Response to the comment of the anonymous referee #4

We thank the referee for his comments, which have helped simplify our approach. Please find below our response (the referee's comments are in italic).

General comments:

As stated in the abstract, the main hypothesis in this paper is that "differences between shallow and deep convection, regarding cloud-base properties as well as entrainment/detrainment rates, can be related to the evaporation of precipitation." If it is true that the differences between shallow and deep convection are related to the evaporation of precipitation, then the shallow scheme should do a good job of parame- terizing deep convection that has no precipitation evaporation. This could be simulated in SAM by turning off precipitation evaporation in the microphysics. That might be a more direct test of the hypothesis.

The importance of precipitation evaporation for deep convection has been demonstrated in several LES studies (e.g., Khairoutdinov and Randall 2006) by turning off the latent heat of vaporization, as suggested by the referee's comment. The results of these studies were mentioned in the manuscript for instance on page 8395 lines 8-16 and on page 8400 lines 20-21. We will add a further reference to these studies also in the introduction. It is not the goal of this study to repeat such analysis, rather we wish to use such ideas for parameterization development.

The entrainment equations (3-8) with their 14 tuning parameters (some of which have three significant digits!) are so complicated that I am afraid that the essential physics of the problem has been obscured. This makes it easy to dismiss the agreement between the simulations and the observations as achievable with any entrainment model that has 14 free parameters. The paper would be improved by simplifying these equations.

We simplified the equations to the following set (and will update section 4.1 accordingly):

$$z_1 = z_{cb} + 2000\,m \quad (1)$$

$$\varepsilon_o(z_{cb}) = 4.1 \cdot 10^{-3}/(\rho_{cb} g w_{cb}) \quad (2)$$

$$\varepsilon_o(z_1) = \exp(-8.3) \cdot \max(RR_{cb}, 0.1)^{-0.2} \quad (3)$$

$$\varepsilon_o(z) = \varepsilon_o(z_{cb}) \left(\frac{z}{z_{cb}}\right)^{\propto} \quad (4)$$

Equation (4) describes the assumed general profile of ε_o. It is fully determined if ε_o is known at two heights, here chosen to be at z_{cb} (see Eq. 2) and z_1 (see Eqs. 1 and 3). Eq. (2) corresponds to Eq. (5) in our manuscript, while Eq. (3) is obtained in a similar way as our previous Eqs. (7) and (8).

This set of equations retains the essence of our previous Eqs. (3-8), i.e. a bulk entrainment rate which varies with height and which decreases with increasing precipitation. At the same time it allows a smoother profile in the vertical (than a layer-based version) and only contains two main free parameters (entrainment at the two anchor heights z_{cb} and z_1) . The expression cannot be simplified further: We need at least two anchor heights to define the vertical profile since $\varepsilon_o(z)$ can increase or decrease with height depending on the situation. Finally, the coefficients in Eqs (2) and (3) are obtained from a linear regression analysis, which is the simplest way for analyzing data. The simplified set of equation is in terms of $\varepsilon_o(z)$ slightly less accurate than the previous version but this doesn't seem to negatively impact the results. We reran all our experiments and found similar results.

It is easy to dismiss entrainment/detrainment rate formulations since they are difficult to verify and well-known tunable parameters in convective parameterizations. Even if our approach is admittedly empirical, as acknowledged on page 8400 lines 10-14, we tried to build in some theoretically expected relationships. Also, we didn't arbitrarily tune the coefficients (as often done) but tried to constrain them based on different LES simulations.

The manuscript does not explain how the "chicken and egg" problem is solved in the convective parameterization. This is a parameterization that predicts rainfall, but requires the rainfall rate to predict it. How is this solved? Is the rain rate from the previous time step used? If so, the scheme would depend unphysically on the time step. Are the equations solved iteratively? If so, is the adjustment (to an equilibrium between rainfall and convective fluxes) assumed to be instantaneous? Since the objective is to look at the diurnal cycle, does this introduce timing problems?

Yes we should have been more explicit about our implementation. We use the rainfall averaged over the last hour because of the on/off nature of convection schemes and to avoid any time step effect, as noted by the referee. We also tried to avoid any iterative loops inside the scheme to update the precipitation. This avoids timing problem in the diurnal cycle due to the scheme trying to adjust in the loops (instead of deepening with time), as also rightly noted by the referee. We will add two new paragraphs, one at the beginning of section 3.2 and one at the beginning of section 4.2 to describe and discuss our implementation.

Specific comments:

Eq 1: What is the theoretical justification for this expression?

The theoretical justification for this expression was given in section 3.1, see especially page 8395 lines 17-24 and page 8396 lines 18-22.

Fig 3: The units on the color bar do not make sense. Are the units g/mˆ2/s/K?

No, the units are actually g m^{-2} s^{-1} bin^{-1}, we will correct this.

Fig 3: Should "solid line" in the caption be "black line"? They are all solid lines.

Yes we will change it to "black line".

Fig 3: If the "black line" is cloud fraction, why is it plotted on a temperature axis?

The black line is cloud fraction. The intent was to give an indication of the shape of the cloud fraction (especially to make the cloud base more easily recognizable) but since this seems to have caused some confusion, we will adapt the x-axis in Fig. 3.

Fig 3: The green lines are very hard to see.

We will change the color of the green lines.

Eq 2b: Why parameterize the effect of convective downdrafts on the sub-cloud-layer MSE when the convective parameterization does not have a representation of convective downdrafts? This equation appears to be correcting a mismatch between MSE_cb and <MSE> that does not exist in the parameterization.

Our approach recognizes that cold pools, whether created by subcloud evaporation as we can do in CAM, or created also by organized convective downdrafts as visible in our ARM LES simulations, generate boundary layer horizontal inhomogeneity, and that the updrafts that form clouds will tend to be those that have higher MSE than the horizontal mean. Cold pools only require spatially localized rain evaporation in the PBL, not necessarily coherent downdrafts descending from high above the PBL top; in fact the downdrafts in tropical marine convection are not very organized or deep. Also, even under nonprecipitating shallow cumulus, MSE will have some horizontal inhomogeneity due to turbulent eddies that transport higher MSE air upward and lower MSE air downward, so we still expect some positive σ_q in that case too (see also our response to the referee's comment about Figure 3).

Eq 2b: Why such a complicated expression for such a simple-looking distribution? Is there a theoretical motivation for this? If no, why not use sigma = max(4e-4,aRR^b), where a and b are constants?

In terms of fitting accuracy, a second-order polynomial fit is more accurate but, in line with the referee's suggestion, a linear fit (even of the form a*RR+b) seems to work equally well when used in our single column model experiments. We will thus simplify Eq. (2b). But note that, as discussed in section 4.3, inclusion of changes in cloud base properties due to changes in the PBL structure is needed to make a sufficient strong positive feedback between convective rainfall and changes in the PBL structure.

Page 8398, Line 8: Figure 3a is used here to support the use of <MSE> for MSE_cb when the rain rate is essentially zero. But, in Figure 3a, the updrafts appear to have MSE that is 1-2 K larger than <MSE>. Is this difference not important?

What the text actually says is that <MSEsat>, the mean *saturated* MSE at the level of maximum cloud fraction, is nearly identical to the mean updraft MSE at this level. Fig. 3a shows that <MSEsat> is around 334.5 K at z=1.4 km (level of maximum cloud fraction), which is indeed nearly identical to the updraft mean MSE (the centroid of the red contours at this level). Another distinct point one can make from Fig. 3a is that MSE_cb is larger than <MSE> by about 1 K (while it is larger by about 4K on Fig. 3b). This 1-K difference matches the value of $L\sigma_q/c_p$ at the minimum rainrate threshold. This confirms the usefulness of the proposed Eq. (2a). We will update section 3.1.2 to make those two points clearer.

Sec 3.2: If the vertical velocities of convective thermals in the sub-cloud layer are not changed by the cold pools, why would there be an enhancement of mass flux? In other words, why should the mass flux be calculated from the ratio of TKE to CIN if TKE is not representative of thermals' vertical velocity? Shouldn't the relevant ratio be wˆ2/CIN?

The cold pool organization may not greatly enhance the vertical velocities of convective thermals, but it does create a broader range of boundary-layer conditions, i. e. a spatial PDF of CIN, such that some thermals are experiencing much smaller CIN than the horizontal mean. The width of the spatial CIN distribution seems empirically to be tied to the boundary layer TKE, motivating a cumulus area fraction that is related to CIN/TKE. Fletcher and Bretherton (2010) investigated a range of possible expressions for the mass-flux closure (including for the vertical velocity) and found that the expression we are using provides the best results. It is not the goal of this paper to repeat their analysis. The work of Fletcher and Bretherton was mentioned for instance in the first paragraph of section 3.

Sec 3.2: How is equation 2 used in UWSDpbl? This goes back to my earlier question about the relevance of equation 2 to a scheme that does not have precipitation-driven downdrafts.

As indicated on page 8399 lines 3-5, the scheme adds σ_q (diagnosed with Eq. 2b) when computing the cloud base properties. For the issue about the missing downdrafts, see our above response.