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## ***Interactive comment on “Eddy covariance measurements of sea spray particles over the Atlantic Ocean” by S. Norris et al.***

**S. Norris et al.**

Received and published: 6 November 2007

All the Reviewers comments have been noted and correction applied where necessary. Below are the authors responce to the main comments made by Reviewer 2. They start with a copy of the Reveiwers comment.

1. The claim that only recently have aerosol sensors become suitable for eddy correlation measurements needs clarification.

The reviewer is correct that instrumentation for bulk eddy correlation has been around for a while and used by those in the field of particle deposition over forests etc. However the instrumentation available has not been able to measure with size segregation and has been suitable primarily for aitkin mode size particles due to the limitations of sampling statistics at low flow rates. In recent years the sea spray field has become

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more interested in aitkin mode particles thus started using some of the available instrumentation to make the first EC measurements of sea spray particles but these have not been size segregated measurements and do not measure particles in the accumulation mode which is also of interest to the sea spray community. Most recently de Leeuw et al have made EC measurements of sea spray particles however they had to apply a large correction due to limited instrument time response. We have reworded the text to clarify this.

2. Eddy correlation does not provide complete isolation from steady state assumptions (page 13246). 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.

The reviewer is correct that use of eddy correlation does not completely remove problems associated with the assumptions of steady state conditions; the text has been modified to reflect this. We are grateful for the reviewers support for our policy of allowing the simple measured fluxes to stand for themselves rather than attempting to address the issues of corrections for deposition fluxes etc, about which there is considerable uncertainty. We hope to be able to address at least some of these issues with more extensive data sets obtained since the WASFAB campaign.

3. The discussion of Taylors hypothesis (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 discussion of Taylors hypothesis has been removed as suggested by both reviewers. We feel it worth retaining the brief discussion of the Ogive curves; while most researchers working with direct eddy correlation fluxes should be familiar with them, many of those from an aerosol science background may be less so. It is true that a 20 minute averaging period is commonly used as are intervals of 30, 10, and occasionally

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as short as 5 minutes. In many cases the choice of averaging interval seems arbitrary and unsupported by evidence that is appropriate.

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

**ACPD**

7, S6631–S6633, 2007

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