First of all, we would like to appreciate the reviewer2's comments and suggestions. In response to the reviewer comments, we have made relevant revisions in the manuscript. Listed below are our answers and the changes made to the manuscript according to the questions and suggestions given by the reviewer. Each comment of the reviewer (colored black) is listed and followed by our responses (colored blue).

Interactive comment on "Dependence of aerosol-precipitation interactions on humidity in a multiplecloud system" by S. S. Lee

Anonymous Referee #2

Received and published: 11 January 2011

General Comments:

This manuscript investigates the impacts of aerosols on deep convective precipitation under varying environmental humidity. Understanding the impacts of aerosol forcing on deep convective is an important area of study, and enhancing our understanding of the role of humidity is necessary. However, there are a number of issues that need addressing before this paper is suitable for publication including aspects such as grid dimensionality and resolution, aerosol initialization, the manner in which the humidity is varied and its CAPE implications, various other assumptions, and certain explanations. Also, very little reference is made to numerous previous papers on the impacts of aerosol forcing on mixed-phase, deep convective systems, particularly in the introduction and conclusion. These sections should be written in context of what has previously been achieved in the field and how this work either supports, challenges or adds to these findings. More specific comments are included below:

Major Comments:

• There are numerous grammatical errors within the manuscript. While typically minor in nature, they will need to be addressed before the manuscript is suitable for publication.

Grammatical errors are corrected.

• Pg 25289: The term "gustiness" can take on a variety of different meanings when referring to storm dynamics. Also, increased gustiness does not automatically imply enhanced low-level convergence as suggested here in the manuscript. In fact, increases in gustiness can at times be associated with a decrease in convergence. This term, which is used throughout the paper, tends be to misleading and needs to be clearly defined or alternatively replaced with a different more appropriate term.

The term "gustiness" is removed and only "low-level convergence" is used.

Further down this page, the authors state that "more intensified gustiness generates more secondary clouds" which is once again misleading as intensified surface gustiness does not necessarily generate secondary convection. Instances throughout the paper need to be better described or clarified.

The increase in secondary clouds due to increase in low-level convergence in Lee et al. (2008a,b), Lee et al. (2009), and Lee et al. (2010) is about the increase in the total number of secondary clouds over the horizontal domain with increasing domain-averaged low-level convergence. Hence, despite the possibility that the intensification of individual convergence does not necessarily lead to more secondary clouds, Lee et al. (2008a,b), Lee et al. (2009), Lee et al. (2010) and this study demonstrated that the overall total number of secondary clouds increased with the increase in the domain-averaged intensity of low-level convergence. Hence, "domain-averaged" is included in the text describing the relation between low-level convergence and secondary clouds when needed to make the text clear; when the description of the relation uses Tables and Figures of domain-averaged or cumulative values, we believe it is not necessary to put "domain-averaged" in the text, since the readers would implicitly understand that the description is based on averaged values.

• Pg 25290: The introduction, and later the conclusion, is rather devoid of references, giving very little mention of any of the previous work reported in the literature specific to the impacts of aerosols on mixed-phase, deep convective systems. While the work of Xue and Feingold (2006) and Jiang (2006) are described (which is appropriate given their entrainment investigations), their work, as the author correctly states, is particular to warm clouds. The referral to mixed phase, deep convective systems, needs to be substantially improved in order to place the current work within the context of what has already been achieved. Previous research such as Andreae et al (2004), Givati and Rosenfeld (2004), Khain et al (2005), Koren et al (2005), Ekman et al (2006), van den Heever et al (2006), Seifert and Beheng (2006), Lynn et al (2007), van den Heever and Cotton (2007), Tao et al (2007) and then more recently Rosenfeld et al (2008), Lerach et al (2008), Khain and Lynn (2009), Ntelekos et al (2009), Storer et al (2010) and van den Heever et al (2010) (found this in the JAS new releases online). All of these papers have looked at different aspects of aerosol forcing on deep convection. Khain et al (2005) and van den Heever and Cotton (2007) in particular examine the possible precipitation enhancement due to subsequent storm dynamics and secondary convection (and should also be referred to again in the conclusion), and Storer et al (2010) discuss CAPE and cold pool impacts, all of which are directly relevant to this work. Also, the statement in the conclusion that "Hence, according to this study, the direct translation of the findings from studies for an isolated cloud to multiple-cloud systems and thus climate can be misleading" would appear to support similar previous conclusions regarding cloud type from Seifert and Beheng (2006), and for a range of multiple cloud types as discussed by Van den Heever et al (2010). These conclusions should be stated within the context of this work.

In the introduction and conclusion, appropriate references are added. Also, the following is added to Conclusion and summary:

(LL650-656 in p22)

This is supported by Seifert and Beheng (2006) who showed different responses of precipitation to aerosol between single-cloud systems and multiple-cloud systems. Also, it is worth pointing out that Lee and Feingold (2010) and van den Heever et al. (2010) showed that different types of clouds even in convective systems with multiple clouds responded differently to aerosol. This and the findings from this study indicate that it is difficult to establish a general rule of relationships between aerosol and clouds for parameterizations in climate models.

• Pg 25293: This reviewer has serious concerns over the grid spacing used, both in the vertical and in the horizontal. A grid spacing of 500m in the horizontal is far from being sufficient to represent the horizontal vorticity at cloud edge that occurs as a result of the aerosol-driven buoyancy gradients. Given that assessing the aerosol-relative humidity relationship through vorticity-driven entrainment is the major goal of this manuscript, this represents a significant problem. Also, while not as serious as the horizontal grid spacing, the grid spacing of 200m in the vertical is not going to capture the cold pool dynamics and associated "gustiness" near the surface very well. While it is understood that a compromise needs to be achieved in order to capture the range of scales involved here, the reviewer does not agree with the author that a grid spacing of 500m represents a "reasonable compromise". The grid domain utilized is relatively small (~125km) and thus significantly increasing the horizontal grid spacing does not seem unreasonable given our current computer resources, and in fact is appears necessary to investigate the problem of interest. Recent work such by researchers such as Khain, for example, in which the far more costly bin microphysics has been utilized have been conducted on 3D grids.

The author attempts to address this problem by running several 2D simulations from which he claims (a) that dimensionality does not impact the robustness of the results (although he does not show this) and (b) that the results using the coarser 3D grids do not change qualitatively and hence are robust. This does however raise several issues/ questions / suggestions:

• It would seem that the best approach would simply be to run the low and high aerosol control simulations in 3D using a grid resolution of between 100 and 200m. These simulations could then compared with the low and high aerosol simulations using the 500m grid spacing. This would eliminate any possible issues arising from dimensionality concerns. If the trends between the high and low aerosol cases in the high resolution 3D runs are similar to those in the 500m 3D simulations, then an argument could be made for the robustness of the results using a coarser domain. This would only require two very high resolution 3D runs, which given our current computer resources and the cloud resolving simulations currently being conducted by the community, should certainly be possible. Can the author please comment on this?

We repeated the high- and low-aerosol runs with the horizontal and vertical grid spacing of 100m for CONTROL. These repeated runs are referred to as the high-aerosol-100m and low-aerosol-100m runs. The high-aerosol-100m and low-aerosol-100m runs replace the high-aerosol-50m and low-aerosol-50m runs in the text and conclusions from these 3D 100m runs are not basically different from those from the 2D 50m runs.

• Should the high-resolution 3D simulation approach just discussed not be possible for some very valid reason, then it is recommended that the results of the 2D simulations using the same grid spacing as the 3D runs are included for the low and high aerosol case. This would mean one extra table, which would not take up a significant amount of space. This would give the reader a sense of how much the use of two-dimensionality impacts the magnitude of the results, even if the results do not change qualitatively.

The suggested 3D simulations with higher resolutions are run instead.

• The use of 2D grid setups typically has a significant impact on aspects such as low-level convergence and the size of the subsidence required between convective cores. Is the low-level convergence different between the 2D and 3D cases, and if so, how much does this differ by? Such a difference may have an impact on the entrainment – convergence relationship and hence on the humidity threshold and thus should be discussed.

The 3D simulations with higher resolutions are run and these simulations replaced the 2D simulations in the old manuscript.

• If the results do not differ qualitatively between 2D and 3D, then why not make use of a 2D grid with more appropriate grid resolutions (50-100m) for all of the simulations rather than using a mixed approach of grid dimensionality and resolution?

The 3D simulations with higher resolutions (suggested by the reviewer here) are run and these simulations replaced the 2D simulations in the manuscript.

• Pg 25293: Is the only major source (apart from the return upon evaporation or sublimation) of aerosol the initialization field?

Yes.

Are aerosol updated as the simulations continue?

After the imposition of the initial background aerosol at the first time step, there is no further external imposition of aerosol on the domain.

If not, this implies that the simulations will become cleaner as they progress. While this may not be an issue for simulations only several hours long, it may have a significant impact on simulations that are being conducted for two days, especially simulations of active convection in which aerosol are being removed by activation and precipitation. Has the author looked at the aerosol numbers at the end of the simulation? How do they compare with the initial fields? This point needs to be addressed and discussed clearly in the manuscript.

Although it is true that aerosol number decreases with time, it is notable that aerosol number decreases in both the high- and low-aerosol runs. The background averaged aerosol number at the last time step is ~ half of that at the first time step for both the high-aerosol and low-aerosol simulations and thus aerosol number is maintained higher in the high-aerosol run than in the low-aerosol run throughout the simulation period in all of the cases. Hence, the results from high-aerosol simulations are affected by larger aerosol concentration than those from low-aerosol simulations throughout the simulation period.

Also, want to note that the key mechanisms leading to stronger low-level convergence with increasing aerosol begin to operate ~ 8 hrs after the beginning of simulations when ~ 20% of aerosol mass is removed for both the high- and low-aerosol runs in all of the cases (cf., Fig. 7, which shows the domain-mean convergence magnitude increases for the high-aerosol run in CONTROL). After the stronger convergence develops, feedbacks among convergence, secondary clouds and condensation lead to the precipitation enhancement at high aerosol and the role of aerosol difference in the enhancement is negligible. This indicates that the effect of aerosol on secondary clouds (leading to the precipitation enhancement at high aerosol) is likely to be robust to the evolution of the aerosol difference from ~ 8 hrs after the beginning of simulations to the end of simulations.

The following is added:

(LL293-296 in p10)

At the last time step, the domain averaged background aerosol number is ~ half of that at the first time step for both the high- and low-aerosol runs. This indicates that aerosol number maintains higher in the high-aerosol run than in the low-aerosol run throughout the simulation period.

(LL401-407 in p14)

Also, it is worth pointing out that after the stronger convergence develops, dynamic feedbacks among convergence, secondary clouds and condensation determine the precipitation enhancement at high aerosol and the microphysical impacts of aerosol on the enhancement is negligible. This indicates that the precipitation enhancement is likely to be robust to the evolution of the difference in aerosol concentration between the high- and low-aerosol runs after the development of stronger convergence, which is consistent with Lee et al. (2008a,b).

• Pg 25294: CAPE can also be significantly influenced by the temperature and humidity of the atmosphere above the PBL. Have the various CAPE values been calculated for the variations in humidity used here? Such values should be presented in the paper because as the author himself states, variations in CAPE can have significant impacts on storm type, strength etc.

The following is added with CAPE part in Table 6:

(LL530-532 in p18)

This is closely linked to small variations in CAPE among simulations (Table 6); the maximum and minimum CAPE among simulations show just ~ 10% difference.

• Pg 25294: Another significant point needs to be considered when considering the humidity variations. By keeping the humidity constant within the PBL and only varying it above the PBL, the entrainment response could be expected to be significantly greater than the "gustiness" or convergence response in the lower levels given that the strength of the cold pool or gust front is strongly dependent on thermodynamic properties of the environment in the lower levels. Thus comparisons of the entrainment to the convergence response are biased in this regard. Such an assumption could potentially change the findings of the paper. For example, if the humidity is reduced both above and below the PBL, while the entrainment will lead to reduced precipitation, the drier air in the PBL may also lead to stronger "gustiness" that can outrun their respective storms therefore causing a collapse in the convection. Rather than helping to counter the effects of the reduced precipitation from entrainment, it may act to enhance this process. Can the author please comment on this? Comments also need to be made in the paper in this regard.

The following is added:

(LL721-733 in p24-25)

To maintain a similar cloud type (represented by the cloud-top height) and thus to isolate the dependence of aerosol-cloud interactions on humidity by excluding the dependence on the cloud type, humidity in the PBL (to which CAPE and thus cloud-top height are strongly sensitive) does not vary among the cases in this study. However, if humidity in the PBL changed together with humidity above the PBL, variation in the precipitation response to aerosol with the varying humidity would be different than shown in this study. This is because the acceleration of downdrafts and its changes induced by those in aerosol concentration are likely to vary more significantly with the varying humidity in the PBL than with the fixed humidity in the PBL among the cases. Although it is not viable to see the sensitivity of results here to the humidity in the PBL due to the fact that the varying humidity in the PBL is likely to change the cloud type, the effect of the PBL humidity on the response of downdrafts (and low-level convergence) and thus precipitation to aerosol merits future study.

• Pg 25295: It would be useful to include a figure that gives the reader a better sense of the MCE being simulated. Is the ensemble simply a convective cluster, or a better organized mesoscale convective system? Such characteristics may be important in terms of the relative importance of the aerosol response to the storm dynamics. Also, the location of the cross-section used in Figure 5 can then be indicted as the values shown in Figure 5 may vary significantly depending on where the cross section is taken through the storm. Also how does the simulated MCE structure or basic characteristics compare with the observations, given that the precipitation is compared with the observations later in this paragraph?

We added Fig. 6b which shows the horizontal distribution of cloud-liquid mixing ratio averaged over the vertical domain; if we include cloud-ice content, it is hard to see the cloud-system organization due to cirrus anvil clouds which cover a large-area of the system and thus prevent us from seeing the detailed network of individual convective cells. In Fig. 6b, we see that cells tend to be clustered. Also, the clustered cells tend to line up diagonally in the domain, which indicates that they form an organized structure. There are not many observed variables to verify the overall pattern of the simulated MCE. However, when we compared cloud fraction for each of low-, mid-, high-cloud regions, there is a good consistency between the simulated MCE and an observation. The cloud fraction is 45, 55, and 78% for low-, mid-, and high-cloud regions, respectively, for the simulated MCE and these are just a few percent different from observed values. Clouds between 0-5, 5-10, and 10-15 km are classified as low-, mid-, and high-clouds, respectively. Also, the simulated averaged cloud-top height over the simulation period shows just ~ 8% difference with the observed height. This indicates that the overall MCE structure is simulated reasonably good.

The following is added:

(LL283-292 in p10)

Figure 6b shows the horizontal distribution of the averaged cloud-liquid mixing ratio over the vertical domain at the same time as Figure 6a is obtained. Figure 6b shows that cells tend to be clustered. Also, the clustered cells tend to line up diagonally in the domain, which indicates that they form an organized structure. Simulated cloud fractions averaged over the simulation period for low-, mid-, and high-clouds are 45, 55, and 78 %, respectively, which is just a few percent different from observed fractions. Here, clouds between 0-5, 5-10, and 10-20 km in altitude are classified as low-, mid-, and high-clouds, respectively. Also, the averaged cloud-top height over the simulation period is 8.5 km, which shows ~ 8% difference with the observed height. This indicates that the overall system structure is reasonably simulated.

• Pg 25298: How are the convective cores being defined? When is a core considered convective? And are these "cores" simply grid points or has some nearest neighbor checking or something to this effect been done in order to assess cohesive cores?

The following is added:

(LL391-394 in p14)

A convective core (which is a grid column classified as the core) satisfies at least one of the following three conditions (Xu et al., 2002): 1) maximum cloud draft strength (w_{max}) is larger than the average over grid columns within 4 km with $w > 1$ m s^{-1}, 2) $w_{max} > 3$ m s^{-1}, or 3) precipitation rate exceeds 25 mm h^{-1}.

Are there more individual storms, larger storms, stronger storms, or some combination of these?

There are more individual storms but with smaller sizes at high aerosol. The time- and domain-averaged updrafts are stronger at high aerosol than at low aerosol, which indicates that, overall, the storms at high aerosol can be considered stronger than those at low aerosol.

The following is added:

(LL394-401 in p14)

More convective clouds produce more cumulative condensation for the enhanced cumulative precipitation through the increased averaged updrafts at high aerosol as also shown in Lee et al. (2008a,b), though the horizontal length of convective clouds, averaged over cloud depth, is smaller at high aerosol. The averaged length is 4.5 and 5.1 km for the high- and low-aerosol runs, respectively. This smaller cloud length at high aerosol than at low aerosol is consistent with Jiang et al. (2006), Xue and Feingold (2006) and Jiang and Feingold (2006).

• Pg 25298: Using convergence as a proxy to enhanced precipitation is somewhat risky in that once a gust front outruns its convective updraft the updraft collapses and becomes less effective in producing precipitation. Under this scenario, strong convergence would still be evident in the grid domain, however, the parent convection would be weakening. Even if secondary convection is possibly developing, can one state with absolute certainty that this is sufficient to offset the collapse of the main updraft? Also, while convergence will result in rising motion, this also does not necessarily imply more precipitation. Other aspects such as inversion layers, moisture supply, updraft strength etc all play a role. Can the author please comment on all of these points?

Here, we do not focus on the detailed individual interactions between convergence, updrafts of parent convection, inversion layers and moisture supply. In other words, we do not focus on individual downdrafts, convergence, updrafts, condensation and precipitation. Instead, we focus on domain-averaged values of these variables and their changes induced by increase in aerosol concentrations. We have just focused on the downdraft-driven domain-averaged convergence and its overall effect on the domain-averaged precipitation as discussed mostly in sections 5.1.2 and 5.1.3. As shown in the comparison between the high-aerosol-no-conv and low-aerosol runs, when stronger downdrafts are not effective in inducing stronger convergence, precipitation suppression is simulated in all of cases; this is because the increase in the domain-averaged condensation (controlled by increase in domain-averaged updrafts) is not large enough to offset the increase in the domain-averaged evaporation as shown in Table 4. This indicates that the increase in the domain-averaged updrafts induced by the increase in the domain-averaged convergence, which increases domain-averaged condensation and thus precipitation, plays a critical role in the enhancement in the domain-averaged precipitation in the high-aerosol run as compared to the low-aerosol run in CONTROL and RH-15% by outweighing the possible weakening of updrafts in parent convective clouds, which tends to decrease the domain-averaged condensation and thus precipitation.

Also, it is notable that inversion-layer height and net moisture supply (our of and into the domain) are determined by large-scale forcing which is identical for all of simulations and thus these factors do not play a role in the precipitation differences among simulations.

• Pg 25299: Aerosol enhancements have also been found to have significant impacts on updraft dynamics through latent heat feedbacks as suggested by Khain et al (2005) and Van den Heever et al (2006), and then again in more recent publications. This may play just as significant role in enhanced surface precipitation rates. Has this been considered by the author? Can the author please comment on this? This needs to be discussed in the manuscript.

Here, we also focused on latent heat feedbacks (between evaporative cooling, low-level convergence, and secondary clouds) and their impacts on condensation (controlled by updrafts). However, we did not focus on the feedbacks between latent heat and dynamics, which are involving melting and freezing. This is because the effect of evaporative cooling on results here is much more important than the effect of ice physical processes including melting and freezing. This is based on comparisons between no-ice runs and ice runs.

The following is added to indicate the insignificant role of ice physics on results:

(LL700-720 in p24)

Enhancement in melting induced by increase in aerosol concentration intensifies downdrafts and low-level convergence together with enhancement in evaporation. Also, increase in freezing induced by the increase in aerosol concentration contributes to more cloud mass and precipitation by enhancing the parcel buoyancy (and therefore updrafts) and thus further intensifying low-level convergence (Khain et al., 2005; van den Heever et al., 2006; Lynn et al., 2007; Rosenfeld et al., 2008; Lerach et al., 2008; Khain and Lynn, 2009; Ntelekos et al., 2009; Lee et al., 2010; van den Heever et al., 2010). Hence, when ice physics and thus melting and freezing are included, the precipitation enhancement induced by the increase in aerosol concentration increases as compared to the precipitation enhancement when ice physics is not included in CONTROL and RH-15%. In RH-35%, the absence of updraft invigoration from the increase in freezing and the absence of downdraft invigoration from the increase in melting lead to the further suppression of precipitation with the increasing aerosol concentration. However, the variations in precipitation (with the varying aerosol concentration) in the absence of ice physics account for ~ 75-80% of the variations in precipitation in the presence of ice physics. Thus, it can be considered that the evaporatively driven intensification of low-level convergence plays a much more important role in the precipitation difference induced by the changes in aerosol concentration than melting and freezing, which is consistent with Lee et al. (2010). This justifies the main focus of this paper, which is on the changes in the intensity of low-level convergence (induced by those in aerosol concentration and evaporative cooling) to explain the precipitation response to aerosol.

• Pg 25299: Can the author please explain why rain evaporation is reduced at high aerosol? Berg et al (2008) and Storer et al (2010) recently found similar results and attributed it to larger drop sizes in higher aerosol cases. Is that the case here? An explanation should be offered either way, making reference to Berg et al (2008) and Storer et al (2010) if necessary.

The following is added:

(LL411-412 in p14)

Consistent with Berg et al (2008) and Storer et al (2010), the smaller rain evaporation is due to larger rain-drop sizes at high aerosol.

• Pg 25299: How does the author know that the greater cloud water evaporation is due to the delayed autoconversion? The manner in which this is written implies that time plays a role. Is this what is implied? Surely the cloud drops are also smaller under higher aerosol cases and this will enhance evaporation rates? This needs to be discussed in the paper.

Yes, the reviewer is right. Larger mass of cloud-liquid both from less autoconversion and smaller droplet size due to increased droplet number concentration enhanced evaporation. We think "less autoconversion" is better than "delayed autoconversion" to explain the increased cloud-liquid mass, since autoconversion and its changes with changing aerosol occur at every time step. Hence, "delayed autoconversion" in the text is replaced with "less autoconversion". Also, the effect of smaller droplets on enhanced evaporation is added to the text as follows (LL415-418 in p14):

The more cloud-liquid evaporation is initiated by less autoconversion which enhances cloud liquid as a source of evaporative cooling and smaller droplets which tend to provide enhanced particle surface areas for evaporation and associated cooling at high aerosol than at low aerosol.

• Pg 25299: Melting can make significant contributions to downdraft strength in deep convective storms and hence low-level convergence. Why has this not been considered here? Can the author please comment?

The following is added:

(LL700-720 in p24)

Enhancement in melting induced by increase in aerosol concentration intensifies downdrafts and low-level convergence together with enhancement in evaporation. Also, increase in freezing induced by the increase in aerosol concentration contributes to more cloud mass and precipitation by enhancing the parcel buoyancy (and therefore updrafts) and thus further intensifying low-level convergence (Khain et al., 2005; van den Heever et al., 2006; Lynn et al., 2007; Rosenfeld et al., 2008; Lerach et al., 2008; Khain and Lynn, 2009; Ntelekos et al., 2009; Lee et al., 2010; van den Heever et al., 2010). Hence, when ice physics and thus melting and freezing are included, the precipitation enhancement induced by the increase in aerosol concentration increases as compared to the precipitation enhancement when ice physics is not included in CONTROL and RH-15%. In RH-35%, the absence of updraft invigoration from the increase in freezing and the absence of downdraft invigoration from the increase in melting lead to the further suppression of precipitation with the increasing aerosol concentration. However, the variations in precipitation (with the varying aerosol concentration) in the absence of ice physics account for ~ 75-80% of the variations in precipitation in the presence of ice physics. Thus, it can be considered that the evaporatively driven intensification of low-level convergence plays a much more important role in the precipitation difference induced by the changes in aerosol concentration than melting and freezing, which is consistent with Lee et al. (2010). This justifies the main focus of this paper, which is on the changes in the intensity of low-level convergence (induced by those in aerosol concentration and evaporative cooling) to explain the precipitation response to aerosol.

• Pg 25299: The buoyancy gradient as discussed will depend on cloud size, with comparisons only being valid for clouds of the same size. There is no guarantee that the clouds are of similar sizes in the simulations. Has the author looked at this? It was stated earlier that there are a greater number of convective cores in the high aerosol case but nothing was stated regarding the size of these cores.

The following is added:

(LL398-401 in p14)

The averaged length is 4.5 and 5.1 km for the high- and low-aerosol runs, respectively. This smaller cloud length at high aerosol than at low aerosol is consistent with Jiang et al. (2006), Xue and Feingold (2006) and Jiang and Feingold (2006).

(LL430-432 in p15)

That the averaged horizontal length of clouds (for each of the x- and y-direction) at high aerosol is smaller than those at low aerosol confirms the larger horizontal buoyancy gradient at high aerosol than at low aerosol.

• Pg 25299 – The buoyancy profiles have been normalized from cloud base (0) to cloud top (1) and averaged over cloudy areas. What types of clouds are represented in this average, or is it all deep convection?

All types of clouds are included. This is because, as mentioned above, we are more interested in the averaged response of cloud system over the domain to the variation in aerosol concentration than the response of individual clouds.

• Pg 25302: Simply because clouds have similar cloud top heights does not imply that the convective ensembles are similar cloud types, as what appears to be implied by the author in this section. A squall line, a mesoscale convective complex and a supercell storm may all have similar cloud heights and yet completely difference storm structures, flow regimes, life times etc. Even if it is likely that the cloud types may be similar from simulation to simulation, unless aspects such as the storm dynamics of these various convective ensembles have actually been examined, the conclusion that "the effect of cloud type on results here is considered excluded reasonably well" is not robust.

As demonstrated in Lee et al. (2008a,b, 2009, 2010), among variables characterizing a cloud type, cloud-top height plays a key role in changes in low-level convergence and precipitation induced by changes in aerosol concentration in convective clouds whose base is around or in the PBL. This is because the higher a cloud is, the longer path downdraft can take when it descends to the surface. Initially, at the level of evaporative cooling, downdraft is stronger at high aerosol than at low aerosol due to more cloud-liquid evaporation. This initial difference can be magnified more when there is a longer path for downdraft to take as in a higher cloud; with longer path, there is more time for the different acceleration of downdraft to make the downdraft difference larger. This is why the cloud-top height is selected to represent cloud type as a control on the effect of aerosol on downdraft and low-level convergence.

The following is added:

(LL523-526 in p18)

Here, the cloud-top height is selected to represent a cloud type as a control on the effect of aerosol on low-level convergence based on findings in Lee et al. (2008a, b, 2009, 2010).

• Pg 25303: Several reasons should be included in the discussion on ice physics as to why the differences between the aerosol runs are reduced, including the role of melting. Given that the differences are reduced in the no ice cases appears to suggest that variations in humidity may play a more important role when ice is present.

The following is added:

(LL700-720 in p24)

Enhancement in melting induced by increase in aerosol concentration intensifies downdrafts and low-level convergence together with enhancement in evaporation. Also, increase in freezing induced by the increase in aerosol concentration contributes to more cloud mass and precipitation by enhancing the parcel buoyancy (and therefore updrafts) and thus further intensifying low-level convergence (Khain et al., 2005; van den Heever et al., 2006; Lynn et al., 2007; Rosenfeld et al., 2008; Lerach et al., 2008; Khain and Lynn, 2009; Ntelekos et al., 2009; Lee et al., 2010; van den Heever et al., 2010). Hence, when ice physics and thus melting and freezing are included, the precipitation enhancement induced by the increase in aerosol concentration increases as compared to the precipitation enhancement when ice physics is not included in CONTROL and RH-15%. In RH-35%, the absence of updraft invigoration from the increase in freezing and the absence of downdraft invigoration from the increase in melting lead to the further suppression of precipitation with the increasing aerosol concentration. However, the variations in precipitation (with the varying aerosol concentration) in the absence of ice physics account for ~ 75-80% of the variations in precipitation in the presence of ice physics. Thus, it can be considered that the evaporatively driven intensification of low-level convergence plays a much more important role in the precipitation difference induced by the changes in aerosol concentration than melting and freezing, which is consistent with Lee et al. (2010). This justifies the main focus of this paper, which is on the changes in the intensity of low-level convergence (induced by those in aerosol concentration and evaporative cooling) to explain the precipitation response to aerosol.

Minor Comments:

• "Increasing aerosol" is often used throughout the paper. This term is too general and can imply a variety of different aspects including number concentration, mass or even size. This should be replaced with terms such as "Increasing aerosol concentrations" or "Increasing aerosol numbers", or at the very least be defined as such at the beginning of the manuscript.

Throughout the manuscript, "increasing aerosol" is replaced with "increasing aerosol concentration"

• Pg 25289: ". . . .. vorticity in the horizontal direction." This needs to be better described. Horizontal vorticity can be found throughout various regions in deep convective storms or ensembles. Presumably the author is referring to the horizontal vorticity at cloud edge, however, this needs to be more clearly stated.

The following is added:

(LL443-444 in p15)

In this study, the center of the horizontal vorticity is located at cloud edge.

• Pg 25290: "Hence, aerosol-induced changes in entrainment (which tends to increase evaporation . . . ." Presumably the author means increased aerosol concentrations? Instances like this need to be corrected throughout the paper.

Throughout the manuscript, "aerosol-induced changes" is replaced by "changes induced by the increase in aerosol concentration"

• Pg 25290: "These systems are . . . .. driven by deep convective clouds" Once again, these descriptions are confusing. What does the author mean by "driven by deep convective clouds"? The driving or forcing of the Asian and Indian monsoons is synoptic in nature, while the deep convective clouds of the ITCZ are the result of convergence in this region. In other cumulus ensembles shallow cumulus may moisten the environment, thereby preceding deep convection, and debates exist over which system is the driving force. This needs to be better described.

Although there are debates over the driving force, it is true that MCEs in the monsoons and ITCZ are coupled with deep convection. Hence, "These systems are observed to be composed of numerous MCEs which are driven by deep convective clouds (Houze, 1993)" in the old manuscript

is replaced with

(LL120-122 in p5)

"These systems are observed to be composed of numerous MCEs coupled with deep convective clouds and associated circulations (Houze, 1993)"

• Pg 25292: The top of the PBL looks more like 1.6 km than 2km.

"2 km" is replaced with "1.6 km" (LL172 in p6)

• Pg 25293: How are the surface heat and moisture fluxes prescribed? Such fluxes will impact the recovery of the cold pools and the associated convergence.

The observed fluxes are prescribed uniformly across the surface following the suggested flux prescription by the ARM specification which has been adopted by the previous numerous ARM simulations. Also, want to emphasize that these fluxes are identical for all of simulations in this study and thus do not have an impact on differences in results among simulations.

The surface-flux description is described as follows (LL183-185 in p7):

"The observed temporally varying surface fluxes of heat and moisture were prescribed uniformly across the surface and they are identical for all of simulations in this paper."

Pg 25293: Dust may operate very effectively as CCN. Can the authors please comment on why dust has been limited to operating as IN?

In this study, we did not consider chemical processes associated with aerosol and just focused the effect of aerosol number concentration on clouds. Hence, chemical processes coating dust particles with soluble substances such as sulfate are not simulated here; when dust particles are coated with soluble substances, they can act as CCN.

The following is added:

(LL206-209 in p7)

In this study, the well-known transformation of dust or black carbon to the soluble-coated CCN via coagulation with soluble substance is not considered. Hence, aerosol composed of either dust or black carbon is assumed to act only as ice nuclei.

• Pg 25293: While aerosols are removed following precipitation reaching the surface, are aerosols within the atmosphere removed by rainout processes? Either way, this needs to be mentioned?

The following is added to justify no consideration of impaction scavenging:

(LL688-699 in p23-24)

In this study, aerosol incorporated into hydrometeors only via nucleation (i.e., nucleation scavenging) is subject to the removal of aerosol from atmosphere by precipitation. Here, we did not include the aerosol removal by precipitation which captures aerosol (i.e., impaction scavenging), since it is known that impaction scavenging only accounts for ~ 10% or less of the total aerosol removal by scavenging. Also, impaction scavenging is most effective when clouds develop heavy precipitation at their mature stages as shown by Ekman et al. (2004, 2006). The key mechanisms leading to stronger low-level convergence with increasing aerosol begin to operate before heavy or maximum precipitation develops as in CONTROL (cf., Figs. 5 and 7, which show the domain-mean convergence magnitude increases before the onset of heavy precipitation), so the neglect of impaction scavenging is not expected to change the qualitative nature of the results.

Also, how are aerosol numbers returned to the simulation following evaporation and sublimation ie how is the aerosol mass that is left following these processes returned to the atmosphere in terms of aerosol number? This should be described in a couple of sentences.

At each grid point which has aerosol mass returned by evaporation and sublimation, the returned mass is added to pre-existing aerosol mass. Then, aerosol number in each bin of aerosol size distribution is determined using this total mass (returned mass + pre-existing mass), aerosol particle density, and an assumed log-normal size distribution as stated in manuscript (LL211-213 in p8). The number of aerosol particles in each size bin of the log-normal distribution is determined in a way that the sum of corresponding aerosol mass over size bins (this is calculated using the mass of an aerosol particle in each size bin we can obtain using particle density) is equal to the total mass predicted.

The following is added:

(LL217-221 in p8)

At each grid point which has aerosol mass returned by evaporation and sublimation, the returned mass is added to pre-existing aerosol mass. Then, aerosol number in each bin of the aerosol size distribution is determined using this total mass (returned mass + pre-existing mass), aerosol particle density, and the assumed log-normal size distribution.

• Pg 25293: Is it stated in the manuscript that aerosol are initialized using the profiles contained in Figure 4 of Fridland et al (2009). Such a figure is not likely to take up much room, and given the importance of these profiles is worth repeating here. Also it would be useful in the manuscript to give a basic description in words as to what the number concentrations of the control case are – order of magnitude would be fine. And how do these number concentrations compare with observations during this time period?

We added Figure 3 in the new manuscript for aerosol distribution averaged over the PBL, since it is well-known that the PBL aerosol affects clouds mostly.

The following is added:

(LL228-234 in p8)

These distributions are tri-modal log-normal distributions with modal diameters of 0.03, 0.18, and 4.4 micron and with standard deviations of 1.12, 1.45, and 1.80 for nuclei, accumulation, and coarse mode, respectively. Figure 3 shows the initial background aerosol distribution averaged over the PBL. The averaged aerosol number (integrated over the distribution) over the PBL is ~ 400 cm^{-3} and this represents the observed number over the PBL reasonably well during the simulation period.

• Pg 25294: Seifert and Beheng (2006), Van den Heever et al (2010) and Storer et al (2010) should be referred to here.

Done.

• Pg 25295: How were the relative humidity decreases of 15% and 35% decided on? Why not also consider 55%, especially given the response of the finer grid spacing described later in the paper? Do any observations during TWP-ICE support such decreases?

These decreases are based on the variation of the averaged humidity above the PBL during the whole TWP-ICE period (lasting 20 days), which is ~ 35%.

The repeated high- and low-aerosol runs for the decrease in relative humidity of 55% show precipitation suppression at high aerosol due to nearly the identical mechanism described for RH-35%, though suppression is larger than in RH-35%. Hence, we believe that experiments with 55% decrease do not add up new information and thus they are not included in the manuscript.

• Pg 25295: It is suggested that the 0 isoline be omitted from Figure 5 as it complicates the figure. It is also recommended that different line thicknesses be used in this figure, as the current lines are difficult to distinguish from one another.

We believe 0 isoline is needed to represent the vertical extent of convective clouds. For example, if we remove 0 isoline, the top of convective cloud at 100 km in the horizontal domain appears to be ~ 8km despite the fact that its real top is ~12km. Hence, keeping 0 isoline makes us see the simulated clouds in a more confident way.

To distinguish lines for cloud liquid from those for cloud ice clearly, lines for cloud liquid are thickened.

• Pg 25297: Units are needed for Table 2.

Done.

• Pg 25298: The term $\left|\nabla \bullet \vec{V}\right|$ is a little confusing, as strictly speaking this term would represent divergence. Presumably only those points where $\left|\nabla \bullet \vec{V}\right| < 0$ are considered in the average and then the absolute value is applied? This should be more clearly represented.

The following is added:

(LL384-385 in p13)

only $\nabla \bullet \vec{V} < 0$ is included in the absolute value and average.

• Pg 25299: The use of cloud water is more appropriate than cloud liquid.

We believe “cloud water” can mean both ice water and liquid water and, hence, using cloud liquid is more appropriate to indicate the liquid phase.

• Pg 25302: Once again it is distressing to see the lack of referencing in the section on the dependence of cloud type and ice physics. Numerous references included those in the first section have previously demonstrated the importance of these aspects and should be included here.

Necessary references are included.