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Geoscientific
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Discussions

Interactive comment on "Meteorological and trace gas factors affecting the number concentration of atmospheric Aitken (D_p=50 nm) particles in the continental boundary layer: parameterization using a multivariate mixed effects model" *by* S. Mikkonen et al.

Anonymous Referee #2

Received and published: 18 October 2010

General Comments:

A statistical parameterization to predict the number density of 50 nm particles over Europe has been developed as a function of time, relative humidity, SO2, NO2, O3, temperature, condensation sink and non-new particle formation days. The rationale is that 50 nm particles can act as cloud condensation nuclei (CCN),and that detailed microphysical models to predict their concentration are computationally intensive rela-

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tive to this parameterization. The paper addresses a modeling problem appropriate for GMDD.

While an interesting treatment, the main problem I had with the paper is the assumption that since 50 nm particles can act as CCN, they do act as CCN. Clearly this is not always the case, depending on meteorological conditions, pre-existing aerosol number concentration, and hygroscopicity of the aerosol (which the authors admit is highly variable at 50 nm). With >50 nm aerosol concentrations as high as 3000 cm-3 at the polluted sites in their data set, it is unlikely that 50 nm particles will activate under many of these conditions. In fact, they are likely to be more important as CCN in clean sites. While I am sure the authors recognize this, the terminology used in the paper slips from specifics of the 50 nm measurements to “CCN concentration” in the Results section. I would suggest calling these “potential CCN” in this section. Additionally, some estimate of the expected supersaturations reached in low level clouds in the regions of study should be made. How these compare with the critical supersaturations of 50 nm particles at the different hygroscopicities measured would aid in understanding how relevant the parameterization for 50 nm particles really is for cloud formation. I agree with Referee 1 that parameterizations for other (larger) particle sizes would be of interest as well.

The rest of the paper is well written, but there are a number of minor points where terminology or details are not well defined, as listed below.

Specific Comments:

p 1188, line 1: “these new particles” are invoked without defining them earlier in the paragraph; earlier the discussion is on atmospheric aerosols in general rather than newly formed particles. p. 1191, line12: NPF days are listed here, but the definitions of event and non-event days are not given until the next section (2.2). Recommend rearranging things here for clarity, defining the terminology first. p. 1193; line 1: insert (CS) after “condensation sink”. p. 1194, line 9: What is the bin size used for N50? p. 1196, Eqn 3:Please clarify utime. Does this refer to time of day? Because based on Fig. 4, time of day is an important variable where the model does not seem to reproduce the variation very well. And a related question, was some measure of solar radiation specifically used in the parameterization? p. 1198, line 1-2: I don't understand how SO_2 can be positively correlated "at all sites", but negative at SPC and Melpitz. p. 1198, 2nd paragraph: Wouldn't NO_2 also be a general indicator of more anthropogenic pollution, and therefore correlate with higher N50? p. 1200, line 15; p. 1203, line 25: The authors should define what they mean by "adequate". Adequate for what? p. 1211, Table 3: I am not sure what "multiannual averages" means. Are they several years of measurements averaged for April, in comparison to the model output for April 2000? As the authors admit, the time periods used in the table do not seem to be the same time periods as for the simulation, so can they really say that "the parameterization (test run) significantly improves the agreement with observations at Melpitz and Hohenpeissenberg"? Is there any way of looking at how well variation within certain time periods are duplicated, rather than just using a bulk median comparison?

Interactive comment on Geosci. Model Dev. Discuss., 3, 1185, 2010.

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