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

Authors’ response to Anonymous Referee #1

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

In the following, we respond to 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.

This paper runs simulations of two different supercells using a suite of microphysics schemes and CCN/CDNC concentrations. They added outputs of microphysical process rates for two of the three microphysics schemes to investigate mechanisms of convective invigoration. Aerosol-induced convective invigoration is currently not well understood, and this paper contributes to the ongoing discussion in the literature on this topic. I recommend minor revisions.

<u>Major Comments:</u>

1. The microphysical process analysis (section 3.2) seems largely disconnected from the cloud mass and centre of gravity analysis (section 3.3). It would be nice if the microphysical process analysis could be used to help explain the results in section 3.3 more. Such a linkage also seems to be part of the goal of the paper which as stated by the authors is “to unravel the microphysical mechanisms responsible for aerosol effects on convection”.

We have significantly revised section 3.3 by using the findings from section 3.2 more directly in explaining some of the effects of the choice of CDNC value or microphysics scheme on the bulk cloud properties through the differences we found in the detailed process rate analysis. This provides a clearer link between the two types of analysis in the paper.

“The evolution of the cloud mass and the mass of the two water phases in the cloud (Fig. 12) in the three microphysics schemesis similar, with a maximum cloud mass of about $2 \cdot 10^{10}$ kg for all microphysics schemes before the splitting of the cell and then about $1.5 \cdot 10^{10}$ kg for the two bulk microphysics schemes (Fig. 12 a,b) and slightly higher cloud masses of up to $1.8 \cdot 10^{10}$ kg in the spectral bin microphysics scheme (Fig. 12 c). The cloud mass and also the difference between the bulk schemes and the bin scheme are dominated by the ice-phase hydrometeors, while the liquid-phase mass is very similar in all three different microphysics schemes, making up about 20-25% of the total cloud mass.
(Page 20, line 24)

There are, however, marked differences in the response to changes of the aerosol proxy between the microphysics schemes. The Morrison scheme shows a decrease of total cloud mass and ice-phase mass by about 10-15% over the range in which we increase the CDNC and no significant changes in the liquid phase. This decrease in ice-phase mass can be directly linked to the changes in the microphysical process rates analysed in Sec. 3.2. The shift of freezing to higher altitudes leads to a reduction in frozen hydrometeors in the mixed phase of the cloud and thus significantly less growth of the ice phase through vapour deposition. In the Thompson scheme, however, increased CDNC leads to an increase in ice-phase and total mass and a small increase in cloud liquid mass. This increase agrees well with the increased deposition due to the changes in the ice hydrometeor partition in the cloud discussed in Sec. 3.2. In the simulations using the SBM scheme, the two phases show a differing response to the aerosol proxy with increased liquid hydrometeor mass and a decrease in ice-phase mass for increasing CCN. 3.2.“ (Page 21, line 5)

“There is a consistent response in the cloud heights for all three microphysics schemes. The microphysics schemes show an increase in the height of the centre of gravity of the entire cloud, which is more pronounced using the Thompson scheme (about 1.5 km) than in the Morrison scheme(about 0.5-1 km). This includes an upward shift in both the liquid and frozen water in the cloud. The increased height of the liquid phase can be directly related to the decrease in the formation of warm rain (Fig. 6) and the more numerous cloud droplets reaching higher up in the cloud in the polluted case compared to the dominating raindrops in the cleanest case (Fig.5). The increase in the altitude of the ice phase in the cloud with increased CDNC can be related to the changes in the altitude of the freezing processes. However, it can also be a result of the lower fall speeds of the ice and snow hydrometeors dominating in the polluted case instead of graupel and hail in the cleanest cases.” (Page 22, line 2)

Minor Comments:

1. The authors may consider changing the title. After reading the paper I understand what is meant by the title, but I don’t know that I understood it beforehand. Just a suggestion.

Thanks for this comment, we have decided to adapt the title to state the purpose of paper more clearly:

“Aerosol effects on deep convection: The propagation of aerosol perturbations through convective cloud microphysics”

2. I think that the goal of the paper could be stated more clearly. It isn’t explicitly stated until the conclusions that the primary aerosol effect that the authors wish to investigate is convective invigoration.

We have adapted the respective part of the introduction to state more clearly that investigating the hypothesis of convective invigoration is one of the main focusses, but not the only focus of this study. The results show that the effects of aerosols are a complex superposition of different changes in the microphysics and the individual components of the latent heating. The integrated changes in freezing turn out to be much smaller than other changes to the latent heat release.

We have adapted the relevant sentence in the introduction of the paper to state the aim of the paper regarding the study of convective invigoration more clearly in the revised manuscript:

"It is, therefore, one of the main goals of this paper to investigate if and how these proposed mechanisms of convective invigoration, especially the proposed invigoration of convection due to additional latent heat release from freezing, manifest themselves in numerical simulations.”
(Page 3, line 1)

3. I don’t understand how fixing the CDNC helps to “isolate” the impact of microphysical pathways. Can the authors clarify what they mean?

We chose to use the fixed CDNC versions of the two microphysics schemes to exclude the extra step of cloud droplet activation in this analysis. Versions of the two microphysics schemes with activation based on prescribing the CCN exist, however, these are implemented in a different way in the two schemes, which would add additional differences between the microphysics schemes.

We have worded that more clearly in the respective paragraphs in the introduction and the methodology:

“To isolate the role of cloud microphysics for aerosol effects on deep convection from additional uncertainties in model-simulated aerosol fields, we apply a fixed cloud droplet number concentration (CDNC) in the two bulk microphysics schemes for each simulation. 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. We vary this CDNC value between different simulations as a proxy for aerosol number concentration. There are versions of both bulk microphysics schemes that include the activation of a fixed CCN spectrum or even interactive aerosols (Thompson and Eidhammer, 2014; Wang et al., 2013). However, the implementation of both the cloud droplet activation and the representation of the aerosol distributions is very different between the two microphysics schemes, which would add additional differences between the schemes compared to representing the perturbations in the form of a varying CDNC.” (Page 5, line 14)

4. The description of the cell tracking algorithm is brief. Can the authors comment on how they handle splitting and merging of convective cells? Splitting is of particular importance to this paper given that they are simulating supercells.

Cell splitting and merging is not explicitly treated in the tracking algorithm we are using here. The tracking algorithm picked up the initial updraft in the cell before splitting and then followed the right-moving cell after the split, while the updraft of the left moving got picked up as a new feature. For this study, this is not an issue as we entirely focus on the microphysical evolution of one of the cells. The way the cell splits seems to be very similar between the different simulations and not strongly affected by the choice of the microphysics scheme or the chosen value for the CDNC/CCN, so we did not analyse this further. The second cell moving to the left of the initial cell direction is not analysed in detail here as it shows very similar results in all aspects discussed. See also answer to comment 3 by Referee #3.

We have extended the description of the tracking algorithm and our choice of cell in the analysis in the revised manuscript to make this clearer to the reader:

“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)

5. I generally like the use of the pie charts on the cross-sections for quickly assessing the relative importance of various processes or hydrometeor amounts. That said, the authors spend a good deal of time discussing the specifics of these figures. I found myself spending a lot of time squinting at the panels, and they were difficult to use for more quantitative analysis. I’m not sure that there is a way to avoid these issues, so I just want to raise them as a comment.

We thank the reviewer for raising this important point that we have also thought about quite a lot when developing the analyses for this paper. We agree that there is a price to pay in the trade-off between a straightforward quantitative analysis and getting the full picture that the two-dimensional presentation with pie charts gives for assessing the structure and time evolution in a vertically resolved way. We have significantly increased the size of many of the pie charts in the revised manuscript by increasing the figure sizes or reducing the axis ranges, which makes the figures much easier to read.

6. Most of the processes in the figures are self-explanatory, but can the authors define "ice processes"?

The processes grouped as "Ice processes" combine all processes transferring mass between the different frozen hydrometeors (e.g. autoconversion of ice particles, collection of cloud ice by snow, etc., see also Table A1 and A2 in the appendix).

We have included a paragraph explaining the grouping of the individual processes depicted in these figures in the revised manuscript:

"For most analyses in this study, the individual microphysical processes are grouped into a consistent set of classes according to their contribution to the hydrometeor mass transfer in the model. This includes the six different phase transitions between frozen hydrometeors, water drops and water vapour (*condensation, evaporation, freezing including riming, melting, deposition* and *sublimation*) as well as the warm *rain formation* due to autoconversion and accretion of cloud droplets and all processes that transfer mass between the different frozen hydrometeors as *ice processes*. For some of the more detailed analyses, this grouping is performed in a more detailed way, e.g. separating freezing and riming processes or splitting them up by the specific hydrometeor class involved in the transfer. A collection of all the individual microphysical process rates represented in the two bulk microphysics schemes including the grouping discussed here is given in the appendix (Table A1 for the Morrison microphysics scheme and in Table A2 for the Thompson microphysics scheme)."
(Page 7, Line 10)

7. Page 9, Line 1: I struggle to identify two distinct regions.

We agree, that "distinct regions" is probably a bit overstated. Still, there is a significantly larger vertical range over which freezing and riming occur in the Morrison scheme, with maxima around these two heights (also visible in the time evolution for the clean case in Fig. 6). We rephrased the text in the revised manuscript:

"During the later stage, the freezing in the simulation using the Morrison microphysics scheme takes place over a substantial vertical range and is strongest at both edges of the mixed-phase region of the cloud at around 8 km and 10 km altitude (Fig. 2 c)." (Page 11, line 4)

8. Page 11, Line 2: By "cloud droplets" do the authors mean number or mass?

We mean cloud droplet mass, we have adapted that accordingly in the text. (Page .., line ..)

9. Page 11, Line 4: Can the authors comment specifically on how the definitions of hydrometeor classes differ and how these differences influence the results?

We have added additional information on the specification of the hydrometeor classes in the introduction and provided more details about it at the relevant parts in the discussion.

One important point is the difference in the parametrisation of individual microphysical processes, or even the existence of processes as in the case of deposition on graupel in the Thompson scheme. We have addressed the impact of that difference more detailed through the inclusion of an additional figure (Fig. 10) and extended discussions of the implications for specific microphysical processes.

"In the simulations with the Thompson microphysics scheme (Fig. 9 c,d), deposition and sublimation processes show very a different behaviour. The strong increase in snow in the cloud with increasing CDNC (Fig. 5 c,d) leads to a strong increase in both deposition and sublimation on

snow. Deposition on ice is on the same order of magnitude for the cleanest case, but not strongly affected by a change in CDNC. Sublimation of graupel only occurs around and below the melting layer and is significantly reduced by increasing CDNC. As deposition on graupel is prohibited in this microphysics scheme, there is no decrease in deposition on graupel associated with the changes in the hydrometeor ratio compensating the increase in deposition on snow. This leads to a strong increase in total deposition with increased CDNC as the main response in the Thompson scheme."
(Page 16 , line 13)

We have added additional details on the definition of the hydrometeor classes and important differences between the bulk schemes in the appendix describing the microphysics schemes in more detail:

"The two bulk microphysics schemes furthermore differ in important parameters regarding the different hydrometeor classes. The Morrison microphysics scheme is used in its configuration that treats the dense frozen hydrometeors as hail with a density of 900 kg m^{-3} , while the simulations with the Thompson microphysics used graupel with a density of 500 kg m^{-3} . The density of cloud ice, however, is higher in the simulations with the Thompson scheme 890 kg m^{-3} compared to the Morrison scheme (500 kg m^{-3}), while snow density is set to 100 kg m −3 for both schemes. The Thompson scheme has a more complex treatment of the snow hydrometeor class compared to the Morrison scheme, making use of a combination of two size distributions and thus allowing for a variation of the density over its evolution (Field et al., 2005; Thompson et al., 2008). The fall speed calculations are based on different equations in the two microphysics schemes, all parameters for the hydrometeor classes are left at their default values."
(Page 30, line 129)

We have added more details on the role of the representation of hydrometeors based on distinct classes and the resulting challenges in the introduction of the paper:

"The separation of the hydrometeors into individual hydrometeor classes in microphysics schemes brings with it specific challenges in resolving the microphysical processes. In bulk schemes, liquid water in the cloud is separated into cloud droplets and raindrops. The collision-coalescence processes leading to the formation of rain from cloud droplets have to be parametrised through the artificial process of droplet autoconversion and a simplified treatment of accretion of droplets by raindrops. The semi-empirical nature of these parametrisations has been shown to be the source of major uncertainty in the assessment of aerosol-cloud interactions in numerical model simulations (Khain et al., 2015; White et al., 2017). In the ice phase, most current microphysics schemes separate the hydrometeors into a number of different classes such as pristine ice, snow, hail or graupel. The equations and parameters for the calculation of the microphysical process rates as well as important physical properties of the hydrometeors, such as shape, density or the specific form of the size distribution are specified for each individual hydrometeor class. These choices additionally impact important physical processes such as the fall speeds of hydrometeors in the calculation of sedimentation or the radiative properties of the hydrometeors. This can lead to abrupt changes to the evolution of the cloud due to a change in the partition between the hydrometeor classes in the ice phase of the cloud (Morrison and Milbrandt, 2014)" (Page 3, line 14)

10. Page 11, Line 7: I assume that the authors track the right-mover of the supercell, but this is not stated explicitly.

We track both cells, but we have only analysed the right-moving cell including the initial stage. We have added a clearer description of the tracking and analysis in the revised manuscript (see also comment 4 for more details).

We have amended the text here and at some other points to state this more clearly:

“As for all the following figures for CASE1, these analyses are based on a combination of the initial stage of the cell and the right-moving cell after the cell split.” (Page 9, line 27)

11. Page 11, Lines 12-18: Try as I might, I can’t see deposition anywhere on Figure 4 (or Fig. 2) so it is difficult to assess the accuracy of these statements.

The enlarged figures and choice of colours makes it easier to distinguish the individual process rates. The deposition processes should now be clearly visible, especially in the panels showing the latent heat release from the processes (e.g. Fig 2 f on page 10).

12. So Figure 9 shows the results from all tracked cells? Why the switch now from looking at just one cell to all the cells?

Throughout the entire paper, we only analyse one of the two tracked cells (see response to comments 4 and 10). We acknowledge that the use of the plural “cells” for the cell in the different cases/microphysics schemes might be misleading, so we have adapted this in the revised manuscript and clarified the respective figure captions (Fig. 12, Fig. 15):

“Total water mass, liquid water mass and frozen water mass in the analysed right-moving cell for the three different microphysics schemes (Morrison: left, Thompson: middle, SBM: right) in CASE1. The jump in the curves occurs at the point where the cell splits into two individual cells”
(Page 22, caption Fig.12)

13. Page 22, Line 15: It was very difficult to tell from the analysis as presented whether there is a near complete transfer of (liquid) condensate mass into the ice phase or not.

This statement was based on the fact that the cloud hydrometeor mass is predominantly made up of ice-phase hydrometeors (Fig.11 and Fig. 14). However, the significant changes in the formation of rain from cloud droplets observed in all microphysics schemes show that there is a significant contribution of warm rain processes to precipitation. Reducing the precipitation indeed gives a significant potential for the invigoration pathway to occur (through additional freezing), whatever the partition between liquid and frozen water in the cloud. We have thus removed this statement from the revised manuscript.

14. Many studies have been performed that investigated the impact of aerosols on deep convection, including some that have shown microphysical process rates. I think that generally the authors could do a better job of discussing how their results agree or disagree with these previous studies.

We have added additional discussion of the results in light of previous studies of aerosol effects on supercells and other isolated deep convective clouds in the conclusions section of the paper, e.g. in the following sections:

“This response is consistent between the different microphysics schemes and confirms earlier studies that stated the importance of changes in the partition between rain and cloud droplets in determining the evolution of freezing and riming (Seifert and Beheng, 2006).”
(Page 27, line 25)

“This confirms results from previous studies on the effects of aerosols on supercells (Khain et al., 2008; Morrison, 2012; Kalina et al., 2014) and other deep convective clouds (Ekman et al., 2011) that pointed out a range of compensating processes limiting convective invigoration and a strong dependency on the environmental conditions in which the cloud develops.”
(Page 28, line 1)