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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Multicomponent aerosol dynamics model UHMA: model development and validation" *by* H. Korhonen et al.

Anonymous Referee #1

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General comments:

This manuscript introduces a new size-segregated, multi-component aerosol dynamics model to be used in atmospheric applications. The model includes the basic aerosol dynamical phenomena and has options for three different size distribution representations. The model framework is also very flexible for future developments. The manuscript is original, clearly written and well organised. In my opinion, the manuscript can be published in Atmospheric Chemistry and Physics after careful consideration of the points mentioned below.

Specific comments:

Page 5, section 2.2.4: The model includes dry deposition, which is essential when applying the model in zero-dimensional boundary layer simulations such as that per-

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formed in section 3.2. In such simulations, it would be equally if not even more important to include the detrainment of particles from the boundary layer (dilution) or entrainment from above the boundary layer. An option for entrainment/detrainment could easily be embedded into the existing model framework.

Page 5, section 2.3: The authors have applied 3 different numerical methods when presenting the time evolution of the particle size distribution. Although relatively well explained in the text, the basic differences between these presentations would be more easy to understand, especially by non-expert readers, if the presentations were illustrated by a simple figure.

Page 6, section 3.1: While the overall discussion on the performance of the size distribution presentations can be considered quite satisfactory, there are a few issues that could be commented in this section. First, are the problems associated with the different methods specific for the nucleation mode, or are they equally important over the whole size range? Second, it seems that the early growth of nucleated clusters is particularly difficult to model accurately. Would the situation be improved if there were effective and numerically accurate parameterization not only for nucleation but also for the early growth of the nuclei? Third, the authors have assumed an equally-spaced grid over the whole particle size range. Could the performance of any method be improved by changing the grid spacing such that it would be denser at some specific size regime? Fourth, only one test case have been presented. Are obtained results equally valid for other initial conditions, or are they dependent on the system concerned.

Page 7, section 3.2: The comparison between modelled and measured nucleation event should be made a bit sharper. First, what is basis for chosen condensable vapour concentrations and their diurnal profiles?. Are they some sort of "best guesses" or have they been optimised to reproduce the measurements as well as possible? Second, it is stated that the model reproduces the measured distribution well. In my opinion the agreement between the model and measurements is qualitative rather than quantitative, especially at sizes below 10 nm. Third, it seems that the measured air mass if

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affected by some sort of dilution during the first few hours. How would the inclusion of this phenomenon affect the simulation results? Fourth, the authors could explain the reason behind low organic volume fraction of the smallest detectable particles shown by Figure 5.

Technical corrections:

Abstract and conclusions: it is said that the used numerical methods can deal with "transport". To avoid potential confusion, the authors might be more specific and explain that they mean "atmospheric transport" between the different grid cells.

Introduction, first paragraph: In the submicron size range, primary organic matter is frequently more important than secondary organic matter. Please add primary organics into the list.

Page 4, the last full paragraph: What is the reasoning behind using the ammonia concentration of 0.1 ppt as a border between binary and ternary nucleation?

Page 6, second full paragraph on the right: organics with a saturation vapour concentration of 10E6 molec/cm3 are traditionally called "non-volatile" rather than semi-volatile.

Interactive comment on Atmos. Chem. Phys. Discuss., 4, 471, 2004.