Review of “Aerosol as a potential factor to control the increasing torrential rain events in urban areas over the last decades” by Seoung Soo Lee et al.

The authors investigate the role of spatial gradients in aerosol concentrations on the formation of heavy precipitation from convective clouds. They use a series of high-resolution simulations with the ARW-model with either a spatially homogeneous aerosol concentration or a spatial gradient in the aerosol concentration. Heavy precipitation coincides with the boundary between the air masses with high- and low aerosol concentration, which is also marked by large convergence. In the simulations with a spatially homogeneous aerosol concentration the convergence zones remain weaker and are less organised. The authors argue that the difference in the convergence fields is a result of larger evaporative cooling in the high-aerosol airmass leading to stronger downdrafts and surface divergence.
While the role of spatial gradients for aerosol-cloud interactions has been little explored and is an interesting topic, there are several major issues with the current manuscript, most importantly the lack of an analysis of the meso-scale circulation (see general comments). Before the manuscript can be accepted for publication these issues need to be addressed by the authors and substantial changes to the manuscript are required.

1 General comments

1. Introduction: The authors claim that the temperature and humidity forcing are homogeneous across a MCS and that spatial variability in the dynamic forcing can not explain the spatial variability in MCS intensity. However, it is well known that meso-scale circulation such as sea-breeze fronts, lake-breezes, or cold-pools have a substantial impact on the evolution of convective clouds and MCS. Also a population of clouds in the same large-scale environment will produces cells of varying intensity and at various evolution stages, which leads to a complex and varied spatial distribution. This is not adequately reflected by the statements by the authors (p. 3, l. 79-86).

2. Introduction / Conclusions (p. 4, l. 117 - p. 5, l. 123 / p. 24, l. 730 - 734) : The authors hypothesise that local variability in aerosol concentrations can drive spatial variability in precipitation. This should be more clearly highlighted as hypothesis. Also, I find this hypothesis highly unlikely as (i) convective clouds (in particularly strongly organised MCS) usually are not stationary and may ingest aerosol from various regions during their lifecycle and (ii) horizontal gradients in aerosol are reduced by turbulent mixing during the transport to cloud base. The spatial variability discussed here appear to be of much smaller scale than those investigated with the simulations with two different aerosol-concentration air masses over an area of about 100 x 100 km.

3. Description of the model: The description of the model and the simulation set-up is scattered across section 2 and 3. In particular, many parts of section 3 detail the set-up of the model domains and the cloud microphysics instead of discussing the investigated case. The model description should be provided in one single section. The authors also say they developed a module to represent the spatial variability of aerosol (p. 7, l. 193-194). It is not clear from the manuscript at this point what processes this entails. Please provide a better description of what processes are included.

4. Results: The analysis of the differences between the simulations, the physical mechanism driving these changes and the presented conclusions are not very convincing to me. While there are certainly differences in the convergence patterns between the runs, the physical mechanism is not clear. From the presented figures, I find it hard to believe that the difference in surface wind between the two air masses with different aerosol concentrations are a result of different latent cooling rates in the two areas, in particular as the convective systems are rather small compared to the extend of the wind field anomaly in the high-aerosol air mass during the initial stages of the simulation. Just looking at the wind fields in Fig. 9, it appears that there are significant differences in the wind field at the lateral boundaries. It would be interesting to investigate whether the changes in the wind field are due to cold pool formation in an upstream area of the domain 3. This is particularly important as the system at least in the initial and mature phase is located very close to the northern domain boundary (e.g. Fig. 7). Along these lines, it would be also important to assert that the meso-scale circulation patterns in the outer domains are similar in the additional sensitivity simulations the authors present. Is it possible that the large differences in the convergence and the lack of organisation is related to changes in the meso-scale circulation in the outer domains? Another factor that is not at all mentioned are radiative effects of the aerosols that could impact the stability between the air masses with different aerosol concentrations. The authors say in the model description, that the aerosols interact with the radiative fluxes. These aspects need further investigation before any firm conclusions about the physical mechanism for the differences between the simulations can be drawn.

5. Results: It is mentioned in the model description that ice- and mixed-phase processes are included in the microphysics module of the model. However, the discussion exclusively looks at warm-phase processes, i.e. using condensation/evaporation, autoconversion/accretion. If the simulations include mixed-phase processes, these need to be included in the analysis as well.

2 Specific comments

1. p. 4, l. 94: What is aerosol supposed to be most representative for?

2. p. 4, l. 105-108: The authors cite two studies to suggest that increasing aerosol concentrations can intensify deep convective clouds by enhanced latent heating due to freezing. This hypothesis has been discussed controversially in recent literature (e.g., van den Heever et al., 2006; Fan et al., 2009; Lebo and Seinfeld, 2011; Lebo, 2017) and this should be mentioned in the introduction.

3. p. 5, l. 148: Please check this reference.

4. p. 9, l. 246: Do you mean the aerosol in the PBL does not vary vertically?

5. p. 9, l. 255: Please chose a more meaningful title for this section. It would also be good to introduce all sensitivity simulations conducted in the paper here. In particular, the simulations with homogeneous aerosol concentrations, since these are the obvious test simulations the reader is expecting for addressing the outlined scientific questions.

6. p. 9, l. 257: The aerosol field consist of two air masses with two different aerosol concentrations and a relatively small transition zone between the two. I would not call this is "high-degree spatial inhomogeneity". Please avoid using this term. However, I agree that the aerosol variability investigated here is larger than in most numerical studies, which do nor represent spatial aerosol variability.

7. p. 9, l. 269: It is claimed that the effects of inhomogeneity and number concentration can be investigated. However, it is not possible discriminate the impact of two changes based on just the two simulations, which have been introduced in the manuscript up to this point.

8. p. 10, l. 303: Please specify whether these are surface precipitation observations or derived from radar data.

9. p. 11, l. 313: Have you interpolated the 3 km observational data to the 500 m model data. The linear interpolation does not represent the correct frequency distribution at higher resolution. A less problematic approach would be to coarse-grain the model data to the resolution of the observational data.

10. e.g. p. 15, l. 427/428: The authors refer at various points to an "extension" or "movement" of the convergence field. I think they refer to changes in the spatial extend or location of regions with high convergence. The formulation should be altered accordingly.

11. p. 18, l. 520: Is the different location of the convergence line in the two simulations taken into account for the calculation of the mean values? And its eastward propagation?

12. p. 20, l. 586: What is the motivation for not switching of latent cooling from rain evaporation? This is usually considered the most important for cold-pool formation and the interaction of deep convective systems with boundary-layer dynamics.

13. Figure 1: Can you include the topography in this plot. This would be interesting for readers not very familiar with the geographic context.

14. Figure 5 and 6: Can you include all the results from all sensitivity experiments in these plots?

15. Figure 7: I find the contour plots extremely hard to read, especially the different contours for the precipitation rate. Would it be possible to use filled contours to show the precipitation rates?

16. Figure 11: It would be interesting to show the evolution of the low-level wind field in these simulations and for earlier times as well.

3 Technical corrections

There are numerous places in the manuscript, where the language is quite awkward and reformulation of the sentences should be considered. In particular, please check the use of articles. A none exhaustive list is provided:

- The authors use phrases like ”frequency or occurrence” in many places (e.g. page 3, line 59; page 9, line 264/265; etc). These ”or”-statements should be removed and just one term be used.
- p. 4, l. 101: “Collision and collection are”
- p. 5, l. 123: “**A** further increase in aerosol loading **in** the district ...”
- p. 5, l. 125: “... create **a** greater inhomogeneity ...”
- p. 5, l. 131: “ ... select **a** MCS over ...”
- p. 7, l. 183: “ ... the large-scale environment ...”
- p. 7, l. 186: “... assumes horizontally homogeneous aerosol properties ... ”’
- p. 7, l. 191: “... assumption of homogeneity and ... spatio-temporal inhomogeneity ...”
- p. 7, l. 193: “... able to represent the inhomogeneity ...”
- p. 7, l. 197: “... with **about 1 km** distance ...”
- p. 7, l. 200: “ ... size distributions **at** those sites ...”
- p. 7, l. 210: “... follow **a** tri-modal ...”
- p. 8, l. 218: “... and aerosol particles are assumed to be internally mixed.”
- p. 8, l. 230: “... above, precipitation is ...”
- p. 10, l. 279: “... has ”low” inhomogeneity ...”
- p. 10, l. 302: “... simulations perform reasonably ...”
- p. 11, l. 316: “... the observed frequency distribution is consistent with the ...”
- p. 12, l. 340: “... initial stages of the precipitating system ...”
- p. 12, l. 354f: Please explicitly state the meaning of these lines again.
- p. 12, l. 360: “By 20:00 LTS the maximum ...”
- p. 13, l. 375: “ ... Figure 7e for easier comparison. This ...”
- p. 13, l. 378: “The system propagates eastwards after 20:00 LST ...”
- p. 15, l. 444: “... the associated larger intensification ...”
- p. 15, l. 456: Can you please rephrase this sentence, its meaning is unclear to me in its current form.
- p. 17, l. 512f: “... there is a larger horizontal wind-speed than in ...”
- p. 21, l. 624: “... vice versa. For this purpose, ...”

References

Fan, J., T. Yuan, J. M. Comstock, S. Ghan, A. Khain, L. R. Leung, Z. Li, V. J. Martins, and M. Ovchinnikov, 2009: Dominant role by vertical wind shear in regulating aerosol effects on deep convective clouds. *J. Geophys. Res. Atmos.*, **114**, D22206, doi:10.1029/2009JD012352.

Lebo, Z., 2017: A numerical investigation of the potential effects of aerosol-induced warming and updraft width and slope on updraft intensity in deep convective clouds. *J. Atmos. Sci.*, doi:10.1175/JAS-D-16-0368.1.

Lebo, Z. J. and J. H. Seinfeld, 2011: Theoretical basis for convective invigoration due to increased aerosol concentration. *Atmos. Chem. Phys.*, **11**, 5407–5429, doi:10.5194/acp-11-5407-2011.

van den Heever, S. C., G. G. Carrió, W. R. Cotton, P. J. DeMott, and A. J. Prenni, 2006: Impacts of nucleating aerosol on florida storms. part I: Mesoscale simulations. *J. Atmos. Sci.*, **63**, 1752–1775, doi:10.1175/JAS3713.1.