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Interactive comment on "Enhanced Trans-Himalaya Pollution Transport to the Tibetan Plateau by the Cut-off Low System" *by* Ruixiong Zhang et al.

Anonymous Referee #2

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The paper by Zhang et al. deals with the transport of pollution from the Indo-Gangetic Plain (IGP) to the Tibetan Plateau. The Authors provide a brief review of the experimental findings from in situ observations. Then, they present the results of a regional transport model in simulating the concentrations of anthropogenic aromatic hydrocarbons (HCs) measured in Tibet in Oct 2010. Finally, they present an approach for correcting possible biases in current emission inventories, and highlight possible problems with state-of-the-art models in simulating transport events associated with specific meteorological conditions. The paper is a nice, clear exercise of observation-constrained modeling. I have one major comment about the impact of this study and two additional specific comments.

First of all, the Authors claim that their analysis has implications for improving the modelling of black carbon (BC) transport to the glaciated regions of Tibet (all the Introduction is dedicated to this topic). However, their approach is based on measurements (in situ and satellite retrievals) of aromatic HCs and of their degradation products (glyoxal). It is certainly true that aromatic hydrocarbons share with BC several emission and transport patterns, but only to a certain extent. For instance, the aromatic HCs are emitted by fossil fuel combustion, gasoline evaporation and solvent use (page 2, line 24), however only the first of these three sectors is of importance for BC. It follows that top-down methods for correcting the emissions of aromatic HCs (Section 2.4) has unclear implications for improving the representation of BC sources in the models. If the scope of the paper is really improving BC modelling in the Himalayan-Tibetan region, then the absence of BC observations poses a major caveat, even if the approach is conceptually valid and in principle it could be extended to experiments involving real BC measurements.

Specific comments:

a. Biomass burning is ruled out from the possible explanations for the difference between observed and retrieved glyoxal concentrations over the IGP, because satellite fire counts show only spot fire occurrence over the Plain with little correspondence with the model-measurement gap (Page 5, lines 10 – 14). However, open burning accounts for only a fraction of biomass burning, which is normally practiced also indoor for cooking, heating etc., undetected by remote sensing. Therefore, I would not rule out the hypothesis of a direct emission of glyoxal from domestic biomass burning.

b. The Authors find a plausible explanation for the rise of aromatic HCs concentrations between 22 and 24 Oct 2010 in the synoptic meteorological conditions in central Asia showing an upper-level cut-off system triggering a southerly circulation from India to Tibet. However, minding that BC can be removed during transport by precipitations, the Authors should provide a more in-depth analysis of the meteorological conditions over the Himalayans during the approach of the low-pressure system. Ap-

parently, on the 22 of October, frontal cloud systems travelled over the Tibet from west to east (http://www.ssec.wisc.edu/data/comp/ir/2010295M0000.gif). The presence of precipitations with possible losses of BC (and not necessarily of aromatic HCs) in the Himalayas should be checked carefully at local meteorological stations.

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