Reply to comments from reviewer #1

General comments:

1. The manuscript contains many typos and errors that the team of authors should really have addressed together through proof-reading before submission for review. I have listed these in my specific comments.

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

2. I believe there are some inconsistencies in the references to ensemble members. I have noted these in my comments on Section 4.2.

Reply : We are very sorry that there were inconsistencies in ensemble member references in the original manuscript. We have very carefully checked they are ok in the new version.

*3. **Figure ordering:** I found the order of figures somewhat counterintuitive and hard to follow. I had to lay my printed copy of the figures and the supplementary figures out next to each other in order to follow the arguments made in the text. Whilst I appreciate that there are already a lot of figures in the paper, I would suggest moving figure S8 to the main paper if possible. Further, the figure ordering in the main paper is not logical. I appreciate that it is difficult to optimally order figures when investigating concurrent sensitivities, but I would recommend placing the order of current Figures 7,6,5 as such. On P10 L4, the reference to Fig 7a, I had to jump ahead several Figures in order to see this. On P10 L34 I had to jump back again to Fig. 5, which is referenced for the first time after Fig.s 7 and 6 are discussed in detail. Why put Fig 5 in its current location? You would make it much easier for the reader if it appeared after 6 and 7.*
Page 11, 1st paragraph: again, you refer to Fig 9 and then immediately after to Fig 11, and Fig 10 is not even mentioned until page 13.

Reply : Thank you for your suggestions. We have re-ordered the figures so that they appear in the sequence they are mentioned in the text. Also, we changed the partitioning of figures between the main paper and the SI to have only the most crucial figures in the main paper following your suggestions and those of reviewer 2. For example, former SI Fig 8a is moved to the main text (new Fig. 8 b).

4. General comments on figures:
Many of the figures have lines joining the points representing each ensemble member. This is misleading, as the abscissa on these figures show ensemble members (a discrete dataset) and not continuous data. I recommend removing these lines.

Reply : We have removed these lines from all figures as suggested.

Figure SI 9 – I tried very hard to understand this Figure, but it many things in it don't make sense to me. See notes under my comments referring to individual figures.

Reply : We are were sorry for the poor description of this figure. The caption and labelling has been improved, so it should be comprehendible now.

5. Section 2:
Stochastic physics – are stochastic physics used in the regional model as well as the global model? Are stochastic physics used in the full set of ensemble runs? (Are you using stochastic physics as well as perturbed initial conditions?) What kind of stochastic physics are used? Which schemes and which parameters? Etc. This needs a little more explanation if you are discussing a study which aims to capture meteorological variability.

Reply : Stochastic physics are only used for the re-run of the global operational ensemble and not in the regional ensemble simulations. This has been clarified in the model set-up description (section 2: p. 4, l. 23-25 & p. 5, l. 9-11). Since stochastic physics are only used to derive the perturbed initial and boundary conditions, they do not influence the actually used ensemble data and we therefore refrain from a more detailed description of the stochastic physics for brevity (reference is provided for interested reader!).

6. Section 3.1:

P6 L13 – Was the model microphysics output passed offline through the same radar algorithm as the Radarnet data? If not, could part of the difference be because the online UM dBZ calculation is different from the dBZ calculation in the Radarnet algorithm?

Reply : No. The radar reflectivity is computed from the modelled hydrometeor properties online, i.e. within the model. For the modelled surface precipitation, we are using the direct model output, i.e. surface precipitation is not diagnosed from the modelled reflectivity fields. The model assumes only Rayleigh scattering and does assumes particles have a single phase, e.g. partially melted hydrometeors do not exist in the modelling world. Conversely, the radar algorithm does not account for sub-cloud evaporation of rain, which has been shown in previous work to affect retrieved surface precipitation rates. Therefore the differences, could be due not only to a deficiency in the model microphysics, but also to issues with the radar-derived surface precipitation or radar reflectivity calculation in the model.

We added the following to clarify this point (p. 6, l. 17-20):

"While the model derived surface precipitation is the sedimentation flux at the surface, the radar derived surface precipitation is computed from the low-level radar reflectivity according to Harrison et al. (2009). Accordingly, the modelled and radar-derived surface precipitation products involve different assumptions. Fore For example, sub-cloud evaporation is not taken into account in the retrieval of surface precipitation rates from observational data."

7. Section 4.1:

P9 L5-6: "These members have a higher cloud fraction" – do you know why this is the case for these members?

Reply : As discussed in section 4.2 these members have a higher boundary layer moisture convergence, which most likely explains the higher cloud fraction. The text has been modified as follows (p. 8, l. 27-30):

"Only in ensemble members 1 and 2 the temperature difference remains smaller than 1.5 K (SI Fig. 7 c). These members have a higher cloud fraction in the morning (not shown), which is likely related to a relatively large large-scale moisture convergence. The higher cloud fraction reduces radiative heating of the land surface explaining the smaller peak land-sea temperature difference."

P9 L17-18: Is this also related to the cloudiness (higher cloud fraction in these members)?

Reply : The smaller wind speed and more southerly wind direction should affect the propagation of the sea-breeze front inland and therefore changes the low-level convergence. However, this relation is not very strict, as e.g. ensemble member 5 to 8 also have similarly high low-level convergence. The latter do not have a particularly large cloud fraction. Therefore, we think the cloud fraction variability is dominated by the differences in large-scale convergence. Although of course differences in sea-breeze convergence strength impact cloud fraction, but these differences are much smaller than those in large-scale convergence.

8. Section 4.2:

P10 L11-12: Members 4 and 7 have a particularly large surface sensible heat flux – can you explain why? It doesn't seem like they stand out in terms of cloudiness (Fig. 6).

Reply : The high sensible heat flux for these members is a combination of high surface temperature and high surface wind speed (Fig. 1). The surface temperature in these ensemble members is already in the upper range at 9 UTC, i.e. before the onset of significant radiative heating. Also the relatively large surface wind-speed is consistent with the upstream profiles. Therefore the large sensible heat fluxes is likely related to changes in the initial and boundary conditions and not so much related to differences in cloudiness. Since the sensible heat flux is not discussed anymore in the revised version of the manuscript, there are no alterations regarding this issue in the text.

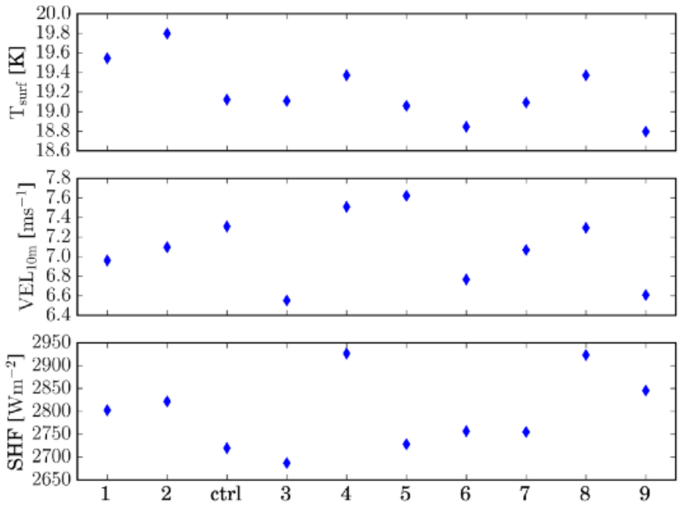

Figure 1. Mean surface temperature (top), 10 m wind velocity (middle) and sensible heat flux (bottom) for the different ensemble members.

P10 L22: largest (smallest) values for ensemble members 8(4) – I find it hard to see by eye on this Figure, but doesn't this actually apply to members 9(5) not 8(4)?

Reply : We are sorry for the incorrect referencing of ensemble members in the text. The reviewer is of course right and we have corrected the text (p. 9, l. 24; checking the numerical values indicates ensemble number 2 has the highest mean cloud top height very closely followed by ensemble member 9).

P10 L22-23: Really? I find this hard to see (Fig 6d vs Fig SI 7c)

Reply : The overall tendency in the mean equilibrium level pressure corresponds to the overall tendency in the mean cloud top height: For example, ensemble 5 with the smallest mean cloud top height has the largest equilibrium level pressure and ensemble 2/9 with the largest mean cloud top height have the smallest equilibrium level pressure. Modified text (p. 9, l. 23-26):
"Mean cloud top height varies by about 750 m between ensemble members (Fig.8) with largest (smallest) values for ensemble member \2 and 9 (5). Variations in mean cloud top height are in general consistent with those of the equilibrium level pressure (SI Fig. 10 c): For example, the equilibrium level pressure in ensemble member 5 is largest, while members 2 and 9 have the smallest equilibrium level pressure."

P10 L25-26: ensemble members 1,2,5,8 have a relatively large fraction of deep clouds – I don't see this. What about e.g. member 6 (Fig SI 8a)?

Reply : The reviewer is correct ensemble member 6 has the largest deep cloud fraction. This sentence is not part of the revised manuscript anymore, because we tried to shorten the manuscript as requested by reviewer 2.

P10 L29: changes in condensate generation, i.e. air mass lifting – have you looked at the dynamical convergence to see if this is the case?

Reply : We are not sure what the reviewer mean with "dynamical convergence". The large-scale boundary layer convergence and the low-level convergence have been discussed in section 4.1 and 4.2. Their variability corresponds in general very well with the variability in G.

P10 L32:

- *"member 8 has a relatively large PE" – I disagree with this. Many others have a greater PE, e.g. 1, ctrl, 6 (Fig. 7)*
- *"and the largest fraction of clouds with tops above 4.3 km" – I also disagree with this. The largest fraction of clouds with tops above 4.3 km is seen in member 6 (Fig SI 8a). I think in this sentence perhaps the authors mean to refer to member #6, not member #8? Then I agree with the statements made in the sentence.*

Reply : This statement refers indeed to ensemble member 6 and has been corrected accordingly (p. 9, l. 34).

P11 L1-4: this final section is not particularly well-explained and no relevance is given. Can you say anything about the processes and impact or importance?

Reply : We have expanded this section as follows (p. 10, l. 8-15):

“Mean reflected shortwave radiation ranges from 130 W m^{-2} to 155 W m^{-2} (Fig. 11 a). The reflected shortwave is influenced by the cloud cover and the cloud droplet number concentrations. The largest (smallest) outgoing shortwave flux is predicted for the ensemble members with the largest (smallest) cloud fraction, i.e. ensemble 1 (8). Since the CDNC variability is small (Fig. 5), the variations in cloud fraction between ensemble members is dominating the variability of outgoing shortwave radiation. Changes in outgoing longwave radiation are on the order of 3 W m^{-2} (Fig. 11 b). The outgoing longwave radiation is influenced by the surface temperature, the cloud top height and the cloud fraction. 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).“

P11 L3-4: Largest (smallest) values occur for ensemble members 2(7) – I only just agree with this. Do you mean member 1 not member 2 for the largest SW radiation values and largest CF?

Reply : Yes, this is member 1. We changed the text accordingly (p. 10, l. 10).

Specific comments and typos:

P1 L16: “consider” -> “considered”
P2 L3: “climate system”
P2 L4: “The main issues...”
P2 L5: “... on model grid scales several orders of magnitude larger, and the...” P2 L6: “In the last few decades” / “In recent decades”
P2 L6-7: “the modification of cloud properties has been studied in particular”
P2 L7-8: “... and the relation between particle number concentrations and radiation” – this whole sentence feels quite clumsy.
P2 L13: “necessitated by” -> “necessary because of”
P2 L15: “changes to simulated for individual clouds” – simulated what?

Reply : Thank you for pointing these out. All issues have been fixed as suggested.

P2 L17: You could also include a reference here to the 2012 paper by Seoung Soo Lee where placing an aerosol perturbation in the mesoscale domain of a simulation led to intensification of convection within an MCS but suppressed precipitation in the larger-scale domain. (Reference provided at the end of this set of comments)

Reply : We have added this reference (p. 2, l. 14-19):
"These interactions can at least partly compensate the large changes simulated for individual clouds (e.g. Lee, 2012; Seifert et al. 2012). In a case-study of tropical deep convection, Lee (2012) found that locally invigorated convection in polluted conditions induces stronger large-scale subsidence resulting in an overall suppression of precipitation on a cloud-system scale. Seifert et al. (2012) demonstrated with simulations extending over three summer seasons that aerosol perturbations can produce large local changes in precipitation, while not significantly changing the mean precipitation.“

P2 L29: Southern Great Plains
P3 L6: What do the authors mean by “cloud-induced changes to large-scale forcing”? Does this refer to large-scale circulation and / or synoptic forcing, or something else?
P3 L8: “has also” -> “also has”
P3 L10: relay -> rely
P3 L11: rises -> raises
P3 L12: datasets
P3 L12: has recently been demonstrated
P3 L14: Southern Great Plains
P3 L23: in future forecasting systems
P3 L28: 30th
P3 L34: baseline

P4 L2: a precipitation -> precipitation
P4 L4: the observed aerosol

Reply : Thank you for pointing these out. All issues have been fixed as suggested.

P4 L7: convective invigoration hypothesis needs a description and / or citation

Reply : We added a reference to the Rosenfeld et al. (2008) paper (p. 4, l. 7)

P4 L10: investigate whether the
P4 L17: Section number missing. (should this be Section 5?)
P4 L23-24: The way this sentence is written doesn't quite make sense.
P4 L25: do you mean "9 members are selected from", not "selected for"?
P4 L32: mesoscale
P5 L3-4: repetition of "current study"; you could just say "to our main conclusions".
P5 L6: In addition to (delete comma)
P5 L11: h a grid -> horizontal grid ?
P5 L32: datasets
P6 L3: peninsula (remove capital P)
P6 L16: have also reported
P6 L15: underdispersive over longer
P6 L31: smaller if (delete comma)
P7 L21, 23, 26: dewpoint
P7 L31: ensemble members
P9 L20: similar, with a well-mixed
P9 L25: temperate -> temperature
P10 L3,8: mesoscale
P10 L4: "G is very well correlated" – have you actually correlated this (or can you)?
P10 L4-5: Figures 4a and 7a are difficult to compare as they are on different pages
P10 L14: convergence
P10 L18: areal
P10 L26: an about 20% -> about a 20%
P10 L 10-31: refer to Fig SI 8a
P11 L3: largest (smallest) cloud fraction – please refer to Fig 6c.
P11 L4: distribution of cloud top heights (Fig. 11b) – you also need to refer to the Fig. showing CTH.
P 11 L8: "low", "high": open quotations are the wrong way wrong (LaTeX `` not "?)
P 11 L9: "which have a factor of 10 lower and higher aerosol number concentrations, respectively, than the standard profile"
P11 L10: altitudes
P11 L10: The mean and effective radius – mean what? Mean radius and effective radius? P11 L12: the first section of this study?
P11 L13: ensemble members

Reply : Thank you for pointing these out. All issues have been fixed as suggested and sentences have been reformulated where necessary.

P11 L17; Figure 5 should be moved, as discussed in the major comments

Reply : The figures have been reordered (s. reply to general comment 3).

P11 L21: "suggest only minor changes in the cloud-base vertical velocity distribution" – can you plot this distribution? Doesn't this contradict the previous statement made about convergence?

Reply : The cloud-base vertical velocity distribution is shown in Fig. 2 (new SI Fig. 9b). This confirms our hypothesis of small changes in the cloud-base vertical velocity distribution. While these changes are small, the variability in average cloud-base updraft between ensemble members is still larger than the difference in average boundary-layer top moisture content, which is

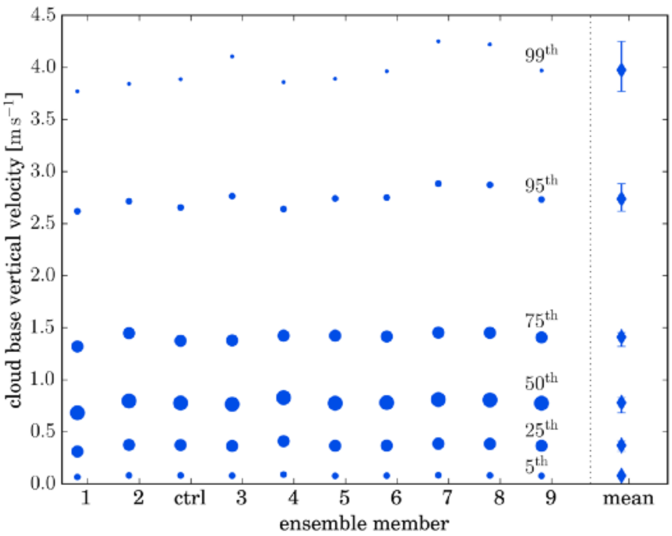

Figure 2. Cloud base vertical velocity distribution for the simulations with the standard aerosol profile considering all grid points at cloud base and a positive vertical velocity for the time period 09 - 19 UTC.

all that we claimed earlier. Note that the discussion of the latter has been removed from the manuscript to meet demands for shortening the text from reviewer 2.

P11 L24: "the number of cells decreases with increasing background aerosol concentration, but the cell area increases" – this is interesting! Can you explain why this happens?

Reply : We can only speculate about the physical reason for this behaviour, which has been done in the first part of the study. We added a brief summary of the hypothesis the revised version (p. 11, l. 8-11): "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. Also, energetic constraints potentially limit an increase in overall lifting and cloud fraction. "

P11 L30: cloud top height increases
P12 L2: "ensemble members 1,2,7,8, and 9" (missing space between "and9")

Reply : Thank you for pointing these out. All issues have been fixed as suggested.

P12 L2: "ensemble members 1,2,7,8, and 9" – this is also true for member 4

Reply : Sorry again for the confusion with the ensemble member numbers. This list should read 1, 2, 4, 7 and 9 and has been corrected accordingly (p. 11, l. 18-19).

P12 L3: "does not increase further (members 1 and 2)" – doesn't member 2 increase?

Reply : The median mean cloud top height does, while the time-average mean cloud top height does not increase from the standard to the high aerosol run. We have clarified this in the text (p. 11, l. 18-19).

P12 L5: higher than 4.3 km shows only
P12 L7: aerosol scenario is likely (remove comma)
P12 L8: "maximum" (open quotation incorrect way round)
P12 L12: "only a small change"
P12 L14: "-4 – 2.5%" - this notation is confusing. Do you mean -4% to -2.5%, or -4% to +2.5%?

Reply : Thank you for pointing these out. All issues have been fixed as suggested.

P12 L18: I do not understand Figure SI9.

Reply : We apologise for the poor presentation of this figure. We have improved the legend and axis labels in the figure as well as the caption (new SI Fig. 14 d).

P12 L21: Can you plot delta G and delta L instead of G, L?

Reply : We could plot ΔG and ΔL instead of G and L, which would make it easier to discern aerosol-induced changes. However, this would make the plot less useful to understand the meteorological variability. ΔG and ΔL were/are shown in previous Fig. 10 (new Fig. 13). We add a plot showing ΔG and ΔL in the format of previous Fig. 7b in the SI (SI Fig. 14 b and 15 b).

P12 L29: "seized" -> "sized"
P 12 L32: simulations in the standard
Reply : Thank you for pointing these out. All issues have been fixed as suggested.

P12 L34: in Figure 7c in my printed copy, member 7 also looks like it has no change

Reply : The change in precipitation for ensemble member 7 is very small. The numeric values indicate that accumulated precipitation slightly decreases when aerosol concentrations are increased from the low to the standard scenario. This is consistent with the position of ensemble member 7 off the one-to-one line in Fig. 13. In contrast, ensemble members 6 and 8 fall almost exactly on the one-to-one line in Fig. 13.

P12 L34: "The latter have a relatively small decrease of PE and comparatively large delta G" – but member 1 also has a decrease in PE and delta G, but a decrease in precip in standard vs low scenarios, and is outside the shading in Figure 10.

Reply : The four ensemble members with the smallest change in PE (from low to standard) are the control, member 6, member 1 and member 8, in this sequence. ΔG for ensemble member 1 is the the second smallest (joint position with ensemble member 3). In contrast, ensemble member 8, which has a similar change in PE, has the third largest ΔG. Also note that changes in PE operate on G, which is much larger in ensemble member 1 than 8, and not on ΔG. Hence, a relatively small change in PE is significant for ensemble member 1, while ΔG still dominates for ensemble member 8 despite a similar absolute change in PE. The text has been modified to make this clearer (p. 12, l. 18-21):
"Exceptions are the control simulation with a small increase in precipitation and ensemble members 6 and 8 with no change in accumulated surface precipitation. These ensemble members have a relatively small decrease of PE as well as a relatively large ΔG and G compared to ensemble members with a similar change in PE (e.g. compare ensemble member 1 and 8)."

P13 L1: "comparatively large delta G" – this is hard to see from Fig 7a. Can you plot delta G and delta L instead of G and L?

Reply : ΔG and ΔL are shown in previous Fig. 10 (new Fig. 13). We also added a figure showing ΔG and ΔL in the format of previous Fig. 7 to the SI. See also reply to comment on P12 L21.

P13 L10: "Exceptions are ensemble members 3,4 and 5..." – you should point out that the behavior in each of these members is different from each other. For the (a) low to standard and (b) standard to high aerosol scenarios, member 3 has an (a) decrease and (b) increase, member 4 has an (a) decrease and (b) decrease, and member 5 has an (a) increase and (b) decrease.

Reply : The text has been modified to: "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. 30-32)

P14 L11: two sections
P14 L20: datasets
P14 L29: realisations
P15 L4: distribution in different cloud top height classes
P 15 L11: Precipitation formation is known...
P1 L21: accordingly displays
P15 L29: very similarly to

Reply : Thank you for pointing these out. All issues have been fixed as suggested.

P15 L34-35: "the liquid water path (...) shows little sensitivity to the aerosol scenario" – actually, there is a decrease in LWP (Fig 9b) which is not that much weaker than the increase in CWP (Fig 9a) – this indicates even more strongly than you currently state that the FWP must increase!

Reply : The text has been modified to read: “However, the liquid waterpath (condensate in the cloud and rain category) shows relatively little sensitivity in its median value, while the mean liquid water path generally decreases with increasing aerosol concentrations.“ (p. 15, l. 29-31)

P16 L9: mesoscale
P17 L2: “perfect” (open quotations incorrect way round)
P17 L3: only slightly different
P18 L4: exact number is
P18 L7: several 100 ensemble members -> several hundreds of ensemble members

Reply : Thank you for pointing these out. All issues have been fixed as suggested.

P18 L11: Why is low-high so different from low-standard and standard-high?

Reply : This is mainly because the aerosol perturbation is a factor 10 larger, if the low and high aerosol scenario are considered, than in either low to standard or standard to high scenario. The larger amplitude in the aerosol perturbations results in larger aerosol-induced changes. However, the meteorological variability is in all combinations the same. Accordingly, the “signal-to-noise“ ratio is larger in the low-high combination than any other. This has been included in the discussion:
“The number of samples required depends on the amplitude of the aerosol perturbation (low and high aerosol scenario versus low/high and standard scenario) as well as the location in the aerosol space (different for increase or decrease relative to the standard aerosol scenario).“ (p. 18, l.8-10)

P18 L13: Accumulated precip stands out here – are you able to explaim why? (It’s the only one that needs fewer observations for an increase of number concentration above the standard scenario).

Reply : For all considered variables except P the aerosol-induced changes are smaller (or identical for CDNC) for an increase of aerosol concentrations above the standard aerosol scenario than for decreasing aerosol concentrations. In part 1, it is hypothesised that thermodynamical constraints lead to the saturation of the aerosol effect for high aerosol conditions. In contrast, for accumulated precipitation the aerosol-induced change increases with increasing aerosol concentrations. This is primarily due to PE changes. In part 1, we hypothesis that this change in PE is due to a larger export of condensate into the stratiform region with less active microphysics prompted by the thermodynamic limitations on cloud top height.
The text has been modified to (p. 18, l. 10-15): “In general, more observations are required for an increase of aerosol number concentrations above the standard scenario, which is related to the thermodynamic constraints on aerosol-induced changes in the considered case discussed in Miltenberger et al. (2018). The only exception is accumulated surface precipitation, for which fewer observations are required for an increase above the standard scenario. This reflects the larger aerosol-induced signal in accumulated precipitation for increased compared to decreased aerosol concentrations.“

P18 L14: “the thermodynamic constraints on aerosol-induces changes...” – constraints for this particular case, or general constraints?

Reply : While there are very likely thermodynamic constraints on aerosol-induced changes in many situations, the conclusions are of course only valid for the investigated case. We have modified the sentence to reflect this (p. 18, l. 12).

P18 L15: allows us to put the aerosol-induced changes / allows the aerosol-induced changes to be put

Reply : Thank you for pointing this out. Fixed as suggested.

Comments on individual figures:
Fig 4: Don’t join these points with lines

Reply : Done.

Fig. 5:

- *Can you comment on why these are so invariant?*

Reply : As discussed on p. 9, l. 15-17 of the manuscript the CDNC is not very variable across the ensemble, because the aerosol concentrations are identical in all members and the cloud base vertical velocity distribution in the different ensemble members is not strongly differing (see also Fig. 2 in this reply).

- *What do the colours represent?*

Reply : Added legend.

- *I think Figure 5 should appear AFTER Figure 6 (given the ordering of discussion in the manuscript)*

Reply : see reply to general comment 3.

Fig. 6: "cloud fraction is the fraction of the domain for which" (add "the", remove comma)

Reply : Done.

Fig 7:

- *Fig. 7a would be clearer if you plotted delta G and delta L instead of absolute values*

Reply : We could plot ΔG and ΔL instead of G and L, which would make it easier to discern aerosol-induced changes. However, this would make the plot less useful to understand the meteorological variability. ΔG and ΔL were/are shown in previous Fig. 10 (new Fig. 13). We add a plot showing ΔG and ΔL in the format of previous Fig. 7b in the SI (SI Fig. 14b and 15b).

- *Don't join these points with lines*
- *"the last column in each panel"*
- *Caption: "means" – do you mean ensemble mean, or ensemble means?*

Reply : Done. The caption has been modified to clarify the raised point.

Fig 10:

- *What do the open versus the filled circles represent?*
- *Legend: should "high processing" be "high aerosol"?*
- *Caption: "black symbols" – I can't see any black symbols on Fig 10*
- *Caption: downward / upward triangles: I can't see any of these on Fig. 10*

Reply : Done. The caption has been modified to clarify the raised points.

Fig. SI 2: "The distributions consider cloudy..."
Fig. SI 3: caption L3: "observational data"

Reply : Done.

Fig. SI 4: I find the dark blue lines hard to distinguish in my printed copy

Reply : The colour scheme has been adapted.

Fig. SI 7: 7b: where are the points for ctrl data on the CIN and CAPE Figures?

Reply : Thanks for spotting this. The data was actually missing from the plot. It has been included in the revised version.

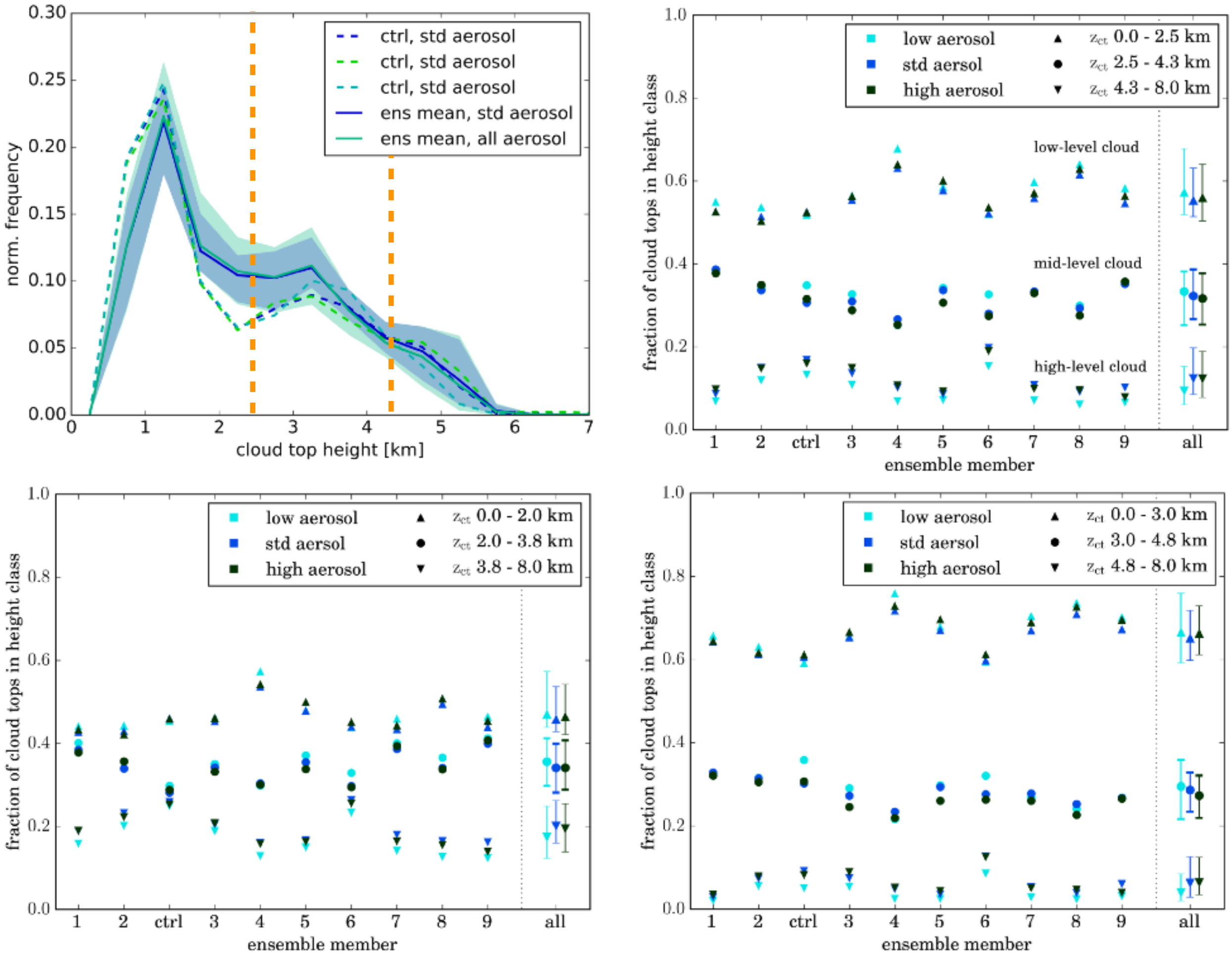

Figure 3. Cloud top height distribution (a) and fractions of cloud top in specific altitude ranges (b-d). The thresholds for these ranges are modified by ±500 m in (c) and (d) compared to those used in the manuscript and (b).

Fig SI 8:

- *How sensitive is this figure to how you choose to define low / medium / deep cloud tops?*

Reply : The altitude bands were chosen to reflect cloud top ranges with a different response to aerosol perturbations (Fig. 3 a). The overall behaviour of the cloud top height fractions does not differ significantly for variations by ± 500 m in the thresholds (Fig. 3 b-d), although the absolute fraction values of course do.

- *It would be worth placing labels on the Figure with "low", "med", "deep" near the relevant set of points, just to make it clearer for the reader.*
- *Again, I don't think these points should be joined with lines.*

Reply : Done.

Fig SI 9:

- *I found it almost impossible to understand this Figure. Are condensation and deposition shown separately, or combined? What are the symbols? What do IG and IL refer to? Also, as mentioned in the major comments, I don't think that the points representing the ensemble members should be joined with lines. This is not a continuous dataset. (My printed copy also has different linestyles in the Figure, which are not explained, but I suggest to remove the lines entirely).*

Reply : We have improved the legend, axis labels and caption.

- *Caption: ... and deposition rates*

Reply : Done.

Comments on tables:

Table 1:

- *Caption: variable (columns)... aerosol scenarios (rows) – aren't these the other way round? (Don't the rows show the variables and the columns the aerosol scenario?)*
- *What do the bold numbers in the table mean?*

Reply : The caption has been modified accordingly.

Table 2:

Why does the low-high scenario need so few samples compared to low-standard or standard-high?

Reply : see reply to specific comment on P18 L11