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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Quantifying sources of black carbon in Western North America using observationally based analysis and an emission tagging technique in the Community Atmosphere Model" *by* R. Zhang et al.

Anonymous Referee #2

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This paper concerns a tagging technique of black carbon (BC) emissions to study the source-receptor relationship for BC in the atmosphere and on snow in Western North America using CAM5. The model results are compared with observations in the region. As most models seems to underestimate BC near the surface at high latitudes, this paper is relevant and might be of great interest to the scientific community. The questions raised in the study is within the scope of ACP. The figures are in good quality and the figure captions explain the figures well. However, for the paper to be published in ACP, some revisions need to be done. The authors should work more on the overall

presentation of their results.

Specific comments:

1. In general, I think the paper is somewhat too long and unfocused. The paper would benefit from a substantial reduction both in the Methods chapter and also the Results and Discussion. An effort to focus those parts would make the paper much easier to read. You explain your methods well, but there are still parts in Methods that can be moved to Introduction or Supplementary. For instance, the second paragraph in 2.1 Observations you discuss what is new in this study and what previous studies have done. 2.1 should only describe the actual observations, and the rest can be skipped/compressed/moved to introduction. There are many equations in 2.3 and 2.4 that can be moved to Supplementary if necessary. Also, in the Results chapter, only results should be presented (and not repetition of Methods for instance). The whole Results chapter can be shortened for clarity.

2. The authors should explicitly state why this study is important. Also, what are the benefits of using this tagging technique instead of doing emissions perturbations? By focusing the paper and skipping parts that are not relevant, the new contribution would be easier to detect.

3. I am a bit confused why you separate out BF from FF and lump together with BB. The ECLIPSE emissions have different sectors compared to the previous ones from Dentener et al (2006). For instance flaring is included as a sector in ECLIPSE. There are no easy way to separate out BF, but you should at least discuss your assumptions further. What uncertainties do you introduce?

4. Klimont et al. is still in preparation the ECLIPSE emissions data set v4a, but in the meantime Stohl et al. 2015 should be a sufficient reference: http://www.atmos-chem-phys-discuss.net/15/15155/2015/acpd-15-15155-2015.html Here, the emissions are described in more detail.

5. In the results chapter; would it be an idea to not use the abbreviations for the source regions? This will make it easier to follow.

6. The radiative forcing sub chapter was unexpected. You have not mentioned this earlier in the paper. How is the forcing calculated? How did you estimate BC DRF in the atmosphere? As a difference between surface and TOA? How do you calculate the surface RF (dimming) compared to output from SNICAR? Also, you conclude that a positive forcing at the surface (?) means heating at the surface. This is not correct, and I would avoid writing this unless you have a fully coupled climate run. Whether BC warms the surface depend on the height of BC in the atmosphere. You also find a correlation between deposition and surface RF. What about the albedo? Solar radiation? You only look at the winter months. The section in its current form seems misplaced. I suggest to either expand the analysis, or to skip this section entirely.

7. I'm curious about the BC in soils. How do you find this in your own analysis? I am not sure if I understood this correctly. Since this is part of your conclusions, it should be elaborated a bit more I think.

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 12957, 2015.