Interactive comment on “The propagation of aerosol perturbations in convective cloud microphysics” by Max Heikenfeld et al.

Authors’ response to Anonymous Referee #3

We would like to thank the reviewer for their detailed comments and suggestions. The feedback pointed out important aspects that required additional clarity or information and helped us a lot to improve these points in the revised manuscript.

In the following, we respond to the reviewer’s comments in **black**, with our answers to the comments in blue and the adapted text from the revised manuscript in green.

We have attached the revised version of the manuscript with tracked changes to the general authors' response. In the general authors’ response (AR), we have added a few additional comments regarding the revised manuscript and points raised by both reviewers.

General comments:

The authors present an analysis of microphysical processes in idealized simulations of deep convective clouds for different aerosol concentrations and three different microphysics schemes. Novel visualization techniques are presented to show the temporal and spatial evolution of the processes and the associated latent heating. A focus of the analysis is whether the “invigoration hypothesis” by Rosenfeld et al. (2008) can be confirmed (and in can not).
This last point is quite interesting and the main reason why I recommend this paper for publication. The manuscript is very well written, and the plots are clear (though a bit small for my taste).
The comparison of the microphysics schemes doesn’t go into depth, and it is a bit unclear what the intention behind the presentation of three schemes is. In particular, the third scheme (SBM) is only shown for a subset of the analyses, although it deviates substantially from the other two. I recommend changes to clarify these points.

We answer to the points raised here (size of the pie chart plots and the choice of analysis for the three microphysics schemes) in more detail where they were raised in the respective detailed comments.

Detailed comments:

1. The abstract mentions that three schemes are used, but not what the benefits of the comparison are. Do they give consistent results regarding the invigoration effect? Can anything be learned from the comparison (e.g. regarding depositional growth of different ice species, which has caused a huge difference)?

We have adapted the abstract to give a clearer overview of our approach and the most important results of the analysis.

2. page 3, line 11-16: here the logical flow is unclear. Why is there a separate paragraph on Glassmeier and Lohmann? This needs an introductory sentence.

We have included this study in the overview of the existing literature since it provides an different approach to understanding the pathways by focussing on an analytical analysis of the equations implemented in a microphysics scheme. We have shortened this section in the revised manuscript and merged it into one paragraph with the overview of other existing studies using numerical simulations with cloud-resolving models:

“In addition to the analysis of process rates in numerical simulations, analytical evaluations of the microphysical rate equations of the microphysics schemes can give important insights into the propagation of aerosol effects in the cloud microphysics (Glassmeier and Lohmann, 2016). This
kind of analytical approach works well for warm-phase clouds but is less conclusive for the response of mixed-phase clouds, especially deep convective clouds, due to many compensating effects and the complexity of the processes involving ice-phase hydrometeors (Glassmeier and Lohmann, 2016).” (Page 2, line 14)

3. The (main) text is not very clear about how many cells are simulated and how the analysis is done when there are two cells. (I assume that you have always either one or two cells, and that the properties of the two cells are averaged, but I have not found this clearly in the text. Maybe I just missed it.)

The tracking algorithm identifies the updraft in the initial cell and then after the split, follows the right-moving cell for the rest of the evolution (red in Fig. 1). All our analysis follows the evolution of this combination of the initial cell and the right moving cell. The second cell (yellow in Fig.1) after the split moving leftwards is picked up as a separate cell. We performed the same analyses for that second cell (not shown) which gave very similar results. Similarly, the dominant cell in the second case, which shows a stronger asymmetry in the magnitude of the two individual cells, is used for all analyses in CASE2. See also answer to comment 4 by Referee #1.

We have adapted the text in the methods section of the revised manuscript (section 2) to explain this more clearly:

“The tracking algorithm does not explicitly treat splitting and merging of convective cells. In all simulated cases in this study, the initial convective cell splits into two separate counter rotating cells early into the simulations. In CASE1 this leads to a relatively symmetric situation with similarly strong individual cells. In both cases, one of the cells develops more directly out of the initial cell, in CASE1 this is the right-moving cell, while in CASE2 this is the stronger left moving cell. In each simulation, this stronger cell gets picked up as a continuation of the initial cell by the tracking algorithm. The second cell has been analysed following the same methodology and showed very similar results in all major aspects. We have thus decided to focus on the analysis of the first cell in this paper and to not discuss the results from the second cell in more detail.” (Page 9, line 12)

4. The model setup description needs more information to make the study reproducible. In particular, Weisman and Klemp (1982, 1984) describe several versions of their idealized sound (different values of qv0), which one is used here? and how exactly is the warm bubble defined? What boundary conditions (open/fixed/periodic) are used? Such information could be given in the appendix.

We have revised the manuscript by adding additional information regarding the two idealised setups to the description of the modelling setup, including more detailed information about the profile and the methods used for the initiation of convection and boundary conditions:

“We simulate two different idealised supercell cases. The first set of simulations (CASE1) is based on the default WRF quarter-circle shear supercell case (Khain and Lynn, 2009; Lebo and Seinfeld, 2011) representative of a supercell case over the Southern Great Plains of the United States. This case uses an initial sounding described in Weisman and Klemp (1982) with a surface temperature of 300 K and a surface vapour mixing ratio of 14 g kg^{-1} . The wind profile is taken from Weisman and Rotunno (2000) and features a wind shear of 40 m s^{-1} made up of a quarter-circle shear up to a height of 2 km and a linear shear further up to 7 km height. The initiation of convection is triggered by a warm bubble with a magnitude of 3 K in potential temperature centred at 1.5 km height in the centre of the domain with a radius of 10 km horizontally and 1.5 km vertically in which the perturbation decays with the square of the cosine towards the edge of the bubble (Morrison, 2012). This type of setup has been used for a number of similar studies in the past (Storer et al., 2010; Morrison and Milbrandt, 2010; Morrison, 2012; Kalina et al., 2014).
To test the representativeness of the results for different cases of idealised deep convection, a set of simulations for a second supercell case (CASE2) is based on an observed supercell storm over Oklahoma in 2008 (Kumjian et al., 2010). In contrast to the first case, the profiles in this case are from observation used in the model experiments in Dawson et al. (2013). This case features a significantly drier initial profile with a surface temperature of 308 K and a surface water vapour mixing ratio of 16 g kg^{-1} along with wind shear of similar magnitude to CASE1. The initiation of convection in this case is created by forced convergence near the surface based on nudging for the vertical velocity over the same volume that is used for the warm bubble in CASE1 according to the methodology described in Naylor and Gilmore (2012) with an updraft speed peaking at 5 m s^{-1} at the centre.
Both cases are simulated without a boundary layer scheme and without the calculation of surface fluxes or radiation. The horizontal grid spacing of the simulations is 1 km to sufficiently resolve the main features of the simulated supercell. We use a model domain size of 84 grid cells in each horizontal dimension and open boundary conditions on each side of the modelling domain. The vertical resolution of the 96 model layers varies from about 50 m at the surface to 300 m at the top of the model. Simulations are performed with a time-step of 5 seconds. The standard model diagnostics and the microphysical pathway diagnostics (Section 2.3) are output every 5 minutes to sufficiently resolve the development of the microphysical processes during the life cycle of the deep convective clouds. (Page 6, line 2)

***5.* What regions/clouds are the two different model setups representative for?**

Both cases are representative for the supercell storms over the Southern Great Plains of the US, we have added additional information on the cases to the description of the model setup. (See response to the previous comment)

6. Can you comment on whether the CDNC concentrations as listed in Table 1 are actually prescribed at all grid points where there is liquid water, or only at cloud base/when new droplets form?

The CDNC is prescribed everywhere in the column where there is liquid water, not only at cloud base. We have amended the text to state that more clearly.

“In each of the schemes, the CDNC is reset to the chosen value at the end of each model time step in all cloudy grid points.” (Page 5, line16)

7. Figure 2 and others: some of the pie charts are very small. Is the reader expected to read these?

We agree that some pie charts were too small in the initial version of the paper, thanks for pointing that out.

We have adapted most of the figures containing pie charts in terms of vertical size and the axis ranges to increase the size of the pie charts where possible. Along with the improved choice of colours (see comment 9) this strongly increases the readability of the pie charts in the revised manuscript. We have made sure we only draw conclusions from pie charts that are big enough to read them from a printed version of the paper or without zooming into a digital version of a manuscript.

It is unavoidable that some of the pie charts get small for some regions of the cloud when sticking to a representative linear relationship between coloured area and mass transfer or latent heating in the plots, as opposed to e.g. a logarithmic representation that we also tested.
Making the figures much larger would have made it difficult to place plots next to each other where different aspects of them can be compared directly, e.g. with regard to the vertical position of the microphysical processes for the different cases. However, as the size of the pie charts is representative of the total process rates, very small pie charts are indicative of regions less relevant in terms of the water turnover in the processes.

8. Figure 2: “contour lines for . . . ice (grey) content”: Is this just cloud ice or cloud ice + snow + graupel + hail?

This contour line includes the mixing ratio of all frozen hydrometeors, we have changed the notation to “frozen (grey) water content” (Page 10, caption Fig. 2) to make it clear what we mean here.

9. Figure 2(e): It looks like there is melting above the melting level?

These pie charts in the centre of the cloud actually show a combination of evaporation and sublimation, but we agree that the combination really looks like the orange we chose for the melting processes.

We have adapted the choice of colours for the melting, evaporation and sublimation processes to make them more distinct and more discernible, especially when they occur in combination.
Together with the increase of the size of the pie charts in the revised manuscript (see also response to comment 7), the respective figures are much easier to read now.

10. Why is there no plot as Fig. 2/3/4 (and more) for the SBM scheme?

The main focus of this paper is on the understanding of the evolution of the microphysical process rates in the two bulk microphysics schemes and the impact of changes in the aerosol proxy. The spectral bin microphysics scheme has been added to set these results into the context of a third microphysics scheme with a decidedly different approach to the representation of specific processes and properties.
We have only implemented the detailed microphysical process analysis for the two bulk microphysics schemes, where these processes are explicitly described as individual process rates in the model microphysics.
A similar comparison including the same visualisation of the detailed process rates in a bin scheme would be very interesting but is beyond the scope of this study and would require substantial additional work to add the respective output to the version of the bin microphysics scheme in WRF. A direct comparison of the process rates between the bulk schemes and the bin scheme would also involve the development of a consistent mapping of the bin-resolving process rates in the bin scheme to the bulk process rates in the bulk schemes – which is far from trivial.

We have phased our approach regarding the two bulk microphysics schemes and the bin microphysics scheme more clearly in the introduction and methodology description of the revised manuscript.

"We compare the results to simulations performed with a bin microphysics scheme (HUJI spectral bin scheme) for a subset of the analyses to investigate whether the effects investigated in more detail through the microphysical pathway analysis for the two bulk microphysics schemes agree with the response of a bin microphysics scheme to perturbations of aerosol proxies."
(Page 5, line 7)

"The detailed analysis of the process rates in this paper are carried out for simulations with these two bulk microphysics schemes. To investigate how the results obtained from the detailed analysis of the two bulk microphysics schemes hold for a bin cloud microphysics scheme, we also include additional simulations with the Hebrew University cloud model (HUCM) spectral-bin microphysics scheme (Khain et al., 2004; Lynn et al., 2005a, b), called SBM in the rest of the paper. We perform a subset of the analyses for this microphysical scheme, excluding the detailed microphysical process rate analysis but including the analysis of changes to the hydrometeor mixing ratios and the bulk cloud properties."
(Page 5, line 23)

11. page 11, line 31: Can you comment on which parameterizations are used for rain freezing vs. cloud drop freezing, and why one is more CCN-dependent than the other?

The freezing parametrisations are given in the appendix A2. However, both freezing parametrisations do not have any dependence on droplet/drop number concentration through the effective radius. Instead, the shift from rain freezing to droplets freezing is purely related to the change in the mixing ratio of the two liquid hydrometeors with a change in CDNC.

We have stated this aspect more clearly in the revised manuscript:

"For both bulk microphysics schemes, freezing of raindrops and cloud droplets occur in two separate layers, with freezing of raindrops at around 8 km and freezing of cloud droplets above a height of 10 km up to 14 km. In both microphysics schemes, freezing of raindrops is strongly decreased for increased CDNC (Fig. 8 b,d), while freezing of cloud droplets is increased by about a factor of three. This is not related to the parametrisation of the freezing processes (described in more detail in appendix A2), which does not include any information about cloud droplet effective radius and raindrop effective radius through the number concentrations. Instead, these changes are purely a result of the shift in the abundance of cloud droplets and raindrops (Fig. 5)."
(Page 14, line 6)

12. Figure 10: There is a substantial difference in evaporation between the two schemes. Why is this? Mixing assumption?

The difference in evaporation between the two bulk schemes can be separated into two different components. First, the evaporation of rain at the bottom of the cloud, which decreases more strongly in the Thompson scheme due to the stronger decrease in precipitation. Second, the changes to evaporation in the higher layers of cloud from the evaporation of cloud droplets. Due to the use of saturation adjustment, the evaporation is not directly controlled by the CDNC and effective radius of the cloud droplets. However, there are strong differences in the deposition rate on frozen hydrometeors, both between the two microphysics schemes and for different CDNC values, especially in the Thompson scheme. These changes in deposition could directly affect the evaporation by significantly changing the water subsaturation in the mixed-phase region of the cloud by further reducing the water vapour in the parts of the clouds that are subsaturated with regards to water but not to ice. This is a manifestation of the Wegener-Bergeron- Findeisen process transferring water from the liquid-phase hydrometeors to the ice-phase hydrometeors.

We have amended the text in the respective paragraph to discuss the differences and changes in evaporation more clearly and further elaborate on this relationship between the evaporation and deposition processes:

"The same limitation applies to the evaporation of cloud droplets, which also cannot show any direct effect from changes in CDNC due to the use of saturation adjustment. However, the evaporation shows much stronger differences between the two microphysics schemes and also a stronger effect of a variation in CDNC (Fig. 11 b,h). The strong changes in the evaporation at higher levels in the mixed-phase region of the cloud, especially for the Thompson scheme, can be explained with the changes in deposition on frozen hydrometeors (Fig. 11 e,k). The increased deposition with increasing CDNC through the changes to the frozen hydrometeors could lead to a further decrease of the saturation vapour pressure over water in the water-subsaturated regions of the cloud and thus additional evaporation. There is also a noticeable decrease in condensation in the higher layers of the mixed-phase region of the cloud at around 10 km for the Thompson scheme (Fig. 11 g), which could be similarly related to the increase in deposition. The evaporation in the lower layers is associated with the evaporation of raindrops. The differences between the two schemes and the variation with changes in CDNC can be directly related to the differences in the amount of rain, which is both higher and more strongly decreasing with increasing CDNC in the Thompson scheme than in the Morrison scheme."
(Page 16, line 29)

“There are large differences between the microphysics schemes in the latent heating and cooling from sublimation and deposition and its response to changes in CDNC. The Morrison scheme shows a significant decrease of both sublimation and deposition with increased CDNC (Fig. 11 e,f). Apart from changes due to the shift in hydrometeors from hail to snow and cloud ice (Fig. 5 and Fig.9), these decreases can be related to the lower amount of ice hydrometeors in the mixed phase region of the cloud. Although these two changes cancel each other to a large extent in the integrated latent heating, the two processes occur at different heights, which results in a shift of latent heating to lower levels, opposing the changes to the freezing and riming processes (Fig. 11 c). Furthermore, this strong decrease in sublimation leads to a decrease in water vapour near the cloud base, which could cause the consistent decrease in condensation at around 5 km altitude in the Morrison scheme (Fig. 11 a).
In the Thompson scheme, sublimation of ice hydrometeors is weak and barely affected by changes in CDNC (Fig. 11 l). However, increases in CDNC lead to an increase in deposition in the higher parts of the cloud (Fig. 11 k). This effect can be explained by the observed shift in hydrometeors from graupel to cloud ice and snow since deposition on graupel is turned off in the Thompson microphysics scheme, while it occurs on both snow and cloud ice. This increase in deposition could be the main reason for the changes observed in evaporation of cloud droplets as it significantly increases the sub-saturation over water in the mixed phase in regions that are supersaturated with respect to ice. This can be interpreted as a manifestation of the Wegener-Bergeron-Findeisen process (Wegener, 1911; Findeisen, 1938; Findeisen et al., 2015; Storelvmo and Tan, 2015), transferring water mass from liquid hydrometeors to the frozen hydrometeors. This constitutes an additional feedback from the changes in the ice phase back onto the liquid phase hydrometeors“ (Page 17, line 10)

13. Page 18: Why is the cloud dissipating with Thompson microphysics? This is a very substantial difference that should be discussed more.

Although we cannot rule out other dynamical explanations for this behaviour, the Thompson scheme shows much stronger cooling from the evaporation of raindrops and melting of frozen hydrometeors below cloud base, which could inhibit the later stages of the cell. This agrees with a short lifetime of the clean simulations for CASE1 with the Thompson scheme, that also show strong evaporation and melting at cloud base. We included this discussion in the revised manuscript:

“As a result, evaporation in the lowest model levels decreases strongly for the high CDNC value in the simulations with the Thompson scheme. Both microphysics schemes show a significant decrease in the total amount of melting of frozen hydrometeors below the melting line at about 4 km height. The strong cooling due to evaporation and melting in the cleanest cases for the simulations with the Thompson scheme (Fig. 6 c) can explain the significantly shorter lifetime of the cell compared to the more polluted cases and the other bulk scheme.” (Page 13, line 18)

“For the Thompson microphysics scheme, this second episode of development in the tracked cell is completely absent for all simulations, with the cloud dissipating after about 60 minutes of simulation time. This is potentially related to the substantially higher cooling at and below cloud base due to the evaporation of rain and the melting of frozen hydrometeors. The cooling can substantially weaken the convective updraft and thus prevent the further development of the cell that takes place in the simulations using the two other microphysics schemes. This finding agrees with a substantially shorter lifetime of

the cleanest case for the simulations with the Thompson scheme in CASE1 (Fig. 6)."
(Page 24, Line 1)

14. It remains a bit unclear to me what the conclusion from the second case is. Are the result regarding the invigoration hypothesis robust? Or is everything so different that not much can be concluded from two cases and one would actually need many more?

Although the two cases are quite different, e.g. regarding the point raised in the last comment, the response of the individual microphysical processes to changes in CDNC are very similar to the ones observed in the first case. However, previous studies (e.g. Khain, 2009) have shown the wide range of responses in deep convective clouds, especially for the simulation of supercell cases.

15. The conclusions could be more quantitative regarding the invigoration effect by giving number for the percentage change in latent heating.

We have calculated the relative change in total latent heating with increasing CDNC and it is negligibly small (a few percent) in all simulations, there is no trend with changes in CDNC that goes beyond the small random variation the between different simulations with each microphysics scheme. This holds for both microphysics scheme and the bin microphysics scheme. The changes to individual components such as deposition or sublimation are much stronger accumulating to relative changes of up to 30 percent, which however cancel out to give no significant response in the total latent heating. The latent heat release of freezing shows no significant changes of integrated heating with CDNC, just like the total latent heating. We have added the integrated latent heating rates to Fig. 10 (Fig. 9 in the old manuscript) and discussed them in more detail in the revised manuscript:

"The changes to the vertically integrated latent heating in the cloud for all three microphysics schemes do not show a significant trend with increasing CDNC (Fig. 10 d,e,f). The Thompson scheme shows lightly higher integrated latent heating for the two simulations with the highest CDNC content, but no consistent trend over the rest of the simulations (Fig. 10 e). The SBM simulations show a slightly decreasing trend of integrated latent heating for the highest CDNC values above 1000 cm^{-3} but no consistent trend over the entire range of values (Fig. 10 f) . Despite the significant change to the altitude of freezing there is no systematic change in the integrated latent heat release from freezing for both bulk microphysics schemes that would contribute to an invigoration of the cloud. In the Morrison scheme, the strong changes in deposition and sublimation almost entirely cancel out when integrated vertically. In the Thompson microphysics scheme, the increase in the integrated latent heat release from deposition cancels out the significant decrease in the integrated evaporation of cloud droplets and rain."
(Page 19, line 7)

<u>Technical comments:</u>

1. page 1, line 24 and many other occurrences: I think it is common to list multiple references for the same statement either in chronological or in reverse chronological order, not in arbitrary order as here.

We have revised the manuscript to order references in the same statement chronologically. Thanks for picking up this mistake.

2. page 5, line 6: scheme -> schemes

Corrected.

3. page 6, caption of Table 1: “10 g/, kg-1”: change “/,” to the latex command “\,”

Corrected.

4. page 14, line 8: “The differences are in part caused by . . .”: This seems to be a repetition, the same was already said in line 4.

We have completely rephrased the paragraph which removes the repetition (See response to comment 12).

5. page 23, line 15: full stop missing after “framework”.

Corrected.