Why do moisture convergence deep convection schemes work for more scales than those they were in principle designed for?

J.-F. Geleyn[1] *and J.-M. Piriou*[2]
[1] *Czech HydroMeteorological Institute, Prague, Czech Republic, on leave of absence from Météo-France*
[2] *Centre National de Recherches Météorologiques, Météo-France, Toulouse, France*

I) Introduction

The title of this note is intentionally provocative, but it reflects two underlying realities. First of all it stems out of the content of a mail written (on 12/01/2004, personal communication) by Philippe Bougeault (who proposed twenty years ago the first operational deep convective parameterisation that combined a mass-flux approach with a single-cloud profile and a Kuo-type closure, Bougeault (1985)) and that will be reproduced (after translation) here.

"*I was well conscious about this limitation (authors' note: of the moisture convergence closure) in 1985, but the problem is that I mostly wanted to fit GATE data, where there is no correlation between CAPE and rainfall, while there is a strong correlation between moisture convergence and rainfall. But, as Mapes rightly says, the latter does not guarantee a causal link because one might mix cause and consequence.* ***But, since it works on this basis at Meteo-France as well as at ECMWF for 20 years, this cannot be that wrong either!***"

Second, and prolonging the ideas expressed by Mapes (1997), it is our opinion that the heavy debate about which measure of Quasi-Equilibrium (QE thereafter) is the most appropriate one for any parameterisation scheme's closure (cloud work function, CAPE-CIN, moisture convergence, …) is ill-placed and reflects a wrong choice of priority. We believe that future parameterisation schemes will put this question to the second rank and first address a far more fundamental one.
The aim of this note is thus to introduce this change of emphasis and to explore some tracks about how to concretise it without throwing away much of the progress that deep convective parameterisation made in the past twenty to thirty years on others aspects. For this reason, our reference tool, when needed, will be the current operational version of the Bougeault scheme. For differences with the 1985 published version, the reader is referred to Gerard and Geleyn (2005).
In Section 2 the basic concepts leading to the mass flux approach and to its above-mentioned combination with a single ascent and a moisture convergence closure will be recalled. Section 3 will be a short introduction on controversies around closure and QE issues. In Section 4 we shall introduce our own search of tracks meant to go around the underlying problems (those of the basic analysis by Mapes), before a short concluding Section 5.

II) Bougeault's mass-flux approach and its particularities

We start from the following principles:

- any deep-convective parameterisation requires some knowledge of the host model's 'resolved' tendencies for its closure;
- given the scales we are targeting at, we must parameterise in an hydrostatic-type framework an essentially non-hydrostatic phenomenon (the integral link between pressure and geopotential cannot be the same for the 'environment' and the 'cloud' if they have the same basis and top);
- parameterised convection is basically intermittent at a given grid-point and we must account for that fact (conditional activity);

- most importantly, while ‘visible’ convection appears like a local auto-organised process, its ‘invisible’ conditions of existence and back-influences on the basic flow are very much of a large-scale type.

Hence, even if parameterisation schemes look at first glance like being built to maintain the correct local vertical gradients of temperature and humidity, we must consider that their main role in the host model is to ultimately interact with the intensity and with the horizontal scale of larger-scale dynamical adjustment motions, up to those of the Hadley cell in case of the ITCZ. The need to establish this distinction is emphasised by tropical observations (Fig. 1): while the convectively active areas are characterised by unstable profiles (the local ‘return to neutrality’ is all but instantaneous), the averaged tropical situation is hardly favourable to a global convective activity (the time-rate of ‘return to neutrality’ cannot therefore be dictated only by the intensity of local imbalances).

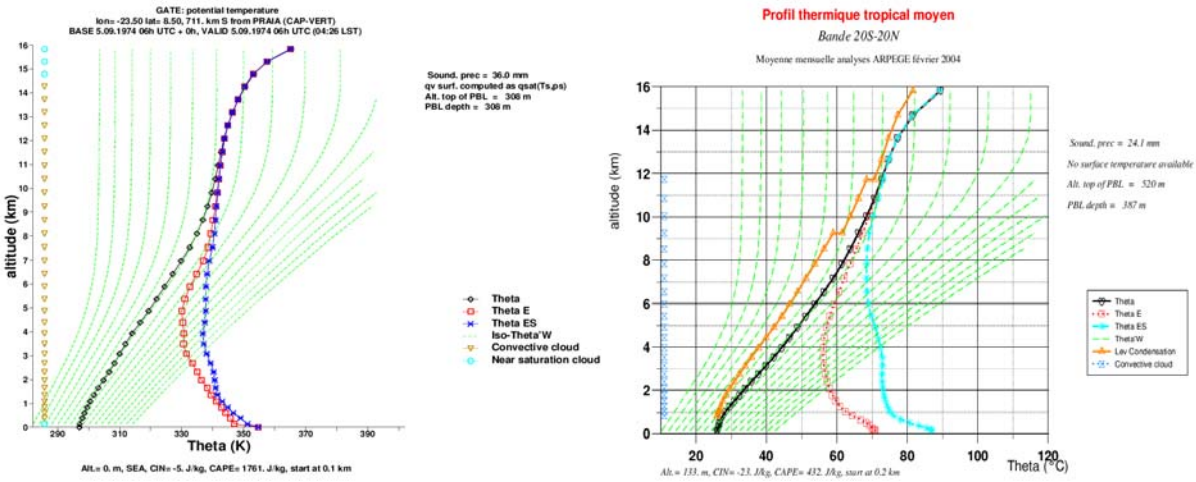

Figure 1: *Tephigrams of a GATE sounding (left) and of the averaged tropical state (right). CAPE being proportional to the area on the right of the most right curve but colder than the warmest point below on the curve next to the left, one sees a lot of conditional instability (of the ‘first kind’) in the ‘perturbed’ case (left) and relatively little in the ‘mean’ one (right).*

We now look at the most basic version of the single-ascent mass-flux framework, obtained under the hypothesis (i) of steady cloud-ascent behaviour and (ii) of negligible updraft area:

$$\left[\frac{\partial \overline{\psi}}{\partial t}\right]_{CV} = -\frac{\partial}{\partial p}\left[M_c\,(\overline{\psi} - \psi_c)\right]$$

$$\frac{\partial M_c}{\partial p} = D - E$$

$$M_c \frac{\partial \psi_c}{\partial p} = E\,(\psi_c - \overline{\psi})$$

where the first equation expresses the sole convective tendency with ψ a generic notation for any conservative property, M_c represents the mass-flux expressed in units of a ‘negative omega’ vertical velocity, D and E are respectively the detrainment and entrainment rates expressed in inverse time units. The subscript c marks the cloud-ascent specific properties and the averaging sign the host model resolved (‘large-scale’ (LS)) values.

We don’t address here the question of deriving an expression for ψ_c. So, assuming that ψ_c has been computed by some existing method, the closure problem becomes that of expressing two out of the three quantities M_c, D and E. In Bougeault (1985) the approach is somewhat different and can be symbolised (on the basis of the same notations) as follows:

$$\frac{d\overline{\psi}}{dt} = -\omega_{LS}\frac{\partial\overline{\psi}}{\partial p} - \left[M_c \frac{\partial\overline{\psi}}{\partial p} + \overline{D}.(\overline{\psi} - \psi_c) - g\frac{dJ_\Psi}{dp} \right] - g\frac{dJ_\Psi}{dp}$$

$$\frac{\partial M_c}{\partial p} \neq D - E \ \ but \ \ \left\{ \int \left(\frac{\partial h}{dt} \right)_{CV} = 0 \ \Rightarrow \ \overline{D} \right\} ; \left(M_c \frac{\partial \psi_c}{\partial p} = E(\psi_c - \overline{\psi}) \right)$$

$$\int M_c \frac{\partial\overline{q}}{\partial p} = \int -\omega_{LS}\frac{\partial\overline{q}}{\partial p} - g\frac{dJ_q}{dp} \ \ (Closure)$$

where the bracketed terms of the first equation correspond to the full RHS of the first equation of the previous set, the second equation of the previous set has been replaced by an integral constraint on a now uniform detrainment rate and the closure indeed expresses the local consumption (LHS) of the total moisture convergence (RHS). $J\psi$ represents the non-convective sub-grid scale fluxes (turbulent ones in general) and, for the sake of simplicity, horizontal advection has been omitted.

Basically, for the convective parameterisation alone, the straight link between M_c, D and E has been replaced by a rule of compensation between convective motions and the turbulent part of their feeding. This has the important consequence that the $\omega_{LS}+M_c$ residual value (what the large scale 'feels' as effective vertical motion) creates terms that are in balance with all local motions while, at steady state, the controlling moisture budget becomes independent of this effective vertical motion:

$$\int \overline{D}.(\overline{q} - q_c) \cong \int g.\frac{dJ_q}{dp}$$

This is in essence the way in which the basic Bougeault scheme tries to make the above-mentioned crucial distinction (illustrated by Figure 1) and probably one of the main reasons of its long-lasting value. Indeed all improvements made later to the scheme never touched this crucial point. But we must also point out that the associated advantage bears in itself its own limitations, because it is obtained only under the too restrictive conditions of (a) cloud stationary behaviour, (b) no possible storage or destorage of moisture from one time step to the next and (c) independence of the moisture source from local convective activity.

III) Some controversies and their link with this note

As any phenomenon being in quasi-equilibrium with its environment -at least at some scales-, deep convection needs both a dissipative mechanism (friction) and a self-enhancing-type activation. Concerning the latter, two theories (CISK for Conditional Instability of the Second Kind & WISHE for Wind Induced Surface Heat Exchange) have been fighting for recognition in the past twenty years. We shall here limit ourselves to the static view of the problem (already complicated enough), therefore without reference to wave propagation arguments.

The CISK approach is to say "condensation ⇒ buoyancy ⇒ updraft motion ⇒ surface pressure drop ⇒ low level convergence ⇒ more moisture ⇒ condensation …", but the WISHE advocates then ask '*Where does the moisture comes from?*'.

The WISH approach is to say "condensation + ascent ⇒ balanced profile maintenance ⇒ sinking in dry regions balanced by radiation ⇒ need of a return flow ⇒ stronger wind leading to more evaporation ⇒ more available moisture ⇒ condensation + ascent …", but the CISK advocates then ask '*What determines the balanced profile?*'.

In fact the truth seems to be situation- and scale-dependent, but there is an important induced difference in the link between what we named earlier the 'visible' and 'invisible' parts of the convective circulations. In the CISK case, 'convection' drives the 'large-scale circulation' while, in the WISHE case, 'convection' controls the 'large-scale circulation'.
This brings us back to the question of the separation between local and larger-scale properties of a given parameterisation scheme. Experimentally it can be shown (Geleyn and Rochas, 1987) that the Bougeault scheme, although having a closure assumption clearly of the CISK type, can have both CISK and WISHE behaviours, something surely related to our previous remark about the virtue of replacing the link between M_c, D and E by a rule of compensation.
Given this reassuring practical result and since, when coming to parameterisation issues, the CISK/WISHE controversy boils down to something we already announced as of second importance, it is time to come to the second and more recent controversy, about the nature and role of QE in understanding and parameterising deep convection.

Historically speaking, the evolution of the QE concept is roughly the following (cf. Mapes, 1997):

- whatever feedback and causality might be at work, it was realised quite early that QE is verified at large scales but not necessarily at scales below;
- studying the phenomenology of convection did lead to the mass-flux concept that helped to codify the issues around QE (Arakawa and Schubert, 1974);
- this shifted the old problem of convective closure from budgets to complex questions about the dynamics of convective circulations;
- but the (likely misleading) answer was to replace a tractable question "which convective clouds are likely to develop in a given environment and which feedback do they have on this environment?" by a more difficult one "which are the quantities that QE convection leaves unchanged?".

Beside that last issue, there is also the problem that QE is not considered under the same angle of view depending whether one prefers the CISK or WISHE theory. In the CISK case, convective circulations are determining the 'larger-scale' vertical motions that in turn force convection. In the WISHE case, only the residual aspects matter (weight and counter-weight motions need a small additional push corresponding to the targeted additional transport, if we take a mechanical analogy).

All this giving the impression that the QE concept has been over-used and/or over-interpreted, it is not surprising that some 'anti-QE' thinking started to appear. Mapes (2003) synthesises it as follows:

- scales are not separable since the 'invisible part' of convection is at the scale of the Rossby radius of deformation;
- forcing and answer are not really separable either (even if we may add that the return flow must be accounted for at the grid-box scale in any parameterisation scheme);
- there is no 'under-law' of convective regions' dynamics that aggregates local behaviours to a simple balance.

We shall see later how to try and convert these negative statements into some positive proposals, but let us mention here that this way to bring back QE to its simplest expression indirectly confirms some impossibility to arbitrate between CISK and WISHE ideas. Indeed, following Le Châtelier's chemical rule, if convective heating follows cooling by adiabatic ascent (somehow equivalent to the control role of convection) the resulting effect would be cooling but if convective heating precedes cooling by adiabatic ascent (somehow equivalent to the driving role of convection) the resulting effect would be heating. Testing this on the basis of statistical observed differences between active and non-active periods at some tropical locations however shows conflicting results (see Figure 2, adapted from Mapes (1997)).

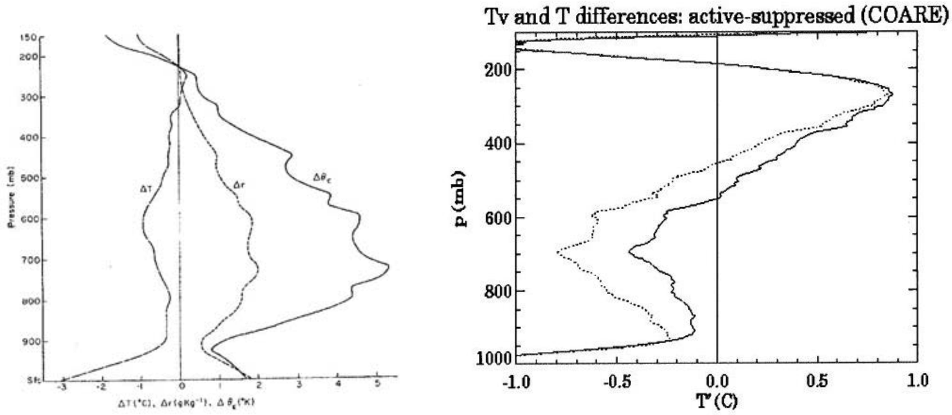

Figure 2: *Left, ~300 Venezuelian sounding (Betts, 1974); right, TOGA-COARE experiment. The left curves of both diagrams represent the difference between convective and non-convective periods in terms of averaged temperatures. One sees that, away from the PBL where turbulence might bias the results, the trend is ambiguous.*

IV) A possible path to revise the basic parameterisation concepts

If believing in the need for a new perspective, Figure 3, also adapted from Mapes (1997), shows how to link the changes in our nature's understanding to changes in modelling paradigms.

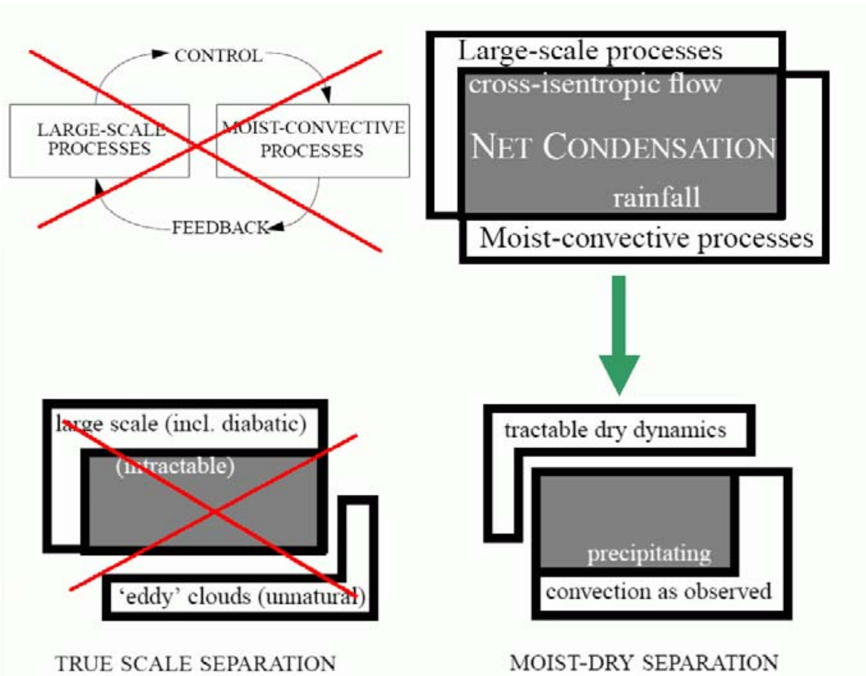

Figure 3: *Top row, nature's understanding; bottom row, parameterisation schemes' boundary choices. Left column, impossible (?) problematic; right column, path not yet enough explored.*

A first immediate consequence is that there is an important intermediate result that is missing (in the 'physical' sense since there is always a mathematical equivalent) in current deep convective parameterisation schemes: the vertical distribution of the condensation rate inside the 'cloud-ascent'. Let us call it BCC (Bulk Convective Condensation) and remark that, if we make it an obliged path for parameterisation calculations, (A) the moist-dry separation of Fig. 3 becomes far easier to track and (B) the distinction between 'resolved' and 'convective' types of precipitation may be ironed out via an appropriate addition of both forms of condensation before other computations relying on this quantity.

This mention of the 'large-scale-type' precipitations (linked to negative large scale ω_{LS}) brings us to the question of the representativity of the large-scale vertical velocity for the parameterisation of sub-grid scale condensation. Let us go back to the first equation of Bougeault's interpretation of the mass-flux concept. In deep convective conditions the absolute value of M_c will be slightly greater than that of ω_{LS} and both will be much bigger than the absolute value of the environmental vertical velocity $\omega_e = \omega_{LS} + M_c$. In other words, the computed large-scale vertical velocity is just the average of the (rare but intense) cloud ascents and of a slightly sinking environment everywhere. Hence the large scale vertical advection term is dynamically meaningless (*but model-wise unavoidable*) and has to be compensated by a good estimate of the mass flux, slightly bigger thanks to surface evaporation. Thus, if QE is doubtful, the mass-flux parameterisation schemes **should not use the diagnosed large-scale vertical velocity as input**, to avoid the risk of a double counting or of an uncontrolled feedback loop.

But what is then left as possible input for the closure assumption? Basically we have CAPE (Convective Available Potential Energy), CIN (Convective INhibition energy) and moisture convergence (the 'good old concept' introduced by Kuo (1965) in order to get rid of the convective adjustment framework and that still shows some usefulness when skilfully used, cf. Section 2). If we come back to our new 'BCC' goal, there is an obvious link with moisture convergence but one that certainly should not lead to identity. Hence the idea to find a 'moisture availability' that goes back to moisture convergence in the ideal case of fulfilled QE but that also encompasses the CAPE and CIN information in the more general case of an evolving behaviour of convective activity. But we may even go further in the way to eradicate hidden QE thinking from the design of parameterisation schemes if we decide to relax the first hypothesis of the basic mass-flux equations, i.e. the one about steady cloud-ascent behaviour. For reasons that would be too long to develop here, this becomes possible if the parameterisation scheme is organised around the provision of BCC to further calculations, this closing a hopefully virtuous circle.

V) Conclusion

The ideas expressed in Section 4 are for the time being not yet fully concretised or tested. There are however encouraging signs that they may lead to a positive evolution of deep convection parameterisation. Furthermore they are not contradicting what was the answer to our 'title question', namely that the link between moisture convergence and convective rainfall is strong enough to allow schemes carefully based on such a closure to be robust and applicable even when the balance is less accurate. Hence we may conclude that Bougeault's special application of the mass-flux concept somehow anticipated the steps we are now advocating, the present proposal being an evolution of the former in order to cover meso-scale-organised and/or rapidly evolving environmental conditions.

References:

Arakawa, A. and W.H. Schubert, 1974: Interaction of a cumulus cloud ensemble with the large-scale environment; Part I. *J. Atmos. Sci.*, **31**, pp. 674-701.

Betts, A.K., 1974: Thermodynamic classification of tropical convective soundings. *Mon. Wea. Rev.*, **102**, pp. 760-764.

Bougeault, P., 1985: A simple parameterisation of the large-scale effects of cumulus convection. *Mon. Wea. Rev.*, **113**, pp. 2108-2121.

Geleyn J.-F. and G. Rochas, 1987: A study of the 'feed-back' aspects of diabatic forcing in the French large-scale hemispheric model 'Emeraude'. *'Diabatic forcing'* **ECMWF Workshop**, pp. 177-208.

Gerard, L. and J.-F. Geleyn, 2005: Evolution of a subgrid deep convection parameterisation in a Limited Area Model with increasing resolution. To appear in *Q. J. R. Meteorol. Soc.*.

Kuo, H.L., 1965: On formation and intensification of tropical cyclones through latent heat release by cumulus convection. *J. Atmos. Sci.*, **22**, pp. 40-63.

Mapes, B.E., 1997: Equilibrium vs. activation control of large-scale variations of deep convection. In '*The physics and parameterisation of moist convection*', **Kluwer Academic Publishers**, pp. 321-358.

Mapes, B.E., 2003: An information-based approach to convective closure. In '*Current issues in the parameterisation of convection*', **BMRC Research Report N°93**, pp. 89-96.