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Project Report

Basic Concepts for Convection Parameterization in Weather Forecast and Climate Models: COST Action ES0905 Final Report

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Abstract: The research network “Basic Concepts for Convection Parameterization in Weather Forecast and Climate Models” was organized with European funding (COST Action ES0905) for the period of 2010–2014. Its extensive brainstorming suggests how the subgrid-scale parameterization problem in atmospheric modeling, especially for convection, can be examined and developed from the point of view of a 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. The need for a drastic change of the current European research culture as concerns policies and funding in order not to further deplete the visions of the European researchers focusing on those basic issues is emphasized.

Keywords: parameterization; convection; subgrid scales

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1. Introduction

The research network COST Action ES0905 "Basic Concepts for Convection Parameterization in Weather Forecast and Climate Models" was organized with European funding over the period 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/essem/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 (http://convection.zmaw.de/).

Our major achievement as a deliverable is a monograph [1]. Reference to this monograph is made frequently throughout the report for this reason. However, the present report is not an abbreviation of the monograph: it is another key deliverable that, by complementing the monograph, more critically analyzes the current convection parameterization issues and presents the future perspectives.

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 by submitting text segments, by proof reading part of or the entirety of the text, as well as general comments. These contributions are listed in the acknowledgments 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 while also reflecting well the overall Action achievements.

By its nature, the present report does not intend to be a comprehensive review, but focus more specifically on the issues listed in MoU and actually discussed in due course of the COST Action. The report particularly does not always remark on the issues already accepted to be important in the community. The emphasis is, more than often, on the issues that are not widely appreciated, and the report tends to take an unconventional view in order to compensate for the currently widely accepted views. For this reason, the report is overall, more critical with the current state-of-the-art of the research. However, we request that the readers not to read us as critics for the sake of critics. The contributing authors also bear these criticisms as our common fault of failing to do better than otherwise. The intention of these critical remarks are so that our convection parameterization research becomes more sound in future to come. Of course, the authors do not claim that all the arguments developed in the present report are ultimately right, but are only the best ones that they can offer for now. Thus, these arguments must be used as a starting point for more critical and constructive debates on the future direction of the convection parameterization research. The general perspectives in the last section of the report are also developed from this point of view.

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.* [2]). Convection is one of the key processes to be parameterized considering its importance in heat and moisture budget of the atmosphere (*cf.* [3]), 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 brain storming 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 endeavor [4]: (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.* Section 2.4.1).

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. Such a careful parameterization study should not be confused with the process studies, either.

A good historical lesson to learn from turbulence research is an improvement of the so-called QN (quasi–normal) model into the EDQN (eddy–damping quasi–normal) model in simulating the turbulent kinetic energy spectrum (*cf.* Ch. VII in [5]). 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. A process study has no contribution here either. Just imagine, if Orszag [6] 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, especially for development and verification of parameterizations.

Of course, this is not 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. Merits of running several models together are hardly denied either. In this way, we can clearly see common traits in a certain class of parameterization more clearly than otherwise (e.g., [7,8]).

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.* [7] 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., [9] and the references therein: see also **Q1.2.1**, **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.* [7] 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.* Section 2.1)?

The lead author would strongly argue that this work has led to a rather misguided convection–parameterization research over the following decade. In short sight, it is easier to control convection height by entrainment and detrainment rather than by closure. Thus, vast research effort is diverted to the former with an overall neglect of the latter.

As it turns out, the closure is rather a key issue for this problem as demonstrated by Bechtold *et al.* [10] 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? In addition, how we identify a possible modification of the closure in this manner? It would be similar to asking to Orszag [6] 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 next section, we examine our major achievements by following our four major activities:

1. Mass-flux based approaches (Section 2.1)
2. Non-Mass Flux based approaches (Section 2.2)
3. High-Resolution Limit (Section 2.3)
4. Physics and Observations (Section 2.4)

These four activities, respectively roughly cover the four secondary objectives listed in MoU (Section 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. On the other hand, the order and the numbering of the Tasks are unaltered from MoU, which is considered like a legal document binding us. This choice is made in order to make it clear that the present report is written directly in response to MoU, though the pre-given order may not be the best. To compensate, extensive inter-references between the Tasks and Questions are given in the text.

In answering these tasks and 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. Tasks and Questions

2.1. 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 [11]. 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 (background turbulence) 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.* Section 2.3).

2.1.1. Overview

Reminders of some basics of mass-flux convection parameterization (*cf.* [12,13] 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.* [14–16], **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.* [17]: 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 [18].

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:

$$M = \eta(z)M_B(t) \tag{1}$$

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 [19]. 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:

$$\frac{\partial M}{\partial z} = E - D \tag{2}$$

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 Equation (2: *cf.* **T1.4**). Here, however, note a subtle point that strictly the mass–flux profile, $\eta(z)$, also changes with time through the change of the entrainment and the detrainment rates by following the change of a large–scale state.

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-the-Art of Closure Hypothesis

Our review on the closure [20] has facilitated in resolving the afternoon convection problem [10]: 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., [21,22]), because they appeared to control the transformation from shallow to deep convection (*cf.* Section 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 [20] has greatly contributed in identifying this key issue. Bechtold *et al.* [10], in turn, have actually implemented our key conclusion into operation.

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 [23]) or in the free troposphere (parcel-environment based closure [24,25]). The boundary-layer controlled closure tends to be more popular in the literature (e.g., [26–28]), 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 [29], 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 forecast models. Though some global climate models do adopt boundary-layer controlled closures [30–32], their behavior tends to noisier than otherwise (*cf.* Figures 3, 4, 6 and 7 of [32], respectively). Under the parcel–environment based closure, 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. Though a process study on trigger of convection over a heterogeneous terrain, for example, may be fascinating (*cf.* [33]), no direct link with the closure problem should be made prematurely (*cf.* Section 2.4.1.2).

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 by Yano *et al.* emphasizes the superiority of the parcel-environment based closure against the boundary–layer controlled closure [20]. 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 [10]. This implementation does not rely on any tuning exercise either: the result is rather insensitive to the major free parameter T^* (in their own notation) for the range of 1–6 K against the reported choice of T^* = 1 K [34]. The reported model improvement does not depend on details of the entrainment-detrainment, either [35].

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 [36]. Note that Bechtold *et al.* [10] also maintain a boundary–layer based closure for shallow convection.

T1.2: Critical Review of the Concept of Convective Quasi-Equilibrium

Convective quasi–equilibrium, as originally proposed by Arakawa and Schubert in 1974 [37], is considered one of the basic concepts in convection closure. A review by Yano and Plant [38], 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 [39]). 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. For example, though Davis *et al.* [40] examine convective quasi–equilibrium, their formulation is based on the re–interpretation of the Arakawa and Schubert's original formulation into a relaxation process (*cf.* Section 4.5 of [38]).

Furthermore, along a similar line of investigation, the convective energy cycle becomes another issue to be closely examined [9,41,42]. **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 fully working quasi–equilibrium based convection parameterization. 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.* Figure 6 [43]). 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 the very 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 in 1974 [37], 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 Equation (150)) from observations. Here, the hypothesis is stated as

$$-\sum_{j=1}^{N} \mathcal{K}_{ij} M_{Bj} + F_i = 0 \tag{3}$$

for the i–th convective type, where $\mathcal{K}_{ij} M_{Bj}$ 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, $\mathcal{K}_{ij}$, are expected to be positive, especially for deep convection due to its stabilization tendency associated with warming of the environment by environmental descent.

Currently intensive work by Jun-Ichi Yano and Robert S. Plant on this issue is underway. An important preliminary finding is that some of the matrix elements, $\mathcal{K}_{ij}$, 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 [37] themselves, by coupling between an extension of their closure hypothesis (Equation (1.3), or their Equation (150)) into a prognostic version (*i.e.*, Equation (142))

$$\frac{dA_i}{dt} = -\sum_{j=1}^{N} \mathcal{K}_{ij} M_{Bj} + F_i \tag{4}$$

and the kinetic energy equation

$$\frac{dK_i}{dt} = A_i M_{Bi} - D_{K,i} \tag{5}$$

presented by their Equation (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 [44,45] proposed to take this pair of equations as the basis of a prognostic closure. The possibility is recently re-visited by Plant and Yano [9,41,42] under a slightly different adaptation.

This convective energy–cycle system, consisting of Equations (4a,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: [41]), as well as transformations from shallow to deep convection [9,42]. 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 (4a,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 Equation (3), which may be generalized into a prognostic form (4). This is coupled with Equation (5), 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., [46]. 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 Formulation 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 [47], 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**, [48]). 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., [49]) and the convective parcel buoyancy (e.g., [37]). 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 [50]. 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

$$F_i + D_{c,i} = 0 \tag{6}$$

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, $\mathcal{K}_{i,j}$, 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

$$D_{c,i} = \sum_j \mathcal{K}_{i,j} M_{B,j} \tag{7}$$

Arakawa and Schubert's convective quasi–equilibrium principle Equation (3) reduces to a special case of Equation (6). This general framework is expected to be useful in order to objectively identify basic principles for choosing more physically based closure conditions.

Note that though stochasticity may be added to a closure condition, it is possible only after defining a deterministic part of closure. See **Q1.3.2**, **T3.3** for further discussions (see also [51]).

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.* Section 2.1.1), 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 ([52]: see also [53,54]). However, as already discussed in Section 2.1.1, 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 [19] 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 [9,42]. 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 [55]. Specific examples of the applications include Melbourne and Stuart [56], and Gottwald and Melbourne [57].

T1.4: Review on Current State–of–the–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.* Equation (2)). A review on these processes is published as [58] 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.* [58], there are two major approaches:

(1) Estimate by directly diagnosing the influx and outflux through the convection–environment interfaces [59,60]. 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 [61,62]. 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.* [63]. Also there is a subtle, but critical difference between the methods by [61,62], 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 [61] 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 [62], 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 [61] 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, φ, which is defined by a vertical integral of

$$\frac{\partial \varphi_c}{\partial z} = -\frac{E}{M}(\varphi_c - \bar{\varphi}) \tag{8}$$

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.* [64]).

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 [61], who explicitly retain the eddy transport in their diagnosis. This idea is further developed into convection parameterization combining the eddy diffusion and mass flux (EDMF: [54], see further **T4.3**). 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 (*cf.* **T4.3**).

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 [59], and Dawe and Austin [60] 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 (*i.e.*, no molecular diffusion), 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. Note that the turbulent processes only lead to continuous distortions of the material surfaces without mixing in the strict sense, though it may look like a mixing (as represented as an eddy diffusion) under a coarse graining. 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 (Figure 1: *cf.* [65] see also [66]).

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).

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 [58] 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 [67,68] and Paluch [69]. In this manner, it also establishes that the current entrainment–detrainment formulation for defining convective mass flux, M, under the Formula (2) is more robust than other existing proposals.

Furthermore, de-Rooy *et al.* [58] suggest some specific research directions:

(i) Critical fractional mixing ratio originally introduced in a context of a buoyancy sorting theory [70]: 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., [71,72]), although the mechanism behind is not yet well understood.

The vertical mass–flux profile is strongly controlled by detrainment as originally pointed out by [73]: see also [72,74]) with theoretical arguments provided by [75]. 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 [73,74,76]. Unfortunately, almost all of the current parameterizations do not yet take this aspect into account. For example, the Kain and Fritsch scheme [70] 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 [73,76].

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 *et al.* [77]. 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.* [58]. In the moist convection context, even a precise physical meaning of the entrainment-detrainment concept is lost, as emphasized by Morton [78]. See Yano [79] for a historical review with extensive references therein. 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 Figure 2 of de Rooy and Siebesma [75]. This basic premise is also well summarized, for example, in the introduction of [80] 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 Figure 2 of [75]. 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 Figure 3 of [81], 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 [82] 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.* [83] 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 (*cf.* Equation (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 Figure 2 of [75] 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.* [77] 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 Equation (8). Second, recall that the mass flux, M, consists of two parts: convective vertical velocity, w_c, and the fractional area, σ_c, for convection,

$$M = \rho\sigma_c w_c \tag{9}$$

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 Equation (2).

The convective vertical velocity is commonly evaluated by taking Equation (15) of [84]. 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.* [18]).)

The fractional area for convection could, in turn, be evaluated by a fully–prognostic version of Equation (2):

$$\frac{\partial \sigma_c}{\partial t} = E - D - \frac{\partial M}{\partial z} \tag{10}$$

Of course, this equation retains both entrainment and detrainment. However, Gerard and Geleyn [85] were able to overcome this difficulty by introducing an alternative equation for σ_c based on the moist–static energy budget (their Equation (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 φ_c via Equation (8)).

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 parameterizations, especially the closure. Critical properties are identified empirically, which may connect the convection parameterization problem with statistical physics theories of critical phenomena (*cf.* [86]). 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 [87].

Peters *et al.* [88] 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 [89].

Peters and Neelin [90] 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.* [91] 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 [92]. A size distribution of mesoscale convective clusters also follows a power law [93,94]. 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 [95].

Peters and Neelin's results [90] 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 [96,97]. Further analysis is needed in order to confirm that the average precipitation is bounded for high water vapor values. Yano *et al.* [98] 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 [90] but not for column-integrated water vapor. These two different tendencies were interpreted as indicative of two different underlying mechanisms: SOC and homeostasis.

Here, homeostasis is understood as a behavior 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 neighbors, 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 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.* [98] interpret Raymond's [99] thermodynamic self–regulation theory as a type of homeostasis (see also Section 6.3 of [38] 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 [100].

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 shade 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.* [101], and further extended in [12,13,102,103]. 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–SCAx [102].

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.* Section 2.1.1, **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 [12,13].

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.* [12,13]).

Arakawa and Wu [104] outline a more general and universal framework for convection parameterization. Readers are strongly encouraged to critically examine these two strikingly different paths proposed here based on SCA and the one proposed by Arakawa and Wu (see also [105]).

2.2. Non-Mass Flux Based Approaches: New Theoretical Ideas

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. Unfortunately, MoU does not list stochasticity as an agenda. However, a book chapter [51] is exclusively devoted to this issue with extensive references therein (but see also **T1.3**, **Q1.3.2**, **T3.3**).

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 [106–110]. 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 formulation 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 [111].

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, as shown by Grant [52,112] for shallow and deep convection, respectively. 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.* Section 1.3, [4]) 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 [113,114] 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 [115] suggest how such characterizations may help to understand microphysical sensitivities in large–eddy simulations.

Yano and Phillips [116] 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 mircophysical 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.* [17]). 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 plumes 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.* Section 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.* **Q3.4.3**).

Q2.1.3: How should a Fully Consistent Energy Budget be Formulated in the Presence of Precipitation Processes?

As we already emphasized in Section 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 [117]. 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 [16,118].

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 [16,118,119], 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 [120], 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 Parameterizations? 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., [121–123].

In particular, Bony and Emanuel [121] 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 quantitative 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 [124] 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.* [125] 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.* [126]. 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: **T1.3**,**Q1.3.2**, **T3.3**, and Plant and Bengstsson [51].

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

Here, the statistical cumulus dynamics refers to the description of the cumulus dynamics in analogy with the statistical mechanics (*cf.* **Q1.7**). It is often argued that subgrid-scale parameterization is fundamentally "statistical" in nature (*cf.* [49]). 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 [11]. 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 (*cf.* [9,42]). 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 [127,128]. 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. The 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 [47], as pointed out by Plant and Yano [48]: it assumes both the cloud work function, A, and the mass flux, M, change in time with the same rate in the order

$$\frac{1}{A}\frac{\partial A}{\partial t} \sim \frac{1}{M}\frac{\partial M}{\partial t} \tag{11}$$

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.*,

$$|\frac{1}{A}\frac{\partial A}{\partial t}| \ll |\frac{1}{M}\frac{\partial M}{\partial t}| \tag{12}$$

at another point under a single derivation process. The two conditions, Equations (8) and (12), 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 modeled 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 [14–16,118,129]: see Section 2.3.1 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 similar issue also arises in dealing with all types of subgrid–scale motions consistently. As suggested in the beginning of Section 2.1, our tradition is to deal with them under a dichotomy between convection and turbulence. However, in this case, too, a careful separation between them must be performed in order to avoid double counting. To some extent, it could be easier to deal with all types of motions under a single formulational framework (*cf.* **Q3.4.2** for more).

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.

2.3. 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-the-Art of High-Resolution Model Parameterization

See a book chapter [130] 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, σ_c, 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 (see Appendix for the details). 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 $\sigma_c \rightarrow 0$. This strategy, currently adopted at ECMWF [131], 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 [132,133].

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) [85,134,135]. 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 [136]. 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.* Section 2.4.2).

Efforts ongoing at DWD (TKE-Scalar Variance mixing scheme: TKESV, by Machulskaya and Mironov [137]) 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 was developed relatively easily at DWD.

In reviewing these different approaches, it may be remarkable to note that the two approaches, 3MT 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., [104,105]).

2.3.1. 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 modeling 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 endeavor. 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. Specific examples for developing a subgrid–scale parameterization in a general manner under the above strategy are: mode decomposition [101], moment expansion [138], and similarity theory ([111], *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 [14] 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 $s = s_{ref} + c_{pd} \ln(\theta_s)$, where s_{ref} and c_{pd} are two constants. Therefore, θ_s is a general measure of moist-air entropy. The important application shown by Marquet and Geleyn [14–16,118,129] is that values and changes in θ_s are significantly different from those of θ_l and θ_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 θ_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 θ_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 θ 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 θ 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 [16,118], 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 by Marquet and Geleyn [118] 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 [14,16]. Hence, a moist–turbulent parameterization schemes with a medium–level of sophistication must be based on the moist-air entropy. It is also important for such a scheme to have a reasonable and independent closure for the cloud amount, which ought to be competitive with the particular roles in organized plumes through 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: High–Resolution Limit: Questions

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

Q3.4.1: 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 to override 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.* [102]. For general possibilities for dealing a parameterization problem as a numerical issue, see **Q1.6.2**.

Q3.4.2: 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 posteriori* technical assumptions that introduce a scale dependence, or else the fact that the scale separation may hold only in a 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' [122] 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 [71]. 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 [139] 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.* Section 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**, **Q3.4.1**.

Clearly the best strategy in parameterization development would be to avoid an introduction of a scale–dependent parameter as much as possible, because it automatically eliminates a need for adjusting a scheme against the model resolution.

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.* **Q3.4.1**). 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.* Section 2.3.1) and in a self-consistent manner (*cf.* **T2.4**), as already emphasized, in order to avoid these operational difficulties.

Q3.4.3: 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 [140,141], 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 behavior 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.

Regardless of their complexity, any formulations for physical processes ultimately contain uncertainties (e.g., values of constant coefficients). Uncertainty of physical formulation leads to a growth of prediction uncertainty with time. Uncertainty growth can be well measured under a general tangent linear formulation which by constructing a linear perturbation equation along the original full model solution. It 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 main question here is 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.

2.4. 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 [142] in response to the first issue. The second issue is dealt with by one of book chapters [143], and some relevant aspects are already discussed in **Q2.1.2**.

A special direction of investigation and interest is the effect of aerosols on the intensity of tropical cyclones. Khain *et al.* [144] and Lynn *et al.* [145] 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 [146,147]. 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-latitudinal 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 [148]. 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 [149] as a result. High correlations are found between Zdr on the one hand, and hail mass and size on the other [150]. This finding opens a way to improve the short–range forecast of hail, its size, and the intensity of hail shafts.

2.4.1. 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.* Sections 1.3 and 1.4): importance in convective dynamics and that in convection parameterization. The following discussions are also developed under an emphasis of this distinction.

2.4.1.1. Downdrafts

Downdrafts have long been identified as a key process in convective dynamics [151–153]. From a theoretical point of view, the importance of downdrafts has been addressed in the context of tropical–cyclone formation [154] as well as that of Madden–Julian oscillations [155]. Although the majority of current operational mass–flux convection parameterizations do include convective downdrafts in one way or another (e.g., [156–159]), 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. 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.

Refer to a book chapter [160] on the state-of-the-art of the downdraft parameterization, including the issues of saturated and unsaturated downdrafts. Also note a link of the issues to the next section.

2.4.1.2. 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 [161,162], maritime deep convection [163–165] and continental deep convection (e.g., [166–169]). 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 [170–172]: but see [9,42]). A cold–pool parameterization coupled with convection is already proposed by Grandpeix and Lafore [173], and Rio *et al.* [30], although we should view it with some caution [174].

However, the evidence for cool–pool triggering of convection remains somewhat circumstantial, and a clear chain of cause and effect has never been identified. For example, Boing *et al.* [171] attempt sensitivity study by performing LES experiments with the four different set–ups with the aim of elucidating a feedback loop. However, this article fails to list the processes actually involved in a feedback loop. Though a careful Lagrangian trajectory analysis is performed, they fail to identify whether the Lagrangian particle has originated from a cold pool or not.

From a point of view of the convective energy cycle already discussed in **Q1.2.1** (see also [9,42]), deep convection is induced from shallow convection by the tendency of the latter continuously destabilizing its environment by evaporative cooling of non–precipitating water as it detrains. More precisely, this process increases both the available potential energy and the cloud work function for deep convection, and ultimately leads to an induction of deep convection, as manifested by a sudden increase of its kinetic energy. In a more realistic situation, the evaporative cooling may more directly induce convective downdrafts, which may immediately generate cold pools underneath. These transformations are clearly associated with an induction of kinetic energy from available potential energy. However, the existing literature does not tell us whether those pre-existing kinetic energy associated either with downdraft or cold pool is transformed into deep–convective kinetic energy, for example, by pressure force, or alternatively, an extra process is involved in order for a cold pool to trigger convection. The CRM can be used for diagnosing these energy–cycle processes more precisely. An outline for such a method is described by Figure 5 and associated discussions in Yano *et al.* [101]. However, unfortunately, this methodology has never been applied in detail even by the original authors.

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.* Section 2.1.1, **T1.1**, **Q1.2.1**). As also emphasized in Section 2.1.1, in order to introduce a trigger process into a parameterization, a radical modification of its formulation is required.

2.4.1.3. Topography

Topography can often help to induce convection by a forced lifting of horizontal winds (*cf.* [175,176]). 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.

2.4.2. 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 [177], 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 [178]. 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.* Section 2.4.2).

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**, **Q3.4.3**). We especially refer to Jaynes [140] and Gregory [141] 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 indispensable ingredients for uncertainty estimates, although they may be useful. Though there are already many applications of Bayesian principle to atmospheric science, the capacity of Bayesian for linking between the statistical errors and physical processes has not much been explored (*cf.* [179,180]). See also **Q3.4.3** 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 [3]. 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.* Section 2.3.1). 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**, Section 2.3.1, and [101] 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 Section 2.3.1: never go backwards. As already suggested in Section 2.4.1, 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 Section 2.3.1, 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 (EDMF) is first introduced by Soares *et al.* [54], and an SCA procedure can derive such a formulation in a more self–consistent manner (*cf.* **T1.5**).

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 separately (*cf.* **T1.5**). 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 Section 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 point. 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 [181], but the response is potentially very large [182], 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., [157].

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 [183]. 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 [184]. For this reason, the size of droplets that grow in the updraft is taken instead of the height above cloud base as in [157] in order to determine the convective precipitation rate. This implementation has been tested in the ECHAM general circulation model by Mewes [185]. A large effect is found probably because of a missing wet scavenging in this model version. The actual convective invigoration hypothesis [182] cannot be tested in a convection parameterization, though, until freezing and ice microphysics are implemented.

2.4.3. 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 [186]. 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 [187–189]. 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 [190], and for elucidating the details of microphysical processes [191]. 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 [192].

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 [20], 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.

3. Conclusions: 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 (Section B.2, *cf.* Section 2.1.1, **T1.1**). MoU also provides a false anticipation (the 4th secondary objective: *cf.* Section 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 openly admit our misjudgment. 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 Sections 1.3, 1.4, 2.4.1, **Q3.4.3** 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 modeling 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.* [49]). 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 Sections 1.3 and 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 [193] 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 in 1970 [6] 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 the actual parameterization concerned, 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 [194] 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. Here, we see a strong need for a shift from the process studies to the formulation studies in order to tackle with the convection parameterization problem in a more direct manner. The present report has suggested in various places how such formulation studies are possible more specifically.

As the present Action has identified, the parameterization problem (as for any other scientific problems) is fundamentally even *ontological* [4]. The present situation is like blind people touching different parts of an elephant (the trunk, a leg, the nose, ...), and arguing harshly over the 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 modeling. 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.

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Author Contributions

The lead author took a responsibility of putting all the contributions together into a single coherent text. He also took a final responsibility of edit. The other authors contributed text segments of varying lengths, read the texts at various drafting stages, and contributed comments, critics, modifications of the text, as well for editing. DM's contribution is limited to Section 2.3.

Conflicts of Interest

The authors declare no conflicts of interest.

Appendix: Derivation of the Result for T3.2

In this Appendix, we outline the results verbally stated in **T3.2**. For the basics of the mass–flux formulation, readers are advised to refer to, for example, [12]. For simplicity, we take a bulk case that the subgrid processes only consist of environment and convection with the subscripts e and c, respectively. Thus, any physical variable, φ, at at any given gird point is decomposed as

$$\varphi = \sigma_e \varphi_e + \sigma_c \varphi_c \quad (A1)$$

where σ_e and σ_c are the fractional areas occupied by environment and convection, respectively. Clearly,

$$\sigma_e + \sigma_c = 1 \quad (A2)$$

With the help of Equation (A2), Equation (A1) may be re-written as

$$\varphi = \varphi_e + \sigma_c(\varphi_c - \varphi_e) \quad (A3)$$

Under the standard mass–flux formulation (*cf.* Section 7 in [12], the asymptotic limit $\sigma_c \to 0$ is taken. As a result, the grid–point value may be approximated by the environmental value in this limit

$$\varphi \to \varphi_e \quad (A4)$$

as seen by referring to Equation (A3).

The goal of this Appendix is to infer the corrections due to finiteness of σ_c. For this purpose, we expand physical variables by σ_c. For example, the grid–point value is expanded as

$$\varphi = \varphi^{(0)} + \sigma_c \varphi^{(1)} + \cdots \quad (A5)$$

where we find

$$\varphi^{(0)} = \varphi_e \quad \text{(A6)}$$

$$\varphi^{(1)} = \varphi_e^{(0)} - \varphi_e \quad \text{(A7)}$$

On the other hand, we find that the environmental value, φ_e, can be most conveniently integrated directly in time without expanding in σ_c by the equation:

$$\sigma_e \frac{\partial}{\partial t}\varphi_e + \frac{D}{\rho}(\varphi_e - \varphi_c) + \sigma_e w_e \frac{\partial}{\partial z}\varphi_e + \sigma_e \mathbf{u}_e \cdot \bar{\nabla}\varphi_e = \sigma_e F_e \quad (A8)$$

which is given as Equation (6.7) in [12]. Here, w_e and $\mathbf{u}_e$ are the environmental vertical and horizontal velocities, and F is the forcing term for a given physical variable. The time integration of Equation (A8) is essentially equivalent to that for the grid–point equation under the standard approximation (*cf.* Equation (7.8) in [12]), but with the grid–point value replaced by the environmental value.

The equivalent equation for the convective component is given by Equation (6.4) of Yano [12]:

$$\sigma_c \frac{\partial}{\partial t}\varphi_c + \frac{E}{\rho}(\varphi_c - \varphi_e) + \sigma_c w_c \frac{\partial}{\partial z}\varphi_c + \sigma_c \mathbf{u}_c \cdot \bar{\nabla}\varphi_c = \sigma_c F_c \quad (A9)$$

The basic idea here is, instead of solving the above prognostic equation directly, to solve the problem diagnostically by performing an expansion in σ_c. Here, we expect that the forcing, $\sigma_c F_c$, in convective scale is a leading–order quantity, thus we expand this term as

$$\sigma_c F_c = F_c^{(0)} + \sigma_c F_c^{(1)} + \cdots \quad (A10)$$

To leading order of expansion, we essentially recover an expression for the standard mass–flux formulation already given by Equation (8) but with forcing:

$$M \frac{\partial \varphi_c^{(0)}}{\partial z} = -E(\varphi_c^{(0)} - \varphi_e) + \rho F_c^{(0)} \quad (A11)$$

This equation can be solved by vertically integrating the right hand side.

To the order σ_c, we obtain a correction equation for the above as

$$M \frac{\partial \varphi_c^{(1)}}{\partial z} + E\varphi_c^{(1)} - \rho F_c^{(1)} = -\frac{\partial}{\partial t}\varphi_c^{(0)} \quad (A12)$$

All the terms gathered in the left hand side is equivalent to the leading order problem (A11) except for that is for $\varphi_c^{(1)}$. Furthermore, they are modified by a temporal tendency, $\partial\varphi_c^{(0)}/\partial t$, already known from the leading order, given in the right hand side. It immediately transpires that this diagnostic procedure is nothing other than performing a time integral of the whole solution under a special "implicit" formula, thus the statement in **T3.2** follows.

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