Atmos. Chem. Phys. Discuss., 15, C365–C369, 2015
www.atmos-chem-phys-discuss.net/15/C365/2015/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Quantifying sources, transport, deposition and radiative forcing of black carbon over the Himalayas and Tibetan Plateau" *by* R. Zhang et al.

Anonymous Referee #2

Received and published: 23 February 2015

Summary and General Comments

The results provide an interesting way to view the relatively pristine HTP that is embedded among major carbon emissions hot spots. My comments reflect a general critique that the authors do not sufficiently motivate the finer points of the discussion. The most confusing points are related to efficiency and finer source-receptor relationships within the HTP, and I think these need clarification before the paper should be published. Otherwise, there are some very interesting emissions impacts results on a very sensitive part of the world (ie. the Third Pole). Figure 4 is fascinating!

There is some repetition in the first 200 lines of text that I would recommend streamlining. One way to do this would be to clarify why some of the studies are mentioned in this study. Bring their relevance to the foreground.

I have some issues trying to understand the utility of the efficiency metric that I think may require some further discussion by the authors before the paper should be published. My recommendation is that the entire efficiency discussion be deleted – it seems underdeveloped and seems to not support the main points of the paper. I also note, however, that I may have misunderstood the calculation, but either way, I requested a direct response to this.

Also, the source-receptor relationships within the HTP are interesting, but I do not understand why I am reading about them.

Specific Comments

Line 100: Citation for sentence starting with "A large fraction. . ."? Could this be better quantified to say approximately what fraction?

Line 102: "Road map" is an odd choice of words for this study. Perhaps just be more specific and less flowery about what makes some scientists think that BC mitigation is a low-hanging fruit.

Line 159: Repetitive with line 117. I recommend deleting one and adding to the one that remains in the text a preview of why this study is highlighted in your study (ie. as a basis of comparison about source regions of BC as cited later in the manuscript).

Line 165: What do you mean by "different inventories" since I'm only aware of the CMIP5 inventories, and this is the only one cited?

Line 173-174: I don't understand. Why is a ratio of biofuel to fossil fuel needed? Doesn't Lamarque et al (2010) emissions include a biofuel category? If not, maybe simply stating that this is why a ratio is needed would be clearer.

Line 200, 203: Does mass mixing ratio, deposition flux, surface mixing ratio as a BC property produce similar results as using column burden? It seems like MMR C value would be much different than column burden, unless this is a z-dependent C calculation. Either way, and similar to other comments, introducing this myriad of metrics is interesting, but it would be helpful to clarify why they are all needed. For example, are C values for MMR and SMR even discussed in this study? From Fig 6, I see deposition and column burden C values.

Line 261: What emission uncertainties? Are these quantified in a peer-reviewed source?

Line 285: Can this improvement for HTP be quantified in some way? The discussion around CAM5 simulated SCF and MODIS SCF uncertainty is muddled and missing a simpler metric of comparison. For example, the average correlation of the SCF in the study area for CAM5 vs MODIS 2001 and MODIS 2000-2013 should be illustrative. What is the average SCF in each season for the different comparisons? Can the improvement from CAM3 to CAM5 be better quantified beyond the citation to Qian et al. 2011? As it is, it's not very convincing to read about dramatic improvements without a number.

Line 294: What is the mean SCF? This is never stated, and "very close" is too vague.

Line 306, Figure 4: This is a very complex graphic, but I think very useful, provided some revisions are made to both the figure and the manuscript text. The figure either should be larger or broken into multiple parts. I think larger would work well given that the orientation of the figure currently does a great job with side-by-side comparisons of source regions impacts on HTP as a function of two seasons. The wind vectors right now are very challenging to read, but maybe a larger figure solves this? Also, somewhere, it should be stated why these 6 source regions were selected from the 16 on Fig 1. This is mentioned on Line 306 ("six major source regions") but I would think that RBU would have some seasonal impact, especially during JJA when fire activity is

high. I do not have the supplemental figures in my version of the manuscript, so I could not judge this.

Line 328: This caveat (no seasonality in FF) should be discussed when talking about what FF means in Section 2.2.

Line 334-337: This text would benefit from referencing your own Fig. 4

Line 377, Section 4.3: This is a key section, but missing from the discussion as to why HTP is broken into multiple source regions. I expected to see this discussion before the conclusion (see my comment line 578) but nothing appeared. To me, the more interesting points of this study are the source-receptor analysis of HTP as a whole. Why should I be interested in more detail? What are the ramifications?

Line 459: Perhaps I have misunderstood this efficiency metric, but here is where I stand on this and I would appreciate a defense/clarification: Section 4.4 seems unnecessary to support the main points of the study. I do not see how this efficiency metric is of much use, especially for HTP. HTP has practically no emissions and I do not see how anyone could practically expect HTP to develop major emission sources. SAS and EAS have enormous emissions. To standardize to present day emissions seems to minimize the impact of the largest emitters on HTP at least. In other words, all this efficiency metric highlights is that the S values in line 207 are divided by a very very small number. To make a more effective efficiency metric, wouldn't all emissions have to be uniform across the globe and source region perturbations of equal magnitude be applied to study the actual efficiency at which an equivalent emissions increase would have on a receptor region? Again, pardon any misunderstanding if I have missed something.

Line 565: This is a very clear summary of what I think are the key results – nicely written!

Line 578: Similar to Line 377, can text be added to this paragraph clarifying why this regional receptor analysis is generally important? Otherwise, I would suggest eliminating

this paragraph from the Conclusions section.

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 77, 2015.