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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Comparison of in situ and columnar aerosol spectral measurements during TexAQS-GoMACCS 2006: testing parameterizations for estimating aerosol fine mode properties" *by* D. B. Atkinson et al.

Anonymous Referee #1

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This is a good paper that utilizes a rich dataset, but I found it difficult to maintain focus while reading it. The authors compare results from using many different measurement techniques, but it is not clear why they are doing this... That is, what is the advantage of using in situ extinction measurements to ascertain the fine mode aerosol contribution, when there are other in situ measurements that do this better?

Part of the problem is that there are so many non-standard acronyms. As a reader, I found myself repeatedly converting cryptic acronyms to something more meaningful. For instance, "AERONET SSRI" should simply be called AERONET inversions, and

that would alleviate the burden on the reader to memorize an acronym that he will probably never see again. Yes, readers are intelligent and can do these conversions, but most of us are too busy already and would rather have things spelled out.

Issues:

On page 17468, line 15, the authors state: values of Angstrom Exponents near to or less than zero pertain to large (coarse mode) particle sizes, while larger values are produced by fine mode particles.

but on page 17479-50 they state:

For period 1 (coarse mode dominated), the average value of alpha_ep for the sub 1 μm data is 1.5, whereas the alpha_ep value for sub 10 μm is around 0.8.

This is a bit of an inconsistency, as 0.8 is greater than zero. On page 17468, the authors should state that Angstrom exponents less than *one* are dominated by the coarse mode, and that values greater than 1 are influenced by the fine mode as well as the coarse mode (i.e., it is not a hard cutoff).

Page 17475, line 2: For coarse-mode dominated aerosols, alpha' is <= 0. Although this is generally true, it is not always true. The authors should change this statement a little bit to reflect that alpha' can be less than zero for some size distributions that are dominated by the fine mode.

Page 17481, line 4: The authors state that the agreement is satisfactory (a subjective term), but don't state why. Some quantitative comments would be nice, like: if the black line is the "truth," then eta is 25% too low for period 1, and is often off by +/- 15% during period 2, etc.

Page 17481, line 7: The authors state that eta is expected to be smaller than SMF, but fig 6 shows the opposite. how come?

Page 17482, line 4: Again, quantifying "satisfactory" is important. Also, are the 24-

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hour averages day and night? AERONET is daytime only, so how does this affect the comparison? Additional statistics would strengthen the paper, perhaps a table with correlations, slopes, intercepts, biases, etc. of all the methods vs. the SMPS or DMPS. Finally, some comments on the agreement/disagreement of the column vs. surface measurements would be appropriate.

Despite these many comments, I believe that this is a good paper worthy of publication. These comments can easily be addressed with relatively minor changes in wording.

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 17465, 2009.