First of all, we appreciate the reviewer's comment and suggestion. In response to them, we have made relevant revisions to the manuscript. Listed below are our answers and the changes made to the manuscript according to the question and suggestion given by the reviewer. The comment of the reviewer (in black) is listed and followed by our responses (in blue).

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.

The following is added:

(LL165-168 on p6)

Note that synoptic features in Figures 1a and 1b are based on reanalysis data that are produced by the Met Office Unified Model (Brown et al., 2012) every 6 hours with a 0.11° × 0.11° resolution.

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.

Observation data, such as PM and precipitation data, are extrapolated or interpolated into each time step and grid point. For these extrapolation and interpolation, the inverse distance weighting (IDW) method is used. The method is one of popular ones for the extrapolation and interpolation and detailed at https://en.wikipedia.org/wiki/Inverse_distance_weighting. To indicate the IDW method used, the following is added:

(LL222-224 on p8)

In this study, the inverse distance weighting method is used for the extrapolation and interpolation of observation data including aerosol mass into grid points and time steps in the model.

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.

In Figures 8 and 12, we added differences in the mass of other precipitable hydrometeors (i.e., raindrops and hail particles) between the runs. Accordingly, text is revised. Here, for the simplicity of the display, hail mass include graupel mass in Figures 8 and 12. Also, want to note that snow mass in Figures 8 and 12 includes the ice-crystal mass for the simplicity of the display.

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.

Figures 7b and 7c are removed and become supplementary Figures 1a and 1b. Text is revised accordingly.

Figures 9c, 9d, 9g, 9h, 9i, 9j, 9m, 9n, 9q, 9r, 9w,9x,9y, and 9z are removed and text is revised accordingly.

Regarding Figure 8, we believe that it is needed to keep all subplots, since we think that as seen in text, each of those subplots delivers important information on the evolution of differences in latent-heat processes and hydrometeors between the control-s and low-aerosol-s runs.

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.

We revised the manuscript including abstract and conclusion to reflect points here. See text for details.

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.

The following is added:

(LL57-59 on p3)

These two areas are the Seoul and Beijing areas and the examination has been done by performing simulations using the Advanced Research Weather Research and Forecasting model as a cloud-system resolving model.

p2 line 29: has -> have

Corrected.

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..."?

Corrected following the reviewer's suggestion here.

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

The maximum precipitation rate was observed at a location whose latitude is ~41N and longitude is ~116E. This location corresponds to the north-center part of the domain.

To clarify the observation source of precipitation rate, the following is added:

(LL159-162 on p6)

Here, similar to the situation in the Seoul area, precipitation in the Beijing area is measured by rain gauges in AWSs hourly with a spatial resolution that ranges from ~1 km to ~10 km. The Beijing area is marked by an inner rectangle in Figure 1b and Figure 2b and dots in the rectangle in Figure 2b mark the selected locations of rain gauges.

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

The following is added in the reference list:

Brown, A., Milton, S., Cullen, M., Golding, B., Mitchell, J., and Shelly, A.: Unified modeling and prediction of weather and climate: A 25-year journey, Bull. Am Meteorol. Soc. 93, 1865–1877, 2012.

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?

In Seoul, only $PM_{2.5}$ is available, while in Beijing, only PM_{10} is available. This is why in Seoul, $PM_{2.5}$ is used to calculate aerosol number concentrations, while in Beijing PM_{10} is used to calculate those concentrations.

Most of CCN are in accumulation mode whose radius range is from ~0.1 to ~1 micrometer for both of the Seoul and Beijing cases as shown in Figure 3. Here, the radius range of the accumulation mode is determined by the AERONET data but not by PM data, hence, using PM_{10} in Beijing and $PM_{2.5}$ in Seoul does not mean that CCN is larger over Beijing than over Seoul. Remember that for the AERONET-observed and -determined aerosol size distribution including accumulation mode, PM_{10} or $PM_{2.5}$ is used to obtain aerosol number concentrations.

AERONET data show that at the surface, aerosols are composed of ammonium sulfate and organic compound. Fulvic acid, succinic acid and levoglucosan are the representative components of organic compound according to other studies such as Lance et al. (2005). These ammonium sulfate and organic compound are representative components of aerosols acting as CCN. Hence, $PM_{2.5}$ and PM_{10}, which are measured at the surface, are assumed to represent aerosols acting as CCN.

To clarify the reasoning behind the assumption pointed by the reviewer here, the corresponding text is revised as follows:

(LL242-245 on p9)

Since ammonium sulfate and organic compound are representative components of CCN, it is assumed that the mass of aerosols that act as CCN is represented by PM2.5 and PM10 for the Seoul and Beijing areas, respectively.

Reference:

Lance, S., A. Nenes, T. A. Rissman, Chemical and dynamical effects on cloud droplet number: Implications for estimates of the aerosol indirect effect, J. Geophys Res., 109, D22208, doi:10.1029/2004JD004596, 2005.

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

The aerosol (chemical) composition is determined by data from the AERONET sites. The composition is based on the average AERONET data over the AERONET sites at a time point, which is 1 hour before the observed clouds start to form, for each of the Seoul and Beijing cases. Here, the AERONET data before clouds start to form are used, since it is found that the AERONET data are not available when clouds are present.

To clarify this, the corresponding text is revised as follows:

(LL237-241 on p9)

The AERONET data are averaged over the AERONET sites at 02:00 LST December 24th 2017 (13:00 LST July 27th 2015), which is 1 hour before the observed MCS forms, for the Seoul (Beijing) case. Based on the average data, it is assumed that aerosol particles are internally mixed with 70 (80) % ammonium sulfate and 30 (20) % organic compound for the Seoul (Beijing) case.

p7, line 197: absorbers -> absorber

Corrected.

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

The corresponding text is revised as follows to make it clear based on this reviewer’s comment and to reflect the other reviewer’s comment:

(LL261-264 on p9)

The distribution parameters of the assumed shape of the size distribution of background aerosols in Figure 3a (3b) are those that are averaged over the AERONET sites at a time point, which is 1 hour before the observed MCS form, for the Seoul (Beijing) case.

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?

The parameters of the size distribution, which are median radius, geometric standard deviation for each mode and partition of aerosol number among modes, are obtained by fitting the AERONET data to the lognormal distribution.

As a way of giving the details of number, median radius, and geometric standard deviation as phrased by the reviewer here, the specific numbers of the parameters are given as follows:

(LL256-261 on p9)

Modal radius of the shape of distribution is 0.015 (0.012), 0.110 (0.085), and 1.413 (1.523) μm, while standard deviation of the shape of distribution is 1.28 (1.10), 1.54 (1.63), and 1.75 (1.73) for Aitken, accumulation and coarse modes, respectively, in the Seoul (Beijing) case. The partition of aerosol number, which is normalized by the total aerosol number of the size distribution, is 0.555 (0.612), 0.444 (0.387), and 0.001 (0.001) for Aitken, accumulation and coarse modes, respectively, in the Seoul (Beijing) case.

In the added text above, the aerosol number normalized by total aerosol number but not by PM mass is given, since the size distribution of interest here is about aerosol number but not aerosol mass.

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

The following is added:

(LL271-273 on p10)

With this exponential decrease, when the altitude reaches the tropopause, background concentrations of aerosols acting as CCN reduce by a factor of ~10 as compared to those at the PBL top.

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?

Aerosol composition and the shape of size distribution or size-distribution parameters (i.e., modal radius and standard deviation of each of Aitken, accumulation and coarse modes, and the partition of aerosol number among those modes) are held fixed in each of the Seoul and Beijing cases as the reviewer thought.

PM data are extrapolated or interpolated into each time step and grid point. For these extrapolation and interpolation, the inverse distance weighting (IDW) method is used. The method is one of popular ones for the extrapolation and interpolation and detailed at https://en.wikipedia.org/wiki/Inverse_distance_weighting. To indicate the IDW method used, the following is added:

(LL222-224 on p8)

In this study, the inverse distance weighting method is used for the extrapolation and interpolation of observation data including aerosol mass into grid points and time steps in the model.

Using the fixed aerosol composition and size-distribution parameters and extrapolated/interpolated PM data at each time step and grid point, the number concentration of background aerosols is determined at each time step and grid point. To clarify this, the corresponding text is revised as follows:

(LL264-268 on p9-10)

By using $PM_{2.5}$ or PM_{10}, which is interpolated and extrapolated to grid points immediately above the surface and time steps, and based on the assumption of aerosol composition and size distribution above, the background number concentrations of aerosols acting as CCN are obtained for the simulation for each of the cases.

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.

Yes, as the reviewer stated here, we wanted 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. Stated differently, we wanted to say that since aerosol concentrations, which are derived from observation, are at grid points and time steps of simulations, the spatiotemporal distributions of aerosol concentrations in those grid points and time steps in simulations can mimic those distributions and associated advection that occurred in reality during the simulation period.

The corresponding text is revised as follows:

(LL297-299 on p10-11)

This enables spatiotemporal distributions of background aerosols in the simulations to mimic those distributions that are observed and particularly associated with observed aerosol advection in reality.

p10, line 300: gird -> grid

Done.

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

The following is added in the reference list:

King, J.: Automatic weather stations, available at https://web.archive.org/web/20090522121225/http://www.automaticweatherstation.com/index.html, 2009.

Associated with the reference, the following text is added:

(LL143-145 on p5-6)

Here, precipitation in the Seoul area is measured by rain gauges in automatic weather stations (AWSs) (King, 2009). The measurement is performed hourly with a spatial resolution that ranges from ~1 km to ~10 km.

(LL159-160 on p6)

Here, similar to the situation in the Seoul area, precipitation in the Beijing area is measured by rain gauges in AWSs hourly with a spatial resolution that ranges from ~1 km to ~10 km.

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?

For the extrapolation and interpolation of observation data, such as PM and precipitation data, the inverse distance weighting (IDW) method is used as stated in one of authors' responses above. The method is one of popular ones for the extrapolation and interpolation and detailed at https://en.wikipedia.org/wiki/Inverse_distance_weighting

It is found that whether observation data are interpolated and extrapolated into model grid points and time steps or model outputs are interpolated and extrapolated into observation sites and time points does not affect the qualitative conclusions drawn from Figure 6.

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.

At 11.4 mm hr-1, the cumulative precipitation frequency is 10 in the control-s run and the frequency is 1 in the low-aerosol-s run. To reflect this, the corresponding text is revised as follows:

(LL422-424 on p15)

In particular, for the precipitation rate of 11.4 mm h^{-1}, there is an increase in the cumulative frequency by a factor of as much as ~10 in the control-s run.

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.

It is found that for the precipitation rates of 28.1 and 30.0 mm hr^{-1} each, the cumulative precipitation frequency increases by a factor of ~10 in the control-b run. The corresponding text is revised as follows:

(LL584-585 on p20)

Particularly, for the precipitation rates of 28.1 and 30.0 mm hr^{-1}, the cumulative frequency increases by a factor of as much as ~10.

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?

Here, we are trying to say that as the reviewer stated, the control-b run becomes greater for precipitation rates <2 mm hr-1 at this time, while the relationship between the control-b and low-aerosol-b runs is unchanged for greater precipitation rates.

At the last time step, as seen in Figure 6b, the frequency of comparatively heavy precipitation whose rates are higher than ~12 mm hr^{-1} rises significantly in the control-b run as compared to that in the low-aerosol-b run. Below ~2 mm hr^{-1}, there is also the greater precipitation frequency in the control-b run than in the low-aerosol-b run. Unlike the situation for precipitation rates above ~12 mm hr^{-1} and below ~2 mm hr^{-1}, for precipitation rates from ~2 to ~12 mm hr^{-1}, the control-aerosol-b run has the lower precipitation frequency than in the low-aerosol-b run. In the corresponding text, we want to inform readers of time points when qualitative patterns of these differences between the runs, which are as shown in Figure 6b, emerge. For this, first, we want to say that at 17:00 LST, the pattern of the higher (lower) cumulative precipitation frequency in the control-b run, as compared to that in the low-aerosol-b run, for precipitation rates higher than ~12 mm hr^{-1} (between ~2 and ~12 mm hr^{-1}) starts to occur. Then, we want to say that at 17:20 LST, the pattern of the higher cumulative precipitation frequency in the control-b run, as compared to that in the low-aerosol-b run, for precipitation rates lower than ~2 mm hr^{-1} starts to occur, while the pattern of the higher (lower) cumulative precipitation frequency in the control-b run, which is established at 17:00 LST, for precipitation rates higher than ~12 mm hr^{-1} (between ~2 and ~12 mm hr^{-1}) maintains as time progresses from 17:00 LST to 17:20 LST. Here, we are not interested in the temporal evolution of absolute values of the frequency in the runs but in when the pattern of higher or lower frequency in the control-b run or in the low-aerosol-b run at specific ranges of the precipitation rates starts to occur.

Based on the argument here, text pointed out here is revised as follows:

(LL601-610 on p20-21)

As time progresses to 17:00 LST, the maximum precipitation rate increases to ~17 mm hr^{-1} and the higher (lower) cumulative precipitation frequency over precipitation rates higher than ~12 mm hr^{-1} (between ~2 and ~12 mm hr^{-1}) in the control-b run than in the low-aerosol-b run, which is described above as shown in Figure 6b for the last time step, starts to emerge (Figure 11b). At 17:20 LST, the higher frequency for precipitation rates below 2 mm hr^{-1} in the control-b run, which is also described above as shown in Figure 6b for the last time step, starts to show up, while the higher (lower) frequency for precipitation rates higher than ~12 mm hr^{-1} (between ~2 and ~12 mm hr^{-1}) in the control-b run, which is established at 17:00 LST, maintains as time progresses from 17:00 LST to 17:20 LST (Figure 11c).

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."

Simplified following the suggestion here.

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.

"with non-zero differences in deposition rates between the runs" is removed. The corresponding text is revised as follows:

(LL654-657 on p22)

Due to stronger updrafts, which are mainly ascribed to more condensation, deposition rates start to be higher at altitudes between ~7 and ~9 km and freezing rates are higher at altitudes between ~4 and ~6 km in the control-b run with the time progress from 15:40 LST to 16:00 LST (Figure 12c).

Also, other text with "with non-zero differences in deposition rates between the runs" is removed and revised.

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

“with non-zero differences in deposition rates between the runs” is removed throughout the manuscript. The corresponding text is revised as follows:

(LL660-663 on p22)

At 17:20 LST, overall, freezing rates are lower at altitudes between ~4 and ~8 km, while overall, snow and hail mass is still lower, and droplet mass is still higher in the control-b run (Figure 12d).

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

Text pointed out here is removed and the following is added in the original discussion for Figure 15:

(LL735-739 on p25)

It is found that this correspondence between condensation and precipitation rates is valid whether analyses to construct Figure 15 are repeated only for a time point at 16:30 LST or for a period between 16:30 and 17:00 LST. These time point and period are related to analyses of the moist static energy as described in Section e 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.

Following the comment here, the corresponding text is revised by replacing the domain-averaged fluxes with total fluxes in text.

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.

Text pointed out here is removed and the following is added in the original discussion for Figure 15:

(LL735-739 on p25)

It is found that this correspondence between condensation and precipitation rates is valid whether analyses to construct Figure 15 are repeated only for a time point at 16:30 LST or for a period between 16:30 and 17:00 LST. These time point and period are related to analyses of the moist static energy as described in Section e below.

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.

The corresponding sentence is revised as follows:

(LL931-934 on p31)

Here, we find that contrary to the traditional understanding, the role of variation of freezing, which is induced by the varying concentration of aerosols acting as CCN but not INPs, in precipitation is negligible as compared to that of condensation and deposition in both of the cases.

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

Done.

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?

As described in Section e, titled “moist static energy”, clouds in area B have greater condensational heating and associated stronger updrafts than those in aera A. Based on these stronger updrafts, we say clouds in area B are stronger than those in area A. Stated differently, clouds in area A are strong and those in area B are less strong. Due to more condensation, condensational heating and associated stronger updrafts, “strong clouds” in area B are vertically thicker with higher water content (or cloud mass) than “less strong clouds” in area A or “less strong clouds” in area A are vertically less thick with lower water content than “strong clouds” in Area B.

In the discussion and conclusions, strong clouds mean clouds in area B and less strong clouds mean clouds in area A. Due to less condensation, condensational heating and associated weaker updrafts in these less strong clouds in area A, these “less strong clouds” are vertically less thick with lower water content than “strong clouds” in area B.

Based on the argument in the above two paragraphs, to classify clouds to “strong clouds” and “less strong clouds”, we use the metric of all of updrafts, cloud thickness and water content.

To clarify points here, the following is added:

(LL899-901 on p30)

note that these strong clouds here involve stronger updrafts via greater condensational heating as described in Section e above and this enables these clouds to be thicker and have higher cloud mass than these less strong clouds.

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

Yes, the potential temperature as well as wind and geopotential height is at the 850 hpa height. Accordingly, the corresponding part of the caption is revised as follows:

Wind (m s^{-1}), equivalent potential temperature (K), and geopotential height (m) at 850 hPa level over Northeast Asia

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

Yes, they are at the surface. This is indicated in the corresponding captions. Also, scaled wind vector is added on the top right corner of each panel in Figures 9 and 13.

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.

The caption is revised as follows:

Figure 15. Mean precipitation rates corresponding to each column-averaged condensation rate for the period between 14:00 and 17:20 LST in the control-b run. One standard deviation of precipitation rates is represented by a vertical bar at each condensation rate.