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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Long-range transport of black carbon to the Pacific Ocean and its dependence on aging timescale" *by* J. Zhang et al.

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The authors investigated source attribution to the Pacific Ocean using a global chemical transport model MOZART-4 by tagging BC tracer to 13 source regions around the globe. They further quantified the aging timescales of those tagged BC tracers by constraining simulation with aircraft measurements from five HIPPO missions. This is a scientifically interesting study. Publications of AeroCom and other works have demonstrated that many global models currently overestimate BC in free and upper troposphere. This study points out a direction to solve this common problem in modeling global BC field. The paper is well written. I recommend publishing the paper on ACP after the authors make some minor modifications.

General Remarks: The authors summarized BC aging timescales associated to 13 source regions. The conclusion is instructive but may be not robust. The authors showed an improved BC simulation by MOZART-4 at current condition with this varying BC aging timescale. But can these aging timescales still be valuable if BC emission and atmospheric oxidant fields are changed in the future in MOZART-4? Can other models apply these adjusted BC aging timescales with some cautions? It may be more useful that the authors explore the key factors that control BC aging, such as emission types, oxidant fields, etc, and parameterize BC aging timescale based on these key factors.

Specific comments: 1. Page 16946 line 22-24: How fast is the aging rate so that the lifetime of BC is dominated by factors that control its local deposition? 2. Page 16946 line 27 – page 16947 line 1: This sentence repeats the first sentence of abstract. 3. Page 16949 line 4: How thick must coating be for a hydrophobic BC to be named as a hydrophilic BC? 4. Page 16954 line 20: It may be good to clarify the terms of "aging", "aging rate", and "aging timescale" used in the paper. 5. Page 16954 line 25-28: Why does biomass burning BC have a larger fraction of coated particles and thicker coatings than urban BC? This seems to conflict with the discussion in section 2.1 (page 16949 line 14-24) that indicates that urban pollutions (i.e. sulfate, nitrate, ozone, and nitrogen oxide) are primary components in coating. Also the short aging timescale of East Asia BC summarized by the authors seems to not support this statement either. 6. Page 16955 equation 3: How about longitude bins? 7. Page 16956 line 27 and Page 16957 line 1-2: Why is the aging of these urban polluted regions vary fast? Is it faster than that of biomass burning dominated regions such as South America, Africa? I am confused since it is not consistent with the observation facts discussed in section 2.5 (see specific comments 5 above). 8. Page 16958 line 12-14: How sensitive is the summarized BC aging timescale to a change in precipitation? In other words, what is the potential uncertainty of aging timescale in response to a potential bias in precipitation predicted by MOZART-4? 9. Figure 5: It is interesting to know that Africa emission is not a dominant contributor to most of the Pacific Ocean since it is the largest contributor to global BC (Page 16960 line 10-11) and Africa is closest to the Pacific Ocean.

Technique corrections: 1. Figure 4: Please change color of improved model results (green line and number) so that it is more distinct from the grey dots that represent measured BC concentrations.

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 16945, 2015.