We thank Dr. B. Stevens and two anonymous referees for their valuable comments to the manuscript and their constructive suggestions. Below, we explain how the comments and suggestions are addressed and make note of the changes we have made to the manuscript.

Referee #1 (B. Stevens)

This is an interesting manuscript that addresses itself to many open questions pertaining to the development, longevity and growth of pockets of open cells. Experiments motivated by state of the art field data from the recent VOCALS campaign are used to explore the role of meteorological factors versus the aerosol. The ideas are clearly presented both through the writing and the figures. The experiments are novel and touch on a number of factors thought to be related to the issues mentioned above. For these reasons the manuscript is in general suitable for publication in Atmospheric Chemistry and Physics.

I believe the quality of the ideas could and should be strengthened. The major weakness of the paper in its current form is that it consists mostly of show and tell and does not attempt to develop a clear message. The authors have interesting things to show, and the narrative is informative, but ideas are poorly developed. My major reservation is that no framework is developed for motivating and comparing the different experiments.
The experiments, while qualitatively well chosen, are quantitatively poorly motivated. From a broader perspective the two key parameters are buoyancy and lifting condensation level. Increasing the moisture enhances the buoyancy and lowers the LCL. Increasing temperature enhances the buoyancy but raises the LCL. This is the theoretical backdrop for many of the experiments they conduct, but it seems as if wasn't really thought about. A 0.45 g kg^{-1} moisture perturbation has an influence on the buoyancy that is equivalent to a 0.08 K temperature perturbation. Similarly one can calculate the depression of the LCL that would occur from a temperature and moisture perturbation. Why compare a 0.5 K temperature change to a 0.45 g kg^{-1} moisture perturbation? If one wants to explore the relative role of cloud deepening (LCL changes) versus secondary circulation changes, why not pick experiments that minimize the change in the buoyancy but maximize the changes in the LCL (this is hard to do perfectly because of the difference between how temperature/moisture perturbations project onto buoyancy in saturated versus unsaturated air.) Likewise a 10 W m^{-2} change in sensible heat fluxes is, in terms of buoyancy, equivalent to a 140 W m^{-2} change in latent heat flux, so why is the response so different for a sensible heat flux as compared to a latent heat flux. Simply from the point of view of energetics the PLFX experiment should be similar to the PSFX experiment, but it is not. Why?
Because no framework has been established for evaluating what a change in moisture means, relative to a change in CCN, or a change in temperature, one has really no basis for comparing the different responses. Hence the paper loses its focus.

We believe that we have not done a good job of conveying the primary motivation for the set of numerical experiments chosen on this study. The perturbation of temperature and moisture used in the experiments are not chosen arbitrarily but are based on the *observed differences* between the precipitating open cell region and the overcast region (see Fig. 1 and case description in section 2). We performed two sets of simulations. The first set utilizes the exact observed different soundings, the results of which show that the difference of the soundings (Δq_v=+0.9 g kg^{-1}; $\Delta\theta$= -1 k) could be a result of precipitation in the open cell region (see section 4.3 and Fig. 11). However, measurements from a 12-hour earlier flight (BAe-146) found similar differences in moisture and temperature prior to precipitation. Therefore, to ensure that the perturbations applied to the sensitivity experiments are not too strong, we carefully reduced the perturbations by half (i.e., randomly distributed with mean Δq_v=+0.45 g kg^{-1} and mean $\Delta\theta$= -0.5 k).

While we do understand the referee's point regarding a theoretical backdrop (e.g. vis-à-vis LCL or buoyancy) for performing numerical experiments, our goal is not to quantitatively compare various perturbations in a constant LCL or constant buoyancy framework, but to *root our approach in the observational variability*. The suggested approach is certainly worth pursuing but we believe that connecting our results to the observations is a different but equally valid approach.

In regards to the questions raised by the referee:

1) **Why compare a 0.5 K temperature change to a 0.45 g kg^{-1} moisture perturbation?**

We did not intend to inter-compare the impacts of different meteorological factors, at least, in a quantitative sense. The main goal of this set of sensitivity experiments was to identify whether each individual perturbation is important in initiating precipitation in the region we focus on by isolating each factor and/or doing reasonable combinations (e.g., decrease in temperature and increase in moisture; increase in both surface sensible heat and latent heat fluxes). Therefore, most comparisons are done through the control experiment, or between the inner perturbed domain and the outer domain. We did not attempt to quantify the importance of each factor in affecting the start time, duration and intensity of precipitation. If we had attempted to do so, the magnitude of perturbations would have had to be carefully justified, and would have had to be comparable.

We agree that changes in buoyancy and lifting condensation level (LCL) are an important measure of the impact of moisture and temperature perturbations on a rising air parcel from the surface; however, due to mixing of air parcels at all resolved scales in the modeled boundary layer and the different origins of air parcels, this measure may not be as effective as predicted by simple theory.

It is important to clarify is that the perturbation of temperature used in this study is ***negative***, so both moisture and temperature perturbations lower the LCL. One common impact of negative temperature perturbation and positive moisture perturbation is that they both increase the boundary-layer relative humidity, which potentially thickens clouds and increases the chance of precipitation. This is the real motivation for the chosen set of sensitivity experiments.

We have added a paragraph that clearly motivates the choice of experiments. For context, we also add a paragraph that includes simple calculation of the effect of these perturbations on buoyancy and LCL.

2) Likewise a 10 W m^{-2} change in sensible heat fluxes is, in terms of buoyancy, equivalent to a 140 W m^{-2} change in latent heat flux, so why is the response so different for a sensible heat flux as compared to a latent heat flux. Simply from the point of view of energetics the PLFX experiment should be similar to the PSFX experiment, but it is not. Why?

In section 3.3, we listed two reasons for this: First, accumulated moisture near the surface is effectively transported to the cloud layer by enhanced buoyancy of thermals due to stronger surface sensible heating. Second, a temperature gradient induced by stronger surface heating in the inner domain generates a circulation with convergent flow in the lower boundary layer that pumps moisture from the adjacent surface layer. The finding of the importance of this type of mesoscale circulation induced by a horizontal temperature gradient in a large domain is one of the novel aspects of this study. Two figures (Figs. R3 and R4) are shown in the response to Referee #2's relevant comments. One figure (Fig. R5) is added to the revised manuscript to illustrate the circulation.

Some more minor issues are as follows:

• RE the title: The only thing that makes the discussions here germane to the southeast Pacific is the depth of the boundary layer. A more descriptive title might be worth considering.

Since the initial profiles of all simulations and perturbations in the sensitivity experiments are all obtained from observations made in the southeast pacific, and because the numerical experiments are motivated by the observations, we do want it to be clear in the title. We agree that a more descriptive title could be better and have changed it to:

"Modeling microphysical and meteorological controls on precipitation and cloud cellular structures in southeast pacific stratocumulus"

• 8344.14 The Peters et al. study here is quite relevant and probably merits discussion.

The study (Petters et al. 2006) has been added to the discussion.

• 8347.22 What do you do about cloud optics. Generally clouds are very nearly conservative scatterers, hence the absorption may disproportionately be due to aerosol. However solar radiation models that have a poorly resolved solar spectrum often over-estimate the cloud absorption. Given the introduction of the cloud microphysics and aerosol model it would be good to know in detail how this is coupled to the radiation.

For computational efficiency, this shortwave radiation scheme used in the WRF model is quite simple. Only liquid water content is used in the calculation of shortwave radiation. No aerosol radiative effect is considered in the current radiation scheme since it is not the focus of this study. In the clean, open cell simulations we doubt that there would be any significant direct radiative effect due to the aerosol and in the closed cell simulations, the cloud will tend to dominate. The radiative effects of absorbing aerosol in stratiform clouds have been investigated elsewhere (e.g., Johnson, QJRMS, 2005). We have made this clear in the revised manuscript.

Based on our recent unpublished DYCOMS-II sensitivity tests using different shortwave radiation schemes in which treatments of solar extinction above model top are also different, the simple scheme we used in this study has larger downward shortwave flux at the model top. This reconfirms our suspicion that the solar absorption was over-estimated, as in some other spectrally coarse shortwave schemes, but the trend of daytime reduction in cloud amount and precipitation is consistent with previous modeling and observational studies (Caldwell et al. 2009; Bretherton et al. 2004). For this reason, we did not quantitatively describe the solar heating effect and instead compare the clean case with the polluted one. To avoid over-interpretation, we now add the following statement in the relevant section 3.1.

"We note, however, that solar heating is likely overestimated in the simulations and consequently, the daytime reduction of cloud water and precipitation is likely also overestimated."

In the second set of sensitivity experiments, which is the major focus of this study, solar radiation is not included.

• **8348.22 The grid spacing is an issue. Particularly the wide body of work that shows that coarse vertical resolution and monotonic schemes greatly exaggerate the decoupling of the layer and the dissipation of the cloud. You may be forming POCS to easily here.**

The 30-m vertical grid spacing is comparable to most LES simulations (although we agree it would be preferable to make it finer and represent entrainment with more fidelity (e.g., Hill et al., JAS 2009). The 300-m horizontal grid spacing is indeed rather coarse. In our earlier DYCOMS simulations we did test the sensitivity to grid size and now refer the reader to that discussion. As stated in the manuscript, it is not computationally feasible to run all these simulations at a significantly higher resolution for such a large domain. And since the mesoscale organization is a central theme in this paper it is important to cover a large domain.

The coarser horizontal resolution does tend to create poorer vertical mixing and therefore more cumulus-like mixing. However, until we generate "sufficient" precipitation, the cells maintain a closed pattern with near-solid cover. The open-cell formation is clearly linked to precipitation, which enhances the cumuliform clouds. We are not claiming that we know just how much precipitation is sufficient to cause the transition, but that precipitation is a necessary condition for the transition. Finally, all experiments consistently have the same resolution and so we don't expect the grid resolution to affect our main conclusions.

• **8349.01 One outstanding puzzle in an attempts to model precipitating boundary layers is the vertical velocity variance profile. Generally models show much more damped circulations than what we could infer from measurements. Including ww as one of your**

observables would help the reader determine if this remains an issue in your simulations. I suppose it does.

In Wang et al. (2009) we demonstrated that like most other models the WRF model underestimates the overall vertical velocity (w) variance when simulating the DYCOMS-II case over the northeast pacific. The same model and physics are used in this study, so we expect that this issue persists. However, analysis of the limited amount of observations of w (Wood et al. 2010) indicates that the peak w variances (in the early morning) simulated by the model (shown in Fig. 4) are fairly reasonable. Peak nighttime modeled values of $\overline{w'^2}$ are approximately 0.5 $m^2 s^{-2}$ and 0.2 $m^2 s^{-2}$ for closed and open cell conditions (Fig. 4), as compared to rough estimates of 0.6 $m^2 s^{-2}$ and 0.3 $m^2 s^{-2}$ for early morning observations (Wood et al., 2010). We now add some notes on this comparison in the manuscript.

• 8349.23 There is the long-standing appreciation that precipitation reduces entrainment deepening (e.g., Stevens et al., 1998), how did you determine here that this was purely a radiative effect.

We agree that the impact of precipitation on entrainment has contributions too. The corresponding sentence has been reformulated as: "… closed-cell clouds in D500 grow higher than open-cell clouds (D30) because of persistently stronger radiative cooling over the closed-cell deck *and the suppression of entrainment due to precipitation (e.g., Stevens 1998) in D30*".

• 8350.01 Isn't this an idea you have the data to test. As it reads now it seems like you are speculating about something that is easy to check in the simulations.

We did look at PDFs of snapshots at different model times and visually inspected vertical cross-section of clouds. There is no clear bimodality as seen in the observed PDFs; however, the peak of the mode varies with time and the distribution is quite wide (~200 m) even for one snapshot. The peak of PDFs corresponds to the inversion cap with strongly buoyant walls contributing to the right of the mode and decaying clouds to the left. On a single cross section, we see open-cell walls in updrafts are higher than adjacent decaying clouds, but the transition is smooth. The spatial and temporal sampling over the entire domain over a few hours further smooth out the variability and widen the distribution. We now reformulate the sentence as:

"The spatial and temporal sampling over the entire domain over a few hours during which time clouds are progressively evolving obscures the observed variation of cloud tops between open-cell walls and decaying cells."

• 8351.24 Of course the simulations with clouds and water vapor have water vapor and cloud absorption. It is not like the water vapor is being exchanged from the cloud. It would be helpful if you understood the sources of SW absorption in your model (see comment above).

We don't assume that water vapor absorption contributions are obvious to every reader and feel that it is important to make this point.

• 8352.25 The figures are nice and concise, but would be better if the x-y plot array were rotated to correspond with the flow visualization.

We arrange the figures in the current way for each quantity to share the same x-axis (to be concise) and to facilitate the comparison if one wishes to.

• 8353.08 I gather that I was not supposed to see this from the plots, is that correct? If so it might help to indicate parenthetically that the evidence for this point is not shown.

Yes, the point is not directly reflected in the figure, but the explanation right after the point is based on model results. This is now clarified in the text.

• 8353.19 But the CCN change is relatively bigger. See my major point above, what is the metric for comparing a change in one quantity to a change in another?

Please also see the response to the major point above. The CCN number concentration change is also based on observations. The PCASP measured 31 (97) cm^{-3} subcloud-layer aerosol particles in the size range between 0.12 and 3.12 μm over POC (overcast) region, and the TSI 3760 (D>10 nm) gave 151 (140) cm^{-3} (see Table 5; Wood et al. 2010). We have an accumulation mode (mean diameter of 0.2 μm) of CCN in the model, which only covers part of the PCASP size range. Practically, the upper bound has been extended to 150 mg^{-1}. Leg-mean cloud drop number concentrations in POCs were as low as 7 cm^{-3}, and were 21 cm^{-3} near cloud base. Accordingly, we used a lower bound of initial CCN concentration of 30 mg^{-1}. The range is by all accounts realistic in the southeast pacific region.

The original value of 12% increase was a mistake, which was the relative increase in water vapor, not cloud water. It has been corrected to "two-fold increase in LWP".

• 8354.19 What is the point of drawing the analogy with thermally direct frontal circulations?

We thought that it may be helpful in understanding how the circulation is formed since the frontal circulation concept is well accepted, but it seems that it has caused some confusion. We have therefore decided to remove it.

• 8356.13 Isn't the Atkinson and Zhang work referring to cold-air outbreaks, which is somewhat different than what you are talking about, or where were you supposing the 1200 Wm^{-2} is coming from?

We used 300 W m^{-2} as the upper bound for testing to keep a constant Bowen ratio of 0.1. Even with such a strong surface latent heat flux, drizzle was not promoted efficiently. We do not claim that it is the same as Atkinson and Zhang's (1996) work, but rather that "the sensitivity test may still be broadly relevant". Please note that the 1200

W m^{-2} is the upper bound of surface heat fluxes ever reported according to this review article.

• 8356.21 Why does “upsidence” (I am not sure that the community benefits from the wider use of this phrase) lead to more entrainment. I think of this being an adiabatic contribution to the evolution of the layer. Please explain.

This was a mistake. We meant that more entrainment is a result of a deeper and more turbulent boundary layer (ref. Garreaud and Munoz, 2004). This sentence has been rephrased.

• 8358.14 Please provide a reference or justification for the estimated aerosol source strength. It might be good to compare to a sea-spray production mechanism.

Baker and Charlson (1990) estimated a new CCN formation rate of 1000 particles m^{-3} s^{-1}, i.e., 3.6 cm^{-3} h^{-1}. To create some bounding simulations, we performed a second set of simulations at a reduced source of 0.72 cm^{-3} h^{-1}. These values can be compared to calculations of new particle production by sea-spray production of ~ 1.6 cm^{-3} h^{-1} using the Clarke et al. (2006) parameterization (Kazil et al. 2010, manuscript in preparation). This is now mentioned in the text. (Note: since these values are all approximate, we use cm^{-3} and mg^{-1} interchangeably in this response but always use mg^{-1} in the model calculations.)

• 8359.17 The figure is missing.

We did not mean to show a figure here. All GOES-10 visible satellite images during the VOCALS-REx can be accessed through on the website of VOCALS field catalog (http://catalog.eol.ucar.edu/vocals/).

• 8360.19 Of course there will be pre-existing variability in the water vapor field during the process of stratocumulus formation, hence deeper more precipitating clouds may exist from the outset, i.e., POCS may often be there from the very beginning.

We are in full agreement that the pre-existing variability in the water vapor field could cause variable depth of clouds and thicker clouds will precipitate more readily, all else being equal. This is the motivation of our sensitivity experiments with water vapor and temperature perturbations. Our conclusion is that the observed difference in water vapor between overcast and open-cell regions can perturb a stratocumulus deck to precipitate and form open cells. Our study does not rule out the possibility that POCs may have been there from the very beginning. However, further analysis of the model results (Fig. 12 in the revised manuscript) shows that precipitation in D30 case is strong enough to the dry/warm sounding to the observed wet/cool one, which means that the pre-existing variability, if it exists, is very likely much smaller than the observed one.

• 8361.11 What does this discussion add beyond what was shown by Savic-Jovic and Stevens.

Here we *quantitatively* discuss the differences in water vapor and temperature between the precipitating clean case and the non-precipitating polluted case. More importantly, the differences are found to be comparable to observed ones. To the best of our knowledge, this has not been discussed in the literature.

Anonymous Referee #2

General comments

The paper presents a model sensitivity study of marine stratocumulus clouds to aerosol concentration and meteorological conditions with regard to the transformation from the closed-cell to open-cell cloud structure. This is a detailed investigation with some interesting new results and deserves published in ACP. I have the following comments.

The authors discussed the formation of POCs in details using a number of sensitivity simulations. But I am not sure what is exactly meant by the open-cell or close-cell structure. Usually, the open-cell structure is established when clouds form in the narrow updrafts with clear descending regions, while the closed-cell refers to the condition where clouds form around the center with thinning clouds in the narrow downdrafts. For this definition, clouds with the open-cell structure have much less coverage than those with the closed-cell. Figs. 5 -7 show the cloud albedo for simulations with six various conditions. Only PCCN clearly displays much reduced cloud cover over the disturbed area. Although the cloud structures in other simulations (FQV, PSFX and SFLX) become much larger, it is not clear that the cloud coverage is much reduced or that the vertical motion pattern is changed to that of an open-cell structure. Do these cloud structures represent the open-cell? If yes, the authors should provide some statements on why they are different from the PCCN and some satellite pictures that show clear thin and narrow bands of open-cell clouds. Or these cloud structures are simply in a transition to a full open-cell condition. In that case, authors should show some fully developed open-cell plots.

Yes, the cloud albedo fields in the original Figs. 5-7 only show the cloud structure in a transition to a full open-cell condition. A sentence has been added to clarify this. There are a few reasons why we did not show some fully developed open-cell structures. First, similar figures have been shown in our previous studies (Wang and Feingold, 2009a, b), which we have been referring to, and more figures would add little new to this paper. Second, this second set of experiments was designed to see what factors are important in triggering precipitation in overcast stratocumulus clouds. Third, the current domain is still too small to allow the full development of open cells after the initiation of precipitation; clouds will wrap into the domain from boundaries in some experiments a few hours later because of the cyclic boundary conditions.

Please see the following additional figures (Figs. R1 and R2) we provide for review purpose.

Another issue with the paper is that some conclusions or results are stated or described without the relevant information being actually shown in the paper. For example, in last

paragraph on page 8350, it is stated that "solar heating breaks up open-cell walls and cloud fraction further reduced". But no results are shown to support these claims.

We agree that not all points we made in the paper are fully illustrated in figures. We only selected ones that are the most important and the least easy to describe in words. In regards to the specific statement, which is based on viewing an animation of cloud field during the daytime, it is actually reflected in the reduction of cloud cover and LWP (Fig. 3a,b). This is now pointed out in the text. For review/inspection purpose, we show a relevant figure here, but we still don't believe it's necessary to add it into the paper.

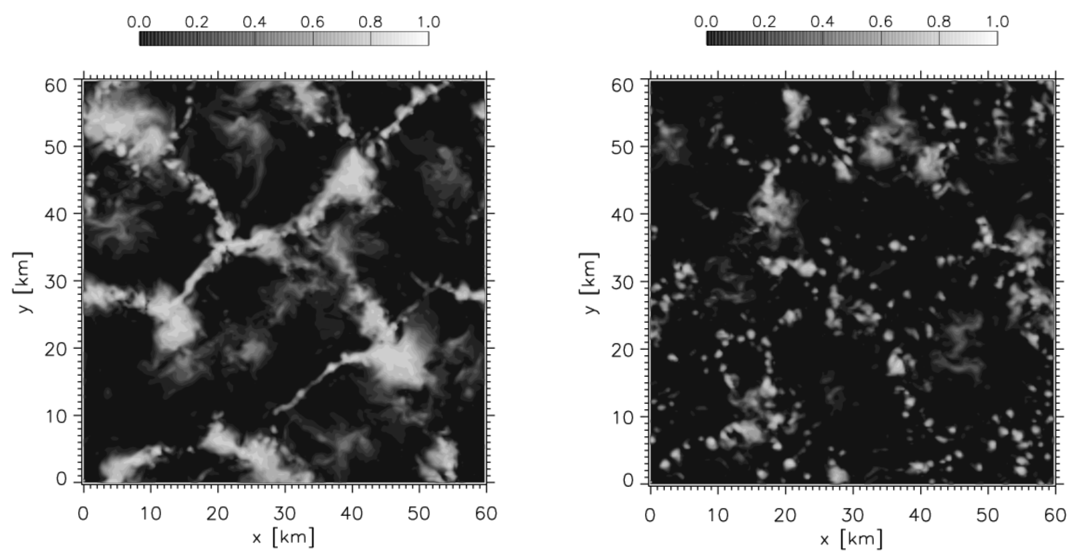

Figure R1: Snapshots of cloud albedo field in D30 show the well-developed open-cell walls (on the left panel) and open-cell walls that are broken-up by solar heating (on the right panel).

Another example is about the impacts of PQV. (I believe this is an important conclusion of the paper.) On page 8353, it is stated that drizzle and the broken clouds almost cover the entire domain by t=8h. But the results are not shown here. This makes harder for readers to understand and appreciate the conclusion. I understand that this paper includes many simulations and it is difficult to show most of the results. However, if results are important for conclusion, they should be shown in the paper.

We chose the snapshot of cloud field at t=6 h that captured the essence of what we are trying to show. Additionally, the PDFs in other panels (Fig.5 b, c, d) integrate all information from t=1 to 8 h. We don't believe it's necessary to show one more snapshot at t=8 h, but, again, we provide the following figure just for review purpose.

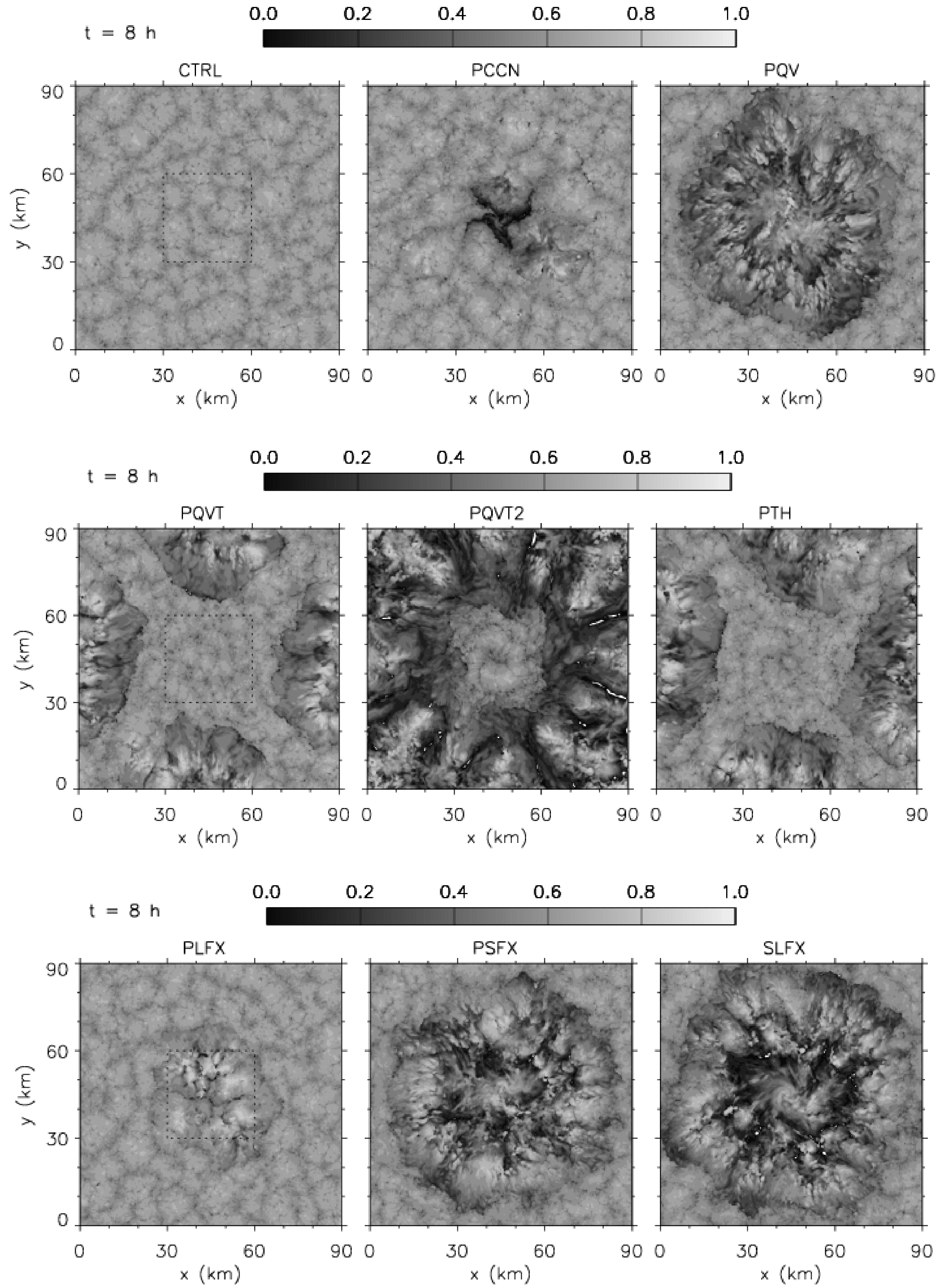
t = 8 h
0.0
0.2
0.4
0.6
0.8
1.0
CTRL
PCCN
PQV
PQVT
PQVT2
PTH
PLFX
PSFX
SLFX
y (km)
x (km)
0
30
60
90

Figure R2: Snapshots of cloud albedo field at t = 8 h for the experiments shown in Figs. 5-7 in the original manuscript. Clouds wrap into the domain from boundaries in some experiments (PQVT, PQVT2 and PTH) because of limited domain size and cyclic boundary conditions.

Specific comments

Page 8346, line 15-20 on specification of wind. The initial wind profiles (Fig. 1) show strong baroclinicity across the inversion that enhances v speed below the inversion. Therefore, the geostrophic wind cannot be constant in the lowest troposphere. Without appropriate thermal wind specification, these wind profile cannot be maintained. I am wondering how the geostrophic winds (or large-scale pressure gradient) are specified.

As briefly mentioned in the manuscript, the initial wind profiles are a simple fit to the few available leg-mean values and the winds near the inversion were just an estimate. We did not attempt to maintain the initial wind profile. The main purpose of including initial winds was to add some wind shear to the simulations, and to provide a more realistic simulation for comparison to observations. According to sensitivity tests in our previous study (Wang and Feingold 2009a), including winds in the simulations affects the appearance of open and closed cells, but does not determine the closed/open cellular state.

Page 8351, line 25-28. The authors state that the warmer and less turbulent cloud layer in D30 is due to the water vapor solar absorption. I think this is very speculative; and I am not convinced. Many processes may contribute to that. A likely cause is drizzle. Because D30 produces more drizzle than D500, the cloud layer may be warmer due to less evaporative cooling available and the subcloud layer cooler due to more evaporation.

We agree that drizzle in D30 causes decoupling of the cloud layer and contributes to the warmer and less turbulent cloud layer, which is illustrated in the figure by comparing two solid lines (representing nighttime). In both D30 and D500 cases, the cloud layer is warmer in the day (dotted lines) than at night (solid lines), showing the solar heating. Even larger difference between the two dotted lines (representing daytime) suggests that solar heating may be stronger in D30 although cloud cover is smaller than in D500 and drizzle rate is smaller in the day (see Fig. 3c), indicating that water vapor absorption is also very important in heating the layer.
To avoid confusion, we rephrase the sentence as:
"This together with the effect of drizzle explains why the cloud layer in D30 is even warmer and less turbulent (see w variances in Fig. 4c) than in D500"

Page 8355 section 3.3. This section is too descriptive and sounds speculative; it does not provide enough information for the results described here. For example, some quantitative data should be given on the "two reasons" for the stronger sensible flux effects. Readers would have difficult time to be convinced about these results.

Our calculations support the statements made in the text: First, accumulated moisture near the surface is effectively transported to the cloud layer by enhanced buoyancy of thermals due to stronger surface sensible heating. Second, a temperature gradient induced by stronger surface heating in the inner domain generates a circulation with convergent flow in the lower boundary layer that pumps moisture from the adjacent surface layer. The finding of the importance of this type of mesoscale circulation induced by a horizontal temperature gradient in a large domain is one of the novel aspects of this study. To illustrate the importance of these mesoscale circulations, we added a new Fig. 7 (same as Fig. R5 in the response to Referee #3 comment 5) for experiment PQVT to the revised manuscript. Here we show two similar figures for experiment PSFX and PLFX.

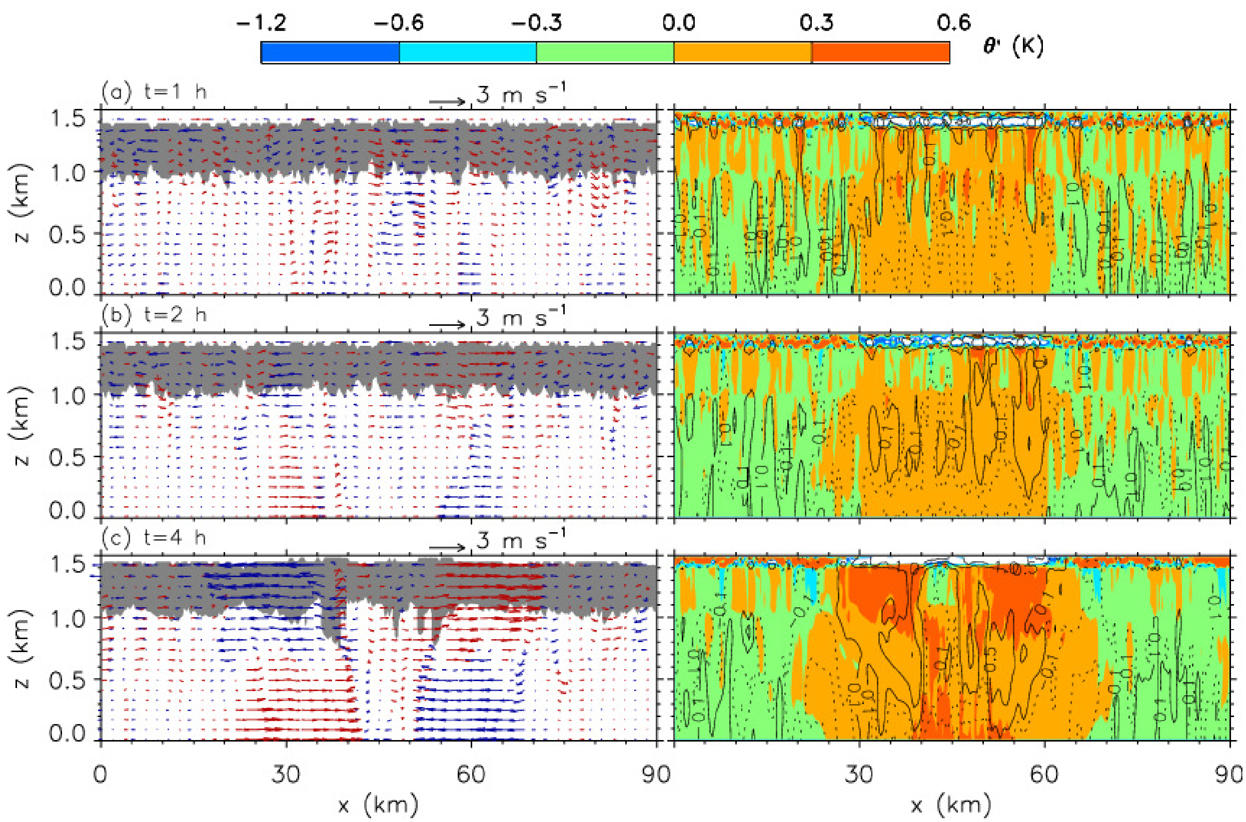

Figure R3: The x-z cross-sections (at y=45 km) from experiment PSFX at (a)-(c) t = 1, 2 and 4 h. (left) Gray shaded areas denote clouds (cloud water mixing ratio > 0.01 g kg^{-1}); arrows represent wind perturbations (u-w) with red for westerly (u>0) and blue for easterly (u<0) to demonstrate the inflow in the lower-level and divergent flow in the upper-level associated with the positive surface sensible heat perturbation in the middle of the domain (originally between x = 30 and 60 km). (right) Shaded colors indicate temperature perturbations, and contours water vapor mixing ratio perturbations (positive by solid lines and negative by dotted lines); perturbations are relative to the horizontal slab average at each level. The circulation transports water vapor into the initially perturbed area, and clouds become thicker in the affected area as the water vapor is enhanced. As the perturbation is persistent, the circulation becomes stronger at a later time.

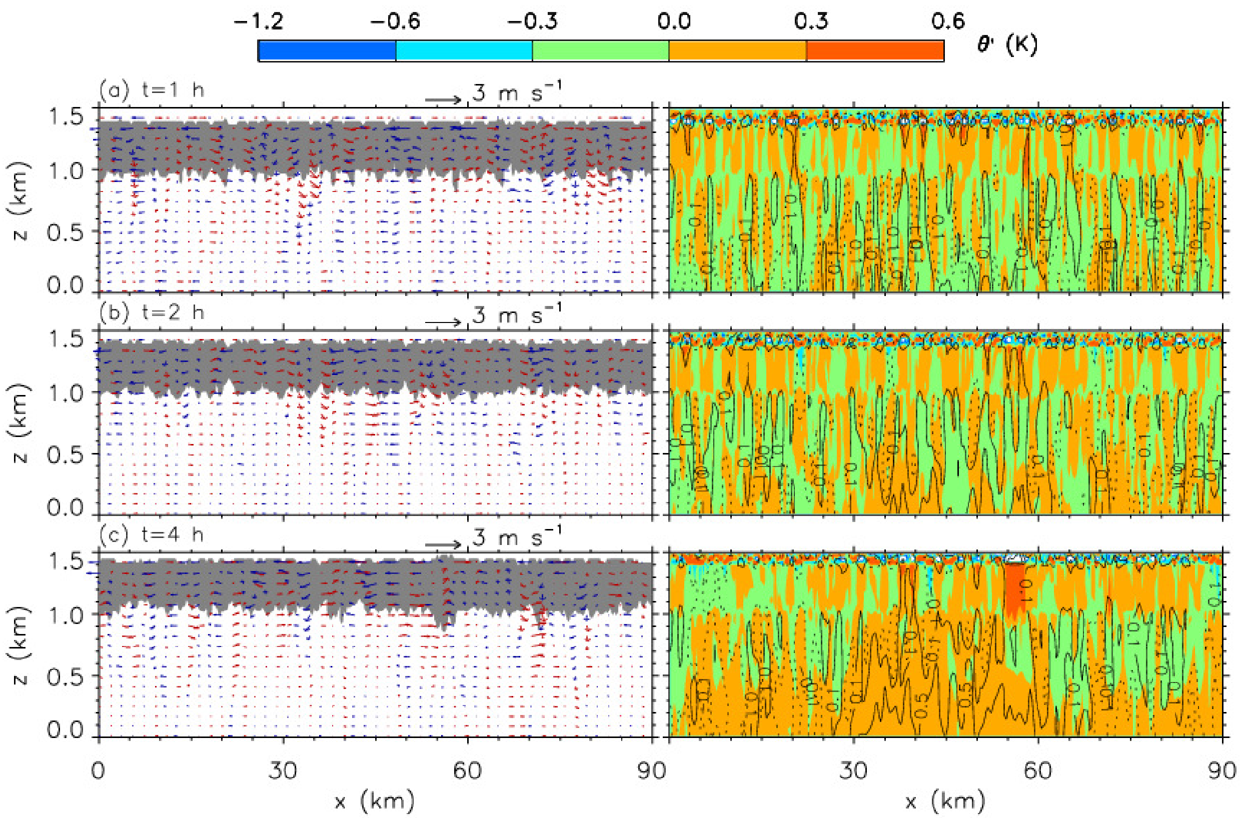

Figure R4: Same as Fig. R3 but for experiment PLFX. The increase in surface latent heat flux does not drive a circulation as seen in Fig. R3. The added water vapor is not effectively transported to the clouds but rather accumulates in the subcloud layer; visually clouds are not thickened yet by t=4 h.

Page 8368, Table 1. "Delta qv + Delta theta" is confusing. It can be changed to ""Delta qv and Delta theta". Same modification may be applied to "Dela SFX + Delta LFX"

Done as suggested.

Anonymous Referee #3

General comments:

The paper addresses the problem of POC formation and its relationship to environmental conditions by using a cloud resolving model and studying sensitivities to CCN concentrations, temperature and humidity profiles, surface sensible and latent heat fluxes, and large scale subsidence. It is a well written paper that presents interesting and valuable results about a relevant problem.

Specific comments

1) One frustrating aspect of reviewing this paper was that among its important references there are 3 papers "in preparation", which are not available for inspection. These papers were especially missed when trying to appreciate the basis for selection of cases or conditions used in the model setups. Therefore, the authors are requested to provide more specifics when using results from these papers, e.g. p.8344,l.9; p.8356,l.10; p.8347 l.14;

Since this paper has been submitted to a special issue, cross-citing of manuscripts in preparation is inevitable, even in an on-line journal like ACPD where the other papers are more readily visible. Therefore we have attempted wherever possible to provide sufficient detail on results from other papers. For example, we refer to Bretherton et al. (2010; p.8344 l. 9) for a generally dry free-troposphere over the VOCALS-REx domain. The sounding in Fig. 1 demonstrates this point for the case considered. We refer to Wood et al. (2010; section 2) for the RF06 measurements. The case has been adequately described for this purpose, although not as comprehensively as in Wood et al. (2010), which has now been submitted and is expected to be available online soon. We now add more specifics about the observed size distribution of accumulation-mode aerosol in RF06.

With the permission of the authors, we would be willing to provide, upon request and via the editor, drafts of the relevant manuscripts for inspection.

2) Thermodynamic initial profiles. Figure 1 shows vertical profiles of thermodynamic variables and winds used to initialize the model. Based on these profiles my estimates indicate a very high supersaturation at the top of the MBL (_200% in the wet case). Please indicate the initial supersaturations in wet and dry cases and comment on the initialization procedure and what impact it might have on the results. Figures 3a and 3b show that some cases have very important changes in the first hours of the model runs: is that related to the initialization shocks?

The initial soundings are associated with cloudy boundary layers, which forces the initial supersaturation in both dry and wet cases to be unrealistically high. This is standard practice in the GEWEX Cloud System Study (GCSS) Boundary-layer Cloud stratocumulus case studies (e.g., http://www.knmi.nl/~siebesma/BLCWG/). The initially highly supersaturated sounding produces a stratocumulus deck, and in less than 10 minutes, the condensation of water onto the aerosol takes up the excessive water vapor and reduces the supersaturation to realistic levels. The release of latent heat changes the assumed potential temperature (i.e., liquid water potential temperature) profile to be consistent with the observed one (see Fig. 11 thick gray lines). At t = 10 minute, the supersaturation is quite reasonable (less than 1% in the domain). Once turbulent eddies in the model have fully developed, the supersaturation and liquid water content will adjust accordingly. As seen in Fig.3, the rapid changes of cloud properties in the first hour are related to the adjustment process but not the initial “shocks”. Usually, results during this model spin-up time are not seriously considered.

3) Missing references, un-justified statements, or missing information:

p. 8347, l.12-13: provide a reference for MBL aerosol composition

This was just assumed in the microphysics scheme for simplicity. According to Wood et al. (2010), subcloud-layer aerosols consist mainly of sea salt. We now change it to sea salt.

p.8347, l.16-17: why are these assumptions unlikely to have significance influence on the results?

This is based on our extensive work addressing the relative importance of aerosol composition, size distribution and number concentration (e.g., Feingold, 2003; GRL and Ervens et al., 2005; JGR). These works showed that the aerosol number concentration is the most important parameter affecting cloud microphysics and that composition is almost always far less important than number concentration, updraft velocity, and aerosol size distribution. This is a result of dynamic readjustment of the system: more soluble particles activate more readily but take up more water vapor and reduce the supersaturation, which counters the tendency for higher activation.
For the purpose of our sensitivity tests, the prescribed aerosol size distributions do not precisely represent the real case, so there is no reason to pursue more accurate composition and size distribution.

p.8348, l.5 and 10: at what height are these vertical velocity values set? (also at p.8362,l.26).

Through the depth of boundary layer. This is now clarified in the manuscript.

p. 8351, l.18-27: there are several processes described but too little explanation of how these processes are appreciated based on what is shown in figure 4. Please provide a more clear connection between the figure profiles and the processes mentioned in the paragraph. figure 4: I see very little discussion of figure 4c) and none of figure 4d). The striking contrast between <w3> in D30 and D500 deserves a comment in terms of what they tell about turbulence structure in both cases.

In response to this and relevant comments by referee #1, we have added the following sentences to discuss about Fig. 4:

Turbulent mixing in the day is weaker, as indicated by the variance of vertical velocity $\overline{w'^2}$. The strong day-night contrast is consistent with simulations by Caldwell and Bretherton (2009). It is noteworthy that $\overline{w'^2}$ would tend to be underestimated in the simulations due to the coarse horizontal grid size (Wang et al. 2009), particularly, during the day when solar heating is likely overestimated by the radiation model. Nevertheless, comparison between early morning aircraft measurements of $\overline{w'^2}$ and nighttime model-derived values show surprisingly good agreement; peak nighttime modeled values of $\overline{w'^2}$ are approximately 0.5 m^2 s^{-2} and 0.2 m^2 s^{-2} for closed and open cell conditions (Fig.

4), as compared to rough estimates of 0.6 $m^2 s^{-2}$ and 0.3 $m^2 s^{-2}$ for early morning observations (Wood et al., 2010). As discussed by Stevens et al. (2005a), predominantly negative $\overline{w'^3}$ in D500 indicates strong downdrafts driven by radiative cooling which is largely reduced in the day. Positive $\overline{w'^3}$ in D30 is indicative of cumuliform convection. The sub-cloud negative $\overline{w'^3}$ in D30 at night is contributed by downdrafts associated with precipitation. Stabilization and decoupling of the cloud layer in the day causes the flat $\overline{w'^3}$ profile in both cases.

p.8353, l.9: the parenthetical values for PQV and PCCN are a mystery to me.

It's CCN number concentration used in PQV and PCCN. Here it's just a reminder. We removed them from the revised manuscript since it causes confusion.

p.8353,l.10: results at t=4h are described, but should say "not shown in fig. 5".

Added as suggested.

p.8356, l.20-22: provide a reference for the models that showed this.

The references are Garreaud and Muñoz (2004) and Rahn and Garreaud (2010). They have been added to the statement.

p.8358,l.14: provide a reference for the VOCALS source strengths

The original VOCALS source strength (2 $cm^{-3} h^{-1}$) is not published yet. We are not able to verify it, so it has been removed from the manuscript.

Instead, we provide an estimation of new particle production by sea-spray production of ~ 1.6 $cm^{-3} h^{-1}$ using the Clarke et al. (2006) parameterization (Kazil et al. 2010, manuscript in preparation). This is now added to the paper.

p.8360,l.17: please specify to what "latter" refers to.

It refers to the "wetter" condition. "latter" is now simply changed to "wetter conditions".

cloud bases: please provide information on cloud base heights (e.g. in connection with Figure 2b and/or figure 3)?

In the model output, a "cloudy grid box" is defined as where cloud water mixing ratio is ≥0.01 g kg^{-1}. Based on this, in each model column cloud base is the lowest cloudy grid box and cloud top is the highest cloudy grid box. Cloud-base rain rate is the rain rate at cloud base.

Observed cloud top is derived from upward-looking radar measurements (uppermost bin where the radar reflectivity exceeds -30 dBZ). This is now clarified in the figure captions.

4) Basic definitions
I cannot find in the paper a clear statement of how POC, or open cells or closed cells are diagnosed based upon model results. For example p. 8349,l.7 calls experiments D30 as open cells and D500 as closed cells, but it is not immediately obvious why. It would be helpful to see somewhere an explicit declaration of how these different conditions are recognized from the results. If not, the paper will be useful only to those "initiated" in POCS.

In this paper open and closed cells are recognized in a quantitative sense by simulated rain rate and cloud fraction. Visually, this is done via snapshots of cloud albedo and the ring-like structure of updrafts (see Fig. 10 for an example).

In regards to the confusion near **p. 8349,l.7,** we made the following clarification:
"… are shown in Fig. 2 for D30 *in which open cells form* and D500 *in which closed cells form*."

5) Mesoscale circulations: p.8354, l.13-28: an important point of the paper is to stress the importance of mesoscale circulations to produce non-local effects and even remote POC formation. Therefore it is highly missed a better description of this circulation using the model results. I would like to see a wind vector map illustrating this circulation, in order to be able to appreciate its intensity and physical plausibility.

A similar circulation has been illustrated by Wang and Feingold (2009b) in different ways. There is some evidence of the circulation shown in Fig. 10. The following figure demonstrates the circulation we described in the text. We now add this Figure to the paper as Fig. 7.

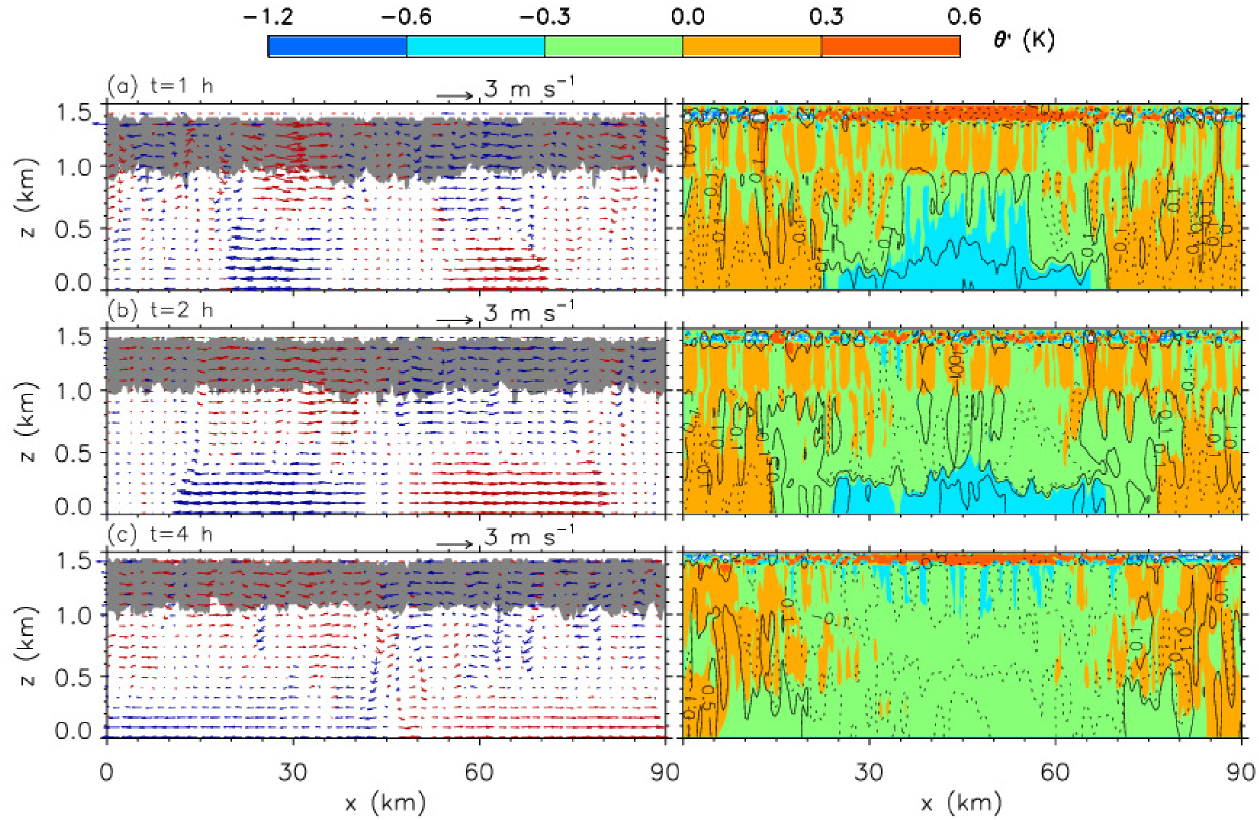

Figure R5: The x-z cross-sections (at y=45 km) from experiment PQVT at (a)-(c) t = 1, 2 and 4 h. (left) Gray shaded areas denote clouds (cloud water mixing ratio > 0.01 g kg^{-1}); arrows represent wind perturbations (u-w) with red for westerly (u>0) and blue for easterly (u<0) to demonstrate the outflow in the lower-level and return flow in the upper-level associated with the negative temperature perturbation in the middle of the domain (originally between x = 30 and 60 km). (right) Shaded colors indicate temperature perturbations, and contours water vapor mixing ratio perturbations (positive by solid lines and negative by dotted lines); perturbations are relative to the horizontal slab average at each level. The circulation transports water vapor out of the initially perturbed area and expands to the entire domain by t = 4 h, and clouds become thinner in the perturbed area as the water vapor is moved away. As the gradient becomes weaker, the circulation is weakened too.

6) Runaway process?: p.8361,l.14: the section concludes stating that POC is a runaway process. Is that really so? Looking at POCs in satellite images I see them many times advecting and deforming but seldom as a runaway expanding/growing POC. I'd appreciate a comment on this and/or a restriction of the stated conclusion.

"Runway process" refers to the positive feedback associated with precipitation process. Namely, precipitation decrease cloud drop number concentration and drives the formation of open-cell circulation (divergent-convergent flow), which in turn intensifies precipitation. Here we meant that once POCs are formed in the stratocumulus deck they cannot go back to the original cloud deck without other changes in the environment.

However, for clarification we now reword the text as follows: “This, together with the monotonic decrease in aerosol concentration associated with coalescence scavenging and precipitation make POC-formation an un-buffered process.”

Technical corrections

Thanks for the very careful reading!

p. 8343, l. 17: Stevens et al. appears to be 2009 and not 2005?

It is 2005, but there was a mistake in the reference which has been corrected.

p. 8346, l. 6: eliminate "the both"

Done.

p. 8346, l. 3: correct the spelling of "previous".

Done (at p. 8347, l.3).

figures: most of the multi panel figures lack naming the panels as a), b),... (e.g. fig. 1, 4, 10,11), and others have names but are not used in the caption (e.g. 2). Make sure all are labeled and referred to in the captions.

Done.

p.8366,l.2: Hartmann with 2 n’s.

Done.

figure 4: please add the location of "the cloud layer" as described in the text.(shading of the "cloud layer" would help).

It would be very messy because there are four of them in each plot. Additionally, the cloud layer evolves during the 6-hour period in which average profiles are taken. Here in the text we really mean the broad concept of the cloud layer, i.e., the upper boundary layer. We add “(i.e., upper boundary layer where clouds present)” after the first appearance of “cloud layer”.

figure 9: start caption with uppercase

Done.

figure 9: there are dotted lines but nowhere explained.

They can be identified just by colors. The dotted lines overlap with the purple solid line that represents case D30 before t = 18 h. The “dotted” line style is used so that the purple solid line is not completely covered and becomes invisible. This is now clarified in the figure caption.

figure 9: should warn the reader that the time axis is different as Fig.3

Done.