Review of cp-2015-187

This manuscript uses an isotope-enabled GCM to understand how the many factors that influence rainfall isotopes shape the $\delta^{18}O$ of rainfall in the Himalaya-Tibet Plateau (HTP) region as topography changes. This is the first manuscript I have seen examining this topic in the HTP region although other papers have used a similar approach in S. America and elsewhere. This is an important topic to understand as it underpins the field of isotope paleoaltimetry and inferences of the topographic history of the HTP region. The paper is well written and informative, especially the author's decomposition of the isotope changes into different processes. The major comments I have relate to the mismatch between the modern and model $\delta^{18}O$ gradients on Tibet and the details of the decomposition method. I recommend acceptance after major revisions. I think the fundamental study and analysis approach is sound. However, additional explanation of the methodology and exploration of the limitations of the model are needed to make the paper useful to readers. I am also not sure their analysis will end with the same conclusions if different methods for decomposing the isotope signal are used (precip weighting climate values, non-linear adiabats that change with temperature etc.).

Major Comments

1. The methods for decomposing the rainfall isotope change into different components was difficult to follow. Additional details would greatly help the reader understand this process. I have a few specific notes about this below but encourage a more general re-thinking of this explanation to make it explicit how each of the terms are derived (particularly the partial differentials, reference values Rvo, To etc.).
    a. 1a. It is not directly clear how the different processes that contribute to rainfall isotope change are actually calculated. For example, how is dRvi (Eq 3) actually calculated? What region is used to define Rvi and Rvo so as to calculate dRvi? Likewise, where is q0 determined (eq 4), T0 (eq. 6) etc. A much more detailed explanation is needed I think. How is the change in elevation contribution to rainfall isotopes calculated (it depends upon dz times lapse rate but also depends upon the Rayleigh distillation, saturation relationship to T). What is the actual analytical term for the partial differential in Eq. 8?
    b. 1b. Furthermore, it is not abundantly clear how the partial differentials in eq. 8 were evaluated. For example, dRp/dRvi (1st partial differential in Eq. 8) depends upon f, etc. (e.g. change in rainfall Rp depends upon initial vapor composition plus modification by isotope fractionation, the magnitude in per-mil also depends upon the initial vapor composition). All of these partial differentials need to be more explicitly evaluated analytically in the paper and the dependence on the sensitivity of the partial differentials to the state at which they are calculated has to be demonstrated as not a factor (as is assumed on p7 L5).
    c. 1c. Decomposition of the absolute humidity term into temperature and relative humidity means that attribution of $\delta^{18}O$ changes to Ts or rh changes depends upon how well the model really captures those two values. Another, and more fundamental way to partition the rainfall $\delta^{18}O$ changes would be to determine how much the specific humidity changes (or T*) contribute to the isotope changes. This gets at the

transport and rainout process more directly. The local T and rh values are not really the source of the isotope changes, T* or q/qo (q[T*]/qo) are the actual cause of the changes (e.g. eq. 4). I would much rather the authors use T* or q/qo changes in their decomposition of the isotope changes (or at least do this in parallel to the T, rh decomposition as they are equivalent). In equation 8 this would combine the Dh and DdTs terms into a single q or T* term. Intuitively this makes more sense: Rp is a function of Rvi, epsilon, and q.

d. 1d. The dz term in Eq. 8 is really a temperature change (or T* change as is assumed in a Rowley type model - actually a Pierrehumbert-type model: Pierrehumbert, 1999 Huascaran d18O as an indicator of tropical climate during the Last Glacial Maximum). So, the q term could be decomposed into an elevation term and a non-elevation term. This would still keep the decomposition focused on the fundamental q/qo term rather than Ts and rh.

e. 1e. Are the values used to decompose the rainfall isotope changes precipitation weighted? That is, are q, T etc. weighted by the precipitation amount in a particular location? One of the reasons that there is such a strong relationship between isotopes and elevation is that the relationship is set only when it rains. This is a very small subset of all atmospheric conditions and thus much of the variability really doesn't matter. It only matters if it is actually raining and the isotope signal is being "sampled." In my opinion, rainfall weighted climate variables are essential for properly decomposing the isotope signal.

2. I think there are major and important differences in the modeled and observed rainfall isotopes (e.g. Fig. 7). This is particularly true for the northern plateau where isotope values in rainfall become so positive that they are nearly identical to low elevation rainfall south of the Himalaya. I disagree with the author's statement that these highly enriched isotope values are from surface processes (p9 l16-20). Bershaw et al, 2012 were discussing the Pamir region to the west and even in their data there is not evidence of non-equilibrium fractionation as would be expected from kinetic effects. Overall there is not a strong d-excess signal on the plateau as would be expected for evaporative processes thus the data indicate a robust positive isotope signal in rainfall values.

This feature, its interpretation, and whether and why it persists in the past are perhaps some of the major questions in Tibetan paleoaltimetry. If it is a persistent feature then ancient isotope values from central and northern Tibet that look like today's values may have come from modern-like elevations. If this is not a persistent feature (e.g. an arm of the Tethys north of Tibet would provide local moisture) then ancient isotope values that are the same as today may actually mean the site was at a low elevation.

My recommendations for this issue are twofold. First, I would like to see two scatterplots in addition to the heat map. The first would be the observed vs. modeled $\delta^{18}O$ rainfall from Fig. 7A along with RMSE estimates. This plot would show how well the model really captures isotope values regardless of location. The second would have latitude as an x-axis and actual observations of oxygen isotope values as the y axis along with values from the model as a continuous line. Values could be from a swath beginning at the south and extending north along the central axis of the plateau or projected in from the plateau. This plot would show how well the model gets the overall isotope gradient even if it doesn't get the absolute values correctly.

Second would be a thorough discussion of what this mismatch means for interpretation of the model experiments. If the model is missing or underrepresents some moisture source or process that is important today on the northern plateau then what does this mean for the conclusions from the model experiments?

3. Cite the original sources for precipitation isotope values on the plateau. These authors should get credit for the major amount of work it takes to generate this type of data and all the credit shouldn't go to the Caves 2015 compilation.
4. One of the major conclusions of the paper is that there is a non-linear effect of elevation changes on isotope values. One expects a non-linear relationship between rainfall isotope values and elevation simply because of the non-linearity of (i) saturated adiabats, (ii) the saturation vapor pressure curve with temperature and (iii) the Rayleigh distillation process itself. Thus the null hypothesis is that isotope changes with elevation from low to intermediate elevations would be less than isotope changes from intermediate to high elevations. Whether the changes are greater than can be explained by the null hypothesis needs to be demonstrated. But, the qualitative observation itself is actually expected from theory. An additional plot would drive this home. What does a Rowley (Pierrehumbert) type model predict for isotope change with elevation and where do these GCM models plot? I would focus this plot on the Himalayan mountain region as this is where the simple model is most applicable. This would be an incredibly useful plot for folks that want to take lessons away from this paper. How similar/different are the results in this paper from a simple model that has been extensively used to reconstruct elevation?
5. There is a bit of a cottage industry in the isotope-enabled GCM field looking at how isotope paleoaltimetry does/doesn't work in different orogenic systems. I would encourage the authors not to fall into the trap of saying "its complicated and you need to take additional factors into account." (This is essentially what is said at the end of the abstract.) Rather, make the information accessible and useful to the readers. Be specific and helpful in the abstract and throughout so that it is directly clear to the readers what specific factors are actually important and how they should be accounted for when reconstructing paleoaltimetry.

Specific Comments

P2 L16-18 – Not all of these references are carbonates or oxygen isotopes

P3 L4 – most studies do take into account changing seawater $\delta^{18}O$ either implicitly through normalization to a low elevation rainfall site or explicitly through correction using various estimates.

P3 L21 – Seems that studies of Ramstein and Fluteau should be mentioned here.

P7 L5 – Is the assumption that sensitivity of the partial derivatives to state is not important ok? It seems this would be fairly easy to test by some simple calculations and then it could be definitively stated.

P11 L1 – Are the Ddts values precipitation weighted? In general are the climatic variables used in the decomposition precipitation weighted? They should be.

P11 L7-10 – How much of the temperature changes are due to comparison to a constant adiabat for all experiments? The adiabats that matter are only ones when moisture is being transported on to the plateau (non-linear with elevation) and the slope should change with T and qo. Thus, inference of non-adiabatic temperature changes could simply reflect the way this is calculated and not actual changes.

P12 L28-30 – One expects a non-linear relationship between rainfall isotope values and elevation simply because of the non-linearity of (i) saturated adiabats, (ii) the saturation vapor pressure curve with temperature and (iii) the Rayleigh distillation process itself. Thus the null hypothesis is that isotope changes with elevation from low to intermediate elevations would be less than isotope changes from intermediate to high elevations. Whether the changes are greater than can be explained by the null hypothesis needs to be demonstrated. But, the qualitative observation itself is actually expected from theory.

P14 L1 – Effects from post-condensation re-evaporation. This should have a distinct d-excess signal that should be evident in the model values. Examination of the d-excess signal spatially could directly answer this question.

P15 L1 – How do these results compare with those of Boos and Kuang (2010)?

P17 L15-16 – "Paleoelevation studies indicate the Himalayas attained their current elevation by the late Miocene." This is not correct. Rowley and Currie (2006) and subsequent authors indicate earlier timing for modern elevations (middle Eocene or earlier).