Reply on RC1

RC1: This study presents results from a suite of thunderstorm simulations in which CCN concentrations, INP concentrations have been varied and in which SIP mechanisms have been activated or deactivated. The authors focus on the response of the lightning activity to these choices and attempt to understand the response through the analysis of the storms' microphysical properties and process rates. Three thunderstorm cases are analyzed. It is a very detailed study. The results regarding the response to CCN and INP concentrations appear to be consistent with previous studies although the present study is perhaps somewhat more robust in that it examines multiple cases. The testing of SIP mechanisms appears to be a more novel aspect of the study and here their results are not entirely consistent with the few other studies that exist. Overall I think the study has potential to be a useful contribution to the community but I do have some major questions about the results.

We thank the reviewer for his/her time and efforts in reviewing our manuscript. The responses to his/her comments are addressed below.

1. The second part of the study regarding SIP mechanisms seems to make the first part of the study (and previous studies regarding CCN/INP concentrations) potentially irrelevant. The authors show that the inclusion of additional SIP mechanisms increasing the lightning flash count by 15-50x – a substantially larger increase than was obtained by varying CCN and INP concentrations. Assuming that the inclusion of SIP mechanisms leads to a more realistic simulation, then how meaningful are the results of the CCN/INP tests with only HM? I would guess that with all SIP mechanisms included, the sensitivity to INP would vanish and perhaps the sensitivity to CCN would also be diminished?

You are right. Our study clearly shows that the SIP effect dominates ice crystal production and cloud electrification over the aerosol effect. But this result must be qualified in view of the assumptions made in the simulations.
In the second part of our study dealing with the effect of SIP on cloud electrification, only one set of CCN and INP number concentration was used. However, Hoarau et al. (2018) have shown that varying the initial number concentration of IFN may modify the cloud ice concentration by up to one order of magnitude. Huang et al. (2025) found a maximum 100-fold increase in flash rate with four different SIP activated (the ice sublimation breakup process is used in addition to the three processes studied in our manuscript). This factor is estimated from their Figure 17. Huang et al. (2025) have also shown different behavior in the total flash rate at low (400 cm^{-3}) and high (4000 cm^{-3}) CCN concentration with and without the four SIP activated. They observed a higher enhancement of the flash rate with SIP processes at higher CCN concentration.
Moreover, only one parameterization of each SIP process was used in our study. In Mansell and Ziegler (2013), a test is carried out on the formulation of the Hallet-Mossop (HM) process. Two different parameterizations are tested: one set of simulations used the parameterization of Ziegler et al. (1986), while the second set of simulations used the one of Hallett and Mossop (1974). Both parameterizations operate in the same range of temperature, between -3 and -8°C. On Figure 9 of Mansell and Ziegler (2013), we can clearly see the large discrepancy in terms of flash density depending on the formulation of the HM process. A factor 8 in terms of total lightning is estimated between the two

simulations when the number concentration of CCN is fixed at 200 cm^{-3}. The difference in terms of total lightning reaches a factor 20 when the CCN number concentration is 5000 cm^{-3}.
There is no doubt that SIPs play an important role in cloud electrification, but large uncertainties remain in their parameterizations used in microphysics schemes (Field et al., 2017; Han et al., 2024, Grzegorczyk et al., 2025). As stressed in the conclusion, the simulation of a real case with available in situ measurements will be useful to constrain the model and to better understand the physical processes at play in cloud electrification.

A paragraph has been added in Section 4.2.4 to discuss these uncertainties:

"Activating SIP processes enhances the ice crystal number concentration and the lightning activity, with an impact 5 times greater than that of aerosol concentration in terms of the number of flashes. This is lower than the 100-fold flash rate increase deduced from Huang et al. (2025) when multiple SIP processes are activated, especially under high CCN concentrations. Numerical studies consistently highlight the dominant role of SIP processes over primary ice production especially in the mixed phase region (Huang et al., 2022; Grzegorczyk et al., 2025). However, SIP efficiency can vary with microphysical conditions. For instance, Zhao and Liu (2022) found reduced SIP rates when using a stronger primary ice nucleation parameterization: cloud glaciation is accelerated, and rain and graupel formation is reduced which inhibits SIP processes. In the present study, SIP sensitivity was tested using only one set of N_{CCN} and N_{INP}, and the sensitivity to SIP parameterization has not been explored. Prior work (e.g. Mansell and Ziegler, 2013) has shown that different HM parameterizations significantly influence electrification."

2. I am very surprised by the near total lack of sensitivity of the CWC profiles to SIP mechanisms (aside from NOSIP). It's not just that the CWC profiles are similar, they are virtually identical. Assuming that this is not an outright error, could it potentially be due to the parameterization of ice crystal collisions or properties in LIMA? For example, perhaps LIMA has a minimum ice crystal size that is being met and so all simulations have the same crystal size despite differences in concentration. Or there is some hard-coded limiter in the collision rate with cloud droplets? Or is there no longer a mixed-phase region? It is just very hard to explain why orders of magnitude differences in the ice crystal number concentration should have absolutely no impact on the cloud water content. It also seems potentially inconsistent with previous discussion of how INP concentrations impact CWC (for a fixed CCN concentration). Why should HM-only with INP variation impact CWC while HM+other SIP should not?

Several factors might be responsible for the lack of sensitivity of CWC with the SIP processes activated. Firstly, it partly results from the data being sampled during the electrification period defined in this study. At the mature stage, the simulations start to diverge; the ALLSIP simulation having the least CWC. However the differences remain small compared to the stronger sensitivity of CWC to aerosol concentrations.
A possible explanation for this weak response is the use of a saturation adjustment scheme in LIMA. This adjustment, applied after all other microphysical processes, forces the environment to reach a strict equilibrium at water saturation at the end of each time step, by condensing water vapor or evaporating cloud droplets depending on whether the air is supersaturated or sub-saturated. In a general way, Khain et al. (2015) examined the bin and

bulk parameterizations of microphysics and stated that “the utilization of saturation adjustment during diffusional growth introduces errors in CWC”. Previous studies have identified limitations of using saturation adjustment, including overestimation of the condensate mass (Khain et al., 2015), and enhanced rain formation that reduces supercooled water in the mixed-phase region (Zhang et al., 2021). Previous studies that reported a decrease in liquid water content with the introduction of SIP processes (Phillips et al., 2022; Huang et al., 2024; Grzegorczyk et al., 2025) explicitly computed condensation and vapor deposition, unlike the LIMA scheme.
Another possible reason is that in the version of LIMA used in this study (v1.0; Vié et al., 2016), snow and graupel number concentrations are not prognostic. In contrast, the extended version of LIMA (v2.0; Taufour et al., 2024), includes prognostic number concentrations for all hydrometeor categories. Taufour et al. (2024) showed that in LIMA v1.0, snow and graupel form rapidly, consuming cloud droplets, raindrops, and ice crystals. In LIMA v2.0 their formation is more gradual.

A discussion about the insensitivity of CWC to SIP processes has been added in 4.4.2.

“The weak sensitivity of CWC to SIP processes may result from several factors. Data sampling during the electrification period limits the detection of differences, which occur more significantly during the storm's mature stage. The use of saturation adjustment in LIMA, which enforces 100\% RH could be a constraint, as it can overestimate condensate mass and enhance rain formation, reducing supercooled water (Khain et al., 2014; Zhang, 2021). In contrast, studies showing stronger CWC responses to SIP explicitly compute condensation and vapor deposition (Phillips et al., 2022; Grzegorczyk et al., 2022; Huang et al., 2025). Additionally, in the version of LIMA used here (v1.0) snow and graupel number concentrations are not prognostic, potentially accelerating their formation and depleting liquid and small ice species as shown by Taufour et al. (2024) in comparisons with LIMA v2.0.”

3. I know that the simulations aren’t meant to be compared to observations, but can the authors comment at least qualitatively on the magnitude of their results? Do previous observational studies support a nearly 10x increase in lightning flashes due to CCN?

We understand the desire to compare the present results with observations or to comment on them qualitatively. However, this is not an easy exercise. In terms of observational studies, most of them use aerosol optical depth (AOD) rather than aerosol number concentration and compare it with lightning discharges over large periods of time (yearly or per season data) (Shi et al., 2020; Proestakis et al., 2016; Dayeh et al., 2021; Wang et al., 2023; Altaratz et al., 2010). In general, the lightning activity is observed to increase between 50 and 150% when the aerosol loading increases (e.g., Naccarato et al., 2003 ; Thornton et al., 2017 ; Peterson, 2023). Studying the effect of aerosols on lightning activity over the Mediterranean sea, Proestakis et al. (2015) have found an enhancement factor of 9 of the number of lightning strikes between low and high AOD. From their Figure 7e, we can see that when the AOD increases from 0.1 to 0.5, the average number of lightning strikes increases between 30 and 280. In numerical studies, the lightning flash enhancement factor associated with different aerosol concentrations can reach higher values. Sun et al. (2023) simulated a multicellular storm during 6 hours, and found a 5-fold increase of the total lightning flashes when the CCN concentration was increased from 400 to 6400 cm^{-3} (see their Figure 18). Huang et al. (2025) showed an enhancement by a factor 60 when the CCN

number concentration is increased from 400 to 4000 cm^{-3} (see their Figure 17) in a simulated squall line.
It must also be noted that the total number of flashes simulated by the model depends on the parameterization of the non-inductive process (Mansell et al., 2005 ; Barthe and Pinty, 2007a) and on the choice of the lightning flash parameters (Barthe and Pinty, 2007b). But, since the cloud electrification and lightning scheme parameters are held constant over the whole set of simulations, this should not affect the results.

A comparison of our results with the literature has been added in Section 3.3 of the new version of the manuscript:

"These values are of the same order of magnitude as the ones in the literature. Sun et al. (2023) found that the total number of flashes was multiplied by 5 when N_{CCN} increased from 400 to 6400 cm^{-3} in a simulated multicell storm developing in a high CAPE environment. Huang et al. (2025) reported a nearly 60-fold increase of the total lightning number in a simulated squall line when the aerosol concentration increased from 400 to 4000 cm^{-3}. Observational studies based on AOD and lightning strikes data report similar increases in lightning activity, with enhancement factors ranging from 1.6 to 9 (Thornton et al., 2017; Naccarato et al., 2003; Proestakis et al., 2016)."

Minor Comments:

1. There are several places where citations are needed, including Line 21-22, 48, and 69-72.

We added citations as recommended:

- Line 20: Reynolds et al. (1957) and Takahashi (1978)
- Lines 21-22: Norville et al. (1991) and Heldson et al. (2001)
- Line 48: van der Heever et al. (2006), Rosenfeld et al. (2008), and Sun et al. (2021)
- Lines 69-72: Mansell and Ziegler (2013), Tan et al. (2017), Yang et al. (2020), Sun et al. (2021), and Yang et al. (2024)

2. Line 86 – sentence is unfinished

The "…" marks the end of the sentence.

3. Lines 145-150 – how was MID-WARM triggered

The WARM case was triggered by a warm bubble of 1.5°C. In the first version of the manuscript, the description of the WARM and MID-WARM set-up was mixed. This is clarified in the new version of the manuscript.

4. Lines 155-161 – what size particles? Can more information be provided about the INP populations?

A single mode of CCN particles was set with a mean radius of 125 nm and a standard deviation (sigma) of 0.69. Concerning INP, a single mode was used with a mean radius of 0.8 µm and a sigma of 1.9. As in Vié et al. (2016) and as recommended by Phillips et al. (2008), the INP mode is composed of 61% of dust, 33% of black carbon and 6% of organic matter. We added these informations in the new version of the manuscript;

5. Just in general, the model setup information was minimal and could be described in greater detail, especially since model initialization files are not provided as part of the code/data/software availability.

In the new version of the manuscript, additional information about the model setup is given in Sections 2.2 and 2.3. The soundings of each idealized case are added as a supplementary material.

6. Line 193 – Not a complete sentence.

This sentence has been modified.

7. Line 238 – what does it mean that the simulations were treated together? That they were averaged together? Or that a representative simulation is shown?

It means that only one of the three simulations is shown. Only the simulation using N_{INP} = 10 L^{-1} is used in the rest of the article. This is clarified in the new version of the manuscript.

8. Line 248 – is the charging rate meant to have a unit of kC/s rather than just kC?

You are right: we checked our script and changed it to C/s in the new version of the manuscript.

9. Line 272 – by my eye CWC drops below 0.01 g/m^3 nearer to -20C than -10C.

It is corrected in the new version of the article.

10. Line 281 – effect of varying NCCN on what is mainly the same?

This is the effect of an increase of N_{CCN} on cloud water content. It has been clarified in the text.

11. The Bergeron process is mentioned a few times. Typically I take this to mean the growth of ice and evaporation of droplets. But in a strong updraft, I assume that supersaturation is produced rapidly enough that supersaturation can be maintained with respect to both liquid and ice such that there is no Bergeron effect.

We agree that in convective updrafts supersaturation is usually maintained (Khain et al., 2012), and that the Bergeron effect operates under limited conditions, not consistently impacting ice particles and liquid droplets in mixed-phase clouds (Korolev et al., 2007). In LIMA, the Bergeron process is not explicitly computed and the effect of riming of cloud droplets on aggregates and graupel is more significant. Therefore, we decided to remove in the manuscript the Bergeron effect.

12. Figure 10 – what are SIP tendencies exactly? The ice number production rate?

Yes, SIP tendencies correspond to the ice number production rate of each SIP process. It has been clarified in the legend of Figure 10.

13. It would be helpful to label the temperature lines in many of the figures.

You are right. The temperature lines have been labelled in the new version of the manuscript.

→ Overall, I found the manuscript to be overly detailed and a little tedious to read. I think that the main points could be conveyed with more concise text. But I leave this to the authors to decide.

In the new version of the manuscript, we've tried to make the text a little more concise. Given the large number of simulations to be processed, however, it is difficult to reduce the descriptions significantly. We have also modified the organization of the first part of the results following the recommendation of reviewer 2

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