

## ***Interactive comment on “The relationship between 0.25–2.5 $\mu\text{m}$ aerosol and $\text{CO}_2$ emissions over a city” by M. Vogt et al.***

**Anonymous Referee #2**

Received and published: 5 November 2010

General:

The authors present a very interesting data set of aerosol flux measurements at a tower in Stockholm. The topic is relevant and such data are still sparse in the literature and definitely worth publishing. The paper has a number of issues that need to be addressed in detail before further consideration. This regards some of the analysis, presentation and discussion of the data as well as the language of the manuscript that need to be carefully checked preferably by a native speaker.

Specific comments:

Abstract: needs a bit more focus on the results, some introduction and general statements could be shortened or removed.

C9474

Page 21524, line 5-10: What about other sources to both  $\text{CO}_2$  and particles like house warming, industry and others? Can they be excluded for the case of Stockholm? Some numbers/facts documenting the dominance of the traffic source both for  $\text{CO}_2$  and particles maybe from an emission inventory would be needed.

Page 21524, line 26-27 and Figure 1: The location of the tower and the discussed Söndra Länken should be clearly marked in the figure.

Page 21526, line 21: How do the (negative)  $\text{CO}_2$  fluxes for south easterly direction depend on time of the day and season? Photosynthetic activity should depend on light and temperature. Showing this in a graph or at least giving the results in the text would strengthen the argumentation very much.

Page 21527, line 2 and Figure 2b. The long range transport versus local urban contribution argument is not 100% convincing just looking at the plot. What about showing a day and a night average curve in Figure 2b? Local contributions to concentrations should be very different day/night (as for fluxes) and long range transport more constant.

Page 21528, line 13-14 and Figure 4: Was this plot done for wind directions around 40-80 only? If yes, add in the text and figure caption. If not, why not, should this be better changed?

Page 21528, line 25, Figure 5: The discussed “exponential increase” for the mass emission factor is probably the beginning of a nearly log-normal-distributed mode at a size larger than  $2 \mu\text{m}$ . A similar mode can be assumed for number distribution below  $0.25 \mu\text{m}$ . This reveals actually to an important limitation of the study that should be discussed and argued for in the manuscript. The cut-off of the OPC it at the maximum of the size distribution both for mass as for number and the measured values will be very sensitive to the cut-off diameter of the instrument. How stable and exact is this cut-off e.g. with changes in ambient conditions or operating conditions? Have you checked for influence of relative humidity on the data? This is also critical in relation

C9475

to the reported number and mass emission factors. How well does the OPC- "PM2.5" emission factor compare with the e.g. TEOM-PM2.5 emission factor when measured at the same site (e.g. Hornsgatan)? Is there a systematic bias when you mix the different methods in Table 2?

Page 21529, line 1-9: Include the TEOM PM2.5 data at Hornsgatan in order to compare the optical and the mass based methods.

Page 21530 and Figure 7: One would expect a very high correlation between  $u^*$  and wind speed especially for the high values where thermal convection is less influencing. How well is the correlation in your data set? Are not both measures very similar and figure 7a and 7b are essentially showing the same effect? If this is the case just give one of the figures and also shorten the discussion. In case both figures contain essentially different information, some justification should be given in the text.

Page 21531 sections 3.6: The whole section need substantial rewriting Line 11-15, sentences not clear. Line 15: catalysts do normally not alter PM emissions (solid particles), do you mean particle filter? Since no differences in the contributions in exhaust/ non-exhaust emissions are given in Table 2 the discussion can not be followed by the reader. Consider to add the division in exhaust / non-exhaust if available in the reference in Table 2 or rewrite discussion.

Have you estimated the range and importance of the various removal processes, deposition, gravimetric settling? Do rain days have significant influence, e.g. due to wet deposition? Technical corrections:

Page 21523, line 3+4: Should this not be the other way around? measurements are needed to do the source apportionment? or do you mean AQ management??

Page 21524, line 19 ...enables...

Page 21528, line 2-5 needs rewriting, not "...amount of fuel burned [g/l]..." but "... amount of emitted pollutant per amount of fuel burned [g/l]..."

C9476

Page 21528, line 8: Do you mean "15 CO2 flux intervals" not "concentration intervals"?

Figure 4, caption: remove one time "...the linear fit..."

Page 21529, line 21: Why only break wear is mentioned, not the other non-exhaust emissions (road and tyre wear, re-suspension)?

---

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 21521, 2010.

C9477