Review


Jun-Ichi Yano 1*, Jean-François Geleyn 1, Martin Köllner 2, Dmitrii Mironov 2, Johannes Quaas 3, Pedro Soares 4, Vaughan T. J. Phillips 5, Robert S. Plant 6, Anna Deluca 7, Pascal Marquet 1, Lukrecia Stulic 8, Zeljka Fuchs 8.

1 CNRM/GAME UMR 3589, Météo-France and CNRS, 31057 Toulouse Cedex, France
2 DWD, Offenbach, Germany
3 University of Leipzig, Leipzig, Germany
4 University of Lisbon, Lisbon, Portugal
5 University of Lund, Lund, Sweden
6 University of Reading, Reading, U.K.
7 Max Planck Institute for the Physics of Complex Systems, Dresden, Germany
8 University of Split, Split, Croatia

* Author to whom correspondence should be addressed; jiy.gfd@gmail.com, [Tel] +33 5 6107 9359, [Fax] +33 5 6107 9326.

Received: xx / Accepted: xx / Published: xx

Abstract: The research network “Basic Concepts for Convection Parameterization in Weather Forecast and Climate Models” is organized under European funding (COST Action ES0905) for the period of 2010–2014. Its extensive brain storming suggests how the subgrid–scale parameterization problem in atmospheric modelling, especially for convection, can be examined and developed from robust theoretical basis. Our main cautions are current emphasis on massive observational data analyses and process studies. The closure and the entrainment–detrainment problems are identified as the two highest priorities for convection parameterization under the mass–flux formulation. A need is emphasized for a drastic change of the current European research culture for policies and funding in order not to further deplete the visions of the European researchers for focusing on those basic issues.

Keywords: Parameterization; Convection; Subgrid Scales
1. Introduction

The research network COST Action ES0905 “Basic Concepts for Convection Parameterization in Weather Forecast and Climate Models” is organized under European funding over a period of 2010-2014. The present paper constitutes a final scientific report of the network activity. The achievements of the present Action are closely examined by following each task agreed, and each question listed in the Memorandum of Understanding (MoU, available at: www.cost.eu/domains_actions/sem/Actions/ES0905). In some cases, a question in concern turns out to be ill-posed or ambiguous. The present report acknowledges such instances, and indeed, considers the need for a re-evaluation of some aspects of the MoU to be an outcome of the Action in its own right.

The report is only concerned with the scientific developments and achievements of the Action network. For information on the actual Action activities (meetings and documents), the readers are strongly encouraged to visit the Action Web site (convection.zmaw.de).

Our major achievement as a deliverable is a monograph (Plant and Yano 2014). Reference to this monograph is made frequently throughout the report for this reason. The present report is another key deliverable complementing this monograph by more succinctly summarizing the current convection parameterization issues.

The present report is assembled by the lead author under his responsibility as a chair of the present Action. Many have contributed to this specific process. Those who have critically contributed are asked to be co-authors, including working group (WG) leaders (JFG, MK, DM, JQ). Many others have contributed text segments, by proof reading parts or entire of the text, as well as general comments. These contributions are listed in the acknowledgements at the end of the present report. Though the present report does not intend to replace an official final report submitted to the COST office, it expresses collective positions of the contributing authors also well reflecting the overall Action achievements.

1.1. Overview

The main objective of the Action has been, as stated in MoU, “to provide clear theoretical guidance on convection parameterization for climate and numerical weather prediction models”.

Here, the problem of parameterization arises because both the weather–forecast and the climate models run only with limited spatial resolutions, and thus many physical processes are not properly represented by falling short of the resolution required for adequate explicit simulation of the process. In other words, there are processes in the “subgrid scales”, which must somehow be included as a part of a model in an indirect, parameteric manner. Such a procedure is called parameterization (cf., McFarlane 2011). Convection is one of the key processes to be parameterized considering its importance in heat and moisture budget of the atmosphere (cf., Arakawa 2004), and the one upon which the present Action is focused.

Here, although the word “parameterization” is often used in a much wider sense in the literature, in the present report, the term is strictly limited to the description of subgrid–scale processes. Many of the atmospheric physical processes (notably cloud microphysics) must often be phenomenologically described by vast simplifications. However, such a phenomenological description should not be confused with the parameterization problem.
The parameterization problem is often considered a highly technical, “engineering” issue without theoretical basis. Often, it is even simply reduced to a matter of “tuning”. The goal of the Action is to suggest how the parameterization problem can be addressed from a more basic theoretical basis, both from perspectives of theoretical physics and applied mathematics. For this purpose, the extensive brainstorming has been performed by organizing a number of meetings.

Here, the results of these theoretical reflections are reported by closely following what has been promised in the MoU. The focus is on the technical questions listed in MoU. However, a more basic intention is, by examining these questions, to suggest more generally what can be addressed from more fundamental perspectives of theoretical physics and applied mathematics, and how. For this reason, efforts are always made to add general introductory remarks in introducing each subject.

1.2. A Key Achievement

MoU specifically lists (Sec. B.1 Background) the following convection-related processes that were still to be resolved: too early onset of afternoon convection over land, underestimation of rainfall maximum, failure to represent the 20–60 day planetary–scale tropical oscillation (the Madden–Julian oscillation). We can safely claim that one of the problems listed there, the afternoon convection, is now solved by the efforts under the present Action. As it turns out, the key is to examine a closure in convection parameterization in a more careful manner, as will be further discussed in T1.1. This is also considered a good example case for demonstrating the importance of theoretical guidance on the convection parameterization problem.

1.3. Identified Pathways

As MoU states, “The Action proposes a clear pathway for more coherent and effective parameterization by integrating existing operational schemes and new theoretical ideas”. As it turns out, instead of a unique pathway, we identify three major pathways for pursuing such an endeavour (Yano et al. 2013a): (1) fundamental turbulence research, (2) close investigations of the parameterization formulation itself, and (3) better understanding of the processes going on both within convection and the boundary layer.

The first approach is based on our understanding that the atmospheric convective processes are fundamentally turbulent. Thus without fundamental understanding of the latter, no real breakthrough can be expected in the convection parameterization problem. The second is rather conventional, and even often considered obsolete, but we very strongly emphasize the importance of a thorough understanding how a given parameterization actually works in order to improve it. We believe that proper emphasis on the first two major pathways is critically missing in the current research efforts. The second is probably even more important than the first for the reasons to be discussed below.

The third is the currently most widely-accepted approach with the use of cloud–resolving models (CRMs) as well as large–eddy simulations (LESs). We are rather critical of the current over-reliance on this approach. Our criticisms are double edged: these studies must be performed with a clear pathway leading to an improvement of a parameterization in mind. At the same time, much in–depth analysis for identifying precise mechanisms (e.g., under energy cycle: cf., Q1.2.1) associated with a given process is required (cf., Sec. 5.1).
Here, the second approach should not be confused with a more conventional, “blind” tuning. Emphasized here is an importance of in-depth understanding of a parameterization itself in order to improve it, and we have to know why it must be modified in a particular manner, and we should also be able to explain why the model can be improved in this manner.

A good historical lesson to learn from the turbulence research is an improvement of the so-called QN (quasi–normal) model into the EDQN (eddy–damping quasi–normal) model in simulating turbulent kinetic energy spectrum (cf., Ch. VII, Leiseur 1987). This improvement was not achieved by any process study of turbulent motions, but rather a close investigation of the QN model itself, even at a level of physical variables which are not explicitly evaluated in actual simulations. As identified, the skewness, one of such variables, tends to steepen with time in a rather singular manner. Thus it suggests an additional damping to the skewness equation is necessary. This further suggests how the kinetic energy equation must be modified consistently.

This is not a simple “tuning” exercise, but a real improvement based on a physical understanding of the behavior of a given parameterization. Without in-depth understanding, no real improvement of a parameterization would be possible. Just imagine, if the author (Orszag 1970) has had focused on intensive process studies of turbulence based on, say, direct numerical simulations (DNSs): he would never have identified a problem with the skewness equation in QN. Note that the key issue is in self-consistency (cf., T2.4 below) of the QN formulation itself, but nothing to do with number of physical processes incorporated into QN.

1.4. Model Comparisons and Process Studies

We emphasize, as stated in MoU, that the aim of the present Action is to “complement” the already existing model comparison studies. In other words, from the outset, we did not intend to perform model comparison studies by ourselves a priori. Rather we are skeptical against those existing model comparison studies: how much we have learned under the model comparison since the pioneering work by Ceselski (1973)? Do the recent comparison studies provide any deeper insights on parameterization?

This is not intended to discredit all the benefits associated with model comparison exercises. A single-column configuration typically adopted for comparison studies is extremely helpful for identifying the workings of subgrid–scale parameterizations in a stand–alone manner with large–scale (resolved–scale) processes prescribed as column–averaged tendencies. These tendencies are often taken from observations from a field campaign, thus these are expected to be more reliable than those found in stand–alone model simulations, or even a typical assimilation data for forecast initialization. In this manner, the role of individual subgrid–scale processes can clearly be examined. For example, the surface fluxes can be prescribed so that the other processes can be examined without feedback from the surface fluxes.

On the other hand, a process study associated with a model comparison can lead to misleading emphasis for the parameterization development. For example, Guichard et al. (2004) suggested...
the importance of transformation from shallow to deep convection in a diurnal convective cycle, as phenomenologically inferred from their CRM simulations. No careful investigation on a possible mechanism behind was performed there. This has, nevertheless, led to extensive process studies on the shallow–to–deep transition (e.g., Yano and Plant 2012a and the references therein: see also Q2.3.1). This conclusion would have been certainly legitimate for guiding a direction for the further studies of the convective processes. However, Guichard et al. (2004) even suggested this transformation process as a key missing element in parameterizations. The difference between these two statements must clearly be distinguished. A suggested focus on the transformation process does not give any key where to look within a parameterization itself: is it an issue of entrainment–detrainment or closure (cf., Sec. 2)?

As it turns out, the closure is rather a key issue for this problem as demonstrated by Bechtold et al. (2014) and fully discussed in T1.1, but rather in contrast to other earlier attempts. Here, one may argue that the transformation process is ultimately linked to the closure problem. However, how can we see this simply by many process experiments? And how we identify a possible modification of the closure in this manner? It would be similar to asking Orszag (1980) to run many DNSs and figure out a problem in the skewness equation of QN. Note that even a skewness budget analysis of DNSs would not point out the problem in QN. We should clearly distinguish between process studies and the parameterization studies: the latter does not follow automatically from the former.

1.5. Organization of the Action

In the following four sections, we examine our major achievements by following our four major activities:

1. Mass-flux based approaches (Sec. 2)
2. Non-Mass Flux based approaches (Sec. 3)
3. High-Resolution Limit (Sec. 4)
4. Physics and Observations (Sec. 5)

These four activities, respectively roughly cover the four secondary objectives listed in MoU (Sec. C.2):

1. Critical analysis of the strengths and weakness of the state-of-the-art convection parameterizations
2. Development of conceptual models of atmospheric convection by exploiting methodologies from theoretical physics and applied mathematics
3. Proposal of a generalized parameterization scheme applicable to all conceivable states of the atmosphere
4. Defining suitable validation methods for convection parameterization against explicit modeling (CRM and LES) as well as against observations, especially satellite data
These secondary objectives are furthermore associated with a list of Tasks to be achieved and a list of Questions to be answered in MoU. In the following sections, we examine how far we have achieved the promised Tasks, and then present our answers for the Questions listed for each category in the MoU. Note that these Tasks and Questions are given by bold–face headings starting with the upper–case initials, T and Q, respectively.

The assigned numbers in MoU are used for the Tasks, whereas the MoU does not assign any numbers to the Questions. Here, the order of the Questions is altered from the MoU so that they are presented side–by–side with the listed Tasks in order. Numbers are assigned to the Questions accordingly. In answering these questions, various new theoretical ideas are often outlined. However, we consider that full development of these ideas are beyond the scope of the present report. References to other recent papers produced by the Action members are made when appropriate to allow readers to delve more thoroughly into some of those ideas that have been pursued to date.

2. Mass-flux based approaches

The majority of both operational weather–forecast and climate–projection models adopt mass–flux based approaches for convection parameterization.

In mass–flux based approaches, the key issues clearly remain the closure and the entrainment–detrainment (Yano et al. 2012a). In spite of progress under the present Action, we are still short of identifying ultimate answers to both issues. Thus, the best recommendation we can make is to re–emphasize an importance of focusing on these two key issues in future research on convection parameterization so long as we decide to stay with the mass–flux based formulation. Presently, maintenance of mass–flux formulation is the basic strategy of all the major operational research centers. For this reason, the following discussion is also naturally focused on these two issues along with other related issues.

However, it should be recognized that the mass–flux formulation is not without limit. Remember especially that this formulation is specifically designed to represent “plume” type convection such as convective cumulus towers as well as smaller–scale equivalent entities found in the boundary–layer and over the inversion layer (cloud topped or not). The formulation clearly does not apply to disorganized turbulent flows typically found in the boundary layer on much smaller scales. An important distinction here is that transport by convective plumes and turbulent mixing are inherently non–local and local, respectively. Thus, a qualitatively different description is required. The last point may be important to bear in mind because these disorganized flows are likely to become more important processes to be parameterized with increasing horizontal resolutions of the models (cf., Sec. 4).

2.1. Overview

Reminders of some basics on the mass-flux convection parameterization (cf., Yano 2014a, b) are due first. As the name suggests, the quantity called mass flux, $M$, which measures vertical mass transport by convection, is a key variable to be determined. Once it is known, in principle, various remaining calculations are relatively straightforward in order to obtain the final answer of the grid-box averaged feedback of convection, as required for any subgrid-scale parameterization. This approach works well
so long as we stay with a standard thermodynamics formulation (with the standard approximations: cf., Marquet 2011, 2014a Marquet and Geleyn 2014, T2.4 below) and microphysical processes (including precipitation) can be neglected. The latter must either be drastically simplified in order to make it fit into the above standard formulation, or alternatively, an explicit treatment of convective vertical velocity is required (cf., Donner 1993: see further Q2.1.2 below). The last is a hard task by itself under the mass-flux formulation, as reviewed in a book chapter (Yano 2014c).

As assumed in many operational schemes, the mass flux can be separated into two factors, one for the vertical profile and the other for a time-dependent amplitude:

\[ M = \eta(z) M_B(t) \]

Here, a subscript \( B \) is added to the amplitude \( M_B(t) \), because customarily it is defined at convection base, although it is misleading to literally consider it to be determined at the convection base for the reason to be explained immediately below (see also Q1.3.1).

Such a separation of variables becomes possible by assuming a steady state for parameterized convective ensembles. This assumption is usually called the “steady plume” hypothesis because these convective ensembles are usually approximated by certain types of plumes. This assumption naturally comes out when the convective scale is much smaller than that of the “resolved” large scales. Under this situation, the time scale for convection is so short that we may assume that convective ensembles are simply in equilibrium with a large-scale state.

Under this hypothesis, we should not think in terms of a naive picture that convection is initiated from a boundary-layer top and gradually grows upwards. Such a transient process is simply not considered (Yano 2011). As a whole, under this standard approximation, a life cycle of individual convective clouds, including an initial trigger, is not at all taken into account. In other words, in order to include those processes, this approximation must first be relaxed.

Under the standard “plume” formulation, a vertical profile, \( \eta(z) \), of mass flux is determined by the entrainment and the detrainment rates, \( E \) and \( D \):

\[ \frac{\partial M}{\partial z} = E - D \]  

(2.1)

Here, entrainment and detrainment, respectively, refer to influx and outflux of air mass into and out of the convection (convective plume), as the above formula suggests. Thus, a key issue reduces to that of prescribing the entrainment and the detrainment rates. Once these parameters are known, a vertical profile of mass flux, \( \eta(z) \), can be determined in a straightforward manner, by vertically integrating Eq. (2.1: cf., T1.4).

A standard hypothesis (convective quasi-equilibrium hypothesis) is to assume that convection is under equilibrium with a given large-scale state. As a result, the amplitude of convection is expected to be determined solely in terms of a large-scale state. This problem is called the closure.

Thus, closure and entrainment-detrainment are identified as the two key problems. Here, it is important to emphasize that, against a common belief, the trigger is not a part of the mass-flux convection parameterization problem for the reason just explained. Though we may choose to set the convective
amplitude to zero (because convection does not exist always), this would simply be a part of the closure formulation.

**T1.1: Review of current state of art of closure hypothesis**

The closure issue is extensively reviewed in Yano et al. (2013b). This review has further facilitated in resolving the afternoon convection problem (Bechtold et al. 2014): the question of onset of convection in late afternoon rather than in early afternoon, as found globally over land, by following the sun with the maximum of conditional instability as conventionally measured by CAPE (convective available potential energy). Many efforts were invested on modifying entrainment–detrainment parameters (e.g., Del Genio 2010, Stratton and Stirling 2012), because they appeared to control the transformation from shallow to deep convection (cf., Sec.1.4). However, as it turns out, the key is rather to improve the closure. Note that the modifications of entrainment–detrainment also achieves this goal, but typically in expense of deteriorating the model climatology. Our effort for a systematic investigation on the closure problem (Yano et al. 2013b) has greatly contributed in identifying this key issue.

The closure strategy tends to be divided into two dichotomous approaches by strongly emphasizing the processes either in the boundary layer (boundary-layer controlled closure: Raymond 1995) or in the free troposphere (parcel-environment based closure: Zhang 2002, 2003). The boundary-layer controlled closure tends to be more popular in the literature (e.g., Mapes 2000, Bretherton et al. 2004, Hohenegger and Bretherton 2011), probably due to the fact that the boundary layer is rich with many processes, apparently providing more possibilities. However, as clearly pointed out by Donner and Phillips (2003), the boundary-layer control closure does not work in practice for mesoscale convective systems, which evolve slowly over many hours, because the processes in the boundary layer are too noisy to be useful as a closure condition. In such situations it is rather the large-scale forcing (e.g., uplifting) from the free troposphere that controls convection. Recall that the trigger from the boundary layer is not a part of the standard mass–flux formulation, as already remarked.

The basic idea of the parcel-environment based closure is to turn off the influence of the boundary layer for modifying CAPE with time when constructing the closure. In this manner, an evolution of parameterized convection not influenced by noisy boundary–layer processes is obtained. The review (Yano et al. 2013b) emphasizes the superiority of the parcel-environment based closure against the boundary–layer controlled closure. After its completion, this parcel-environment based closure is actually adopted at ECMWF. It is found that this relatively straightforward modification of the closure essentially solves the problem of the afternoon convection (a proper phase for the convective diurnal cycle) without any additional modifications to the model (Bechtold et al. 2014).

The advantage of the parcel-environment based closure over the boundary-layer controlled closure can hardly be overemphasized, especially considering the current strong popularity of the latter. A fundamental inconsistency of the latter principle with the current mass–flux formulation cannot be overemphasized either, considering the overall lack of attention on this aspect in the literature. Nevertheless, we should not insist that the former closure always works, or that the boundary–layer control of convection is never important. For example, isolated scattered deep convection over intense surface heating, such as over land, may be more directly influenced by the boundary layer processes.
There is also an indication that the latter principle works rather well for shallow convection in practice (W. de Rooy, personal communication, 2014).

T1.2: Critical review of the concept of convective quasi-equilibrium

Convective quasi–equilibrium, as originally proposed by Arakawa and Schubert (1974), is considered one of the basic concepts in convection closure. A review by Yano and Plant (2012b), completed under the present Action, elucidates the richness of this concept with extensive potential possibilities for further investigating it from various perspectives. Especially, there are two contrasting possibilities for interpreting this concept: under a thermodynamic analogy, as originally suggested by Arakawa and Schubert, or as a type of slow manifold condition (or a balance condition: Leith 1980). The review suggests that the latter interpretation may be more constructive.

The review also suggests the importance of a more systematic observational verification of Arakawa and Schubert’s hypothesis in the form that was originally proposed. Surprisingly, such basic diagnostic studies are not found in the literature in spite of their critical importance for more basic understanding of this concept.

Furthermore, along a similar line of investigation, the convective energy cycle becomes another issue to be closely examined (Yano and Plant 2012a, c, Plant and Yano 2013). Q1.2.1 discusses this issue further, but in short, Arakawa and Schubert’s convective quasi–equilibrium is defined as a balanced state in the cloud–work function budget, which constitutes a part of the convective energy cycle. Importantly, the results from the energy–cycle investigations suggest that the concept of convective quasi–equilibrium could be more widely applicable than is usually supposed, with only a minor extension.

The principle of convective quasi–equilibrium is often harshly criticized from a phenomenological basis. For example, the importance of convective life–cycles is typically emphasized, which is not considered under quasi–equilibrium. The issue of self–organized criticality to be discussed in Q1.7 could be a more serious issue.

However, ironically, we have never made a quasi–equilibrium based convection parameterization fully working. Consider a precipitation time series generated by an operational convection parameterization, ostensibly constructed under the quasi-equilibrium hypothesis: it is often highly noisy suggesting that the system as operationally formulated does not stay as a slow process as the hypothesis intends to maintain (cf., Fig. 6 Yano et al. 2012b). In other words, operational quasi–equilibrium based convection parameterizations are not working in the way that they are designed to work. Making a quasi–equilibrium based parameterization actually work properly is clearly a more urgent issue before moving beyond the quasi–equilibrium framework. At least we need to understand, why it does not work in the way intended.

Q1.2.1: How can the convective quasi-equilibrium principle be generalized to a system subject to time-dependent forcing? How can a memory effect (e.g., from a convection event the day before) possibly be incorporated into quasi-equilibrium principle?

The importance of the issue raised here cannot be overemphasized. Even 40 years after the publication of the original article by Arakawa and Schubert (1974), it is very surprising to find that their original formulation is hardly tested systematically in the literature, as already mentioned in T1.2 above. Though we are sure that there are lot of technical tests performed at an operational level, none of them is carefully reported in the literature.
This issue can be considered at two different levels. The first is a more direct verification of Arakawa and Schubert’s convective quasi-equilibrium hypothesis (their Eq. 150) from observations. Here, the hypothesis is stated as

\[-\sum_{j=1}^{N} K_{ij} M_{Bj} + F_i = 0 \quad (2.2)\]

for the \(i\)-th convective type, where \(K_{ij} M_{Bj}\) is a rate that the \(j\)-th convective type consumes the potential energy (or more precisely, cloud work function) for the \(i\)-th convective type, \(M_{Bj}\) is the cloud–base mass-flux for the \(j\)-th convective type, and \(F_i\) is the rate that large–scale processes produce the \(i\)-th convective–type potential energy. Here, \(N\) convection types are considered. The matrix elements, \(K_{ij}\), are expected to be positive, especially for deep convection due to its stabilization tendency associated with warming of the environment by environmental descent.

Currently an intensive work is underway on this issue by J. I. Yano and R. S. Plant. An important preliminary finding is that some of the matrix elements, \(K_{ij}\), can be negative due to a destabilization tendency of shallow convection associated with the re–evaporation of detrained cloudy air.

Second, the convective quasi-equilibrium principle can be generalized into a fully-prognostic formulation, as already indicated by Arakawa and Schubert (1974) themselves, by coupling between an extension of their closure hypothesis (Eq. 2.2, or their Eq. 150) into a prognostic version (i.e., Eq. 142)

\[
\frac{dA_i}{dt} = -\sum_{j=1}^{N} K_{ij} M_{Bj} + F_i, \quad (2.3a)
\]

and the kinetic energy equation

\[
\frac{dK_i}{dt} = A_i M_{Bi} - D_{K,i} \quad (2.3b)
\]

presented by their Eq. (132). Here, \(A_i\) is the cloud work function, \(K_i\) the convective kinetic energy for the \(i\)-th convective type and the term \(D_{K,i}\) represents the energy dissipation rate. Randall and Pan (1993), Pan and Randall (1998) proposed to take this pair of equations as the basis of a prognostic closure. The possibility is recently re-visited by Yano and Plant (2012a, c), Plant and Yano (2013) under a slightly different adaptation.

This convective energy–cycle system, consisting of Eqs. (2.3a, b), describes evolution of an ensemble of convective systems, rather than individual convective elements. It can explain basic convective processes: e.g. convective life–cycles consisting of discharge (trigger) and recharge (suppression and recovery: Yano and Plant 2012c), as well as transformations from shallow to deep convection (Yano and Plant 2012a, Plant and Yano 2013). This result contains a strong implication, because against a common perception, the model demonstrates that an explicit trigger condition is not an indispensable ingredient in order to simulate a convective life cycle. Here, the life cycle of a convective ensemble is simulated solely by considering a modulation of convection ensemble under an energy-cycle description, keeping the evolution of individual convective elements implicit.

This energy-cycle formulation (2.3a, b) is, in principle, straightforward to implement into any mass-flux based convection parameterization, only by switching the existing closure without changing the entrainment-detrainment formulation. Most of the current closures take an analogous form to Eq. (2.2), which may be generalized into a prognostic form (2.3a). This is coupled with Eq. (2.3b), which computes
the convective kinetic energy prognostically. The latter equation is further re-interpreted as a prognostic equation for the mass flux, by assuming a certain functional relationship between $K_i$ and $M_{B,i}$. Here, a key is to couple shallow and deep convection in this manner, which are typically treated independently in current schemes.

Many prognostic formulations for closure have already been proposed in the literature in various forms, e.g., Chen and Bougeault (1992). However, it is important to emphasize that the formulation based on the convective energy cycle presented herein is the most natural extension of Arakawa and Schubert’s convective quasi-equilibrium principle to a prognostic framework.

Q1.2.2: Are there theoretical formulations available that could be used to directly test convective quasi-equilibrium (e.g., based on population dynamics)?

Clearly this question is inspired by a work of Wagner and Graf (2010), which suggests that it is possible to derive a population dynamics system starting from Arakawa and Schubert’s spectrum mass-flux formulation (however see T2.4, Plant and Yano 2011). Thus, it is also natural to ask the question other way round: can we construct and test a closure hypothesis (e.g., convective quasi-equilibrium) based on a more general theory (e.g., population dynamics)?

We have turned away from this direction during the Action for several reasons: 1) so far we have failed to identify a robust theoretical formulation that leads to a direct test of convective quasi-equilibrium or any other closure hypothesis; 2) it is dangerous to introduce an auxiliary theoretical condition to a parameterization problem without strong physical basis. This can make a parameterization more ad hoc, rather than making it more robust; 3) the convective quasi-equilibrium can better be tested in a more direct manner based on Arakawa and Schubert’s original formulation as concluded in response to Q1.2.1.

T1.3: Proposal for a general framework of parameterization closure

A closure condition is often derived as a stationarity condition of a vertically-integrated physical quantity. The two best known choices are water vapor (e.g., Kuo 1974) and the convective parcel buoyancy (e.g., Arakawa and Schubert 1974). The latter leads to a definition of CAPE or the cloud work function, more generally. [See Q1.3.1 for an alternative possibility.]

A formulation for a closure under a generalization of this principle has been developed (Yano 2014d). It is found that regardless of the specific choice of a physical quantity (or of any linear combinations of those), the closure condition takes the form of a balance between large-scale forcing, $F_i$, and convective response, $D_{c,i}$, as in the case of the original Arakawa and Schubert’s quasi-equilibrium hypothesis, so that the closure condition can be written as

$$F_i + D_{c,i} = 0 \quad (2.4)$$

for the $i$-th convective type. It is also found that the convective response term takes a form of an integral kernel, or a matrix, $K_{i,j}$, in the discrete case, describing the interactions between different convective types, as in the Arakawa and Schubert’s original formulation based on the cloud-work function budget. As a result, the convective response is given by

$$D_{c,i} = \sum_j K_{i,j} M_{B,j}.$$
Arakawa and Schubert’s convective quasi–equilibrium principle (2.2) reduces to a special case of Eq. (2.4). This general framework is expected to be useful in order to objectively identify basic principles for choosing more physically based closure conditions.

Q1.3.1: Is it feasible to re-formulate the closure problem as that of the lower boundary condition of the system? Is it desirable to do so?

Formally speaking, the closure problem in mass-flux parameterization is that of defining the mass-flux at the convection base (cf., Sec. 2.1), and thus it may also be considered as a boundary condition. For example, the UM shallow-convection scheme is closed in this matter by taking a turbulent velocity measure as a constraint (Grant 2001: see also Neggers et al. 2004, Soares et al. 2004). However, as already discussed in Sec. 2.1, it is rather misleading to take the mass-flux closure problem as a type of bottom boundary condition, because what we really need is a general measure of convective strength, independent of any reference height, after a mass-flux vertical profile is normalized in a certain manner. It is just our “old” custom normalizing it by the convection-base value, but there is no strong reason to do so, especially if the mass-flux profile increases substantially from the convection base. This is the same reason as more generally why it is rather misleading to consider convection to be controlled by the boundary layer as already emphasized in T1.1. See Yano (2011) for more.

Q1.3.2: How does the fundamentally chaotic and turbulent nature of atmospheric flows affect the closure of parameterizations? Can the quasi-equilibrium still be applied for these flows?

The convective energy cycle system, already discussed in Q1.2.1, is also the best approach for answering this question. In order to elucidate a chaotic behavior we have to take at least three convective modes. Studies have examined the one and two mode cases so far (Yano and Plant 2012a, Plant and Yano 2013).

A finite departure from strict convective quasi-equilibrium may also be considered a stochastic process. Such a general framework, the method of homogenization, is outlined, for example, by Penland (2003). Specific examples of the applications include: Melbourne and Stuart (2011), Gottwald and Melbourne (2013).

T1.4: Review on current state of art of entrainment-detrainment formulations

Entrainment and detrainment are technical terms referring respectively to the rate that mass enters into convection from the environment, and exits from convection to the environment (cf., Eq. 2.1). A review on these processes is published as de Rooy et al. (2013) under the present Action.

T1.5: Critical review of existing methods for estimating entrainment and detrainment rates from CRM and LES

As addressed in de Rooy et al. (2013), there are two major approaches:

1) Estimate by directly diagnosing the influx and outflux through the convection–environment interfaces (Romps 2010, Dawe and Austin 2011). Both of these studies use an artificial tracer for identifying the convection–environment interfaces.
2) Less direct estimates based on a budget analysis of a thermodynamic variable (Siebesma and Cuijpers 1995, Swann 2001). The distinction between convection and environment is made by a threshold based criterion (vertical velocity, cloudiness, buoyancy, or a combination of those).

Unfortunately, these two approaches do not give the same estimates, but the former tends to give substantially larger values than the latter. The result suggests that we should not take the notion of the entrainment and the detrainment rates too literally, but they have meaning only under a context of a budget of a given variable that is diagnosed. Strictly, the latter estimate depends on a choice of a variable, as suggested by Yano et al. (2004). Also there is a subtle, but critical difference between the methods by Siebesma and Cuijpers (1995) and Swann (2001), as discussed immediately below.

It should be emphasized that the second approach is based on an exact formulation for a budget of a given transport variable (temperature, moisture) derived from an original full LES–CRM system without any approximations. Thus, if a parameterization scheme could estimate all the terms given under this formulation, a self–consistent evaluation of convective vertical transport would be possible. However, the main problem is that it is hard to identify a closed formulation that can recover such a result. The current mass–flux formulation is definitely not designed in this manner.

For the very last reason, neither approach gives entrainment and detrainment rates that lead to a mass flux profile that can predict vertical transport of a given variable exactly under a mass–flux parameterization. To some extent, Siebesma and Cuijpers (1995) make this issue explicit by including a contribution of an eddy convective transport term (a deviation from a simple mass–flux based estimate) as a part of the estimation formula.

Swann (2001), in turn, avoids this problem by taking an effective value for a convective component obtained from a detrainment term, rather than a simple conditionally–averaged convective value. As a result, under his procedure, the convective vertical flux is exact under the prescribed procedure for obtaining entrainment and detrainment in combination with the use of the effective convective value. Nevertheless, a contribution of environmental eddy flux must still be counted for separately. Furthermore, introduction of the effective value makes the convective–component budget equation inconsistent with the standard formulation, though a difference would be negligible so long as a rate of temporal change of fractional area for convection is also negligible.

Any of the estimation methods (whether direct or indirect) are also rather sensitive to thresholds applied for the distinction between convection and the environment. For example, Siebesma and Cuijpers (1995) show that the values of entrainment and detrainment rates vary by 50% whether considering convection as a whole or only its core part (defined to have both positive vertical velocity and buoyancy). This sensitivity stems from the fact that when convection as a whole is considered, the differences between cloud and environment are smaller than if using the cloud core. Under this difference, in order to recover the same total convective vertical transport, $M\varphi_c$, for an arbitrary physical variable, $\varphi$, which is defined by a vertical integral of

$$\frac{\partial \varphi_c}{\partial z} = -\frac{E}{M}(\varphi_c - \bar{\varphi}), \quad (2.5)$$

we have to assume different mixing coefficients (fractional entrainment rate), $E/M$, depending on this difference, $\varphi_c - \bar{\varphi}$, between convection (core or cloud) and the environment. The same argument follows when a more direct estimate of entrainment–detrainment rates is performed. Note that in some convective...
schemes, the eddy convective transport, $M'(\varphi_c - \bar{\varphi})$, is considered in terms of the eddy convective mass flux, $M'$, instead. Also note that being consistent with the analysis methods in concern, we assume only one type of convection in this discussion, dropping the subscript $i$ for now.

Here, we should clearly distinguish between the issue of diagnosis based on LES–CRM and the computations of a convective profile within a parameterization. In the former case, all the terms are simply directly diagnosed (estimated) from LES–CRM output, and thus a self–consistent answer is obtained automatically. On the other hand, in running a parameterization, none of those terms are known a priori, thus they must somehow be all diagnosed (computed) in a self–contained manner without referring to any extra data. Clearly, the latter is much harder.

The most fundamental reason that these LES–CRM based entrainment–detrainment estimates do not find a unique formulation nor a unique choice of threshold is that those CRMs and LESs do not satisfy a SCA (segmentally–constant approximation) constraint that is assumed under the mass–flux formulation, as discussed later in Q1.8. In principle, better estimates of entrainment–detrainment would be possible by systematically exploiting a model under SCA but without entrainment–detrainment hypothesis. However, such a possibility is still to be fully investigated (cf., Yano and Baizig 2012).

An alternative perspective to this problem is to add an additional vertical eddy transport term estimated by a turbulence scheme. This perspective is consistent with the formulation proposed by Siebesma and Cuijpers (1995), who explicitly retain the eddy transport in their diagnosis. Of course, the turbulence scheme must be developed in such a manner that it can give an eddy–transport value consistent with an LES–CRM diagnosis. This is another issue to be resolved.

Q1.5.1: From a critical review of existing methods for estimating entrainment and detrainment rates from CRM and LES, what are the advantages and disadvantages of the various approaches?

The approach by Romps (2010) and Dawe and Austin (2011) presumably provides a more direct estimate of the air mass exchange rate crossing a convection–environment interface. However, there are subtle issues associated with the definition of the convection–environment interface, and how to keep track of it accurately.

First note that to an inviscid limit, which is a good approximation in convective scales, there would be strictly no exchange of air mass by crossing an interface defined in a Lagrangian sense. Such an interface would simply be continuously distorted with time by a typical turbulent tendency of stretching and folding into an increasingly complex shape, presumably leading to a fractal. Such an interface evolution would numerically become increasingly intractable with time, with increasingly higher resolutions required. Clearly the computation results would be highly dependent on the model resolution.

An interface between convection (cloud) and the environment does not evolve strictly in a Lagrangian sense, but as soon as the cloud air evaporates, the given air is re–classified as an environment, and vice versa. Such a reclassification is numerically involved by itself, and the result would also sensitively depend on a precise microphysical evaluation for evaporation. In this very respect, we may emphasize the importance of returning to laboratory experiments in order to perform measurements not contaminated by numerical issues. Indeed a laboratory experiment can reveal many more details of entrainment–detrainment processes than a typical LES can achieve (Fig. 1: cf., Korczyk et al. 2012, see also Diwan et al. 2014).

Q1.8: In principle, better estimates of entrainment–detrainment would be possible by systematically exploiting a model under SCA but without entrainment–detrainment hypothesis. However, such a possibility is still to be fully investigated (cf., Yano and Baizig 2012).
A cautionary note should be raised in conjunction with laboratory experiments in relation to the life–cycle issues discussed previously. An LES–CRM simulation will produce many cumulus clouds and an estimation of entrainment/detrainment rates across the full simulation effectively produces an average over the individual cloud life–cycles in a manner that is well suited to the consideration of an ensemble of clouds within a parameterization. The laboratory experiments focus on a single, isolated, buoyant plume, which is at once to their great advantage and disadvantage. See Q1.6.1 for related issues.

See T1.5 for further comparisons of the entrainment–detrainment evaluation methods.

**T1.6: Proposal and recommendation for the entrainment-detrainment problem**

The most important general suggestion drawn from de Rooy *et al.* (2013) is an extensive use of CRM and LES in order to systematically evaluate the entrainment and the detrainment rates so that an extensive data base can be developed. As discussed in T1.5, such methodologies have already been well established.

Extensive LES studies for shallow convection have established that mixing between convection and the environment is dominated by lateral mixing across the convection–environment interfaces rather than a vertical mixing from the convection top, as proposed by Squires (1958a, b) and Paluch (1979). In this manner, it also establishes that the current entrainment–detrainment formulation for defining convective mass flux, $M$, under the formula (2.1) is more robust than other existing proposals.

Furthermore, de Rooy *et al.* (2013) suggest some specific research directions:

i) Critical fractional mixing ratio originally introduced in a context of a buoyancy sorting theory (Kain and Fritsch 1990): the critical fraction is defined as the mixing fraction between convective and environmental air that leads to neutral buoyancy. Mixing with less or more environmental air from this critical fraction leads to positive or negative buoyancy respectively. This division line is expected to play an important role in entrainment–detrainment processes.

ii) Relative-humidity dependence of entrainment-detrainment rate: the introduction of such dependence clearly improves model behavior (e.g., Bechtold *et al.* 2008, Derbyshire *et al.* 2011), although the mechanism behind is not yet well understood.

The vertical mass–flux profile is strongly controlled by detrainment as originally pointed out by de Rooy and Siebesma (2008: see also Derbyshire *et al.* 2011, Böing *et al.* 2012a) with theoretical arguments provided by de Rooy and Siebesma (2010). This finding has important consequences for the parameterization of convection: the critical mixing fraction correlates well with the detrainment rate, providing a possibility for taking it as a key parameter (de Rooy and Siebesma 2008, Neggers 2009, Böing *et al.* 2012a). Unfortunately, almost all of the current parameterizations do not yet take this aspect into account. For example, the Kain and Fritsch (1990) scheme assumes that entrainment and detrainment vary in opposite senses as functions of the critical mixing fraction. Some schemes have just begun to take this effect fully into account (de Rooy and Siebesma 2008, Neggers 2009).
Figure 1. A cross section of a thermal plume generated in a laboratory with use of a humidifier as a buoyancy source. Distribution of condensed water is shown by gray tone (courtesy: Anna Gorska and Szymon Malinowski).
In spite of this progress, as a whole, unfortunately, we clearly fail to identify any solid theoretical guiding principle for investigating the entrainment–detrainment problem. The main problem stems from the fact that a plume is a clear oversimplification of atmospheric convection, as discussed in Q1.6.1 next.

**Q1.6.1: What is the precise physical meaning of entrainment and detrainment?**

The concept of entrainment is best established under the original context of the entraining plume experiment performed with a water tank by Morton, Taylor, and Turner (1956). Once we try to extend this concept to moist atmospheric convection, we begin to face extensive controversies, some of which are discussed in de Rooy *et al.* (2013). In the moist convection context, even a precise physical meaning of the entrainment-detrainment concept is lost, as emphasized by Morton *et al.* (1997; see also Yano 2014e). It simply reduces to a method for calculating lateral (and sometimes vertical) mixing crossing the boundaries of the air that is designated as convection.

The original entrainment-plume model is based on a premise that the convective plume has a relatively well-defined boundary against the environment, also approximately fixed with time. This idea is schematically well represented in Fig. 2 of de Rooy and Siebesma (2010). This basic premise is also well summarized, for example, in the introduction of Squires and Turner (1962) by comparing this concept (as termed “entraining jet” in this paper) against the concept of a bubble or thermal. Here, in spite of the recent trend of more emphasizing the detrainment of air from the convective plume, this “jet” idea, in the sense of assuming a well-defined boundary with the environment, has hardly changed since then.

Another important premise, along with the existence of a well-defined boundary, is that the lateral exchange of the air between the convective plume and the environment is performed by eddies of scales much smaller than that of the plume itself. This idea is also schematically well represented by Fig. 2 of de Rooy and Siebesma (2010). The massive detrainment at the cloud top of individual clouds also contributes to the lateral exchange in an important manner.

However, do the true atmospheric moist convective systems actually behave in this manner? Our interpretation of Doppler radar measurements of winds given, for example, by Fig. 3 of Bringi *et al.* (1997), may provide hints to this question. Here, keep in mind that Doppler radar is typically sensitive to precipitation, not cloud-particles, whenever there is precipitation present. Thus, one cannot infer cloud-edge sharpness with most Doppler radar in precipitating convection.

Probably the most striking feature of this convective element captured by a series of Doppler radar images with a frequency of every few minutes is its transient behavior without representing any fixed boundary in time. Furthermore, the flows around a convective cloud are subjectively rather “laminar”. Within the limit of resolution of these radar measurements, we do not see any turbulent–looking “eddies” around the cloud. Instead, these “laminar” flows appear to provide extensive exchange of mass between the cloud and the environment. The frame (d’) in their figure is probably the best one to make this point with a well-defined laminar inflow at a middle level.

Examination of these images does not exclude an obvious possibility that there are extensive turbulent eddies contributing to mixing at the scales unresolved by the radar. However, it is hard to believe that these unresolved small–scale eddies are responsible for most of the mixing between convection and the environment. This doubt is particularly justified by the fact that the whole cloud shape changes markedly over time without a well–defined fixed convection–environment interface.
A three-dimensional animation of a boundary-layer convective cloudy plume prepared by Harm Jonker and his collaborators (personal communication, March 2009) also makes the same point: highly transient nature of the convective plume, from which it appears hard to justify the traditional “steadiness” hypothesis of the convective plume. Based on such observation, Heus et al. (2008) emphasize the existence of a “buffer zone” (descending shell) between the plume core and the environment. This “buffer zone” appears to roughly correspond to a “fuzzy” boundary of a cloud identified in terms of a high water–vapor concentration.

The concept also appears to be consistent with the interpretation presented above that there is no boundary fixed in time between the cloud and the environment. Keep in mind that the boundary between the cloud (i.e., visible cloud-particles) and environment is perfectly sharp and extremely well defined at any instant. The main issue here is that it fluctuates in time and has a fractal structure.

Of course, the argument above is slightly misleading in the sense that almost everyone would agree with a highly transient nature of realistic atmospheric convection. Moreover, many would also argue that the approximation of convection by a “steady plumes” adopted for the mass–flux convection parameterization, which also leads to a separation of the whole problem into closure and entrainment–detrainment, as introduced in Sec. 2.1, is a picture emerging only after an ensemble average of those individual clouds that has an equilibrium solution. In other words, schematics such as Fig. 2 of de Rooy and Siebesma (2010) should not be taken too literally. Thus, the real question would be how to re–construct a steady plume solution under an ensemble averaging procedure for those transient convective clouds with their interfaces with the environment continuously changing with time. Such a systematic procedure is still needed.

Q1.6.2: If they provide nothing other than artificial tuning parameters, how could they be replaced with more physically-based quantities?

Unfortunately, this question is not well posed. On the one hand, entrainment and detrainment are well–defined quantities that can be diagnosed objectively from CRM and LES, as already emphasized in T1.5. In this very respect, entrainment and detrainment are far from artificial tuning parameters, but clearly physically given. On the other hand, as emphasized in Q1.6.1, the basic physical mechanism driving these entrainment–detrainment processes is far from obvious. At least, the original idea of entrainment proposed by Morton et al. (1956) for their laboratory convective plumes does not apply to atmospheric convection in any literal sense. Without such a theoretical basis, it may be rather easier to treat them purely as tuning parameters than anything physically based.

An approach that may be more constructive, however, can also be pursued under a variant of the conventional mass–flux framework. Note first that the convective profiles can be evaluated knowing only the entrainment rate, without knowing the detrainment rate, as seen by Eq. (2.5: cf., Sec. 2.a, Piriou et al. 2007). Second, recall that the mass flux, \( M \), consists of two parts: convective vertical velocity, \( w_c \), and the fractional area, \( \sigma \), for convection,

\[
M = \rho \sigma w_c
\]

Thus, if we could compute these two quantities separately, there would no longer be a need for knowing entrainment and detrainment rates for use in computing the mass flux via Eq. (2.1).
The convective vertical velocity is commonly evaluated by taking Eq. (15) of Levine (1959). Clearly this is a historical misquotation, because the formula in concern is derived for a spatially isolated spherical bubble, but not for a steady plume. Nevertheless, this use may be considered a necessary evil in order to evaluate the convective vertical velocity under a mass–flux formulation. The equation is formulated without entrainment and detrainment, at least in an explicit manner. [Levine’s drag coefficient can be equated to the fractional entrainment rate if the same derivation is repeated under a more formal application of the mass–flux formulation (and SCA: cf., book chapter, Yano 2014c).]

The fractional area for convection could, in turn, be evaluated by a fully–prognostic version of Eq. (2.1):

\[
\frac{\partial \sigma}{\partial t} = E - D - \frac{\partial M}{\partial z}
\]

Of course, this equation retains both entrainment and detrainment. However, Gerard and Geleyn (2005) were able to overcome this difficulty by introducing an alternative equation for \( \sigma \) based on the moist–static energy budget (their Eq. 11). In this manner, the mass flux can be evaluated without knowing entrainment and detrainment rates. Note that once the mass flux is known, the entrainment rate may be diagnosed backwards under certain assumptions. (Entrainment would still be required to compute the vertical profile of in-convection variables \( \phi_c \) via Eq. (2.5)).

A similar idea can be pursued with a further generalization of the mass flux framework into NAM-SCA, and effectively viewing the convection parameterization as a numerical issue rather than anything physical. In order to represent subgrid-scale convection, we do not want to have too strong convective vertical velocity (or too weak either). In order to control the degree of convective vertical velocity in a desirable manner (from a numerical point of view, in order to make the computations smooth), we need to adjust the fractional area for convection so that convection becomes neither too strong or too weak. Such an adjustment can be performed with a relatively simple numerical procedure without explicitly invoking an entrainment and detrainment rates. Such a formulation is relatively straightforward within NAM–SCA, as will be discussed in Q1.8 below.

**Q1.7: How strong and how robust is the observational evidence for self-organized criticality of atmospheric convection?**

Empirical studies across a broad range of observational scales have been attempted to characterize aspects of convective phenomena in order to constrain convective parametrizations, especially the closure. Critical properties are identified empirically, which may connect the convection parametrization problem with statistical physics theories of critical phenomena (cf., Stanley 1971). A broad range of atmospheric phenomena present scale-free distributions. Particularly, many atmospheric phenomena related to precipitation are associated with many characteristic temporal and spatial scales and present long-range correlations, which may result from the coupling between nonlinear mechanisms at different scales (Vattay and Harnos 1994).

Peters et al. (2002) analyzed high-temporal-resolution precipitation data and defined “episodic” precipitation events in a similar manner to avalanches in cellular-automaton models. It was found that a distribution of the precipitation event sizes (integrated rain rate over duration of the event) follows a power law over several orders of magnitude. A power-law distribution suggests criticality, but it is not
a sufficient condition because trivial non–critical mechanisms can also lead to power laws (Newman 2005).

Peters and Neelin (2006) provided further evidence using TRMM satellite data over tropical oceans. A relationship between the satellite–estimated precipitation and the column–integrated water vapor is compatible with a continuous phase transition, in which large areas enter a convectively active phase above a critical value of column–integrated water vapor. Furthermore, they showed that precipitation events tend to be concentrated around the critical point. The precipitation variance was also found the largest around this point. These results can be interpreted in terms of a departure from quasi–equilibrium, and its scale-free behavior is consistent with the self–organized criticality (SOC). Furthermore, Peters et al. (2010) verified another expectation from the SOC framework, i.e., a similarity of power–law exponents independent of the locations by using high temporal–resolution precipitation data. Data from the tropics was also found to exhibit an approximate power–law decay in auto–lag correlation (Neelin et al. 2008). A size distribution of mesoscale convective clusters also follows a power law (Peters et al. 2009, Wood and Field 2011). These results suggest criticality of the atmospheric convective system, although alternative explanations for the observed behaviors are also possible. For example, a theory based on a stability threshold for boundary-layer water vapor is able to reproduce some aspects of the observed characteristics (Muller et al. 2009).

Peters and Neelin’s (2006) results appear to be robust, except for it is not clear how the relationship actually looks like above the critical point. The retrieved rain rates may be underestimated by TRMM microwave (TMI) due to a wrong retrieval method (Steve Krueger, personal communication, Seo et al. 2007). Further analysis is needed in order to confirm that the average precipitation is bounded for high water vapor values.

Yano et al. (2012) suggest, by analyzing an idealized planetary–scale convection simulation, that the shape of the relationship for upper values for precipitation would depend on a dependent variable chosen. For the column–integrated total water, as well as for condensed water, the results showed a similar tendency as in Peters and Neelin (2006) but not for column–integrated water vapor. These two different tendencies were interpred as indicative of two different underlying mechanisms: SOC and homeostasis.

Here, homeostasis is understood as a behaviour of a system that keeps internal conditions rather stable in spite of external excitations. The system places a long delay before responding to an external excitation. It is almost indifferent to the excitation until a certain threshold is reached. Beyond that, however, the system responds with a high amplitude. A fast increase in the amplitude of reaction, just above a threshold can be considered as type of SOC. Under SOC, on the other hand, every sub–system of a given system has a threshold–dependent dynamics. Energy is accumulated (like grains on a column in the sand pile) and when a threshold is exceeded, a fast reaction (e.g., few grains are expelled) is induced in such manner that the sub–system returns to an equilibrium state, i.e., a state under the threshold. There is a propagation of the effects to the nearest neighbours, which further associate sub–systems spatially connected together. This spatially extended events are called “avalanches”. These avalanches extend over many different space scales, involving various sets of sub–systems. Such extensive involvements of subsystems lead to an allusion to “criticality”. This is a major difference from the homeostasis, which only involves a single–scale system and is based on dynamical equations, sufficiently nonlinear
to support threshold-type evolution. In invoking homeostasis to the physical picture of atmospheric convection, one must to make sure that there is a kind of isolation of a sub-system so that it does not react to an external drive. Yano et al. (2012) interpret Reymond’s (2000) thermodynamic self-regulation theory as a type of homeostasis (see also Sec. 6.3, Yano and Plant 2012b for a review).

In practice, both SOC and criticality are the mathematical concepts that must be applied with great care to the real systems. Particularly, we should always keep in mind that theories are built upon for systems with infinite sizes, whereas the real systems have only finite sizes. For example, even for well-established cellular–automata SOC models, the relationship between tuning and order parameters can be substantially different from a standard picture discussed so far, as also found in second–order phase transitions in some cases (Peters and Pruessner, 2009).

As a whole, the evidence for atmospheric convective SOC still needs to be further investigated. This is challenging problem due to a lack of data for high water–vapor values. Future analysis, thanks to the advent of a new generation of satellite observation, such as the Global Precipitation Measurement (GPM) mission, may shed light on this issue. Along with the continuous observational investigations, large–domain LES/CRM simulations are also much encouraged.

**Q1.8: Can a general unified formulation of convection parameterization be constructed on the basis of mass fluxes?**

The mass-flux formulation can be considered to be built upon a geometrical constraint called segmentally constant approximation (SCA). This idea is first proposed by Yano et al. (2005), and further extended by Yano et al. (2010), Yano (2012a, 2014a, b).

Here, SCA is considered a basis for constructing a standard mass–flux formulation. For example, an application of SCA to a nonhydrostatic anelastic model is called NAM–SCA (Yano et al. 2010). A system purely constrained by SCA is general in the sense that any subgrid–scale processes that can be well represented under SCA would fit into this framework: such structures would include convective–and mesoscale updrafts and downdrafts, stratiform clouds, as well as various organized structures in the boundary layer such as cold pools.

A standard mass–flux parameterization can be derived from this prototype SCA model by adding three additional constraints:

i) entrainment–detrainment hypothesis (cf., Sec. 2.1, T1.5, Q1.5.1, T1.6, Q1.6.1, Q1.6.2)

ii) environment hypothesis: the hypothesis that all of the subgrid components (convection) are exclusively surrounded by a special component called the “environment”

iii) asymptotic limit of vanishing fractional areas for convection, such that the “environment” occupies almost the whole grid–box domain.

The formulation structure of the mass–flux parameterization is carefully discussed in Yano (2014a, b).

It may be important to emphasize that all these three constraints can be introduced without specifying whether subgrid–scale processes are convective or not, at least at a very formal level. The only real question is the degree to which a given subgrid–scale process can be described under these constraints. This also measures a degree of generality of mass–flux formulation.
At the same time, it is also emphasized that we can generalize the standard mass–flux formulation by removing some of the above constraints. In this manner, we can develop a general subgrid–scale parameterization by starting from the mass–flux formulation and then relaxing it in well-defined ways by removing or generalizing each of the standard constraints. From these perspectives, SCA provides a general framework for developing subgrid–scale parameterizations (cf., Yano 2014a, b).


The key goal of this part of the Action activities is to identify useful new theoretical/mathematical ideas for convection parameterization development and studies. Specifically, we consider the approaches based on: Hamiltonian dynamics, similarity theories, probability density, and statistical mechanics.

Q2.0: Does the Hamiltonian framework help to develop a general theory for statistical cumulus dynamics?

The investigations of Hamiltonian dynamics and Lie algebra are major theoretical developments under the present COST Action (Bihlo 2011, Cardoso-Bihlo and Popovych, 2012, Popovych and Bihlo 2012, Bihlo and Bluman 2013, Bihlo et al. 2014). In general, symmetries of differential equations are fundamental constraints on how physically self-consistent parameterizations must be constructed for a given system. Lie group analysis of systems of differential equations provides a very general framework for examining such geometrical properties of a system by means of studying its behavior under various symmetry transformations. The Hamiltonian framework furthermore simplifies these procedures. For the sake of structural consistency, the identified symmetries must also be preserved even when a parameterization is introduced to a system. This methodology can also be systematically applied to the mass-flux convection parameterization formulations so that fundamental theoretical constraints on the closure are obtained. This is considered an important future direction. See Q2.3.5 for further.

T2.1: Review of similarity theories

Similarity theories, mostly developed in studies of turbulent flows, consist of two major steps: 1) perform a dimensional analysis so that a given system is nondimensionalized with a set of nondimensional parameters that characterize the behavior of the system; and then, 2) write down a nondimensional similarity solution that characterizes the system. In atmospheric science, this method is extensively exploited in boundary–layer studies generally for turbulent statistics, but more specifically for defining a vertical profile of vertical eddy fluxes. The latter are defined by a nondimensionalized profile function under the similarity theory. A book chapter is devoted to a review of this approach (Grant 2014).

A particularly fascinating aspect of similarity theory is that, in principle, it contains the mass-flux formulation as a special case. Under this perspective, the mass flux formulation results from studying the Reynolds flux budgets. The similarity theory perspective furthermore suggests that approaches for convection parameterization are far from unique. In this very respect, this theory must be further pursued as an over-encompassing framework for all the subgrid-scale processes. Clearly this approach is currently under-investigated.
Note that the similarity theory is a particular choice for pursuing the first pathway identified under the present Action (cf., Sec. 1.3, Yano et al. 2013a) by basing the convection parameterization development upon turbulence studies. Furthermore, the similarity theories may be considered a special case of nondimensional asymptotic expansion approaches. The latter perspective allows us to generalize similarity theories, which are primarily developed for steady states, to time–dependent problems. It can also be generalized from a point of view of moment expansions, which relate to Reynolds budgets.

**Q2.1.1: What are the key non-dimensional parameters that characterize the microphysical processes?**

In fluid mechanics as well as in geophysical fluid dynamics, it is a standard procedure to nondimensionalize a system before making any investigations. The principal nondimensional parameters of a system are identified, and that in turn defines a phase space to explore in order to understand the behavior of the system. This approach is still far from a standard procedure for microphysical investigations.

Possibilities for exploiting a dimensional analysis in a microphysical system in order to identify scaling relations are pursued by Seifert and Stevens (2010), and Seifert and Zängel (2010) for an idealized one–dimensional vertical model and an idealized orographical precipitation system, respectively. Here, their focus is in identifying the characteristic time scales of a given system. Stevens and Seifert (2010) suggest how such characterizations may help to understand microphysical sensitivities in large–eddy simulations.

Yano and Phillips (2011) provide a specific example for how a microphysical system can be nondimensionalized under an idealized zero-dimensional system, considering ice multiplication processes under ice-ice collisions. As it turns out, in this case, the whole behavior of the system is characterized by a single nondimensional parameter. The value of this nondimensional parameter can be estimated observationally, and thus the constructed phase diagram enables us to judge whether a given observed regime is under an explosive ice multiplication phase (a particular possibility identified in this study) or not.

So far, only preliminary investigations have been performed. A full–scale investigation of the microphysical system under a systematic nondimensionalization is a promising direction, but one that is still to be taken.

**Q2.1.2: How can the correlation be determined between the microphysical (e.g., precipitation rate) and dynamical variables (e.g., plume vertical velocity)?**

In the four years of the present Action, we have declined to pursue this possibility. A correlation analysis is well known to be susceptible of producing misleading conclusions and it appears to us that it is difficult to construct a clean correlation analysis of the issue that would identify a useful physical causality.

The most formal and robust manner for coupling between microphysical processes and convective dynamics within a parameterization context is to define the convective vertical velocity consistently. The key issue from a microphysical point of view is that it is imperative to specify a vertical velocity distribution for a sub-convective scale in order to describe the microphysical processes properly (in a satisfying manner, as done within a CRM: cf., Donner 1993). On the other hand, a standard bulk convection parameterization can only provide a single convective vertical velocity. The argument
can easily move ahead to propose a crucial need for adopting a spectral description of parameterized convection.

However, this argument is likely to be rather short-circuited. First of all, a spectrum of convective types does not provide a distribution for the sub-convective scales as required for proper microphysical descriptions. Second, the microphysics expects a time-evolving convective dynamics, whereas a standard mass-flux formulation only provides a steady solution by assuming a steady plume. Technically, a prognostic description of convection under the mass flux formulation is straightforward under its SCA extension (cf., Q1.8).

Lastly, and most importantly, what microphysicists would like to implement in a convection parameterization is rather an explicit microphysics, although it may well be phenomenologically developed (cf., Sec. 1.1). Efforts are clearly required to develop microphysical descriptions to a parameterized level so that, possibly, fine details of the convective dynamics may be no longer necessary (cf., Q4.3.1).

The last point further leads us to a more general question: to what extent are microphysical details required for a given situation and a given purpose? Here, the microphysicists tend to emphasize strong local sensitivities to microphysical choices. On the other hand, the dynamicists tend to emphasize a final mean output. Such inclinations can point towards opposite conclusions for obvious reasons, and doubtless we need to find an appropriate intermediate position (cf., Q3.4.4).

**Q2.1.3: How should a fully consistent energy budget be formulated in the presence of precipitation processes?**

As we already emphasized in Sec. 1.3, more intensive investigations of the parameterization problem form the turbulence point of view are required. However, Q2.1.3 is typical of the issues that must be addressed when this pathway is pursued.

Purely from a point of view of mechanics, this is rather a trivial question: one performs a formal energy integral for the vertical momentum equation. Although it is limited to a linear case, the clearest elucidation of this method is offered by Chandrasekhar (1961). A precipitation effect would simply be found as a water-loading effect in the buoyancy term. This contribution would be consistently carried over to a final energy-integral result. Furthermore, the water-loading effect can be reintegrated to the “classical buoyancy terms” under a consistent formulation (Marquet and Geleyn 2013, 2014).

The real issue arises when this energy budget is re-written in the context of the moment expansion framework, on which similarity theory (cf., T2.1) is based. The moment-based subgrid-scale description has been extensively developed in turbulence studies, with extensive applications in the dry turbulent boundary layer. This theoretical framework works well when the whole process is conservative. Constructing such a strictly conservative theory becomes difficult for the moist atmosphere, due to the existence of differential water flux (Durran and Klemp 1982, Marquet and Geleyn 2013, 2014), and once a precipitation process starts, the whole framework, unfortunately, breaks down even under standard approximations.

On the other hand, invocation of the Liouville principle (cf., T2.2 and the following questions) provides a more straightforward description for the evolution of the water distribution under precipitation processes so long as the processes are described purely in terms of a single macrophysical point. Note
that the precipitation process itself would be more conveniently treated under a traditional moment-based
description as a part of the eddy transport.

Hence, the turbulent–kinetic energy evolution under precipitation is best described under a time
splitting approach: compute the traditional turbulent process (including water fall out) using a
moment-based approach, and then update the microphysical tendencies based on the Liouville principle.

T2.2: Review of probability-density based approaches

See a book chapter (Machulskaya 2014), which reviews probability–density based cloud schemes.
Clouds are highly inhomogeneous for a wide range of scales, and most of them are not well resolved in
numerical models. Thus, a need arises for describing subgrid–scale cloud distributions. In the following,
we are also going to focus on the issues of cloud schemes.

Q2.2.1: How can current probability-density based approaches be generalized?

The best general approach would be to take that of time splitting between the physics part and the
transport part. The physics part (i.e., single-point processes) is handled by the Liouville equation, as
further emphasized in Q2.2.5. On the other hand, the transport part (eddy transport) is handled based on
the moment-based description, invoking an assumed pdf approach (cf., Q2.2.3).

Q2.2.2: How can convective processes be incorporated into probability-based cloud parameteriza-
tions? Can suitable extensions of the approach be made consistently?

This question can be interpreted in two different ways: 1) a possibility of treating convection (or
more precisely deep convective towers) as a part of a probability–based cloud scheme; or, 2) incorporate
the effect of convection (especially deep convection) or interaction with deep convection as a part of
a probability–based cloud scheme. In the latter case, convection is treated by a different scheme, say,
based on mass flux, and it is not counted as a part of the cloud considered by the given probability–based
scheme. In other words, the cloud scheme only deals with the so–called stratiform clouds.

In pursuing the first possibility, the tail of the probability distribution becomes important, because
deep convective towers tend to produce high water mixing ratios. In order to well account for the tail
part of a distribution, higher–order moments must be included in a formulation. The inclusion of a
skewness would be a minimum in order to take this step, and has been followed by e.g., Bony and

In particular, Bony and Emanuel (2001) claim that the shape of the probability distribution is altered
so that at every time step, the in–cloud value of cloud water equals the sum of those diagnosed by a
traditional large–scale saturation and a convection parameterization. However, a careful examination of
their formulation suggests that this statement is rather an understanding than anything actually derived
as a formulation (cf., T2.4). In general, it is not obvious how to describe convective evolution in terms of
higher–order moments (e.g., skewness) regardless of whether the issues are handled in a self–contained
manner or under a coupling with an independent convection scheme. The difficulty stems from a simple
fact that a spatially–localized high water concentration associated with deep convective towers is not
easily translated into a quantative value of skewness.

The second possibility is, in principle, more straightforward: the cloudy air detrained from convection
is counted as an additional source term in a cloud scheme budget. This additional source term would be
relatively easily added under a formulation based on the Liouville equation: the convective source enters
as a flux term that shifts the water distribution from lower to higher values by extending the tail of the distribution. UM PC2 (Wilson et al. 2008) takes into account the feeding of clouds from convection to stratiform, at least conceptually, in this manner, but without explicitly invoking the Liouville principle.

Alternatively, Klein et al. (2005) try to deal with this problem by considering moments (variances) associated with deep convection. However, this alternative approach is not quite practical for the mass–flux based convection parameterization, which does not deal with these variants by default. The convection parameterization would have to be further be elaborated for this purpose. The above Liouville–based approach, on the other hand, can handle the problem without explicitly invoking higher moments for convection.

Q2.2.3: Is the moment expansion a good approximation for determining the time-evolution of the probability density? What is the limit of this approach?

This question specifically refers to an idea of assumed pdf originally developed by Golaz et al. (2002). Although this approach is attractive with a possibility of truncating the moments by post–analysis by CRM or LES for a given case, it appears hard to generalize it easily without further testing. Unfortunately, we fail to identify any suitable mathematical theorem for measuring the convergence of the pdf under a moment expansion.

Q2.2.4: Could the Fokker-Planck equation provide a useful general framework?

The Fokker–Planck equation is a generalization of the Liouville equation that is appropriate for certain stochastic systems. In other words, the Fokker–Planck equation reduces to the Liouville equation when the system is deterministic.

In the context of cloud parameterizations, we should note that the concept of “pdf” (probability density function) is slightly misleading, but it is better called “ddf” (distribution density function), because here we are dealing with a distribution of a variable (e.g., total–water mixing ratio) over a grid box rather than any probability (e.g., chance to find a condensed water at a given point). Current approaches for cloud schemes are, in principle, deterministic, although stochasticity may sometimes be added (with a possible confusion arising from the subtle distinction between pdf and ddf).

For issues of stochasticity itself, see: Q1.3.2, T3.3.

Q2.2.5: How can microphysics be included properly into the probability-density description?

The Liouville equation is the answer, because it describes any single physical–point processes well, as already discussed in Q2.2.1. Here, the assumed pdf approach becomes rather awkward, because it is hard to include microphysical processes (a process conditioned by a physical–space point) into moment equations.

T2.3: Assessments of possibilities for statistical cumulus dynamics

It is often argued that subgrid-scale parameterization is fundamentally “statistical” in nature (cf., Kuo 1974). However, little is known of the statistical dynamics for atmospheric convective ensembles. This is a domain that is clearly under–investigated. If we really wish to establish convection parameterization under a solid basis, this is definitely where much further work is needed. The present Action has initiated some preliminary investigations. Especially, we have identified renormalization group theory (RNG) as
a potentially solid starting point (Yano et al. 2012a). We strongly emphasize the importance of more extensive efforts towards this direction.

**Q2.3.1: How can a standard, “non-interacting”, statistical description of plumes be generalized to account for plume interactions?**

When this question was originally formulated, we did not fully appreciate the fact that the conventional spectrum mass-flux formulation does consider the interactions between convective plumes, albeit in an indirect sense, through the environment. Work with the Action has begun the analysis of the interactions between convection types within this framework, with a view to providing an assessment of whether or not such interactions have important implications and consequences for convection parameterization performance.

Perhaps the best example for making this point is the transformation from shallow to deep convection as elucidated for the two–mode mass–flux formulation by Yano and Plant (2012a), Plant and Yano (2013). Without mutual interactions, shallow convection is a self-destabilizing process associated with its tendency for moistening and cooling, whereas deep convection is a self-stabilizing process associated with its tendency for drying and warming. A proper coupling between these two types of convection is a key for properly simulating the transformation process. As already emphasized in Q1.2.1, this is a formulation that can be relatively easily implemented into operational models as well.

Considering more direct interactions between convective elements is straightforward under the SCA framework (cf., Q1.8). A key missing step is to develop a proper statistical theory under this framework.

**Q2.3.2: How can plume-plume interactions and their role in convection organization be determined?**

Several steps must still be taken in order to fully investigate this issue under a framework of statistical mechanics. First is an extensive elementary study under the SCA framework. Second is a need for developing a proper Hamiltonian framework for the SCA system so that this system can be more easily cast into a framework suitable for statistical mechanics analyses under a Hamiltonian formulation (cf., Q2.3.5).

**Q2.3.3: How can the transient, life-cycle behavior of plumes be taken into account for the statistical plume dynamics?**

Extensive statistics can be developed by examining both CRM and LES outputs of convection simulations (Plant, 2009; M. Sakradzija et al., manuscript in preparation). The next question is how
to develop a self-contained self-consistent statistical theory based on these numerically accumulated statistics. However, see T1.1, Q1.2.1 for reservations for advancing towards this direction.

**Q2.3.4: How can a statistical description be formulated for the two-way feedbacks between convective elements and their “large-scale” environment?**

The convective energy-cycle description already discussed in Q1.2.1 would be the best candidate for this goal. Technically, it is straightforward to couple this convective energy-cycle system with simple models for large-scale tropical dynamics. This is an important next step to take.

**Q2.3.5: How can statistical plume dynamics best be described within a Hamiltonian framework?**

As is well known in statistical physics, once a Hamiltonian of a given system is known, various statistical quantities associated with this system can be evaluated in a straightforward manner through a partition function. There is no technical difficulty for developing such a Hamiltonian formulation (so long as we take nondissipative limit to a system) for an atmospheric convective system. Much extensive investments and funding are clearly required towards this goal.

**T2.4: Proposal for a consistent subgrid-scale convection formulation**

In common scientific discourses, consistency of a given theoretical formulation presents two major distinct meanings:

1) Self-consistency

2) Consistency with physics

The first definition refers to the self-consistency of the logic when a formulation is developed in deductive systematic manner. We suggest to take the first definition for consistency for parameterization in order to avoid possible confusions discussed below.

An example of inconsistency in logic is, for example, found in Wagner and Graf (2010), as pointed out by Plant and Yano (2012): it assumes both the cloud work function, $A$, and the mass flux, $M$, change in time with the same rate in the order

$$\frac{1}{A} \frac{\partial A}{\partial t} \sim \frac{1}{M} \frac{\partial M}{\partial t} \tag{3.1}$$

at a one point, and then the rate of change of the cloud work function is much slower than that of the mass flux i.e.,

$$\left| \frac{1}{A} \frac{\partial A}{\partial t} \right| \ll \left| \frac{1}{M} \frac{\partial M}{\partial t} \right| \tag{3.2}$$

at another point under a single derivation process. The two conditions, Eqs. (3.1) and (3.2), are clearly contradicting each other. Thus the derivation is clearly not consistent. However, here and elsewhere, a value of a heuristic derivation should not be disputed. A consequence of a logical inconsistency is often hard to measure, and a practical benefit wins over.

On the other hand, some people take the word differently. In this second definition, the question is posed whether a given formulation is consistent with a given physics, or known physics. For example, the conventional mass–flux formulation for convection parameterization can be regarded as inconsistent because it does not take into account the role of gravity waves in convective dynamics. By the same token, quasi-geostrophic dynamics can also be regarded as inconsistent because it also does not take into
account the contributions of gravity waves to the dynamics. It is debatable whether the first example is problematic, but in the second example, quasi–geostrophic dynamics is widely accepted despite this point.

In the second definition, we have to carefully define the relevant physics. Clearly, all the physical descriptions in atmospheric models do not take account of quantum effects, which are considered negligible for all the modelled processes. This type of inconsistency is not an issue. The role of gravity waves is more subtle, although it is still likely that in many situations they can be neglected. From this point of view, this type of consistency is better re-interpreted in terms of the accuracy of an approximation adopted.

From a practical point of view, consistency of the thermodynamic treatment warrants special attention. Traditionally, atmospheric thermodynamics are often considered under various arbitrary approximations, and it is even difficult to examine the self–consistency in retrospect. One of our major achievements is to show how atmospheric thermodynamics can be constructed in a self–consistent manner (Marquet 2011, 2014a,b, Marquet and Geleyn 2013, 2014: see Q3.4.1 for more).

The relationships between the cloud and the convective schemes, already discussed in Q2.2.2 provide a good example for further considering the issues of self–consistency of parameterization.

Two approaches were discussed in Q2.2.2. The first is to establish mutual consistency between the convective and the cloud schemes. “Consistency” here means a logical consistency by writing the same physical processes in two different ways within two different parameterizations. More precisely, in this case, an “equivalence” of the logic must be established. A classic example of such an equivalence of logic is found in quantum mechanics between the matrix–based formulation of Heisenberg and the wave–equation based formulation of Schrödinger. The equivalence of the formulations may be established by a mathematical transformation between the two. Such a robust equivalence is hardly established in parameterization literature.

The second approach is to carefully divide clouds into convective and non–convective parts, and let the convection and the cloud parameterizations deal each part separately. In this second case, an issue of double counting must be avoided. Here, a notion of dichotomy between convection and environment introduced by standard convection parameterizations becomes important. In order to avoid any double counting, the cloud scheme should deal with the clouds only in the environmental part and not in convective part. This is the basic principle of retaining mutual consistency of two physical processes: separate them into different subdomains over a grid box. The concept of SCA helps to handle this issue in lucid manner (cf., Q1.8).

A corollary to this discussion is that, regardless of the decisions made about how consistency is to be achieved in a model, it is essential that the decision be made clearly, cleanly and openly. The developers of the individual parameterizations must all be agreed on the strategy. Moreover, model users should be aware that parameterization schemes are not necessarily interchangeable: a particular convection
paramaterization should not be expected to function well if coupled to cloud or boundary layer schemes that do not share its assumptions about which scheme is treating which processes. The literature suggests that such awareness is not always as strong as we would wish.

4. High-Resolution Limit

As resolution increases both for weather–forecast and climate models, a number of new aspects must be addressed, especially the adequacy of the present convection parameterizations. Convection parameterization is traditionally constructed by assuming a smallness of the convective scales compared to a resolved scale (i.e., scale separation principle). A parameterization scheme must somehow be adjusted based on the model resolution (i.e., resolution–dependency). These are the issues to be addressed in the present section.

T3.1: Review of state of art of high-resolution model parameterization

See a book chapter (Gerard 2014) for a review, and T.3.3 for further discussions.

T3.2: Analysis based on asymptotic expansion approach

We may consider that traditional parameterizations are constructed under an asymptotic limit of scale separation (the constraint iii) in Q1.8. For a parameter for the asymptotic expansion, we may take the fractional area, $\sigma$, occupied by convection. This is a standard small parameter adopted in mass-flux convection parameterization, which is taken to be asymptotically small.

However, as model resolution increases, this asymptotic limit becomes less valid. In order to address this issue, the exercise proposed here in the MoU was to move to a higher order in the expansion so that a more accurate description may be obtained. As an example, a higher–order correction to a standard mass-flux convection parameterization formulation was attempted. As it turns out, the obtained higher-order correction is nothing other than a particular type of numerical time-stepping scheme that makes the scheme weakly prognostic. This is essentially consistent with the result obtained by more directly removing the asymptotic limit: the mass–flux formulation becomes fully prognostic as a result.

T3.3: Proposal and recommendation for high-resolution model parameterization

So far as the mass-flux based parameterization is concerned, a standard asymptotic limit of vanishing fractional convective area must be removed when a model resolution is taken high enough so that a standard scale separation is no longer satisfied. Thus, the formal answer to this issue is to make the mass-flux parameterization fully prognostic, also by taking out a standard “steady plume” hypothesis, as remarked in T3.2.

However, as far as we are aware, this fully drastic measure is not yet taken at any operational research centers so far. Several different approaches are under consideration.

The first approach is to stick to the standard mass–flux convection parameterization formulation based on an asymptotic limit of $\sigma \to 0$. This strategy, currently adopted at ECMWF (P. Bechtold, personal communication), may be justified at the most fundamental level, by the fact that a good asymptotic expansion often works extremely well even when an expansion parameter is re-set to unity. Such a behavior can also be well anticipated for mass-flux convection parameterization. At the practical level, what the model can actually resolve (i.e., the effective resolution) is typically more than few times larger.
than a formal model resolution, as defined by a grid-box size, due to the fact that a spatial gradient must be evaluated numerically by taking over several grid points.

Under this approach, a key missing element is lateral communication of convective variability between the grid boxes. As a partial effort for compensation of this defect, the convection parameterization has been coupled with a stochastic cellular automaton scheme. The latter mimics lateral interactions associated with convective processes in a very crude, but helpful manner (Bengtsson et al. 2011, 2013).

The second approach is to move towards a more prognostic framework in an incremental manner under a framework of traditional parameterization. This effort is called 3MT (Module Multiscale Microphysics and Transport: Gerard and Geleyn 2005, Gerard 2007, Gerard et al. 2009). Although it may be considered somehow “backwards” in a sense as going to be criticized in T4.3 below, careful efforts are made in these studies to avoid any double counting in the interactions between the otherwise–competing computations of thermodynamic adjustment and convective latent heat release, as well as latent heat storage for downdrafts.

The third approach is to add a stochastic aspect to a standard scheme based on the quasi-equilibrium hypothesis in order to represent finite departures from quasi-equilibrium that can be expected in the high resolution limit (Plant and Craig 2008). An important technical detail under this implementation is the need for defining an effective environment larger than the grid–box size, over which the standard quasi-equilibrium assumption can reasonably be applied. The approach can therefore be considered as a downscaling of the convective response, which implies a stochastic formulation (cf., Sec. 5.2).

Efforts ongoing at DWD (TKE-Scalar Variance mixing scheme: TKESV: Machulskaya and Mironov, 2013) identify a key issue in the high-resolution limit as being an improvement of a boundary-layer scheme associated with cloud processes. Here, a major challenge is the inclusion of a proper water cycle in the context of a traditional turbulence parameterization (cf., Q2.1.3). A hybrid approach combining the traditional moment-based approach and a subgrid-scale distribution is adopted for this purpose. When a relatively simple distribution is pre-assumed for a latter, a closed formulation can be developed relatively easily.

In reviewing these different approaches, it may be remarkable to note that the two approaches, 3MP and TKESV, adopt mutually consistent thermodynamic descriptions. On the other hand, these two efforts take contrasting approaches in dealing with the dichotomy between convection and turbulence. 3MT takes into account a gradual shift from convective to turbulent regimes with the latter being delegated in a self-consistent manner to the other parameterization schemes and to the dynamics. On the other hand, TKESV reduces the issues of all of the subgrid–scale motions to that of a turbulence problem (cf., T2.4).

Some further perspectives can be found in e.g., Arakawa et al. (2011), Arakawa and Wu (2013).

**Q3.4: High–Resolution Limit: Questions**

The following questions are listed for the high–resolution limit in MoU.

**Q3.4.1: More general and flexible parameterization at higher resolutions**

The issue for making a parameterization general and flexible is best discussed under a universal setting. Issues at higher resolution would simply be considered a special case of this general problem. We even argue that this is a moral for modelling rather than any specific scientific issue. Thus, our following answer is also presented in such manner.
We propose the three basic dictums:

1) Start from the basic laws of physics (and chemistry: cf., T4.3)

2) Perform a systematic and logically consistent deduction from the above (cf., T2.4)

3) Sometimes it may be necessary to introduce certain approximations and hypotheses. These must be listed carefully so that you would know later where you introduced them and why.

Atmospheric sciences are considered applications of the basic laws of physics (and chemistry). Since the Norwegian school established modern meteorology, it remains the basic principle of our discipline, because otherwise we lose a robustness in our scientific endeavour. Of course, not all the laws of physics are precisely known for atmospheric processes. Cloud microphysics would be the best example that must tackle with numerous unknowns.

However, we must start from robust physics that we can rely upon. Another way to restate the first dictum above is: “never invent an equation”. Thus, any development must start from robust known physics, and the uncertainty of our physical understanding of a given process must properly be accounted for in the development process of the parameterization (cf., T4.3).

A parameterization is, by definition, a parametric representation of the full physics that describes the subgrid scales. Thus, a certain deduction from the full physics is required in order to arrive such a parametric representation. Such a deduction process must be self-consistent and logical: a simple moral dictum. While simply said, in practice a completely self-consistent logical deduction is almost always not possible for many complex problems in parameterization. Certain approximations and hypotheses must inevitably be introduced. At a more practical level, thus those approximations and hypotheses must carefully be listed during the deduction process with careful notes about extent of their validity and limits. In this manner, we would be able to say how much generality and consistency is lost in the deduction process. Here, the main moral lesson is: be honest with these.

A difficulty for pursuing such an honest path is that the literature is so confused that it is often not easy to identify the basic physical principles behind a given parameterization. A very patient literature survey is often required.

Specific examples for developing a subgrid–scale parameterization in a general manner under the above strategy are: mode decomposition (Yano et al. 2005), moment expansion (Mironov 2009), and similarity theory (Grant 2014, cf., T2.1). SCA introduced in Q1.8 may be considered a special application of mode decomposition.

The main wisdom stated above may be rephrased as “start from robust physics that we can trust and never reinvent a wheel”. An unsuspected issue concerns the moist-air entropy, because although the liquid-water and equivalent potential temperatures are commonly used to compute the specific values and the changes in moist-air entropy, this is only valid for the special case of closed systems where total water content \( q_t \) and thus dry-air content \( q_d = 1 - q_t \) are constant for a moving parcel in the absence of sources and sinks.

Marquet (2011) proposes a more general definition of specific moist-air entropy which can be computed directly from the local, basic properties of the fluid and which is valid for the general case of barycentric motions of open fluid parcels, where both \( q_t \) and \( q_d \) vary in space and in time. Computations
are made by applying the third law of thermodynamics, because it is needed to determine absolute values of dry-air and water-vapor entropies independently of each other. The result is that moist-air entropy can be written as \( s = s_{\text{ref}} + c_{pd} \ln(\theta_s) \), where \( s_{\text{ref}} \) and \( c_{pd} \) are two constants. Therefore, \( \theta_s \) is a general measure of moist-air entropy. The important application shown in Marquet (2011, 2014a,b) and Marquet and Geleyn (2013, 2014) is that values and changes in \( \theta_s \) are significantly different from those of \( \theta_l \) and \( \theta_e \) if \( q_t \) and \( q_d \) are not constant. This is especially observed in the upper part of marine stratocumulus and, more generally, at the boundaries of clouds.

Barycentric and open-system considerations show that the moist-air entropy defined in terms of \( \theta_s \) is at the same time: 1) a Lagrangian tracer; and, 2) a state function of an atmospheric parcel. All previous proposals in this direction fulfilled only one of the two above properties. Furthermore, observational evidence for cases of entropy balance in marine stratocumulus shows a strong homogeneity of \( \theta_s \), not only in the vertical, but also horizontally: i.e., between cloudy areas and clear air patches. It is expected that these two properties could also be valid for shallow convection, with asymptotic turbulent and mass-flux-type tendencies being in competition with diabatic heating rates.

Several implications are drawn here. First, the moist-air entropy potential temperature \( \theta \) plays the double role of: i) a natural marker of isentropic processes; and, ii) an indirect buoyancy-marker (unlike in the fully dry case where the dry-air value \( \theta \) directly plays such a role). Indeed, the Brunt-Väisälä frequency can be separated in terms of vertical gradients of moist-air entropy and total water content (Marquet and Geleyn 2013, 2014), almost independently whether condensation/evaporation takes place or not within parcels (simply because moist-air entropy is conserved for adiabatic and closed processes).

The main impact of moisture on moist-air entropy is the water-vapor content, which is already contained in unsaturated regions and outside clouds. The condensed water observed in saturated regions and clouds leads to smaller correction terms. An interesting feature suggested in Marquet and Geleyn (2013) is that the large impact of water vapor does not modify so much the formulation of the Brunt-Väisälä frequency when going from the fully dry-air (no water vapor) to the “moist-air” (cloudy) formulations.

These results indicate that parameterization schemes relying on phenomenological representations of the links between condensation/evaporation and microphysics might not be the only answer to the challenges discussed here.

From the entropy budget point of view (and hence perhaps also for the energy or enthalpy budget) the issue that matters is the presence of precipitation and entrainment/detrainment processes as generators of irreversibility and as witness of the open character of atmospheric parcels’ trajectories. This is especially true for marine stratocumulus where clouds have comparable entropy to unsaturated patches and subsiding dry-air above (Marquet 2011, Marquet and Geleyn 2014).

Hence medium–sophistication moist–turbulent parameterization schemes based on the moist-air entropy and encompassing in a single view the whole of the grid-mesh and having a reasonable and independent closure for the cloud amount ought to be competitive with respect to those particularizing the role of organized plumes (namely eddy-diffusivity mass-flux schemes).
Note that two of the approaches for the high-resolution limit discussed in T3.3 (3MT and TKESV) are perfectly compatible with this new type of thinking.

**Q3.4.2: Which scales of motion should be parameterized and under which circumstances?**

A very naive approach to this problem is to examine how much variability is lost by averaging numerically–generated output data from a very high resolution simulation with a CRM or LES that well resolves the fine-scale processes of possible interest. The analysis is then repeated as a function of the averaging scale. In fact, this exercise could even be performed analytically, if a power–law spectrum is assumed for a given variable.

However, one should realize that whether a process needs to be parameterized or not cannot be simply judged by whether the process is active or above a given spatial scale. The problem is much more involved for several reasons:

i) Any process in question cannot be characterized by a single scale (or wavenumber), but is more likely to consist of a continuous spectrum. In general, a method for extracting a particular process of concern is not trivial.

ii) Whether a process is well resolved or not cannot be simply decided by a given grid size. In order for a spatial scale to be adequately resolved, usually several grid points are required. As a corollary of this, and of point i), the grid size required depends on both the type of process under consideration and the numerics.

iii) Thus the question of whether a process is resolved or not is not a simple dichotomic question.

With these considerations, it would rather be fair to conclude that the question here itself is ill posed. It further suggests the importance of a gradual transition from a fully parameterized to a well–resolved regime (cf., T3.3).

A way for overriding this issue could be to handle the issue of subgrid–scale parameterization like that of an adaptive mesh–refinement. A certain numerical criterion (e.g., local variance) may be posed as a criterion for mesh refinement. A similar criterion may be developed for subgrid–scale parameterizations. A conceptual link between the parameterization problem and numerical mesh–refinement is suggested by Yano et al. (2010). For general possibilities for dealing a parameterization problem as a numerical issue, see Q1.6.2.

**Q3.4.3: How can convection parameterization be made resolution-independent in order to avoid double-counting of energy-containing scales of motion or loss of particular scales?**

There are two key aspects to be kept in mind in answering this question. First is the fact that basic formulations for many subgrid–scale parameterizations, including mass flux as well as an assumed pdf, are given in a resolution independent manner, at least at the outset. This is often a consequence of the assumed scale separation. It is either various a posterori technical assumptions that introduce a scale dependence, or else the fact that the scale separation may hold only in an approximate sense. In the mass–flux formulation, major sources of scale dependence are found both in closure and entrainment–detrainment assumptions. For example, in the closure calculations a common practice is to introduce a scale–dependent relaxation time–scale, and satisfactory results at different model resolutions can only
be obtained by adjusting such a parameter with the grid size. A similar issue is identified in Tompkins’ (2002) cloud scheme, which contains three rather arbitrary relaxation time–scales.

However, once these arbitrary relaxation time–scales are identified in a scheme, a procedure for adjusting them may be rather straightforward. From a dimensional analysis, and also particularly invoking a Taylor’s frozen turbulence hypothesis, such time–scales can often be expected to be proportional to model resolution. Based on this reasoning, the adjustment time–scale in the ECMWF convection parameterization closure is, indeed, set to proportional to the model resolution (Bechtold et al. 2008). This argument can, in principle, be applied to any parameterization parameters: we can estimate the scale dependence of a given parameter based on a dimensional analysis. Importantly, we do not require any more sophisticated physical analysis here. The proportionality factor can be considered as a rather straightforward “tuning”. A classical example is Smagorinksy’s (1963) eddy diffusion coefficient, which is designed to be proportional to the square of the model grid length. The pre-factor here is considered as “tuning” but the functional form is known beforehand. This is another example when we do not require extensive process studies (cf., Sec. 1.4): almost everything can be defined within a parameterization in a stand–alone manner under a good and careful theoretical construction.

A simple application of this idea for using dimensional analysis and scaling leads to a simple condition for turning off convection parameterization with increasing resolution. Convection could be characterized by a turn–over time scale and the criterion would be to turn off the convection parameterization when the turn–over time scale is longer than the minimum resolved time–scale. The latter would be estimated as a factor of few of the model time step with the exact factor depending on the model numerics. Since the former is proportional to the convection height, this condition would turn off parameterized convection first for the deepest clouds and gradually for shallower ones also as the model resolution increases. Here, again, we caution against a common custom of turning off a convection parameterization completely at a somewhat arbitrary model resolution, as already suggested in T3.3, Q3.4.2.

Clearly the best strategy in parameterization development would be to avoid an introduction of a scale–dependent parameter as much as possible.

The second aspect to realize is that a scale gap is not a prerequisite for parameterizations. A separation between above–grid and subgrid scales is made rather in arbitrary manner (cf., Q3.4.2). In this respect, the best strategy for avoiding a double counting is to keep a consistency of a given parameterization with an original full system. For example, in order to avoid a double counting of energy–containing scales, a parameterization should contain a consistent energy cycle.

It is often anticipated that as a whole, resolved and parameterized convection are “communicating vessels” in a model. Thus, when parameterized convection is strong there is less intense and/or less likelihood of producing resolved convection and vice versa. Due to this tendency, the issue of double counting would not come out as a serious one most of time in operational experiences. However, this is true only if a model is well designed, and in fact, many models suffer from problems because they are not able to perform such a smooth transition between the “communicating vessels”. This emphasizes the need for carefully constructing scale–independent physical schemes based on the principles outlined here. It, furthermore, reminds us the importance of constructing all physical schemes with due regard
to generality (cf., Q3.4.1) and in a self-consistent manner (cf., T2.4), as already emphasized, in order to avoid these operational difficulties.

**Q3.4.4: What is the degree of complexity of physics required at a given horizontal resolution?**

Currently, various model sensitivities are discussed in a somewhat arbitrary manner: cloud physicists tend to focus on smaller scales in order to emphasize the sensitivities of model behavior to microphysical details, whereas dynamicists tend to focus on larger scales in order to emphasize the dynamical control of a given system. The ultimate question of sensitivities depends on the time and the space scales at which the model is intended to provide useful results. Such considerations of scale dependence must clearly be included in any sensitivity studies.

From a point of view of probability theory (Jaynes 2003, Gregory 2005), this issue would be considered that of Occam’s principle: if two physical schemes with different complexities provide us an equally good result (under a certain error measure), we should take a simpler one among the two (cf., link to T4.2).

Issues of physics complexity must also be considered in terms of the capacity of a given model for performing over a range of model resolutions. Some schemes may represent a well–behaved homogeneous behaviour over a wide range of resolutions, avoiding brutal changes of forecast model structure and avoiding any parasitic manifestations, such as grid-point storms at the scales where convection must still be parameterized although it may partially be resolved. Such schemes would likely be able to be extended with additional physical complexity relatively easily in comparison with schemes that behave less well over the same variation of resolution.

Uncertainty growth can be well measured under a general tangent linear formulation which provides an exponential deviation rate from the original full solution with time as well as a preferred direction for the deviation (i.e., a spatial pattern growing with time). This method also allows us to systematically examine feedback of a physical process to all the others. Note that the question here includes a sensitivity to the other physical processes (parameterized or not) by changing a one. In principle, the uncertainty growth estimated by a tangent linear method can be translated into a probability description by writing down the corresponding Liouville equation for a given tangent linear system. In these sensitivity–uncertainty analyses, uncertainties associated with physical parameters as well as those associated with an initial condition, observational uncertainties can be quantified.

5. Physics and Observations

This section examines various physical processes (notably cloud microphysics) important for convection as well as issues of observations.

**T4.1: Review of subgrid-scale microphysical parameterizations**

This assignment can be interpreted in two different ways: i) review of microphysical parameterizations themselves (i.e., phenomenological description of the microphysics); and, ii) review of cloud microphysical treatments in convection parameterization. A review on the bin and bulk microphysical formulations has been developed (Khain et al. 2014a) in response to the first issue. The second issue is
A special direction of investigation and interest is the effect of aerosols on the intensity of tropical cyclones. Khain et al. (2014b) and Lynn et al. (2014) show that a model with bin-microphysics is able to predict the intensity of TC much better than current bulk-parameterization schemes. Furthermore, continental aerosols involved in the TC circulation during landfall decrease the intensity of TC to the same extent as the sea surface temperature cooling caused by the TC–ocean interaction. These studies indicate the existence of an important hail–related mechanism that affects TC intensity that should be taken into account to improve the skill of the TC forecasts.

The present Action has also developed an innovative new theory for time-dependent freezing (Phillips et al. 2014a, b). This work highlights another key advantage of bin microphysics schemes: representing particle properties that have strong size-dependence. Wet growth of hail happens only when a critical size is exceeded, and particles that become wet carry their liquid during size-dependent sedimentation. A new theory for such freezing is developed for bin-microphysical schemes. The theory encompasses wet growth of hail, graupel and freezing drops.

The new algorithm has been implemented into the Hebrew University Cloud Model (HUCM) and mid-latitude hail storms have been simulated under different aerosol conditions. It is shown that a large hail width diameter of several centimeters forms only in the case of high (continental) aerosol loading (Ilotovich et al. 2014). It is also shown that hail increases precipitation efficiency leading to an increase in surface precipitation with an increase in the aerosol concentration.

For the first time all the parameters measured by Doppler polarimetric radar have been evaluated. These parameters have been calculated according to their definitions using size distribution functions of different hydrometeors in HUCM. A long-standing problem of the formation of so-called columns of differential reflectivity Zdr is solved by Kumjian et al. (2014) as a result. High correlations are found between Zdr on the one hand, and hail mass and size on the other (Khain et al. 2014c). This finding opens a way to improve the short-range forecast of hail, its size, and the intensity of hailshafts.

5.1. Further Processes to be incorporated into Convection Parameterizations

In addition to the cloud microphysics, the following processes may be considered to be important for convective dynamics as well as convection parameterization. However, again, we emphasize here the difference between the two issues (cf., Secs. 1.3, 1.4): importance in convective dynamics and that in convection parameterization. The following discussions are also developed under an emphasis of this distinction.

5.1.1. Downdrafts

Downdrafts have long been identified as a key process in convective dynamics (Zipser 1969, 1977, Houze and Betts 1981). From a theoretical point of view, the importance of downdrafts has been addressed in the context of tropical–cyclone formation (Emanuel 1989) as well as that of Madden–Julian oscillations (Yano and Emanuel 1991). Although the majority of current operational mass–flux convection parameterizations do include convective downdrafts in one way or another (e.g., Fritsch and
Chappell 1980, Tiedtke 1989, Zhang and McFarlane 1995, Bechtold et al. 2001), they are implemented rather in an *ad hoc* manner. The downdraft formulation must be more carefully constructed from a more general principle, e.g., SCA (cf., Q1.8).

However, the real importance of downdrafts in convection parameterization must also be carefully re-assessed. Recall that the thermodynamic role of convection is to dry and cool the boundary layer by a vertical transport process. The updraft and downdraft essentially perform the same function by transporting thermodynamic anomalies with opposite signs in opposite directions. In the above listed theoretical investigations, it is interesting to note that the downdraft strength is measured in terms of precipitation efficiency. This further suggests that the same effect may be achieved by simply re-distributing the downdraft effect into deep updraft and shallow–convective mixing. Thus, sensitivities of downdrafts on model behavior demonstrated by these studies do not necessarily demonstrate the true importance the downdraft representation in convection parameterization.

5.1.2. Cold pools

The cold pool in the boundary layer is often considered a major triggering mechanism for convection. Observations suggest that cold pool-generated convective cells occur for shallow maritime convection (Warner et al. 1979, Zuidema et al. 2012), maritime deep convection (Barnes and Garstang 1982, Addis et al. 1984, Young et al. 1995) and continental deep convection (e.g., Lima and Wilson 2008, Flamant 2009, Lothon et al. 2011, Dione et al. 2013). Moreover, numerical studies appear to suggest that cold pools promote the organization of clouds into larger structures and thereby aid the transition from shallow to deep convection (Khairoutdinov and Randall 2006, Böing et al. 2012b, Schlemmer and Hohenegger 2014: but see Yano and Plant 2012a, Plant and Yano 2013).

A cold–pool parameterization coupled with convection is already proposed (Grandpeix and Lafore 2010), although we should view it with some caution (Yano 2012b). However, the evidence for cool–pool triggering of convection remains somewhat circumstantial, and a clear chain of cause and effect has never been identified. Much of the literature rather crudely argues that cold pools induces gust fronts, and then a gust front induces a low–level convergence within a stagnating air mass, which ultimately leads to convective–scale uplifting, and hence a triggering mechanism for convection. If we take this chain of causality literally, in fact, a cold pool would not “trigger” convection in any simple manner. The literature is unfortunately confused by quoting papers on cold pools and gust fronts alongside each other without clearly distinguishing between the two. More importantly, recall the limits of the idea of boundary–layer control of convection under mass–flux formulation discussed in T1.1, and more extensively in Yano et al. (2013b).

We may even point out that the concept of a “trigger” is never clearly defined in literature, but always referred to in a phenomenological manner, even in an allegorical sense. The same notion is never found either in fluid mechanics or turbulence studies. Recall that we have already emphasized that the notion of a “trigger” is fundamentally at odds with the basic formulation of the mass–flux parameterization (cf., Sec. 2.1, T1.1, Q1.2.1).
5.1.3. Topography

Topography can often help to induce convection by a forced lifting of horizontal winds (cf., Browning et al. 1974, Fuhrer and Schär 2005). Thus, subgrid–scale topography is likely to play an important role in triggering convection. Studies on subgrid–scale topographic trigger of convection as well as assessment of the possible need for incorporating this into a parameterization are still much missing.

5.2. Link to the downscaling problem

As already emphasized in several places, the goal of parameterization is to provide a grid–box averaged feedback of a subgrid–scale process to a large–scale model. Thus, any subgrid–scale details themselves are beyond a scope of a parameterization problem. However, in some applications, these subgrid–scale details often become their own particular interests. A particularly important example is a prediction of local extreme rainfall, that typically happens at a scale much smaller than a model grid size. A procedure for obtaining such subgrid–scale details is called downscaling. A link between parameterization and downscaling is emphasized by Yano (2010), and much coordinated efforts on these two problems are awaited.

T4.2: Proposal and recommendation on observational validations

A review on observational validation of precipitation is found as one of the book chapters (Rezacova et al. 2014). However, unfortunately, current validation efforts are strongly application oriented and weak in theoretical, mathematical basis. Especially, the current methods are not able to identify a missing physical process that has led to a failed forecast. The use of wavelet analyses, for example, could help to overcome this by establishing a link between forecast validations and model physical processes. The importance of the precipitation–forecast validation is also strongly linked to issues associated with the singular nature of precipitation statistics (strongly departing from Gaussian, and even from log-Gaussian against a common belief), leading to particular importance of investigating extreme statistics (cf., Sec. 5.2).

In the longer term, the need for probabilistic quantifications of the forecast should be emphasized, as already suggested at several places (cf., Q2.1.2, Q3.4.4). We especially refer to Jaynes (2003) and Gregory (2005) for the basics of the probability as an objective measure of uncertainties. From the point of view of probability theory, the goal of the model verification would be to reduce the model uncertainties by objectively examining the model errors. In order to make such a procedure useful and effective, forecast errors and model uncertainties must be linked together in a direct and quantitative manner. Unfortunately, many of the statistical methods found in general literature are not satisfactory for this purpose. The Bayesian principle (op. cit.) is rather an exception that can provide such a direct link so that from a given forecast error, an uncertainty associated with a particular parameter in parameterization, for example, can objectively and quantitatively estimated. The principle also tells us that ensemble, sample space, randomization, etc. as typically employed in statistical methods are not indispensible ingredients for uncertainty estimates, although they may be useful.
See Q3.4.4 for a link to issues of required model complexity and uncertainties.

**T4.3: Proposal and recommendation for a parameterization with unified physics**

The issue of “unified physics” is often raised in existing reviews on the subgrid–scale parameterization problem. The best example would be Arakawa (2004). To directly quote from his abstract: ‘for future climate models the scope of the problem must be drastically expanded from “cumulus parameterization” to “unified cloud parameterization,” or even to “unified model physics.”’ This is an extremely challenging task both intellectually and computationally, ...’ However, we have to immediately realize that, at the most fundamental level, there is no need for unifying any physics in the very context of atmospheric science.

The best historical example for an issue of unification of physics is the one between mechanics and electromagnetism encountered towards the end of the 19th century. The system of mechanics is invariant under the Galilean transform, whereas the system of electromagnetism is invariant under the Lorentz transform. Thus these two systems were not compatible each other. This led to a discovery (or more precisely a proposal) of relativity by Einstein, that unified the physics.

However, there is no analogous issue of unification in atmospheric science. Our model construction starts from a single physics. Of course, this is not to say that all the physics are already known. That is clearly not the case, particularly for cloud microphysics. However, the issue of “unified physics” in atmospheric science arises not through any apparent contradiction in the basic physics but only after a model construction begins.

To develop a numerical model of the atmosphere, often, a set of people are assigned separately for the development of different physical schemes: one for clouds, another for convection, a third for boundary layer processes, etc. Often this is required because the development of each aspect needs intensive concentration of work. This also leads to separate development of code for different “physics”. However, a complete separation of efforts does not work ultimately, because, for example, clouds are often associated with convection, and convection with clouds. The treatment of clouds and convection within the boundary layer faces a similar issues: should they be treated as a part of a boundary-layer scheme simply because they reside in the boundary layer?

It transpires that the issue of unification of the physics only happens in retrospect, and only as a result of uncoordinated efforts of physical parameterization development. If everything were developed under a single formulation, such a need should never arise afterwards. In this very respect, the main issue is more of a matter of the organization of model development rather than a real scientific issue (cf., Q3.4.1). In other words, use your pencil and paper, and write down everything together, before you begin to type down even a single line of code. Coding may be individual work, but you have to write down everything on paper together before getting to that phase.

In order to follow such a method of course a certain general methodology is required. That has been the main purpose of the present Action. See T2.4, Q3.4.1, and Yano et al. (2005) for further discussions.

It could be tempting to add the existing parameterization codes together in consistent manner from a practical point of view. However, such an approach could easily turn out be to less practical in the long term. One may add one more dictum to a list already given in Q3.4.1: never go backwards. As already suggested in Sec. 5.1.1, for example, it is something of a historical mistake that downdrafts are introduced into the mass–flux formulation in an ad hoc manner.
As already emphasized in Q3.4.1, it is imperative to re–derive all the schemes from basic principles in order to establish unified physics, checking consistency of each hypothesis and approximation. This may sound a painful process. However, this is what every researcher is expected to do whenever he or she tries to use a certain physical scheme. In the end, if the original development has been done relatively well, the resulting modifications to a code could also be relatively modest.

In order to establish such consistency in combination with the use of a mass–flux parameterization, SCA and its further relaxations would become an important guiding principle, as already suggested in T2.4. By relaxing SCA, it is straightforward to take into account of a certain distribution over a particular subgrid–scale component (segment); especially, over the environment. Such an idea is first introduced by Soares et al. (2004), and an SCA procedure can derive such a formulation in a more self–consistent manner.

All of the non–mass flux based parameterizations, such as eddy transport, must be handled in this manner for consistency. Note that these non–mass–flux–based parameterizations may be introduced into different subgrid–scale components, for example, into the environment and convection. In order to maintain overall consistency, a fractional area occupied by a given subgrid–scale component must explicitly be added to the formulation. Note that this pre–factor is usually neglected in standard non–mass–flux parameterizations assuming that convection occupies only an asymptotically vanishing fraction. In the high resolution limit, this assumption becomes no longer true, as already discussed in T3.2 and T3.3.

Importantly, such a pre–factor can easily be added to an existing code without changing its whole structure. This is just an example to demonstrate how the consistency of physics can be re–established by starting from the first principle of derivations, but without changing the whole code structure: use your pencil and paper carefully all the way, which is a totally different task than coding.

**Q4.3.1: How can a microphysical formulation (which is by itself a parameterization) be made resolution dependent?**

First of all, as a minor correction, according to the terminology already introduced in Sec. 1.1, the microphysical formulation is not by itself a parameterization, although it is most of time phenomenologically formulated. These phenomenological microphysical descriptions are defined at each “macroscopic” point. In this context a macroscopic point is defined as having a spatial extent large enough so that enough molecules are contained therein but also small enough so that spatial inhomogeneities generated by turbulent atmospheric flows are not perceptible over this scale. Such a scale is roughly estimated as a micrometer scale.

A typical model resolution is clearly much larger that this scale, and thus a spatial average must be applied to the phenomenological microphysical description developed for a single macroscopic spoint. Averaging leads to various Reynolds’ stress–like and nonlinear cross terms, which cannot be described in terms of resolved–scale variables in any obvious manner.
Little explicit investigation of this issue has been performed so far. Especially, a mathematical theory is required in order to estimate cross correlation terms under an expected distribution of the variables in concern.

**Q4.3.2: Can detailed microphysics with its sensitivity to environmental aerosols be incorporated into a mass-flux convection parameterization? Are the current approaches self-consistent of not? If not, how can it be achieved?**

The response of convective clouds and precipitation to aerosol perturbations is intricate and depends strongly on the thermodynamic environment (Khain 2008), but the response is potentially very large (Rosenfeld et al. 2008), thus an adequate representation of the effects in numerical weather prediction would be desirable, and probably more so for climate projections. The microphysics currently applied within convective parameterizations has not much improved since e.g., Tiedtke (1989).

The primary interactions between aerosols and convection are the wet scavenging of aerosols by precipitation, and the co-variation of wet aerosol mass and aerosol optical depth with cloud properties in the humid environment of clouds (Boucher and Quaas, 2013). A very simplified microphysical representation of the effect of aerosols on convective precipitation would be to use a “critical effective radius”, or a threshold in effective droplet size before the onset of rain (Freud and Rosenfeld 2012). For this reason, the size of droplets that grow in the updraft is taken instead of the height above cloud base as in Tiedtke (1989) in order to determine the convective precipitation rate. This implementation has been tested in the ECHAM general circulation model by Mewes (2013). A large effect is found probably because of a missing wet scavenging in this model version. The actual convective invigoration hypothesis (Rosenfeld et al. 2008) cannot be tested in a convection parameterization, though, until freezing and ice microphysics are implemented.

Here is another important question which should have been added in MoU:

**5.3. How can observations be used for convection parameterization studies?**

Observations (and measurements) are fundamental to science. However, further observations are not necessarily useful per se, and they require a context in which it has been established what we have to observe (or measure) for a given purpose, including specifications of the necessary temporal and spatial resolution as well as the accuracy of the measurements.

Parameterization development and evaluation may ideally take a two-step approach (Lohmann et al. 2007). Insights into new processes and initial parameterization formulation should be guided by theory and process-level observations (laboratory experiments and field studies). The latter may be substituted by LES/CRM–based modelling, if suitable observations are unavailable. However, once implemented, further testing and evaluation are required in order to ensure that the parameterization works satisfactorily for all weather situations and at the scales used in a given model. Satellite observations are probably the most valuable source of information for the latter purpose, since they offer a large range of parameters over comparatively long time series and at a very large to global coverage. The A-train satellites may be noted as a particular example. In order to facilitate such comparisons, “satellite simulators” have been developed, which emulate satellite retrievals by making use of model information for the subgrid-scale variabilities of, for example, clouds leading to statistics summarizing
the model performances (Bodas-Salcedo et al. 2011, Nam and Quaas, 2013, Nam et al. 2014). A large range of methodologies has also been developed in terms of process-oriented metrics, e.g., for investigating the life cycle of cirrus from convective detrainment (Gehlot and Quaas 2013), and for elucidating the details of microphysical processes (Suzuki et al. 2011). In addition to those techniques focusing on individual parameterizations, data assimilation techniques can also be exploited as a means of objectively adjusting convection parameters and learning about parameter choices and parameterizations (Schirber et al. 2013).

However, fundamental limits of current satellite measurements must be recognized. Most satellites measure only cloud optical properties (either in visible or invisible range of light). These quantities, unfortunately, do not provide much useful direct information about the dynamical convective processes (e.g., vertical velocity).

In reviewing the closure problem (Yano et al. 2013b), the difficulties were striking for the apparently simple task of identifying convection objectively from observations. Most of the observational analyses that we have reviewed use the precipitation rate as a measure of convection. Although this could be, partially, acceptable over the tropics, such a measure is no longer useful enough over the midlatitudes, where much of the precipitation originates from synoptic scale processes. It may be needless to emphasize that satellite images (such as outgoing wave radiation) are even less reliable as a direct measure of convection.

After a long process of discussions, we finally identify lightning data as, probably, the best measure of convection currently available at a routine level. In our best knowledge, lightning happens only in association with strong vertical motions and extensive ice, so that the cloud must be high enough and dynamically very active. A fair objection to this methodology would be the fact that we would still miss some convection under this strategy, and that other factors (e.g., aerosol conditions, degree of glaciation of convection) may also influence the lightning even if the vertical motions are unchanged. We also have to keep in mind that this is only a qualitative measure without giving any specific quantification such as convective vertical velocity. However, importantly, when there is lightning, this is a sure sign that there is convection.

Satellite lightening data from NASA’s OTD (Optical Transient Detector) mission exists for the period of April 1995–March 2000 (thunder.msfc.nasa.gov). Lightning data from a ground-based lightning–location system is also available over Europe. Such a network has a high detection efficiency (70-90%) and location accuracy (< 1 km). European Cooperation for Lightning Detection (EUCLID: euclid.org), consisting of 140 sensors in 19 countries, is currently the most comprehensive network over Europe organized under a collaboration among national lightning detecting networks. Based on this measurement network, we are planning to perform systematic correlation analysis between lightning frequencies and other physical variables (column-integrated water, CAPE, etc). This project would be considered an important outcome from the present COST Action.

As a whole, we emphasize the importance of identifying the key variables as well as processes in order to analyze convection for the development, verification, and validation of parameterizations. Very ironically, in this very respect, the traditional Q1 and Q2 analysis based on a conventional sounding network can be considered as the most powerful observational tool for convection parameterization.
studies. The reason is very simple: that these are the outputs we need from a parameterization, and thus must be verified observationally.

6. Retrospective and Perspective

The Action is to identify the closure and the entrainment–detrainment problems as the two highest priorities for convection parameterization studies under the mass–flux formulation. The conclusion is rather obvious in retrospect: closure and entrainment–detrainment are the two major cornerstones in mass–flux convection parameterization formulation. Unless these two problems are solved satisfactory, the operational convection parameterization under mass–flux formulation would never work satisfactory.

It is rather surprising to realize that it took us four years of reflections in order to reach this very simple basic conclusion collectively. The original MoU, prepared by the members by extending editing, has even failed to single out these two basic issues. MoU has wrongly identified the “convective triggering conditions” as one of key elements of mass–flux formulation along with closure and entrainment–detrainment (Sec. B.2, cf., Sec. 2.1, T1.1). MoU also provides a false anticipation (the 4th secondary objective: cf., Sec. 1.5) that extensive process studies by CRM and LES would by themselves automatically lead to improvements of parameterization. In the end of the present Action, we strongly caution such a naive perspective. Though the value of the process studies should hardly diminish in its own right, they do not serve for a purpose of parameterization improvements by themselves automatically unless we approach to the problem from a good understanding of the latter. This point is already extensively discussed in Secs. 1.3, 1.4, 5.1, and re–iterated again below.

The intensive theoretical reflections on the subgrid–scale parameterization problem over these four years have been a very unique exercise. We can safely claim that all our meetings have been great success with great satisfactions of the participants. However, this rather reflects a sad fact that such in–depth discussions on the issues from scientific theoretical perspectives (and just simply making any logic of an argument straight) are rarely organized nowadays with dominance of approaches seeking technological solutions with massive modelling and remote sensing data. At the more basic level, the present Action gives a lesson on importance to just sit and reflect: the current scientific culture needs to be much changed towards this direction.

The present report summarizes the achievements which have been made during the Action ES0905. Those results would have been possibly only under a support of COST, as suggested in the last paragraph. A healthy scientific environment in our discipline (as in many others) requires healthy respect and balance between observational, modeling and theoretical investigations. An imbalanced situation ultimately hinders progress. The present Action has been motivated from a sense that theoretical investigations are currently under–weighted. This sense has been reinforced during the Action itself, which has shown promise that even relatively modest efforts towards redressing the balance could prove extremely fruitful.

However, setting the two identified priorities, closure and entrainment–detrainment, actually into forefront of research for coming years is already challenging. We first of all need to overcome the basic strategies of participating agencies already so hard–wired differently before we can launch such an ambitious project.
The current economic crisis rectifies this general tendency further with even more emphasis on technological renovations rather than fundamental research at the EU level. Budget cuts at a level of individual institution is more than often associated with a more focus on short term deliverables and products, rather than more fundamental research. Such short-term focused (and short-sighted) strategies are likely to lead to depletion of creative real innovative ideas in longer terms.

Improving parameterization is a long and difficult path, due to the many feedbacks within numerical models for weather and climate. True improvements are possible only in association with fundamental research. Though in short terms quick dirty fixes are possible, they would be likely to lead to long-term deterioration of true quality of models.

This whole situation has already begun to deplete future visions for research: in spite of the very logical basic conclusion, majority of participating researchers do not feel ready to focus themselves on these basic issues. Clearly we are short of specific plans for tackling them. On the closure problem, a few possibilities are listed, but we are far from reaching any consensus. The situation with entrainment and detrainment is even worse. Though we identify couple of elements to consider, there is no identified line for further theoretical investigations.

Less is even said about perspectives for developing a more fundamental research from a turbulence perspective. It is long stated that convection parameterization is a statistical description (cf., Kuo 1974). However, a statistical mechanics for describing ensemble convection system is still to be emerged even after 40 years. Here, we even face a difficulty for obtaining a funding towards this goal at an European level. Under this perspective, the phenomenologically-oriented process study would become the highest priority for years to come, though fundamental limitations of this approach must well be kept in mind.

The best lesson to learn is an improvement of QN into EDQN as discussed in Secs. 1.3, 1.4. It is easy to criticize the QN model from a phenomenological perspective. Indeed, it neglects various phenomenologically well-known aspects of turbulence: intermittency, inhomogeneity, etc. In the same token, it is easy to criticize the current convection parameterization based on lack of various phenomenological elements: lack of life cycle, coupling with boundary layer, mesoscale organization, etc.

However, making these statements by itself does not improve a parameterization in any manner. Further investigations on these processes themselves do not contribute to an improvement of a parameterization in any direct manner, either. In order to move to this next step, a strategy (or more precisely a formulation) for implementing those processes must exist.

Even first required is a clear demonstration that lack of these processes is actually causing a problem within a given parameterization. Recall that a parameterization may run satisfactory by still missing various processes what we may consider to be crucial. For example, Yano and Lane (2014) suggest that the wind shear may not be crucial for thermodynamic parameterization of convection, although it plays a critical role in organizing convection in mesoscale. The goal of the parameterization is just to get a grid-box averaged feedback of a given whole process (not each element) correct. No more detail counts by itself.

In this respect, the evolution of QN into EDQN is very instructive. The study of Orszag (1970) on the QN system identified that the real problem of QN is not lack of intermittency or inhomogeneity, but simply due to an explosive tendency in growth of skewness in this system. This lesson tells us that
it is far more important to examine the behavior of an actual parameterization in concern, rather than
performing extensive phenomenological process studies.

Turbulence studies also suggest that what appears to be phenomenologically important may not be at all crucial for purely describing large–scale feedbacks. For example, Tobias and Marston (2013) show that even a simple statistical model truncated at the second order of cumulant (which is a variant of moment adopted in the QN model) can reproduce realistic multiple jets for planetary atmospheres for a realistic parameter range, in spite of the fact that this model neglects many intrinsic characteristics of the turbulence, as the case for QN.

As the present Action has identified, the parameterization problem (as for any other scientific problems) is fundamentally even ontological (Yano et al. 2013a). The present situation is like blind people touching different parts of an elephant (the trunk, a leg, the nose, ...), and argue harshly over a true nature of the elephant without realizing that they are only touching a part of it. We scientists are, unfortunately, not far from those blind people so long as the parameterization problem is concerned.

Thus, the last recommendation in concluding the present Action is to create a permanent organization for playing such an ontological role in the parameterization problem, possibly along with the other fundamental problems in atmospheric modelling. Here, the ontology should not mean pure philosophical studies. Rather, it should be a way for examining the whole structure of a given problem from a theoretical perspective in order to avoid myopic tendencies of research. The present Action has played this role for last four years, and it is time to pass this responsibility to a permanent entity.

Acknowledgements

The present report has emerged through the meetings as well as many informal discussions face to face and over e-mails for the whole duration of the COST Action ES0905. The lead author sincerely thanks to all the COST participants for this reason. Especially, in preparing the manuscript, Florin Spineanu, Linda Schlemmer, Alexander Khain have provided text segments. Parts of the manuscript are carefully proof read by Peter Bechtold, Alexander Bihlo, Elsa Cardoso, Alan L. M. Grant. Marja Bister and Sandra Turner have provided specific comments. Wim de Rooy has gone through the whole manuscript several times for critical reading and with suggestions for elaborations. Discussions on turbulence modelling with Steve M. Tobias and a reference on TRMM data from Steve Krueger are also acknowledged. The image for Fig. 1 is provided by Szymon Malinowski. DM’s contribution to the present paper is limited to the WG3-related issues.
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