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Atmospheric Chemistry and Physics Discussions

Interactive comment on "On the diurnal variability of particle properties related to black carbon in Mexico City" *by* D. Baumgardner et al.

Anonymous Referee #2

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General comments:

This paper is quite interesting. In summary, the paper examines: (1) the nature and time evolution of the aerosol mass distribution, with specific emphasis on "black carbon" (BC), including such items as the BC mass fraction of the aerosols; (2) some chemistry "correlations" associated with the BC-containing aerosols, (BCA); and (3) the time evolution of scattering and optical properties of the BCA.

The paper is worthy of publication for two reasons. The first of these is that the paper highlights the use of the unique measurement capabilities of the SP2 instrument. This instrument allows the BCA properties to be measured – in the real atmosphere - on a particle-by-particle basis and on very short time scales. Even the thickness of the

coating that might surround a BC core can be estimated. Second, these measurements provide a means to examine aspects of aerosol evolution that might be difficult to measure using other techniques (i.e., the black carbon mass fraction as a function of time).

Still, I have a few problems with the paper that need to be addressed; see below.

Specific comments:

First, I am concerned about the optical and physical properties used for computations that involve BC. For example, the BC refractive index and density used in the paper are 1.75 - 0.44i and 2.24 g/mˆ2, respectively. Based on the recent analyses of Bond and Bergstrom (2006), these values are outside ranges thought to be realistic. As noted in the "Recommendations" section of Bond and Bergstrom, "The value commonly used by climate modelers (m = 1.75 -0.44i at 550 nm) represents none of the possible refractive indices and should be retired." Similarly, the BC density chosen for this study is far above that recommended by Bond and Bergstrom (1.7 to 1.9 g/mˆ3).

With these points in mind, I recommend that the calculations be redone using the more realistic values of BC refractive index and density. My hope is that this will be a relatively simple matter. I cannot tell whether the new computations would significantly alter the conclusions of the paper (for example, the conclusions drawn from Fig.1 will not be changed, but other conclusions, say from Fig. 7, might be altered significantly).

Second, this paper looks at aerosols only in the fine model. This is understandable because of the size measurement limitations of the SP2 instrument, as explained in the paper. Still, the coarse mode may have a significant BC content in the Mexico City Metropolitan Area. Johnson et al. (2005) show electron micrographs of BCAs that are relatively large ($>$ 1 um). As noted by Johnson et al., these aerosols are probably significantly aged, and the paper provides some justification for this assertion. In regards to the present study, some discussion for the neglect of the coarse mode is warranted. A suggestion: perhaps the close vicinity of the instruments to the primary

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sources of BC might imply a lack of aging, at least in the early morning hours. This would probably not be true as the day progresses.

Third, some computations in the paper rely on volume mixing of refractive indices. The issue of volume mixing is thoroughly discussed in Bond et al. (2006), in which they state that "theoretical calculations can predict absorption enhancement for volume-mixed particles that is not physically reasonable." I am left wondering if the calculations used in this paper should use a more realistic model of mixing, perhaps a shell/core model. Again, I cannot know in advance to what extent this would alter the conclusions of the paper, but I don't think volume averaging is justified. My hope is that the authors could easily start up a shell/core model and redo their results.

In summary, I think it would be a good idea to redo the calculations being mindful of the comments provided above. The conclusions can then be re-examined and publication can proceed – unless the new calculations uncover some insurmountable problem. I doubt that such problems will surface, but let's make sure.

Bond, T. C., and Bergstrom, R. W.: Light absorption by carbonaceous particles: An investigative review, Aerosol Science and Technology, 40, 27-67, 2006.

Bond, T. C., Habib, G., and Bergstrom, R. W., Limitations in the enhancement of visible light absorption due to mixing state, J. Geophys. Res., 111, D20211, doi:10.1029/2006JD007315. 2006.

Johnson, K. S., Zuberi, B., Molina, L. T., Molina, M. J., Iedema, M. J., Cowin, J. P., Gaspar, J. D., Wang, C., and Laskin, A.: Processing of soot in an urban environment: case study from the Mexico City Metropolitan Area, Atmos. Chem. Phys., 5, 3033-3043, 2005.

I like Figure 8! Way to go! I particularly like comparisons of the various quantities derived from either the SP2 or the nephelometer and/or PSAP.

Technical comments:

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Page 1626, line 6. Bond et al. (2006) do not make the claim that the extinction efficiency of BC is enhanced by coated particles. Rather they state that the absorption is enhanced. A little clarification is necessary here.

Page 1632, line 24. The h' after 24 is probably a typo.

Page 1640, line 24. Trivial typo. Semi-colon after T. C. - should be a comma.

Page 1648, Fig.5. The definition of mass density could be clarified in the text (page 1633, last paragraph).

Interactive comment on Atmos. Chem. Phys. Discuss., 7, 1623, 2007.