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Interactive comment on "Measurements of Black Carbon Specific Absorption in the Mexico City Metropolitan Area during the MCMA 2003 Field Campaign" *by* J. C. Barnard et al.

Anonymous Referee #3

Received and published: 18 July 2005

General Comments

The paper addresses the important issue of estimating the specific absorption of black carbon (BC) and its amount within an atmospheric column for a site in Mexico City. Overall, the paper is interesting, contributes to the field, and should be published. Generally, the text is well written and easy to follow. To support the conclusions reached, I believe that some aspects the analysis uncertainties should be better quantified before publication, as discussed below.

Specific Comments

Two methods (I and II) are used to estimate the specific absorption within the atmospheric column.

1) Regarding Method I

a) The assumption is made that the surface BC value may be extrapolated throughout the height of the aerosol mixing layer, assuming uniform mixing. It is later discussed that this is probably an overestimation, and might be responsible for the 20% bias relative to Method II (a negative bias in specific absorption, a positive bias in BC column amount). The question is: might the estimate of the aerosol concentration aloft be improved by using the intensity of the lidar backscatter signal to scale the surface concentration with height? It would be making the assumption that the aerosols that dominate the lidar return are uniformly mixed with the BC, but this might be a more gentle assumption than assuming uniformity of BC within the aerosol mixing layer. (Of course, if the lidar backscatter is uniform with height, then the result would not change.)

b) Also, can an estimated uncertainty be given for Method I?

2) Regarding Method II, the determination of M_BC involves some assumptions that should be quantified in the method's uncertainty. M_BC is linear in f_v and rho_BC:

a) As discussed in the text, rho_BC is taken as 2.0 g/cm3 even though Schuster et al. (2005) give a plausible range of 1.7 to 2.0. The authors adopt 2.0 to enable comparison with Schuster, but no other reason is given as to why 2.0 is a better value for this site. Is there a reason? If not, then can the statement be supported in the abstract and text that these results along, with those of Schuster, suggest that the value is in the rang of 8 to 10 mˆ2/g, since at least part of the result is assigned the same input value as Schuster? (Part of this issue is already addressed by the uncertainty given at the bottom of Section 2.2, but the uncertainty should be carried through in how the conclusions are presented.)

b) The calculation of f_v requires the complex index of refraction for BC, taken as 2.0 -

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1.0i. This is certainly a reasonable value, but is it the only one that might be applicable here? If not, then what is the range of reasonable values and how sensitive are the results to that range?

c) Together, the uncertainties in 2 a and b represent the dominant sources of uncertainty (I think), which should be quantified and included in the results (abstract and conclusions).

Other specific comments:

pg 4084, line 10: “a more rigorous method”; As discussed above, it is more involved but is it necessarily more “rigorous” given the uncertainties?

pg 4084, line 14: What is the uncertainty for the specific absorption? (+/- mˆ2/g)

pg 4086, line 1: Approximately how far is the building removed from the road and, more to the point, how far is sufficient for the aerosol to be considered to be “aged” vs. “fresh” for the wind speeds in the area? (This doesn’t need to be precise, but some justification is requested for claiming that the aerosol must be ‘aged.’)

pg 4088, line 16: How is the top level of the “bulk of the aerosols” determined? Is it quantified somehow, or done by eye using plots of the lidar backscatter identifying sharp gradients?

pg 4091, line 3, “is a proxy for the concentration throughout the mixed layer”: Recommend: “serves as an index of the integrated concentration within the mixed layer” (since the wording of the former sentence implies, at least to me, that it is approximately constant with height, which I would disagree with).

pg 4096: The discussion of the wavelength power is well intended, but confusing in its current form. The AERONET volume distributions and motor vehicle aerosols seem most relevant, and suggest a power of -1. Are biomass aerosols (power=-2) and Moosmuller (-2.7) relevant enough to be mentioned here? The current discussion reads as equally weighted between the -1 and non, but it isn’t clear to me that they address

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the same type of carbonaceous aerosol. If they do, then the other values should be included in an uncertainty estimate; but, if my reading is accurate, then the -2 and -2.7 might be mentioned briefly, but de-emphasized and without an additional uncertainty analysis.

pg 4096, last line: Doesn't the radii size limit discussion assume that Fuller et al. used the same f_v? If so, do they?

pg 4097, line 24: All of the f_v and omega seem anti-correlated (not just 18 April).

pg 4098, line 13: "removing these value from considerationĚ": With the work that went into Method I, it seems a shame to dismiss its potential contribution to the conclusions of the work. Even if General Comment 1.a cannot be pursued, it would seem reasonable that this estimate of Method I places a lower bound on the estimate of al. (once wavelength adjusted), which would help the part of the discussion that focuses on whether or not the value should be greater than or less than 10 mˆ2/g and by how much.

pg 4099, line 16: I'm not sure I understand why this would increase the optical thickness at the AERONET site. Why would it accumulate (pile up) there as opposed to just advect across as it did at the MFRSR site?

pg 4100, line 8: If Dubovik finds 1.47, why not give the % difference on specific absorption between that value and your 1.56 (instead of 1.33)?

Fig 5: Are the AERONET optical depths adjusted from 550 to 500 using the Angstrom exponent?

Technical Comments

(Note: [...] indicate text addition, X...X indicate text deletion)

pg 4084, line 5: "the [monochromatic] absorption cross section"

pg 4084, line 12: "method I" is not defined in the abstract (although presumably it is the

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first approach mentioned). Please clarify.

pg 4084, line 13: either add a comma after “suggest that” or remove the comma after MCMA.

pg 4084, line 21: “the [monochromatic] absorption cross section”

pg 4085, line 12: “because of[:] the”

pg 4088, line 21: variation XreminiscentX [that lags] the [surface] BC concentrations.

pg 4089, line4+5: “500 nm)[,] inferred from MFRSR measurements[,] and...”

pg 4090, line 22: “XobservedX BC concentrations [observed at the surface] are well-mixed in the boundary layer below the mixing height, XandX BC...”

pg 4091, line 11: “about the [internal or external] mixing state of BC.” (Since you are making an assumption about the atmospheric mixing.)

pg 4092, line 2: “does not absorb [significantly].”

pg 4097, line 18: suggest: “actual M_BC and, correspondingly, al is likely...”

pg 4097, line 26: “are converted [to] values”

pg 4098, line 15: “XindicateX [suggest] that...”

pg 4099, line 21: “attributed to BC [by Schuster], when...”

pg 4101, line 20: “to XoursX [Method II].”

Fig 1: “[Surface] black carbon concentration”

Fig 1, 2 and in text: Julian day is used (which start 1st of January, 4713 BC) instead of Year day.

Fig 2. “Panel (a) shows XatX [a] time...”

Interactive comment on Atmos. Chem. Phys. Discuss., 5, 4083, 2005.