Review: *Sensitivity study of the aerosol effects on a supercell storm throughout its lifetime* by A. Takeishi and T. Storelvmo, submitted to ACP, 2014

General comments

This study investigates aerosol effects on a supercell storm through its full lifetime by performing 10-hour simulations that capture the termination of precipitation from the supercell. A large number of simulations are performed using WRF, including eight cloud droplet or CCN activation concentrations as a proxy for aerosol loading in three different microphysics schemes. The authors conclude that the sensitivity of precipitation to aerosol loading results primarily from changes in the amount of graupel produced through riming.

Considering the full convective cloud lifetime when assessing aerosol indirect effects on deep convection is an important current research topic. Additionally, the authors present a large number of simulations (128) to test the robustness of the results. This paper is therefore well suited for ACP. However, this reviewer has major concerns regarding the lack of relevant papers cited and therefore the context of these results relative to previous works, the manner in which the results are presented within the figures, the interpretation of the results, and the conclusions drawn from the data presented. These major concerns are outlined in further detail below, and following, a few more problematic but more specific major comments are listed.

Major comments

1. Citation of previous works: The authors are missing a great number of highly relevant citations throughout the introduction and text body, and as such, the results presented in this manuscript are not placed in proper context of previous research. It is therefore not clear what new contribution this study makes to the field of aerosol-deep convection interactions.
    a. There have been multiple studies investigating aerosol indirect effects on deep convection while considering different stages of the convective lifetime (e.g. van den Heever et al. 2006, JAS; Tao et al. 2007, JGR; Fan et al. 2013, PNAS; Storer and van den Heever 2013, JAS) that have not been cited. This reviewer is not aware of a previous study on aerosol indirect effects on a supercell storm specifically, while considering the full storm lifetime; previous studies of aerosol effects on supercells have not captured

the dissipating stage. It is therefore suggested that the authors motivate their study in this regard instead.

  b. Related to the above point, while the authors cite Morrison (2012), they have not cited numerous other studies investigating aerosol indirect effects on supercells, including Seifert and Behing (2006, Meteor. & Atmos. Phys.), Lerach et al. (2008, GRL), Khain and Lynn (2009, JGR), Storer et al. (2010, JAS), and Lim et al. (2011, JGR), some of which investigated multiple microphysics schemes including the schemes used here.
  c. Importance of graupel: many previous papers have noted the impact of the microphysical representation of graupel on modeling of deep convection, both non-supercellular and supercellular, and some of which were in the context of aerosol indirect effects. See, e.g., Johnson et al. (1993, J. Appl. Meteor.), Gilmore et al. (2004, MWR), Li et al. (2009, JGR), Morrison and Milbrandt (2011, MWR), Lim et al. (2011, JGR), Bryan and Morrison (2012, MWR), Milbrandt and Morrison (2013, JAS), and Adams-Selin et al. (2013a, MWR, 2013b, WAF) for a start. Lim et al. (2011) concluded that the representation of graupel is highly important in simulations of aerosol indirect effects on supercells. The authors' conclusion in this manuscript regarding the treatment of graupel is not new for convection in general, for supercells, or for aerosol indirect effects on deep convection.
  d. Convective invigoration (abstract line 20, p. 24090 line 9, p. 24099 line 3, p. 24102 line 11-12): Several papers have discussed the ideas of convective invigoration before Rosenfeld et al. (2008) and should be properly referenced, including Andreae et al. (2004, Science), Khain et al. (2005, QJRMS), and van den Heever et al. (2006, JAS).
  e. There are many other instances where citations are lacking or statements are incorrect regarding previous literature. Some of these are noted in the specific comments below. Citations and comparison with previous works are also needed throughout the results section.

2. Model setup: A number of questions regarding the thermodynamic environment, initialization, and model setup need clarification.
    a. What is the magnitude of the vertical wind shear? The authors state that they are using a quarter circle hodograph and cite Weisman and Klemp (1982) for the thermodynamic and wind profiles, but WK82 used a range of linear shear profiles.
    b. How exactly was the moisture profile modified from WK82? How much was it dried outside of their moisture ring (the soundings shown in fig. 1 still appear very moist throughout the troposphere), and how did the moisture vary from the edge of the moisture ring to the center? What is the boundary layer water vapor mixing ratio?
    c. Can the authors please justify their choice of periodic boundary conditions for these simulations? For instance, gravity waves will propagate farther than the 600 km

domain length over the 10 h duration of the simulation and could interact with the convection. Have the authors tested open-radiative boundary conditions to ensure the boundaries are not influencing the solution?

3. Figures and interpretation/discussion of results:
    a. The authors should provide a figure demonstrating that their simulations are in fact producing supercells, especially given that this is part of their title. For instance, a plan view of the accumulated surface precipitation distribution/precipitation rate, midlevel updraft strength, midlevel vorticity, or similarly could be shown. Such a figure could replace fig. 12, as it is difficult to see the storm structure or any details of the surface precipitation distribution from fig. 12, and the individual panels are too small.
    b. How fast does the convection propagate? Furthermore, the quarter-circle shear should favor storm splitting with a right-moving supercell and a left-moving multicell. Is there any multicellular precipitation produced or does this convection dissipate? In other words, is the precipitation shown in figs. 2, 3, and 9 entirely from the supercell or also from the left-moving convection?
    c. Results from the Morrison scheme are presented in only one paragraph and no quantitative evaluation of the changes in precipitation are provided; only general qualitative trends are discussed. Further discussion and investigation of why the Morrison scheme does not display the same sensitivities as the other two schemes is warranted. The authors should also explain why the convection is much shorter lived and produces less precipitation than with the Milbrandt-Yau scheme. This itself may explain why the trends with aerosol loading are not the same. The authors also show that when graupel is used instead of hail, the precipitation response is notably different (fig. 2c and g). In particular, the simulations with graupel instead of hail produce much more precipitation, and the temporal evolution of precipitation is changed. These results should be analyzed and discussed, especially given the authors' conclusions regarding the importance of graupel.
    d. Maximum and minimum vertical velocity time series do not adequately demonstrate convective invigoration or a lack thereof. Have the authors examined average profiles of updrafts exceeding some threshold, e.g. 1 m s^{-1}, or looked at CFADs of vertical velocity for the cleaner vs. more polluted simulations? Such figures would be better suited to demonstrate the conclusions drawn regarding convective invigoration throughout the manuscript. Additionally, previous works have found that invigoration may be more important in earlier stages of convection than in mature or dissipating stages. The authors need to provide a more thorough investigation of aerosol impacts on vertical velocity throughout the supercell

lifetime, in keeping with the main point of the paper. Finally, opposite conclusions regarding convective invigoration within the simulations are stated in the abstract ("…characteristics of convective invigoration are seen in the first few hours") and the conclusions ("In contrast to our modeling results, invigoration of convection is often observed…"), neither of which is supported by the figures.

e. Riming of graupel in the Milbrandt-Yau and Thompson schemes: The authors attribute the changes in graupel riming rate and hence graupel production and precipitation accumulation to changes in the cloud droplet sizes and number distribution (or mixing ratio for the Thompson scheme). However, the snow number concentration and slope parameter are also important in the riming efficiency equation, but no analysis is presented of the snow characteristics for different aerosol loadings, and only one mention of it was made in the text. What is the role of changes to snow number and size distributions?
f. Changes to the cold pools are mentioned several times throughout the discussion of the results. Yet there are no figures presented that have to do with cold pools and hence no justification for any of these statements.

Specific comments

4. Abstract lines 4-5: Stating that aerosol impacts on deep convection are model dependent is misleading. Some discrepancy may be due to microphysical schemes, but many previous studies have suggested that aerosol impacts on deep convective precipitation are environment- or cloud-type dependent, not model dependent.
5. P. 24090 line 20: It is much more likely that the vertical resolution, rather than the use of the z vs. eta vertical coordinate, would make a significant difference in the simulations presented here vs. in Morrison (2012), especially given that these are both idealized simulations with no topography. Did the authors intend to point out here that the vertical resolution rather than the vertical coordinate was different between the two studies?
6. P. 24091 line 1: It should be noted here that the study by Fan et al. (2009) was of *isolated* deep convection. This is an important point because multiple studies have shown that aerosol impacts on convection vary according to cloud type.
7. P. 24091 lines 12-13: It is not clear what point the authors are making with this sentence. Simulations with a large enough domain to include a spreading anvil and cold pool have been performed many times, including all the references listed above.
8. P. 24091 lines 15-16: This statement is not correct; there have been many studies of aerosol effects on deep convection that have utilized more than just a few aerosol concentrations, including many of the papers listed above.

9. P. 24091 lines 20-22: This is not necessarily true if the environment [i.e. humidity (Khain et al. 2008, JAS), wind shear (Fan et al. 2009, JGR), instability (Storer et al. 2010, JAS), etc.] is not varied. Many recent studies have pointed to the importance of the environment in influencing aerosol impacts on deep convection that may explain some of the discrepancies between models and observations and among models.
10. P. 24092 line 21: The 15 km radius moisture ring will not be a moisture source when the convection moves off of the initial thermal perturbation location, which could be on the order of minutes. It is not possible to assess the validity of this statement given that no spatial precipitation distributions are shown and no storm motions are given.
11. P. 24093 line 5: Can the authors please expand on the statement that relatively strong surface drag leads to the eventual dissipation of the convection?
12. P. 24093 lines 20-26: This discussion is confusing because the authors say that the microphysics schemes are two-moment, but then state that cloud droplet number concentration is prescribed. It should be clarified that the Morrison scheme is one-moment for cloud, and that the Thompson scheme is only two-moment for rain and ice but is one-moment for cloud, snow, and graupel.
13. P. 24093 line 27 – p. 24094 line 3: This discussion regarding the Milbrandt-Yau scheme is confusing. Can the authors please clarify how the concentration of activated CCN is calculated, given that there is no aerosol field and no explicit aerosol activation scheme?
14. P. 24096 line 20 – p. 24097 line 2: Preventing melting of graupel, especially if most of the surface precipitation accumulation is from melting graupel as inferred in this study and has been shown before, could significantly alter the dynamics and therefore the evolution of the simulation, as stated by the authors. Can the authors please provide some proof that this simulation is still representative, at least in a bulk sense, of the original simulation that includes melting?
15. P. 24098 line 10: *decrease* should be *increase* instead
16. P. 24098 lines 11-19: This entire discussion is confusing. In particular, can the authors please clarify how in-cloud droplet concentrations can decrease with increasing aerosol loading? This is completely counter-intuitive and some physical explanation is needed.
17. P. 24098 lines 20-24: It is not clear how this discussion regarding trends in the cold pool characteristics is related to the rest of the paragraph. Furthermore, if trends in the cold pool temperatures are important in explaining the results, a figure demonstrating this should be included.
18. P. 24101 line 24: What is meant by “differences in heating”?
19. P. 24101 line 27: Again, Fan et al. (2009) looked at isolated deep convection; the response to environmental shear is expected to be very different for isolated convection and supercells.

20. P. 24101 line 29 – p. 24102 line 1: But the sounding is still very moist throughout the troposphere. What is the LCL? The authors have not demonstrated the lack of secondary convection along the cold pool nor the spatial precipitation distribution in any of the simulations.
21. P. 24012 lines 11-17: This discussion and the physical chain of events described here is unclear.
22. Text on all figures, except figs. 5 and 10, is nearly impossible to read.
23. Figs. 2, 3, and 9: Please provide some justification for separating the accumulated precipitation between hours 0-4 and 4-10. The conclusions regarding precipitation sensitivity throughout the storm lifetime could change depending on this time division.