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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Applications of Lagrangian dispersion modeling to the analysis of changes in the specific absorption of elemental carbon" *by* J. C. Doran et al.

D. Baumgardner (Referee)

darrel@servidor.unam.mx

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This study incorporates a mesoscale transport model to try and connect the dots between a major source of elemental carbon, Mexico City, and regions downwind (T0 and T1) with the objective of relating the specific absorption of these particles to their age. The paper is very well written, understandable and concise. The approach is reasonable and the results are consistent with the physical mechanisms of how carbon particles age and become internally mixed.

There are three comments/recommendations that I would like to make. The first is that I believe it would be useful to include in the analysis a measurement of a semi-

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conservative tracer to evaluate the dispersion of EC that is being forecast by the model. Carbon monoxide would be a good tracer since it does not oxidize quickly and could also be compared with the airborne measurements by the G-1. It would also be very useful for comparing the EC to CO relationships at T1 and t2 with that at T0. The EC to CO relationship is a relative measurement of the fraction of emissions produced by diesel versus gasoline. If the majority of the emissions are coming from Mexico City, this ratio should be approximately maintained. On the other hand, intrusion of biomass emissions or other sources more local to T1 or T2 will shift this slope. For example, in our measurements at the Cortez Pass, we see that the EC/CO slope is steeper when air comes from the Puebla valley as compared to a more shallow slope when air comes from Mexico City. This is a result of much more agricultural activity to the east with larger number of diesel vehicles with respect to gasoline engines as compared to Mexico City.

The second comment is that I would not expect the trends in specific absorption that we measured at our site at the university (Baumgardner et al., 2007) to be the same as measured at T0 and T1. Our values of around 5.5 m2/g are on the low end of those reported by Bond and Bergstrom, but still within their standard deviation. We measured very close to the source so that the only factor that changes the internal mixing of the EC, and hence its specific absorption, is a moderate increase during the day of secondary organics but the distance between the source and our measurement, approximately 300 m, is so short that there is little time for diffusional or condensational growth to occur. Hence, we see increases consistent with some change in mixing state but not as much as would be expected when there are several hours for processing to occur. This bring me to my last recommendation.

The recent work by Moteki and Kondo, published this year:

Moteki, N., Y. Kondo, Y. Miyazaki, N. Takegawa, Y. Komazaki, G. Kurata, T. Shirai, D. R. Blake, T. Miyakawa, and M. Koike (2007), Evolution of mixing state of black carbon particles: Aircraft measurements over the western Pacific in March 2004, Geophys.

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would be a good reference for comparison with the Mexico City study since the age of the EC downwind is similar to that evaluated downwind of Nagoya in the Japanese study. They are able to evaluate the mixing state of the EC directly and relate it to the age, although they don’t calculate specific absorption. I think it would round out the Mexico City work to include this reference.

Interactive comment on Atmos. Chem. Phys. Discuss., 7, 14989, 2007.