Atmos. Chem. Phys. Discuss., 10, C12209–C12219, 2011
www.atmos-chem-phys-discuss.net/10/C12209/2011/

Atmospheric Chemistry and Physics Discussions

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Interactive Comment

Interactive comment on "Dependence of aerosol-precipitation interactions on humidity in a multiple-cloud 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.
- 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. 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.
- 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.

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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.

- 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

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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?
- 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

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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 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.
- 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?
- Pg 25293: Is the only major source (apart from the return upon evaporation or sublimation) of aerosol the initialization field? Are aerosol updated as the simulations continue? 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.
- 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 pre-

sented in the paper because as the author himself states, variations in CAPE can have significant impacts on storm type, strength etc.

- 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.

- 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?

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

- 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?
- 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.
- 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.
- 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

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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.

- 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?
- 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.
- 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?
- 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.
- 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

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to suggest that variations in humidity may play a more important role when ice is present.

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

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- 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.
- Pg 25293: Dust may operate very effectively as CCN. Can the authors please comment on why dust has been limited to operating as IN?
- 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? 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.
- 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?
- Pg 25294: Seifert and Beheng (2006), Van den Heever et al (2010) and Storer et al (2010) should be referred to here.
- 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?
- 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.

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- Pg 25297: Units are needed for Table 2.
- Pg 25298: The term $|\nabla.V|$ is a little confusing, as strictly speaking this term would represent divergence. Presumably only those points where $\nabla.V < 0$ are considered in the average and then the absolute value is applied? This should be more clearly represented.
- Pg 25299: The use of cloud water is more appropriate than cloud liquid.
- 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.

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 25287, 2010.

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