

***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.**

Anonymous Referee #1

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

General Comments

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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.

Specific comments

Abstract

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

Methodology

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

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

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.

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

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

References

Coddington, O., K. S. Schmidt, P. Pilewskie, W. J. Gore, R. W. Bergstrom, M. Román, J. Redemann, P. B. Russell, J. Liu, and C. C. Schaaf (2008), Aircraft measurements of spectral surface albedo and its consistency with ground-based and space-borne observations, *J. Geophys. Res.*, doi:10.1029/2008JD010089, in press. [PDF] (accepted 29 May 2008). available at <http://www.agu.org/contents/journals/ViewPapersInPress.do?journalCode=JD#id2008JD010089>

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