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Comment on acp-2020-1284

Anonymous Referee #2

Referee comment on "Aerosol effects on electrification and lightning discharges in a multicell thunderstorm simulated by the WRF-ELEC model" by Mengyu Sun et al., Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2020-1284-RC2>, 2021

General comments:

The submitted paper is directed to study the effects of CCN concentration on lightning activity. Although the hypothesis of a link between the lightning frequency and CCN concentration is not new there are still only limited numbers of papers investigating the physical processes responsible for these links. However, the results of these studies are not unambiguous. Therefore, any further analysis in this direction (as is in the submitted paper) is well come for the scientific community.

To reveal the effect of the CCN on microphysics and cloud dynamics and their impact on thunderstorm electrification and lightning, the authors present the results from numerical simulations with two different CCN concentrations: a polluted case (P-case) and a continental case (C-case). The Weather Research and Forecasting (WRF) Model coupled with bulk lightning model and a two-moment bulk microphysics scheme used to simulate a multicell thunderstorm is adequate for the aim of their study. The title corresponds to the content of the paper. The abstract is informative. Many recent and appropriate papers are cited in the Introduction. The paper as a whole is well structure. Most of the figures are of good quality. The analysis of the results of model simulations is directed to reveal the physical processes responsible for the significantly higher frequency of lightning in the P-case, as well as the earlier start of the discharge in the C-case. However, some conclusions are based on assumptions, rather than on detailed analyses of the corresponding numerical simulation results. A more comprehensive discussion, related to the mechanism by which the differences (due to the impact of CCN) in microphysics, thermodynamics, and cloud dynamics affect the electrification of clouds, is required. Additional information, figures and analyses are needed to convince the reader of the validity of the drawn conclusions.

Based on the above I recommend the submitted paper to be **revised** taking into account the specific comments and recommendation below.

Specific comments:

3. Model overview

It is necessary to provide additional information related to the numerical model and design of simulation, and not just direct the reader to the relevant papers.

On Ln 141-143 it is written:

"The initiation of cloud droplets (both for cloud base and in-cloud) uses an expression based on Twomey (1959).

"Please explain a little bit more and give the empirical expression proposed by Twomey (1959). Since the CCN spectra is approximated by power dependence on supersaturation using two time- and location-dependent coefficients, what were their specific values that you used in numerical simulations of C- and P-case? Does supersaturation threshold specified above which CCN number do not increase in the model?

Ln 145-147 it is written:

"Non-inductive charge separation resulting from rebounding collisions between graupel-hail and snow-cloud ice are all parameterized (Mansell et al., 2005)".

Instead of just directing the reader to the relevant paper, it is necessary to give additional brief information about the calculation of magnitude and sign of separated charge at non-inductive interaction of the hydrometeors. It is useful to be noted that the sign of the separated charge based on the results in Saunders and Peck (1998) depends not only on the cloud water content but on in-cloud temperature and on rime accretion rate, which in turn additionally depends on the mean fall velocity of riming ice particles. And it is also not correct that in Mansell et al., 2005 rebounding collisions between graupel-hail and snow-cloud ice are taken into account. In Mansell et al, 2005 it is written *Charge separation rates are calculated for rebounding collisions of graupel with cloud ice and aggregate*" and *"No charge separation is calculated for rebounding collisions between snow aggregates and ice crystals"*. You should mention the rebounding collisions between various ice-phase particles (ice, graupel, snow, hail), which resulting in the separation of charge in the frame of your numerical simulations.

Give also additional information related to the discharge model parametrization – i.e. how

the electric field is simulated, what is the prescribed breakdown threshold, the values of the cylinder cross section C in eq.1, how the net total charge density is altered after the discharge and others. In section 4.5, where you analyze the reasons leading to delay in charge onset in P-case you pay special attention to the area in which the total charge density is $> +0.1 \text{ nC}\cdot\text{m}^{-3}$ or $< -0.1 \text{ nC}\cdot\text{m}^{-3}$. It can be assumed that this is related to some basic assumption at lightning parametrization in the numerical model. If so, it must be also explained in section 3. Model overview.

Please, explain also how the effective radius of various hydrometeors (presented in Fig.7) is calculated. The information on the initial and boundary conditions given on line 166-168, namely: *"The nested model configuration for the simulations are shown in Table 1. The $1^\circ \times 1^\circ$ NCEP GFS (Global Forecast System) data is used to establish the initial and boundary conditions."*

is insufficient. You should specify the starting time and the hourly interval (1h, 3h, or more hours) updates of GFS data used for your numerical simulations. I do not understand why do you use $1^\circ \times 1^\circ$ NCEP GFS data instead of $0,25^\circ \times 0,25^\circ$ (or at least $0,5^\circ \times 0,5^\circ$) GFS data. And please explain, how do you perform nesting technique from a domain with a resolution of roughly $100 \times 100 \text{ km}$ to a domain with a resolution of $6 \times 6 \text{ km}$?

4. Results

Before presenting the detailed analysis of the specific results, it would be useful to give the most general information about the simulated thunderstorms with two different CCN concentrations – at least the moment of the beginning of the formation of both simulated thunderstorms, their life time, the cloud base height, the maximum of cloud top height, the maximum updraft velocity, the amount of precipitation and others.

4.1 Radar reflectivity and lightning flashes of multicell

The presented results in Fig. 2 and Fig 3 reveal that the numerical model used for the study of CCN impact on lightning activity adequately simulates some of the main manifestations of the real observed multicell thunderstorm. However, on the basis of this information alone, it is not possible to draw any other conclusions, including the statement in the last section, namely:

Ln 405-406

"The simulated distributions and spatio-temporal development of radar reflectivity are in overall agreement with observations."

For this purpose, it is necessary to present at least observed radar reflectivity as a function of height in different stages of thunderstorm development with a 1.5-hour delay. And I assume that conclusion would be that the observed radar reflectivity is in agreement with simulated radar reflectivity only in the polluted case.

Since "the impacts of aerosol on lightning activity will only be evaluated in the southeastern Beijing area (116.0°E-117.5°E)" (Ln 193-194)

for clarity, denote in Fig. 2 the regions for which the results are analyzed. Please, explain how the presented in-cloud characteristics were averaged horizontally and in which region the lightning frequencies, shown in Fig.3 have been detected or simulated. It is useful also to specify the average rainfall only in the analyzed region?

Ln 199-202

"According to the evolution of radar reflectivity and lightning activity, the thunderstorm was divided into four periods: the beginning stage (before 11:00 UTC), the developing stage (11:00-12:30 UTC), the mature stage (12:30-13:30 UTC) and the dissipating stage (after 13:30 UTC) of the thunderstorm"

It is necessary to explain what the criteria were for determining the time intervals of the four stages of real and simulated development of a thunderstorm. I do not agree that lightning activity is suitable to be used for this purpose. An idea about the stages of thunderstorm development can be obtained for example on the basis of the evolution of heights of radar reflectivity 5 dBz and 45 dBz, the maximum radar reflectivity with the moment and height of its achievement. That is why I recommend to add such information (or something else) especially for the simulated C- and P-case. Based on chosen criteria, please give information for the corresponding four periods in the simulated C- and P-case and denote on the time scales in Fig.3b, Fig.5 (a-j), Fig.6 (a-d) the intervals of the mature stage in both simulated cases. This will facilitate to follow the analysis of the presented results and the conclusions drawn.

The conclusions made on the basis of the analysis of the results presented in Fig. 4, namely

Ln 217-221 *"There is no lightning initiation for the P-case at the beginning of the thunderstorm (08:30-09:30 UTC, Fig. 4a), however, all categories of lightning are initiated in the C-case, indicating that the first discharging is delayed under polluted condition."*

are not unambiguous. It is necessary to indicate whether the delay of lightning initiation in the simulated P-case is due to the delay in the development of the thunderstorm or if the delay is due to another cause. For that reason, it would be useful if you present at least the evolution of cloud top height in P- and C-case starting from the early moment of thunderstorm formation before lightning initialization.

4.2 Microphysical properties of multicell

In Fig. 5 the vertical profiles of averaged horizontally mass mixing ratios and number concentration for different categories of hydrometeors as a function of time are shown for C- and P-case. No sound conclusions can be drawn for the impact of CCN concentration on cloud dynamics and microphysics on the basis of the horizontally averaged values alone. Information related to the corresponding maximum values, the height and time of their achievements has to be presented. For detailed analysis, it is also necessary to show additionally plots with the maximum values of updrafts and downdrafts as a function of time and height. I assume that the isolines of updrafts and downdrafts presented in Fig. 5 i and 5 j are horizontally averaged values, because they are too low for thunderstorms with an overshooting top, producing lightning. Please discuss also the reason for a gap in maximum radar reflectivity between 9:20 UTC and 10 UTC and between 10:30 UTC and 11:15 UTC in Fig. 5 i. It will be also helpful if somehow the interval of the mature stage on the time scales in any of the plots in Fig. 5 is indicated. Why are there no plots of the mass mixing ratios and number concentration of snow and hail, especially considering that in section 4.3 the charge carried by these ice- particles is presented and discussed? And what about the simulated precipitation in C- and P-case? Please, add appropriate information and plots – for example, peak rainfall rate (mm/h) as a function of time, total rainfall volume and others. Without such information, the analysis of the physical processes responsible for the difference in lightning frequency in both simulated thunderstorm cases is limited and the conclusions are not reliable.

In Figure 6, the temporal variations of the domain-averaged effective radius for various hydrometeors are shown. Here, the analysis is also very superficial. There is no suggestion for the reasons related to the very pronounced radius fluctuation of most hydrometeors, why the effective rain drops radius is larger in P-case and why the largest value of effective graupel radius in P-case is very early (9:30 UTC). One can assume that the radius fluctuations could be due to the updraft velocities fluctuations and that the larger effective graupel radius in the P-case is a result of the higher updraft velocity in the P-case, and not only due to larger graupel mass. However, without appropriate analyses and any information on how the effective radius of the corresponding hydrometeors was determined, the above assumption may not be relevant. And why the temporal variation of the domain- averaged effective radius for hail and snow is not shown?

I do not understand for what reason the authors have decided first to consider the profile of total charge density and the charge carried by different ice-particles during the mature stage, then in the initial stage and without any attention to the impact of microphysics and

dynamics of simulated clouds on their electrification during the maximum of lightning frequency. I do not think that it is appropriate to have separate sections - 4.4 Convective strength and 4.5 Delay of first flash in polluted case. All this can be presented in one section 4.3 The relationship between electrification, microphysics and dynamics. Again, the analysis presented in this section is cursory, often based on assumptions rather than on a profound investigation of the relation between microphysics, dynamics and thunderstorm charging. In order to get an idea about the formation of the charge structure during the different stages of thunderstorm development, it is appropriate to present horizontally averaged total charge density as a function of height and time, indicating for each of the stages of cloud development the maximum (minimum) positive (negative) total charge density together with the height and the moment of their achievement. Consideration of these results together with the results shown in Fig. 5 can give a general idea of the relationship between the mass of the ice-particles, updraft and downdraft velocity and time-height distribution of charge density in the polluted and in the continental case. In this analysis, it may be useful to pay attention to the time and amount of precipitation. And then to analyze the impact of microphysics and dynamics on thunderstorm electrification in 3 specific moments sequentially: at the initial moment of thunderstorm development (Fig. 11 and Fig.12), during the period of maximum lightning frequencies (it is necessary to show additionally appropriate figures) and finally during the mature stage (Fig. 7 and Fig.8) in the two simulated C- and P-cases. In my opinion, such a sequence of considerations would allow more detailed analysis to be made to clarify the processes responsible for the significantly higher lightning frequency in the P-case and the earlier onset of the discharge in the C-case. However, more comprehensive analyses have to be done, rather than only listing the results in the presented figures. For this purpose, it would be useful to look (at some specific moments of thunderstorms development) the profiles of the maximum updraft velocity and the mass of some hydrometeors related to thunderstorm charging.

Technical corrections:

1. Denote in Fig. 2 the regions for which the results are analyzed
2. The isotherms shown in the figures are very pale and difficult to see. It is also useful to show -30 and -40 isotherms.
3. The labels indicating the vertical velocities in Fig. 5 i and Fig.5j are blurred – they should be brighter.
4. It is better to use one and the same color for the results of C- and P-case simulation. So, it is desirable to use in Fig.4 (similar as in Fig.3b and in Fig.6) blue color for the C-case and red for the P-case.
5. The caption of Fig. 7 and others similar has to be revised because the figure does not show any microphysical characteristics.

