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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 #2:

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

Reply to minor comments:

1. Abstract + model description: If I have understood correctly, the CAR-FMI model is first used to obtain a 'dilution-term' for UHMA. Then, the 'modified-UHMA' is used to simulate size distribution evolution. If this is correct, please indicate so clearly, and be careful not to speak about 'modeling system', 'coupled model' etc. Also, is this all that is modified in UHMA (first line of section 3.2)? If there are others, please indicate them



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clearly.

That is correct; the CAR-FMI model is first used to extract the dilution equations that are later used as an input to the modified UHMA aerosol dynamics model to simulate the evolution of the particle number size distribution nearby the roadside. We, therefore, clarified this in the revised model description.

2. Section 3.1: It remains unclear where this lognormal fitting procedure has been applied. Please indicate this in the section. Also, why is there no clear statistics about the modal variables? I suspect that fig. 8 is an attempt in this direction, but it is very difficult to interpret. Why is the color scale in fig. 8 arbitrary? I suggest adding a table with clearer information about the lognormal variables. Actually, the use of this fitting procedure would be very useful in interpreting the results of Pohjola et al. Since their modeling approach is modal, it could be useful to simulate the evolution of e.g. 3 modes, and compare with experimental distributions, fitted with the procedure of this paper.

We stated the use of the lognormal fitting in this study. The figure caption will be also revised to better describe the figures. The scale used in this figure is arbitrary. For comparison purposes, we used the same scale in all subfigures. Now all these points are stated clearly in the figure caption.

3. The literature-part is very Europe-dominated; I'm very surprised to see e.g. the work by M. Jacobson (+ some other U.S. groups) missing. Especially, since it would be very useful to have a fully coupled modeling system, it would be useful for the authors to perform a thorough literature search on the topic.

Since this paper concerns more data analysis, and modeling is in a minor role, we would suggest to skip a thorough literature search on aerosol dynamic models. However, we have added the references where evolution of particle number concentration and size distribution near roadways has been simulated by aerosol dynamic models, and where also dilution and mixing with ambient air has been taken into account in



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same way.

4. Is traffic-induced turbulence included in the model?

Traffic-originated turbulence is modelled in the CAR-FMI model with the semi-empirical treatment of Petersen (1980) that is based mainly on the General Motors experiments. (Petersen, W., 1980. User's guide for HIWAY2, a highway air pollution model. EPA-600/8-80-018. US Environmental Protection Agency, Research Triangle Park, NC, 69 pp.)

In addition, in this study (Hussein et al.) we have allowed for the influence of the traffic-induced turbulence semi-empirically, in the mathemetical treatment of the source term for atmospheric dispersion. We assume that secondary formed particles by nucleation have already been formed during exhaust dilution after the exhaust tube (in less than 0.5 s). Therefore we do not model explicitly the immediate vicinity of a vehicle where vehicle-induced turbulence is substantially more important than atmospheric turbulence; however, we take these into account implicitly by increasing the box height linearly over the road during emissions. There is a detailed description on how this is performed in part I of this double paper (Pohjola et al.)..

5. Even if the modeling approach itself seems ok, I would have hoped for some more analysis on the model results. Now, in the conclusions, it is only stated that model simulations underpredicted the particle concentrations at the measurement site. Why is this? As I see it, there are several assumptions made (the effects of which have not been quantified properly): - The initial, roadside distribution is not known. - The dilution/dispersion model results have not been put under a sensitivity study (it would be useful to show one figure with a concentation diluting, and experiencing no other dynamics, as it is moving away from the road - then one would get a better feeling about the dilution-process). - How is condensational growth (and amount of condensable vapors) treated? ...and is there uncertainty in this? And finally, since the parallel work by Pohjola et al. have 'the missing' initial distributions (and several more measurements



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along the way), why has this model system not been compared with that data set???

We would prefer to keep the model use here as a model exercise only (as discussed above) and no further deeper analysis will therefore be presented in this article (and it is not clear whether such an analysis would be justified in view of the emission uncertainties etc.). However, we revised the discussion part and re-formulated the conclusions based on the outcomes of this model exercise.

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