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Interactive comment on "Eddy covariance measurements and parameterisation of traffic related particle emissions in an urban environment" *by* E. M. Mårtensson et al.

Anonymous Referee #3

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The paper offers valuable results on direct flux measurements of aerosol particles over an urban landscape and this type of data is really scarce. I suggest that the paper will be accepted after the following items are concerned:

1. The language is generally quite good but it requires a bit more polishing.
2. In several parts the authors use the concept of footprint although, strictly speaking, no real footprint analysis has been carried out (abstract, line 12; p. 5549, l. 7; p. 5551, l. 26; p. 5552, l. 25; section 3.3., p. 5560, p. 5564). The studied area

is only divided to some geometrical sectors and it is said that the footprint area is quite large. To call the analysis as footprint analysis, one should not necessarily need to make footprint climatology (calculate footprint for each 1/2 h), but at least to estimate the average radial (and lateral) extensions of footprint areas for stable, near-neutral and unstable cases. Prior to that, one should define whether the footprint is estimated for, for example, 50%, 75% or 90% contributions. However, since no footprint model, which includes the possible effects of phase transition processes, exists, one should rely on gas transport footprint models as rough estimators. That said, I am not in fact asking the authors to make the real footprint calculations, although they could bring more information for the analysis, but to change the wording "footprint" to "qualitative source area analysis", for example. I made rough calculation (it's very easy) using N. Kljun's quite reliable parameterization (available at http://footprint.kljun.net/): putting sw = 0.5 m/s, u* = 0.38 m/s, z = 118 m, BL height = 1000 m, z0 = 2 m and footprint contribution = 75% and I obtained that the location of the maximum is at distance of 600 m and the extension to 75

3. Since the measurement height is 118 m I am wondering how much bias the assumption of measuring within constant flux (surface) layer may produce. The authors mention the problem in P. 5560 but not discuss the problem further. One should consider and correct the flux divergence which is not easy task but a bit more should be said on that.

4. Another thing related to the measurement height is the averaging time. 30 min is used as always, but this is more or less good compromise at heights of lowest tens of metres, higher up the averaging time should be lengthened, at least in principle, to catch also the largest turbulent eddies. I suggest that the fluxes are calculated also as 1 h averages to see the effect, I guess it is not big for present results, but I think it would be good to check.

ACPD

5, S2759–S2761, 2005

5. On P. 5548 it is mentioned that turbulence causes an uncertainty of about 10%. Is that value just adopted from Rannik and Vesala, 1999? It was based on gas measurements at 23 m over the forest and in the case of this paper the value could be much different.

6. P. 5551, l. 2-3, "...deposition fluxes...should be dependent on aerosol number...", partly true, but also the stratification, surface roughness etc. affect. These are the factors which determines, beside particle size, the deposition velocity and the velocity together with concentration gives the flux.

7. P. 5554 l. 3-4: I don't understand the meaning of "when energy decay with the lower frequency left of the peak".

8. There exists the famous "night time problem" in eddy cov. CO_2 studies: fluxes measured under low friction velocities tends to drop down, although they are storage corrected (accumulation rate based on concentration profile) and temperature classified (normalised respiration rate). There seems to exist kind of threshold u* value for that, although the transition is not very sharp. For that reason most researchers filter out low night-time u* data. On P. 5560 l. 7-8 it said "Eq. 7 ...overestimates the nighttime flux". This sounds similar problem. Some comments?

9. Table 1. w'T' does not have units of W/m^2

10. Many Figs. have too small fonts (like Fig 2.), they were better in the first version of the paper!

Interactive comment on Atmos. Chem. Phys. Discuss., 5, 5541, 2005.