

Interactive comment on “Eddy covariance measurements with high-resolution time-of-flight aerosol mass spectrometry: a new approach to chemically-resolved aerosol fluxes” by D. K. Farmer et al.

Anonymous Referee #1

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General Comments:

In this paper a novel instrument, which has been described in detail in Kimmel et al. (2010), is used to measure size- and chemically-resolved fluxes over a forest. The focus of the paper is on the technique of using the instrument to make flux measurements, and on the errors associated with these measurements. The measurement technique is outlined in detail, an attempt is made to estimate the measurement errors, and some diurnal flux and concentration measurements made during the BEARPEX

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campaign are presented.

The application of this new instrument to high frequency flux measurements has great potential for improved knowledge of aerosol deposition processes. The authors do an excellent job of discussing both the limitations and the possibilities of this technique. My only complaint is that the writing is often repetitive and overly detailed in places. Specific examples and other corrections are listed below.

Specific Comments:

Pg 5869, l 28+: “EC flux measurements must be taken fast enough...” This is an oversimplification since the contribution from high frequencies decreases following a $-4/3$ power law. Similarly, the following “Measurements must be averaged over 30 min...” is not true. 30 minutes is just a convenient balance between number errors and stationarity in some (or most) circumstances. Figure 3 demonstrates that much of the contribution to the flux comes from lower frequencies. At the very least, “must” should be removed from both statements.

Pg 5870, l 3-9: There is an overemphasis here on the challenge of time precision, especially given the errors associated with the time lag correction and the 20 cm distance between the inlet and the anemometer. These lines could be summarized by saying that faster, evenly spaced measurements will reduce errors. Also, the phrase “time grid” seems like a odd way of saying periodic time interval.

Pg 5873, l 24+: As above, this seems like too much information to say that less than 1 in 200 data points had to be thrown out due when the processor couldn’t catch up.

Pg 5876 – 5878: Steps 1, 3, and 6 are processes, while steps 2, 4, and 5 are corrections. They should be separated. Step 3 could be removed as it doesn’t contribute anything, and step 6 is a calibration or a conversion (a change in units), not a correction.

Pg 5876, l 15: Rotation also corrects for surrounding slope effects as well as instrument

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leveling.

Pg 5879–80 and Fig 3: The frequency and cospectrum should both be normalized so the results can be compared with other studies. See Ahlm et al., 2009 (ACPD Vol 9), Gronholm et al., 2009 (JGR Vol 114), or Buzorius et al, 1998 (J. Aerosol Sci. Vol 29) for examples. The individual dots could be removed from the figure as they obscure the binned results, and the gray dots are buried beneath the black dots. It is also unclear if the binned results are all positive or negative, or a mixture of both.

Pg 5880, l 26 – Pg 5881, l 17: This paragraph seems unnecessary in that errors are first calculated and discussed, after which it is stated that the calculated errors are irrelevant for this particular instrument.

Pg 5881, l 2: DL is used before it is defined at l 23.

Pg 5882, l 11: “smaller in magnitude” might be better than “below”.

Pg 5882, l 27: “increasing with increasing u^* and for unstable conditions.” could be “increasing with u^* and instability.”

Pg 5882, l 29 – Pg 5883, l 4: Is this just the same as saying the assumption of horizontal homogeneity may not be true?

Pg 5884–5 and Fig. 4: I don’t understand how the slope relates to uncertainty. Is it not possible that there could be a large amount of scatter in the measurements but the slope could randomly turn out to be 1?

Pg 5885, l 10–11: What is the “slope of flux”?

Pg 5887, l 1: A better reference for temperature fluctuation dampening is Rannik et al., 1997 (JGR Vol 102, 12789-12794).

Pg 5888: Would “Ammonium Deposition” be a better title for this section? It seems to be the only measurement result discussed here.

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Fig. 5–7: State if time is UTC or PDT.

Technical Corrections:

Eqs 1, 2 and R1 should be followed by punctuation.

Pg 5870, l 18: “Martensson” should be “Mårtensson”.

Pg 5871, l 22: Should “L.” be italicized?

Pg 5872, l 20: “were” should be “was”.

Pg 5873, l 12: “mode” should be “Mode” (to be consistent with l 10).

Pg 5874, l 13: Should “(iii)” be “(i) and (iii)”, since they are both high speed?

Pg 5874, l 16: Should this be “ $m/z \leq 0.5$ ”?

Pg 5879, l 4: “the stationarity requirement” should be in brackets or preceded by a colon.

Pg 5879, l 8: “...clearly visible, though rarely occurred” is grammatically wrong, as is “..., thus typically meeting...”.

Pg 5881, l 29+: If the mean errors of 248, 360, and 194% aren’t typos, they need to be discussed.

Pg 5885, l 3: To avoid capitalization, “R2 for fluxes...” could be “The values of r^2 for fluxes...”.

Kaimal et al, and Solomon et al. are both missing from the references.

Interactive comment on Atmos. Meas. Tech. Discuss., 3, 5867, 2010.

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