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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "Sensitivity study of the aerosol effects on a supercell storm throughout its lifetime" *by* A. Takeishi and T. Storelvmo

Anonymous Referee #1

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1 General Comments

The authors present the result of a large suite of numerical simulations of an idealized supercell performed using the Weather Research and Forecasting (WRF) model. The main purpose of this study is to address aerosol effects on deep convection, specifically in the context of an idealized supercell. While the extensive number of simulations is very useful for determining the dependency of the aerosol effects on such factors as model initialization and microphysics parameterization, there are several shortcomings that must be addressed before proceeding with the publication process. My major concerns are outlined below.

2 Major Comments

1. **Introduction:** I found the introduction to be lacking a sufficient amount of discussion that is pertinent to the current study. For example, the discussion of the Twomey and Albrecht effects and the buffering effect are much more relevant for studies on shallow convection or stratiform clouds. This discussion is comparable in length to the discussion of previous studies that have examined aerosol effects on deep convective clouds. In fact, only three studies, i.e., Van den Heever and Cotton (2007), Nissan and Tuomi (2013) and Fan et al. (2009) are discussed. While Nissan and Tuomi (2013) did examine a supercell, the other two studies did not. There are several other studies in the literature that have examined supercell simulations (e.g., Khain and Lynn, 2009; Lebo and Seinfeld, 2011), specifically the case analyzed in the current work. Moreover, the authors fail to discuss other avenues in which aerosol perturbations may influence deep convective systems, e.g., cold pool effects (see, e.g., Tao et al., 2007; Lee et al., 2008; Seigel and van den Heever, 2013; Lebo and Morrison, 2014).

2. **Model Setup and Initialization:** A large portion of my concerns related to the model setup and initialization may be due to a lack of necessary details in Section 2. Regardless, this is an important point given that it is challenging to interpret the findings presented when the framework is poorly defined in the text. Some of my major concerns regarding this aspect are as follows:

   (a) Why did you choose to use periodic boundary conditions? I am concerned that over the course of a 10-h simulation, portions of the system may begin to interact in an nonphysical manner.

   (b) Is 20 km a sufficient model top? Many studies looking at continental deep convection have used model tops as high as 24 km to ensure that the overshooting tops are sufficiently far from the Rayleigh damping layer. Based on Figs. 6 and 11, there appears to be condensed water (in the form of ice) above 15 km, which is within the damping layer. While I am not suggesting that the simulations should all be rerun with a higher model top, I think it would be good, at least in the responses, to examine a simulation with a higher model top to ensure that the results are insensitive to condensed water entering the damping layer.

(c) While I agree that the vertical resolution is important, it is unclear as to how the vertical levels are stretched. There is most likely no cloud at the surface; therefore, stating that the resolution in the lowest model layer is 210 m provides the reader with little insight into the resolution within the cloudy region of the domain. I suspect that 40 levels may not be sufficient to accurately resolve 3D motions in such a vigorous system, especially if the vertical resolution exceeds 500 m at heights below cloud top.

(d) While I understand why the standard Weisman and Klemp (1982) sounding is altered in this study, my concern is that the results (especially some of the insensitivity to changes in aerosol or, in this case, the cloud droplet number concentration) are related to the fact that the storm really never reaches a mature stage. Based on Fig. 12, this is especially true for the simulations performed using the Morrison and Thompson parameterizations. Did you perform simulations with slightly moister soundings? My other concern related to this point is that the updrafts are fairly weak compared to other studies of the same system (Khain and Lynn, 2009; Lebo and Seinfeld, 2011). In particular, Fig. 2b of (Khain and Lynn, 2009) presents the maximum vertical velocities for a set of simulations using the Thompson parameterization and for the same supercell (albeit without reducing the water vapor mixing ratio at the lowest model layers). They showed that the maximum vertical velocities are maintained between 40 and 60 m s^{-1} throughout their simulations. I understand that you intended for the system to ultimately die off to examine the complete life cycle. However, it appears as though the system never fully develops given the rather weak maximum updrafts and the immediate

downward trend after approximately 30 min into the simulations (which is about the amount of time in which the initial bubble still has some effect on the model fields).

3. **Analysis:** In general, I find the use of maximum and minimum vertical velocities to be insufficient when looking for small effects caused by changes in aerosol loading. I would rather see figures showing either PDFs or some statistical moment of the PDFs. It is nearly impossible to detect robust changes between two curves when the curves are based on a single point within the domain at any given time. Moreover, the conclusions presented on P24099, L16-17 are dependent on the lack of a tendency in the maximum/minimum vertical velocities. I am not surprised that there is little difference in the maxima/minima when changing the aerosol (droplet) number concentrations.

   When comparing Fig. 3a with Fig. 5, it is not clear how the fraction of frozen precipitation can be 100% at the end of the simulations. Fig. 3a suggests that in all but one case, precipitation stops before the end of the simulations. Even though melting is shut off for the simulations shown in Fig. 5, I am not sure I understand why there is still precipitation reaching the ground unless the amount is very tiny. Please clarify.

   The bar charts are also not very well discussed in the text. I think that these figures require more context. Moreover, it needs to be clear to the reader why some panels use 4 h as a demarcation point, while others use 2 h. Please also use consistent axes within individual figures when possible. For example, Fig. 7a has a different y axis than Fig. 7e.

4. Some statements appear to be drawing general conclusions about deep convection, e.g., P25100, L14-15. However, a single supercell is examined in this paper. It is challenging if not impossible to draw general conclusions about “deep convection” based solely on a few simulations of the same supercell. I would highly

recommend that the language be changed throughout the text to ensure that this point it made clear to the reader. The results presented in this paper are not necessarily applicable to other forms of deep convection and other regions of the globe.

5. The primary conclusion of the paper, at least based on the abstract, is as follows "These results emphasize the importance of accurate representations of graupel formation in microphysics schemes." How is this different from the results of Morrison and Milbrandt (2011) with regard to highlighting the importance of graupel/hail parameterizations?

3 Minor Comments

1. P24089, L4-18: I recommend either removing this portion of the introduction or, at the very least, shortening the discussion.

2. P24089, L26-28: This statement seems a bit unjustified. Perhaps including a reference or two would bolster this point.

3. P24090: In general, please consider including relevant details regarding the cited works, especially the system that was analyzed. For example, the current study examines a supercell, while Van den Heever and Cotton (2007) examined general deep convective clouds moving over an urban area and Fan et al. (2009) examined isolate convective towers. Such details are important when comparing one study to another because the effects on, e.g., supercells, are likely going to differ from those on, e.g., squall lines.

4. P24091, L15-16: While it is true that most studies have used a limited number of aerosol number concentrations, there are a few studies that have examined

a larger range (e.g., Han et al., 2012; Lebo and Morrison, 2014). I think these works should be noted here.

5. P24093, L1: I think it would be useful to the reader to include a sentence or two regarding the potential effects of radiation in the context of aerosol influences.

6. P24094, L3: Please provide a range of predicted droplet number concentrations for the different scenarios. For example, when the model relationship is multiplied by 2, what is the range of droplet number concentrations that you find? This information is useful when comparing the simulations performed with different microphysics.

7. P24094, L12: Why did you choose to *decrease* the resolution for the sensitivity run? I would have thought that an increase in the horizontal resolution would be more justified here.

8. P24095, L22-23: The sentence "Variation in cloud droplet number concentration makes the simulations more realistic" is awkward. Please revise.

9. P24095, L23-24: Interestingly, there has been a recent push within the cloud model community to move away from hydrometeor categories, especially for the ice phase, and into methods that predict particle properties (e.g., Morrison and Milbrandt, 2014; Morrison et al., 2014). I would argue that having an additional species, while useful in that it allows for predicting a denser ice specie, may not necessarily make the model more "realistic". In reality, there is a continuum in the particle spectra and not distinct classes. It might be useful to the reader to at least comment on this point in the text and use a different term.

10. P24097, L20: It is not clear what is meant by "Figs. 14-11"? Perhaps you meant "Fig."?

11. P24097, L23: It is not clear what is meant by “aerosol increases”. Please be more specific, e.g., “increases in aerosol number concentration" or “larger aerosol particles".
12. P24098, L19-21: Cold pool strength (or intensity) is typically quantified as the vertically integrated buoyancy within the cold pool (e.g., Rotunno et al., 1988). The “cold pool" is more than just a region of air with a different temperature. This region is often categorized by different hydrometeor fields and a downward flux of negative buoyancy.
13. P24099, L5: Hail grows via the accretion of liquid drops onto the surface of an ice core. The term “riming” is not typically used in the context of hail growth.
14. P24101, L6-13: I am a bit puzzled by the reference to Cheng et al. (2010) here. Cheng et al. (2010) examined frontal systems, not supercells. There have been several studies that have outlined potential pathways for changes in aerosol loading to affect deep convective clouds (e.g., Khain et al., 2004; Lebo and Morrison, 2014).
15. P24102, L14-17: I am not sure that I follow the argument presented here in the manuscript. The initial thermal bubble should be fairly ineffective at altering the simulation after 30-60 min. It is not clear how changing the initial forcing will alter graupel production throughout the simulation.

4 General Comments

1. I find the use of “fine” when discussing the resolution of the simulations to be a bit misleading. Most contemporary studies on deep convective clouds, especially in the context of aerosol effects, are using horizontal resolutions of O(1 km).

In this regard, it might be useful to review Bryan et al. (2003) and Bryan and Morrison (2012) for insight into how the chosen horizontal resolution may alter the simulated storm characteristics.

2. Regarding the discussion in Section 3.1, an overview of why the Morrison parameterization may be largely insensitive to aerosol effects, especially in the context of deep convection is presented in Lebo et al. (2012).
3. Please review the figures to ensure that they are consistent. For example, Fig. 10 uses hours for the x axis, while Fig. 11 uses 10-min intervals for the x axis. The inconsistent axes make it difficult to compare the figures. Moreover, in Fig. 7c, at least one of the curves appears to go off the graph. There also issues with the placement of hyphens when defining some of the simulations, e.g., “2 km-resolution” should be “2-km resolution".
4. Following from the end of the previous point, there are numerous grammatical errors in the text, especially related to punctuation. I would typically list such errors. However, given the large number of these errors and the list of major comments above, I will refrain from including such suggestions until the paper is revised.

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