Replies to review RC2
'Aerosol-cloud interactions in mixed-phase convective clouds. Part 1: Aerosol perturbations' by Miltenberger et al.

1. **Grammar**: I found the text to be quite difficult (if not impossible) to follow in places due to the very large number of significant grammar errors. While I would typically provide a detailed list of such errors and corrects; the number of mistakes is too large for such details at this point in the review processes. Thus, I provide a list of items for the authors to review:

- **a. Oxford comma**: The Oxford comma is inconsistently used in the paper, making the intended meaning often difficult to determine. I suggest that the authors consider using it throughout to make it very clear that a list is being defined versus a sub-clause that further defines a term or concept.
- **b. Hyphens**: Hyphens are also used inconsistently thought the paper. For example, "cloud-top" and "cloud top" are used. There are also places where hyphens are needed, e.g., "upper-level stable layer" instead of "upper level stable layer". Please review the use of hyphens, especially in compound adjectives.
- **c. Subject-verb agreement**: There are numerous sentences in the manuscript in which the subject is singular and the verb is plural (or vice versa). For example, on Page 1, Line 25, the subject is "response", which is singular, and the verb is "suggest", which is plural. Moreover, the singular form of verbs is used when the term "data" is the subject; however, "data" is plural. Please review and make changes throughout the paper. Also note that "reflectivity" is singular.
- **d. Punctuation**: In particular, commas are used incorrectly throughout the paper (in addition to the Oxford comma discussed above). In many cases, it makes it very difficult to read the sentence and gain a coherent understanding of the intended meaning. In some cases, the lack of commas results in run-on sentences. There were several sentences in the text that I had to read several times before I was finally able to understand the authors' intention. For example, when using a phrase that introduces a sentence, a comma should follow, such as "According to their analysis, the balance between...", a comma should precede the reference on Page 3, Line 27, "After about 11 UTC, clouds organized...". These are just examples. Another set of examples in which commas are misused but create run-ons is as follows (just examples), "The CASIM module provides options for one- or two-way coupling between aerosol properties and cloud properties, and simulations are performed in both modes" and "Boundary layer processes, including surface fluxes of moisture and heat, are parameterized with the blended boundary layer scheme (Locket et al., 2015), and sub-grid scale turbulent processes are represented...".
- **e. Incomplete sentences**: Please ensure that all sentences are complete (subject and verb). For example, the text on Page 7, Lines 29-31, form two incomplete sentences.

Change to paper: Thank you for pointing out these inconsistencies in the manuscript. We have checked the manuscript very carefully for the raised issues.

2. **Lack of supporting evidence and number of figures**: There are many places in the text, primarily in the discussion of the results where a conclusion is drawn without supporting evidence. My initial suggestion would be to at least plot the fields of interest to confirm that the conclusions are true; however, there are already too many figures in the paper (not to mention that it is hard to follow the analysis because the referenced figures switch back and for from those presented in the main text and those in the supplementary material). I suggest that the authors think very carefully about what figures are absolutely important to telling their story. If a figure is mentioned in passing, remove it in favor of a figure that shows that the conclusions are robust.
Change to paper: We have checked the text for missing evidence. Where necessary we have added figures (mainly to the SI). For examples please see the more detailed comments below. In other instances, we have clarified whether the statement is a hypothesis or indicated that the figures is not shown, e.g., for sensitivity tests in the initial set-up of the model (the graupel fall speed relation).

For example, on Page 9, Line 22, it is noted that convection deepens with larger convergence forming along the sea breeze lines. Can you show this in the simulations? Not all figures need to be direct model-obs comparisons; the model can be used to justify your conclusions and fill in the gaps where the observations are lacking sufficient information.
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.

Furthermore, some of the figures selected for the manuscript are difficult to read (partially due to the incomplete information given, e.g., units—see comment below regarding units in general—and even just a lack of axis titles). For example, Fig. 4 and the corresponding text on Page 11, first paragraph, are very difficult to follow. Perhaps another figure format would better convey the results?
Reply: We have double checked all figures include axis titles, whoever we could identify no plots with missing axis titles except map plots, for which longitude and latitude were not labelled.
We also checked that the unit notation is consistent throughout the paper.
Regarding the Hovmöller plots for column aerosol loading (Fig. 4), we will swap these with the cross-sections of the aerosol field (SI Fig. 7) and adjust the text on page 11 to make the discussions clearer.
Change to paper: Thanks for pointing this out. Checked axis titles and units. We swapped Fig. 4 and SI Fig. 7, as the latter is probably more intuitive for the reader. We modified the text in section 4 accordingly.

Moreover, conclusions are drawn regarding process rates but these values are not shown. These rates are predicted by the model. Did you look at the rates to confirm the conclusions?
Reply: We do not have the rate output available. The rates can be inferred by looking at the difference between the aerosol fields from the passive aerosol and the aerosol processing simulation. The aerosol fields in the passive aerosol run are only affected by advection and show that the low values inside clouds are due to activation to CCN. Due to the low aerosol values there can be no activation. If there was no secondary activation the vertical decrease in CDNC that occurs in the aerosol processing runs would be also expected in the runs with passive aerosol. This is the only mechanism that is able to replenish CDNC in the model.
For the increases in accumulation and coarse mode aerosol below cloud base, at cloud edges and in dissipating clouds there is also only one possible explanation given the model formulation: The only process in the model that can increase aerosol concentrations (except convergence) is evaporation of hydrometeors.
Change to paper: We included cross-sections of the aerosol fields and aerosol Hovmöller plots for the passive aerosol run in the SI. In addition we modified the text in section 4.1 to make clear why we are able to draw these conclusions.

Along these lines, I suggest that as the authors consolidate the figures, that the text be consolidated. The paper is long (my best guess is ~10,000-12,000 words), and this is just Part 1. My opinion is that less is more in some cases; you do not need to discuss every variable; instead, focus on the results that are most relevant to the story that you want to tell and the biggest conclusions. Otherwise, the important implications are muddled.
Change to paper: We have shortened the text, where this was possible without altering the scientific message, e.g., new section 5 (was section 4.2 ff previously).

3. References: There are several places in the text where references should be included but are missing. For example, on Page 2, Lines 11-12, a reference or several references should be included for this "concept". In the discussion of aerosol regeneration, several references could be included but are omitted. Consider referencing Xue et al. (2010, J. Atmos. Sci.) and Mitra et al. (1992, J. Aerosol Sci.), just to name a few.
Change to paper: Thank you for pointing out these references.We have included additional references at the suggested locations.

Moreover, there are errors in the list of references that should be addressed (e.g., n/a for page numbers)
Change to paper: *Thank you for spotting these errors. We checked the reference list and added the missing information.*

4. Analysis: There are several places in the text where the authors simply describe a figure but provide not reasoning for the differences depicted in such figures. For example, in Section 4.3, I just kept asking myself "why?" If details regarding why differences are observed are omitted, then I suggest shortening the discussion of the relevant topics and focusing on other aspects of the simulations.
Change to paper: Section 4.3 was reformulated. We have checked the paper for instances of observations described without an explanation and made sure an explanation or hypothesis is provided whenever possible. It has been clarified that the previous section 4.2ff (now section 5) merely describes the changes in cloud field properties due to aerosol, while the physical mechanisms are discussed in the following section. We think that makes the argumentation easier to follow, as many changes are interlinked. Section 5 was shortened relative to the previous sections.

Furthermore, regarding the analysis of G and L, it appears that this is only applicable for a closed system. Based on my understanding of the simulations, this is not the case because moisture could (and should) be advected through the inner domain's boundaries. Thus, vapor may condense in the domain but be lost through the boundaries; it appears as though this is not accounted for; moreover, it is unclear how important this is in terms of the main results of the paper.
Reply: The analysis of G and L is strictly only valid if there is no advection out of the domain, as the reviewer points out correctly. This is of course not the case in the presented model simulations. The effects this has are discussed in the original paper (paragraph starting at p. 14, line 30 in the original paper and Fig. 8a). It was found that there are only small differences in the advective terms between runs with different aerosol concentrations and we therefore think the analysis is valid even without considering the advective terms explicitly in the rest of the paper.
Change to paper: We reformulated parts of this paragraph in the paper to make the argumentation clearer (p. 15, l. 24 to p. 16, l. 1).

Minor Concerns
1. In general, please be consistent with the verb tense in the paper. Present and past tense are used throughout the discussion of the results, making it hard to determine if the authors intended for a sentence to be a general idea or specifically related to the case study.
Change to paper: We carefully checked the verb tense in the paper.

2. In general, the units are kind of a mess in the paper. There are many places where spaces are not present, making it difficult to figure out what the units are supposed to be. Also, the units in figures are missing in places or change from figure to figure (e.g., degrees east versus degrees west longitude; the later is preferable for the study area so that negative coordinates are not needed). Consider using inverse units throughout the paper and in figures. Also, the use of "**" to represent an exponent is odd for a manuscript.
Change to paper: We have check all units in the paper and ensured the typesetting is consistent throughout.

3. Please review the subscripts and superscripts in the figures. The variables are not consistent between the main text and the figures because of differences in the use of subscripts and superscripts.
Change to paper: We made all sub- and superscripts consistent between text and figures.

4. The naming convention used for the runs changes from one figure to the next.
Change to paper: We checked all the run names and ensured they are consistent.

5. Page 1, Lines 15-18: The definition of invigoration is not in line with how it is commonly presented in the literature, i.e., related to enhanced lofting of liquid above the freezing level where subsequent freezing increases latent heating aloft and increases buoyancy. Please revise accordingly.
Reply: We use the term „convective invigoration" for the occurrence of higher updrafts in convective clouds in high aerosol environments, which is consistent with the use of invigoration in the literature. However, in contrast to the common presentation in literature, we do not find strongly enhanced latent heating due to freezing. Instead latent heating in the warm-phase section of the clouds is enhanced. This partly contradicts the results presented so far in literature, but is consistent with the results of our modelling study and is supported by multiple figures.
Change to paper: We reformulated the sentence to better reflect, where our findings disagree with literature.

6. Page 1, Line 21: What are the thermodynamic constraints?
Change to paper: Replaced „thermodynamic constraints" with „stable layer in the upper troposphere".

7. Page 5, Line 9: Why is the model top set to 40 km? Most modeling studies of even the deepest convection in the troposphere use model tops of 20-25 km. This seems as though a lot of computational cost is wasted simulating nearly the entire stratosphere.
Reply: The model top is identical to that used in the operational configuration of the Unified Model for regional model simulations (UKV-nest with a grid-spacing of 1.5 km). Since changing the model top can impact the reflection of gravity waves from the upper boundary, we think it is best to use the set-up developed and tested in operational use.

8. Page 6, Line 19: The density selected for graupel is quite low, especially compared to what is commonly used in microphysics schemes. I believe some additional justification is needed.
Reply: We have tested a range of different graupel density and consistent diameter-fallspeed relations. The chosen relation provides the best agreement with the observed rain rate distribution. A figure showing the changes in the precipitation rate distribution for a number of graupel diameter-fallspeed relations is shown in Fig. 6 in reply to reviewer #1.
Change to paper: The paper does already state our main motivation for choosing this graupel density and corresponding diameter-fallspeed relation.

9. Page 6, Line 28: Number density is not a conserved variable; please explain.
Reply: We are not sure what this comment is referring to. It is not stated in the paper that number density is a conserved variable. The referenced line states that aerosol number density is a prognostic variable in UM-CASIM.

10. Page 7, Line 7: Is the Abdul-Razzal and Ghan (2000) activation parameterization particularly applicable to high-resolution simulations of convection?
Reply: The Abdul-Razzak and Ghan (2000) activation parameterisation is one of the published parameterisations for activation of cloud droplets from the aerosol based on the prognostic vertical velocity. It is therefore suitable for the purpose in the presented study. This parameterisation has been use in previous high-resolution model studies (e.g., Grosvenor et al., 2017).

11. Page 8, Line 19: Where are the satellite data?
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.

12. Page 9, Line 17: What is meant by "sub-cloud evaporation in the radar diagnostic"? Do you mean that the simulated radar reflectivity is somehow accounting for the model-predicted evaporation rate?

Reply: Surface precipitation rate is derived from radar data by a fixed relationship between reflectivity and surface rain rate (a so-called Z-R formula; Harrison et al., 2016). However, sub-cloud evaporation can influence the surface precipitation rate and this is not taken into account very well by using a fixed Z-R formula (e.g., Li and Srivastava, 2001). In contrast, sub-cloud evaporation is explicit modelled in the NWP simulations. This difference in the representation of sub-cloud evaporation (or lack thereof) likely contributes to the different performance of the model simulations in the evaluation against radar-derived surface precipitation rates and radar reflectivity.
Change to paper: We reformulated the sentence for clarification (p. 9, l. 17/18).

13. Page 9, Line 20: Why did you choose 18 dBZ? Do you have a reference for such a choice? It is later stated that there is sensitivity (albeit small) to this choice; this should be expanded upon to convince the reader that the results are really robust.
Reply: 18 dBZ is often used in radar networks as threshold for the radar echo top (e.g., Lakshmanan et al., 2013; Scovell and Al-Sakka, 2016).
Change to paper: We included a quantitative description of the sensitivity to the threshold in the paper (p. 10, l. 17-19).

14. Figure 7: Why not use a box-and-whisker plot (or something similar); the way the model output is presented makes it difficult to really understand the figure.
Change to paper: Thank you for this suggestion. The new plots are indeed easier to read. We have changed the plots as suggested. Note that a bug in the code used to generate the original figures was detected after the submission of the paper. The updated plot has been included in the paper. The text in section 5.3 has been modified accordingly.

Other Concerns

1. Page 1, Line 8: Change “match to observed” to “correspondence with observed“.
2. Page 1, Line 1: Change “effect” to “affect”.
3. Page 2, Line 1: Remove “and” at the end of the line.
4. Page 2, Line 5: Remove “The” at the beginning of the sentence.
5. Page 2, Line 21: Change “processes involved” to “relevant processes”.
6. Page 4, Line 11: Remove either “including” or “e.g.,” because including both is redundant.
7. Page 4, Line 14: Define “COPE”.
8. Page 4, Line 15: Add “the” before “UK”.
9. Page 4, Line 24: This sentence does not make sense.
10. Page 4, Lines 29-30: This sentence needs to be reworded.
11. Page 5, Lines 15-16: Consider just saying that the operational microphysics was replaced and omit the “in addition to the standard model code”; this should be obvious to the reader.
12. Page 5, Line 25: Change “simulations, because:” to “simulations because”.
13. Page 6, Line 15: I believe that these are the zeroth and third moments.
14. Page 6, Lines 14-15: This sentence is confusing (perhaps it is just the lack of an Oxford comma), but I am not completely sure. Also, the use of “relation” and “relations” is confusing. Is there a single relation for everything?
15. Page 6, Line 27: Insoluble is not hyphenated.
16. Page 7, Line 16: Change “traced” to “tracked”.
17. Page 7, Line 28: Change to “The initial aerosol conditions”.
18. Page 16, Lines 24-25: This sentence needs to be reword because it appears as though you are defining depths with units of m/s.

Change to paper: Thanks for spotting these. All suggested changes have been included and sentences have been reformulated.

References

Abdul-Razzak, H., and S. J. Ghan (2000). A parameterization of aerosol activation. 2. Multiple aerosol types. *J. Geophys. Res.*, **105**, 6837–6844.

Grosvenor, D. P., Field, P. R., Hill, A. A., and B. J. Shipway (2017). The relative importance of macrophysical and cloud albedo changes for aerosol-induced radiative effects in closed-cell stratocumulus: insight from the modelling of a case study. *Atmos. Chem. Phys.*, **17**, 5155–5183, doi:10.5194/acp-17-5155-2017.

Harrison, D. L., Scovell, R. W., and M. Kitchen (2009). High-resolution precipitation estimates for hydrological uses. *Proceedings of the Institution of Civil Engineers - Water Management*, **162**, 125–135, doi:10.1680/wama.2009.162.2.125.

Lakshmanan, V., Hondl, K., Potvin, C. K., and D. Preignitz (2013). An improved method for estimating radar echo-top height. *Wea. Forecasting*, **28**, 481—488, doi:10.1175/WAF-D-12-00084.1.

Li, X., and R. C. Srivastava, R. C (2001). An analytical solution for raindrop evaporation and its application to radar rainfall measurements. *J. Appl. Meteor.*, **40**, 1607–1616, doi:10.1175/1520-0450(2001)040<1607:AASFRE>2.0.CO;2, 2001.