Interactive comment on "Aerosol Effects on Electrification and Lightning Discharges in a Multicell Thunderstorm Simulated by the WRF-ELEC Model" by Mengyu Sun et al.

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Reply to Referee 3

The authors present a concise study of aerosol (acting as CCN) impacts on electrification and lightning activity in a case study simulation of severe deep convection over Beijing. Regional simulations are performed using the WRF model with a lightning parameterization. Two simulations are performed: a 'polluted' (P) case with CCN concentrations consistent with those observed in the Beijing area, and a 'continental' (C) case with CCN concentrations that represent standard continental conditions (but which still remain high compared to e.g. clean oceanic conditions). The model reproduces the convective event relatively well, with convection occurring slightly earlier in the model than observed. When adjusted for timing of convection, the polluted case has a similar flash count and evolution of the flash count variability to observations while the continental case has a completely different intensity and evolution of lightning flashes, with the onset of lightning occurring earlier than the polluted case but the lightning intensity being much weaker throughout the duration of convection. The authors assess the differences in the lightning activity between the two cases in terms of the cloud microphysics and show that the polluted case has increased amounts of cloud water, suppressed amounts of graupel and increased amounts of cloud ice compared to the continental case and that there are stronger updrafts and downdrafts in the polluted case. The differences in the cloud charge structure are then related to these morphological differences in the cloud microphysical structure, which is used to explain the differences in lightning evolution and intensity between the two cases. The paper is mostly well structured with informative and clear figures, however much of the discussion of the aerosol impacts on microphysical development and processes is speculative rather than supported by thorough analysis. This is a major weakness of the study which should be addressed in a revised version of the paper before it is suitable for publication.

Response:

Many thanks for the instructive and constructive comments. We have studied them carefully and have addressed them in the revised manuscript. Below are the point-to-point responses to the reviewer's comments.

General comments:

1. The manuscript is in need of careful English language editing throughout, particularly in the abstract and introduction. There are too many for a reviewer to spend time providing a full list of typos and language corrections.

Response:

Thanks for this comment. We will improve our text following the general and specific comments below.

2. The paper would benefit from an explanation of inductive vs non-inductive charging mechanisms for the reader familiar with cloud microphysics but not charging mechanisms and lightning. Similarly, discussion of the dipole/tripole charge structure needs more explanation and placing in context of which is more likely to have occurred in the observed case.

Response:

Thanks for this comment. We have optimized the explanation of inductive and non-inductive charging mechanism, and dipole/tripole charge structure in the revised version at several parts. For example, lines 166-168, lines 170-172, lines 406-414.

Lines 167-169: "**Non-inductive (i.e., independent of external electric fields) charge separation resulting from rebounding collisions between various ice-phase particles (ice, graupel, snow, hail)**".

Lines 171-173: "**Inductive charging requires a pre-existing electric field to induce charge on the surfaces of the colliding particles (Mansell et al., 2005)**".

Lines 408-413: "**normal dipole (upper charge positive, lower charge negative; e.g., Thomas et al., 2001) …normal tripole (a dominant region of negative charge with positive charge above and a positive charge below with approximately the same order of magnitude of charge, Simpson and Scrase, 1937; William et al., 1989)**".

3. The authors make many statements about microphysical process differences between the two cases, but do not provide any analysis of these processes or comparison to what would be expected to have occurred in the real observed case.

Response:

Thanks for this comment. We have made comparison of the radar reflectivity as a function of time, hourly peak rainfall rate, and the spatial distribution of precipitation between the observation and both simulations. The comparisons show that the simulated distributions and spatio-temporal development of radar reflectivity and precipitation under polluted condition are in overall agreement with observations. In additional, we added related analysis in several part, for example: lines 251-254; lines 259-277; Figure 5, 6, and 7.

4. The authors state that certain sections of the model domain are excluded from the analysis, but then show many figures in which the microphysical structure of the two simulations are averaged horizontally. Details of this horizontal averaging process need to be given to ensure that they are consistent across all the analysis and appropriate for the particular scientific questions being addressed.

Response:

Thanks for this comment. We have defined "domain" as "**the impacts of aerosol on lightning activity will only be evaluated in the southeastern Beijing area (39.4 °N-40.6 °N, 116.0 °E-117.5 °E, shown in Fig. 4d; here on, 'domain' for short)**" (revised version, lines 256-258) and denoted in the Figure 4d (revised version). And we clarified the details of horizontal average process as follows: "**For each quantity, the mass mixing ratio and number concentration of hydrometeors are averaged over the analyzed region (denoted in Figure 2d) at a given altitude.**"(Revised version, lines 312-314)

Specific comments:

Section 3 (Model overview): the model setup up and boundary forcing should be described (with appropriate references) in the body of the text as well as summarized in a table. More information on the model setup is required: what is the simulation start time, how much spin-up time is discarded from the analysis (if at all), are both nests run without a convection scheme (I believe this is the case?), how do the authors downscale from 1-degree global data to their 6 km nest, what is the geographical coverage of the two model nests and the placement of the 2km nest inside the 6 km nest (a map would help)?

Response:

Thanks for these questions. This comment includes three questions. We will deal with these one by one.

(A) Regard the model setup.

We have added information on the model setup in the revised version (lines 207-220). And yes, both nests run without a convection scheme.

(B) Regarding how to perform nesting technique, the WRF Preprocessing System (WPS, shown in Figure R1) is used to configure real-data simulations. First, we use "geogrid" program to define dimensions and horizontal resolution of domains (here we set the 6 km and 2 km nests). “Geogrid” also provides values for static fields at each model grid point. And then, the “ungrib” program is deployed to read GRIB files (a WMO standard file format for storing regularly-distributed fileds) and extract meteorological fields (Vtables are used to extract fields). Finally, we use the “metgrid” program to interpolate meteorological data (extracted by ungrib) to simulation domains (defined by geogrid) horizontally. More details could be found at: https://www2.mmm.ucar.edu/wrf/users/.

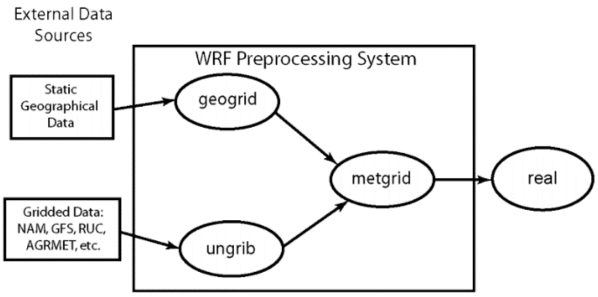

Figure R1 WPS program flowchart

(C) Regarding the geographical coverage of the two model nests

Figure R2 shows the grid domains used in all of the simulations. Since the nested domain setup has been added in the revised version (lines 207-210), we have not put in the revised manuscript.

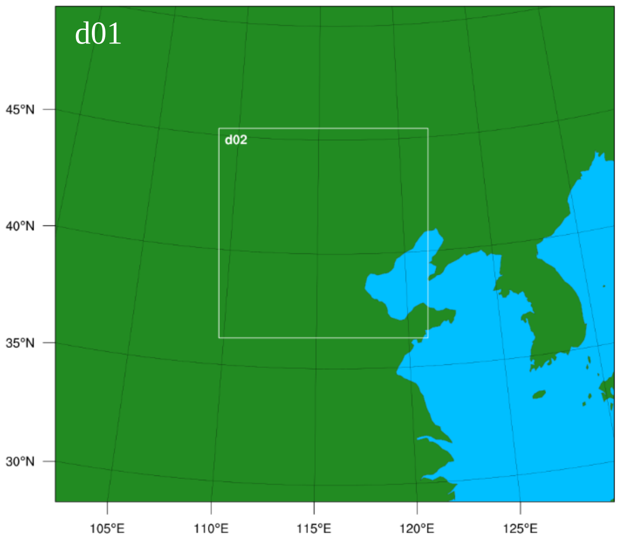

Figure R2 Grid domain used in the simulations

L154: “grids for short” - grids usually refers to the entire set of model grids (nested), not points. I suggest using ‘points’, or just ‘grid points’.

Response:

Thanks for the suggestion. We have changed to “grid points” in the revised version.

L171: “The average value of the observed aerosol concentration before thunderstorm initiation is much higher in the Beijing area” - higher than what? And how does this compare to the two CCN concentrations you have selected as your polluted and continental values?

Response:

Thanks for this question. The hourly average mass concentration of $PM_{2.5}$ on 11 August 2017, and related references have been added in the revised version to clarify. “**The average value of the observed $PM_{2.5}$ concentration before the thunderstorm initiation (more than 110 μg m^{-3}) is much higher than the 3-year mean $PM_{2.5}$ concentration (69.4±54.8 μg m^{-3}) in the Beijing area (Liu et al., 2018).**” And the CCN concentration is selected as the polluted case which is consistent with observation. The initial number concentration for the C-case is consistent with typical continental conditions (e.g. Hobbs and Rangno, 1985; Mansell et al., 2005; Mansell, personal communication, 2019). (Revised version, lines 224-233, Figure 3)

L200-215 (and Fig 4): Does light/heavy/moderate etc refer to lightning density (flashes occurring in terms of number of grid points)? Clarify this in the wording.

Response:

Thanks for this suggestion. Yes, these four lightning intensity categories have been clarified in the revised manuscript. (Lines 291-294, Figure 9)

L233: Is the horizontal averaging performed over the entire model domain or excluding the region in the NW where the convection was different from the observed case?

Response:

Thanks for this suggestion. We have defined “domain” as “**the impacts of aerosol on lightning activity will only be evaluated in the southeastern Beijing area (39.4 °N-40.6 °N, 116.0 °E-117.5 °E, shown in Fig. 4d; here on, 'domain' for short)**” (revised version, lines 256-258) and denoted in the Figure 4d (revised version).

Fig 5: The polluted case has an extra cell (at approx. 09:45 UTC) that doesn’t develop in the continental case. Can you explain why this is and what impact this has on the results? Given that you are forcing both simulations with the same boundary data, this may affect the subsequent development of convection in the P-case compared to the C-case.

Response:

We appreciate this suggestion. In light of this comment, we have analyzed the radar reflectivity and precipitation (Figure 6, 7, revised version) at the beginning stage of the thunderstorm for the observation and both simulations. Since the observed radar reflectivity during 11:00-11:24 UTC is missing, so Figure R3 shows the radar echo before and after 11:15 UTC (09:45 UTC in simulations) for comparison. In the early stage of the thunderstorm, the radar echo for the P-case is relatively similar with these of the C-case. We realized that the area we chose in the initial stage was too small. The extra cell in figure 5j (initial version) probably because that the previous selected area did not include the cell in the C-case. Therefore, we have enlarged the region (shown in Figure 4d, revised version). The microphysics along with further electrification and lightning activities of this thunderstorm have been re-analyzed. The dynamic-thermodynamic processes do affect the development of thunderstorm significantly (e.g. Williams et al., 2005; Guo et al., 2016; Wang et al., 2018; Zhao et al., 2020), the re-analyzed results still suggest that under polluted condition lightning activity is significantly enhanced. Moreover, the precipitation duration is longer under polluted condition (Figure 10c and 10d, revised version), which also provides evidence for the enhancement of convection. With such improvements, we believe the analysis of the physical processes is much appropriate for explaining the difference in the electrification and discharging in both simulated cases.

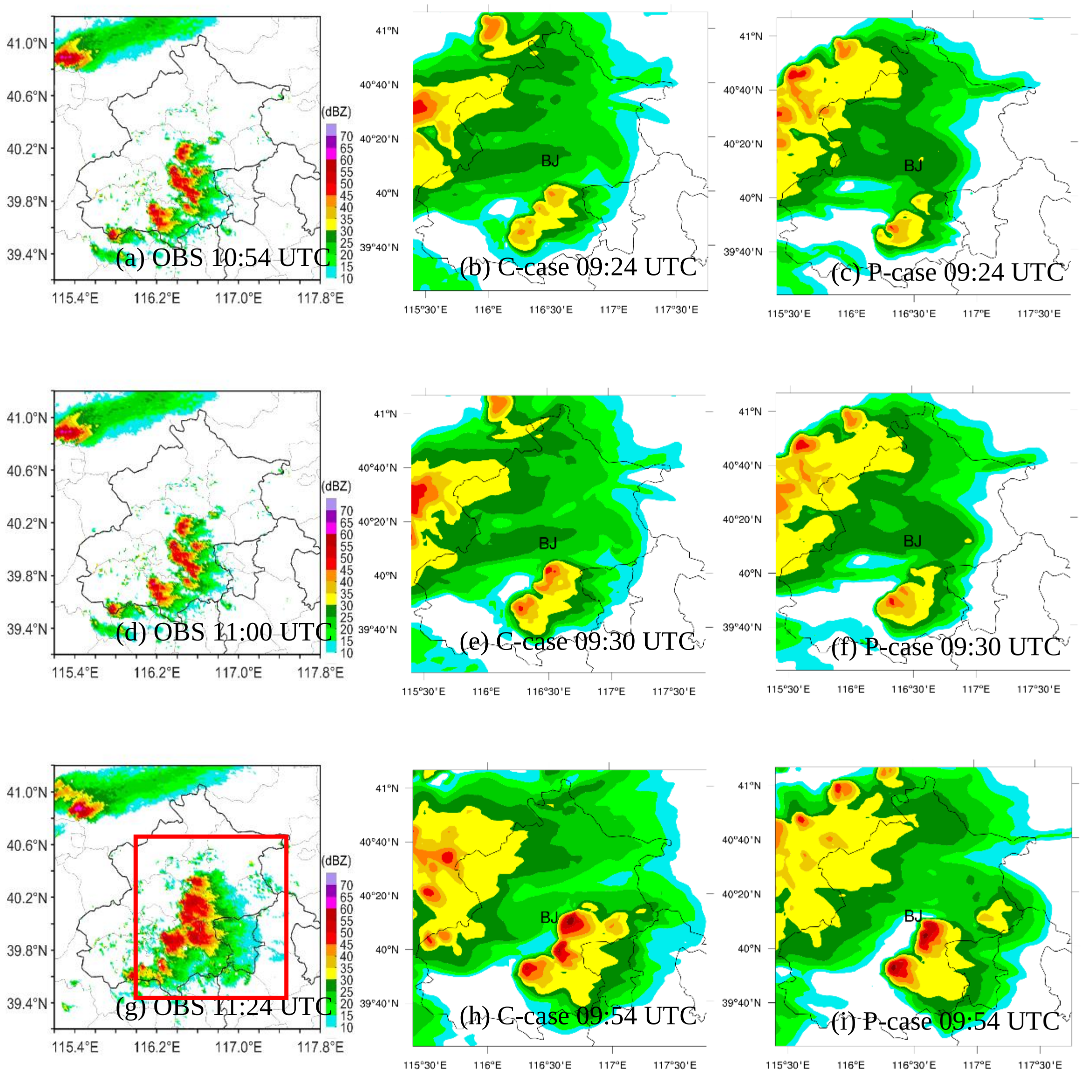

Figure R3 Radar reflectivity (unit: dBZ) between observation and simulation for the C- and P-cases, the simulation was earlier than observation about 1.5 h. (a), (d), (g) Observation at 10:54 UTC, 11:00 UTC and 11:24 UTC. (b), (e), (h) Simulation for the C-case at 09:24 UTC, 09:30 UTC and 09:54 UTC. (c), (f), (i) Simulation for the P-case at 09:24 UTC, 09:30 UTC and 09:54 UTC, respectively. The red rectangle in Fig. R3g denotes the region where the simulated results are analyzed in this study.

L336: “microphysical and electrical processes” - do the authors mean the CCN have a direct electrical impact on the charging, and not just through the impact on microphysics?

Response:

Thanks for this question. This sentence has been modified as follows: “**indicating that aerosol plays an important role in affecting the accumulated charge density through microphysical and further electrical processes**”. (Revised version, lines 447-448)

L350: discussion on convective strength - at the start of this section you say there is no significant difference in convective strength between the two cases, but it was stated in L320 that the P-case has stronger updrafts and downdrafts than the C-case. This contradicts saying “vertical convective strength did not vary significantly under different aerosol conditions”. Similarly at the end of this section you then say there is increased latent heating at upper levels that strengthens convection and enhances lightning activity (L364), which contradicts the first sentence which says there is no difference in convective strength.

Response:

These are good questions and comments. For clarity, we have used the temporal variation of the vertical profiles of peak latent heating (Figure 12, revised version) to make comparison of latent heat between both simulations during the whole duration of the thunderstorm. The latent heat shown in Figure 12 results from both condensation and freezing. For the condensation latent heat, we modified the related analysis in Part “**4.2 Microphysical properties of multicell”,** for example:

Lines 323-329: “**Under polluted condition, more aerosols could be activated into cloud droplets and more water vapor condenses onto these droplets, leading**

to large cloud water content and small droplet size (Lynn et al., 2007; Wang et al., 2011; Zhao et al., 2015; Jiang et al., 2017). Thereby, relatively more latent heat of condensation released in the P-case where large cloud water content exists, which can be seen in the vertical distribution of peak latent heat (Figure 12)."

For the frozen latent heat, the related analysis has been added as follows: "**the maximum of peak latent heat in the P-case occurs above 10 km at 09:30 UTC (Figure 12), indicating that more cloud droplets are lifted to the upper levels (< -40 °C) and converted into ice crystals at the beginning stage of the thunderstorm. … The high value of latent heat existed in the higher levels (above 10 km) reveals a large amount release of frozen latent heat. Previous studies also found that elevated aerosol loading contributed to the increasing frozen latent heat (e.g., Khain et al., 2005; Lynn et al. 2007; Storer et al., 2010; Li et al., 2017).**"(Revised version, lines 349-356)

L445: The authors note here that their study only considers aerosol impacts on this case of convection through perturbations to the liquid phase development of the cloud. One question might then be what are the dominant aerosol sources observed in the case study region, particularly at the time of this event, and whether they are more effective CCN or IN?

Response:

Thanks for this question. As for the aerosol sources, we analyzed the concentration of SO_4^{2-}, NO_3^-, Cl^-, NH_4^+ in the case, which could be treated as effective source of CCN. Black carbon (BC) and organic matter (OM), which are believed to be effective IN (Murray et al., 2010), are also taken into consideration. As shown in Figure R4, the mass fraction of BC in $PM_{2.5}$ is rather small (< 3%), which is consistent with Liu et al. (2020). According to Zhang et al. (2013), soil dust, organic carbon (OC), and BC had similar patterns of waning in the summer. Based on these analysis, we only consider aerosol impacts on lightning acting as CCN. In this case, the proportion of OM in PM_1 is little higher than that of NH_4^+ and SO_4^{2-}. The effect of IN on the microphysical and electrical processes will be considered in our future study.

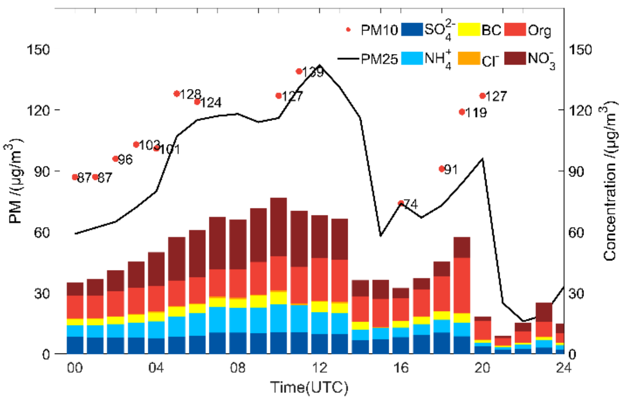

Figure R4 Hourly mass concentration of $PM_{2.5}$, PM_{10} and chemical composition of PM_1 [SO_4^{2-}, NH_4^+, NO_3^-, Cl^-, black carbon (BC), organic matter (Org)] at urban area. The black line represents $PM_{2.5}$, the red dots represent PM_{10}, and the columns represent different chemical composition. The real-time hourly average ground levels of $PM_{2.5}$ are from the China National Environmental Monitoring Center. Other sampling site is located on the tower campus of the State Key Laboratory of Atmospheric Boundary Layer Physics and Atmospheric Chemistry (LAPC), Institute of Atmospheric Physics (39.97 °N, 116.37 °E).

Technical corrections:

Fig 3b and Fig 6 captions: the colours should be noted as well as linestyle. In fact, it would be better to use one of either (a) colour or (b) linestyle to denote the two cases, not a combination of both.

Response:

Thanks for this suggestion. We have modified accordingly.

References

Liu H, Pan X, Liu D, et al. Mixing characteristics of refractory black carbon aerosols at an urban site in Beijing[J]. Atmospheric Chemistry and Physics, 2020, 20(9): 5771-5785.

Liu Z, Gao W, Yu Y, et al. Characteristics of PM 2.5 mass concentrations and chemical species in urban and background areas of China: emerging results from the CARE-China network[J]. Atmospheric Chemistry and Physics, 2018, 18(12): 8849-8871.

Murray B J, Wilson T W, Dobbie S, et al. Heterogeneous nucleation of ice particles on glassy aerosols under cirrus conditions[J]. Nature Geoscience, 2010, 3(4): 233-237.

Zhang R, Jing J, Tao J, et al. Chemical characterization and source apportionment of PM 2.5 in Beijing: seasonal perspective[J]. Atmospheric Chemistry and Physics, 2013, 13(14): 7053-7074.