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Comment on acp-2021-917

Anonymous Referee #2

Referee comment on "Examination of aerosol impacts on convective clouds and precipitation in two metropolitan areas in East Asia; how varying depths of convective clouds between the areas diversify those aerosol effects?" by Seoung Soo Lee et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2021-917-RC2, 2022

Effect of CCN concentration on convective precipitation formation in two case studies is presented. State of the art cloud resolving model with detailed representation for cloud microphysics is employed. The analysis highlights the role of aerosol in affecting the formation of heavy precipitation on both studied locations. In the Beijing case the analysis related to changes in surface precipitation is interesting. Especially how precipitation pattern changes towards stronger precipitation. I guess the computational cost of the simulations is relatively high, and thus ensembles with more variability in the aerosol properties cannot be employed to make the analysis stronger. Based on one pair of simulations, the uncertainty related to the overall strength of processes is high as small change in initial conditions could affect the relative aerosol effect a lot. However, how different processes and feedbacks work needs to be analyzed in more detail before I can recommend the manuscript to be accepted to be published in ACP.

It seems like the changes in condensation rate are now presented as the only reason for the increased updrafts. It goes also in opposite as stronger updrafts automatically increases also condensation rate. Increase in the aerosol concentration followed by increase in droplet concentration should not produce very significant change in the condensation rate as droplet concentration are expected to be relatively high even in the low aerosol simulations. Presenting the droplet number concentration would be thus highly beneficial. I would expect it is more likely that delayed formation of precipitation in warm processes is affecting more on liquid water content than the increased condensation through enhanced cloud droplet formation. This could also affect cloud radiative properties. Also, there is no discussion on how much column averaged condensation rates are affected by the change in the fraction of cloudy grid boxes or how for example the cloud top height is changing due to aerosol changes and cloud invigoration. The increase in condensation rate can be directly connected to increase in the precipitation like is done in the manuscript, but connecting the change in condensation rate to increased droplet number concentration requires more evidence. For example, what would happen if you just increase the condensation coefficient in low aerosol case?

In the abstract the statement "In both of the areas, aerosol-induced changes in freezing play a negligible role in aerosol-precipitation interactions as compared to the role played by aerosol-induced changes in condensation and deposition." is quite strong as the number of ice particles is not affected by aerosol. Thus, through the manuscript, I suggest changing the wording to be about the change in CCN instead the change in aerosol.

The analysis of gust fronts is somewhat separated from the rest of the analysis. What is the role of droplet evaporation through the simulations in driving the instability? Especial when compared against the enhanced condensation from increased droplet concentration?

Specific comments:

Line 59: "With increasing aerosol loading or concentrations, cloud-particle sizes and autoconversion, which represent cloud microphysical properties, can be changed." Autoconversion is not a microphysical property but a parameterized process representation to create precipitation from cloud droplets. As you have binned microphysics employed, I would expect there is no need for the autoconversion.

Line 192: "based on the fact that aerosol composition does not vary significantly over the domain and during the whole period with the observed clouds." How do you know this? AERONET does not provide data in cloudy conditions.

Line 202: Aitken mode, not nucleation

Line 223: IN concentration seems high, what is the temperature dependence for heterogeneous nucleation and what is the actual ice number concentration in the simulations?

Line 241: What happens to aerosol from evaporating hydrometeors? If aerosol is recovered, information is lost, and I would not say that evolution is followed.

Line 280: "However, background aerosol concentration acting as INP at each time step and grid point in the low-aerosol-s run is not different from that in the control-s run during the simulation period." This is a very surprising selection as throughout the manuscript the effect of aerosol on precipitation is discussed. Please change the wording and describe in more detail the temperature dependence of IN concentration.

Line 320: What is the total number of stations? Agreement with observations seems to be very good. Would it be possible to add the locations of stations into maps to see how well those cover the simulated area?

343: What does “cumulative frequency distributions of precipitation rates” mean. How is rain rate cumulated. Also, more information is needed on the process used to extend the observations to fill the simulated domain.

402: The number of cloud droplets formed should be presented to see how high concentrations can be found from the clouds to estimate the formation efficiency of warm precipitation.

403: Are the differences in condensation rate meaningful to produce changes in updrafts? The increase in the condensation rate is five order of magnitude smaller than the change in liquid water content, thus being much longer than the lifetime of a single convective cell. Or is the main reason for increased liquid water the decrease in the precipitation formation efficiency which together with an increase in the updraft mass flux increases the amount of liquid water at higher altitudes also. This effect is not analyzed at all. I would expect the droplet concentration even in the low aerosol case to be high enough to maintain the supersaturation with respect to liquid water very close to zero.

Line 422: It is surprising that differences in freezing rates are so small. Does it include also the ice/snow formed in collisions between liquid and frozen hydrometeors? Delayed warm precipitation formation should increase the liquid water content in mixed phase part of the cloud and thus provide more liquid for freezing also. Overall, can you estimate how big fraction of precipitation is formed through cold and warm processes and how it is changing with changing CCN concentration?

Line 658: Binned microphysics is employed, so there should not be a need for autoconversion as cloud droplet grow through coagulation coalescence to precipitation?

Lines 673-763: This is very interesting analysis and the change in the energy flow seems quite strong. The static energy itself is probably only slightly affected by aerosol (or is there more water in the PBL), so the change comes from the wind vector or how the averaging is done. As the selection of limiting value for areas A and B is quite arbitrary but still affecting the size of A and B areas, I would expect that the differences between different aerosol cases change when different criteria for areas A and B is employed. And thus, averaging affect the outcome. Maybe using net flux instead of domain averaged values could make this more convincing.

Figure 8: Is it only snow here, or does it include all frozen hydrometeors? Also, the change in cloud fraction should be presented as all variables are presented area averaged. In addition, information related to temperature profile, at least the melting and

heterogeneous freezing levels would help in understanding the changes at different altitudes.

Figure 9: This is complicated figure, and it is difficult to see what happens. Maybe the quality good be improved through decreasing the number of panels.