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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

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I. General Comments: (1) Comments from Reviewer: 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) Author's response: We thank the reviewer for a very thorough review and

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many thoughtful comments and suggestions, which have helped us to improve the manuscript significantly.

II. Major Comments: 1. Introduction: (1) Comments from Reviewer: 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) Author's response: We appreciate the reviewer's suggestions that we should include a more thorough literature reviews and also appreciate the list of studies he/she provided. We agree that our introduction did not clearly explain what studies have been done so far, as well as what additional information we can provide with our current study. The original manuscript is modified and the reviewer's suggestions are reflected. (3) Author's changes in manuscript: The new manuscript now includes more relevant references and the introduction is rewritten in order to place the present study in a better context.

2. Model Setup and Initialization: (a) (1) Comments from Reviewer: 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. (2) Author's response: Our main goal is to simulate a supercell storm throughout its lifetime. Given its relatively long lifetime, the open boundary condition is not appro-

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priate, as the domain would keep losing mass and the simulation would become more unphysical with time. With the periodic boundary condition, this issue can be avoided. At the same time, however, the periodic boundary condition may allow the spreading anvils and cold pools to interact with themselves if they spread out of the domain and come back from the other side. We initially ran all the simulations with a 200km*200km domain to find that this unrealistic "self-interaction" indeed happens. Therefore, we changed the domain size to 600km*600km and made sure that this interaction does not take place within 10 hours of our simulations, even in the most vigorous convective case in which the cold pool spreads quite quickly (Fig. 1).

Caption Figure 1: 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 now added a paragraph to Section 2 briefly explaining the above.

(b) (1) Comments from Reviewer: 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. (2) Author's response: We are thankful to the reviewer for making this very valid comment, which lead us to discover that the original vertical tick marks in Fig. 4, 6, 8, and 11 were in fact incorrect. The original tick marks assumed constant dz, which was not the case in our study (eta coordinate). Now those figures are updated with correct tick marks. According to the new figures, even the overshooting top does not reach the damping layer (15 km or above). Moreover, we have conducted additional simulations in which the model top was extended up to 24 km, as was suggested by the reviewer. The cloud top height is nearly the same in these simulations (Fig. 2). Thus, we conclude that the damping layer is not affecting our results, and the use of 20km as a domain top is appropriate in our study.

Caption Figure 2: Meridionally integrated sum of all hydrometeors [kg/kg] in the Morrison (a), the Milbrandt-Yau (b), and the Thompson (c) runs with a 20km-top (left) and a 24km-top (right) after 50 minutes. This time is around when the cloud reaches the highest level. (3) Author's changes in manuscript In the new manuscript we clearly explain the validity of using 20 km as a model top in this study.

(c) (1) Comments from Reviewer: 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. (2) Author's response: We have now added a new table (Fig. 3) listing the 40 vertical levels in the simulations at the beginning of the runs. These are obtained from zonally and meridionally averaged geopotential height values. Please note that these heights vary temporarily and spatially in the simulations, as we employ the eta coordinate. We agree with the reviewer that resolving the cloud layers is important, but having fine vertical resolution below the cloud base is also important especially in determining instability. According to Fig. 3, the vertical resolution starts exceeding 500m above around 9km. This indicates that most of the cloud layers are resolved at 500m or even finer resolution. Moreover, it seems reasonable to have many points in the lower atmosphere and fewer in the stratosphere by employing the eta coordinate, instead of having a constant vertical spacing. We have conducted additional simulations with doubled vertical resolution (80 levels). Although the results are not expected

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to be identical, the clouds still give similar rainfall pattern (Fig. 4) in these simulations. Given the many sensitivity simulations performed in this study, running all of them with 80 vertical levels would be prohibitively expensive computationally. Thus, we conclude that 40 levels is a good compromise, which allows a high number of simulations while still reasonably resolving the cloud systems.

Caption Figure 3: Horizontally averaged vertical model levels at the beginning of the simulations Caption Figure 4: Accumulated precipitation [mm] in the standard cases (a, c, e) and the cases with doubled vertical resolution (b, d, f) with the Morrison (a, b), the Milbrandt-Yau (c, d), and the Thompson (e, f) runs after 10h. (3) Author's changes in manuscript: We discuss the validity of using 40 vertical levels with eta-coordinate and have added Fig. 3 as Table 2 in the new manuscript.

(d) (1) Comments from Reviewer: 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). (2) Author's response: Although Fig. 12 could mislead readers into thinking that some storm systems never reach a mature stage, they actually all do. Since the magnitude and the speed of storm development differ depending on the choice of microphysics scheme, Fig. 12 does not capture the most developed stage of the storms in all three cases at the same time. Supplement 1 shows that all of the storms are vertically extensive and have anvil clouds and an overshooting top, though their magnitudes vary. The base sounding is moist enough to produce deep convection with a cold pool that spreads quite extensively throughout the domain (Fig. 1). We initially ran some simulations using the original Weisman and Klemp (1982) sounding as a base sounding, but in those simulations the atmospheric condition was found to be too (conditionally) unstable for the storm to stop precipitating. As for the updrafts/downdrafts, we intended to reduce the maximum vertical velocities by making the environmental sounding drier and also adding surface drag. This is because 40-60m/s maximum vertical velocities are too high compared to what is usually observed in supercell storms (Bluestein et al., 1988; Marshall et al., 1995; Stolzenburg et al., 1998). We think that our storms have relatively realistic vertical velocities and realistic storm termination.

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.

3. Analysis: (1) Comments from Reviewer: 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 depen-

dent 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. (2) Author's response: We understand your concern that the maximum and minimum vertical velocities at one point in the domain might not represent the changes in the entire storm system. Now we present the frequency distribution of different vertical velocity bins for each run (Fig. 5-7). In the Morrison runs (Fig. 5), 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. 6), 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. 7), 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.

Caption Figure 5: 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). 4h is chosen because it is in the middle of storm activity. Caption Figure 6: Same as Fig. 5 but in the Milbrandt-Yau runs. Caption Figure 7: Same as Fig. 5 but in the Thompson runs. (3) Author's changes in manuscript We have added Fig. 5-7 in the new manuscript and have extended the discussion on the vertical velocity changes with aerosol concentrations.

(1) Comments from Reviewer: 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. (2) Author's response: The fraction of frozen precipitation is calculated only if the amount of precipitation in the past 10 minutes is larger than 0 mm. If there is no precipitation in the past 10 minutes, the fraction is set to zero. In the simulations without melting of certain hydrometeor(s), the amount of precipitation is relatively small at the end of the simulations (Fig. 8). However, it should be noted that in these simulations the whole dynamics of the storm could be different from the standard cases especially later in the simulations, given the lack of latent heat. Thus we do not consider this figure quantitatively. What can be inferred from these simulations is which hydrometeor is in general most responsible for the surface precipitation. Moreover, this can be inferred only if there is any drastic change in the state (liquid/solid) of precipitation when some hydrometeors do not melt.

Caption Figure 8: Time evolution of percentages [%] of frozen precipitation in total precipitation reaching the surface in the past 10 minutes (solid, left), when graupel and hail do not melt in the runs with the Milbrandt-Yau scheme. Precipitation rates in the past 10 minutes are plotted too (dashed, right). If there is no surface precipitation in the past 10 minutes, the percentages are set to be zero. (3) Author's changes in manuscript The manuscript is modified so that the intent of these additional simulations is clearer.

(1) Comments from Reviewer: 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. (2) Author's response: The bar charts are updated so that all of them have the same y-axis range, except for the no-wind runs. This is because the amount of precipitation from the no-wind runs is quite different from (much less than) the other runs, due to the lack of vertical wind shear that allows supercells to live for a long time. The demarcation point is 4 h (or 2 h in the no-wind cases) because

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it is roughly the time when about a half of the total precipitation reaches the ground with the Milbrandt-Yau microphysics scheme that we analyze extensively in our study. It is 2 h in the no-wind cases because the storm lifetime is much shorter in those runs. (3) Author's changes in manuscript We have now added a more thorough discussion of the bar charts in the text. Figs. 2, 3, and 9 are updated in the new manuscript and the reason for using 4 h as a demarcation point is explained in the figure caption.

4. (1) Comments from Reviewer: 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. (2) Author's response: We agree, and have made sure that it is clear throughout the new manuscript that our findings from this study are specifically applicable to supercells, and may not be valid for deep convective clouds in general. (3) Author's changes in manuscript The manuscript is modified to reflect that our findings are not necessarily valid for all forms of deep convection.

5. (1) Comments from Reviewer: 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? (2) Author's response: The importance of graupel is just one of several conclusions in this study. In addition to that, we have shown the dependency of the results on microphysics scheme, and the importance of testing several aerosol concentrations throughout the lifecycle of a storm. The original manuscript is modified so that those conclusions are clearly summarized throughout the text. (3) Author's changes in manuscript: We now clearly identify all the main conclusions above

in the new manuscript.

III. Minor Comments: 1-5. (1) Comments from Reviewer: 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. (2) Author's response: We have modified the manuscript as suggested.

6. (1) Comments from Reviewer: 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. (2) Author's response: The droplet number concentration was calculated, under the assumption that the ideal gas law holds for the

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density calculation (Fig. 9). It is clear that the droplet number concentration monotonically increases as the number of activated CCN increases in the simulations (Fig. 10). Also, the number concentrations generally seem to be higher than the prescribed values in the Morrison and the Thompson runs. However, it is unclear if the increase is linear or not from these figures.

Caption Figure 9: 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. Caption Figure 10: Frequency distribution of different cloud droplet number concentration bins in the Milbrandt-Yau runs after 4h. The distribution does not change much with time, and 4h is chosen because it is in the middle of storm activity.

7. (1) Comments from Reviewer: 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. (2) Author's response: We decreased the horizontal resolution to see if we can draw the same conclusions from these simulations with less computational expense. (3) Author's changes in manuscript: The new manuscript explains why we have run some simulations with a lower horizontal resolution of 2 km.

8-11. (1) Comments from Reviewer: 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". (2) Author’s response: We have modified the manuscript as suggested.

12. (1) Comments from Reviewer: 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. (2) Author’s response: Instead of “cold pool strength”, we now refer to “temperature at the lowest model level” in the new manuscript.

13-14. (1) Comments from Reviewer: 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). (2) Author’s response: We have modified the manuscript as suggested.

15. (1) Comments from Reviewer: 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. (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 Rosenfeld et al. (2008), 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."

IV. General Comments: 1. (1) Comments from Reviewer: 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) Author's response: We now refrain from the term "fine resolution" when describing our simulations, and also have included references to several studies which used higher resolution than this study.

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2. (1) Comments from Reviewer: 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). (2) Author's response: Thanks for bringing the Lebo et al.-study to our attention, we are now referring to it in our discussion of the simulations with the Morrison parameterization.

3-4. (1) Comments from Reviewer: 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. (2) Author's response: We apologize for typos and inconsistencies in the original manuscript, and have done our best to correct these in the revised version.

Additional References:

Bluestein, H. B., E. W. McCaul Jr., G. P. Byrd, and G. R. Woodall, 1988: Mobile sounding observations of a thunderstorm near the dryline: The Canadian, Texas storm of 7 May 1986, Mon. Weather Rev., 116, 1790-1804.

Marshall, T. C., W. D. Rust, and M. Stolzenburg, 1995: Electrical structure and updraft speeds in thunderstorms over the southern Great Plains, J. Geophys. Res., 100, 1001-1015.

Stolzenburg, M., W. D. Rust, and T. C. Marshall, 1998: Electrical structure in thunderstorm convective regions: 2. Isolated storms, J. Geophys. Res., 103, 14079-14096.

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Please also note the supplement to this comment:
http://www.atmos-chem-phys-discuss.net/14/C10682/2015/acpd-14-C10682-2015-supplement.zip

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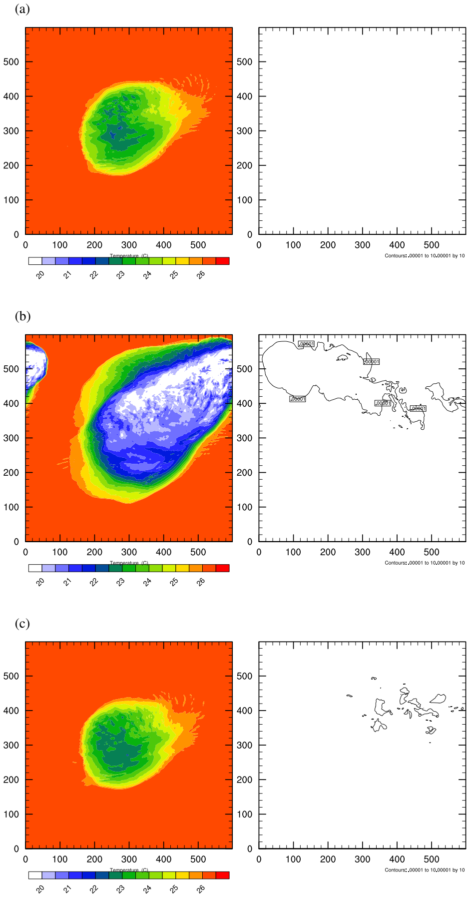

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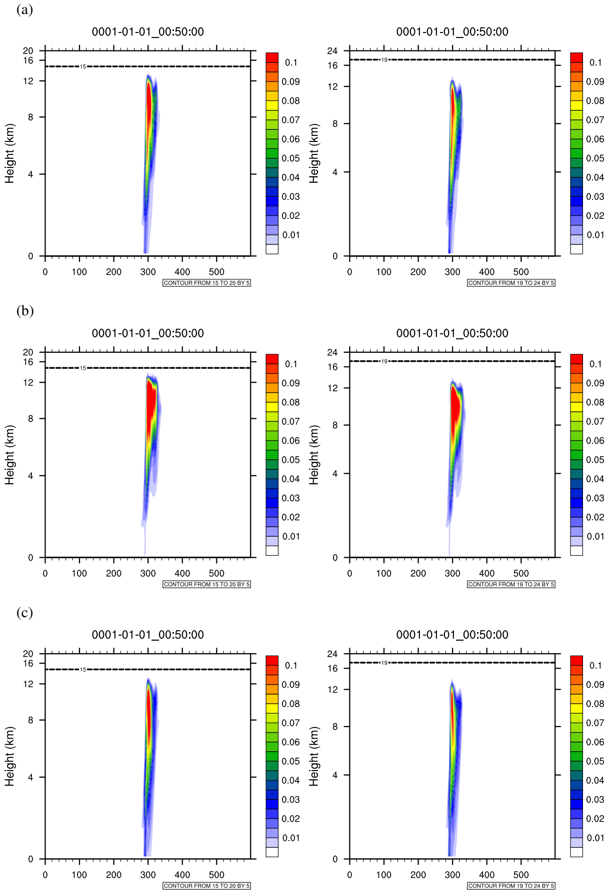

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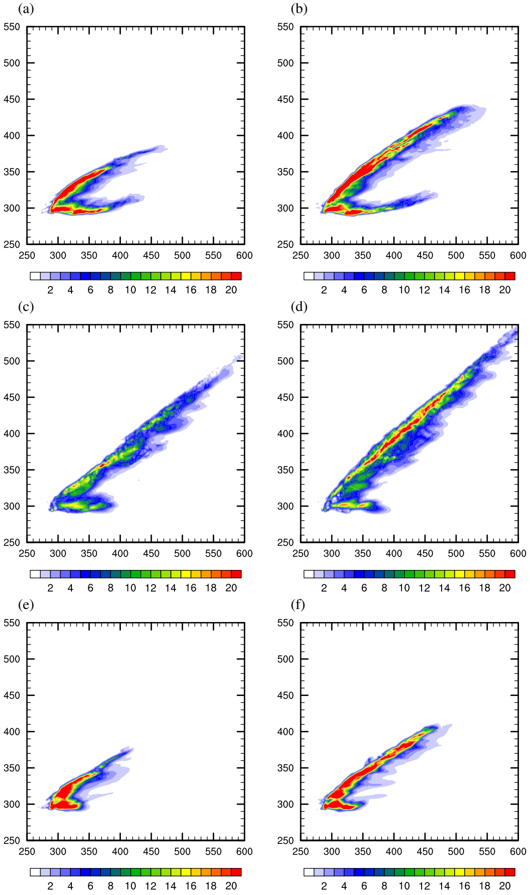

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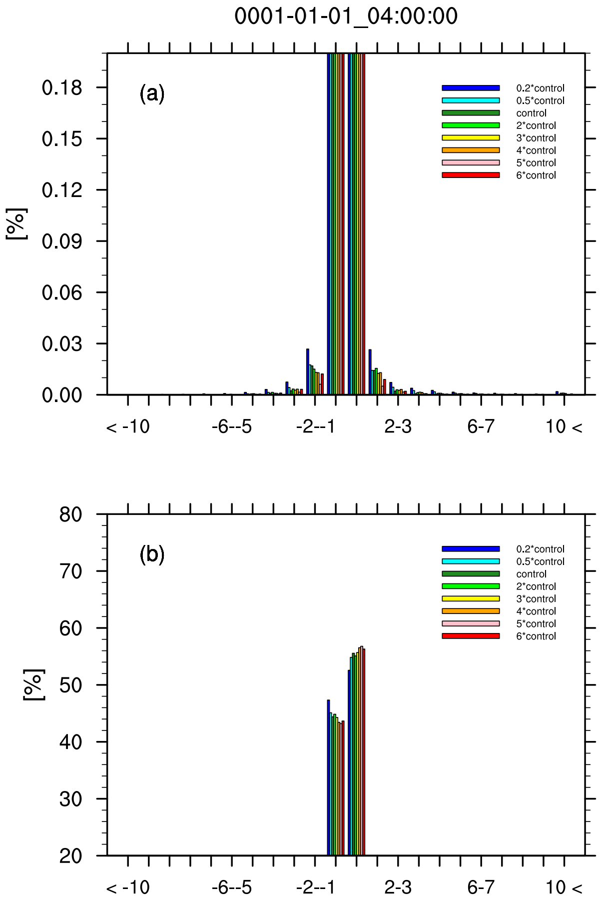

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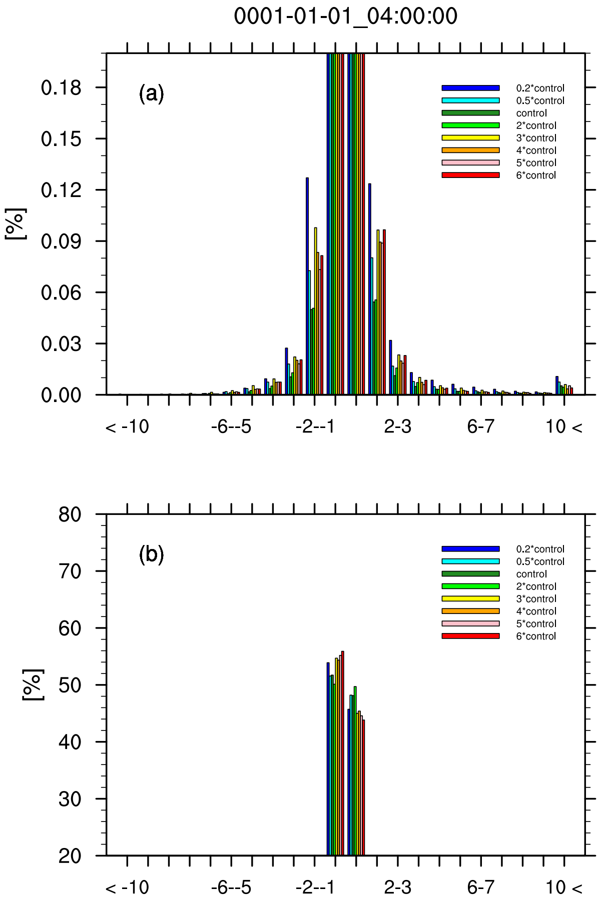

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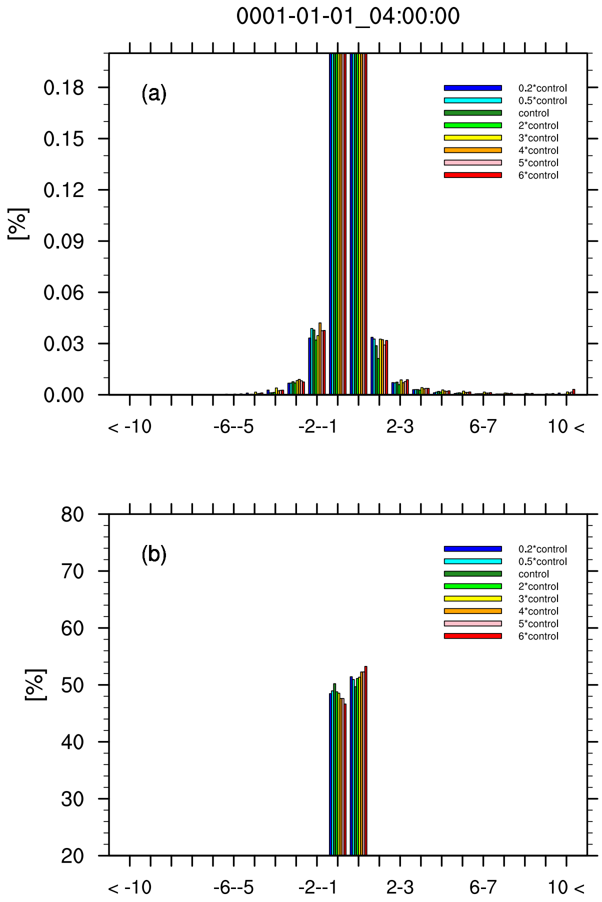

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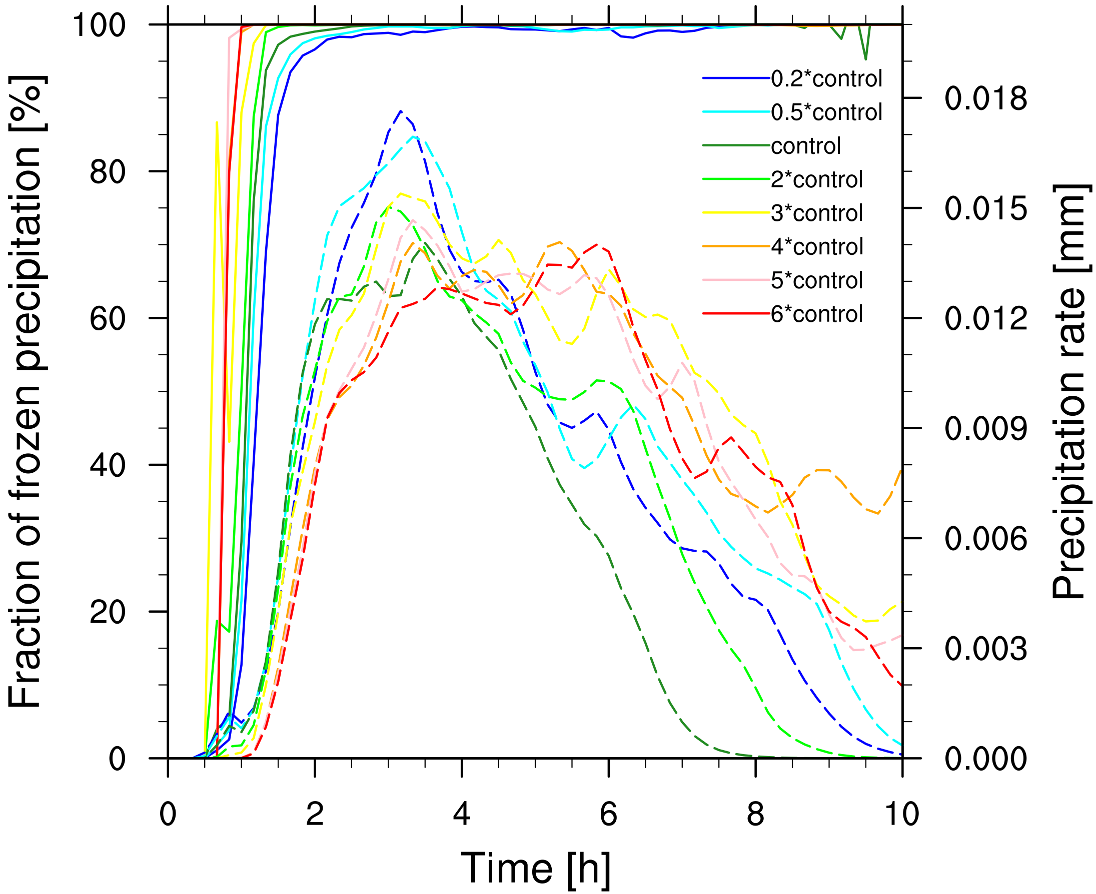

Fig. 8. Caption is in the text

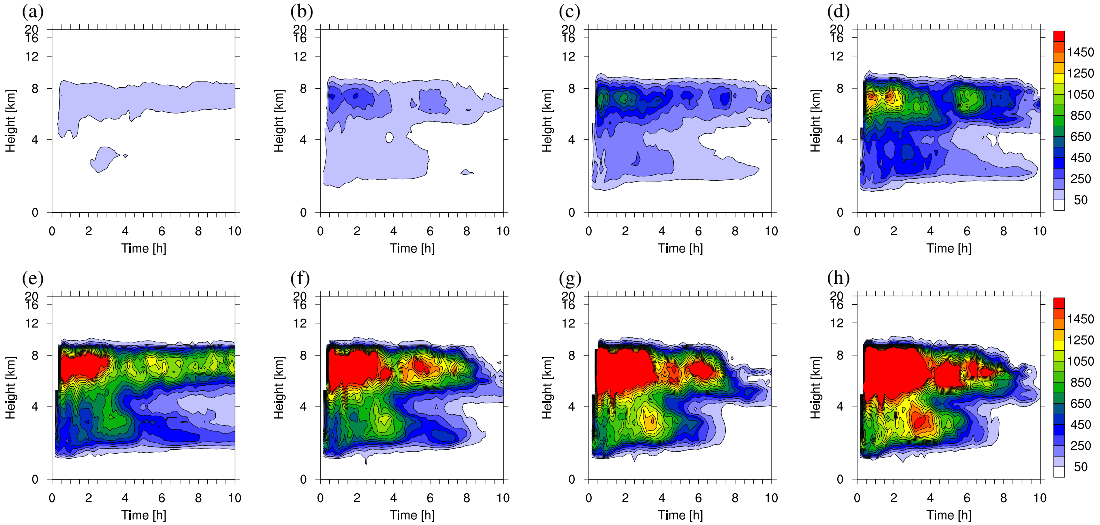

Fig. 9. Caption is in the text

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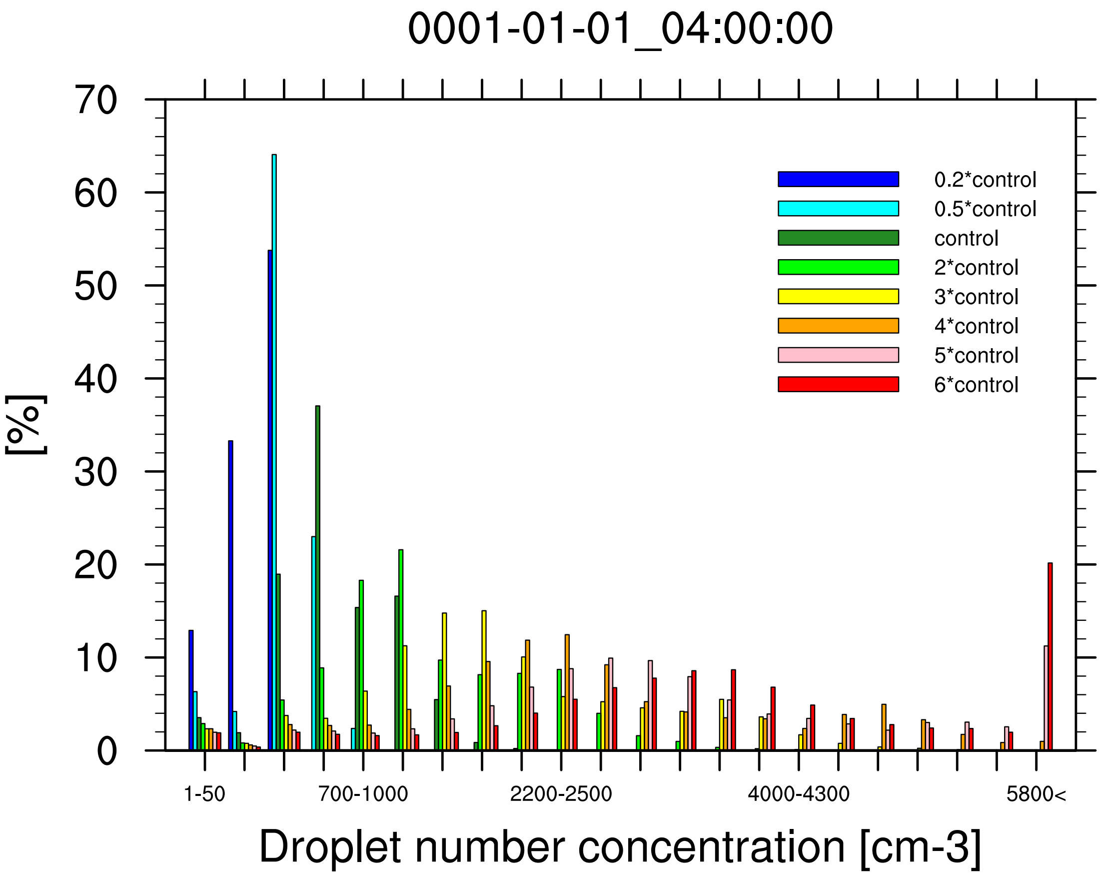

Fig. 10. Caption is in the text

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