Atmos. Chem. Phys. Discuss., 9, S314–S317, 2009
www.atmos-chem-phys-discuss.net/9/S314/2009/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "The time evolution of aerosol size distribution over the Mexico City plateau" *by* L. I. Kleinman et al.

Anonymous Referee #2

Received and published: 20 February 2009

This manuscript is generally well-written, presenting results clearly (with a few exceptions that are pointed out below) and with a suitable amount of discussion and analysis. The topic is appropriate for ACP, and I recommend its publication once the following issues are addressed.

1. The authors should be aware of the following manuscripts:

Smith, J.N., M.J. Dunn, T.M. VanReken, K. Iida, M.R. Stolzenburg, P.H. McMurry, and L.G. Huey, Chemical composition of atmospheric nanoparticles formed from nucleation in Tecamac, Mexico: Evidence for an important role for organic species in nanoparticle growth, Geophysical Research Letters, 35 (L04808), L04808, doi:10.1029/2007GL032523, 2008.

Full Screen / Esc
Printer-friendly Version
Interactive Discussion
Discussion Paper

Iida, K., M.R. Stolzenburg, P.H. McMurry, and J.N. Smith, Estimating nanoparticle growth rates from size-dependent charged fractions - Analysis of new particle formation events in Mexico City, Journal of Geophysical Research, 113 (D05207), doi:10.1029/2007JD009260, 2008.

These papers are probably better than the Wang 2006 reference from the MILAGRO science meeting. The observations from these papers support many of the conclusions of the current paper, e.g., organics play a key role in growth and this growth of the nucleation mode into the accumulation mode occurs frequently and often in the afternoon.

2. I am a little unclear about the size ranges selected for the analyses of volume and number growth. In Sec. 4.2 it is stated that the accumulation mode is used for the analyses, however this is defined earlier as extending to 1 micron, which is outside the range of SMPS measurements made with the DMA. In some cases, as in the PCASP measurements, the authors add in DMA-obtained volumes, but it is not clear whether the same is done for the DMA data at the high diameter end (in fact, it seems from the sentence that begins on line 1, page 1630, that the upper size limits for each instrument were used to define the accumulation mode, but surely this cannot be the case for the DMA, which cuts off at 400 nm and thus cannot fully capture the accumulation mode). It seems what is missing is a clearer description of the size ranges covered with each instrument for each analysis. It seems most appropriate in the first paragraph of Sec. 4.2, where the authors already state the ranges and modifications for some of the measurements.

3. Why is the AMS-derived data included in Figure 2a and 3a. By including them separately it implies that they are different data, but I believe that Figure 3a "all" is the same as that shown in Figure 2a "AMS." If this is true, and brief mention of this would confirm the reader's understanding of the data.

4. Some discussion of the expected lifetimes of the accumulation mode would add

ACPD
9, S314–S317, 2009

value to this paper. For example, the Jaenicke parameterizations of the atmospheric lifetimes of accumulation mode aerosol are approx. 10 days (ref: Jaenicke, R., Physical Aspects of the Atmospheric Aerosol, in Chemistry of the Polluted and Unpolluted Troposphere, edited by H.W. Georgii, and W. Jaeschke, pp. 341-373, D. Reidel Publishing Co., Dordrecht, 1982. - other similar manuscripts by Jaenicke abound). By contrast, the chemical clock used in the current paper estimates a maximum processing time of about a day (pg 1627, line 20). Also, growth rates of at most 20 nm/hour have been observed in Mexico City (ref: Iida 2008, above), thus it seems unlikely that the observations reported here will see appreciable differences in the volume distribution (in particular) over just a one-day time span. That is, most of the particles measured in the accumulation mode have probably advected into site, thus differences in chemical processing times would seem to make little difference in the mass or volume distribution. The authors state this as a conclusion of the current work (and point out that the Brock et al. paper also did not observe differences in the volume distribution), however it stands to reason that one would mostly expect this to be the case wherever such measurements are performed.

5. It would seem entirely likely that coagulation is an important mechanism for inclusion into the modeling portion of this study. Coagulation is especially important when (1) high concentrations of particles are present (an N^2 dependence, where N is number concentration) and (2) smaller Aitken-mode particles co-exist with larger accumulation mode particles. Both conditions seem to be present in the current study, and the latter would surely lead to the smearing of the distributions in Fig. 10.

6. If the volume distributions in Figs. 6 and 7 are normalized, then no units should be displayed for the y-axis.

7. In general, the grammar and typographic errors are minimal. I did find a few:

Pg 1634, line 5: I think the intention is to state "tens of nm," but when I see this I think "10 seconds of nm." Maybe best to just replace it with the word "tens."

ACPD

9, S314–S317, 2009

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 1621, 2009.

ACPD

9, S314–S317, 2009

Interactive Comment