Reply to comments from reviewer #2

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.

Reply : The figures have been reconsidered and there have been significant changes to the distribution of figures between the main text and the SI as well as to the ordering of the figures. All figures are now ordered according to their mentioning in the text. Sections 3 and 4 have been shortened to streamline the text and focus on the key results. However, these sections are still important for the understanding of the results and for providing context on the ensemble performance as well as the magnitude of the meteorological changes, so we believe these sections belong into the manuscript

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.

Reply : We have included the statistical analysis in the text in section 5 and 6. These sections contain now references to Table 1.

Besides, there are many other technical issues and questions which I am listing in the following Specific comments and Typos.

Reply : Thank you very much for pointing these out! We have corrected all the highlighted issues and proof-read the new manuscript more carefully.

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".*

Reply : Thank you for pointing this out. For sake of brevity, we modified the text to say that anthropogenic aerosol emissions have changed significantly over historic period without specifying any trends (p. 2, l. 6-8): "In recent decades, the modification of cloud properties by aerosols has received particular attention, as anthropogenic aerosol emissions have changed strongly over the historic period."

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)*

Reply : The selection procedure was already described on p. 4, l. 20-27. We have reformulated this paragraph to make the description clearer and to make the text more concise (p. 4, l. 27-31):

“The selection of the ensemble members for dynamical downscaling is based on the time-series of moisture convergence and moist static energy convergence computed over the regional model domain from the global model fields (Fig. 1). These timeseries are then used to construct a similarity matrix by summing the Euclidean distances of moisture convergence and moist static energy convergence. Using the the algorithm by J. H. Ward (1963) 9 clusters are defined and from each cluster the member closest to the mean cluster time series is chosen for downscaling."

- *Page 5, line 11: please clarify the sentence “[...] nested simulations with a h a grid [...]”.*

Reply : Sentence was reformulated.

- *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.*

Reply : We have removed these references here.

- *Page 5, lines 17-18: How and why moisture conservation is enforced?*

Reply : The methodology for moisture conservation is described in detail in the two cited papers by Aranami et al.. It is beyond the scope of this paper to provide a description of this methodology.

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.*

Reply : If the surface moisture flux is included the order of ensemble members (if ordering from largest to smallest moisture flux) changes. For example, the lateral PBL moisture flux (red points in Fig. 4 of the manuscript) suggests ensemble member 3 has a larger moisture flux than member 4. However, the surface moisture flux in member 4 is larger than that of member 3 (green symbols in the same figure). Hence, if the total boundary layer moisture flux is considered member 4 has a larger flux then member 3. The text has been modified to (p. 8, l. 15-16):
“The surface moisture flux adds some modifications to the boundary layer moisture budget, e.g. compare total and and lateral moisture convergence for ensemble members 3 and 4 and member 7 and 8, respectively.“

- *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.*

Reply : We are really sorry that in several instance throughout the paper the wrong ensemble member numbers were referred to in the text. This is one of the instances. The reviewer is of course right that here it should read control, ensemble member 4 and 8 (instead of members 4 and 7). The section on surface heat fluxes has been removed from the manuscript to shorten the paper as suggested by the reviewer, so these changes are not actually applied in the new manuscript.

- *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?*

Reply : The chosen threshold for cloud top height (condensed water content larger than 10^{-6} kg kg^{-1}) is typically used in modelling studies to reflect detectability in observational studies and avoid issues with very small numeric values in models (e.g. Fridlind et al. 2010). The condensed waterpath threshold (10^{-3} kg m^{-2}) is derived from this value: For a column to be classified as cloudy the minimum condensed water content needs over about 1000 m altitude range, i.e. for about 10 model levels (all considerations based on low-level values of gridspacing and atmospheric density). The condensed water path threshold is below the estimated lower detection limit of microwave satellite instruments (0.02 kg m-2, Grosvenor et al. 2017). These references have been added to the revised version of the manuscript (p. 9, l. 20-21).

- *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).*

Reply : Yes, of course. The text has been corrected accordingly (p. 9, l. 24).

- *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.*

Reply : The reviewer is right, this should refer to ensemble members 4 and 8. This section is not part of the revised manuscript anymore.

- *Page 10, line 28: based on Fig. 7d, I think that the variation in PE is higher than 5 %.*

Reply : We mean the difference between any to PE values does not exceed 0.05. We recognise that the percent notation introduces confusion. To avoid this confusion and address comments from reviewer 1 regarding this sentence, the new text reads: "In contrast, PE does not vary systematically with the large-scale convergence (Fig. 9 b)." (p. 9, l. 30-31)

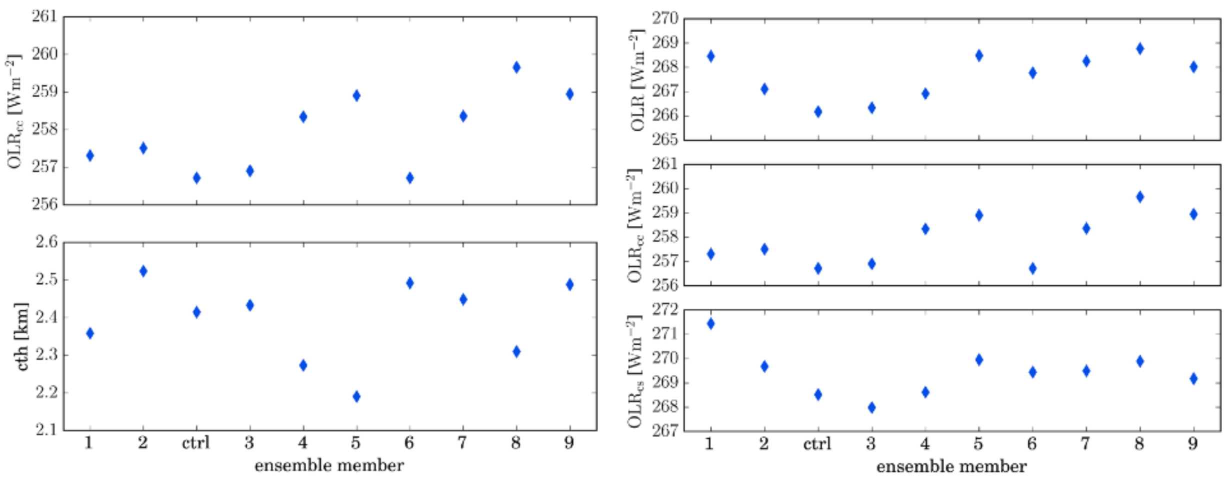

Figure 1. Mean outgoing longwave radiation for cloudy grid points (top left) as well as mean cloud top height (bottom left) and mean cloud fraction (bottom right). The panels on the right show the total mean outgoing longwave (top) and outgoing longwave from cloudy (middle) and clear sky (bottom) gridpoints. Only simulations with the standard aerosol conditions are shown.

- *Page 10, line 32: based on Fig. 7d, I think ensemble member 6 has a relatively large PE (instead of 8).*

Reply : Yes, this has been corrected (p. 9, l. 34).

- *Page 11, line 3: based on Fig. 11a, "the largest (smallest) values occur for ensemble member 1 (8)" (instead of 2 (7)).*

Reply : Yes, this has been corrected (p. 10, l. 10).

Section 5:

- *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?*

Reply : Thank you for pointing this out. A more careful analysis shows that there is a relatively good correspondence of the mean OLR from cloud grid points and the mean cloud top height (left panels of Fig. 1). The match is of course not perfect, as OLR is very sensitive to cloud top temperature (~T^4) and therefore changes in the distribution matter as well, which are not reflected in considering the mean cloud top height. There is also a relatively strong variation in the clear sky

outgoing long wave radiation, which is caused by different surface temperatures, cloud positions, e.g. more or less cloud over the ocean, and water vapour path in the different ensemble members (Fig. 1, right panels). The domain average outgoing longwave radiation is a combination of these two contributions, but is strongly weighted towards the clear sky outgoing longwave due to the overall small cloud fraction ($\lessapprox$ 0.15, Fig. 2).
We have included Fig. 2 in the SI and altered the text as follows (p. 10, l. 13-15): “While differences in the cloud top height distribution contribute to the variability in outgoing longwave radiation, variations in the clear sky outgoing longwave radiation dominate the overall variability due to the relatively small cloud fraction (SI Fig. 13).“

- *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)?*

Reply : We acknowledge that the section title is misleading. This section discusses aerosol-induced changes in the high and low aerosol scenario relative to the ensemble member with the same meteorological and the standard aerosol scenario. We have renamed the section “Aerosol-induced cloud property changes in different meteorological ensemble members (paired meteorology)“

- *Page 11, lines 24-25: does it mean that there are less clouds but larger?*

Reply : Yes exactly. This is included in the revised text (including also changes made in response to reviewer 1): "It has been hypothesised in the first part of this study, that the slower conversion of condensate to precipitation in high aerosol conditions allows clouds to grow larger and merging with other updraft cores resulting in ***overall fewer, but larger clouds***.“ (p. 11, l. 7-9)

- *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.*

Reply : Yes, this has been corrected. (p. 11, l. 15-17): “In the control run and ensemble members 4 and 6, the decrease of the mean cloud top height is due to a reduction in the medium altitude fraction, while in all other members changes in the low cloud top fraction dominate.“

- *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?*

Reply : In ensemble member 7 surface precipitation is decreasing slightly. We agree this is hardly visible from the previous Fig. 7c, but it is clear from its position of the one-to-one line in previous Fig. 10. Previous Fig. 7c has been moved to the appendix and includes now a plot of ΔP, which should make this even clearer (new SI Fig. 11).
As for the increase in the surface precipitation in the control simulation: We think this is due to the relative large increase in G (largest of all ensemble members) and the increase in PE. In fact, the control is the only ensemble member, for which PE increases from the low to the standard aerosol scenario. With out a detailed analysis of the thermodynamic, latent heating, and hydrometeor profiles similar to Miltenberger et al. (2018), it is difficult to speculate on the physical processes driving these changes. However, such an analysis is beyond the scope of the paper.

- *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.*

Reply : This point has not been well made in the previous manuscript, the text has been altered to: “These ensemble members have a relatively small decrease of PE as well as a relatively large ΔG andG compared to ensemble members with a similar change in PE (e.g. compare ensemble member 1 and 8).“ (p. 12, l. 19-21)

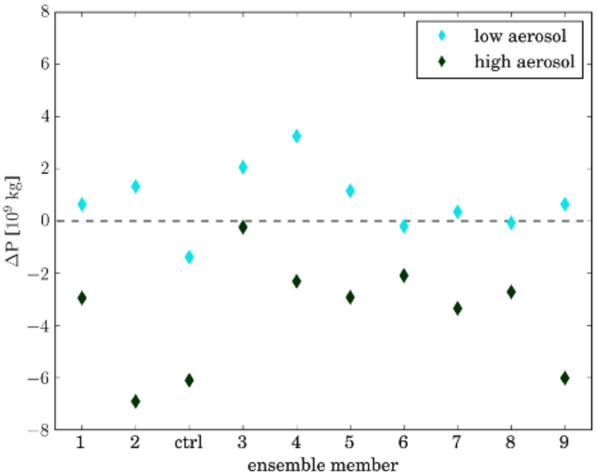

Figure 2. Difference in accumulated precipitation between the low and high aerosol scenario, respectively, and standard scenario.

-*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."*

Reply : If the numeric values are considered all have a slight increase in precipitation (Fig. 2). This figure has been included in the SI. However, changes in member 6 and 8 are very small, so we have changed the text to: "Accordingly, the precipitation response in these cases is either dominated by ΔG (control) or ΔG and ΔPE are of equal importance (member 6 and 8), as also indicated by their position in the shaded area in Fig. 12." (p. 12, l. 21-23)

- *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.*

Reply : The amplitude of the ΔPE and ΔG does not in alone indicate, which one is more important, since ΔPE operates on G and not on ΔG only. The part of the ΔG - ΔL parameter space, in which changes in ΔG dominate is highlighted by the shaded areas in new Fig. 13. The derivation for this is presented in Appendix A of Miltenberger et al. (2018). All points except those representing the control and ensemble members 6 and 8 fall clearly outside this area.
We have clarified the basis for our conclusion in the revised manuscript: "This response is in all ensemble members dominated by PE changes (points outside the shaded area)." (p. 12, l. 25/26)

- *Page 13, line 3: consider adding "(decrease)" after "precipitation response".*

Reply : The sentence has been altered to "If the aerosol concentration is enhanced beyond the standard scenario, the precipitation decreases in all ensemble members (points above the one-to-one line)." (p. 12, l. 24/25)

- *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.*

Reply : We think ensemble member 5 belongs in the exception list, as the precipitation change is almost symmetric for an increase or decrease of aerosol concentrations (Fig. 4). The text has been modified to make this clearer: "Only in ensemble member 3 does the mean precipitation rate not decrease further in the high aerosol scenario, while in ensemble members 4 and 5 the decrease between the standard and the high aerosol scenario is comparable to the decrease between the low and standard scenario." (p. 12, l. 30-31)
Thanks for pointing out that 75th percentile of mean accumulated precipitation is turning zero on some of the ensemble members for the high aerosol scenario. Since the distribution shown in these plots represents the temporal variability, this indicates an increased frequency of 10 min intervals without significant precipitation. The

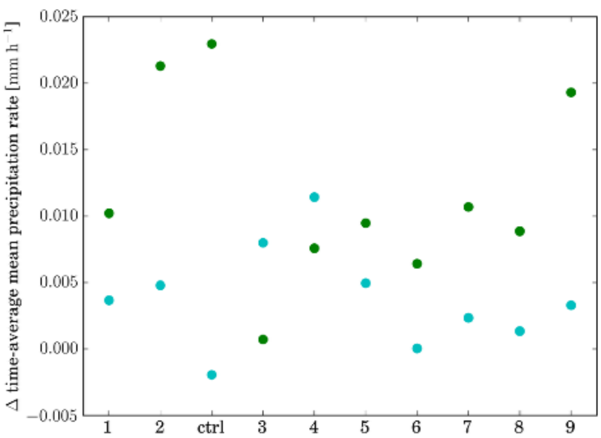

Figure 3. Change in the average mean precipitation rate (cyan: low - std aerosol scenario, green: std-high aerosol scenario).

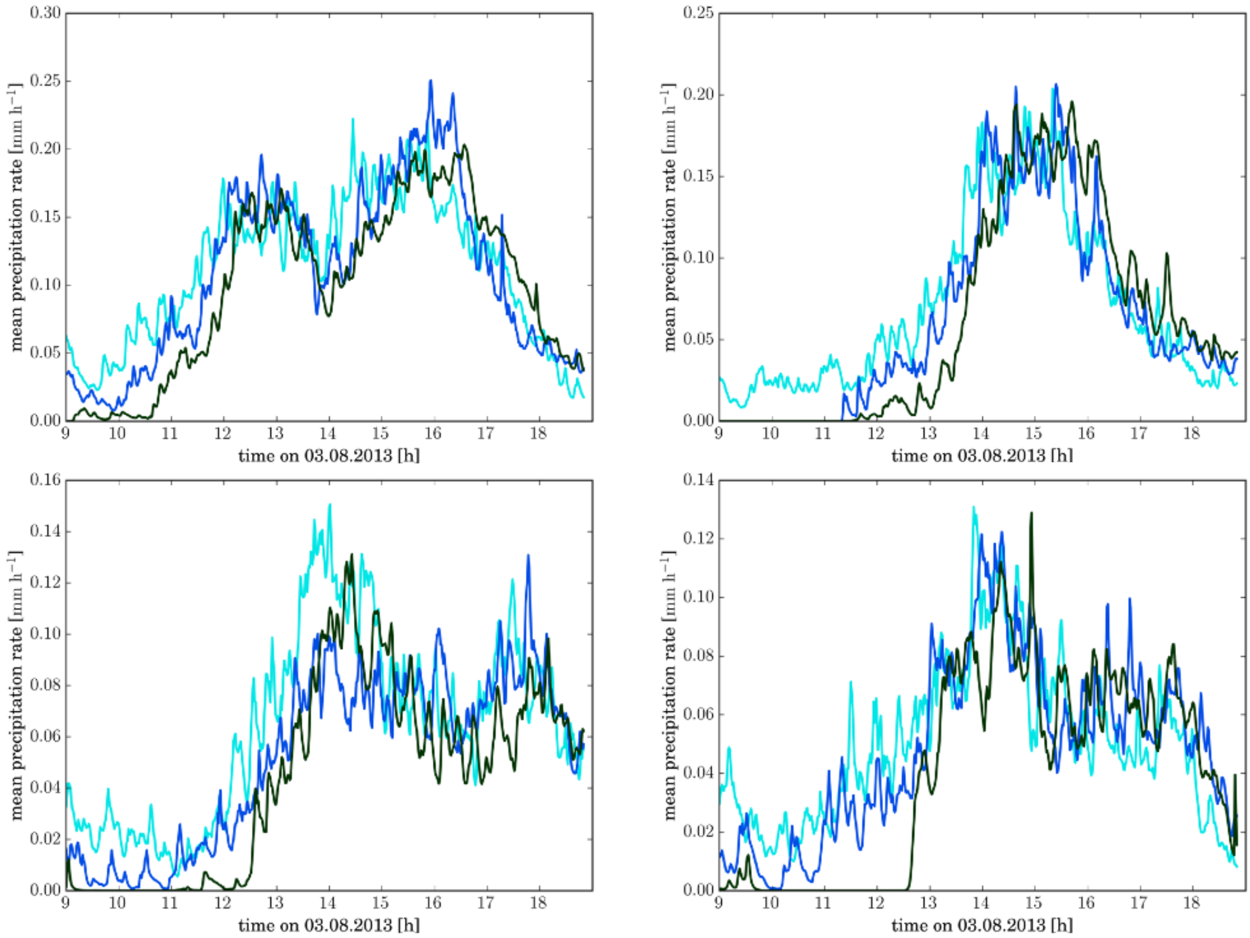

Figure 4. Time series of mean precipitation for ensemble member 1 (top left), 3 (top right), 4 (bottom left) and 8 (bottom right) for all three aerosol scenarios (cyan: low, blue: standard, green: high).

complete suppression of precipitation in members 3, 4 and 8 is due to much later onset of surface precipitation (Fig. 4). Note that higher percentiles in most runs are zero in most of the ensemble membersAs discussed for the control run in the first part of the presented study, clouds are mostly have lower cloud tops during the morning period and aerosol-induced changes have been found to be larger in this period. As this is extensively discussed in Miltenberger et al. (2018), we do not include this here. In particular, as splitting the response in different time periods would make the article more lengthy, while reviewers have already asked to shorten the manuscript.

- *Page 13, lines 12-14: I do not think "all percentiles up to and including the 75th 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.*

Reply : The reviewer is right. The text has been modified accordingly (p. 12. l. 33-34): "All percentiles up to and including the 75th percentile show an increase from the low to the high aerosol concentration, while the 99th percentiles are generally smallest (largest) for the high (standard) aerosol scenario."

- *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.*

Reply : In the first part of the study, we have shown that at least for simulations very similar to the control run enhanced freezing does not contribute to convective invigoration in the investigated clouds. We therefore refrain from citing the convective invigoration hypothesis based on enhanced freezing here.

- *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).*

Reply : The changes in cloud fraction (between 2 and 9 % depending on ensemble member) are very small compared to the changes in CDNC (factor ~7). Therefore the latter dominate the change in reflected shortwave radiation. The revised manuscript explains this in more detail (p. 13, l. 20-23):
"This change is consistent with the aerosol-induced change in CDNC and the cloud albedo effect (Twomey, 1977). The co-occurring decrease of cloud fraction under high aerosol conditions (between 2 and 9 % for a factor 10 aerosol change) counteracts the CDNC effect, but the cloud albedo effect dominates due to the large amplitude of the CDNC change (about a factor 7 for a factor 10 aerosol change)."

- *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".*

Reply : The aerosol direct effect is not included in the model simulations. We have added the suggested reference and altered the text according to the other suggests (see reply to previous comment, p. 13, l. 20-23).

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".*

Reply : Thank you for this suggestion. The text has been altered accordingly (p. 14, l. 7).

- *Page 14, lines 11-19: I miss the authors saying something regarding changes in WP and LWP in this paragraph.*

Reply : Change in WP and LWP are discussed on p. 15, l. 33ff (old manuscript) and p. 15, l. 28-33 (revised manuscript).

- *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?*

Reply : We only use the model output between 9 and 19 UTC, since this is the time period of main convective activity. Also, the first few hours of the simulation may be affected by model spin-up and later on some high-level cirrus clouds are advected into the domain, which influence the domain integrated cloud variables. We do not want to incorporate these, as they are not related to the convective clouds along the sea-breeze convergence and are mainly dominated by the boundary conditions for the innermost nest.
Fig. 12 actually shows the cloud properties for the 9 to 19 UTC time frame as all other plots. The caption has been corrected accordingly.

- *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?*

Reply : The list of figures has been adapted. All plots contain only data from 9 - 19 UTC. We state this more clearly now in section 2 model and data (p. X5 l. 15/16): "All simulations are run for 24 h. If not stated otherwise, the analysis presented in this paper focusses on the time period between 9 and 19 UTC, i.e. the time period of main convective activity."

- *Page 14, line 30: the variables are not plotted in a box-plot but in an error bar type plot.*

Reply : Yes, of course. Thank you for pointing this out.
The sentence has been corrected (p. 14, l. 20-24): “If instantaneous realisations of the different (domain-averaged) variables would be considered (box-plots on left side of the figures), the variability would be much larger than suggested by the domain mean plots (right side of the plots).“

- *Page 14 line 35 and page 15, line 1: could it be related to a longer cloud lifetime?*

Reply : We are not sure what the reviewer is referring to. Is it possible that this comment refers to p. 15, l. 35/p. 16, l. 1. If so, a longer lifetime could of course influence the frozen fraction as well, but it is not possible to investigate this effect in our simulations, we do prefer not to comment on any cloud lifetime changes.

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)*

Reply : The sentence has been modified to: “Changes in aerosol concentrations can potentially modify cloud field properties, e.g. cell number and size, cloud depth, cloud fraction, and the domain-wide condensate budget (condensate gain and loss, precipitation rate).“ (p. 16, l. 14)

- *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.*

Reply : The ensemble members are paired according to their meteorological initial conditions, i.e. are computed by assuming at the cloud properties for the three simulations with different aerosol but identical meteorological initial conditions are not independent. The statistical analysis of paired ensemble members tests the significance of aerosol-induced changes, if aerosol perturbations are considered for identical meteorological conditions but for a number of cases. Hence this reflects the approach taken by most previous modelling studies. We think the difference in the significance between the unpaired and paired ensemble is interesting and may offer an explanation as to why ACI is often found more pronounced in model, case-study based analysis as compared to observational studies.
We agree that the sample size is an issue in the presented work, which is aggregated by the pairing of ensemble members for statistical analysis. However, we decide to still use show this data, as it is consistent with the physical analysis of changes and therefore we think broad picture painted by the statistical analysis is not severely affected by sample size issues.
We modified the introductory text in section 5 (p. 10, l. 22-27):
“In this section, we compare the aerosol signal in the different meteorological ensemble members, i.e. the difference in realisations with different aerosol scenarios but identical aerosol initial and boundary conditions. Thereby we test the robustness of aerosol-induced changes to small perturbations in the meteorological conditions. To quantify the significance of aerosol-induced changes we use a two-sided t-test for ensemble members paired according to meteorological conditions (Table. 1). Using paired ensemble members reflects the interdependence of cloud properties in realisations with different aerosol but identical meteorological initial and boundary conditions.“
And in section 7 (p. 16, l. 20-22):

„First, the idealised situation where the meteorological initial conditions are identical for different aerosol perturbations is assessed by pairing ensemble members according to the meteorological initial conditions. This is equivalent to testing the statistical significance of the differences between realisations with different aerosol scenarios and identical meteorological initial and boundary conditions.

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?

Reply : Yes certainly. New text: "Consistent with previous studies, we find that the aerosol signals in variables closely related to aerosol concentrations, such as for example cloud droplet number concentrations, are easier to retrieve than for variables that are linked to aerosol concentrations by a series of complex processes, such as for example accumulated surface precipitation." (p. 18, l. 32-34)

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)*

Reply : We agree that the previous section titles were not ideal. They have been altered to:
Section 4: Cloud property variability in the meteorological ensemble (standard-aerosol scenario only)
Section 5: Cloud property changes between ensemble members with different aerosol and identical meteorological initial and boundary conditions
Section 6: Contribution of aerosol and meteorology perturbations to overall cloud property variability

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.*

Reply : Thank you for finding these. The references have been corrected.

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.*

Reply : Figures have been re-order to match their mentioning in the text. Note that some figures have been moved to the SI and the order they are mentioned in the text has also be somewhat changed.

- *Remove linking lines between ensemble members from the following figures: 4b, 7a and b, SI 8a, and SI 9.*

Reply : Done.

- *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.*

Reply : We refrain from changing the colour palette, as it is identical to the one used in the already published first part of the manuscript. However, to make it more easy to distinguish the differences between aerosol scenario, we have supplied difference plots (low / high scenario - std scenario).

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?*

    Reply : The selection of ensemble members for downscaling is described in section 2 (p. 4, l. 27-32). We added a reference to this description in the caption.

  - *Could you change the x-axis ticks in a way that both graphs (a and b) have the same.*

    Reply : Done.

- *Fig. 3: please add the data information in the last column "mean" in c and d, otherwise remove it from the graphs.*

Reply : Columns have been removed.

- *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.*

Reply : Figure 4 b and c have been removed from the manuscript, as they are not discussed in the revised text. The caption has been corrected.

- *Fig. 5: since it does not show big differences between ensemble members I would suggest moving it to the SI.*

Reply : We have moved Fig. 5 b to the SI. Fig. 5a remains in the manuscript, as it is the only variable with a clear separation from between aerosol scenarios. This is itself an important point of the manuscript. Also, we think this plots helps the reader to understand the following plots of this type, which are more messy due to the larger meteorology-induced variability.

- *Fig. 7: as said before, I recommend changing the color palette of the graphic or enlarging the figure.*

Reply : We refrain from changing the colour palette, as it is identical to the one used in the already published first part of the manuscript. However, to make it more easy to distinguish the differences between aerosol scenario, we have supplied difference plots (low / high scenario - std scenario).

- *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".*

Reply : Legend has been modified and the text in the caption has been corrected.

- *Fig.12: this is a really interesting and helping figure. 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.*

Reply : Thank you for the positive comments on this figure. The additional information has been included in the caption.

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.*

Reply : This figure is now in the main paper (new Fig. 2).

- *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.*

Reply : The suggested improvements have been made.

- *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".*

Reply : Done.

- *SI Fig. 6: why ensemble 4 with low aerosol look so different from the others? Is there any apparent reason?*

Reply : Thanks for spotting this. There was an error in the plotting routine affecting in particular this specific ensemble member. The plot has been corrected.

- *SI Fig. 7: CAPE for cntl simulation is missing. And please, either add values for the last column or remove "mean" from the graphic.*

Reply : Thanks for spotting this. The plot has been corrected.

- *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) [...]".*

Reply : The caption as well as the plot itself have been improved and the inconsistent/unclear notation have been cleaned up.

Tables:

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

Reply : see reply to comment on Page 16, lines 22-34

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".

Reply : Thank you very much for pointing these out! We corrected all issues as suggested.