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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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In his short comment C2411, Jun-Ichi raises some interesting philosophical points about CIN closure and cumulus parameterization about which we would like to provide another perspective. Cathy is on a research cruise taking observations in the tropical ITCZ , and in any case, most of these points really refer back to an earlier 2010 JAS paper by Fletcher and Bretherton, so after having discussed this response with her, I will take them on.

Jun-Ichi's three main points are (in his words)

1. It is rather illusionally to consider that the cloud–base mass flux, M_B, literally determines the strength of convection.

2. The notion that a plume is originated from the cloud base and gradually evolves upwards with time is simply not consistent with the steady plume hypothesis assumed in the standard mass flux formulation.
3. It is misleading to use CIN based on a simple parcel–lifting principle in the convectively well–mixed boundary, where the motions are more buoyancy–driven than the value of CIN would indicate.

Let's take these three points in turn. First, we agree that if one uses a steady-state ensemble plume as a parameterized model of the cumulus cloud field, one can certainly specify the plume mass flux at any altitude within the cumulus layer. However, there are two compelling reasons for using the *cloud base* mass flux for this purpose. First, boundary-layer based mass flux closures regard the mass-flux closure as an adaptor that harmonizes the boundary layer and the cumulus ensemble by venting air from the boundary layer so that the boundary layer updraft LCL remains slightly above the boundary layer top, allowing a subset of strong overshooting eddies to become cumulus clouds. Conceptually, the boundary layer controls cloud base mass flux and the plume model then controls how this mass is vertically distributed in cumulus clouds; their combination maintains a quasi-equilibrium thermodynamic structure when the forcing is sufficiently slowly varying. Second, LES and observations show that most cumulus clouds are rather shallow, even when some reach quite deep. From the perspective of causality, does it make sense for a closure to predict cumulus mass flux at a level to which many cumulus clouds are not even penetrating? Jun-Ichi also makes the point that the 'roots' of cumulus updrafts may be traced well below cloud base, which we acknowledge; in fact the Park and Bretherton (2009, *J. Clim.*) parameterization explicitly includes a moisture perturbation in the cloud base updrafts that derives from their near-surface origin. One may still choose to treat these roots as also comprising a component of boundary layer mixing, as also done in the more unified EDMF/dual-MF approach of Neggers, Siebesma and others.

Jun-Ichi's second point is a fundamental critique of essentially all current cumulus parameterizations. It is widely understood that cumulus parameterization is well-posed only given an assumption of separation of space scales between the resolved-scale motions and the cumulus scale. It is sometimes forgotten that the same issue applies to time scales. That is, the timescale of evolution of the resolved-scale flow should be much longer than the turnover time of individual cumulus updrafts. In this case, the steady-state ensemble plume assumption is reasonably accurate. However, cumulus parameterizations are frequently applied in situations in which these assumptions do not strictly apply, e. g. the diurnal cycle of cumulus convection over land, or near fronts or coasts, or in models with 10-20 km grid spacing. This inevitably leads to parameterization errors. Use of a time-dependent or stochastic plume may help with such errors, but can't be expected to fully address a lack of timescale separation.

CIN closure actually has the philosophical advantage of applying on the boundary layer turnover timescale rather than the deep convective turnover timescale, so formally it is reasonable to use a CIN closure with a time-dependent prognostic plume model (which we haven't yet tried to develop). However, CIN closure also is just as philosophically appropriate to a cumulus ensemble which is evolving slower than the timescale of individual updrafts, the case traditionally treated with steady-state plume models and CAPE-regulating mass-flux closures.

We note that while Raymond's (1995) BLQ may imply $(dCAPE/dt)_{BL} \approx 0$, this is *not* a general characteristic of boundary-layer based mass flux closures. As can be seen in Fig. 10 of Hohenegger and Bretherton, the boundary-layer moist static energy can evolve as quickly using a column model with a CIN closure as in a CRM in response to realistic diurnally-varying or synoptically-varying forcing. We do not regard this as a persuasive argument against CIN closure.

Jun-Ichi's third point seems most significant to us. We agree that on the face of it, a positive buoyancy flux at the cloud base suggests that updrafts must be on average more buoyant than downdrafts at this level, and this needs further investigation using

CRM of deep cumulus convection. There are however two points to make about this discussion. First, we have to look at the buoyancy flux averaged across a relevant grid cell size over a relevant timescale for parameterization. The five-day average profiles shown in Jun-Ichi's Fig. 1 can be expected to smear out any layer of instantaneous negative buoyancy flux, unless cloud base remains remarkably steady. Second, one must understand what equilibrium CIN closure is trying to maintain rather than focusing on exactly how it does this. In a CRM, there are cold pools and mesoscale circulations that create a range of LCLs and cloud bases across the computational domain. Negatively buoyant evaporatively-driven downdrafts may contribute at least as strongly to the buoyancy flux as updrafts driven by surface fluxes. Horizontally averaging this heterogeneity may not clarify the parcel dynamics of updrafts that condense into the base of cumulus clouds. The basic role of the CIN closure is to ensure that a few (but not all) boundary layer updrafts can become buoyant cumuli. Just like the entrainment closures often used at boundary layer tops, the closure idealizes there as being a single cloud base and a flat-topped boundary layer at which the closure can be applied, recognizing that nature is more complex but that the closure can still approximately maintain the same feedbacks between boundary layer and cumulus layer as naturally occur.

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