Atmos. Chem. Phys. Discuss., 10, C1016–C1019, 2010
www.atmos-chem-phys-discuss.net/10/C1016/2010/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Size-dependent aerosol deposition velocities during BEARPEX'07" *by* R. J. Vong et al.

Anonymous Referee #4

Received and published: 29 March 2010

Review of "Size-dependent aerosol deposition velocities during BEARPEX '07" by R.J. Vong et al.

The paper presents data of size-resolved particle number flux collected during the BEARPEX '07 field experiment in the California Sierra Nevada. The measurement approach for obtaining size-resolved particle flux in this size range is still relatively novel, and the authors do a fairly good job of explaining their measurements and the subsequent data analysis to obtain deposition velocities. They perform a very good analysis of their data set and present it well, allowing readers to understand both the strengths and weaknesses of the data. Their work to understand and correct for the impact of hygroscopic growth on their deposition velocities is particularly useful.

That said, the paper is seriously flawed in a way that makes it difficult to justify pub-

lication without major revision. Simply put, the authors do not do nearly enough to interpret their results or to indicate how their work fits within the context of the broader field. Presenting data, even well-processed and well-described data, is not by itself a valuable contribution to the literature except in rare circumstances (which do not apply here). In their introduction, the authors refer to the recent review by Pryor et al. and highlight the need for greater understanding of the rates and mechanisms of dry deposition for accumulation mode particles. This is certainly true and is good motivation for the present work. However, I fail to see what the author's have done to reduce the uncertainty and/or improve our understanding of the relevant mechanisms. Do the results ultimately make sense? How do these measurements compare with earlier observations? How do they compare with the numerous models in the literature that predict size-resolved particle deposition velocities? What do any similarities and/or differences tell us about the mechanisms at work? All of these questions are implicitly left as exercises for the reader. Perhaps the uncertainties in the data are too large to say anything significant about the data. If that is the case then the uncertainties are, unfortunately, simply too large.

Beyond this major critique of the paper, I have several additional more specific questions and comments:

Page 4650, Line 5: 'dia' is used here and throughout the paper as an abbreviation for particle diameter. This is not an abbreviation in general use; it would be better to use a more common alternative, such as Dp.

Page 4652, Lines 9-11: The authors indicate an undercounting issue for the FAST instrument? How large a bias was this? Was it size or concentration dependent? More detail is necessary to properly understand what impact this correction has on overall data quality.

Page 4652, Lines 14-17: The authors briefly describe how they used paired OPCs, one with a non-ambient RH, to determine the hygroscopic growth parameters. Because

ACPD

10, C1016–C1019, 2010

of the ultimate importance of hygroscopicity in calculating deposition velocities, it is critical to explain in more detail how the hygroscopic parameter was determined. How was the humidity controlled? What range of humidities was used? How sensitive was the parameter value to the specifics of this approach?

Page 4652, Lines 17-20: This sentence reads as if the OPC sampling lines ran all the way down the tower to ground level. What were the losses in these lines? How were they checked? Were they size dependent? How did any loss correction for the OPC affect the undercounting correction for the FAST instrument?

Page 4653, Lines 4-7: The orientation of the sampling boom was changed 2-3 times per day, by hand. The diameter of the inlet nozzle was also changed, with unknown frequency. How did these procedures affect data quality?

Page 4654, Lines 20-21: The authors mention a few periods when forest fires elevated particle concentrations. This is a place where some added analysis would be helpful. Were the deposition velocities measured on those days more or less than other days? If so, why? Different meteorology, hygroscopicity?

Page 4654, Line 25 to Page 4655, Line 2: This normalization approach assumes that the counting efficiency correction is independent of concentration, does it not? Often the opposite is true, so is this something the authors verified experimentally?

Page 4656, Line 3: The 0.2 Hz number looks like an optimistic reading of the figure. The curves in the top plot of Figure 2 appear to flatten for frequencies greater than 0.05 Hz.

Page 4656, Lines 14-23: If the comment above is accurate, then that would seem to have a significant impact on the analysis described here. 50% or more of the measured particle flux occurs at frequencies greater than 0.05 Hz.

Page 4657, Lines 23-25: Again, understanding exactly how the RH control was handled and how the hygroscopic growth parameter was estimated is critical to this calculation.

ACPD

10, C1016–C1019, 2010

Page 4658, Lines 25-26: Please add a reference for the “Junge” slope.

Page 4659, Lines 24-28: This is not a very rigorous error estimate. How do the uncertainties in the individual deposition velocities propagate in this averaging?

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 4649, 2010.