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Atmospheric Chemistry and Physics Discussions

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

Anonymous Referee #2

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Comments on: "Size-dependent aerosol deposition velocities during BEARPEX'07" by Vong, et al. 10, 4649–4672, 2010 ACPD.

Evaluation: This manuscript describes experimental work to measure the dry deposition velocities of accumulation mode aerosols (in the size range 0.25 <diameter<1 μm) to a pine forest surface using an eddy correlation technique. Number fluxes of aerosols were measured as a function of particle size over a short period. These were then used to compute the size dependent deposition velocities. Diurnal averaged deposition velocities are then produced. Deposition velocities were also examined as a function of surface layer turbulent transport velocity rates, friction speed. Whilst the approach and the results are not particularly novel they do build on previous experiences in the community to minimize uncertainties due to a number of confounding issues

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Interactive Discussion
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and so represent probably the most careful experimental study yet of aerosol velocities using this approach. Furthermore the authors have considered the confounding influence of hygroscopic growth of aerosols on the covariance approach by using two optical particle counters, one of which as operated in “wet mode”, to calculate the RH growth response of the ambient aerosol. This is used along with sensible and latent heat fluxes in a correction derived in previous work to calculate the error in deposition velocity and so improve the results. This negates the need of composition measurements to determine the true f(RH) response. Uncertainties associated with particle counting also were evaluated in the usual way. The results show that Vd = ranged from -0.2 to -1.0 cm s-1 during daytime periods. They also show that |Vd | increases with friction velocity and particle diameter and is consistent with previous studies which are referenced in full.

I find no significant issues now with the measurement or analytical approach as it is applied. The measurement site and relevant canopy structure information is described sufficiently to allow the results to be compared with other data sets, something which is often not done, allowing use in deposition model applications. The uncertainty analysis is sufficient and good descriptions of the quality controls and analysis applied to the time series data to evaluate the performance of the EC system is provided. These have also been improved based on initial reviewer comments. This could be expanded however the references cited to previous work are sufficient so there is really no need. The responses to my previous comments have been addressed fully.

Minor Comments which may or not be acted on and will not affect the publication: 1. I note that most of my original comments have been addressed particularly those with respect to the lack of any stationarity criteria applied to the time series which now improves confidence in the results when comparing to more recent data sets. 2. It would be useful to attempt to provide a parametrisation for Vd(r, u*) but can understand why this is an issue due to the large uncertainties for particles grater than 0.4 μm. 3. It would be useful to state/show the variation of Vd (median) with stability as this remains

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an outstanding issue in this field or at least explain this. 4. Although there is a general reference to the EFLAT experiment error comparisons, it would useful to briefly summarise by quantifying in the final section what the typical size of the correction on Vd was due to hygroscopic factors, so as to assess previous data sets which could not investigate this due to lack of f(RH) measurements. This is probably the most important part of the paper. 5. It would be useful to put a statement in the conclusion to quantify the median deposition velocity in the measured size ranges compared to those predicted by typical models. 6. Why then are the measured deposition velocities measured significantly greater than those predicted using model parametrisations mainly based on early wind tunnel data? This is stated but no hypothesis is suggested. 7. Finally what effect would this humidity correction have on PM1 mass fluxes? Should we be worried about them from the perspective of mass deposition rates?

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