

***Interactive comment on* “Technical Note: In-situ quantification of aerosol sources and sinks over regional geographical scales” by G. Buzorius**

Anonymous Referee #4

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The manuscript reports airborne eddy covariance flux measurements over the ocean and land in the atmospheric mixed layer. One of the innovative aspects of this study raised by the author is an improvement of the spatial resolution to 3 km segments in measuring airborne aerosol flux. The choice of the spatial resolution is very challenging and it should be done considering the ability to capture almost all the turbulent flux co-spectral modes and the spatial homogeneity which is a basic requirement of the eddy covariance technique. Somehow the author acknowledges the systematic and random uncertainty due to the shorter spatial resolution and non-stationarity of the turbulent time-series. However the author needs to address the detailed comments below, before the manuscript is suitable for publication in ACP.

Page 1304 line 23. remove "and" after the comma, e.g. "below the flown altitude,

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correlating the aerosol fluxes...".

Page 1305. The experiment setup section should include a better description of the experimental sites. I would change the title of this section into "The site description and the experiment setup". I would start with the description of the sites and then proceed with the experiment setup. The lines 6-13 in the section 4.4 give a description of the continental site and then should be put here and not in the results part.

Page 1305 line 9 add "(CPC)" after "condensation particle counter".

Page 1306 line 5 remove "shown in Eq.(1) below".

Page 1306 lines 7-9 are fragmented, please reformulate this sentence. Add also the corresponding spatial averaging (10 km and 3 km) to the mentioned averaging time periods.

Page 1306 line 12 replace "condensation particle counter (CPC)" with "CPC".

Page 1306 lines 11-18. About the random flux errors. Those errors are due to vertical wind speed error and limited counting statistics. The author reports an estimation of the flux error equal to $0.1 \times 10^6 \text{ \# m}^{-2} \text{ s}^{-1}$ referring to Buzorius et al., 2006. The author should clarify if in this study such flux error estimation is for a time window of 60 or 200 sec. Assuming that the std of w and c would be the same for 60 and 200 sec (which probably is not true), the error flux estimated according to Buzorius et al., 2006 equations would increase by a factor of 2 reducing the time window from 200 to 60 sec. The difference is small, but please comment on that.

Page 1306 line 11-18. About the systematic error. The author should give typical value for this error, which is due to limited response time of CPC. Since few lines below the author refers to Buzorius et al., 2006 for a detailed discussion of the measurement error, I assumed that he used the same approach to correct the fluxes in this study, e.g. the flux correction for high frequency loss amounts to about 30%, as derived by the Horst (1997) formula. Buzorius et al. (2006) shows that the wT co-spectrum (Fig.

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7g) and the wc co-spectrum (Fig. 7h) follow the expected surface layer scaling at high and low frequencies, then the use of Horst(1997) formula could be justified. However the normalized frequency nm at which the co-spectra peak is about 0.2, then larger than the theoretical surface layer value (0.085) for neutral and unstable stratification. Using the actual nm value (0.2) in the Horst(1997) formula, the flux correction for high frequency loss increases to about 50%. Since this systematic error is quite large, the author needs to check this issue and eventually recalculate the fluxes accounting for the actual correction factors.

Pages 1306-1307. Footprint. The first two sentences of this section could be removed, since a general definition of footprint function is not necessary. The rest of the section should be better reformulated, and the author should give footprint estimation for both the marine and continental sites.

Pages 1307-1308. Webb correction. In my opinion also this section (included the Eq.2) should be removed, since an explanation of the Webb correction is not necessary. The author could briefly discuss it at the end of section 3.1, giving the correction percentages for the marine and continental sites.

Page 1308 Line 11. Why "two atmospheric scalars"?

Page 1308 Line 19-25. You should mention in the methodology section (3.1) that the flux are calculated using a "kind of moving average" as is explained in Buzorius etal (2006), otherwise the reader cannot understand the Fig. 1, where over a distance of 40 km (800 sec) you have so many points representing fluxes averaged over 5 km (100 sec) and 3 km (60 sec).

Page 1309 Lines 24-27. can be moved to the methodology section as I suggested above.

Page 1310 Lines 15-20 How the linear detrending works in presence of such sharp rise in aerosol concentration?

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Page 1311 Lines 9-11. What are the estimated values of the effective source strength? Are those values reasonable for what you expect from ship emission rate?

Page 1315 Lines 7-16 I would move these lines to the site description and footprint sections, as I earlier mentioned.

Page 1315 Lines 16-18 I would remove the sentence about the large eddies from here.

Page 1316 Line 3. Remove the sentence "The aerosol number concentration was sampled at 10 Hz".

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 1301, 2009.

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