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Interactive Comment

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 #2

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This paper presents a global modeling analysis to constrain the first-order aging timescale of black carbon based on observations from the HIPPO campaign. The analysis involves performing several sensitivity studies where the aging time scale was varied, with the BC tracer tagged according to different geographic source regions. Optimal aging time scales for each source region are then found by minimizing the error between simulated BC mixing ratios and HIPPO observations. The tagging of BC also allows quantifying the contribution of BC from different source regions to various receptor regions, including the Pacific Ocean, which is an area of interest as BC over this area is suspected to have significant climate impacts.

This is an interesting paper, which makes innovative use of HIPPO data. It supports previous studies that found that the first-order aging time scale that is used in many global models should not be a fixed value, but should depend on local conditions. The paper fits well into the scope of ACP and is for the most part well-written. The discussion in Section 5 is very instructive. My main comments concern the optimization procedure in Section 2.5. I hope the reviewers can resolve these in a revised version.

Major Comments:

The description of the optimization procedure in Section 2.5 needs to be improved (line 22 – 25). It sounds like the authors performed 13 simulations, with constant aging time scale for each of these simulations. It is not clear how the area-specific aging time scales are obtained from these 13 simulations. I believe that the constraint is used that $\mathrm{BC}(i,j,t) = \sum_{k=1}^{n_\mathrm{source}} \mathrm{BC}(i,j,t,k)$, and then $\mathrm{BC}(i,j,t)$ is reconstructed using all possible recombinations of $\mathrm{BC}(i,j,t,k)$ from the 13 sensitivity runs. Finally, it is checked which $\mathrm{BC}(i,j,t)$ best matches the observations. Please clarify this procedure.

If this is what happens, my main concern is how stable the procedure is, i. e., given the large number of permutations to calculate candidate $\mathrm{BC}(i,j,t)$ values, it could happen that many different combinations of $\mathrm{BC}(i,j,t,k)$ lead to a similarly small error, and that the authors are fitting noise. One way to check this would be to use a testing and a training set, which might not be possible given the limited amount of observations. Another way to check this would be to visually inspect plots where the error is graphed as a function of parameter that is varied, keeping all other parameters constant. It should be very obvious if these curves look sufficiently smooth so that a robust minimum can be identified.

The values listed in Table 2 do look questionable: two thirds of these values are either 4 or 200, which are the minimum and maximum values in the set of aging time scales used for the sensitivity runs. This could mean that the range of aging time scales chosen was not large enough. The authors discuss the physical interpretation of the optimized aging time scales on page 16957, but there are several examples that are hard to interpret. For example, for SU the time scale is 200 h for June and 4 h for

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August. Several other examples along this line can be found.

Minor comments:

1. Rearrange the order of the columns in table 1 and 2, so that they show the source regions in the same order, to facilitate the comparison of the two tables for the reader.

2. Section 2.2: How do you know that using the updated dry and wet deposition schemes results in an improvement of the model performance?

3. p. 16953, line 27: “approximately equal”: Please quantify this statement.

4. “Normalized mean absolute error” should be “Mean normalized absolute error”

5. In equation (3), are the simulated and observed BC values taken at the same time?

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