

***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.**

Anonymous Referee #2

Received and published: 7 May 2007

Review

of 'Evaluation and modelling of the size fractionated aerosol number concentration measurements near a major road in Helsinki' by Hussein et al. (ACPD 7, 4001-4034)

This well written paper, reports particle size distribution measurements near a road in Helsinki and compares the results with a sectional aerosol model.

I had the 'pleasure' of reviewing a similar paper (Pohjola et al., ACPD 7, 2819-2856) by partly the same authors, SUBMITTED THE SAME DAY. This paper described experiments conducted at the same location (a different time) and also modelled with an

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aerosol process model (a more cruder one).

I really dislike this way of 'trying to maximise' the amount of papers. Both submitted papers have their flaws (but different ones) and would benefit a lot from combining. Still, there is practically no cross-referencing between these papers, and, actually the work by Hussein et al. isn't at all mentioned in this paper by Pohjola et al.

This paper by Hussein et al. is much more thorough in describing the experimental results, and also, the modeling approach used (the sectional model UHMA) is more up-to-date. However, this paper suffers from not knowing the 'initial distributions' at road side (instead, Pohjola et al. have measurements of the initial distributions). In contrast, an 'evaluation' of the cruder aerosol process model, as done in Pohjola et al., would really require a comparison to a sectional model such as UHMA.

So my main question is, why did the authors not use the complete data set, and compare both models against this set (and with each other) ???

Summing up, both manuscripts would really benefit from merging - this would in fact result in a really good paper. If, however, this is not the intention, my recommendation is to accept the paper by Hussein et al. with some minor modifications and reject the paper by Pohjola et al.

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 clearly.

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

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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.

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.

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

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

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

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