematically shifted to lower values. Tables tend to cancel this signal by averaging over a box, but authors should moderate their statement.

Page 10, line 16: should read table 1 instead of table 3 ?

Fig. 7-8: Why does RDMfixed_T scenario from Table 1 not appear on these figures ?

Figure 13: Seven lines, but only 5 legends.

line 16 : "of" missing. Page 12, line 2 : "O18-enriched".

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