General Comments

This study investigates the impact of enhanced coalescence due to droplet charge in high-fidelity simulations using the superdroplet method. While the methodology and results are thoroughly presented, the authors' key assumption of instantaneous droplet charging is not well-founded or discussed, and likely leads to a strong overestimate of the impact of droplet charge on precipitation. Similarly, the authors do not attempt to disentangle purely microphysical effects from flow field variability that stems from microphysics-flow coupling (vs. using a method like piggybacking). These reservations about the scientific merits combined with more minor concerns about the writing itself (including typos and inappropriate references) lead me to recommend major revisions before this paper be considered for publication.

Specific comments

1. The selected references for describing the importance of droplet coalescence (e.g. line 35) are not appropriate general references for this statement. For instance, Rosenfeld 2008 specifically concerns the a controversial mechanism of aerosol convective invigoration, which has relatively little to do with coalescence. Craig 1995 discusses radiative effects which are also not inherently specific to coalescence. Forbes & Clark does not even appear in the references. More appropriate citations would include review papers, chapters from the IPCC or a classical cloud microphysics textbook, or studies which specifically investigate droplet coalescence. Likewise in line 483: the stated impact of increasing aerosol concentration is the Twomey effect, and should not be attributed to Rosenfeld 2008!
2. I have some issues with your notation and definitions in section 2.4. Line 144: Given that both E0 and Ees are presumably efficiencies varying between 0 and 1, they should be multiplicative rather than additive. I am also confused by the notation of Rp, rp, QR, and qr, as the text describes these radii and charges as being general to "large droplets" or "small droplets", whereas I would imagine them to be descriptive of the larger droplet R and smaller droplet r for a given pair (R, r). What does the subscript "p" refer to?
3. The assumption in 212-213 that droplets charge instantaneously following coalescence seems like a major flaw in this study which would lead to a strong overestimate of the effects of electro-coalescence in an LES. Given that you are using the superdroplet method for this study, you should in fact be able to model the time response of charging on a given superdroplet as an additional attribute! This would provide a much more trustworthy study of the effects of droplet charge on coalescence that could actually be used to quantify and suggest whether this effect is notable. As it stands however, this assumption undermines the findings of this study and is not adequately discussed as a limitation or confounding factor in the abstract or conclusions. Furthermore, the values chosen for alpha in the numerical experiments are not well-justified with values or ranges measured in real clouds.
4. I also take issue with the comparisons made between different simulations given that the flow-field and microphysics are fully coupled. A more appropriate way to analyze the purely microphysical effects of electro-coalescence would be through the common technique of piggybacking, as other studies have shown that differences due to small perturbations to the flow field often outpace differences related to microphysics effects.

I do like the approach of using 50 ensemble members to analyze statistics of the superdroplet simulations, but I'm not convinced that this would help isolate microphysics from flow-field variability. Furthermore, in section 2.5 and 2.6, is it not clear whether the simulations performed are DNS or LES as there is no sub-grid scale turbulence model (line 340), nor is there any mention of what impact neglecting SGS turbulence would have on the results.

5. In general when discussing figures and results, details from the figure caption which describe the various lines are repeated in the full text unnecessarily (e.g. lines 240-245, line 356-364, and elsewhere throughout). This repetition should be removed and avoided.
6. Lines 401-406 suggest that the trend going from LA to MA conditions is opposite from the trend going from MA to HA conditions, when the figure in fact indicates that the trend is consistent. Increasing the aerosol concentration appears to uniformly delay and reduce the precipitation quantity.

Technical comments

- Line 33: "play a key role in cloud formation" should be "rain formation"
- Line 62: the Greenfield Gap should be concisely defined
- Line 200: there is an extra close-parentheses ")" after Khain04
- Line 203: there is an extraneous comma after "the IM treatment"
- Line 228: if alpha is a ratio, it should be unitless and have a maximum value of 1. Can you clarify the definition here?
- Line 305-306: Why are the number concentrations written this way, as "3 x" something?
- Line 312-313: "SDM requires less computational cost..." compared to what? Be specific; it requires more computational cost than a bulk method!
- Equation 27: what is p?
- Lines 433-435 are repeated from earlier in the paragraph.
- Lines 485-490 are a very good summary of key findings and insights from this study