Response to anonymous referee #5

We thank the referee for his constructive comments. The comments of the referee have been rewritten in italic, our response is in plain script.

The present paper discusses a key issue of convection parameterization: can shallow and deep convection be represented by a unified scheme? The authors claim yes, if including the impact of precipitation in an existing shallow convection scheme. The paper investigates key aspects of the question: relationship between cloud base mass-flux and boundary layer turbulence, entrainment and evaporation of precipitation. It also points to the importance of being able to represent both oceanic and continental convection, testing their development on different case-studies. The methodology an- nounced in the introduction, using LES and CRM results to evaluate their hypothesis and fix parameters, is very attractive.
While the question is very well posed in the introduction, the use of LES/CRM and different case studies very relevant, I felt at the end quite disappointed by the proposed improvements as it appears to me that they were by some aspects in contradiction with main ideas raised in the paper, as explained in the following.

We don't think that the proposed improvements were in contradiction with the main ideas raised in the paper. The main idea is that the effects of precipitation on the convective development should be included in a convective parameterization, and all the three proposed modifications indeed try to represent such effects: see our response below.

It is assumed that deep convection development is closely related to boundary layer turbulence, which means that the modification of the boundary layer by deep convection is of particular importance. However, the authors try to represent this without including the explicit effect of the processes that have been shown since many years to play a key role in the modification of the boundary layer by deep convection: downdrafts and cold pools, that are driven by the evaporation of precipitation. Instead of this, they directly use an estimation of the evaporative potential to modify the cloud base mass- flux. This modification is attributed to a source of TKE coming from the evaporation of rain within the boundary layer, modification which is not taken into account to compute boundary layer characteristics. In addition, the proposed evaporative potential (=RRcb x PBLH) does not take into account the humidity of the boundary layer: for a given rain rate at a given cloud base the rain will experience different evaporation depending on the humidity of the underlying boundary layer. As the authors put a concern about being able to represent convection in many different conditions, I wonder how such a model would behave over a very dry region, the Sahel for example, where boundary layer characteristics would be very different from the West Pacific or the Great Plains.

Rain evaporation does feed back into the layer-mean temperature and moisture equations at each grid level, and thereby affects the PBL. We are aware that our scheme doesn't include downdrafts (as indicated several times in our manuscript) and that this would be the logical next step. However, there is no consensus about how best to parameterize convective downdrafts driven by rain evaporation, and the cumulus parameterizations in many weather and climate models do not explicitly include unsaturated downdrafts. We would argue that our proposed modifications are sensible and consistent even without explicitly including a downdraft scheme. They all try to include organizational effects of precipitation on the convective development, which have been shown of importance for convection and are generally not included in convective parameterizations. Cold pools only require spatially localized rain evaporation in the PBL, not coherent downdrafts descending from high above the PBL top; in fact the downdrafts in tropical marine convection are not very organized or deep and arguably are not a zeroth order parameterization issue.

We don't agree with the referee's comment that we do not consider the effects of cold pools on the PBL; in fact we would argue that augmenting PBL TKE via a 'cold pool' contribution (our equation 1) for the purpose of the cumulus mass flux closure is an advance over existing PBL schemes, and directly affects the mean boundary layer properties through the very tight coupling between the boundary layer and the convection scheme produced by a CIN/TKE closure. As indicated in Fletcher and Bretherton (2010), this type of closure maintains the cumulus base near the top of the PBL: an increase in cloud base mass flux due to cold pool effects will thus feed back on the height of the PBL. We performed sensitivity tests where we added the cold-pool TKE source from Eq. 1 directly in the PBL scheme (rather than in the convection scheme) and didn't find any difference. We will add these remarks in our revised version in sections 2.2 and 3.2.

Using the relative humidity as another predictor does not seem to improve the regression (see below Figure). The PBL height in Eq. 1 implicitly accounts for this effect because if the PBL top is saturated and the PBL is convectively well-mixed, its surface relative humidity will deviate from saturation in proportion to the PBL height. We will add this remark after Eq. (1) in section 3.1.1.

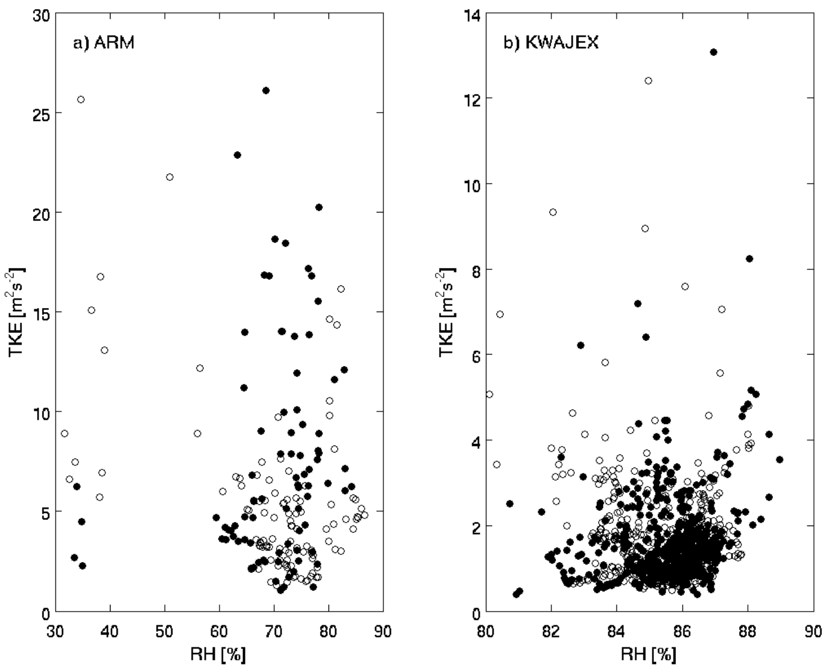

Fig: As Fig. 2 in our manuscript but as function of relative humidity averaged over the boundary layer. There does not seem to be a strong relationship between TKE and relative humidity.

Again, the mean updraft MSE at cloud base is directly related to properties of the boundary layer between 200 and 400m. As the authors mention from fig. 9, 10 and 11, computed boundary layer properties are quite misrepresented during the deep convective period. I would thus not expect that the SCM should give correct MSEcb and updraft effects in such conditions. In addition, I am wondering how sensitive relations 2a and 2b are to the choice of the considered layer (200-400m): why this choice? What does it change to include the surface layer or not?

We already acknowledged in our manuscript (see page 8405 lines 2-10) that the simulation misrepresents the boundary layer properties towards the end of the convective period due to the design of the PBL scheme. This critic again applies to all the simulations, including the default CAM scheme, and is thus not a result of our modifications. Regarding the choice of the considered layer: Fletcher and Bretherton (2010) found that taking the 200-400 m layer gave the best results, see their paper for more details.

It is also assumed that deep convective clouds entrain less than shallow clouds, be- cause of their size. As the aim of the paper is to propose a unified scheme for shallow and deep convection, I found it quite disappointing to propose to separate the cloud layer into three different layers (the heights of which are chosen arbitrary and may not be adapted for all types of clouds), corresponding to three different cloud regimes: hu- milis, congestus and cumulonimbus. The authors do not try to identify which universal processes would control entrainment in both shallow and deep convection. Some stud- ies (for example Gregory, QJRMS, 2001 or Del Genio & Wu, Journal of Climate, 2010) propose formulations which seem suitable for both shallow and deep convection, re- lating entrainment to buoyancy and vertical velocity within the updrafts. Of course the question is not easy and debated for years and in those studies some parameters of the formulation of entrainment are still different for the shallow or deep regimes. But I feel that this is a key aspect of the question whether or not we can represent shallow and deep convection in a unified way. In addition, it seems to me that the rain rate at cloud base could be more a consequence of entrainment, detrainment and mid-troposphere characteritics rather than what controls it.

We acknowledge that our formulation of entrainment/detrainment rates may have been too complicated and thus may have eclipsed its essence. Taking into account the referee's comment, we simplified our previous Eqs (3-8) to the following set:

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

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

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

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

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.

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 obtained similar results.

We believe that the proposed entrainment formulation, based on buoyancy sorting and indirectly updraft size (through precipitation at cloud base and Eqs (1-4)), is universal. Buoyancy sorting is a universal process, whose principle has been employed in different parameterizations (as noted by the referee). Also the increase in updraft size and corresponding decrease in entrainment from shallow to deep convection has been documented in several LES studies (e.g. Kuang and Bretherton 2006, Khairoutdinov and Randall 2006). Compared to the formulations of Gregory or del Genio mentioned by the referee (or other formulations), which multiply their entrainment rates with different constants for shallow and deep convection, we thus think that our formulation is actually more universal.

Finally there is obviously a positive feedback in the sense that smaller entrainment rates will yield more precipitation, as noted by the referee. However we believe that the proposed feedback (precipitation->evaporation->larger clouds->smaller entrainment->more precipitation) is a plausible one. This mechanism is supported by LES studies (e.g. Khairoutdinov and Randall 2006), which have shown that removing evaporation yields smaller clouds, larger entrainment rates and less precipitation. It is also consistent with principles of organization, i.e strongly precipitating clouds organize themselves which help sustaining convection (e.g., Mapes and Naeles 2010).

To summarize, it may exist a relationship between the cloud base mass-flux or entrain- ment and the rain rate at cloud base. But this relationship results probably from several feedbacks between updrafts, downdrafts, the mid-troposphere and the boundary layer. In the present study, imposing those relationships may help improving the results. But I am wondering if the involved feedbacks are correct and if results are better for good reasons.

In summary we plan to rewrite section 4.1 to better present our formulation and clarify the above discussed issues. We are in sympathy with the referee's comment that it is always difficult to know whether improvements are for good reasons but this is a general issue with model development. And we do have several LES studies which support the proposed feedbacks.

It seems to me that the improvements obtained are mostly due to the UWS shallow *convection scheme, particularly for the timing of convection on day 178 over land, as the shallow phase pre-conditioning deep convection is better represented. The fact that this scheme is then able to simulate correctly the deep convection phase is more questionable to me, and the authors should focus more on what is still missing to do it correctly, trying to improve some shortcomings of their present formulations. If they finally show that aspects not needed for shallow convection are key for deep convection, the relevance of a unified scheme is questionable. I still think that the present paper contains many aspects worthy of publication. However, I feel that it needs to be rewritten in such a way to explain more clearly what steps forward it allows and what limitations of this approach it highlights, in order to discuss more deeply the feasibility of a unified parameterization for shallow and deep convection.*

We don't agree that the improvements are mostly due to the UWS shallow convection. Looking at Figs. 8-11 (and especially 8b) it is clear that UWS alone cannot reproduce deep convection.
Hence in summary and in response to the referee's comment, we have simplified our entrainment formulation and will give more details on the logic and limitations behind the proposed modifications (especially entrainment and cloud base mass flux, as discussed above). Our manuscript already contained quite some discussion about the advantage and limitation of our approach (e.g., first paragraph section 3, p. 8399 lines 19-27, p 8404 lines 2-9, p 8405 lines 2-10, p 8405 lines 15-27, p 8406 lines 14-16, p 8407 lines 24-26, conclusions). We will update the conclusions to make this even clearer. We believe that our manuscript shows that unifying shallow and deep convection is feasible. The main limitations (no downdraft and partly empirically tuned entrainment rates) are similar to the limitations of current deep convection schemes, but our formulation allows for supplementary interactions with the boundary layer and includes some of the organizational effects of precipitation (as opposed to current schemes), which are thought of key importance for convection.

Some missing aspects:
- As the UW shallow convection scheme from Bretherton et al. (2004) is the starting point of the study, I would suggest to repeat main equations in section 2.2, instead of referring to the original paper. In particular, all equations that are particularly relevant for the rest of the paper: the closure, conservation equations for updraft properties, the w equation and the formulation of entrainment and detrainment.

We will repeat the equations for the closure, conservation equations for updraft properties and the w equation in section 2.2. The formulation for entrainment and detrainment was already partially given in section 2.2. We won't include the equations for the critical mixing fraction χ_c because the derivation is quite lengthy and the main point here is that χ_c depends upon updraft and environmental properties, as mentioned. We also don't include the equation for the penetrative entrainment since it is not of key importance for the present manuscript.

- It is not clear how some variables are retrieved from the SAM simulations: for exemple ε0 shown in fig.7 or the mass-flux shown in fig.9 and 11. Vertical profiles of entrainment from SAM for shallow versus deep convection would be instructive.

We will go through the manuscript to make sure that all the used variables are defined. We will especially add some more information about the retrieved variables in paragraph 2 of section 3, first paragraph of section 3.1.1, first paragraph of section 3.1.2 and a new paragraph in section 4.1 to define ε_0 and later w_{cb}. We will also clarify the computation of the mass flux in Figs. 9 and 11.

- Is there a separate treatment of large-scale clouds in the SCM? The precipitation rate RR used in SAM corresponds, I guess, to the total precipitation over the domain, which includes convective rain and rain from the anvil. Is the rain from the anvil supposed to be represented by the deep convection scheme?

There is a separate treatment of large-scale clouds. As long as the grid box is not fully saturated, all the rain formation mechanisms are represented by the deep convection scheme.

- How much can we trust the simulation of rain rates from CRM?

We discussed briefly this point on page 8392 lines 27-28 and on page 8393 lines 1-5. As indicated, previous studies have shown that SAM was able to reproduce the convective development fairly accurately compared to observations (Khairoutdinov and Randall 2003, Blossey et al. 2007, Siebesma et al. 2003).