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Atmospheric Chemistry and Physics Discussions

Interactive comment on "A pseudo-Lagrangian model study of the size distribution properties over Scandinavia: transport from Aspvreten to Värriö" *by* P. Tunved et al.

Anonymous Referee #2

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Referee comments for acpd-2004-0163

Title : A pseudo-Lagrangian model study of the size distribution ... Authors : P. Tunved et al.

———————— General Comments:

This a very nice model study of aerosol dynamics in a few airmasses travelling between two European measurement stations.

I believe this paper is within the scope of ACP and it addresses relatively significant

scientific questions. The methodology of the paper is sound, but not ground-breaking: The use of trajectories as a background for aerosol modelling is a well know method. I am pleased to see such studies of aerosol dynamics. The sensitivity of the model studies was studied by varying several key parameters, but some of them (e.g. organic species properties) were left to lower priority. The title is descriptive of paper contents and the abstract was appropriate.

Results gave new insights to aerosol dynamical modelling also in larger scales. Although the usability of the results are partly limited because of low number of modelled cases due very tight trajectory selection, but partly also enchanced by the very same choice.

The authors have used quite a lot of references, but they seem to be appropriate in quality.

Language was easy to follow and good, although some of the text was a bit heavy to read. Text was clear enough though. Some graphs require minor corrections.

———————— Specific Comments

In the end of subsection 2.2 authors explain that they used only trajectrories that had spend majority of the modelled time period in boundary layer. Is this 50% of the time? How much non-bounady layer transport was assumed and how high did the chosen trajectories go?

In subsection 2.3 (pg 7763): the model highest diameter was 2 micrometers. How much do the authors think that larger particles from this still contribute for the coagulation in these cases?

pg 7764, line 11. Why is the latitude dependent biomass valid in Finland? A reference would be quite necessary.

pg 7765, line 26. How good approximation is the use of Hyytiala measurements for the inorganic species? How much do they change even in Hyytiala in time? Are the

ACPD

4, S2837–S2840, 2004

changes in SO2 significant? If the temporal changes in (especially) SO2 concentrations are large the approximation of constant concentrations would require some explaining.

Chapter 3.1: How typical are the concentrations in the modelled days for the measurement stations? Are they representative for the usual case and is the concentrations how much dependent of the time of the year.

Chapter 3.2 First para: How was the inital ratio of organics etc. chosen? Some sort of reasoning behind the choice would be necessary. If the values were educated guesses, how sensitive the results were for the them?

Chapter 3.3.3: Authors first claim that dry deposition is important, but then in next sentence explain that in their model studies it had basically no effect at all. Even an explanation is in given in conclusions, there would have been nice to have some text in this chapter also. Are the authors implying that dry deposition does not have significant effect on aerosol concentrations in comparision to wet deposition?

Chapter 3.3.5 last para: "..the mass concentration was not significantly changed by this". The coagulation does not directly change mass concentration at all. Does even the little change come from the difference in the condensation rates? How large is the mass change overall in the absence of coagulation?

Authors had chosen only clear sky conditions for the modelling effort. The reasoning behind this is quite clear for some readers, but more explanation of the for this would have been appropriate. I would also liked to see in the paper some discussion on the direction of the results if the sky would have been cloudy (emission rates etc) and how often the sky is really this clear in the measurement stations and in the atmosphere between them..

A small table or a nice list of the modelled days would be useful (their dates, average temperatures, other key species average concentrations). This could help in future comparisions with similar model runs. Easiest would be to include it in table 2.

ACPD

4, S2837–S2840, 2004

Authors have claimed that their aerosol model uses Nano-Koehler approach of Kulmala et al 2001. The nano-Koehler theory is very sensitive of the organic species properties, as well as concentrations. The authors had tested the sensitivity of this model run with different concentrations of single model organic, but I would have liked to have some discussion on the properties of the organic species. Especially as they have seemed to use density of 1.5 g/cm3 for the modelled organic, which I would like to see the reasoning of the use for.

Authors have not used any primary emissions in the model run(s). By using commonly available emission databases (e.g. from EMEP) they could have used them in a comparision case as a crude estimate of the emissions. How good do the authors think is their approximation of no sources during the transport? Order of magnitude analysis would be sufficient for this.

Technical Corrections:

Figure 4 suffers from quite hard-to read y-axis (too low number of ticks 3 lower ones).

In figure 9 second subpicture from the top: Legend is on the picture.

Also in fig 9: Use some sort of indices showing which case is which picture (even though it should be evident, it makes reading much easier)

Interactive comment on Atmos. Chem. Phys. Discuss., 4, 7757, 2004.