Replies to review RC1
'Aerosol-cloud interactions in mixed-phase convective clouds. Part 1: Aerosol perturbations' by Miltenberger et al.

General comments
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.
reply: We have changed the introduction accordingly and explicitly stated for each citation, whether a bulk or a bin scheme was used.

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.
reply: We do not see any artificial features such as gravity waves originating from flow adjustments from the coarser resolution in the 1km domain. The current operational set-up of the Unified Model at the UK Met Office does use no intermediate nests for downscaling from global to kilometre-scale models (Clark et al., 2016).
Also previous published studies with the UM have stepped down from global to kilometre-scale resolution as well and confirm that the UM is able to handle this transition (e.g., Field et al., 2017; Grosvenor et al. 2017) and did provide reasonable results. The study by Field et al. (2017) shows that the mesoscale features do not change strongly for simulations with grid spacings of 16 km to 1 km (all nested directly in the global UM model). Furthermore the modelled cloud field structure are comparable in the Field et al. (2014) (using intermediated nests) and the Field et al. (2017) (no intermediate nests).

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?
reply: Simulations with 1 km and 500 m horizontal grid spacing have also been tested, but they compare less well to observations then the presented simulations with 250 m grid spacing. The comparison against observed surface rain rate and radar reflectivity is shown in Fig. 1. Fig. 2 shows map plots of column maximum reflectivity for simulations with different grid spacings and the observations at 14 UTC.
We have not tested simulations with even higher horizontal resolution as it becomes very expensive to cover the necessary area for the formation of the sea-breeze front at, e.g., 125 m grid-spacing. We therefore do not know whether the model convergences. Given the satisfactory performance of the simulations compared to observational data and the general elusiveness of convergence (Stein et al. 2014, Stein et al. 2015, Hanley et al. 2015), we think the 250 m grid spacing is an appropriate choice balancing the size of the domain with a satisfactory representation of the convective clouds.
change to paper: We have included the comparison of observations to the lower resolution simulations in the Supplementary information of the paper and added some text in section 2.1 (p. 5, l. 13/14).

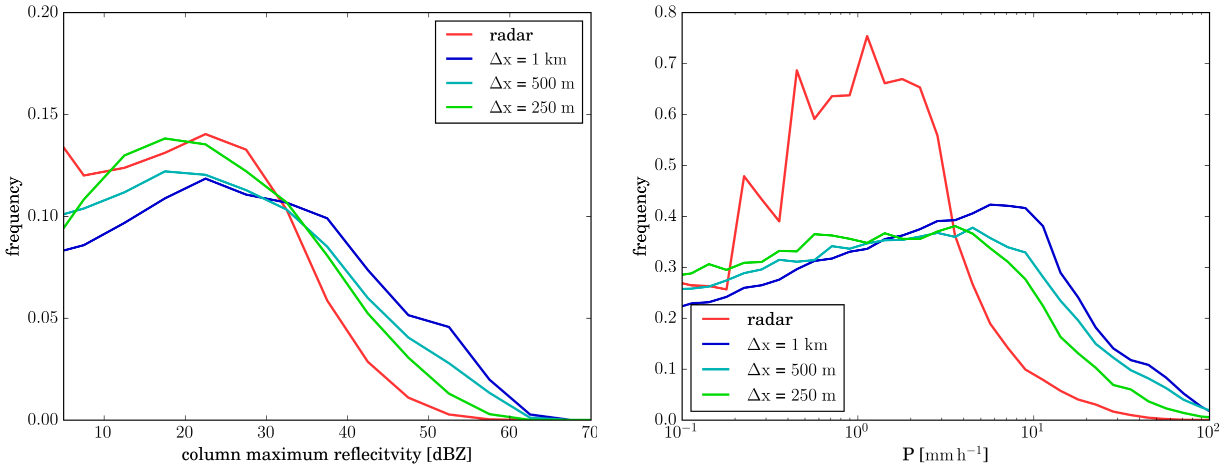


Fig. 1. *Distribution of column maximum reflectivity (left) and surface precipitation rate (right) from observational data (red line) and simulations with different grid spacings (cold colours).*

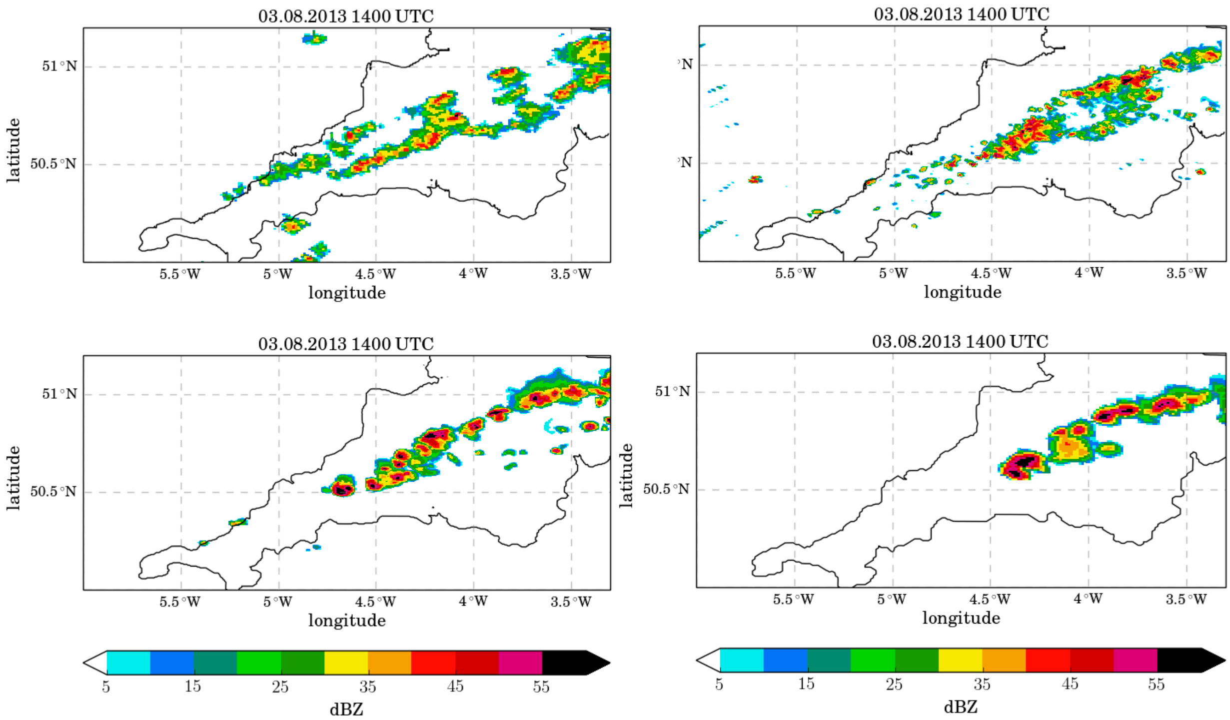


Fig. 2. *Column maximum radar reflectivity at 14 UTC from observations (top left) and model simulations with grid spacings of 250 m (top right), 500 m (bottom left) and 1 km (bottom right).*

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?
reply: It may be a possibility some of the differences is due to spin-up. Although the model is already running for 9 hours at the time we start comparing the simulation to the observational data. This should be enough spin-up time for an NWP model. We think the model fails to organise the small convective cells forming in the early morning hours to larger clouds due to the absence of the forcing from the sea-breeze convergence. Larger cells form in the observations, which propagate for longer distances. It is known that NWP models have problems to generate larger cells in weak forcing situations (e.g., Stein et al., 2014; Hanley et al., 2015).

Fig. 2 shows the column maximum reflectivity at 10 UTC (top row) and precipitation Hovmöller plots (bottom row). The Hovmöller plots suggest that there are cells initiated in the model also before the convective line forms, but they do not grow to the same sizes as in the observations. *Change to paper: We have added a sentence pertaining to the potential reason for the underestimation of the precipitation early in the simulation in sec. 3.1 (p. 9, l. 1-6).*

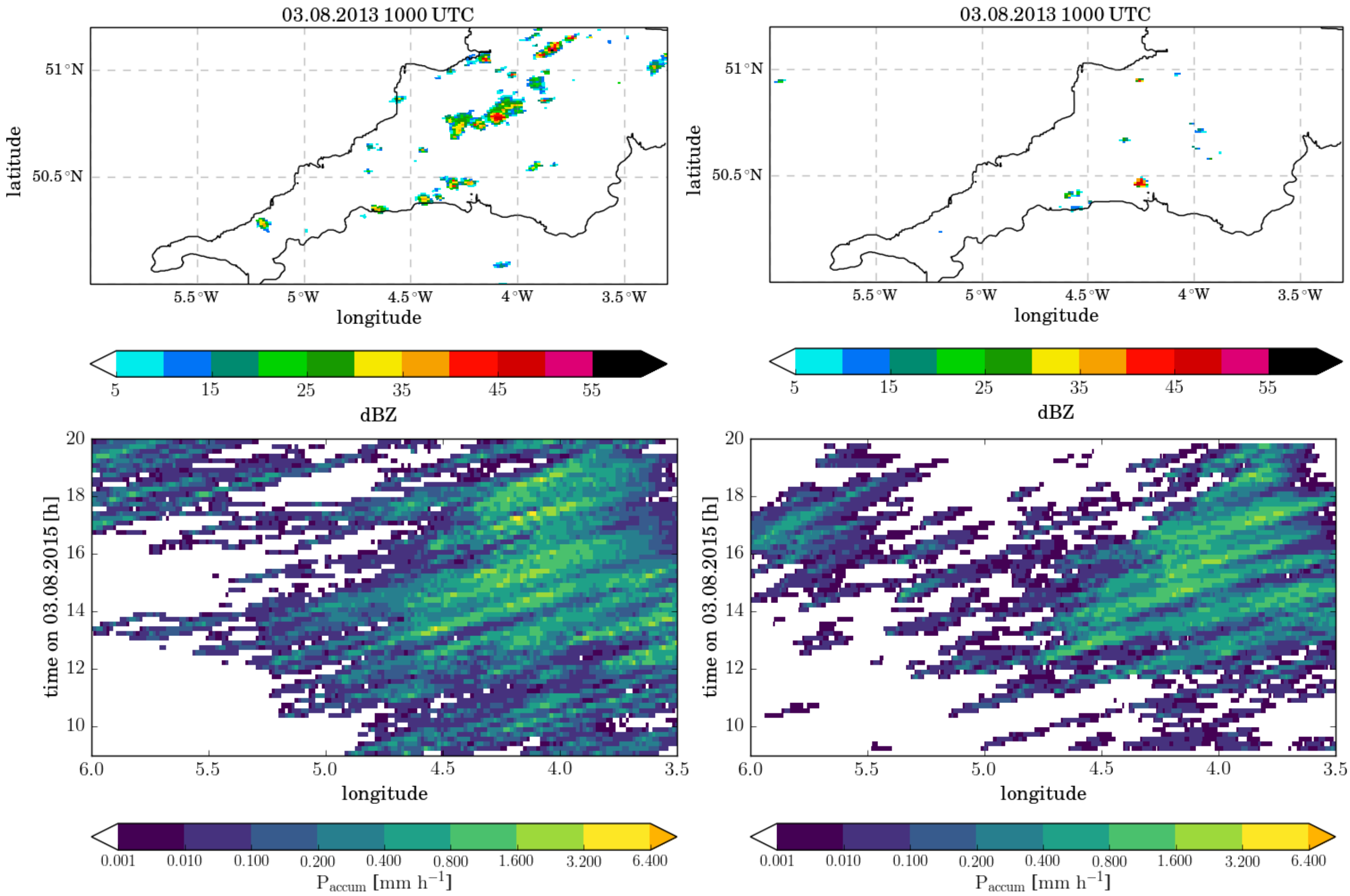


***Fig. 3.** top: column maximum radar reflectivity from observations (left) and model simulations (right) at 10 UTC. bottom: Hovmöller plot of accumulated precipitation from radar observations (left) and the model simulation (right).*

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”.

reply: Thank you for raising this important issues.The distributions shown in the paper are including points with precipitation only. Distributions including all data points are shown in Fig. 4 a. Qualitatively the same behaviour occurs if distributions are normalised with all points in the domain and time or only those where precipitation occurs. This is also the case if distributions for individual times are considered (Fig 4 b). Therefore the original conclusion holds that there are (a) fewer instances in space of weakly precipitating points in cloud and (b) fewer precipitating points over all.

change to paper: We explicitly state in figure captions and in the text that distributions over cloudy points only are shown. We also comment on the consistency in behaviour in time and a larger underestimation of weak precipitation rates, if all points are considered (p. 9, l. 13-16 & l. 23-28).

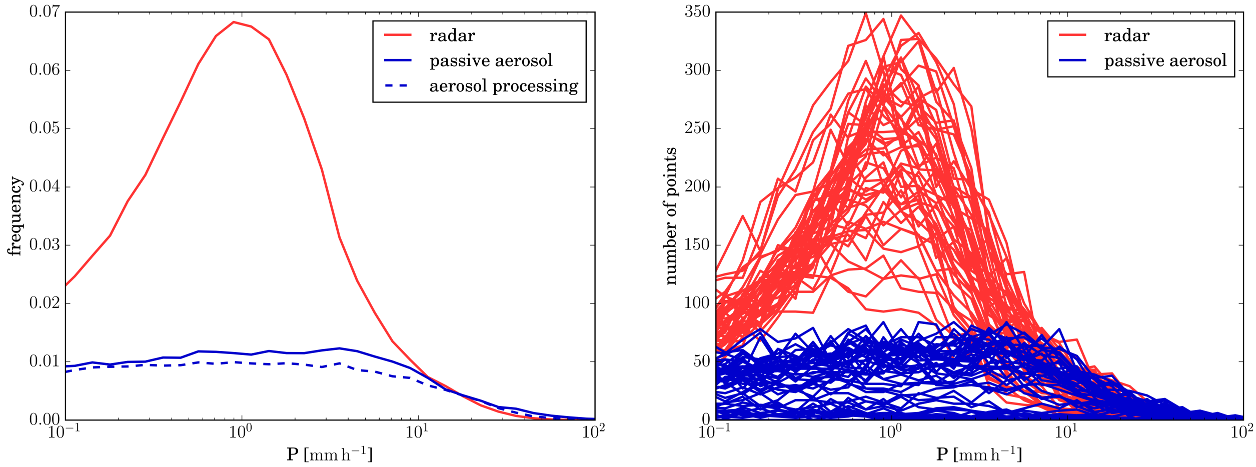


Fig. 4. *(a) Distribution of precipitation rate including also none precipitating grid boxes. The observations and the model simulation contains the same number of points, so this is equivalent to scaling with the number of grid points. (b) Distribution of number of grid points in each precipitation bin for each 10 min interval between 10 UTC and 18 UTC separately (individual lines).*

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)?
reply: It is certainly possible that there are problems with the modelled radar reflectivity.
change to paper: We added some discussion reflecting this aspect in section 3.1 (p. 10, l. 4-6).

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)?

reply: We take this comment to refer to section 4.1, i.e., the discussion of differences between the simulations with passive aerosol and aerosol processing. In the passive aerosol simulations the aerosol fields are essentially identical to the upstream boundary conditions, i.e., any changes in the aerosol field relative to the upstream values (left hand side of the plots) in the Hovmöller plots (Fig. 4 in the paper) or the cross-sections (SI Fig. 7) can be interpreted as a result of the aerosol processing. We provide the respective plots for the passive aerosol runs to confirm this in Fig. 4 of the paper and SI Fig. 10, 11. Because of this passive nature of the aerosol field, the processes named as a cause for certain features of the field (e.g., less secondary activation, reduced interstitial aerosol, enhanced coarse mode) are the only processes in the model that act on the aerosol fields with the right sign in the considered region (in-cloud, below cloud, outflow). We therefore think it is not necessary to add another table or figure to the paper given that there are already 10+ figures.
Change to paper: We also reformulated section 4.1 to better reflect the reasoning behind the statements (clarifying there is no change to the aerosol fields in the simulations with passive aerosol and processes added for the aerosol processing run).

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?
Change to paper: We changed the sections such that the discussion of aerosol processing and description of aerosol induced changes in the cloud field are now in a separate sections (new section 4 and 5, respectively).

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.
see reply to point 7 of the review

Specific comments
Reply: Thank you for these comments and for spotting the errors in the formulation!
P2 L1 - ‘and modifications’ should be ‚modifications’
Change to paper: as suggested

P2 L21-22 - it would be good to give some examples of studies which disagree on magnitude and/or sign.
Change to paper: We have added references to the Tao et al. JGR 2007 study, which shows different responses in surface precipitation for the different clouds with the same modelling framework. We also reference the Khain et al. 2009 paper, which summarises different studies with varied precipitation responses.

P2 L29 - ‘dominated with increasing environmental relative humidity’ doesn’t make sense.
Change to paper: reformulated

P3 L10 - ‘This conceptual idea has been developed using simulations of individual clouds under idealised conditions’ - can you cite examples here?
Change to paper: We included references to the following studies: Khain et al. JAS 2004, Khain et al. JAS 2005, and Rosenfeld et al. 2008.

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?
Reply: In the Johnson et al. (2015) parameters that are inherently uncertain, e.g., the graupel density (if not parameterised), and unknown parameters, e.g., immersion freezing coefficient, have

been perturbed. Several parameters, as for example the immersion freezing coefficients, are inherently uncertain and unknown.
Change to paper: We changed the text to clarify this (p. 3, l. 19-20).

P4 L9 - 'provided' -> 'provides'
Change to paper: as suggested

P4 L26 - 'Part 2' - presumably part 2 is a second paper to follow the present paper?
reply: Yes.

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.
Change to paper: as suggested

P5 L27 - 'the residence time of air in the model domain is only several hours' - is this shown anywhere?
Change to paper: This was not shown. Added how this estimate was obtained (p. 6, l. 6)

P5 L32 - 'aerosols' -> ‚aerosol'
Change to paper: as suggested

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?
Change to paper: Was not shown. We do not show the respective figure in the paper or the SI, since there are already a lot of figures, this sensitivity has been documented in previous papers, and is not significant for the scientific conclusions in the paper. The figure is shown in Fig. 6 in this reply.

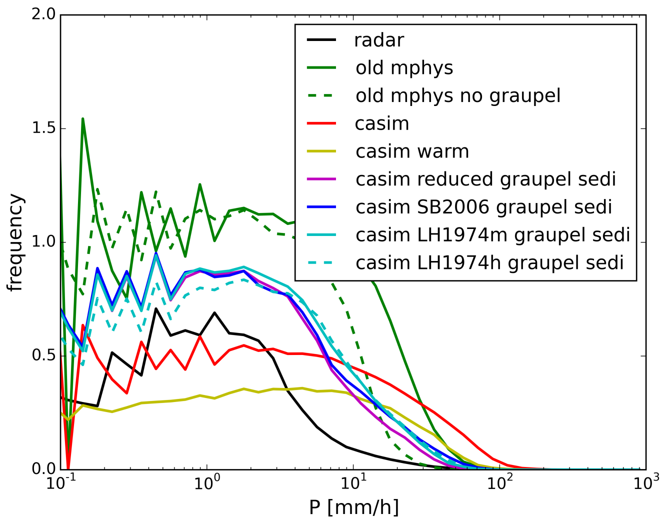


***Fig. 6.** Sensitivity of surface precipitation rate distribution to various graupel mass-diameter fallspeed relations. Compare the cyan, blue, magenta and red lines. The red line is for the original CASIM formulations, the magenta with a reduced fall speed, the blue with the Seifert and Beheng (2006) relation, the dashed cyan line for the Locatelli and Hobbs (1974) high graupel density relation, and the solid cyan line for the Locatelli and Hobbs (1974) medium graupel density.*

P6 L27 - 'in-soluble' -> 'insoluble'
P7 L20 - 'in the sections' -> 'in sections'
Change to paper: as suggested

P8 L14 - SI Figs. 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.
Change to paper: We have moved two of the subplots (12 UTC & 14 UTC) for the passive aerosol simulation in the main part of the paper. For brevity the rest of the panels remain in the Supplementary Information. We also moved the plots showing the domain configuration and the aerosol size distribution into the SI in order to not to inflate the number of figures in the main paper. These are mainly of interest to other modellers and therefore we think they do not necessarily have to be in the main paper. All information on the aerosol size distribution and domain settings is still contained in either Tab. 2 or the text.

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.
Change to paper: We have included the contours showing the convergence at 250m above ground in all map plots (new Fig. 1, SI Fig. 2, 3 & 9). In addition we included a time series plot of convergence in the Supplementary information.

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.
Reply: The reviewer is certainly right that the double-line feature is less pronounced in the simulations than in the observational data and certainly least clear in the passive aerosol run.
Change to paper: We have modified the text accordingly (p. 8, l. 28/29).

P8 L19 - 'satellite data' - there has been no satellite data presented or discussed in the paper thus far.
Reply / change to paper: The satellite data we have is from a geostationary satellite and therefore the image quality over south-west England is not very good. We prefer not to show the images in the paper. The satellite images provide very little additional information relative to the radar data and we therefore removed the references to the satellite data from the paper.

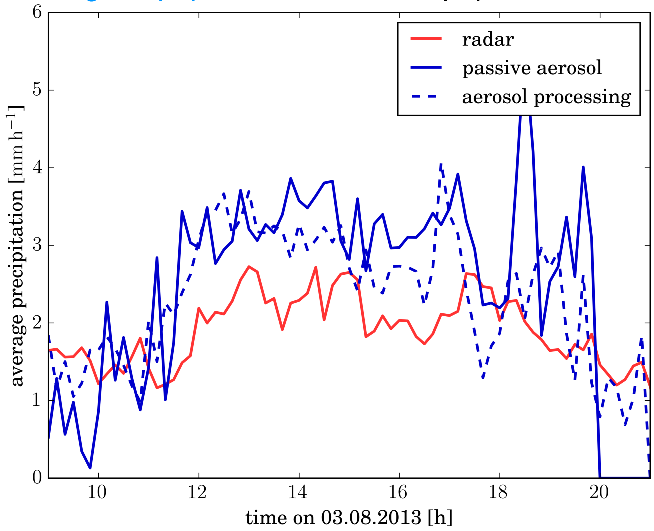
P8 L20 - 'north-westerly' - do the authors mean north-easterly? (or SW to NE?)
Change to paper: as suggested

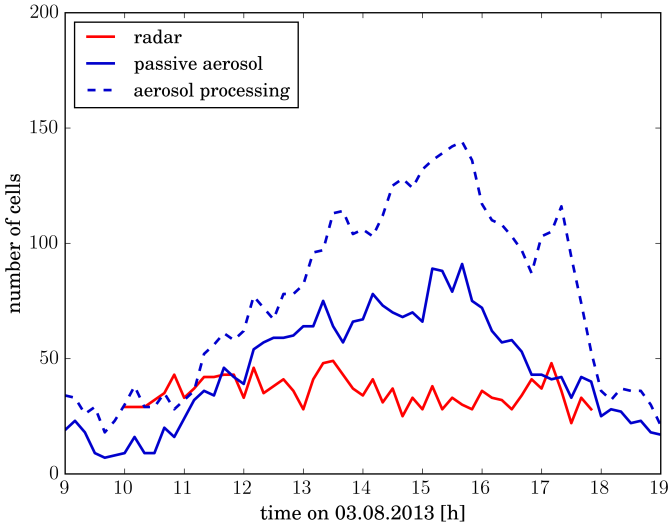
P8 L23-24 - see general comment about spin-up
Reply / change to paper: see general comment 4

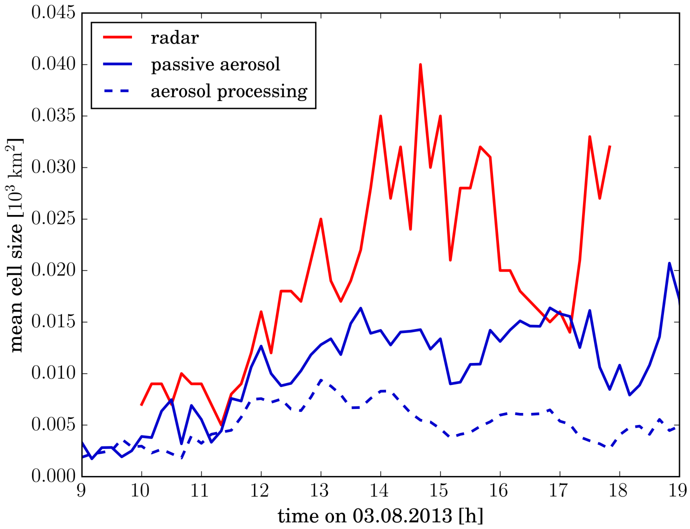
P8 L27-28 - 'The smaller domain average precipitation in the model is mainly due to the overall smaller cell sizes' - what happens if you compare precipitation from cells only (i.e. not domain average), or domain average weighted by cell fraction. Do you get better agreement between the model and observations?
Reply: Mean precipitation rate from cells only is shown in Fig. 5. In contrast to the domain-average precipitation, it overestimates precipitation. This is consistent with the conclusion, that the cloudy area is underestimated in the model compared to observations. The number of cells in the observations is comparable or smaller than the one in the model simulations (Fig. 5 bottom left). The mean cell is larger in the observations than in the model (Fig. 5 bottom right). Accordingly we conclude that the smaller cell sizes are causing the underestimation of precipitation.
Change to paper: We have included these figures in the SI and referenced them in the text (p. 9, l. 1-6).

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.
Change to paper: We have included the contours showing the convergence at 250m above ground in all map plots (new Fig. 1, SI Fig. 2, 3 & 9). In addition we included a time series plot of convergence in the Supplementary information.

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?
Reply: both overestimate for 10(5)-20dBZ, but underestimate below
Change to paper: The text in the paper has been modified along the lines of the reply to general







comment 5 (p. 9, l. 23-27).

P9 L16 - ‘...where the model underrepresents the medium surface precipitation rates’ - refer to Fig. 3a here
Change to paper: as suggested

P9 L16 - 18 - See general comment 6
Reply / change to paper: 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?
Reply / change to paper: see general comment 4

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?

Reply: The regions used for the analysis of the observations and the model are the same. However, cirrus clouds are present in the observational data. If they form outside the domain of the high-resolution simulation and are not present in the global model simulations, they will not be present in the analysis domain.
Change to paper: Reformulated sentence: „… due to higher level ice clouds, which are not present in the model simulations."

***Fig. 5.** Average precipitation rate for points with non-zero precipitation (top). Comparison of cell number (bottom left) and mean size (bottom right) from model simulations and radar observations. Cells are defined as continuous areas of composite radar reflectivity larger than 25 dBZ.*

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?
Reply: These are some high level clouds forming towards the end of the simulation (after 1730 UTC). Where they overlap with lower-level clouds they are included in the composite CDNC profile.
Change to paper: We modified the figure to include only contributions from the lowest cloud in a given profile.

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.
Reply: We reformulated the sentence (p. 11, l. 20).

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.
Change to paper: We replaced Fig. 4 (Hovmöller plots) by SI Fig. 7a, d-f (cross-sections). Fig. 4 is now in the supplementary information. We think this will make the argumentation more easy to follow. All figure references have been changed accordingly.

P11 L6-8 - see general comment 7
Reply / change to paper: see general comment 7

P11 L6 - 'in the cloud' -> 'in the clouds'
Change to paper: as suggested

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).
Reply: s. general comment 7.
Change to paper: We have reformulated section 4.1 (section 4 in the revised paper) to clarify the points raised.

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).
Reply / change to paper: see general comment 8

P12 L11 - how is a cell or cell size defined?
Change to paper: We transferred this information from the figure caption to the main text (remains in the figure caption as well!).

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?
Reply: Yes.
Change to paper: We have made this more clear in the paper (e.g. p.16, l. 9/10; p. 21, l. 23/24).

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?
Reply: The reason is the different position of the cloud top height to the base of the stable layer. For simulations with passive aerosols the cloud top height is larger for the low aerosol scenario compared to the simulations with aerosol processing. It is therefore closer to the use of the stable layer and accordingly the change in cloud top height is smaller for enhanced aerosol concentrations. This was already explained in the original paper on p. 12, l. 27-32.
Change to paper: We have partly reformulated the paragraph in question to make the argumentation clearer (p. 13, l. 24 to p. 14, l. 5).

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'
Change to paper: as suggested

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?
Reply: This section has been reformulated according to suggestions by reviewer #2 (new section 4).

P15 L10 - 'correspond to simulations, for which' -> 'correspond to simulations for which'
P16 L5 - 'importance For' -> 'importance. For'
Change to paper: as suggested

P17 L1-2 - 'the vertical velocity is almost unaltered as is the cloud base temperature' - which figure shows this?
Reply: This was not explicitly shown for the cloud base temperature, which we now indicate. The small difference in vertical velocity can be inferred from the small difference in the kinetic energy profile in Fig. 10 b. We have modified the text to make this more clear (p. 18, l. 8).

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?
Reply: This is true for increasing aerosol concentrations from the low to high aerosol scenario. The impact is small for the transition from the high to very high aerosol scenario, as there is hardly any rain above the 0ºC line in the high aerosol scenario already.
Change to paper: We added an additional sentence to reflect this (p. 18, l. 16-19).

P17 L15-19 - How is this shown? (See general comment 7)
Reply: We cannot show this very easily as we do not have flux data or mixing terms available. The statements in this section are mainly speculations about the most likely mechanism explaining the observed profiles.
Change to paper: We rephrased the section to reflect the speculative nature of the statements (p. 18, l. 20-35).

P18 L33-P19 L1 - see general comment 6
Reply: s. general comment 6.
Change to paper: We modified the sentence according to the comment.

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
Change to paper: as suggested

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'

Change to paper: as suggested. The inset is now a separate figure, so it should be well readable

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

Change to paper: as suggested

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

Change to paper: as suggested

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'

Change to paper: As suggested.

Fig A1:
- 'Appendix B' -> 'Appendix A' ?

Change to paper: As suggested.

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.

Change to paper: As suggested.

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)?

Change to paper: As suggested.

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

Change to paper: Added explanation to figure caption.

SI Fig 7:
- ‘redish’ -> ‘reddish’

Change to paper: as suggested

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

Change to paper: as suggested

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

Change to paper: as suggested

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

Change to paper: as suggested

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

Reply: There is some isolated high level clouds in this simulation between 6 and 7 km.
- This figure isn’t referred to in the manuscript

Change to paper: Figure has been removed.

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

Change to paper: as suggested

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

Change to paper: Figure has been removed.

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