Atmos. Chem. Phys. Discuss., 10, C2185–C2190, 2010
www.atmos-chem-phys-discuss.net/10/C2185/2010/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Aerosol effects on deep convective clouds: impact of changes in aerosol size distribution and aerosol activation parameterization" *by* A. M. L. Ekman et al.

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Received and published: 28 April 2010

We would like to express our appreciation to the reviewer for the careful reading of our manuscript and for providing constructive suggestions and comments. In the following section, we give our responses to each of the comments or questions in a format that lists the comments in order and each followed by our corresponding answer.

Reviewer #2

Major comments:

1. *Model: In regard to the model used in the study, it is stated in Section 2 that the ice nuclei (IN) concentration is fixed. What value is used for the simulations and why is this chosen? I am assuming that a fixed value is used so as to totally isolate the effect of increases in the aerosol concentration on activation, however, would it not make more sense to include some simple parameterization of IN as a function of temperature? Moreover,it is also stated in section 2 that all rain drops formed are assumed to have a radius 40* μm*. It is unclear as to whether the size is fixed for all times or if this is just the size that newly formed raindrops take on during the timestep in which they are formed. How sensitive are the results to the choice of 40*μm*? From Rosenfeld et al. (2008), convective invigoration should depend strongly on the autoconversion process.*

Reply: This is appears to be a misunderstanding due to an unclear formulation in the previous version of the manuscript. The number of activated ice nuclei is not constant in the model, it is the number of aerosols available as ice nuclei that is constant (100 cm^{-3}). The number of heterogeneously nucleated ice crystals is a function of temperature according to the parameterization by Cotton et al. (1986). This is now clarified in the new version of the manuscript. Regarding the rain drops, it is only at the time step formed that they have a radius of 40 μm. After they are formed, the rain drop size may change due to condensation, evaporation, etc. The model results are to some extent dependent on the inital radius assumption in that if the rain drops formed are assumed to e.g. be smaller, the droplets may evaporate faster and the sensitivity becomes different. But this is also one of the main points with the study - that the size of the aerosols and thereby the droplets and rain drops formed may impact on the sensitivity of the deep convection. We recognize the fact that a two-moment microphysics scheme has its limitations (cf. also answer to reviewer 1, question 1). However, it should be noted that several of the studies referred to by Rosenfeld et al. (2008) are two-moment microphysics models. We are not claiming that we necessarily

simulate the "truth" using our model. What we would like to point out is that the size of the aerosols and the way the aerosols are treated by the microphysics module (e.g. by wet scavenging) may influence the sensitivity of the model results. This was not clearly pointed out in the previous version of the manuscript and we have modified the model description and the conclusions for clarification.

2. *B) Aerosols: From Figure 1, it seems as if the total aerosol concentration in the lowest 2 km is about 750* $cm{-}3$ *and above 2 km it drops to 100 to 200* $cm{-}3$*. Also, from Section 2.2 we can conclude that these values are reduced to about 375* $cm{-}3$ *and 50-100* $cm{-}3$ *for the medium pollution case and 187.5* $cm{-}3$ *and 25-50* $cm{-}3$ *for the low pollution scenario. At first glance, the values reported for the domain- and time-averaged cloud droplet number concentration in Figure 3 seem very low. However, it is unclear where activation occurs predominantly from the text and figures. From what is provided, it appears as though cloud base is above 2 km and so most of the activation is occurring where the aerosol concentration is lowest within the column. Since the aerosol concentrations are low here, i.e., <200* $cm{-}3$ *for all scenarios, one would expect most particles to activate. Figure 3 corroborates this statement. However, the study of Rosenfeld et al. (2008) showed that convective invigoration due to an increase in the aerosol number concentration is expected to peak when the aerosol number concentration is around 1200* $cm{-}3$*. Additionally, Rosenfeld et al. (2008) show that as the aerosol number concentration approaches 100* $cm{-}3$*, the invigoration becomes negligible. This might explain why there are "relatively small differences in convective strength obtained for all sensitivity simulations ... " in this study. This should be addressed in the manuscript by either providing additional details in Section 2.2 and the conclusions, or by performing an additional set of simulations with higher aerosol concentration (e.g., 200% of the high case).*

Reply: The time- and domain-averaged cloud droplet number concentrations in Figure

3 are for the whole model domain, i.e. $400x400km^2$. The liquid part of the cloud covers less than $100x100km^2$, which is why the averages in e.g. Figure 3 are so low. The activation of cloud droplets actually starts already at the 2nd model level, i.e. at 800m, where the aerosol number concentration is the highest. So the sensitivity runs actually do cover the range of aerosol concentrations where the sensitivity should be high (according to the Rosenfeld et al., 2008 paper). We have added text in the manuscript in Section 3.1 clarifying this. In addition, we have added figures for the time development of the cloud droplet number concentration (cf. answer to Question 3 below).

3. *Figures: In general, the quantity and quality of the figures is lacking in the manuscript. In particular, I found that the second paragraph of Section 3.1 would be more understandable if it were accompanied by a figure of the cloud droplet number concentration and the liquid water content as a function of height. Moreover, a figure portraying the graupel mixing ratio and mean updraft velocity as a function of time would clearly show the results discussed in Section 3.4. As alluded to above, the paper lacks information regarding the vertical structure of the environment (e.g., initial temperature and moisture profiles, mean profiles of the condensed mixing ratios, etc.) The only vertical information that we are provided by the authors is that of a domain averaged temperature increase/decrease in Figures 5 and 6. It is not clear however from the captions if these changes in temperature are due to latent heating, advection, shortwave warming, etc. Lastly, these figures show a decrease in temperature above 8 to 10 km in many simulations. Is this in any way related to changes in cloud top height and thus changes in condensed water mass?*

Reply: According to the reviewers' suggestion, we have added figures in the manuscript showing (for all simulations) the time development of mean updraft velocity, cloud droplet number concentration and graupel amount.

Minor comments:

A. *Lines 14 to 16 on page 6343: There are no references listed for the studies performed that show a decrease in precipitation with an increase in the aerosol number concentration.*

Reply: Ekman et al. (2007) and Rosenfeld et al. (2008) show examples of both increasing and decreasing precipitation rates with increasing aerosol concentrations. This has been clarified in the new version of the manuscript.

B. *Last paragraph: It is stated in the introduction that Fan et al. (2009) show that under weak vertical wind shear, aerosol effects on deep convective clouds are larger than for strong vertical wind shear. The last sentence states the opposite.*

Reply: This appears to be a misunderstanding. In the manuscript, it is stated in the introduction that under weak vertical wind shear, there is first an increase in latent heat release that invigorates the convection up to an optimal loading. After this, the convection is suppressed. For strong vertical wind shear, the convection is always suppressed with increasing aerosol concentration. It was not stated that the effect (invigoration or suppression) was larger under weak vertical wind shear. In absolute terms, the effect of increasing aerosol concentration is larger when vertical wind shear is strong. In the new version of the manuscript, this has now been clarified.

3. *Throughout the manuscript: Köhler is used first on line 17 of page 6345 and is then used throughout the remainder of the paper in a different form, namely "Koehler".*

These should be changed for consistency. Moreover, the names of the simulations are defined in Section 2.2, but from there on many of the names are given backwards, e.g., aero-koehler becomes koehler-aero. These should also be changed so that the names are consistent throughout the manuscript.

Reply: This has been corrected in the new version of the manuscript.

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 6341, 2010.