Atmos. Chem. Phys. Discuss., 14, C10810–C10843, 2015
www.atmos-chem-phys-discuss.net/14/C10810/2015/

Atmospheric Chemistry and Physics
Discussions
Open Access

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

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Received and published: 5 January 2015

*Figure 4 and Figure 6 in the previous Author’s Response were swapped. I apologize for this mistake. Other than this, everything is the same as the previous Author’s Response.

*Please note that all the figures mentioned below refer to the new figures attached to the Authors’ response, unless noted.

General Comments:

(1) Comments from Reviewer: This study investigates aerosol effects on a supercell storm through its full lifetime by performing 10âĂŘhour 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.

(2) Author’s response: We thank the reviewer for a very thorough and thoughtful review, which has helped us improve the original manuscript significantly. We acknowledge that we had neglected to cite some very relevant papers from the existing literature, and have done our best to correct for that in the revised manuscript.

Major Comments:

1. Introduction:

(1) Comments from Reviewer: 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.

(2) Author’s response: We are now citing most of the relevant works pointed out to us by the reviewer, and have also attempted to clarify what new contribution our study

makes, as follows:

a. (1) Comments from Reviewer: 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.

(2) Author's response: We now emphasize that our current work is one of few studies (the only study?) that capture the entire lifecycle of a supercell storm.

(3) Author's changes in manuscript: The manuscript has been modified so that the uniqueness of this study is clear.

b. (1) Comments from Reviewer: 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.

(2) Author's response: Thank you for pointing out these very relevant studies to us, we are now citing them in the introduction.

c. (1) Comments from Reviewer: Importance of graupel: many previous papers have noted the impact of the microphysical representation of graupel on modeling of deep convection, both nonâĂŘsupercellular 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-Â■âĂŘSelin 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.

(2) Author's response: We now cite several of the earlier studies that mention the importance of graupel representations in microphysics schemes, and clarify that in this respect our study merely confirms the conclusions of these previous studies.

d. (1) Comments from Reviewer: Convective invigoration (abstract line 20, p. 24090 line 9, p. 24099 line 3, p. 24102 line 11âĂŘ12): 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).

(2) Author's response: We have modified the manuscript to reflect that Rosenfeld et al. (2008) was not the first paper mentioning the idea of convective invigoration, and are citing some of the additional studies mentioned by the reviewer.

e. (1) Comments from Reviewer: 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) Author's response: We are now citing many more papers and are also striving to better place our results in the context of pre-existing literature.

2. Model setup: a. (1) Comments from Reviewer: 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.

(2) Author's response: We have now added a new figure (Fig. 1) to the manuscript showing the hodograph of the wind profile used in this study. It is quarter-circular in the lowest 2 km.

Caption Figure 1: Hodograph of initial wind profile, with values plotted every 500 m, and an additional value at 3 km (between 2.75 km and 3.25 km). The wind speed and the direction above 7.25 km are the same as those at 7.25 km.

(3) Author's changes in manuscript: Figure 1 has been added to the manuscript.

b. (1) Comments from Reviewer: 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?

(2) Author's response: The moisture profile was manually modified so that the lowest 3km has 80% (lowest level) to 100% (3km) of original moisture. Above 3km the moisture profile is exactly the same as the original one. The horizontal transition between the moist bubble and the drier surroundings is not gradual. In other words, initially there are sharp gradients in temperature and moisture along the edge of the heat bubble and the moisture ring, respectively. However, the existence of the moisture ring weakens the sharp transition in CAPE to some extent. The water vapor mixing ratio in the boundary layer is around 11 g/kg and 14 g/kg in the base sounding and inside the moisture ring, respectively.

(3) Author's changes in manuscript: We have now added more information regarding the initial moisture profile to the manuscript.

c. (1) Comments from Reviewer: 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 openâĂŘradiative boundary conditions to ensure the boundaries are not influencing the solution?

(2) Author's response: Open boundary conditions are not suitable for this study, as the domain would then keep losing mass with time, gradually making the simulation more unphysical and unrealistic. This is the main reason why we chose periodic boundary conditions for this study. As time progresses, the cold pool and anvil clouds spread, and they exit and re-enter the domain from the other side. However, we made sure that no "self-interaction" occurs within 10 hours (Fig. 2). Although gravity waves could re-enter the domain and interact with the convection later in the simulation, we feel that this is not necessarily a great concern. Gravity waves from various sources are frequently excited in the real atmosphere as well. Most importantly, this is still more realistic than an atmosphere that constantly loses mass.

Caption Figure 2: Air temperature at the lowest model level (left) and a rough outline (contour line at 0.00001) of vertically integrated ice mixing ratio [kg/kg] (right) with the Morrison (a), the Milbrandt-Yau (b), and the Thompson (c) schemes after 10h. For all microphysics schemes, the figure shows results from the cleanest case (0.2*control), because in these cases the cold pools spread the most.

(3) Author's changes in manuscript: We have clarified the reason and the validity of using the periodic boundary conditions for this study.

3. Figures and interpretation/discussion of results: a. (1) Comments from Reviewer: 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.

(2) Author's response: We appreciate the reviewer's suggestion and agree that it is important to clearly demonstrate the existence of a supercell in our simulations. We are now doing so by providing more figures as suggested, instead of qualitatively showing it in Fig. 12. Figure 3 is a plan view of the accumulated precipitation distribution in the control runs with each microphysics scheme, and Supplement 1 shows meridionally (left column) and zonally (right column) integrated amounts of all hydrometeors. It is clear that the convection is vertically extensive, the overshooting top reaches as high as 12km, and the surface precipitation comes from both the left- and right-moving cells. The movie also shows that the intensity, lifetime, and the timing of the storm development are all somewhat different, depending on the microphysics scheme.

Caption Figure 3: Plane view of the accumulated surface precipitation with the Morrison (a), the Milbdrandt-Yau (b), and the Thompson (c) schemes after 10 h. Please note that the figures are zoomed and only a part of domain is shown.

Caption Supplement 1 (movie): Meridionally (left column) and zonally (right column) integrated sum of all hydrometeors [kg/kg] in the Morrison (top), the Milbrandt-Yau (middle), and the Thompson (bottom) runs.

(3) Author's changes in manuscript: We have removed Fig. 12 in the new manuscript and clarified that the storm is vigorous and vertically extensive.

b. (1) Comments from Reviewer: 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?

(2) Author's response: The speed of the storm propagation can be inferred from Supplement 1 as it shows how fast the storm core moves zonally and meridionally. It moves approximately 100km in the first 4h, meaning its speed is approximately 25km/h. As can be seen in Fig. 3 and Supplement 1, the precipitation comes from both left- and

right-moving cells, but more from the left-moving cell. Supplement 2 shows storm relative helicity, which indicates the possibility of storm rotation. The movie clearly shows helicity of opposing signs for the right- and left-moving cells, and it is clear that there is relatively large helicity in the areas where it precipitates. Therefore, we conclude that the convection is not likely just multicellular, as the large helicity indicates rotating convection.

Caption Supplement 2 (movie): Storm relative helicity [m2/s2] (color) calculated while using the wind profile under 3000m, and the amount of precipitation in the past 10 minutes (black contour at 1mm), in the control run with the Morrison (top), the Milbrandt-Yau (middle), and the Thompson (bottom) schemes.

c. (1) Comments from Reviewer: 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 MilbrandtâĂŘYau 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.

(2) Author's response: Although it is challenging to identify the reason why the changes in the Morrison runs are not systematic, we agree that the original manuscript lacks a discussion on the possible explanations for this insensitivity. However, due to the numerous microphysical processes that are represented differently in the schemes, understanding why the results with two different microphysics schemes are different is not trivial. In this regard, the discussion is confined to the insensitivity of the Morrison

scheme, rather than a comparison.

(3) Author's changes in manuscript: We have added more thorough discussion of the results with the Morrison scheme, based on the results and discussions of Lebo et al. (2012) and Morrison (2012).

d. (1) Comments from Reviewer: 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.

(2) Author's response: We understand the reviewer's concern that the maximum/minimum vertical velocities might not adequately represent the intensity of the entire storm system. Figure 4-6 shows the frequency distribution of different vertical velocity bins. In the Morrison runs (Fig. 4), there seems to be a tendency of weakening in updraft/downdraft as aerosol increases, which was not clear in the original Fig. 2b. This result is actually consistent with the changes in the amount of accumulated precipitation; the runs with larger amount of rainfall have stronger updrafts/downdrafts. In the Milbrandt-Yau runs (Fig. 5), changes in vertical velocity with aerosol concentration are quite consistent with the changes in rainfall; in the histogram, we see the U-shape, and this is not just a temporary feature at 4 h. In the Thompson runs (Fig. 6), the vertical

velocity does not show any systematic change with aerosol concentrations. Therefore, the statement in the original manuscript P24099 L16-17 "though the vertical velocities hardly show this tendency (Fig. 9b)." stays unchanged. Thus, it depends on the microphysics scheme whether max/min vertical velocities are representative of the overall vertical velocity changes. Convection does not get invigorated with aerosol concentration in our simulations, but the delay in the onset of precipitation that precedes the convective invigoration is seen in this study. We considered this as one of the "characteristics of convective invigoration". We will modify the manuscript so that it does not mislead readers in this respect. Fig. 2a, 3a and 9a in our original manuscript show this delay. Convective invigoration is also characterized by an increase in the amount of precipitation. In this regard, our simulations did not show any evidence of convective invigoration. This is what we meant by "In contrast to our modeling results,...". We hope that this is clearer in the revised manuscript.

Caption Figure 4: Frequency distribution of different vertical velocity bins in the Morrison runs after 4h. In order to show the details of the bars, the Y-axis range is from 0 to 0.2 in (a) and from 0 to 20 in (b). 4 h is chosen because it is in the middle of storm activity.

Caption Figure 5: Same as Fig. 4 but in the Milbrandt-Yau runs.

Caption Figure 6: Same as Fig. 4 but in the Thompson runs.

(3) Author's changes in manuscript: The manuscript has been modified so that the confusion regarding the convective invigoration in our simulations is clarified.

e. (1) Comments from Reviewer: 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?

(2) Author's response: We have analyzed how the snow mixing ratio and the number concentration change with aerosol concentration, and found that those changes are not as significant as those in cloud droplets. Comparing Additional Figs. 7 and 8, we find that the increase in snow crystal number concentration is smaller than the increase in snow mixing ratio. This means that the size of snow crystals does not decrease drastically with aerosol concentration. Also, Fig. 7 shows that the increase in snow number concentration is, at most, just doubling (from the cleanest case to the most polluted case), which is a much smaller increase than that in cloud droplets. Therefore, we think that cloud droplets are the main factor contributing to the change in riming rates.

Caption Figure 7: Time evolution of horizontally averaged snow crystal number concentration [kg-1] in 8 runs with different aerosol concentrations; (a) 0.2*control, (b) 0.5*control, (c) control, (d) 2*control, (e) 3*control, (f) 4*control, (g) 5*control, and (h) 6*control in the Milbrandt-Yau runs.

Caption Figure 8: Time evolution of horizontally averaged snow mixing ratio [g/kg] in 8 runs with different aerosol concentrations; (a) 0.2*control, (b) 0.5*control, (c) control, (d) 2*control, (e) 3*control, (f) 4*control, (g) 5*control, and (h) 6*control in the Milbrandt-Yau runs.

(3) Author's changes in manuscript: In the revised manuscript we have now explained why we think there is a dominant influence from cloud droplets and a relatively small contribution from snow crystals on the changing riming rate with aerosol concentration.

f. (1) Comments from Reviewer: 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.

(2) Author's response: Instead of using the word "cold pool", we are now using "temperature at the lowest vertical level". This temperature information can be used as a proxy of cold pool strength. Figure 9 shows a time series of the temperature at the lowest vertical level in the Milbrant-Yau runs. According to this figure, the more it precipitates, the lower the temperature is. This is intuitive, as precipitation entails evaporation of rain in the lower atmosphere.

Caption Figure 9: Time series of the temperature at the lowest vertical level [C].

(3) Author's changes in manuscript: "Cold pool strength" is now modified to "temperature at the lowest level" that can be used as a proxy for the cold pool strength.

Specific comments:

4. (1) Comments from Reviewer: 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.

(2) Author's response: Unlike WRF, some models have only one microphysics scheme available. In this case, using different models generally means running simulations with different microphysics schemes. As demonstrated here and in previous studies, changes in aerosol concentration influence cloud microphysics differently depending on the microphysics scheme chosen. In this regard, aerosol-cloud interaction can be model-dependent. However, we appreciate the comment and agree that that the phrasing in the original paper could be interpreted as if the microphysics scheme difference is the only factor contributing to different cloud sensitivity to aerosol perturbations. We certainly agree that other factors (wind shear, humidity, etc.) could be equally, or more, important.

(3) Author's changes in manuscript: We have modified the manuscript accordingly.

5. (1) Comments from Reviewer: 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?

(2) Author's response: Having a higher resolution, whether horizontal or vertical, is important for cloud-resolving models to more accurately reproduce clouds. However, given limited computational resources and the large number of sensitivity simulations conducted in this study, (which had the same number of vertical levels as M12), we chose to prioritize resolution in the lower atmosphere by employing the eta coordinate. Conditional instability, as measured by CAPE values, is extremely sensitive to the resolution of the lowest levels. As shown in Fig. 10, our simulations have vertical resolution less than 500m in most of the cloud layers. Although the number of vertical levels is still 40, this finer vertical resolution in the lower atmosphere could contribute to a better representation of a realistic cloud system.

Caption Figure 10: Horizontally averaged vertical model levels at the beginning of the simulations

(3) Author's changes in manuscript: We mention the validity of using 40 vertical levels with eta-coordinate and have added Fig. 10 as Table 2 in the new manuscript.

6. (1) Comments from Reviewer: 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.

(2) Author's response: We have now clarified that the aerosol impacts on deep convection can be different depending on the cloud type.

(3) Author's changes in manuscript: The manuscript has been modified accordingly.

7. (1) Comments from Reviewer: 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.

(2) Author's response: Given the two requirements mentioned in the previous sentence, the open boundary condition is not suitable for this study, as the domain would then keep losing mass over the long simulation period. When periodic boundary conditions are used, the domain has to be large enough so that the spreading cold pool and anvil clouds do not interact with themselves after they re-enter the domain from the opposite side. This issue is not relevant for the simulations with open boundary conditions, as this self-interaction does not occur.

8. (1) Comments from Reviewer: 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.

(2) Author's response: Although most of the studies have usually had clean, polluted, and moderate aerosol concentrations (3 cases), or 5 cases in their simulations, we would like to thank the reviewer for making us aware some of previous studies that used a larger number aerosol concentrations. We are now referring to them in the revised manuscript.

(3) Author's changes in manuscript: We have cited these papers in the manuscript.

9. (1) Comments from Reviewer: P. 24091 lines 20âĂŘ22: 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.

(2) Author's response: Although we agree that the environmental factors could affect the aerosol impacts on deep convective clouds, inaccurate representation of aerosol effects on cloud microphysics could also contribute to discrepancies between models and observations. In this study we show some discrepancies between simulations that are solely due to the difference in microphysics scheme. Thus, some differences among models could also stem from differences in microphysics scheme. However, in our revised manuscript we will point to the importance of the environment under which clouds develop.

(3) Author's changes in manuscript: We mention the importance of environments in determining the aerosol effects on deep convective clouds.

10. (1) Comments from Reviewer: 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.

(2) Author's response: We agree that the moisture ring cannot be a large moisture source, given its small spatial coverage. The moisture ring is more important in making the transition in CAPE towards the center less abrupt. We will modify the manuscript accordingly.

(3) Author's changes in manuscript: The description of moisture ring is modified.

11. (1) Comments from Reviewer: 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?

(2) Author's response: Faster motion of cold pools would trigger faster upward motion along the edge of the cold pool, which would lead to stronger secondary convection because there is less time for entrainment to occur. Therefore, if the cold pools move slowly due to surface drag, there would be more time for entrainment, and weaker convection with less rainfall would occur. As evaporation of precipitation could strengthen cold pools, adding surface drag could lead to faster dissipation of cold pool, or the storm system.

(3) Author's changes in manuscript: Now the manuscript has been modified and the more thorough explanation for the connection between surface drag and the storm dissipation has been included.

12. (1) Comments from Reviewer: P. 24093 lines 20âĂŘ26: 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.

(2) Author's response: Thank you for pointing out this important point for describing the microphysics schemes accurately. Yes, now we have modified the manuscript so that it clarifies which hydrometeors are two-moment.

(3) Author's changes in manuscript: We have now included more detailed description of the microphysics schemes in the manuscript.

13. (1) Comments from Reviewer: P. 24093 line 27 – p. 24094 line 3: This discussion regarding the MilbrandtâĂŘYau 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?

(2) Author's response: In the Milbrandt-Yau scheme, the number of activated CCN is calculated with the equation (10) in Cohard et al. (1998). This calculation assumed background aerosol concentration of 842/cm3 (in their Table 1) with continental aerosols. In their study explicit aerosol activation is included. By using the relationship

between supersaturation and the number of activated CCN obtained by Cohard et al. (1998), the Milbrandt-Yau scheme is able to calculate the number of activated CCN, though this scheme itself does not include explicit aerosol activation scheme.

(3) Author's changes in manuscript: We explain more clearly how the number of activated CCN is calculated in the Milbrandt-Yau scheme.

14. (1) Comments from Reviewer: 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?

(2) Author's response: These simulations could indicate which hydrometeor is most responsible for the surface precipitation only if there is any drastic change in the state (liquid/solid) of precipitation when some hydrometeors do not melt. Thus, if the fraction of frozen precipitation, as shown in Fig. 5 and Fig. 10 in the original manuscript, does not increase drastically, we are not able to attribute the precipitation to one specific type of hydrometeors. When hail does not melt in the Milbrandt-Yau runs, the frozen fraction stayed low (nearly zero), whereas it rose drastically (as in Fig. 5) when graupel did not melt. In the Thompson runs, the fraction was nearly zero in the standard cases, whereas it increased drastically when graupel does not melt. Although later in the simulation the fraction lowers and we attribute this to storm dynamics that is most likely different from the standard cases, the fact that the first few hours of frozen fraction drastically increased in both the Milbrandt-Yau and Thompson runs suggests the dominance of graupel in producing precipitation. However, we understand that graupel is not the only hydrometeor contributing to the surface precipitation, and the manuscript will be modified so that it does not give such a wrong impression.

(3) Author's changes in manuscript: We have now made it clearer in the manuscript

how these simulations should be interpreted, and we clearly state that graupel is not the only hydrometeor contributing to precipitation.

15. (1) Comments from Reviewer: P. 24098 line 10: decrease should be increase instead

(2) Author's response: We have modified the manuscript accordingly.

16. (1) Comments from Reviewer: P. 24098 lines 11âĂŘ19: 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.

(2) Author's response: Those specific runs have anomalously short cloud lifetimes that contributed to the anomalous domain-average cloud droplet number concentration. We have checked the in-cloud average droplet number concentration and found that it monotonically increases with aerosol concentration (Fig. 11). The original manuscript is now modified accordingly. The short-lived runs are anomalous compared to all the other simulations shown in Fig. 7 in the original manuscript, and we cannot offer any physical explanations to this apparent randomness. We chose to focus on the overall behavior shown by the majority of the runs. As for the whole discussion, we have now tried to rewrite it so that it is less confusing to the reader.

Caption Figure 11: Time evolution of horizontally averaged cloud droplet number concentration [cm-3] in 8 runs with different aerosol concentrations; (a) 0.2*control, (b) 0.5*control, (c) control, (d) 2*control, (e) 3*control, (f) 4*control, (g) 5*control, and (h) 6*control in the Milbrandt-Yau runs.

(3) Author's changes in manuscript: We have modified the statement that in-cloud droplet is anomalously low in some runs.

17. (1) Comments from Reviewer: 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.

(2) Author's response: The runs with a larger amount of accumulated precipitation (original Fig. 3) tend to have a lower temperature at the lowest vertical level (Fig. 9). This temperature is a proxy for the cold pool strength. The stronger cold pool is induced by a larger amount of rain evaporation. As is shown in Fig. 12 however, the vertical motion along the edge of the cold pool is not strong enough to produce surface precipitation there.

Caption Figure 12: Temperature at the lowest vertical level (color) and accumulated precipitation (contours at 1 and 20 mm) distribution after 10h of simulations in the Morrison (a), Milbrandt-Yau (b), and Thompson (c) runs.

(3) Author's changes in manuscript: Now Fig. 12 is added to the manuscript.

18. (1) Comments from Reviewer: P. 24101 line 24: What is meant by "differences in heating"?

(2) Author's response: By "differences in heating" we meant the differences in the initial heat bubble temperature.

(3) Author's changes in manuscript: We have now rewritten the sentence to make this clear.

19. (1) Comments from Reviewer: 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.

(2) Author's response: We understand that the response of a supercell could be different from that of an isolated cell. We have modified the manuscript to reflect that.

20. (1) Comments from Reviewer: 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.

(2) Author's response: The LCL of the base sounding is 1469m (843 hPa). According to Fig. 12, the precipitation pattern does not follow the edge of the spreading cold pool, indicating that there is no moist convection along the edge.

(3) Author's changes in manuscript: We have now included Fig. 12 in the manuscript.

21. (1) Comments from Reviewer: P. 24012 lines 11-17: This discussion and the physical chain of events described here is unclear.

(2) Author's response: We think that once a lot of graupel forms in the simulations, the storm is invigorated by the huge release of latent heat and the strong cold pool, and it keeps producing a lot of cold rain. Therefore, in order to reduce the portion of cold precipitation, vertical velocities have to be weaker initially and less graupel should be produced in the first place. Thus, we think that the way we initiate the convection could also influence the results.

(3) Author's changes in manuscript: A part of modified conclusions: "If we attribute it (the increase in precipitation) to the convective invigoration proposed by previous studies (Andreae et al., 2004; Khain et al., 2005; Rosenfeld et al., 2008; van den Heever et al., 2006), then the contribution of cold rain to the total precipitation might be too high in these simulations. That is, if more warm rain is produced, the droplet availability for cold rain would be drastically different from case to case, depending on the aerosol concentration. Such droplet availability is one of the key factors differentiating polluted clouds from clean clouds in the convective invigoration theory. Currently initial vertical motions are so strong that a lot of droplets are transported aloft in all cases and available for graupel and cold rain formation. This dominance of cold rain could possibly be avoided by having gradual and weaker heating of the surface air, instead of having a strong heat bubble; if the initial upward motion is weakened by gradual/weaker heating and there is more time for warm rain to form, less droplets would be left to produce

graupel, and this change in droplet availability may eventually allow the convective invigoration to occur in the simulations."

22. (1) Comments from Reviewer: Text on all figures, except figs. 5 and 10, is nearly impossible to read.

(2) Author's response: We have changed the texts on the figures accordingly.

23. (1) Comments from Reviewer: 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.

(2) Author's response: The demarcation point is 4h (or 2h in the no-wind cases) because it is roughly the time when about half of the total precipitation reaches the ground in the Milbrandt-Yau runs that we analyze intensively in our study. It is 2h in the no-wind cases because the storm lifetime is much shorter in those runs. However, the conclusions do not change with the choice of this demarcation point. The discussions for the Morrison and the Milbrandt-Yau runs do not depend on this timing, and according to original Fig. 9(a), the Thompson runs also give the same conclusion even if we choose another timing during when the storm precipitates intensely.

(3) Author's changes in manuscript: We have clarified in the manuscript why those demarcation points are chosen.

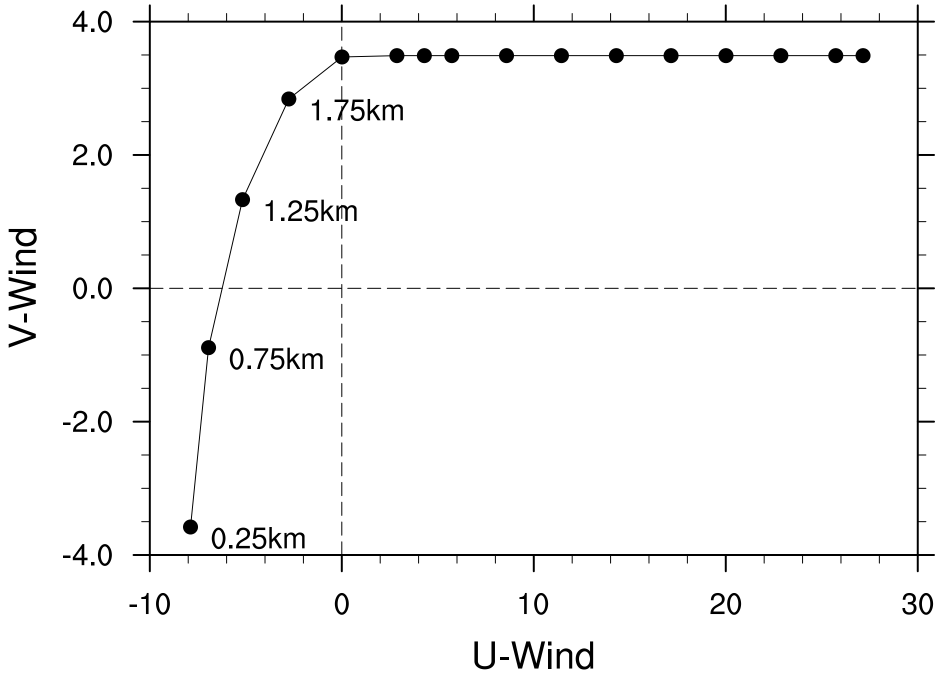
Additional Reference:

Lebo, Z. J., Morrison, H., and Seinfeld, J. H., 2012: Are simulated aerosol-induced effects on deep convective clouds strongly dependent on saturation adjustment?, Atmos. Chem. Phys., 12, 9941-9964.

Please also note the supplement to this comment:
http://www.atmos-chem-phys-discuss.net/14/C10810/2015/acpd-14-C10810-2015-

supplement.zip

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 24087, 2014.



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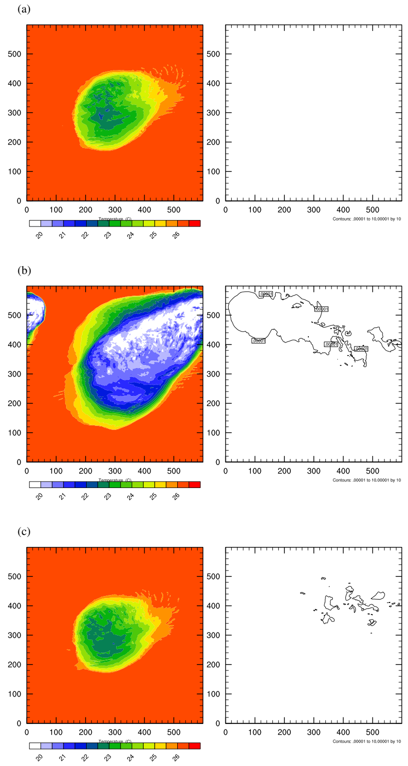

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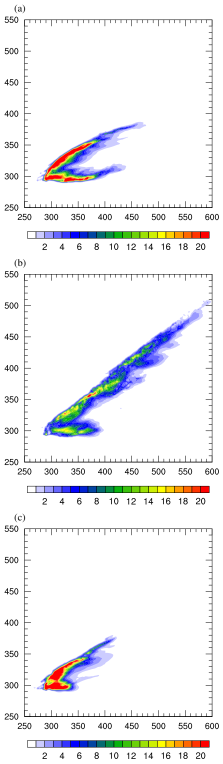

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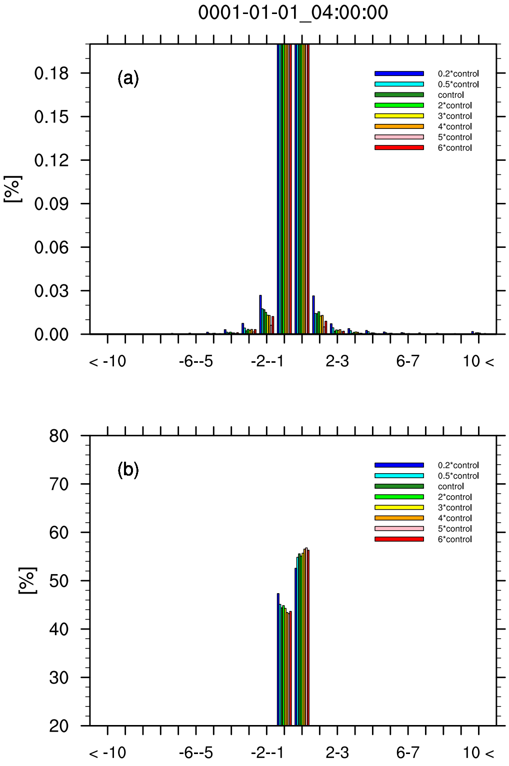

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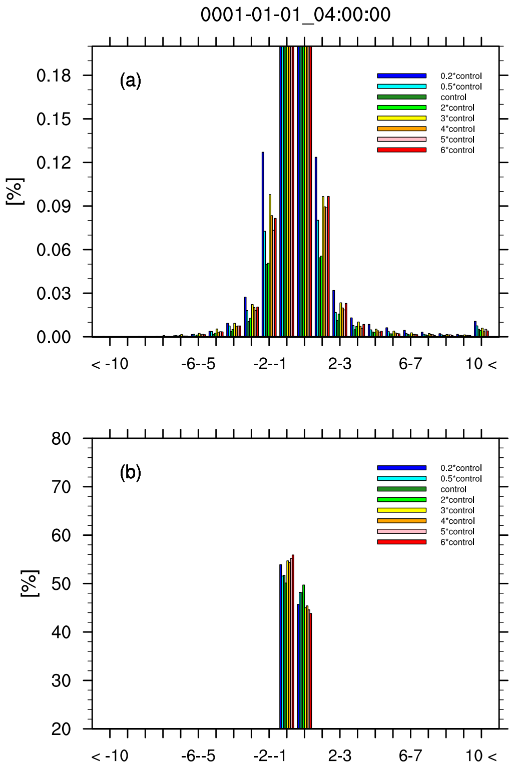

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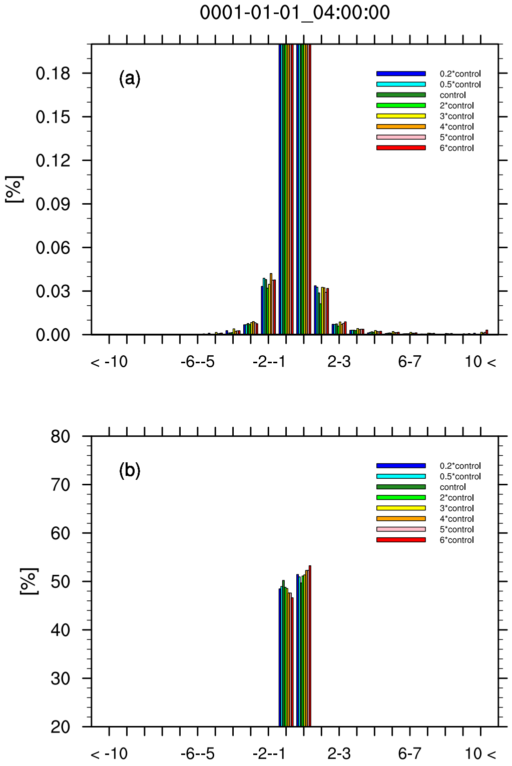


Fig. 6. Caption is in the text

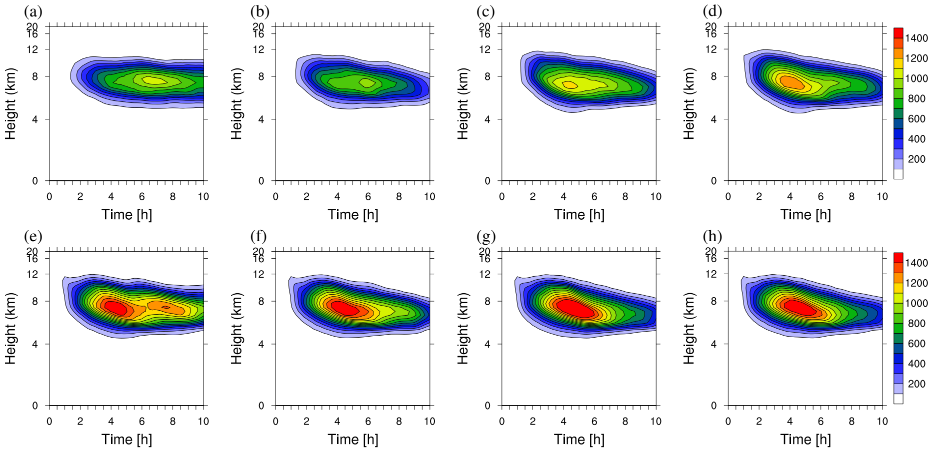
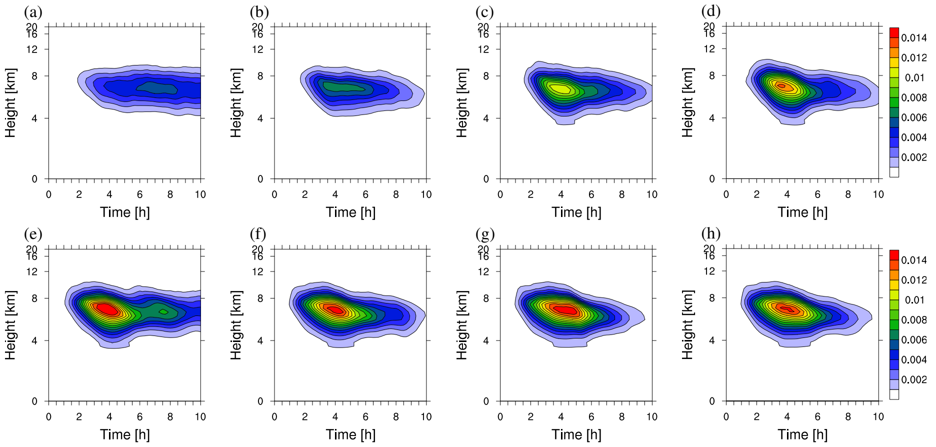


Fig. 7. Caption is in the text



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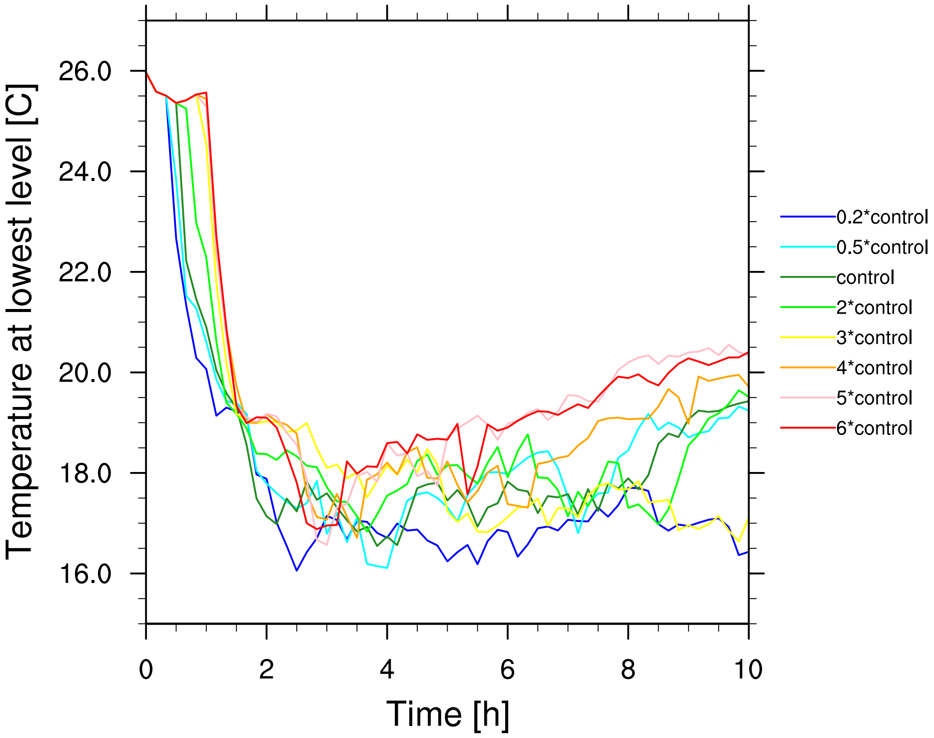

Fig. 9. Caption is in the text

Level	Height [m]
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4	759.2
5	985.6
6	1216.7
7	1452.8
8	1693.9
9	1940.3
10	2192.4
11	2450.3
12	2714.5
13	2985.5
14	3263.5
15	3549.1
16	3842.7
17	4145.0
18	4456.4
19	4777.8
20	5110.0
21	5453.8
22	5810.3
23	6180.6
24	6566.0
25	6967.9
26	7388.1
27	7828.7
28	8292.1
29	8780.9
30	9298.3
31	9848.7
32	10438.0
33	11072.3
34	11761.3
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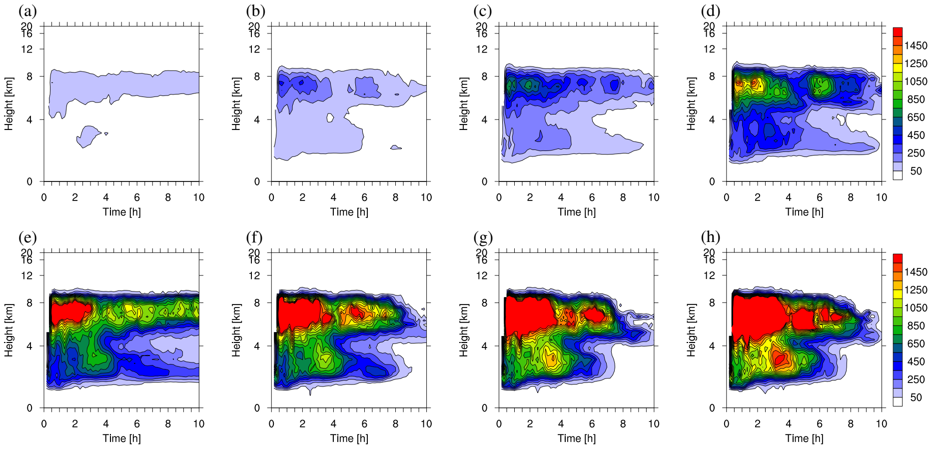
37	14392.0
38	15580.1
39	17038.2
40	18921.3

Fig. 10. Caption is in the text



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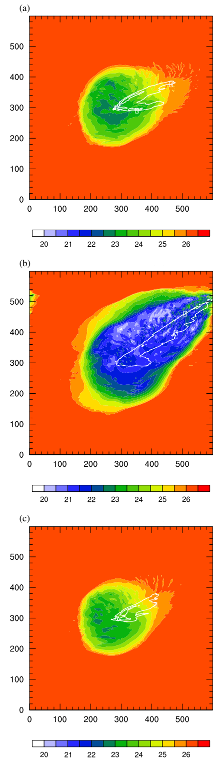

Fig. 12. Caption is in the text