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Interactive comment on "Simulating deep convection with a shallow convection scheme" *by* C. Hohenegger and C. S. Bretherton

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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 announced 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.

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.

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?

It is also assumed that deep convective clouds entrain less than shallow clouds, because 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: humilis, congestus and cumulonimbus. The authors do not try to identify which universal processes would control entrainment in both shallow and deep convection. Some studies (for example Gregory, QJRMS, 2001 or Del Genio & Wu, Journal of Climate, 2010) propose formulations which seem suitable for both shallow and deep convection, relating 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.

To summarize, it may exist a relationship between the cloud base mass-flux or entrainment 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.

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.

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.

- It is not clear how some variables are retrieved from the SAM simulations: for exemple $\varepsilon 0$ 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.

- 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?

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

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 8385, 2011.