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# *Interactive comment on* "Marine submicron aerosol sources, sinks and chemical fluxes" *by* D. Ceburnis et al.

**Anonymous Referee #3**

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I read this paper with some interest but ultimately was unclear regarding what new (innovative) insights were gained. The data are clearly presented but I did not see hypothesis testing being conducted or new ideas/methods being presented. Rather the data are used to 'confirm' existing knowledge – which to some degree is Ok but are these data (with all the associated uncertainties) moving us beyond the current state of 'certainty' in those expectations? The flux data set has been previously reported in Geever et al (2005) – though this current manuscript has a different focus.

Thus the summary of my review is: - The data set and analysis seem 'fine' but I doubt they are really well suited to address the profiles of different components (due to averaging, uncertainty etc). - The manuscript is in general clearly presented – indeed the introduction is a very useful review. . . BUT. . . - The manuscript – in my opinion – lacks



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the scientific impact that would merit publication in ACP.

Explanation of this opinion is offered below: - If we look at the abstract the only result that is described is; “A strong power law relationship between fluxes and wind speed has been obtained not only for primary sea salt and sea spray, but also for secondary water soluble organic matter. The power law relationship between sea salt flux (FSSS) and 10 m height wind speed (U10) (FSSS=0.0011U103.15) compared very well with existing parameterisations using different approaches.” I think this is reasonable and expected based on previous work and theoretical predictions. (i.e. the flux should be a constant time U10 raised to some power that is approximately 3.). But it is also based on rather few observations and does not per se move parameterizations forward. - If we look at the conclusions it too presents only very “general” findings.

Details and specifics: - The inferences about the gradients is based on fifteen PM1 gradient samples collected during 13 month period (most of about 1 week in duration). Thus I suspect the uncertainty is rather high and much higher than the estimates given in the manuscript – e.g. gas-particle partitioning (on the filter) ought to be considered? Given the large amount of non-stationarity (again not considered in the uncertainty) can new physical insights be derived? Can 3 points in the vertical really be used really be used to derive robust information about the form of the profile? - The plot of dependence of the coefficient of turbulent-transfer Kz on the horizontal wind speed and normalized standard deviation of horizontal wind speed during April 2008, shows (as expected) Kz increases with increasing turbulence (wherein sigma-u is used as a proxy) – is this surprising? Does it yield new insights? I don’t think so. - I am not sure the average shown in Figure 4 has any real meaning – it seems to convolute many processes and again I wasn’t quite sure what physical insight one was suppose to derive? - Minor point: I do not think the eddy covariance method was introduced by Buzorius (or indeed that he would claim to have introduced it); ‘Eddy covariance method introduced by Buzorius et al. (1998)’ - Figure 5. A scatter plot of sulphate neutralisation by ammonium with respect to sampling height. I suspect a height-color



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scale/legend is necessary. But does one really expect a relationship here between NH4+/SO42- ratios in 1 week duration samples where within sample variability must be huge can one be sure this is representative of the atmosphere? And what real 'point' is being made here? - Figure 6. Plots of sea salt and secondary species which resembled primary production concentration pattern: SSS vs. NO3(top left); SSS vs. Oxalate (top right); SSS vs. MSA (bottom left) and WSOC vs. WSON (also plotted as the sum of dimethylamine and diethylamine) (bottom right). ** what is the hypothesis that is being tested here? This seems a little like 'data mining' or exploratory analysis rather than a final 'result'. - Figure 7 is again presenting the 15 points as confirmation of the power law presented by Ceburnis et al. (2008). I guess the uncertainty in wind speed represents the standard deviation around the mean but the vertical uncertainty bars should reflect the total flux uncertainty and surely should be much higher than are indicated here? - Figure 8 – how should one interpret the very large non-zero intercept? - Figure 9 – seems a little bit hard to read and also I am not sure really how to interpret it. Maybe removing parts of the graph where there are no data would help, maybe plot the data uncertainty would help too. - Figure 10 is gain presented as 'confirmation' of past work but is presented without any sort of uncertainty and with many caveats.

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