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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.

Anonymous Referee #4

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This work conducts numerous sensitivity simulations using a global chemical transport model (MOZART-4) by changing the aging timescale of the tagged BC from various geographical source regions. The authors optimize the aging timescale of BC emitted from each source region by minimizing errors in vertical profiles of BC mass mixing ratios between the simulations and HIPPO aircraft measurements. They investigate the contributions of BC from each source region to BC loading over each receptor region, and examine relationship between lifetime of BC and its aging timescale.

This study is interesting and scientifically important. The subject is of great interest to ACP. For the most part, the manuscript is written clearly. However, the description and validity of the optimization approach in section 2.5 are not sufficient, which are cause for concern (see Major comments). Once these points are addressed satisfactory, the

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paper should in my opinion be suitable for publication in ACP.

Major comments:

My main concern is the validity of the optimization approach shown in section 2.5. In definition of the NMAE in equation (3), the authors normalize the absolute errors by the minimum of observed and modeled BC so that NMAE weights both high bias and low bias equally. However, I am not convinced that this approach can estimate the optimized aging timescale of BC.

For example, we consider one data point for simplicity in the following two cases: Case A: BCo=4, BCm=2.5; NMAE = 1.5/2.5 = 0.6 (greater NMAE). Case B: BCo=4, BCm = 6; NMAE = 2/4 = 0.5 (smaller NMAE). According to this approach, case B shows better model results because of the smaller NMAE, although I think that case A shows better agreement between the modeled and observed BC values.

I guess that modeled BC (BCm) in equation (3) is sum of the 13 tagged BC from each source region, and contributions of the tagged BC from each region to the total BC over the HIPPO regions are largely different. I think that variations of the BC mass mixing ratio due to changes in the aging timescale are greater for the tagged BC from the large-contributed source regions (e.g., East Asia) and smaller from the small-contributed regions. In this case, the approach may have limitations to estimate the optimized aging timescale for BC from the small-contributed regions, because the total BC mixing ratio is dominated by BC from the large-contributed regions. According to Table 1, the optimized aging timescales are 4 hours for BC from the large-contributed source regions (page 16959, lines 4-5) and are greater values (120-200 hours) from the small-contributed regions.

If I am misunderstanding the optimization approach, the authors should clarify their approach and discuss the validity of their approach. If possible, in addition to the present approach, it would be better to optimize the BC aging rate by other conventional approaches (e.g., mean normalized gross error, normalized mean error for each altitude

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range, taylor diagram, etc.) and evaluate the statistical values in a comprehensive manner, which could give the confidence in the validity of the estimates. If these evaluations are difficult, the authors should at least address limitations of their approach and discuss possible errors included in their results.

The authors chose the aging timescale between 4 hours and 200 hours in section 2.5. On the other hand, Table 1 shows that the best-fit aging timescales are 4 hours and 200 hours in many cases (minimum or maximum). I believe that conclusions of this paper (e.g., faster aging over the anthropogenic source regions and slower aging over high latitude regions) will not be changed even if the authors expand the range of the timescale, however the authors need to discuss the validity of their estimation.

The authors should improve the description of the optimization approach in section 2.5. Please clarify how the 13 tagged BC are used in equation (3). Please describe the time information for the modeled and observed BC. Are these compared the same time? Please indicate time resolution of the observation data and model output (hourly, daily, monthly?) used in equation (3).

Additional comments:

Page 16951, line 14, "with 28 vertical levels": Please indicate the top boundary of the model.

Page 16951, lines 15-16: Is MACCity inventory (Lamarque et al., 2010) used for the CMIP5 project?

Page 16951, lines 9-10: Does "C" include ice content?

Page 16954, lines 12-13: Please show a few references at the end of the sentence.

Page 16954, line 28: Schwarz et al. (2008"b") is not shown in the reference list.

Page 16958, lines 6, Figure S1: Typo? Figure S3 in the present manuscript?

Page 16958, lines 14-21: In the Southern Hemisphere during HIPPO5 (Figure 4), the

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improved BC (green) is smaller than the observed BC (black), however the original BC (red) is greater than the observed BC. If the model does not capture abrupt biomass burning emission events, the original BC would also be smaller than the observed BC. I am not satisfied with the author's explanation, because differences in modeled BC values (green and red) are caused mainly by the wet deposition of BC.

Equation (3) and Table 1, terminology: "Normalized mean absolute error (NMAE)" should be "mean normalized absolute error"? "Normalized mean bias (NMB)" should be "mean normalized bias" or "mean fractional bias"?

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