

## ***Interactive comment on “Size-dependent aerosol deposition velocities during BEARPEX’07” by R. J. Vong et al.***

**R. J. Vong et al.**

VONG@COAS.oregonstate.edu

Received and published: 8 April 2010

Response to interactive comment by Anonymous Referee #4 on “Size-dependent aerosol deposition velocities during BEARPEX’07” by R.J. Vong et al. ACPD 10, 4649–4672, 2010.

Referee #4 states that the paper is “. . . a very good analysis of the data and (that we) present it well, allowing readers to understand both the strengths and weaknesses of the data” but that more interpretation and context are needed.

In the revisions detailed in these four responses to referees, we address these issues and describe how the paper will be improved along these lines including adding statistical descriptions, clarifying wording, and increasing comparisons to other work. That

C1372

said, it was never our intention to be able to fully delineate the mechanisms controlling aerosol turbulent flux by means of data from a single ~three week measurement campaign. It is our opinion, and apparently that of the other referees, that the work presented in our ACPD manuscript is a valuable contribution to the literature specifically because it does identify and quantify uncertainties and produces some of the very few field data on the size dependence of aerosol Vd for accumulation mode particles. In particular the quantification of each term in the hygroscopic growth correction ought to be very valuable to other researchers attempting such measurements; these are the first measurements of all these components of the correction that have been taken simultaneously with the flux measurements. The uncertainties are not too large for the data to be useful.

ACPD online paper page # (referenced by the posted version’s text page and line number, as does the referee)

p. 4650 “Change dia to Dp” for particle diameter. These changes will be made in the revisions.

p. 4652, lines 9-11 “How large a bias was (the undercounting issue) ? Was it size or concentration dependent ?” The FAST counting efficiency correction was based on laboratory generated particles sampled out of a large volume for comparison of FAST counts to those for a DMT UHSAS (which is considered to be the reference instrument by the FAST manufacturer, DMT) . The undercount determination was performed for each of the six FAST diameter intervals separately and was greatest for the smallest particles, varying from 3.3X at 0.3  $\mu\text{m}$  diameter to 1.7X at 0.51  $\mu\text{m}$  diameter. As stated in the text, the use of deposition velocities mathematically removed this “calibration/correction” factor from the EC results. The value was not concentration dependent in that BEARPEX counts were low enough that there never was any “coincidence” issue. The FAST counts one particle at a time. Additionally, the lab comparison included changing the aspiration velocity from 50 to 80 m/sec without any change in comparison results (this provides different concentrations of the test aerosol ). Finally, the lab

C1373

based undercount ratios were quite similar to those obtained by comparing the Welas OPCs in the field to the FAST over longer sampling periods. In essence, the FAST does not “see all of the particles” that pass through its nominal sensing volume but true concentrations were available by correcting for this counting efficiency. Nevertheless, we removed this “counting correction” from the results when Vd was calculated as the “concentration scaled covariance” in part because Vd may be a more useful variable for others.

p. 4652, lines 14-17 ~ Provide more documentation on the OPCs and gamma . As noted in our response to referee #3 (see the response listed there under p. C774, b), three additional references have been provided about previous use of this same twin OPC system to study hygroscopic growth (Hegg et al., GRL 33, L21808, 2006; AS&T 41, 873, 2007; ACPD 8, 7193, 2008). The text already states on p.4652, line 15 that “the two OPCs . . .(achieved) . . .RH that bracketed the ambient RH”. Thus the hygroscopic growth was measured for the ambient RH variation with height AGL on that particular day and time of day by setting the output RH of the OPC humidifiers on a twice-daily, or more frequent, basis.

p.4652, lines 17-20 ~What are the OPC sampling line losses ? Particle losses in the OPC inlet line from the top of the tower to the OPC were less than 0.7% for all diameters from 0.2 to 1.0  $\mu\text{m}$  including gravitational, diffusional and inertial mechanisms for a reasonable particle density of 1.5 g/cc. This does not affect the FAST counting correction because these losses are small and because the FAST was compared in the lab to an UHSAS as the primary counting efficiency check/correction.

p.4653, line 4-7 ~Discuss the FAST inlet issues FAST aspiration velocity was determined by the blower setting and the inlet cross sectional area. A series of nozzles with ratios of cross sectional area of 1.5 were selected when the boom direction was changed to approximately match the recent 30 min. ambient wind speed. This was done less than 3 times per day due to the steady nature of the daytime, upslope winds. The FAST face velocity was thus close (usually within ~25%) to ambient wind speed

C1374

but not fully isokinetic. Using formulas from Barron,P. and Willeke,K. (Aerosol Measurement: Principles, Techniques, and Applications”, Wiley-Interscience, 2005) , for non-isokinetic flow, inlet collection efficiency at 1  $\mu\text{m}$  diameter, 2 m/sec wind speed, and 25% non-isokinetic match of face and wind velocities result in a loss of 0.3% at 25 deg C. Other aspiration losses for accumulation mode particles are not significant because the inlet was pointed into the prevailing wind direction (twice per day usually, but up to 3-5 times) by orienting the sampling boom direction and because of this match of velocities. Thus losses due to the FAST inlet are negligible.

p.4654, lines 20-21 ~ Did occasional forest fires affect the results ? The few days with likely fire impacts produced few data on Vd that survived the screening criteria such that no effect on the Vd data is likely. Advection of fire plumes could have produced non-stationarity and changes in flux with height (divergence) but the QA/QC criteria of Vickers and Mahrt eliminated these results before this type of misinterpretation could occur. EC requires that advection and “time change” be small for fluxes to be valid. Data with concentration ‘peaks’ should not and do not get included in the EC results.

p.4654, line 25... ~FAST counting efficiency [see earlier response for p. 4652, lines 9-11]

p.4656, line 3 and p.4656, lines 14-23 ~What are the high frequency losses in flux and Vd ? (this is very similar to the response given to Reviewer 3, indexed in that response at p.C775, e)

The reviewer makes a good point that the text stated that “flattening . . . in the (particle) spectrum suggests that noise is present and that the FAST did not fully (the word “fully” has been added in the revised version) resolve . . .at the higher frequencies” and that this flattening ( begins to occur ) at 0.05 Hz instead of our statement of 0.2 Hz on p.4656 lines 3-4, line 13, line 15 and line 23. These increase the estimate of lost flux due to lack of frequency response for this approach above the value of 15.6 % that was stated in the text. The text on p.4656, middle paragraph will be revised to acknowledge

C1375

that this particular method for estimating loss of flux gives a range of values from 15 to ~50% for the spectrum flattening at 0.2, 0.1 (reviewer3), and 0.05 Hz.

However, the above discussion does not change the better of the two estimates of lost flux due to frequency response and the exact point of flattening in the spectrum perhaps is less critical than suggested by the original text. The smaller error estimate (6 -9%) that is based on cospectral similarity between particles, heat, and water vapor is more valid and unchanged by this improved interpretation of the implications of the top panel in Figure 2. In fact the bottom panel of Fig.2 demonstrates that the FAST captured ca. 50% of its total covariance with vertical wind at frequencies above 0.05 Hz, suggesting that the spectrum only appears to be “white noise” above 0.05/0.1/0.2 Hz. (if there is covariance neither variable can be truly random) That is to say, the cospectrum is more relevant to loss of flux (reference on cospectral similarity has been added in the revisions: Eugster and Senn, 1995) and the capture of flux at  $f > 0.1/0.05$  Hz indicates that the FAST is not completely noise above 0.1/0.05 Hz despite the appearance in the top panel Figure 2. Text is added to this effect in several places in the revisions including changing the parenthetical statement on p.4656, line 23 to read “(where the spectrum appears to become mainly white)”.

In the conclusions we will modify the statement referred to by the reviewer to read “A comparison of the cospectra for particles with those for heat and water vapor suggests that the measured aerosol fluxes are underestimated by a minimum of 6-9 % and these underestimates may be larger”. We think that it is best to not correct the final results for frequency response due to irregularities in the particle cospectrum but of course we estimate and document these errors.

The word “substantially” will be deleted from the conclusions (line 12). We do not believe that the spectrum looks completely white at 0.05 or 0.1 Hz and at any rate covariance has been measured at higher frequencies. The FAST has to produce more than random noise above 0.05/0.2 Hz despite the appearance of the spectrum. The cospectra comparisons provide the better estimate of flux loss.

C1376

p.4657, lines 23-25 [please see our response to your point indexed at p. 4652, lines 14-17]

p.4658, lines 25-26 “please add a reference for the Junge slope”. We will add the citation: Junge,C., “Air chemistry and radioactivity”, Vol.4 , Academic Press, New York , NY, 33-39, 1963.

p.4659, lines 24-28 “This is not a very rigorous error estimate” There must be some misunderstanding about what is referred to here (the so called ‘error bars’ plotted in Figure 6). These are labeled as data “standard errors” in the figure caption and text on p.4659 line 27. This is not represented as a total or specific error but is simply the variation in the plotted means, i.e. [data std deviation /sqrt-N] for each  $U^*$  class in Figure 6. Figure 7 displays the dominant random error: those due to particle “counting”. Systematic errors such as hygroscopic growth, WPL, and frequency response are discussed elsewhere.. We have not performed any statistical propagation of errors because these counting errors dominate other random errors.

---

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

C1377