The following is a review of the article entitled “Aerosol-cloud interactions in mixed-phase convective clouds. Part 2: Meteorological ensemble.” by A. Miltenberger and coauthors for publication in *Atmospheric Chemistry and Physics* journal.

**General conclusion:**

I think the article is appropriate for *Atmospheric Chemistry and Physics*, because it is an interesting study which investigates a case of moderately deep convection developed along sea-breeze convergence over the southwestern peninsula of the UK, by the use of high-resolution model simulation ensembles, with the motivation of enlightening the relative contribution of variations in meteorological and aerosol initial and boundary conditions to different cloud properties.

From my point of view, the main article strength is that it is able to disentangle (to a certain extent) whether the cloud and precipitation effects are due to the meteorological variability or due to aerosol background concentration initial conditions, at least for the case study of mixed-phase convective clouds. Moreover, I think the choice of an increase and decrease in passive aerosol concentration by a factor of 10 is appropriate for the simulations with perturbed aerosol profiles. On the contrary, the main weakness is that there is too much description of the case study until the interesting conclusions are reached. That, at my understanding, makes the reading too much detailed and tedious to follow. Therefore, I would recommend to reduce the number of figures (or move them to the SI) and get to the point on the important findings and conclusions (basically sections 5, 6 and 7) sooner. Also, I would suggest to the authors to use the significance results presented in Table 1 (unpaired) all over the whole discussion text, since it has important implications whether a result is significative or not. For instance, in section *5.4.Radiation* I would add that only OSR results are significative (and only those regarding the comparison between low-standard and low-high aerosol concentrations) and not OLR results, thus the reader do not have to wait until the end of the paper (section 7) to know that some of those differences described before are in fact not significative. Besides, there are many other technical issues and questions which I am listing in the following *Specific comments* and *Typos*.

Ultimately, I recommend the article publication in ACP because it is clearly contributing to enhance the knowledge of the scientific community regarding to aerosol-cloud interactions in mixed-phase convective clouds, but a major revision is required because the paper could still be improved.

**Specific comments:**

Section 1: The introduction is appropriate since it is explaining the nowadays main issues, providing the necessary state-of-art, and introducing the contribution of the present study.

- Page 2, lines 6-9: however it is true that in the last decades was a large increase in anthropogenic aerosol emissions, I would add that "the emissions have decreased in the last decades (in comparison the 80s-90s maximums) thanks to the introduction of pollution policies in the developed countries in the Northern Hemisphere".

Section 2:

- Page 4, lines 20-27: the fact that 9 ensemble members were selected is repeated 3 times in only 8 lines, please consider rewriting the paragraph. Moreover, could you explain how they were chosen among the 33 global ensemble members? (see comment in Fig. 1)
- Page 5, line 11: please clarify the sentence "[…] nested simulations with a h a grid […]".
- Page 5, lines 13-14: consider the necessity of repeating the same set of references for CASIM since they have been cited already in page 3, lines 33-34.
- Page 5, lines 17-18: How and why moisture conservation is enforced?

Section 4:

- Page 10, lines 9-10: could you clarify why "the surface moisture flux adds some modifications to the boundary layer moisture budged, e.g. ensemble members 3 and 4, and members 7 and 8, respectively", because I am not able to see it in the figures.
- Page 10, line 11: from my interpretation of figure 4b, I think ensemble members cntl, 4 and 8 have a particularly large surface sensible heat flux.
- Page 10, lines 18-19: could you add references/citations to the cloud top height threshold based on the condensed water content, and to the cloud fraction based on the condensed water path?
- Page 10, line 22: Are not ensemble members 2 (instead of 8) and 5 (instead of 4) those with the largest and smallest mean cloud top height, respectively? (for standard aerosol case).
- Page 10, line 25: based on figure 4a, I would say that ensemble members 2, 4 and 8 are those with high surface moisture fluxes (and not 4 and 7). On the other hand only ensemble members 4 and 8 have larger low-cloud fraction.
- Page 10, line 28: based on Fig. 7d, I think that the variation in PE is higher than 5 %.
- Page 10, line 32: based on Fig. 7d, I think ensemble member 6 has a relatively large PE (instead of 8).
- Page 11, line 3: based on Fig. 11a, "the largest (smallest) values occur for ensemble member 1 (8)" (instead of 2 (7)).

- Page 11, lines 3-4: please, recheck the sentence regarding the relation between outgoing longwave radiation and the cloud top heights since for instance in Fig. 6 it is seen that ensemble members 4 and 5 have similar mean CHT but on the other hand large differences on OLR. How do you explain that?

Section 5:

- Page 11, line 5: why the section title says "identical meteorological initial and boundary conditions" when in fact here are discussed the differences between ensemble members with different meteorological initial and boundary conditions (as stated in line 13 in the same page)?
- Page 11, lines 24-25: does it mean that there are less clouds but larger?
- Page12, line 2: according to SI Fig. 8a, I think ensemble member number 4 is missing from the list of ensemble members where the change in low cloud top fraction is dominant.
- Page 12, lines 32-34: Is not ensemble member number 7 also fitting in the exception list? At least this is what I can see from Fig. 7c. For this reason, I recommend changing the graphic color palette or enlarging the figure. Anyway, what do you think is the reason why the control simulation has higher surface precipitation with the low aerosol scenario?
- Page 12 line 34 and page 13 line 1. Could you check the affirmation again? I do not see the comparatively large condensate gain in Figure 7a.
- Page 13, line 1: please check the following inconsistency: you stated "Accordingly, the precipitation increase for these members…" when you just said in page 12 line 34 that "[…] members 6 and 8 with no change in accumulated surface precipitation."
- Page 13, line 2: I do not see clear the sentence "For the other members, the change in PE dominates over changes in condensate production" because the change in PE is also large for ensemble members 6-8, and for some of the other members it is actually not that large.
- Page 13, line 3: consider adding "(decrease)" after "precipitation response".
- Page 13, line 10: I am not sure if ensemble member number 5 falls in this exception list, could you please check it again? Additionally, from figure 8a it is remarkable that for some ensemble members (3, 4 and 8) the mean precipitation turns to zero with the highest aerosol concentration scenario, perhaps you would like to highlight it into the discussion.
- Page 13, lines 12-14: I do not think "all percentiles up to and including the $75^{th}$ percentile show an increase with the aerosol concentration" for all ensemble members, since in ensemble member 4 the standard aerosol is lower than the other two and in member 7 the high aerosol is lower than the low and standard aerosol concentration.
- Page 13, lines 16-17: consider adding to the discussion the fact that, as other studies have shown, enhanced aerosol scenarios suggest more freezing processes inside clouds and invigoration, provably due to longer cloud lifetime.
- Page 13, lines 33 and 34: I agree with the sentence "This change is consistent with the increased CDNC and small impact of the aerosol scenario on the cloud fraction", however,

the change (decrease) in the CF shown in Fig. 6 would cause the opposite effect. How do you explain that? (The same reasoning applies in the sentence in page 15, lines 7-8).

- Page 13, lines 30-34, and page 14, line 1-2: how do you know that radiative signal presented here is mainly due to CDNC changes and not due to an increase in aerosol scattering (the so-called 'direct effect')? Moreover, consider referring here to the 'indirect effect' or 'cloud albedo effect' and adding "Twomey, 1974" citation reference. Consider also adding "(increase)" after "due to CDNC changes".

Section 6:

- Page 14, line 16: I would rather prefer "changes follow a similar pattern for each meteorological ensemble member" than "changes are similar for each meteorological ensemble member".
- Page 14, lines 11-19: I miss the authors saying something regarding changes in WP and LWP in this paragraph.
- Page 14, lines 28-29: why the authors only consider the time frame from 9 to 19 UTC while the model was run from 0 to 24h on the 3/8/13? Is it due to meteorological reasons or because of the model spin-up time period? Moreover, why in Fig. 12 is used the time average 10 - 19UTC? Is it a typo?
- Page 14, line 29: please change "Figs. 5-11" for "Figs. 5-9 and 11". Moreover, is Fig. 10 done with the data from 0 to 24h or with data from 9 to 19h?
- Page 14, line 30: the variables are not plotted in a box-plot but in an error bar type plot.
- Page 14 line 35 and page 15, line 1: could it be related to a longer cloud lifetime?

Section 7:

- Page 16, lines 17-19: I suggest changing the sentence since some of the cloud properties stated here are poorly modified by the aerosol perturbations (e.g. cloud fraction), not modified considering all perturbations (e.g. condensation gain G is not significative for standard-high comparison), and not modified if unpaired cases are considered (e.g. precipitation rate or cloud fraction are only significative for paired cases)
- Page 16, lines 22-34: Could you explain in more detail how the significance analysis of paired and unpaired was done? How do you pair the ensemble members? I do not really see the advantage of the paired significances with so few ensemble members and it looks to me confusing, if not misleading, since as you already say in page 16, lines 26-27, the statistical analysis is based on a very small sample, which affects the validity of several assumptions. Therefore, I personally prefer the results with unpaired cases because the sample is already too small to be paired and because the results with unpaired cases better reflect the results and error bars (spread) shown in all figures and in particular in Fig 12, even at the expense of having less significative results.

Section 8:

- Page 18, lines 31-32: could you give an example of those “variables closely related to aerosol concentrations” and for those “variables that are linked to aerosol concentrations by a series of complex processes” which apply to the investigated case?

I would suggest changing the section 4, 5 and 6 titles since they are not helpful for understanding the article structure. In my opinion, it would be easier for the reader if they are rewritten somehow in that way:

- Section 4: Comparison of the cloud-properties results among 10 different meteorological ensemble members (unperturbed aerosol profiles)
- Section 5: Analysis of the results regarding aerosol-induced changes (3 aerosol concentration scenarios) among 10 ensemble members
- And section 6: Cloud-adjustments attribution (due to initial meteorological and boundary conditions or aerosol concentration loads)

References:

- Fan et al. 2016: has the DOI link repeated.
- Sheffield et al. 2015: has the DOI reference repeated?
- Tao et al. 2012: please check if the reference between the DOI and the year should be there.

Figures:

General comments on the figures:

Generally speaking, the way the figures are presented is a bit chaotic. First of all, they are not correctly ordered, and secondly there are too many. I suggest the following improvements:

- Re-organize all the figures in order of appearance in the text. For example: figures 6 and 7 are referenced in the text before 4 and 5 have been, and figure 11 before figure 10.
- Remove linking lines between ensemble members from the following figures: 4b, 7a and b, SI 8a, and SI 9.
- Consider changing the color palette for the different aerosol load runs in the following figures, since it is difficult to differentiate them: 4b, 7, SI 8a, and SI 9.

Comments on figures from the main discussion paper:

- Fig. 1: in the caption it is stated that 9 ensemble members were chosen from 33. Could you give more information on that? How they were chosen? Which criteria were used?

  Could you change the x-axis ticks in a way that both graphs (a and b) have the same.
- Fig. 3: please add the data information in the last column "mean" in c and d, otherwise remove it from the graphs.
- Fig. 4: consider removing it or moving it to the SI since it is only cited once in the paper (and actually only Fig. 4a), the latent heat flux (Fig. 4b) is not used in for the discussion, and the sensible heat flux figure is only used once. Moreover, I think the caption is wrong because it does not match with any of the three graphs and legends.
- Fig. 5: since it does not show big differences between ensemble members I would suggest moving it to the SI.
- Fig. 7: as said before, I recommend changing the color palette of the graphic or enlarging the figure.
- Fig. 10: please change the legend with the symbols that appear in the graph (i.e. there are no squares in the graph). Also the caption is wrong since there are no "black symbols", "downward pointing triangles" or "upward pointing triangles".
- Fig. 12: this is a really interesting and helping figure. I just want to say that adding a legend or a caption explanation regarding the colors, as well as regarding the acronyms used in the x-axis, would improve it. Please, also include if CDNC is at the cloud base or at the top.

Comments on the SI figures:

- SI Fig. 1: I would include this figure in the main paper (not in the supplement) since it helps the reader quickly identifying the region on the model simulations have been done and how they look like.
- SI Fig. 4: it needs some improvements since it is not intuitive. I would suggest plotting in different colors the temperature and the dew temperature profiles, from both model and observational data.
- SI Fig 6 and 7 captions: "The box plots represent the temporal variabilitiy of each variable" should read "The box plots represent the temporal variability of the variable" or "The box plots represent the temporal variability of the variable for each ensemble member".
- SI Fig. 6: why ensemble 4 with low aerosol look so different from the others? Is there any apparent reason?
- SI Fig. 7: CAPE for cntl simulation is missing. And please, either add values for the last column or remove "mean" from the graphic.

- SI Fig. 9: I suggest adding into the caption the description of IG and IL (from the legend) as well as “[…] condensation (C) and deposition rate (D) […]”.

Tables:

- Table 1: as mentioned before, I would remove the unpaired results.

**Typos:**

- Page 4, line 17: “section ??” should read “section 5”.
- Page 7, line 24 and line 28: “15 UTC” should read “15.20 UTC”.
- Page 10, line 17: “coherent areas” or “consecutive areas”?
- Page 10, line 18: “arial fraction” should read “areal fraction”.
- Page 11, line 9: the word “and” is missing between “lower” and “respectively”.
- Page 11, line 13: the word “part” is missing between “first” and “of this study”.
- Page 12, line 5: remove “is” from the sentence.
- Page 13, line 10: remove “also” from the sentence.
- Page 13, line 32: add the word “by” after “cloud top and”.
- Page 13, line 34: change “increasing” by “increased”.
- Page 14, line 11: “section” should read “sections”.
- Page 18, line 21: “aerosol-induce” should read “aerosol-induced”.