Review of the manuscript ‘Aerosol-cloud interactions in mixed-phase convective clouds. Part 1: Aerosol perturbations’ by Miltenberger et al.

The authors use a newly-developed interactive cloud-aerosol microphysics scheme implemented in the Unified Model to perform high-resolution simulations of mixed-phase convective clouds which develop along a sea breeze convergence line. The simulations are evaluated against observations taken during the COPE field campaign. The simulations compare favourably to the observed thermodynamic and cloud properties. The impact of aerosol perturbations on simulated cloud properties, reflectivity and precipitation are investigated using the new microphysics scheme, as is the impact of passive aerosol vs allowing cloud-processing of aerosol. Including cloud processing of aerosol generally leads to increased model performance in terms of cloud base cloud droplet number concentration (CDNC) and reflectivity values but leads to an underestimation of domain-averaged surface precipitation rates and smaller convective cell sizes and numbers, although total cloud fraction is relatively unaffected. Although cloud processing of aerosol leads to changes in the cloud geometry, it does not affect the mechanisms of the cloud response to aerosol perturbations. Under an enhanced aerosol scenario, precipitation is initially suppressed but becomes enhanced once convection becomes organised. This is mostly due to modifications to condensate generation in the liquid-phase part of the cloud, and due to changes in precipitation efficiency. The authors report increased vertical velocities in convective cores and increased condensate production under enhanced aerosol environments (“convective invigoration”), but precipitation enhancement resulting from convective invigoration is limited by thermodynamic constraints on the cloud depth.

The manuscript is generally very well-written and uses a novel analysis technique of decomposing the precipitation response to changes in condensate generation and loss in order to identify and quantify the impact of aerosol perturbations. I find the results interesting and the study makes a good contribution to the body of literature on aerosol-cloud interactions.

However, the manuscript contains some confusing statements in places. There are also several claims made about aerosol-cloud microphysical processes occurring in the model and between the model configurations without figures or discussion of results provided to support these claims. Further, I found that the structure introducing the results of the perturbation experiments does not flow particularly well, and the first discussion of the perturbation results is somewhat obscured.

I have some concerns about the impact of a modelling framework that steps down directly from global model to a 1km grid, and would like to see that this doesn’t have an impact on the high-resolution nests. Further, I would like to know whether the 250 m grid length used in this study is appropriate for the particular clouds investigated, as it has been shown that increasing spatial resolution does not necessarily lead to better representation of simulated storm morphology (e.g. Stein et al. 2014, 2015).

I have provided a list of general comments below. I recommend publication in ACP subject to major revisions.

General comments:

Introduction:

1. The paper focuses on simulations performed with a new bulk microphysics scheme with explicit aerosol processing. The literature cited in the introduction discusses simulated cloud response to aerosol but the studies cited include both bin and bulk schemes. It would therefore be good to note in the introduction which of these studies cited use bin schemes and which use bulk schemes.

Data and Methods:

2. The modelling framework used by the authors steps down from global (N512 resolution) to a 1 km nest, without stepping down through coarser outer nests. Can the authors show that this doesn't lead to any spurious artefacts either in the 1 km domain or in the boundary conditions for the 250 m generated from the 1 km nest? Especially with the 1 km nest containing land boundaries on its NW and SE sides, I have some concerns that stepping down from global to relatively fine resolution could have an undesired impact on the high-resolution domains.

3. The use of a 250 m grid length for the analysis domain: It has been shown that increasing spatial resolution does not necessarily lead to better representation of simulated storm morphology, particularly with respect to the width and intensity of simulated storm structures compared to those observed (e.g. Stein et al. 2014, 2015). Can the authors show that the choice of a 250 m grid length in the simulations presented in the paper is appropriate (compared to observations), compared to other grid lengths? Do the authors know whether the simulated storm structures in the current study have converged at the 250 m grid length used?

Results:

4. Model spinup and early isolated cells: Is it possible that the delayed precipitation development in the simulation compared to the observations, especially the generation of isolated cells early by the model which remain small and do not produce surface precipitation, is because the model is not fully spun up at this time? If not, is there another explanation for the lack of precipitation from these isolated cells compared to observations, given the relatively good agreement between the precipitation from the organised convection in the model and observations at later times?

5. Full distributions are presented and statements such as 'the underestimation of domain average precipitation is related to a smaller extent of weakly precipitating areas' are inferred (e.g. P9 L1: "is related to a smaller extent of weakly precipitating areas"). However, it is not possible to make conclusions on area / extent from the distributions alone as the full distributions contain both spatial and temporal components. That is, from the precipitation rate distribution alone it is not possible to distinguish whether the model underestimates precip rates compared to the radar observations (a) because there are fewer occurrences of

cloud in the model compared to the observed cloud, but which have the same precip rates as the observed cloud, or (b) whether there is the same amount of cloud in the model as that observed but with weaker precip rates compared to observations, or (c) a combination of less cloud with weaker precip rates. Are you able to show surface precip rates averaged below-cloud only, or similar figure comparisons, to distinguish between these potential cases? Otherwise, it may be more appropriate to phrase such statements in terms of e.g. “a reduced frequency of weakly precipitating points”.

6. When the authors compare radar-derived and simulated rain rates, the claim is made several times in the manuscript that because the overall agreement between observed and modelled radar reflectivity distributions is better than that seen between the radar derived rain rate and modelled rain rate, this suggests potential problems with the radar derived surface precipitation rate for medium to low precipitation rates.

Whilst I agree that this is possible, could this not also be due to differences in the way that dBZ is calculated from the radar and from the model? i.e. could it not be that the radar-derived rain rates are correct (even if the model doesn’t agree with them) and the simulated reflectivity values are wrong (even if they agree with the radar, i.e. the model appears to agree but for the wrong reasons)?

7. The authors make many statements on the processes responsible for certain behaviors. Examples are “these changes are due to the depletion of interstitial aerosols inside the cloud in the runs with aerosol processing, which impedes secondary activation in the model” (P11 L31 - 32) ; “While the Aitken mode is depleted downstream of convective cells, the accumulation mode increases due to evaporative release of aerosol. The collision-coalescence processes in the cloud lead to a transfer of aerosol from the Aitken to the accumulation mode. The coarse mode aerosol is increasing in cloudy areas mainly due to sub-cloud evaporation of rain and downstream of convective cells.” (P11 L4-6), and similar instances occur throughout the text. However, no further information is given to justify these statements.

Are the authors able to provide comparisons of process rates or of e.g. interstitial aerosol amounts or of sub-cloud evaporation (in the first and second examples given above, respectively) to back up each of these statements? Even one table showing the difference in the aerosol process rates or amounts of (interstitial aerosol, secondary activation, etc) for the passive vs processed aerosol runs would make this immediately clear to the reader.

Further, the authors refer to depletion, enhancement and relative increases, but only show figures of fields from the simulation with aerosol processing. Are the authors referring to a description of the fields in this run only (e.g. depletion of aerosol in-cloud vs outside of cloud in the processed aerosol case), or a comparison with the passive aerosol run (e.g. in-cloud depletion of aerosol in the processed case vs the passive case)?

8. In general the discussion of the perturbed aerosol conditions does not flow particularly well. It is first introduced, with no figure reference, in section 4.1 which discusses aerosol processing vs passive aerosol treatment, and thus I found the perturbed aerosol discussion

somewhat obscured. Would it be possible to have a separate subsection discussing the impact of perturbed aerosol conditions, to make it clearer to the reader?

9. It would be useful to see SI Fig 7 c - f also shown as difference plots against the passive aerosol run to show the impact of cloud aerosol processing on the cloud and aerosol fields.

Specific comments:
P2 L1 - ‘and modifications’ should be ‘modifications’
P2 L21-22 - it would be good to give some examples of studies which disagree on magnitude and/or sign.
P2 L29 - ‘dominated with increasing environmental relative humidity’ doesn’t make sense.
P3 L10 - ‘This conceptual idea has been developed using simulations of individual clouds under idealised conditions’ - can you cite examples here?
P3 L13-15 ‘Also, Johnson et al. (2015) demonstrated that the precipitation signal is dependent on the values of uncertain parameters within the cloud microphysics parameterisation (parameter uncertainty).’ - are these uncertain parameters inherently uncertain (variability in the parameters themselves), or uncertain because their values are unknown?
P4 L9 - ‘provided’ -> ‘provides’
P4 L26 - ‘Part 2’ - presumably part 2 is a second paper to follow the present paper?
P5 L7 - ‘Fig 1a’ - I think the authors refer to Fig 1b.
P5 L22 - ‘Fig 1b’ - I think the authors refer to Fig. 1a.
P5 L23 - ‘mean boundary layer top’? ‘Top of the model’ sounds like the authors refer to the model top.
P5 L27 - ‘the residence time of air in the model domain is only several hours’ - is this shown anywhere?
P5 L32 - ‘aerosols’ -> ‘aerosol’
P6 L17-18 - ‘The simulated precipitation rate and reflectivity distributions are particularly sensitive to the assumed graupel density and diameter-fall speed relation’ - is this shown anywhere?
P6 L27 - ‘in-soluble’ -> ‘insoluble’
P7 L20 - ‘in the sections’ -> ‘in sections’
P8 L14 - SI Fig.s 1 and 2: it would perhaps be useful to the reader to have these in the main body of the paper rather than the supplementary information, for the reader to get a qualitative feel for the model behaviour in the first figure presented.
P8 L17-18 - ‘While the majority of clouds develop along the convergence lines’: for the simulation data it would be useful to have the convergence lines plotted on the Figures along with the reflectivity and coastline.
P8 L18-19 - ‘A double-line feature also appears in model simulations’: I find it hard to agree with this in some of the figures, especially the no aerosol processing figures.
P8 L19 - ‘satellite data’ - there has been no satellite data presented or discussed in the paper thus far.
P8 L20 - ‘north-westerly’ - do the authors mean north-easterly? (or SW to NE?)
P8 L23-24 - see general comment about spin-up
P8 L27-28 - ‘The smaller domain average precipitation in the model is mainly due to the overall smaller cell sizes’ - what happens if you compare precipitation from cells only (i.e. not

domain average), or domain average weighted by cell fraction. Do you get better agreement between the model and observations?

P8 L30-31 - 'The cessation of precipitation is linked to the dissolution of the convergence lines': Again, it would be useful to have the convergence lines plotted on Fig.s SI 1 and 2 to show this.

P9 L9 - 'The area covered' - see general comment 5

P9 L10 - 'The area covered by clouds with low reflectivity (< 10 dBZ) is underestimated in both simulations' - doesn't the processed aerosol case overestimate the occurrence of low reflectivity cloud?

P9 L16 - '...where the model underrepresents the medium surface precipitation rates' - refer to Fig. 3a here

P9 L16 - 18 - See general comment 6

P9 L22-24 - 'The maximum cloud depth in the observations only shows a small increase from about 5 km to about 5.5 − 6 km, while maximum cloud top height in the model increases from 3.5 km to 5 − 5.5 km' - see general point 4: could the delayed / weaker cloud development in the model be due to spinup?

P9 L28 - 'The larger maximum cloud top heights in the radar observations are mainly due to higher level ice clouds likely forming outside the model domain' - then why not restrict the analysis of the observations to the same region as the model domain analysed?

P10 L11 - Fig SI 5a - What is the high frequency occurrence of larger CDNC values high above the cloud base (5 to 7 km) which is seen in the passive aerosol case but not the processed case?

P10 L30 - 'will be robust' - robust compared to what? There are no observations to support the aerosol perturbation experiments, so any conclusions made about the processes that occur in the perturbation experiments can only be made in the context of the model with respect to itself.

P11 L4 - 'Aitken and accumulation mode aerosol concentrations are reduced inside the clouds due to CCN 5 activation.' - I can't see this easily from Fig 4. The accumulation mode seems to increase, not decrease.

P11 L6-8 - see general comment 7

P11 L6 - 'in the cloud' -> 'in the clouds'

P11 L16 - 'The Aitken mode is depleted within cloud' - is it actually depleted compared to the no processing case (which I imagine it is) or is there just a smaller aerosol number compared to other regions? Can you give a figure for each case (processed vs passive aerosol) or a difference figure to show the depletion?

P11 L22 - 'but the relative increases are more wide-spread and have a larger amplitude' - are these relative increases compared to another run? How are the relative increases shown?

P11 L24 - 29: Comparison of aerosol processing and aerosol concentrations. It is not clear which figure shows the passive vs processed aerosol statement in the first sentence. Also the rest of this paragraph discusses perturbations in aerosol concentration, but this has not yet been discussed and it is not clear which figure shows the aerosol perturbation results for the passive vs processed case discussed in this paragraph. (See general comment 8)

P11 L31-32 - 'These changes are due to the depletion of interstitial aerosols inside the cloud in the runs with processing, which impedes secondary activation in the model' - how do we know this? (See general comment 7).

P12 L3 - 'The impact of aerosol processing on aerosol fields and hydrometeor number concentrations is qualitatively very similar for simulations with perturbed aerosol initial conditions, except for the low aerosol simulations (not shown)' - the results of the aerosol perturbations haven't really been introduced (see general comment 8).
P12 L11 - how is a cell or cell size defined?
P12 L13-14 - 'Aerosol induced changes in cell number and size are smaller for aerosol concentrations enhanced above the standard aerosol profile compared to reduced aerosol concentrations' - does this indicate the transition from a CCN-limited to a dynamically-limited (updraft-limited) situation?
P12 L24-25 - 'For enhanced aerosol concentrations changes in cloud top height are larger in the aerosol processing than the passive aerosol simulation.' - this is interesting! Are you able to explain why?
P12 L26 - reference to SI Fig 8 - I think the authors mean to refer to Si Fig 6 here.
P13 L31 - 'seized' -> 'sized'
P14 L6 - 'is increasing' -> 'increasing'
P14 L14-15 - 'We hypothesise that the signal is consistent with parcel models in the highest percentile, because these correspond to updraft regions in clouds at the mature lifecycle stage, for which condensate production is compensating for losses due to precipitation production.' - could you help verify this by also showing condensed water path as a function of precip rate from the mature updrafts only?
P15 L10 - 'correspond to simulations, for which' -> 'correspond to simulations for which'
P16 L5 - 'importance For' -> 'importance. For'
P17 L1-2 - 'the vertical velocity is almost unaltered as is the cloud base temperature' - which figure shows this?
P17 L7-8 - 'The higher condensate amounts towards cloud top are also supported by slower conversion rates of cloud condensate into rain' - this is only true for the high vs standard aerosol case?
P17 L15-19 - How is this shown? (See general comment 7)
P18 L33-P19 L1 - see general comment 6
P19 L10 - remove commas from sentence in bullet point 2
P19 L12-13 - remove commas from first sentence in bullet point 3
P19 L16 - 'feedback mechanism, which' -> 'feedback mechanism which'
P19 L19-21 - this sentence is quite complicated and has too many commas. Can you simplify? Also 'two.way' -> 'two-way'
P20 L15 - 'hypothesis' -> 'hypothesise'
P21 Eqn A2 - per, ctr are not defined (I assume they mean 'perturbation' and 'control'?)
P22 L2 - remove commas in this sentence

Figures:

Fig 1:

- The image quality in the inset panel in 1a is not good, I find it hard to see in my printed copy.
- 'Aitken model' -> 'Aitken mode'
- 'bar showing' -> 'bar shows'

Fig 2:

- Fig 2a caption needs to state this is a timeseries

Fig 3:
- Panels (a) and (b0 are not labelled (I assume (a) is passive and (b) is processed aerosol case)

Fig 12:
- Can you label the columns ‘uncapped’ / ‘capped’ and the rows ‘low / high’?
- Caption: ‘scenario, in’ -> ‘scenario in’
- Caption: ‘one, in’ -> ‘one in’
- Caption: ‘during of the cloud’ -> ‘during the cloud’

Fig A1:
- ‘Appendix B’ -> ‘Appendix A’ ?

SI Fig 1, 2, 8:
- It is hard to see the land outline in my printed copy
- I would like to see convergence (even just a single contour) plotted on the model figures so that the reader can identify lines of convergence relative to the reflectivity.

SI Fig 3;
- The black lines (solid / dash for passive / processed aerosol) in the legend are confusing. I was expecting to see a black dashed and black solid line in the distribution in Fig 3a. I would remove these lines from the legend and just put the description in the figure caption.
- What is ‘aerosol processing new’ (labelled in the legend)?

SI Fig 4:
- What is the difference between sb and ml in Fig 5d?

SI Fig 7:
- ‘redish’ -> ‘reddish’

SI Fig 8:
- I would find it easier to compare the panels in this figure if each panel had an extra caption describing the aerosol processing and concentration, e.g.: passive low, passive high, processed low, processed high

SI Fig 11:
- Caption: ‘(c) all hydrometeor’ -> ‘(a) all hydrometeor’

SI Fig 12:
- ‘Change mean’ -> ‘Change in mean’

SI Fig 15:
- (b), (d) - what happens in the high aerosol case between 6 and 7 km? There is a broken line.

- This figure isn’t referred to in the manuscript

SI Fig 16:
- Caption: typo in ‘hydrometeors’
- Caption: ‘indicate es’ -> ‘indicates’

SI Fig.s 17, 18:
- These figures are not referred to in the manuscript