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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Particle number size distributions in urban air before and after volatilisation" *by* W. Birmili et al.

Anonymous Referee #2

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The text describes carefully done air quality measurments in an urban environment. The main results are not really surprising but still I find the text good and worth publishing. Especially the data handling is innovative and surely worth publishing. The size-dependent shrinking factors and their temporal variation in sections 3.2.2 and 3.2.3 are very interesting. It somewhat disturbs me in this text that formulas and equations are clearly avoided even though they would make the presentation clearer and unambiguous. You could easily add a few formulas there.

I have some correction and modification suggestions:

P. 9178 The MAAP and the aethalometer comparison is reported inadequately by only giving the offset, correlation coefficient and averages. Rather give the linear fit as BC(aeth) = k * BC(MAAP) + BC0, Rˆ2 = xx.xx

The MAAP is presented in the " ..., the multiangle absorption photometer (MAAP; cf. Sheridan et al., 2005)." The proper reference to MAAP is not Sheridan et al. but Petzold & Schönlinner, J. Aer. Sci 2004 - look for the full ref.

p. 9179 2.4 Diameter shrinking factors it is written " This method was originally developed for the analysis of the hygroscopic growth of particle size distributions (Birmili et al., 2009) but has since been applied to quantify the loss in particle diameter after passing through a TD (Engler et al., 2007)." There is some logical error in this: the method was originally developed and presented by Birmili et al in 2009 but since then (= later) been applied by Engler et al in 2007. How is this explained?

p. 9184 3.1.4 Auto-correlation analysis The whole procedure is kind of intuitively clear to me but still not exactly. Especially when it goes to the next phase, the factors Fa – Fd and the integrals of these. This all should be explained more in detail, including a couple of formulas. Actually this seems to be an interesting and useful way of handling data – has it been used by others? If yes, give references. If not, the more there is reason to really give the formulas starting really from the definition of ACF.

p. 9189 lines 20 – 26 " one equivalent of non-volatile PM appears to cause more light absorbance in winter compared to summer. ... Another explanation, which is hard to verify, could be the presence of absorbing aerosol other than soot during the summer period." The latter is plausible. The aethalometer inlet cutoff was 2.5 μm and your annual cycle showed a peak of 2 μm particles in summer. Soil dust particles may easily contain some light absorbing material that the aethalometer reports as BC.

Figure 11. Bad selection of colors: at least my eyes can hardly distinguish spring and winter dots. Actually I suggest you would make a figure 11B where the same data would be presented as with a average squares and some ranges, for instance 99th percentiles in both x and y directions for all 4 seasons. This would be more significant.

Fig 12. Messy, too many diameters in one rose. Why don't you select only the diameters that you used earlier for instance in fig 5? Or make subfigs.

p 9191 lines 15-16: "... Figure 14 and 14 gives the mean particle size distributions, and Fig. 14 the mean virtual potential temperature ..." Missing the subfigure letters in the text.

All the figures associated with the trajectory cluster analysis are messy, there are not clear differences between the various descriptive parameters, for instance size distributions associated with many of the clusters. The main reason seems to be that there are so many trajectory clusters and some of them are actually not very different from each other. I would suggest reducing the number of clusters and redoing the clustering – or if this is too big a work at least reduce the number of size distributions etc. presented in the figures and explain in the text that the size distributions associated with the clusters nn-nn were statistically not different. Just do something to make the figs 14a-c clearer.

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 9171, 2009.