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Interactive comment on "Correlation between traffic density and particle size distribution in a street canyon and the dependence on wind direction" *by* J. Voigtländer et al.

J. Voigtländer et al.

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We would like to thank the referee for his useful and insightful comments regarding this work.

We took the reference comments into regard in the following way:

General Comments:

"This manuscript presents an investigation of the correlation between traffic density and size resolved particle concentrations. The data analysis shows in several ways of plots that wind direction has a main influence on the concentrations measured in a street canyon. As stated in the paper this supports previous

findings and has been shown in many other locations and for other pollutants. The authors present a very interesting and comprehensive data set, but the data analysis, discussions and conclusions are limited to correlation analysis with traffic counts. The results from the correlation analysis are overinterpreted and some conclusions not fully justified (size dependence of the correlation coefficient, ratio of particles originating from traffic, lack of temperature dependence, see detailed comments)."

The detailed comments were answered downwards.

"More use should be made of the background data measured at IfT to separate the traffic contribution to the particle concentrations from the background. The reasons to not include them (p. 4092) are not obvious since Fig. 6b shows clearly that IfT seems to be a good background side for the street canyon Eisenbahnstr."

IfT is not an ideal regional background station. IfT itself is influenced by traffic. The next busy road is about 100 m from the measurement site. Data from IfT showed the same diurnal variability as data from Eisenbahnstrasse. We observed maxima at the rush hours on weekdays. Thus, using the difference between Eisenbahnstrasse and IfT, the calculated correlation coefficients gets slightly lower (for particles larger than about 20 nm). We also made a graph, but unfortunately, it cannot be shown here.

"Also gaseous pollutants that are very good tracers for traffic pollution as NOx and CO could be included in such an analysis."

Of course, measurements of gaseous pollutants would be very good indicators for traffic pollution. Unfortunately, no such data were available for this study.

Detailed Comments:

"A major change in traffic volume occurred in Jan 2004. All data analysis should be made separately for times before and after this major change since the traffic composition has changed, the traffic signal is much smaller and other sources

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due to the reconstruction might have appeared."

Correlation analysis presented in the paper were performed focussed on data before road reconstruction. In the revision of the paper, this was pointed out again. Data after were less influenced by traffic and the calculated correlation coefficients were much lower. During road reconstruction, traffic counts were difficult. Mainly, the road was only partly closed, but the lanes were changed several. Times, the road was completely closed. Additionally, measurements were influenced by the reconstruction works. Thus, these data cannot be used for solid analysis.

"IfT data should be included also in Fig. 4 to see the change in the traffic contribution (street - urban) background."

We followed the proposal and included IfT data in the figure, even if IfT was not a ideal urban background station for Eisenbahnstrasse.

"Table 1a is missing"

The error was corrected. 'A' was deleted and the order of the letters was changed.

"The regression analysis might be useful for a first interpretation of the data, but how much solid conclusions can be drawn from this type of analysis alone is questionable. Results from Table 1 are not sufficiently discussed. How should the different parts of the table (a-d, (a) is missing) be interpreted. Temperature and maybe RH could shift the change the shape or the position of the maximum of the size distribution without altering the total number concentration. This will not show up in the regression analysis. This limitation should be discussed when stating that those parameters have no effect. Moreover the conclusion that 70% of the particles originate from traffic (p. 4094) is not supported by presented analysis. It can not be drawn from the linear correlation of e.g. Fig. 9, it requires rather the use of the background data from IfT or some factor analysis methods."

The discussion part was partly rewritten and extended. More explanations for the

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regression analysis were included in the manuscript. We agree with the referee, that a shift in particle size distribution could not be shown in the regression. First, influence of pressure is negligible. RH show a small influence, if using all data. Excluding values above 98 percent RH, this influence will be strongly reduced. For RH measurements we used a Vaisala HMP243, which has a accuracy of $\pm 0.1^{\circ}$C. Thus, values above this limit (actually already lower values) were clearly within the uncertainty. On the other hand, small changes in this range of RH may cause large changes in particle diameter. Hygroscopic growth below 90 or even 95 percent RH is generally relatively low, especially for soot particles from traffic exhaust (Weingarnter1997) and in urban street canyons with high traffic volume the fraction of soot and slightly soluble material is large (about 50 percent, Rose2006). Thus, the particle shift due to RH should be low. In our measurements, no shift due to RH was identified. Unfortunately, graphs cannot be added to an AC. Otherwise, we could show a figure with particle size distributions for different values of RH (and temperature).

Temperature is more difficult. We know, that other study observed a temperature (or seasonal) dependency. We did not. One reason is, that we only measured during winter. Thus, we only investigated a relative small size range of temperature. Second, we observed a "temperature" dependency with higher particle concentrations during lower values. But these cases with low temperature below 0°C were often connected with northerly wind (25 percent, against 7 percent for data above 6°C). But northerly wind means lee side conditions with high particle concentrations. The effect due to the wind parameter was larger and covered the temperature effect, which is clearly possible. Additionally, no shift in particle size distribution due to temperature was found in our measurements. We regret, that it cannot be shown in a graph.

We added this explanations also to the paper.

"The scatter or noise the average diurnal profiles in Fig 6. a+b is relatively large compared to Fig.5. What wind direction ranges are used in Fig.6. for north / south wind. Is the range sufficiently wide to allow enough data behind each

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average data point. Fig. 6 shows also that correlation analysis."

The higher noise in was due to a higher time resolution in figure 6. We have changed the time resolution for consistency of the graphs. The range was wide enough, because northerly or southerly wind means a wind direction sector of 90 degree. Thus, we had enough data for each point available.

"The size distribution plots (Fig. 7) are important for interpretation and comparison with other studies. This are also the original measurements. Here the IfT size distribution should be included for in formation about background. The plot should be done for the northerly / southerly winds separately."

We adopted the proposal and included particle measurements from the IfT in figure 7. Data were also separated into the timer period before and after the beginning of the road restructuring. An additional plot was added for different wind directions.

"The maximum here is located around 20 nm particle size. This is where most particles from traffic are emitted. It is a bit surprising that the correlation analysis (Fig. 9) shows a minimum at the just the 20 nm size, where one would expect a maximum due to the high traffic contribution. This has to be discussed and clarified and sets the question how detailed the structure (position of maxima) of the curve in Fig. 9 really can be used for interpretation. The below 10 nm maxima seems to appear only for some of the northern wind directions (see Fig. 10)."

Unfortunately we had an old graph in the manuscript using Person correlation coefficients. We replace the graph with one using Spearman correlation coefficients.

"The flow pattern shown in Fig. 1 can be found in literature and could be omitted and replaced by a reference."

In adoption to the proposal the figure was omitted and replaced by a reference.

"The authors develop a new CFD model, the motivation for adding a new model

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to the already large number of existing street canyon models, rather than using an existing one is not stated. The use made of the model in this paper is very limited."

Of course, there already exist a large number of CFD models. In this study we used our own simplified model, which was developed for education in our institute (Voigtlaender2005). The model has no advantages against other existing models, but is really limited.

The CFD-model was necessary and used to generalize the measurements of vertical wind speed, which took place at another time period (June-July04) than the traffic counts (Oct03-Mar04). We use our own model to test, if such an simplified model is able to describe the flow pattern inside the canyon. Qualitatively, it did it very well. The correlation coefficient between measurements and model was about 0.95. Thus, the model is useable for our application. We want to show, that with our measurements of horizontal roof wind speed we are able to estimate the vertical wind speed at the particle inlet in the street canyon (Voigtlaender2005).

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