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Comment

Interactive comment on “Spatiotemporal distribution of light-absorbing carbon and its relationship to other atmospheric pollutants in Stockholm” by P. Krecl et al.

Anonymous Referee #1

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General comments

The paper represents light-absorbing carbon mass concentrations MLAC from three different urban locations from Stockholm, Sweden, and from rural station close to Stockholm. The spatial variability of MLAC has not been widely studied and therefore the study makes an important addition to the field and decision makers. The authors also raise an important questions that whether MLAC should also be regulated and not just PM_{2.5} and PM₁₀ as it has adverse health effects and is known to play a role in climate change.

However, there are some corrections and improvements in the data analysis that needs

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to be taken into account before the manuscript should be accepted for publication to ACP.

Specific scientific comments

The main rural site is Aspvreten but as PM_{2.5} is only measured there for a short time period the authors use PM_{2.5} measured in Norr Malma to replace the missing PM_{2.5}. There should be some information about the correlation of PM_{2.5} measured in these two locations during the simultaneous measurements.

The statistical analysis in the manuscript should be improved.

-Table 1 lists mean MLAC measured in different locations and it is said that the values are from the period of simultaneous measurements. It is unclear what by these simultaneous measurements is meant. If concentrations or correlations from different locations are compared, only hours when data from all four measurement locations exists should be involved in the analysis. The number of these data points should be given as they are used in statistical etc. analysis. Same applies to the entire manuscript when comparisons/correlations between the different sites is made.

-In Section 4.5, the authors use unpaired t-test to compare the weekday and weekend concentrations. In order to use the t-test, the sample sets should be normally distributed, which usually is not the case for pollutant concentrations. If this is the case, some data transformations before the test should be done to get realistic significances.

-Also it would be good to add root mean square errors in the statistical analysis as they are more representative for the goodness of the fit. This applies particularly when comparing MLAC measured with PSAP and Aethalometer (P13288, L8-9).

Plotting frequency distributions for each site would be an easy way to visualize the differences between the measurement sites. This would be more clear way than the list of percentiles in Table 1.

In the manuscript there should be more analysis/discussion about the effect of different

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meteorological conditions on the observed behaviour. Beside WS and WD, also rain is important. In the paragraph starting from P13289 meteorological variables are listed but no analysis/interpretation is involved.

In Section 3.6, the authors discuss about the wind direction dependence of different pollutants. For Torkel and Aspvreten higher MLAC is measured from E and SE and it is said to correspond the predominant wind direction at Torkel and Aspvreten during the wild fires. In order to connect the wild fire plume and increased concentrations, the authors should show how the trajectories during the wild fires behaved. This could easily be done by using HYSPLIT etc.

The section describing the contribution of urban sources to the regional background (P13297, L4-15) should be largely rewritten in more understandable way. The method of the analysis and figure 8 was not clear except after reading Ketzal et al. (2004) paper.

Minor comments

In the manuscript, MLAC is defined as light-absorbing carbon mass concentration. However, later in the text it is referred as MLAC concentration even though it should only be referred as MLAC.

Section 3.2 explains the methods to define mass absorption cross section and coefficients. However, in some occasions (P13285, L1, L15) the authors also use attenuation coefficients. There should be consistency with terminology in the manuscript.

P13287, L12-14: The instrumentation used to measure meteorological variables should be listed (maybe in a table?)

P13282, L8: Carbon mentioned two times

P13282, L22: NO₂ not explained even though on line 19 several components are listed by name.

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P13288, L2: The meaning of bap is not mentioned.

P13293, L7: Is LT the local time? This should be explained in the text.

P13293, L8: “pollutants concentrations” should be “pollutant concentrations”

P13294, L15-22: The authors discuss about the effect on wind speed on PM10 and on LAC and refer to Vogt et al. (2010) paper. In Vogt et al., they study number concentrations for particle size range 0.8–2.5 μm and therefore their measurements do not represent PM10 mass concentration. Also referring to the behaviour of particle fluxes is questionable here as fluxes and concentrations have different source areas and the reasons for similar behaviour can be different. This part should be rewritten.

P13295, L14: What are the WD sectors? 10 degree, 20 degree?

In reference list, Bond and Bergstrom had a wrong year and Vogt et al. still referred to ACPD paper and not the ACP paper.

Table 2 and 3 could be combined.

In Fig. 7, the regression lines should be with different line types.

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 13279, 2011.

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