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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Estimation of the mass absorption cross section of the organic carbon component of aerosols in the Mexico City Metropolitan Area (MCMA)" *by* J. C. Barnard et al.

J. C. Barnard et al.

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We are pleased to respond to the comments provided by reviewer #1. For convenience, we have pasted the reviewer's comments in this response, so the reader can see these comments, and our responses, all in the same document.

Reviewer 1:

Review of Barnard et al. Estimation of the mass absorption cross section of the organic carbon component of aerosols in the Mexico City Metropolitan Area (MCMA) Barnard et al. present an analysis of radiometer data taken in the Mexico City Metropolitan area and determine the single scattering albedo and mass absorption cross section of the organic carbon component. The paper presents some very interesting results and

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should be published in Atmos. Chem. Phys. I have a couple general comments and some more specific comments.

General Comments

The technique used by the authors is to take the measured spectral downward solar fluxes at the surface, compute the flux while accounting for known radiative effects, and then attribute the difference to organic carbon. While this is a commonly used procedure, the authors seem to be a bit more confident of the results than I think is justified. The reason for my concern is that there are assumptions whose accuracy is difficult to determine. For example, assuming that black carbon has an exact 1/lambda relationship isn't justified. As a theoretical limit it is quite useful, but I doubt that in the Mexico City atmosphere the AAE value would be exactly 1.0. That being said, the honesty of the authors in Table 2 is quite refreshing and their results for OC are extremely interesting and quite believable. Perhaps they should comment on the existence of the "wiggles" in Figure 4 that may be a characteristic of the types of OC in Mexico City. The authors neglected to discuss the effect of ozone on their analysis. I would assume that there is plenty of ozone in the Mexico City atmosphere and they should indicate what concentration they used for their radiative transfer model and if they used the same scale height as for NO2 and SO2. Also it would be useful if they mentioned what the maximum optical depths due to NO2, SO2 and O3 were so that the reader can get a sense of the importance of including these gases.

[reply] In regards to the 1/lambda "law", the reviewer is right in assuming that it might not work well for realistically sized atmospheric aerosols. As shown in Bohren and Huffmann (1983), the 1/lambda relationship is valid in the limit of very small particles (Rayleigh regime). However, there is evidence that this expression is also approximately valid for larger particles. For example, Bergstrom et al. (2002) show that the 1/lambda relationship works quite well for aerosols collected during the TARFOX field campaign. Kirchstetter also finds that 1/lambda describes the absorption variation with wavelength for traffic aerosols. Here's some text that we've added in section 2.4:

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"However, several studies (Kirchstetter et al., 2004; Bergstrom et al., 2002, and references therein) have shown that it holds approximately for realistically sized aerosols in locations where BC absorption is thought to be dominant. For example, Fig. 1 in Kirchstetter et al. shows a very good fit of the "normalized light attenuation" to a lambdaˆ-1 relationship in a wavelength range of 330 nm to 1000 nm (the range in which the attenuation measurements were made)."

At this stage, we are reluctant to attribute the wiggles seen in the MAC data (Figure 4) to the type of OC in the Mexico City area.

For these calculations, we assumed a climatological value of ozone of 275 DU, using ozone data from the TOMS instrument for April 2003. (http://jwocky.gsfc.nasa.gov/). There could be a lot of ozone in the Mexico City atmosphere, but still most of the ozone, and ozone absorption, resides in the stratosphere. For example, a surface concentration of 120 ppb (a large surface concentration for Mexico City, see Lei et al., ACP, 2007) amounts to about 6 DU, assuming a scale height of 700 meters. This is less than 2% of the total columnar ozone. Therefore fluctuations in surface ozone concentration have no significant bearing on the outcome of our calculations.

Ozone absorption is accounted for in our calculations for the all the important ozone absorption bands, the Chappius band and the Hartley-Higgins bands, as summarized by Goody and Yung (1989).

We have noted this in the text, section 2.2:

"The raw tau-ext values also were corrected from ozone absorption in the Chappius and Hartley-Higgins bands (Goody and Yung, 1989), assuming a columnar ozone value of 275 DU, a climatological value for the atmospheric column in April 2003 obtained from the satellite borne Total Ozone Mapping Spectrometer (TOMS) instrument (http://jwocky.gsfc.nasa.gov). Over all cases considered here, maximum optical thicknesses for these absorbing species are 1.67, 0.03, and 0.04, for ozone (303 nm), NO2 (400 nm), and SO2 (300 nm), respectively."

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Specific comments Abstract P 10190 line 8/9 "can not" and "only possible" is a bit strong. Perhaps "most likely" is more appropriate

[reply] This suggested change seems reasonable. We have changed the wording from "only possible" to "most likely".

Methodology P 10196 line 8 should there be a period instead of a colon?

[reply] We corrected this typo.

P 10196 Coddington et al. (2008) discuss the surface albedo for Mexico City. The paper is now accepted and should be referenced.

[reply] We agree. We have added the Coddington reference and added text that indicates Coddington's albedo values are close to ours. Here's some text added to the revised paper, section 2.1:

"Our albedo values compare favorably to those found by Coddington et al. (2008) during the MILAGRO campaign. They used upward and downward irradiances from the Solar Spectral Flux Radiometer (SSFR) deployed on an aircraft to find the flight level albedo, which was extrapolated to the surface using a radiative transfer model, similar to the method applied to the G-1 data. For the portion of the flights directly over Mexico City, and for the two days shown in Fig. 4a of Coddington et al., the values of the surface albedo are, for example, 0.10 and 0.12 at 500 nm. Our value at this wavelength is 0.10, exhibiting good agreement. At 870 nm, the difference between our albedo and Coddington et al.'s is 0.015."

P 10197 line 17 If the retrievals are using the same technique, the fact that the results using two instruments are the same indicates that the instruments agree with each other. It's not an independent check of the retrieval method.

[reply] The reviewer makes a good point, however, after looking at the text in question, we believe that we have not made the claim that the good agreement is an independent check of the retrieval method. Also, we did not use the same retrievals techniques for

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the SR and MFRSR.

Because we did not use the same retrieval methods to find SSA from the SR (method 1 - uses TUV4.4 with SR actinic flux) and from the MFRSR (method 2 - method of Kassianov et al. that uses MFRSR irradiances), there's a question to about how the two different retrieval methods would perform when applied to the same data. We examined this question by adapting method 1 (now called modified method 1) so that it could be applied to the MFRSR irradiance data. Slight differences were noted between SSA from the modified method 1 and method 2. We have indicated this in the text, section 2.1:

"The actinic fluxes from the SR were inverted to find SSA using a different method, based on the TUV4.4 model. Goering et al. (2005) and Michalsky and Kiedron (2008) describe the use of this model applied to surface irradiance measurements to find SSA from either UV-MFRSR measurements or Rotating Shadowband Spectrometer measurements. The method we employ here is slightly different than theirs because we use the TUV4.4 with actinic flux, not irradiance, to find SSA. To compare the Kassianov et al. and TUV4.4 methods, we applied the TUV4.4 methodology to the MFRSR irradiance data to infer SSA and then compared these values to those obtained from the technique of Kassianov et al. (2007). The largest difference was 0.02, although typical differences are 0.01 or less."

P 10199 line 8 - diel should be diurnal? line 11 - OA should be OC? [Figure 3 has OA also]

[reply] (forgive our nitpicking) "Diel" is defined as "involving a 24 hour period that usually includes the day and adjoining night". Some of our colleagues think that "diel" is a better adjective than "diurnal" when describing a daily cycle that also includes the night. So we are letting "diel" stand.

Table 1 The table is a bit confusing in that the Angstrom exponent and the AAE values are not clear. It took me a while to realize that the first number is the Angstrom exponent

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for the extinction and the last two numbers are the AAE values for the two ranges quoted. This should be made more clear.

[reply] Reviewer #2 also could not understand the AAE values in this paper. To correct this problem, we have put the AAE values in a new table, new Table 2;, which is much easier to understand. We have more to say about this correction in our response to reveiwer #2.

Interactive comment on Atmos. Chem. Phys. Discuss., 8, 10189, 2008.