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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 #1

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This paper applies multivariate data analysis in order to derive a simple parameterization for atmospheric cloud condensation production associated with atmospheric aerosol formation or primary emissions of very small particles. The parameterization is evaluated over central Europe and compared with large-scale model prediction. The paper is definitely original, and the developed tool might be useful for the scientific community. The text itself is clearly written and well organized. A few issues should be addressed before the paper can be accepted for publication in GMD.

Detailed comments:

Page 1188, lines 18-25. As a rule of thumb, the size limits of 50 and 100 nm may be good estimates for the minimum particle diameter causing the indirect and direct radiative effects, respectively. However, it is well know the effective CCN activation diameter depends on particle size and composition, along with the selected value of supersaturation. As a result, the minimum CCN activation diameter probably ranges between about 50 and 100 nm for most boundary layer clouds. I am not against the selection of 50 nm here, but the authors should explicitly bring up this variability in the text with appropriate literature references. Similarly, it should be pointed out that direct radiative effect start to become important after 100 nm (there is no sharp size cut here). Furthermore, it should be stated whether the authors refer to particle dry or wet diameter, since the latter one is sensitive to the local relative humidity.

Besides atmospheric measurements and model investigations, theoretical frameworks have been derived to investigate the efficacy by which nucleated particle produce CCN in the atmosphere (e.g. Pierce and Adams (2007) Atmos. Chem. Phys. 7, 1367-1379; Kuang et al. (2008) GRL 110, doi:10.1029/2009GL037584, and references therein). This should be briefly mentioned in the manuscript.

The scientific/technical objectives of this paper should be explicitly stated in the Introduction. One of the aims have been mentioned on page 1192 (lines 13-15), but that is too late.

I understand that the analysis performed in section 3.4 was meant as a preliminary test of the performance of the new tool/parameterization in a large-scale modeling framework. Therefore, only a time period of one month and three stations were used for the comparison. I see a potential problem here: since the comparison was made against data from the same locations based on which the parameterization was developed, isn't there a danger of getting a biased (too positive) view on the performance of the parameterization? Could it be possible to run the model for a month in some later year when, for example, more size distribution data associated with EUSAAR or EUCAARI measurements are available? Is it really too expensive to run a model for a few more months, so that the comparison to the measurement data would be easier (there are many more 1-year long data sets). Finally, why to compare only at 50 nm? Both primary and secondary particles grow beyond this size, so additional information on the performance of this parameterization would be obtained if also other sizes were compared (e.g. 80 and 100 nm).

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