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Atmospheric Chemistry and Physics Discussions

Interactive comment on "The role of the particle size distribution in assessing aerosol composition effects on simulated droplet activation" *by* D. S. Ward et al.

Anonymous Referee #2

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This paper consists of two parts: In the first part the authors investigate the sensitivity of the cloud condensation nuclei (CCN) activity with respect to aerosol composition changes by means of Lagrangian parcel model simulations. The aerosol composition effect is treated by using the aerosol hygroscopicity as a single parameter following the κ-Koehler theory of Petters and Kreidenweis (2007). The authors build on previous work by Reutter et al. (2009) and extend their simulations to capture changes in the aerosol size distribution characterized by the geometric median radius of a unimodal aerosol mode. The major finding on top of previous work is that the composition sensitivity of the CCN activity crucially depends on the location of the aerosol critical radius relative the geometric median radius of the aerosol mode. The practical implication of

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this finding is that the previous classification attempt by Reutter et al. (2009) can be misleading under certain conditions.
In the second part of the paper the results of the parcel model studies are used to investigate the sensitivity of a mixed-phase orographic precipitation case to changes in the aerosol composition and size. For the particular case studied the sensitivity of precipitation to aerosol composition changes is found to be small if the aerosol hygroscopicity is varied within the range of observations. Moderate sensitivities comparable to typical changes in the aerosol number concentrations are found only if extreme and unrealistic values for the aerosol hygroscopicity are used. Based on these findings the authors argue that the effect of the aerosol composition on cloud physics can be neglected in cloud regimes with high water supersaturation and rural continental aerosol conditions.

General comments

The paper is very well organized, relatively easy to read and presents interesting and relevant findings that meet the general scope of ACP. In general, the scientific content of the paper is novel and merits publication after revisions.
The authors propose that the aerosol composition effect can be neglected in the particular case they are investigating and imply that the statement can be generalized to other cloud regimes. Given the practical implications of this finding and the proposition made by the authors I would see it as necessary to extend the second part of the paper also to an updraft-limited cloud case.

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Specific comments

1. Section 2.1: Although appropriate references are given in the text some more information on the parcel model could be provided. Typically, parcel models do not consider sedimentation of hydrometeors. The water supersaturation reached in the model also depends on ice-phase processes such as the Bergeron-Findeisen process. The initiation of ice in a cloud in turns depends on heterogeneous ice nucleation. How are these processes treated in the model and how could they potentially affect the results? Some more discussion could be added here.
   It would also be helpful to summarize the parameter range used in the parcel model simulations in a table.

2. Section 2.3: The authors cite McFiggans et al. (2006) and state that the CCN activity is determined by the number of particles and the gradient of the size distribution. Later the authors argue that variations in the geometric standard deviation play a minor role and refer to Antilla and Kerminen (2007). In light of the McFiggans et al. (2006) statement this argument seems counter intuitive. Some clarification is needed here.
   Throughout the study the geometric standard deviation is kept constant at σ=1.8. Why have the authors chosen σ=1.8? I suggest to either add a reference or back up this value with the observations. It would be interesting to see an additional sensitivity by varying the width of the mode within a reasonable range.

3. Section 3.3: One wonders why the authors have chosen an orographic mixed-phase case. From the presentation in section 2.2 one could have guessed a priori that the sensitivity would come out small because an aerosol-limited case has been investigated. With respect to the conclusions drawn from the model simulations I would argue that it is perhaps more interesting to look at an updraft-limited case (e.g., by modifying the initial conditions) and see if the conclusions still hold.

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Furthermore, I would argue that for precipitation from a mixed-phase cloud the composition effect is more important for the ice-phase (e.g., for heterogeneous ice nucleation) than for the CCN activity (see for example Muhlbauer and Lohmann 2009). A short discussion in this direction would be helpful for clarification.

4. P. 4207, l. 4-11: The definitions and explanations of the spillover should appear earlier in the text. E.g., moving the paragraph to p. 4206 after l. 5 seems more appropriate to me.

Technical comments

1. I would suggest to add vertical lines to Fig. 1 to better discern the "updraft-limited" case from the "aerosol-limited" case.

2. For clarification I would suggest to add the annotations "aerosol-limited", "transition regime" and "updraft-limited" to Fig. 2.

3. Fig. 6 and 7 have poor quality. Try using color shading to improve the quality and enlarge the figures.

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 4189, 2010.