

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

J.-I. Yano

jun-ichi.yano@zmaw.de

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Introduction: PBL-Based Mass–Flux Closure

The present paper pursues the concept that can be called the *PBL(planetary–boundary layer)–based closure* in order to extend a shallow–convection scheme to deep convection. Importance of evaporation of deep–convective precipitation is emphasized from this very perspective in order to modify the closure condition for deep convection. The present comment, in turn, focuses on the limits of applying the PBL–based closure to the mass–flux convection parameterization.

Probably, the concept of the PBL–based closure is traced to the idea of “activation–control” principle proposed by Mapes (1997). This principle, first of all, emphasizes

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importance of activation (or triggering) rather than strictly adhering to the traditional principle of quasi–equilibrium. In order convection to be triggered, an air parcel must pass a barrier created by convective inhibition (CIN) by embodying sufficient turbulent kinetic energy (TKE).

The “activation–control” principle is turned into a closure condition (PBL–based closure), in my best knowledge, first by Mapes (2000) with his Eq. (3) in defining the magnitude of shallow convective heating rate, which is assumed to be proportional to $\exp(-\text{CIN}/\text{TKE})$. This approach is further elaborated by Kuang and Bretherton (2006), and Fletcher and Bretherton (2010). The present work can be considered an extension of those earlier work.

The idea of PBL–based closure is intuitively appealing. As stated by Fletcher and Bretherton (2010) towards the end of their Sec. 4, “Philosophically, it seems more natural to relate cloud–based mass flux to properties of the boundary layer (CIN and TKE), where the updrafts originate, than to those of the midtroposphere (ECAPE).” Here, ECAPE stands for entraining CAPE. Unfortunately, however, I believe, this closure principle appears to be not quite consistent with the basic principles of the whole mass–flux parameterization.

As going to be argued below, the “activation–control” is a drastically different principle from traditional convective quasi–equilibrium. Being the current mass–flux parameterization formulated under the framework of the latter, introducing the former as a closure hypothesis simply calminates into self–contradiction. That is the main intent of the present comment.

More specifically, I am going to point out three major issues:

- 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

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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.

These issues are discussed further in the following sections in order.

Principle of Mass–Flux Closure

In order to see the issues behind as clear as possible, let us also begin from basics as much as possible: the core of the mass–flux convection parameterization is in defining a vertical profile of the convective (updraft) mass–flux, M (and also possibly that for the downdraft). Once the mass–flux, M , is properly defined, convective transport rate of all the conserved variables can be evaluated in a more or less straightforward manner from the mass flux.

The convective mass–flux, M , is, in turn, traditionally defined by two steps by decomposing it into two parts:

$$M(z, env) = M_B(env)\eta(z, env). \quad (1)$$

Here, $M_B(env)$ is the mass flux defined at the cloud base, $z = z_B$, and $\eta(z, env)$ is a relative vertical profile. Here, an argument, env , is added in both quantities in order to indicate that they are diagnostically defined by a given “large–scale” environmental state. This assumption is satisfied so long as the convective adjustment time–scale is much shorter than that for the large–scale environmental processes. Recall that this is the basic assumption behind the principle of *convective quasi–equilibrium*.

In the standard formulation, the mass flux is defined in terms of the prescribed fractional entrainment and detrainment rates, ϵ and δ :

$$\frac{1}{M} \frac{dM}{dz} = \epsilon - \delta. \quad (2)$$

A vertical integral of Eq. (2) provides the mass flux, M .

Substitution of Eq. (1) into Eq. (2) simply shows that the relative mass–flux profile, η , is also defined by a vertical integral of an equivalent equation:

$$\frac{1}{\eta} \frac{d\eta}{dz} = \epsilon - \delta. \quad (3)$$

As a result, the remaining problem reduces to that of determining the mass flux, M_B , at the cloud base. This problem is called closure.

In this manner, we *may* consider that the purpose of closure is to determine the convective mass–flux at the cloud base, $z = z_B$. However, we have to realize that there is nothing special about the cloud base in the above formulation, but the cloud base is merely taken as a reference level for defining the magnitude of the convective mass–flux.

In order to see this point more explicitly, let us re–write Eq. (1) as

$$M(z, env) = M(z_0, env)\tilde{\eta}(z, z_0, env) \quad (4)$$

with an arbitrary reference height, $z = z_0$. This reference level is arbitrary also in the respect that Eq. (2) can be integrated vertically starting from any vertical level. Thus, there is no strong reason to take a particular form (1) rather than a more general form (4).

For this reason, I personally find it a bad tradition of taking the cloud base as a reference level for determining the convection strength. Arguably, Fletcher and Bretherton (2010) have found a robust relationship between the cloud–base mass flux and a PBL–related variable by a scatter–plot analysis of LES. However, it does not guarantee that they would find the same result by repeating the same analysis at a different vertical level. Note that convective plumes they examined do not satisfy the steady hypothesis to be discussed in the next section. Thus, a scatter–plot analysis at any particular

vertical level is likely to be contaminated by local fluctuations. In fact, this point is demonstrated by an upper frame of Fig. 4 of Fletcher and Bretherton (2010).

In order to remove such local fluctuations from the analysis, probably, a better alternative would be to take a measure based on a vertical integral. For this reason, we may rather normalize the reference vertical profile, η , by a condition:

$$\langle \eta^2 \rangle = 1,$$

where $\langle * \rangle$ designates a vertical average. As a result, the total mass flux is given by

$$M(z, env) = M_0 \eta(z, env) \quad (5)$$

with its amplitude defined by

$$M_0 = \langle M^2 \rangle^{1/2}. \quad (6)$$

The mass–flux amplitude defined by Eq. (6) (or equivalent) would be likely a better measure for seeking a closure condition from LES or CRM.

The issue could be further linked to our tradition of assuming that the moist convective plume is originated from the cloud base (saturation level, or lifting condensation level, from a parcel dynamics point of view). Probably this is artificial, because we see no dynamical reason to expect that a convective plume starts from a saturation level (or a cloud base). It is more likely that a dynamical root of convective updraft can be traced downwards below the cloud base into PBL.

According to Fletcher and Bretherton (2010), the present authors take the convective updraft as “saturated updrafts with vertical velocity $w > 0.5 \text{ m s}^{-1}$.” As a result, the convective mass flux is automatically set zero below the cloud base (saturation level) as seen in their black curves in Fig. 9(b), Fig. 11(b), and Fig. 12(c). By definition, no convective mass flux is identified below the cloud base. As a result, the method fails to trace a dynamical mass flux further below.

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Steady–Plume Hypothesis

The above argument has deliberately remained mathematical. Among the all possible reference levels, physically speaking, the cloud base, chosen as the bottom of the moist–convective plume, is probably still most plausible. The above quotation from Fletcher and Bretherton (2010) clearly makes this point. The convective plume would be physically initiated from the cloud base and, then, it develops upwards with time. Such a time evolution of plume would be evaluated by vertically integrating Eq. (2), taking a Lagrangian perspective that the vertical integral could be re–interpreted as a time integral.

However, such a perspective is misleading because the plume is assumed to be “steady” in the standard mass–flux formulation. In order to fully understand the physical implication of the “steady plume” hypothesis, the assumptions associated with Eq. (1) must also be fully understood: mass flux is completely slaved to the “large–scale” environment. In other words, absolutely no internal dynamics of the plume (e.g., its own life cycle) is taken into account in the above formulation. The mass flux is simply adjusted to a new profile every time step by following evolution of the environment, even without asking what happened in time between.

In order to consider the time evolution (e.g., life cycle) of a plume, instead of Eq. (2) we have to consider

$$\rho \frac{\partial \sigma}{\partial t} + \frac{\partial M}{\partial z} = M(\epsilon - \delta). \quad (7)$$

Here, ρ is the air density, σ is a fractional area occupied by convective updraft in a grid box.

For the sake of argument, let us take Eq. (7) and suppose that we try to integrate it vertically in Lagrangian sense. However, as the plume grows upwards with time, the fractional area, σ , occupied by the plume also grows with time, as given by the first term in Eq. (7). This term may be considered as representing a local adjustment process against a tendency of a plume growing upwards with time. Without going into details

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of solving this unsteady plume problem properly, it transpires that extensive vertical communication happens (not to mention a role of dynamic pressure), as a result, before the whole plume reaches equilibrium as a whole. It transpires that the strength of a final steady plume cannot be defined simply by a bottom boundary condition. Rather, it is a vertical integral of the whole state of the atmosphere that contributes in defining the magnitude of a final steady plume. The cloud base cannot be singled out as if a sole determinant of the plume strength.

In this respect, Mapes' "activation–control" clearly speaks of an initial triggering process of a transient plume. It is not obvious how this principle applies to an equilibrium state, which should realize at a much later stage of the whole evolution of the system. We should also realize that the traditional convection parameterization never talks about individual plumes literally but only in ensemble sense. Even when a bulk plume is considered in a parameterization, formally, it is interpreted as effects of an ensemble of plumes integrated into a single plume as a representation (cf., Plant 2010). Individual plumes may well be always in transition, but the common wisdom with the traditional convection parameterization is to consider that these ensemble plumes are as a whole in equilibrium. This is the rationale behind assuming a steady plume. As a result, the "activation" condition should not come into play so long as the traditional mass–flux formulation is under consideration.

It is true that a "triggering" condition is introduced more than often in operational models. Unfortunately, however, it should be understood as an inevitable evil in order to make the operational models running under imperfect convection parameterizations. The fact does not change that the idea of "activation" and "triggering" is fundamentally in odd with the basic principle of quasi–equilibrium.

It could be more appropriate to interpret that Mapes' "activation control" implicitly assumes the atmospheric convective system consisting of a series of ascending "bubbles". The bubble theory (e.g., Ludlam and Scorer 1953, Scorer and Ludlam 1953, Levine 1959) was a strong alternative theory for describing atmospheric convection.

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However, the idea was somehow abandoned somewhere around the beginning of the 70s. Mapes' theory urges us to reconsider this alternative possibility seriously.

Parcel–environment Equilibrium

Note that the PBL–based closure is based a premise that the convection is primarily controlled by a state of PBL rather than that of the free troposphere. This issue could further examined in terms of the temporal tendency of CAPE (convective available potential energy), which may be separated into the two contributions, those coming from the variability of the boundary–layer and those of the free troposphere (parcel environment):

$$\frac{dCAPE}{dt} = \left(\frac{dCAPE}{dt}\right)_{BL} + \left(\frac{dCAPE}{dt}\right)_{PE} \quad (8)$$

Here, the subscripts, *BL* and *PE*, designated the contributions from the boundary layer and the free troposphere (parcel environment), respectively.

Arakawa and Schubert's (1974) convective quasi–equilibrium hypothesis can be stated as

$$\frac{dCAPE}{dt} \simeq 0.$$

The PBL–based closure may be interpreted as being based on the idea that such an quasi–equilibrium tendency is better satisfied for the boundary–layer component, i.e.,

$$\left(\frac{dCAPE}{dt}\right)_{BL} \simeq 0.$$

This corresponds to the concept of boundary–layer quasi–equilibrium proposed by Raymond (1995).

Zhang (2002, 2003), and Donner and Phillips (2003) carefully examined the CAPE budget (Eq. 8) from sounding data both from midlatitudes and tropics. They showed that the quasi–equilibrium tendency is better satisfied for the parcel–environment con-

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tribution, i.e.,

$$\left(\frac{dCAPE}{dt}\right)_{PE} \simeq 0$$

rather than for the boundary–layer component. Their analysis suggests that a parcel–environment based closure is better observationally supported than a PBL–based closure.

Is CIN physical?

It is commonly believed that in order convection to be initiated, the system must overcome an inhibitive barrier called CIN. I personally object to this point of view, but such a perspective arises mostly from an artificial thinking based on the lifting parcel procedure.

In typical tropical situations of initiation of deep convection, the boundary layer is already well mixed by convection. In other words, the boundary–layer flow is buoyancy driven: an upward motion is most likely associated with a positive buoyancy, and a downward motion most likely with a negative buoyancy. Thus, when the local buoyancy is defined as an weighted average with vertical velocity of actual buoyancy felt by individual local air particles, we no longer see any negatively–buoyant zone, probably except for an inversion layer at the top of the well–mixed layer. In this respect, within a well–mixed boundary layer, no convection inhibition exists, physically speaking.

In order to see this point explicitly, we define an effective buoyancy, b^* , by

$$b^* \equiv \frac{\overline{\rho w b}}{(\rho w)^*}. \quad (9)$$

Here, bar designates a horizontal average, w is the vertical velocity, $(\rho w)^*$ is a root–square mean over the atmospheric column volume (i.e., CRM modelling domain), i.e., $(\rho w)^* \equiv \langle (\rho w)^2 \rangle^{1/2}$, b is the buoyancy as locally defined within a CRM in vertical momentum equation.

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Fig. 1 (reproduced from Fig. 2 of Yano 2003) shows vertical profiles of standard parcel–lifting buoyancy (a) and the effective buoyancy, b^* , (b) based on the definition (9). Three CRM runs from the TOGA–COARE period for three different convective regimes are taken as examples (see the figure caption for details). Yano et al. (2005a) propose to call the vertical integral of b^* PEC (potential energy convertibility).

The convective inhibition is clearly visible under the standard lifting parcel calculations for the lowest 1–km for all the convective regimes. On the other hand, the actual buoyancy, b^* , shows no negative buoyancy except for the suppressed regime after a dry intrusion event (dotted curve), in which case an inversion at the top of the boundary layer exhibits a very narrow zone with negative buoyancy.

As a result, under the actual convective–scale dynamics, the role of inhibition control is not as strong as it appears by a simple–minded parcel analysis. This also suggests that the “activation control” process is not as strong as Mapes tends to argue, though this does not at all discredit his whole argument. It further poses a question on physical basis of adopting CIN for a closure variable.

Final Remarks

The PBL–based closure is conceptually inconsistent with the standard mass–flux formulation we adopt today for the reason that the cloud base has no special meaning under the latter formulation. Despite success of this approach so far, due to inconsistency in the formulation the whole affairs may ultimately go astray. In other words, in order to properly implement a PBL–controlled (activation–control) description of convection parameterization, the whole formulation of parameterization must be re–written in such way that it is no longer constrained by quasi–equilibrium and steady–plume hypotheses, but transient nature of convection is fully taken into account.

The last point made is more general, and pointing to a unphysical nature of the concept of CIN: plots of vertical profiles of effective mean buoyancy felt by convective–scale air particles (Fig. 1(b)) show no such convective inhibition in PBL except for a possibility

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of very shallow convective–inhibitive inversion layer.

Convection may well be interacting with PBL. However, it is likely that a more intricate integration of PBL effects into the whole closure problem would be required, rather than trying to define the whole convection strength solely in terms of the PBL (and subcloud–layer) quantities as attempted in the present series of papers.

A full discussion of consistent mass–flux closure would be beyond the scope of the present comment. Nevertheless, the two major possibilities are pointed out. The first possibility is conservative and to stay with the traditional framework of quasi–equilibrium principle with the steady–plume hypothesis. Under this traditional framework, an integral constraint to the system should be derived.

For example, in the original formulation for convective quasi–equilibrium hypothesis by Arakawa and Schubert (1974), the contribution from the subcloud layer (PBL), as given by their Eq. (B32), constitutes a part of the whole mass–flux kernel, $K(\lambda, \lambda')$. As Randall and Pan (1993), Pan and Randall (1998) show, this formulation constitutes a quasi–closed prognostic description for the energy cycle of the convective system with contributions of PBL processes fully taken into account.

Probably needless to emphasize, but under this energy–cycle description, not CAPE but a more generalized cloud–work function plays a central role. PEC introduced in the last section may be considered as its estimate. ECAPE criticized by Fletcher and Bretherton (2010) in the quotation above is a special limit to the cloud work function obtained by setting $\eta = 1$ in the latter definition. However, neither CIN or TEK directly enters into this formulation.

The second possible approach is to more radically plunge into a fully prognostic description of PBL processes fully coupled with convection. Such a scheme can be constructed either based on a general mode decomposition (Yano et al., 2005b) or segmentally–constant approximation (Yano et al., 2010) applied to both PBL and convective processes. Careful diagnoses of LES and CRM under these frameworks would

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provide useful information in order to close the formulations under these approaches.

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Figure Caption

Fig. 1: Mean vertical profile for (a) parcel-lifted (CAPE) buoyancy, and (b) convection – driving (PEC) buoyancy. Three cases from the TOGA–COARE period are considered: a dry intrusion event (14–18 November 1992, marked by D with dotted curves), an easterly–wind regime (10–14 December 1992, marked by E with long–dashed curves), and a westerly–wind burst (20–24 December 1992, marked by W with the dotted–dashed curves).

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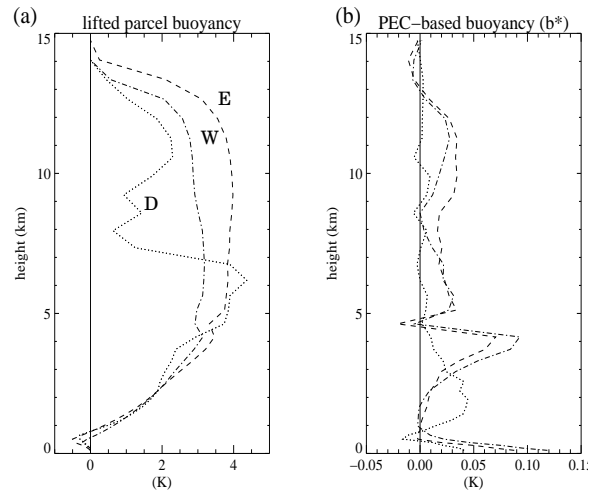


Fig. 1. Mean vertical profile for (a) parcel-lifted (CAPE) buoyancy, and (b) convection-driving (PEC) buoyancy. Three cases from the TOGA-COARE period are considered. See the full Figure Caption above.