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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "Analysis of transpacific transport of black carbon during HIPPO-3: implications for black carbon aging" *by* Z. Shen et al.

Anonymous Referee #1

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This study uses springtime HIPPO flights across the Pacific to test the GEOS-Chem simulation of BC and aging/transport processes. The manuscript is straight-forward and clearly presented. I have a few major comments which should be addressed prior to publication.

1. Page 509, lines 25-29: You mention the importance of dry deposition in the Introduction but never discuss the dry deposition flux simulated with GEOS-Chem in the text. Is the dry deposition of BC high or low compared to other studies? Could near-field dry deposition processes impact your simulation?

2. Page 517: The high BC concentrations aloft in April are surprising, given that the authors rule out biomass burning. Could you speculate as to the cause? Is this evidence of lofting or drier export? Is there a mechanism that you could use in the model to reproduce this signature at ∼6km?

3. I'm a little unclear as to what we learn from the analysis of Section 5.2. Why did the authors assume N=5? How do the results change if you change N? Figure 5 shows that changing the aging rate does not significantly improve the model ability to capture the vertical profile (just shifts it). Did the authors consider if there might be errors in the cloud top heights or vertical distribution of precipitation? Or rain rate? Or type of event? Your paper shows that neither modifications to the emissions nor aging rate can substantially improve the model simulation, so I'm left with the question of why this simulation performs so poorly compared to these observations.

MINOR

1. The HIPPO flight tracks are not shown in any figure. These should be included somewhere so that the reader has a better idea of the domain of the observations.

2. Abstract, line 15: uncertainties in removal as well as transport?

3. Page 512, line 23: Please specify if the biomass burning emissions used are for 2010.

4. Page 512/513: The adjoint model is based on an older version of GEOS-Chem. What inconsistencies does this introduce in the analysis?

5. Page 516, line 12 and line 22: The domain of Figure 1 extends beyond Asia (includes the Middle East). If the percentages listed in the figure are for the entire domain, they should not be cited in the text as representing "from Asia" or "combustion in China".

6. Page 516, line 17: How different are March and April in the GFED inventory used?

7. Page 518, lines 20-28: Your assumption of diagonal error covariances is also unre-

alistic and likely introduces substantial uncertainties in the inversion.

8. End of Section 5.1: What about co-emitted organics in biomass burning?

9. Section 5.2: The description of Figure 8b is confusing

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 505, 2014.