Response to anonymous referee #1

We would like to thank referee 1 for his constructive comments, which have helped improve the presentation of our results. The comments of the referee are in italic, our response is in plain script.

General Comments

There is much that is praiseworthy in this article: (i) the basic idea that a shallow scheme can be adapted to handle deep convection by taking into account a few key effects of precipitation processes is an interesting one; (ii) the use of CRM data to study such effects is entirely appropriate; (iii) single-column model results with the modified scheme are encouraging. Therefore, I would consider the article to be ultimately well worth publishing in ACP.

Thanks for the comment.

However, in a parameterization paper particularly, details of the methods used are extremely important, and it is in specifying those details where the paper does have some weaknesses that are discussed below. I would particularly highlight points 3 and 8 below. Re point 3, is it true that the fits are strongly weighted towards KWAJEX, and very weakly weighted towards BOMEX as Fig 4 would suggest? Re point 8, is the scheme iterative, and if not then is the alternative computation approach actually defensible?

As suggested by the referee, we will modify the text to give more details on the scheme, on the investigated quantities, on the performed modifications and on our implementation (see below our response to the specific comments for more details). Regarding point 3, it is true that the fit is more strongly weighted towards KWAJEX than towards BOMEX as Fig. 4 suggests. However we can see on Fig. 12 that this does not deteriorate the BOMEX simulation. This is mainly due to the fact that our modifications need some precipitation to become effective. We will add a paragraph to discuss these sampling issues in the conclusion. With respect to point 8, we don't use iterative loops to estimate the new terms. The required predictors velocity at cloud base (Eq. 5) and precipitation at cloud base (Eqs. 1, 2,6-8) are averaged over the last simulation hour and are then updated at the end of the convection scheme for the next time step. The other predictors (PBLH, height of cloud base, density at cloud base) are instantaneous values. We employ values averaged over the last hour for velocity and precipitation to avoid the danger of the on-off nature of most convection schemes, as noted by the referee in his specific comment 8. We don't use any iterative loop since, when convection transitions from shallow to deep convection, it is not in equilibrium: if we would use an iterative loop, our cloud would deepen during the iteration loop until reaching its deep stage (instead of deepening with time). This would destroy the diurnal cycle. We will add two paragraphs, one at the beginning of section 3.2 and one at the beginning of section 4.2 to discuss these issues and better describe the implementation of our modifications.

I would also like to encourage the authors to address one further question, perhaps with a paragraph added to the conclusions. To what extent are their modifications generic and extendable to other shallow convection parameterizations operating with other boundary layer schemes, and to what extent are they specific to their particular model configuration? No doubt the authors hope that their paper will attract the attention of other researchers into parameterization, and clearly its impact will be much greater if at least some of their methods could be usefully taken over or adapted for other models

We don't see any difficulties for implementing the changes concerning cloud base thermodynamic properties and entrainment/detrainment rates in other bulk mass-flux schemes, as these modifications use predictors (especially precipitation at cloud base) which should be available in any scheme (note also our response to the specific comment 11 below). The modification concerning the cloud base mass flux would require a TKE-based closure. We will add this in the last paragraph of our conclusions.

Specific Comments

1. *Sect 2.2. It would be useful to add a line of clarification about the role of TKE: i.e., it seems that the UW shallow scheme must be used alongside a boundary layer scheme that provides TKE as an output.*

   We plan to address this point by adding that "CIN is implicitly computed within the scheme, while TKE must be provided by the boundary layer scheme" at the end of the first paragraph of Sect 2.2. We will also add some few lines to repeat the important property of a TKE-based closure, i.e. it acts to keep the cumulus base near the top of the PBL.

2. *On various occasions the authors refer to a spatial average without specifying how that average has been performed (height integral, pressure integral, mass- weighted height integral....?). They should be specific about the averaging methods.*

   We will check the different occurrences of averaged properties along the text and clarify those, especially in the second paragraph of section3, first paragraph of section 3.1.1, first paragraph of section 3.1.2 and the captions of Figs. 2 and 4.

   • *p8395, line 8. The TKE average. :* TKE is averaged over the depth of the PBL, we will move the definition for the PBL depth from the 3rd paragraph of section 3.1.1 to the first one.
   • *p8395, line 23. The cloud-base is also a type of domain-average? Assuming so, is it calculated for each point and averaged, or do you construct an average profile, and then calculate the cloud-base?* It is calculated for each point and then averaged (to be added in the first paragraph of section 3.1.1)
   • *p8395, lines 26-27 and p8398, line 6. The 200-400m means?* The mean between the heights 200 and 400m (will be explained in the first

paragraph of section 3.1.1).

• Fig 10. The 1km average? We will add in the text "the lowest 1 km of the atmosphere, as a rough estimate of the PBL depth".

• p8396, line 1 and p8398, line 13. At least in the latter case, please confirm that the horizontal standard deviation of q was computed (how?) directly from the SAM humidity data. Yes it was computed directly from the SAM humidity data. Like all other quantities, it is computed at each time step and then averaged over one hour. We will add this in the 2nd paragraph of section 3.

3. *The article includes various scatter plots in which results from different experiments are presented together. Also, fits are sometimes constructed to the points on these plots. However, it is important to establish how many points have been taken from which experiment at which time, and so what will be weighting of the different experiments in the fits, and how independent are the data points. Thus, the authors must be explicit about their data sampling strategy for Figures 2, 4 and 7.*

   We will add the number of points used to construct Figs. 2, 4 and 7 in the corresponding captions of the Figures. The times considered were already indicated (i.e., onset and mature phase). And as mentioned above, we will add a paragraph in the conclusion to discuss sampling issues. We cannot exclude that sampling issues might influence our results. However we already considered quite a large sampling dataset (as compared to other studies). Also, the fact that we are able to simulate three very different situations (BOMEX, KWAJEX and ARM) gives us some confidence with respect to our sampling strategy.

4. *p8396, Eq. 1. Is the value of TKEdry obtained from the fit consistent with TKE values found in SAM before the onset of precipitation?*

   As can be seen in Fig. 2 the fit crosses the y-axis in the middle of the filled points with zero precipitation (which correspond to the points before the onset of precipitation), so the fit is consistent.

5. *p8396, lines 16-17. This sentence seems to invite a lag-correlation analysis to test the suggestion. Have the authors attempted such an analysis?*

   We haven't attempted a lag-correlation analysis. The sentence referred to the visual inspection of Fig. 2, where it can be seen that the white points (from the decay phase) lie further away from the regression line than the black points, especially in ARM.

6. *Sect. 3.1, last sentence. This explains why RRcb is an appropriate predictor, but not why it is appropriate to take a logarithm?*

   There is actually not so much difference between using a second-order logarithm fit or a first order linear relationship to predict σ_q. In terms of fitting accuracy, Eq. (2b) is the most accurate but, based on the referee's comment, we also performed single column model simulations using a linear relationship between RRcb and σ_q and obtained almost identical results. We will thus simplify Eq. (2b) in our revised version.

7. *Sect. 3.2. The actual mass-flux closure equations are important, as well as the description of them that is given here. It would be helpful to the reader to show these, making the article more self-contained. Note for example that on p8407, line 12, the modifications to cloud base mass flux are not known to us from Eq. 1, which tells us only the modifications to TKE.*

   We will add the mass-flux closure equations in the first paragraph of section 3.2 and correspondingly clarify the different occurrences of cloud-base modifications (as on p8407 line 12).

8. *p8399, line 2. The first example of a generic issue with this paper. The rain rate at cloud base is used to modify various aspects of a shallow convection scheme, such as the entrainment and (here) the closure. However, the rain rate is gen- erally thought of as an output from a convection scheme rather than an input to it. For example, the rain rate will depend on the closure, but here we have a clo- sure that depends on rain rate. Thus, I am led to believe that the scheme must surely be iterative. Is that so? If so, how does the iteration work? If not, do the authors perhaps use the rain rate from the previous timestep? But that would seem fraught with danger, given the on-off nature of most convection schemes. In short, it is far from obvious from the information given that the scheme is ac- tually use-able in practice and I need to be told how the scheme's calculations were actually performed. (I note that on p8411, line 11 there is the phrase "prior precipitation". The word prior here seems very important, so I was somewhat perturbed to find it mentioned for the first time in the penultimate paragraph of the conlusions!)*

   See our above response to the general comment.

9. *Sect. 4.1, 2nd paragraph. There is an issue about causality here. For lower values of 0 (however they are produced), we would expect deeper clouds and more precipitation. Thus "covariability of ε_0 and precipitation is expected" as the authors state, but this point by itself does not say anything at all about a causal mechanism or boundary layer organization. I actually agree that the suggested mechanism is a very plausible one, but the language used needs to be much more careful.*

   We agree that there is a positive feedback, in the sense that lower entrainment rates will lead to larger precipitation amounts. However we believe that the explanation presented in the next sentence (i.e., precipitation yields organization yields larger clouds yields smaller entrainment rates) is a very plausible one. It is also supported by LES studies (Kuang and Bretherton 2006, Khairoutdinov and Randall 2006).

10. *Sect. 4.1. How was a value of the bulk entrainment coefficient ε_0 estimated from SAM data? Extracting entrainment rates $\varepsilon = \varepsilon_0 \chi_c^2$ from CRM data is a contentious issue in itself: e.g., dependent on the definition of updrafts. The authors need to make clear how they obtained ε_0 values.*

We will add at the beginning of section 4.1 a paragraph to explain how we computed the entrainment rates. Our methodology follows previous LES studies (e.g., de Rooy and Siebesma 2008). The entrainment rates are estimated using the equations for a simple plume model: $\frac{\partial M}{\partial z} = M(\varepsilon - \delta)$ and $\frac{\partial \psi}{\partial z} = \varepsilon(\bar{\psi} - \psi)$. We sample all the cloudy points to compute the mass flux M and average it over a one-hour time interval. As approximately conserved updraft variable ψ we employ the mass-flux weighted frozen moist static energy, which is again sampled over all cloudy points and hourly averaged. $\bar{\psi}$ corresponds to the domain and hourly averaged frozen moist static energy. Knowing ε and δ we can compute ε_o by solving the buoyancy sorting relations. We will also clarify in section 4.1 how we estimated the velocity at cloud base.

11. *p8401, Eq. 5. It is worth commenting that this uses the vertical velocity at cloud base, which is presumably predicted in the UWS scheme but would not in general be available to a mass flux scheme.*

    The velocity at cloud base is not directly predicted in the UWS scheme, but it can be derived from the closure equation, i.e. Fletcher and Bretherton (2010) indicated that $0.28\sqrt{TKE} + 0.64$ is a good estimate for the velocity at cloud base. Hence it would be in general available to any mass flux scheme as long as the PBL scheme provides a reliable estimate of TKE. We will clarify this in the first paragraph of section 4.2.

12. *p8402, line 16. Please clarify whether Figs. 7a and 7c include data from the decay phase.*

    Yes, only Fig. 7b does not include the decay phase. We will clarify this in the Figure caption.

13. *p8403, line 16. What is the reasoning behind this change?*

    There is no clear theoretical argument speaking for detraining water either before or after performing buoyancy sorting. In purely shallow convection cases, this does not make any difference, but in deep convection cases, we obtained better results by detraining water after buoyancy sorting. We will add “as this tends to improve the results” on p. 8403.

Technical Corrections

We will modify the manuscript accordingly, thanks.