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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Evaluation and modeling of the size fractionated aerosol number concentration measurements near a major road in Helsinki" *by* T. Hussein et al.

T. Hussein et al.

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Reply to comments by Referee #3:

Kindly see our general reply to all reviewers submitted in "AC S2625".

Reply to specific comments

>Section 2.1. In Figure 1 an overview map encompassing large parts of Helsinki is presented. While this map is useful to obtain a general overview, the model calculations of traffic exhaust concentrate on a much smaller environment around the road Itavayla. It is this smaller area which matters for the understanding of the pollutant measurements as well as the simulated results. Consequently, a smaller scale map

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encompassing the area 500 meters around the roadside measurement site should be added.

Done and updated to the revised version.

>Section 2.2.1. You mention inversion. Do you mean multiple charge inversion?

“Inversion” here stands for “data inversion” of the measured mobility size distributions to obtain the particle number size distributions. This is now stated clearly in the article.

>Section 2.2.2. There is no statement about the absolute uncertainty of the particle distribution measurement. From which concentration difference or ratio can the size distributions at the background and roadside be considered significantly different? What are the uncertainties at the lower and upper size distribution tail?

The DMPS instrument used at the background site has been operating to produce a long-term data set of particle number size distributions. It has been well maintained and calibrated. Therefore, the SMPS instrument that was used at the roadside site was previously compared and calibrated (before and after the measurement campaign) to the DMPS instrument at the background site. The difference between both instruments (within the common size range of the measurement 8-320 nm) was less than 5%. This difference is smaller than the concentration differences between both sites.

>Section 3.2. This whole section is difficult to follow. Even after reading this section several times I still do not understand how the authors modeled the size distributions. It needs to be made much clearer, how the three models are embedded into each other, or which parameters are passed over from one to the other model. UHMA is introduced to be an aerosol process model. The authors write, however, that UHMA requires magnitudes such as traffic densities and mixed layer height. How is this possible? UHMA describes the condensation of sulphuric acid, organic vapours and water. Please indicate which values of these fluxes were used to initialize UHMA. The values are among those essential for the understanding of the simulation results, and it is not sufficient to

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refer to another source of literature.

This section is now revised and hopefully clearer.

>Section 4: I guess that this results section could be much better structured if sub-sections were introduced. As much as 5 full pages of the final print version of the manuscript (Fig. 3-6 and 10) are devoted to show explicit time histories of original data. This is quite a lot compared to the overall length of the actual experiment. Have you considered selecting a few of these graphs or condensing this information towards its salient features?

We merged some of the figures and revised/re-structured the results section.

>Figure 8: This figure is very busy. I wonder whether the figure could not be replaced by a table which summarizes the modal parameters in compact statistical form. In addition, it would be worthwhile to provide a sub-set of modal parameters for those episodes compared with simulations, so that other researchers can refer to these values for their own simulations.

Done and updated to the revised version.

>Figure 9: Please indicate bars of uncertainty of the correlation coefficient in this Figure.

Done and updated to the revised version.

>Reply to questions/suggestions

>Background reference values: Figure 7 (case IV) demonstrates that there are cases when the roadside concentrations were much below those at the background. This feature in the data needs more explanation, since it means that the background site might in fact not always be suitable to serve as a background site. (It might probably be less suitable than can be judged from looking at the few case studies presented.

Done and updated to the revised version.

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>In Fig. 1 the background site is shown to be located closer to the densely populated parts of the city center of Helsinki than the background site, which could be an apparent reason for this). An approach could be to plot the wind-directional dependence of particle concentration at the two stations over the entire measurement period. Distinct wind sectors should then be defined, which warrant an appropriate use of the assumption roadside concentrations > background concentrations. The uncertainty in this assumption should also be discussed in its effect on the results.

The selection of the wind sectors to define the road sector was based on the wind-sector analysis that was not presented in the article. However, we will a detailed map for the roadside site to give the right impression about the sectors used. In the measurement site description we also talked about the surroundings of both sites.

>One of the major input parameters of the model is the size-dependent particle emission factor. The quality of the comparison between simulation and model will greatly rely on the choice of this parameter. Since this parameter is so important, more direct information is necessary under which circumstances the parameter was obtained. A plot of this size-dependent particle emission factor would be useful as an appendix. In this case, it is not sufficient to refer to another paper.

Because the modeling part was mainly described in more details in the Pohjola et al article and because the intention now is to submit double articles in revised form as PART-I and PART-II, this matter should be clear.

>The share of heavy duty vehicles (HDV) is only available as an average figure. Do you think that a diurnal variation of this HDV traffic share can be neglected in the discussion of the results?

The available traffic density was available as the total number of vehicles regardless to the type of car as LDV or HDV. It is therefore not directly possible to undergo conclusions with respect to the share of HDV.

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>Although a complete aerosol dynamics model was used, we learn only little about the concrete values of coagulation losses and condensation growth as relevant over the transport distance between road and receptor point. In Fig. 10, it appears that the simulated size distributions have significantly greater mean diameters than in the observations. Can you attribute these deviations to aerosol dynamics processes or dilution processes, or can they again only be explained by uncertainties in the particle emission factors used?

They are mainly due to uncertainties in the particle emission factor. We clarified this in the revised discussion.

>The diurnal cycle of vehicular traffic shows two pointed maxima in the morning and in the afternoon (Fig. 2). The afternoon traffic always exceeds the morning maximum in peak traffic volume. The experimental particle concentrations, however, usually show afternoon values that are lower than the morning values, probably due to diurnal changes in meteorological dilution. Can you check whether the model reproduces the actually observed (average) diurnal trends of particle concentrations? Such aspects of dispersions simulations have seldom been shown.

This is an interesting question that merits further investigation. However, the data we have in this study in addition to its limitations do not permit considering it. However, the performance of the CAR-FMI model in terms of the diurnal concentration variations has been studied previously in case of NOx and NO2, and the agreement with measured data was found to be satisfactory (e.g., Karppinen et al., 2000). Karppinen, A, J. Kukkonen, T. Elolähde, M. Konttinen and T. Koskentalo, 2000. A modelling system for predicting urban air pollution, Comparison of model predictions with the data of an urban measurement network. Atmos. Environ. 34-22, pp 3735-3743.

>Reply to technical issues

>Section 5: It is an unfortunate choice to write a separate section out of five lines of text. These results could be integrated easily into a restructured Section 4.

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This small section was removed.

>Language: While the overall standard of English in the paper is satisfactory, the manuscript appears to be unnecessarily flawed by grammatical errors (use of prepositions, the tense, the definite and the indefinite article), typesetting errors, and several awkward sentences. Can some of the native speakers among the authors fulfill the appropriate corrections in the text? An incomplete list of examples: Model evaluation exercise aiming to predict, Measurement site locations, Distribution spectra...

Done and updated to the revised version.

>Figure 11 is unacceptable in this form – Major areas of data points are obscured by the legend.

Done and updated to the revised version.

Interactive comment on Atmos. Chem. Phys. Discuss., 7, 4001, 2007.