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Comment on acp-2021-917

Anonymous Referee #1

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-RC1, 2021

This paper presents two cases studies of deep convective cloud systems. The authors perform simulations with a cloud-system resolving model to determine the impacts of increased concentrations of cloud condensation nuclei on these cloud systems, most specifically precipitation. The authors' analysis provides a valuable contribution to understanding aerosol-cloud interactions in deep convective cloud. However, some important details are missing from sections 2 and 3 of the manuscript. I discuss these in more detail below. I recommend the manuscript for publication, provided that my following concerns are addressed.

General Comments:

The authors do not currently provide the origin of any of the meteorological information presented in Sect. 2 or Fig. 1. I assume that this is sourced from a reanalysis. The authors must specify which reanalysis this information is sourced from and credit it appropriately (usually, with a citation). If some of the statements in this section are sourced from local observations, these should also be credited appropriately.

In multiple locations in the paper, the authors state that observations were interpolated and extrapolated to the model domain, without giving the method used. Given that there multiple valid methods of interpolating such data, as well a several methods that would be wholly inappropriate for this study, the authors should specify how the interpolation and extrapolation was done.

For both case studies, differences in snow mass based on aerosol concentrations are discussed, but differences in hail and graupel are not mentioned. Is this because the differences are insignificant? Or was the mass of graupel and hail insignificant in all cases

- in other words, all frozen water mass took the form of snow? Please specify this in the text.

The authors should consider whether it is necessary to show the full time evolution of the Soeul case, or whether they feel that some subplots of Figures 7, 8, and especially 9 can be moved to a supplement.

Technical comments:

In this study, the modifications in aerosol concentrations only affect clouds through their role as cloud condensation nuclei (CCN); direct effects of aerosol on radiative transfer are neglected and ice-nucleating particle (INP) concentrations are held constant. This is a fine experimental approach, and I don't wish to increase the scope of the paper. However, in both the abstract and conclusions, the authors never use the term CCN. I request that the authors change at least one use of "aerosol" to "cloud condensation nuclei" in both the abstract and the conclusions in order to make the focus of the paper more clear to a time-constrained reader.

Please also include the name of the model used in the abstract. This will help other researchers using the same model and researchers interested in comparing results between models to find your research.

p2 line 29: has -> have

p2, lines 65-66: The first half of this sentence currently sounds like there is a decrease in cloud liquid which is not the case. Perhaps the authors should rephrase this as "...less cloud liquid forming raindrops…"?

p5, line 124: Where is the precipitation rate recorded? What is the source of this statement?

p6, line 164: Brown et al. (2012) does not appear in the reference list.

p6, lines 178-180: Is there a reasoning behind this assumption? Specifically, is there a reason to assume that aerosol acting as CCN is larger over Beijing than Seoul?

p7, lines 189-190: Why were these proportions chosen for the two sites?

p7, line 197: absorbers -> absorber

p7, line 205: “with”: do the authors mean within?

p7: Please give the details of the three aerosol modes for each case: number, median diameter, and geometric standard deviation. It might be most appropriate to give the number normalised by the PM2.5 or PM10 mass. How were they chosen? Are these fits to the AERONET data?

p7, line 216: The aerosol decreases exponentially, but with what exponent?

p8, lines 212-213: Based on the previous text, I thought that the relative size distributions and aerosol compositions were held fixed for each case. Therefore, only the aerosol number concentrations should need to be interpolated or extrapolated, right? Additionally, how was the extrapolation/interpolation done? Are concentrations linearly interpolated and extrapolated?

p8-9, lines 241-245: I find this sentence very confusing. The previous description by the authors seems to make it pretty clear that the background aerosol concentrations is a diagnostic field. For example, lines 218-220 “Once background aerosol properties (i.e., aerosol number concentrations, size distribution and composition) are put into each grid point and time step, those properties at each grid point and time step do not change during the course of the simulations.” However, the phrasing of this sentence suggests that aerosol transport or advection is a process explicitly simulated by the model. Are the authors trying to say that, because the out-of-cloud aerosol concentrations are derived from observations, their spatial patterns and temporal evolution will mimic advection that occurred in reality during the case study time period? Please clarify.

p10, line 300: gird -> grid

p11, line 319: Do the authors have a reference for the AWS?

p12, lines 346-348 and Figure 6: why are the observations interpolated and extrapolated? How are they interpolated and extrapolated? Linearly? Why isn’t the model subsampled to the times and locations of the observations?

p12-13, lines 365-367: The authors should be more precise here. For what range is the difference between control-s and low-aerosol-s greater than a factor of 10? To my eye, this does not seem to occur below 11 mm h-1.

p17, lines 497-498: The authors should be more precise here. They should use the greatest whole number for which the statement is true, instead of 12 mm hr-1. From looking at Fig. 6b, the two precipitation frequencies don’t seem to differ by a factor of 10 for precipitation rates less than 27 mm hr-1.

p17, lines 517-520: This sentence is confusing. It sounds like the authors are saying that the distinctive pattern (control-b greater for precipitation rates <2 mm hr-1 or >22 mm hr-1, low-aerosol-b greater for precipitation rates between 2 and 12 mm hr-1) is emerging at this time. However, they already stated that the pattern started to emerge at 17:00. Are they authors simply saying that the differences between the two simulations have become more pronounced? Are they trying to state that control-b becomes greater for precipitation rates <2 mm hr-1 at this time, while the relationship between control-b and low-aerosol-b is unchanged for greater precipitation rates? Or are they trying to state that the cumulative frequency distribution of control-b has changed from 17:00 to 17:20, while the cumulative frequency distribution of low-aerosol-b remained relatively unchanged during this time period?

p18, lines 541-542: This sentence does not make sense as currently written. I think that this sentence can be simplified to “This leads to more condensation in the control-b run.”

p18, lines 550-551: Why do the authors specify that the differences are at altitudes “with non-zero differences in deposition rates between the runs”? This is not only redundant, it makes the sentence confusing.

p19, lines 553-554: As above, why specify that the differences are where the differences are non-zero?

p23, lines 675-678: see note regarding p24, lines 727-734 below.

p24, lines 716-719: Why divide by the total number of grid cells? If averaging is to be done, it seems more intuitive to average only over cells containing the boundary between areas A and B. An analogous variable would be the cloud droplet number concentration: when an average is taken, typically only cloudy grid cells would be included in the average. I recommend using the total (net) flux instead. The text would be simpler if you discussed the total flux instead, and it would not alter your conclusions.

p24, lines 727-734: This is repetitive with respect to lines 675-678, and with respect to the original discussion of Fig. 15 on pages 20-21. It would be better to instead note during the original discussion of Fig. 15 that the calculation was repeated for the restricted time periods, and the correspondence between the specified condensation rates and precipitation rates were found to be valid for the restricted time periods. Then it would not be necessary to repeat so much text multiple times.

p28, lines 833-836: The authors should either change "aerosol-induced" to "CCN-induced" for this sentence, or add the qualification that this is at fixed INP concentrations. The results may have been different if INP concentrations were reduced by the same factor as CCN concentrations, and this effect would still be an aerosol-induced variation in freezing.

p29, line 863: please remove "the" between "steal" and "more".

Throughout the discussion and conclusions, the authors refer to "strong clouds". Do the authors mean vertically-thick clouds, or high-water-content clouds, or are they using some other metric for strength?

Figure 1: Is the potential temperature shown at the 850 hPa height, like the wind, or at a different vertical level?

Fig. 9 and 13: The wind vectors are not mentioned in the figure captions. Are these at the surface?

Fig. 15: It should be specified in the caption that data from the beginning to the simulation to 17:20 was used for this figure.