**ACP-2009- 731**
**Response to Referees**

We appreciate the constructive suggestions made by Anonymous Referees 1 and 2, which have improved the revision of our original manuscript.

We also realized shortly after submission of the original manuscript that the LES cloud-base precipitation rate was being diagnosed at the lowest height of any cloud, which is a biased estimate of cloud base. We now use the median cloud base across all LES grid columns to diagnose the cloud-base precipitation rate throughout the paper. This more than doubles the cloud-base precipitation rate in our simulations compared to the old approach, reducing the dry bias of the LES precipitation parameterization compared to observationally-based estimates.

**Response to Referee 1**

<u>Response to general comments</u>

1. We agree that it is 'not unexpected' that if (a) an LES simulates a boundary layer that looks well mixed and (b) if the MLM parameterizations are tuned to optimally match the LES, then the MLM and LES should exhibit similar sensitivity to droplet concentration, and we acknowledge more clearly at various points in the revision.

2. We feel that the aims of this study are comparable to those of Sandu et al. 2008. As the referee notes, there are some complementary aspects that we have tried to more fully acknowledge in the revision through various small textual changes. Their study considered a more complex situation including a diurnal cycle in which mixed-layer assumptions break down during part of the day.

Our results suggest that their main finding, that the response of the MLM-predicted cloud thickness to aerosol changes is opposite to that of the LES, is fundamentally driven by a factor not included in their chosen MLM entrainment closure (cloud droplet size or 'sedimentation' feedbacks on entrainment). By focusing on a case in which the boundary layer remains well-mixed throughout the simulation, we can get the most basic comparison of the MLM and LES sensitivity to droplet concentration. The referee asserts that 'Nevertheless, they [Sandu et al] acknowledge the fact that the main reason for which their MLM gives a LWP change of opposite sign compared to the one of the LES is the fact that the entrainment parameterization does not correctly account for the dependency of entrainment on the strength of the condensed water sedimentation, and that it does not account for the redistribution of latent heat within the boundary layer by the precipitation.' We fully agree with this interpretation of the issue with their MLM, but we cannot find where in the paper Sandu et al make these statements. In fact, at the end of their Sec. 4.1.3 they downplay the importance of sedimentation feedbacks, stating the following: '*It might be surprising that parameterizations that explicitly include a sedimentation effect did not better match our simulations. However, initial estimates of sedimentation effects on entrainment tended to exaggerate this effect by assuming an unrealistically broad droplet spectrum. Moreover sedimentation effects are implicitly captured by many of the parameterizations as they help determine the buoyancy and liquid water flux profiles which the parameterizations use as input.*' Our sedimentation parameterization uses a conservative estimate of the droplet spectral width and our MLM accurately includes feedback of entrainment on the buoyancy flux profile; nevertheless our Fig. 8 shows the sensitivity of droplet concentration to LWP almost disappears in the MLM without inclusion of a direct sedimentation feedback in the entrainment closure.

3. In the revision, we replace 'observationally-based default' with 'best-guess'. We fully agree that the observational estimates of entrainment and precipitation have large uncertainties, and we agree that there is no direct observational evidence of a droplet-size feedback on entrainment. Nevertheless, we do feel it is appropriate to question the extent to which LES-based studies of aerosol cloud feedback are trustworthy and to raise the level of awareness of the importance of likely LES microphysical and entrainment biases.

Response to specific comments

1. Have added the italized words: 'Low aerosol concentrations lead to *marine boundary layers with* reduced cloud fraction, more precipitation and a more decoupled and cumuliform character' – we hope this makes more sense to the referee.

2. To address this referee point, we have added a clause ', because they involve a subtle interplay between turbulence, vertical structure, entrainment and evaporating precipitation.' after '…global models struggle to represent', and a sentence 'Intermediate aerosol concentrations can support fully cloud-covered but drizzling boundary layers in which subcloud cooling from evaporating drizzle inhibits turbulent mixing and reduces entrainment of dry air, supporting stratocumulus with a high liquid water path, e. g. \citep{LuSeinfeld2005}.'

3. Have revised wording to 'sometimes' following referee suggestion. As the referee points out, exact well-mixedness is an idealization anyway, so using 'fairly' seems appropriate in this context.

4. We totally agree with the referee, who answers his/her own question here. We have added wording to that effect. Unquestionably, a serious limitation of using MLMs to study Sc aerosol indirect effects is that they occur during daytime when the PBL is most likely to be decoupled.

5. We've added a description and citation for the LES advection scheme, and mentioned that the radiative heating parameterization will be discussed in a later section.

6. An overlying low stratus cloud greatly decreases the radiative cooling of underlying clear moist air, so we think our idealization is appropriate.

7. The GCSS RF01 case specs tried to capture the strong stratification that was observed above the Sc layer. However, the specification was designed for simulations of a few hours, not long simulations in which the inversion that may move up and down several hundred meters. In addition, it is hard to cleanly compare with an MLM because with the DYCOMS-II specification, the inversion strength becomes a bit ambiguous. This is why we used a simpler specification that has a similar effect.

8. Following the referee suggestion, we now say 'The 5~day simulations are long enough to see the boundary layer depth and cloud thickness evolve much of the way toward an equilibrium.'

9. To clarify both of the referee's points, we reword as follows:
'This manifestation of over-entrainment is an important systematic error of the LES for cloud-aerosol-precipitation interaction. Firstly, too thin a cloud will not precipitate as easily. Secondly, the total longwave cooling across a thin Sc layer is much more sensitive to a given change in LWP than for a thick Sc layer (e. g. Fig. 7 of Stevens et al.

(2005b)). This distorts the feedback between aerosols, LWP and the radiative driving of the boundary layer.'

10. We have rewritten this paragraph using the referee's three suggestions.

11. The paragraph called out by the referee is about how entrainment in our simulations can increase with *N*, even though LWP decreases with *N*. To our reading, the Stevens et al. 1998 paper refers to the effects of drizzle rather than sedimentation on entrainment. Their 'asymmetric vertical currents' discussion discusses why the buoyancy flux and TKE are less in the upper part of a drizzling boundary layer compared to a nondrizzling boundary layer. In their case, the drizzling boundary layer has less LWP *and* less entrainment. While we have no problems with their discussion, we don't think it is necessary to include here.

12. To address the referee criticism, we have replaced 'Specifically, we can define' with 'We quantify this argument by defining…'

13. Have added '1—5 day average' before 'value' to address referee query about time range over which estimates are being made. To address the other referee comment, we have reworded to 'The reduction in convective velocity is accomplished by decreased LWP, which causes cloud thinning and lower fractional cloud cover (less radiative driving), both of which diminish the vertically-integrated buoyancy production of turbulence.'

14. Our point was different, not about why the high-N entrainment efficiency is larger, but instead about how this is manifest in simulations. We have reworded to clarify: 'That is, the larger $N = 150$ entrainment *efficiency* does not lead to a larger sustained entrainment *rate* compared to N=30. Instead, the N=150 cloud layer entrains at about the same rate as N=30, but accomplishes this entrainment with less turbulence and lower LWP. '

15. Decoupling doesn't have to reduce Sc cloud fraction, while horizontal inhomogeneity should promote gaps in the Sc.

16. Caldwell and Bretherton (2009) tuned the entrainment efficiency based on DYCOMS cases RF01 and RF02 and the EPIC diurnal cycle of Caldwell and Bretherton (2005). This is noted in a sentence added to the MLM model formulation section 2.2 of the revision.

17. We did not keep time series of the MLM entrainment efficiency for our simulations, but in the GCSS RF01/RF02 cases the MLM entrainment efficiency was roughly half as large as the LES.

18. We acknowledge the referee's point, while noting this terminology of drizzle being 'stabilizing' is commonly used both to discuss LES and MLM results for which the result is still a well-mixed PBL with somewhat reduced convective turbulence (e. g. Wood

2007 JAS). We have reworded this sentence to avoid use of the word stabilizing: 'The N=50 MLM simulation generates enough evaporating cloud base drizzle to significantly reduce buoyancy production of turbulence and entrainment.'

The runaway drizzle feedback is illustrated in the Figure 3 that is under discussion, so does not require reading between the lines. In revision, we have made closer reference to the figure to make this clear.

19. In the revision we clarify that the values of $w_{sed}/w_*$ were estimated using the profiles in Fig. 2, which are 2-2.25 day averages from the LES simulations.

20. We experimented with several presentations of Figure 4, including the one suggested by the referee. We prefer the existing presentation because it clearly shows the distinctly different entrainment efficiencies of the four LES cases, while also indicating the quality of our fit.

In response to the second subcomment, we have revised 'It is reassuring that…' to 'It is reassuring (though not surprising) that…'.

21. To clarify the referee's query, we've revised this paragraph to 'The MLM does not show as much sensitivity of inversion height to N as does the LES. The separation of LES inversion heights for different N occurs between 1.5 and 2 days, when the cloud in the N150 simulation becomes thin and the cloud fraction decreases to 80%. This reduces the boundary layer radiative driving for turbulence, the entrainment rate, and ultimately the inversion height compared to the N30 simulation, in which cloud cover remains 85% or higher throughout. A comparable sensitivity of radiative driving to cloud thickness cannot occur in the MLM, where by construction the cloud fraction remains 100% even as the cloud becomes very thin.'

22. They are 16 h-5 d averages, as clarified in the revised text.

23. Have revised to add ', at least for our modest LES domain size' to indicate that the threshold was based on our LES results. We don't know how well it would apply with a larger LES domain.

24. Fig. 8 of S2009 shows MLM simulations that both include and neglect surface precipitation. In both cases, their simulations show a thinner cloud for lower N. They contrast this with their LES simulations (all of which include a parameterization of precipitation), which show the opposite effect. In particular, their Fig. 8a does show a comparison of two nonprecipitating MLM simulations, so the referee needs to clarify what is meant by this comment.

25. Sentence has been revised to 'Interestingly, their LES has a similar diurnal change of the LWP difference between low and high N as in the MLM, even though the daily-mean LWP difference is quite different than for the MLM.'

TC1. Thanks for this suggestion, which we are following.

TC2. Our usage is correct according to the style guide at
http://www.colorado.edu/Publications/styleguide/abbrev.html
The pronoun should harmonize with the way the *acronym* would be said in speech, not with the way the abbreviated phrase would be said.

TC3. Corrected.

TC4. Corrected.

TC5. Corrected

**Response to Referee 2**

Overview

The referee implicitly criticizes the use of an LES whose drizzle parameterization does not agree well with an observationally-based fit, and the use of a single idealized sounding for this sensitivity study. We feel that these limitations, while real, do not greatly reduce the value of our study.

The GCSS DYCOMS RF02 nocturnal drizzling stratocumulus intercomparison of Ackerman et al. (2009) showed that the SAM LES which we used reproduces the cloud base drizzle rate at least as well as most other LES in that study. No study has observationally tested empirical drizzle parameterizations at the much lower LWPs that we simulated, so the discrepancy between observations and model results may not purely result from model bias. In addition, for light drizzle, the value of cloud-base precipitation is very sensitive to the definition of cloud base, which is different between studies.

With regard to a choice of case study and initialization, it makes sense to first compare a MLM and an LES in a situation that both observations and prior simulations suggest will be a well-mixed boundary layer. Our paper has established (for the first time, we believe) what it takes to make an MLM and an LES agree on the sensitivity of such a boundary layer to changes in cloud droplet concentration. That this requires retuning of the MLM is interesting. To our eyes, that we can reproduce the LES response of LWP to N to within 25-50% using the MLM is a useful level of agreement. This is especially true in comparison with all the other much larger uncertainties in quantitatively predicting aerosol effects on clouds and climate.

Specific comments

1. This comment is multifaceted, so we respond to it in parts:

(a) Can the MLM reproduce the LES for a drizzling mixed layer?

OUr LES does not readily produce well mixed layers with heavy surface drizzle, and in fact these are rarely observed. Our best case of an observed heavily drizzling mixed layer is from the POC environment of Research Flight RF06 of VOCALS-REx. We will present LES modeling results on this case separately; the MLM does a qualitatively good job of reproducing the LES evolution over an 8-hour simulation period before daytime decoupling sets in. We have not found a simple perturbation of the DYCOMS RF01 case for which the LES gives a heavily-drizzling mixed layer, which is why we haven't added such a case to this paper.

(b) LES drizzle biases?

The discrepancy between the LES-simulated cloud base drizzle rates and the Comstock et al. (2004) parameterization is of concern (and a bit of a puzzle to us), but as we point out in the revised paper, the Comstock et al. (2004) parameterization was fitted based on observations of heavy drizzle and may be quite inaccurate for very light drizzle rates such as in our simulations. From the Ackerman et al. (2009) nocturnal drizzling stratocumulus intercomparison, we have every reason to think our LES gives as good a representation of heavier Sc drizzle as most other LES with bulk microphysics that are in active use today.

2. We have added a paragraph to the LES model formulation section in accordance with the reviewer's valid point:
' The LES neglects the finite timescales both for mixing of cloudy and subsaturated air within a grid cell, and for the evaporation of cloud droplets in subsaturated air . Both these effects decrease the efficiency of cloud-top entrainment in the LES, because they slow down the evaporative cooling of air undergoing turbulent mixing in the entrainment zone (Hill et al. 2009); the finite evaporation timescale is sensitive to droplet size and would act to enhance the feedback of sedimentation on entrainment simulated by our LES (to which our MLM is also tuned).'

3. In the revision, we have explained $\rho_a$ and $\rho_w$ . The other symbols mentioned by the referee were in fact already defined at their first appearance (inversion height $z_i$ on p25857, entrainment rate $w_e$ on p25858, and $h+$ and $q+$ on p25860.)

4. The units in Eq. (5) have been revised by adding an extra m s$^{-1}$. $N$ has units of m$^{-3}$ and LWP has units of kg m$^{-2}$. Hence $(\mathrm{LWP}/N^2)^{1/3}$ has units of kg$^{1/3}$m$^{4/3}$. Eq. (5) is included since it is how we diagnose inversion-base $w_{sed}$ from the LES. This is explained in the revision.

5. We are following the same conventions for defining $z_i$ and $w_e$ as in the GCSS DYCOMS RF01 paper. We have revised the definition of convective velocity to be more precise.

6. We've revised the wording from 'thin clouds are more radiatively susceptible' to 'the total longwave cooling across a thin Sc layer is more sensitive to a given change in LWP than for a thick Sc layer (e. g. Fig. 7 of Stevens et al. (2005))'. In this reference the net boundary layer radiative flux divergence $\Delta F_{rad}$ and the entrainment rate were plotted vs. LWP for all the LES models in the GCSS DYCOMS RF01 intercomparison. For models with LWP < 30 g m$^{-2}$, $\Delta F_{rad}$ and $w_e$ decreases as LWP decrease, while for LWP > 30 g m$^{-2}$ this was not the case. That is, as the simulated cloud gets thin and develops more breaks, it is no longer opaque in the infrared and the radiative driving of the boundary layer becomes more sensitive to the cloud thickness. A similar phenomenon is clearly seen in our simulations. Because they include some clear-sky cooling in columns with thin or no cloud, our simulations are not quite as sensitive to cloud thickness, especially for 20 < LWP < 30 g m$^{-2}$.

7. We have revised our wording to 'The 5~day simulations, which we refer to as N150, N50, N30 and N10, are long enough to see the boundary layer depth and cloud thickness evolve much of the way toward an equilibrium.' We have now extended the simulations

out past 10 days, and they do reach equilibrium at a LWP similar to the 5 day value. During the 5-10 day period, the LES boundary layer becomes slightly more decoupled because the buoyancy flux drops nearly to zero everywhere below the cloud base. Hence the LES LWP does not increase as fast as in the MLM. At ten days, the relative sensitivity of LWP to N is similar to at 5 days, so we are just sticking with the 0-5 day results to avoid these complications.

8. We've revised the statement to avoid reference to a specific LWP threshold. The relation between broken, thin cloud, less radiative cooling and lower entrainment, which was what we were trying to emphasize, is not dependent on such a threshold.

9. In revision, we've added the following explanatory statement about the 2-2.25 day time interval used to make the plotted profiles:
'This time was chosen to be long enough to allow all simulations to adjust into a phase in which their cloud properties are no longer rapidly evolving, yet short enough that they all have inversion heights less than 150~m apart, and hence similar above-inversion conditions and inversion jumps.'

10. As stated in the revision, no LES results were plotted for the first two hours, because this is a spin-up phase. During that time, the LWP drops from 60 to 30-35 g $m^{-2}$.

11. We have revised to the following: 'Entrainment drops to zero after 8 h because convective velocity (which is the MLM predictor of boundary-layer turbulence) drops to zero. Without turbulence, the boundary layer will not remain well-mixed, so the MLM is no longer applicable. Hence no MLM results are plotted after this time.'

12. Done

13. We did not weight the N=10 case highly in tuning $a_2$ because this LES run was far from well-mixed – the revision mentions the agreement is less good for this case. For N= 150, it is a judgment call whether $a_2 = 110$ gives a bad fit. There are red crosses at abscissas between 50 and 160, with the fit line at 100. As already acknowledged in the paper, the MLM entrainment closure does a far from perfect job of predicting the hourly LES entrainment rate at any value of N. A Lilly-type entrainment closure weighted toward the buoyancy production in the upper part of the mixed layer would probably have slightly more skill, but we decided that our tuning of the MLM to the LES was adequate to make the basic points of this paper.

14. To capture the spirit of the referee's comment and better describe the Bretherton et al. (2007) simulations used to tune $a_{sed}$, we've revised 'but a different set of cases' to 'using a different set of sensitivity studies based on the unmodified GCSS DYCOMS RF01 case'

15. As pointed out in the revision, the observational fit line is an extrapolation from heavily drizzling clouds with substantial mesoscale horizontal inhomogeneity and much higher LWP (100+ g $m^{-2}$) than those we are simulating (~15-30 g $m^{-2}$), so significant

disagreement of the LES-simulated drizzle rate with this fit line may not be as serious as the referee is implying.

16. Reference corrected to Ackerman et al. (2009)…our mistake.

17. The word 'qualitatively' is used indiscriminately in our field, often to characterize much poorer agreement than that between our LES and MLM sensitivities to N. Thus we want to avoid using 'qualitative agreement' in the paper. However, we acknowledge the referee's point in the penultimate sentence of the introduction, which now reads: 'for stratocumulus-capped mixed layers, the sensitivity of LWP to cloud droplet concentration is comparable (within less than a factor of two) for our LES and an appropriately configured MLM'.

18. In Ackerman et al. (2009)'s intercomparison study, the relation between LWP and subcloud drizzle rate in the SAM LES (using exactly the same microphysical parameterization as we do) compares better than most other LESs with an observational best guess. It seems inappropriate to us to emphasize apparent microphysical biases of this LES that are neither proven nor outstanding compared to other LES models. See response to comment 15.

19. We've added a reference to Fig. 2 of Bretherton et al. (2006), which shows a vertical profile of the reduction in buoyancy flux due to evaporating drizzle.

20. Revised to 'To obtain results comparable to the LES, the entrainment and drizzle parameterizations of the MLM must be carefully tuned to the LES.'