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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Characteristics of size distributions at urban and rural locations in New York" *by* M.-S. Bae et al.

Anonymous Referee #2

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This manuscript describes aerosol number size distribution measurements conducted with twin-SMPS systems at three different sites during four intensive field campaigns in New York state. The authors conclude that diffusional losses in the sampling lines of the SMPS have a significant effect on the data and should be accounted for. They also give some general characteristics of the size distributions at the studied sites. The results are intersting, and the data sets are unique. However, in its' present form, I think this manuscript is not scientifically significant enough to be published in ACP, and requires quite significant additional analysis and re-structuring. My comments are below:

General comments:

1) First of all, I strongly agree with Reviewer 1's comments, which I think the authors

should carefully consider.

2) The authors now use a lot of space to describe the diffusional loss calculation that was done based on a basic tube loss correction scheme. Based on their calculations, the authors conclude that diffusional losses indeed need to be accounted for in their sampling lines. I think this is general knowledge in aerosol science (e.g. described in basic text book of aerosol measurements such as Hinds(1999)), and most aerosol scientists are already correcting their SMPS and CPC data for diffusion (or at least they should be!). Besides this, as pointed out by Reviewer 1, the standard loss correction even does not seem to be working optimally for the data. Potential reasons for this could e.g. turbulence introduced by connectors, inlets and curvature of the tubing. Since the authors are not providing any new improved loss correction scheme, I do not think that making the point that diffusional losses in sampling lines are important is worth the space that it is given now. On the other hand, I think it is nice to see particle number size distribution data from New York state - the data sets from these three sites are unique. However, I think that these data sets have not been analyzed deeply enough in this manuscript to provide significant scientific insight into the characteristics of the studied environments. Therefore, I suggest that the authors concentrate on either a) quantitatively characterizing the diffusion losses and improving loss correction they are applying now - maybe resulting in an improved loss correction scheme that others could use too; or b) settle with doing the standard correction they are doing now, acknowledge its uncertainties, and concentrate on doing a more detailed analysis and reporting of the characteristics of the size distributions and number concentrations. This would perhaps mean combining the current manuscript with the "companion study" that the authors refer to on p. 85-86.

3) In particular, I would like to see a more detailed comparison and discussion of 1) the sources of particle number and mass at the different sites (for instance what are the relative contributions of traffic, long range transport or nucleation at different sitest); 2) the characteristics of the nucleation events at the different sites (as described by e.g.

Dal Maso et al., 2005); 3) comparison of the results to similar studies that have been conducted around the world in other environments (see e.g. Kulmala et al., 2004 for a review). Now the authors are reporting very general results from the sites and not really putting them into a context where they would be useful for the scientific community.

4) To gain more insight to the analysis, the authors might want to look at air mass trajectories on the studied periods (calculated with e.g. HYSPLIT trajectory model http://ready.arl.noaa.gov/HYSPLIT.php). This would probably help in analyzing the contribution and origin of different particle sources, and also give knowledge whether some air masses prevail when particle formation events or intense pollution episodes take place.

5) Were there meteorological data (such as ambient RH, temperature, wind direction, solar radiation, pressure or trace gas concentrations) available at these sites? A comparison to meteorological data recorded at the measurement sites might also give insight on e.g. the effect of boundary layer dynamics on particle concentrations.

6) Regarding the characteristics of the nucleation events, it would be good if the authors would report particle particle formation and growth rates - this would help putting the results to a larger context.

Detailed comments:

7) p. 74, Eq. 2: Was the diffusion coefficient corrected for temperature? What about slip-correction? The authors should refer to a source where they have taken the diffusion coefficient formulation.

8) p. 76, Eq. 6: The authors are calculating the mean free path of air. However, in the condensational sink calculation, they should use the mean free path of the condensing vapor (often assumed to have the properties of sulfuric acid) - so in this case Eq. 6 and its application for $\lambda_r = 0.0664\mu$ m does not apply. There is thus probably an error in the condensational sink calculations.

9) Generally, the authors often give far to many decimals in their numbers, considering the accuracy of the experiments. For instance, in Table 1, the size ranges are given with the accuracy of 0.1 nm. I find it hard to believe that this was the precision of the instrument.

10) p. 79, line 24: The authors state that they do not understand the reasons behind the differences in the DMA merging points. On the other hand they imply that it might be related to the fact that one campaing was in winter and the other in summer. Could the authors elaborate on this. For instance, what were the ambient temperatures or relative humidities? What about the temperatures / humidities in the SMPS-systems? Was the sample dried? These issues might have an effect on the interpretation of the measurements.

11) The urban site was the only site that had measurements for both summer and winter. In section 3.8. the authors should make a clearer point of comparing the characteristics of particle size distributions in summer and winter. One can, for instace clearly see that there are a lot more particles in the winter than in the summer. This is naturally probably related to 1) combustion 2) lower atmospheric boundary layer during the winter.

12) I suggest that the authors plot the size distributions (Figs. 11) in logarithmic concentration scale. This would reveal some of the the general features of the data better.

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 69, 2010.