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Atmospheric
Chemistry
and Physics
Discussions

Interactive comment on "Cloud-system resolving model simulations of aerosol indirect effects on tropical deep convection and its thermodynamic environment" *by* H. Morrison and W. W. Grabowski

Anonymous Referee #1

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The present paper describes an interesting and insightful study of aerosol effects on convective clouds, radiation and precipitation in an idealized framework of convective radiative equilibrium. Cloud-system resolving ensemble model simulations are performed with a 2D anelastic model driven by time-dependent forcing functions for six consecutive days of a monsoon period. The forcing functions are derived from TWP-ICE observations.

The major findings and conclusions of the paper are: First, domain averaged surface precipitation is relatively insensitive to aerosol perturbations because the water budgets for the investigated cases are largely driven by the prescribed large-scale forcing

and latent heat fluxes at the surface. Second, large differences in the domain-averaged shortwave and longwave TOA radiative fluxes are found in individual ensemble realizations and are a result of random fluctuations inherently linked to the low predictability of convection. Therefore, ensemble simulations are necessary to robustly estimate aerosol indirect effects in cloud-system resolving simulations. Third, cloud top heights and microphysical properties of convective anvils are sensitive to aerosols with generally higher and optically thicker anvil clouds in the polluted simulations. This result is in line with many other studies but the interesting aspect is that the higher anvils are a direct result of the homogeneous freezing of increased droplet number concentrations (due to increased aerosol number concentrations) rather than a dynamical invigoration of convection through latent heating.

The paper is scientifically relevant and meets the general scope of Atmospheric Chemistry and Physics. The discussions in the manuscript are clear and concise. Thus, I would recommend publication of the manuscript after minor revisions.

Below are comments and technical corrections:

Minor comments:

1. P. 3, l. 13: Most of the discussion in the introduction reflects the effect of aerosols on warm-phase collision/coalescence only. However, for some cloud types and especially at colder temperatures collision/coalescence is not the dominant microphysical process (in terms of generating precipitation) and compensation through other microphysical pathways may occur. Since the authors find that ice microphysics is crucial in their simulations some discussion on aerosol effects on mixed-phase clouds (e. g., ice nucleation) would be good to precondition the reader.

2. P. 4, l. 7: Rephrase sentence: . . . "have suggested that aerosols can either invigorate or weaken convective cloud growth depending on ...".

3. Fig. 14 shows that the convective mass flux decreases with increasing aerosol

number concentration as does the percentage fraction of convective updrafts. However, the decrease in the fraction of convective updrafts seems to be stronger than the decrease in effective mass flux. This would imply that the vertical velocities in individual updrafts could be higher in the polluted case than in the clean case because a similar mass flux must be maintained through a smaller number of updraft cores. Often, in cloud resolving modeling studies comparison of updraft velocities (between clean and polluted runs) are performed by comparing the pdfs of updrafts in terms of vertical velocity rather than mass fluxes. So, how much of the discrepancy found in this study and discussed on p. 22 is due to the use of a different metric? Please clarify.

4. P. 25: As mentioned by the authors, crucial controls on the outcome of the presented simulations are the microphysical properties of the anvil cirrus and the microphysical mechanisms leading to freezing of supercooled liquid water. It seems that the faith we can put into these simulations will hinge on the confidence we have in understanding ice nucleation (especially the role of heterogeneous ice nucleation versus homogeneous ice nucleation in deep convective clouds). If some of these supercooled cloud droplets would freeze more effectively and earlier through a heterogeneous nucleation mechanisms than the sensitivity of the simulations to the anvil cirrus microphysics would presumably be much smaller. Also, ice crystal number concentrations would be constrained by ice nuclei rather than cloud droplets, which would lead to lower ice number concentrations, larger ice crystals and larger sedimentation velocities. Maybe an additional sensitivity study could clarify the role of heterogeneous vs. homogeneous freezing.

5. P. 26, l. 20: The authors argue that the sensitivity tests with respect to domain resolution give little difference when the grid spacing is reduced from 1 km to 0.5 km. I wonder if this is because horizontal advection of cloud variables have been neglected in the model. The significant change in results from 4 km to 2 km may be because the higher resolution simulations give a better representation of the large-scale convection, which is no longer improved at higher resolutions. However, at higher resolutions horizontal transport of (sedimenting and non-sedimenting) hydrometeors clearly becomes more important and should not be neglected.

Technical corrections:

1. P. 2, l. 3: Replace “cloud system-resolving” with “cloud-system resolving” here and elsewhere in the manuscript 2. P. 3., l. 8: Add “stratification” after “atmospheric” or rephrase sentence. 3. P. 4., l. 17: Remove “is” after “response”

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 15573, 2011.