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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Eddy covariance measurements of sea spray particles over theAtlantic Ocean" *by* S. Norris et al.

Anonymous Referee #2

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General Comments

This paper describes estimates of the surface flux of sea-spray particles from the ocean using the direct eddy-covariance measurements technique. The observations are taken from the end of a long pier on the Atlantic coast. The basic sensors are a sonic anemometer and a commercial fast particle spectrometer that has been extensively rebuilt and package for this specific task. The aerosol sensor is a significant improvement over regular commercial instruments. The measurements from the pier are limited and principally serve to demonstrate the utility of the technique and the value of the aerosol sensor. The topic is important and this is a significant advance in technology, so the contribution is useful. The paper is reasonably easy to follow and fairly well focused on the topic. There are considerable minor typographical errors and

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clear room for improved clarity.

Specific Comments

*The claim that only recently have aerosol sensors become suitable for eddy correlation measurements needs clarification. I recall paper by Sievering and a paper by Duan et al from the late 80's. The real power in the CLASP is it provides useful data with good spectral resolution and it compact size and ruggedness make is suitable for marine applications.

*Eddy correlation does not provide complete isolation from steady state assumptions (page 13246). Since the observation is not made at the interface, there is still a possibility that a vertical flux gradient may be important particularly for particles of 0.1 micron size where concentrations are often far from boundary layer equilibrium. There particle fluxes there is further uncertainty about the nature of the source itself. This issue is related to later comments (page 13256) about the equivalence of the turbulent flux and the aerosol source function. The authors are correct that there is some confusion (even chaos) in the interpretation of the source function and they are probably right to just present their values without further manipulation.

*A lot of the paper is devoted to experimental detail including error analysis, the issue of interference by deposition of advected aerosols, etc. I think the authors would do well to examine the balance of actual details of their work the reader needs to know versus a lot of standard flux measurement boilerplate. The discussion of Taylor's hypotheseis (page 13250) is unnecessary and should be removed. There is probably no need to discuss and explain the ogive approach since 20 min. averages are commonly used. A simple sentence to the effect that the aerosol flux ogives looked good is sufficient (with a reference or two).

*The comparisons with the currently favored source functions are impressive. The 6 orders of magnitude are rapidly shrinking.

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7, S6060–S6062, 2007

Technical Corrections

P13244. Abstract, line 10. Do you really mean an upper size limit of 0.35 μm?

P13245. lines 7-8 have unneeded detail: just replace with “wave breaking”.

Line 12. “**Is** rich in bubbles”.

P13246. Lines 7-13. Repetitive with earlier discussion – delete.

P13247. Line 26. You mention that De Leeuw 2003 used a heated inlet and that the CPD did too, but I could not find it explicitly stated in the paper that you did with the CLASP.

P13252. Line 18. Should be “were excluded”

P13253. Clarification that measured quantity is dry radius (also Fig 5 and Table 1)?

P13254. How was u* estimated?

P13254. Last line. “spectra **are**”

P13256. Line 1. “were influenced”

Lines 11-15. I don’t see why the log wind profile is relevant to height correction of flux profile. Suggest deleting the sentence.

Table 1. The correlation of the linear fit, R, appears to mislabeled “reg.coef”. I suggest clarifying (is this R or R^2?) and including the value of R for the u* fit also. That way we could see if friction velocity contains more relevant information that wind speed.

Table 2. The “statistical” errors quoted here are for a single 20-min realization or N 20-min values in the wind speed bin? If it is the 2^{nd} case, then we need the values for N.

Interactive comment on Atmos. Chem. Phys. Discuss., 7, 13243, 2007.