Geosci. Model Dev. Discuss., 4, C1799–C1808, 2012
www.geosci-model-dev-discuss.net/4/C1799/2012/

Geoscientific
Model Development
Discussions

Interactive comment on "Mass-flux subgrid-scale parameterization in analogy with multi-component flows: a formulation towards scale independence" *by* J.-I. Yano

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Received and published: 13 May 2012

Reply to the Anonymous Referee #2

I much appreciate the anonymous referee's extensive critical comments. I presume the referee's strong reservations on my paper echos those from the other silent readers of GMDD. Thus I have taken all these reservations into serious considerations.

In my own reading, the referee's major misgiving to the present article stems from the referee's background as an operational modeller. Clearly from this perspective, the present paper does not have much to offer: what is presented in this article is not readily codable nor implmentable directly into a model and running it. However, the

referee has to realize that in order to reach this stage, we have to go through various different phases of research. Probably, the original manuscript was not clear on this point, either.

The present paper intends to offer such a theoretical formulation to the parameterization problem. What presented here are not "chicken nuggets": I am sorry to say that there is nothing ready to bite without cooking. I rather expect that the readers to develop their own schemes based on their own specific needs by taking a general formulation presented here.

In this very respect, it must be emphasized that the present paper offers a completely new starting point for subgrid-scale physical parameterizations. Such an effort in fresh is urgently needed, I believe, especially under our current situation of facing the "gray zone" problem. If this problem is to be taken seriously, it is clear that a drastic modification of the current parameterization is required.

The present paper proposes a general strategy that leads to a single unified formulation for various subgrid–scale processes for this purpose. If this proposal is to be taken seriously, this must not be an effort of a single researcher, but it must turn into an effort of a whole community. That is the main reason for presenting a new formulation in this style.

It must be understood that before a parameterization can become operational, it must take various research phases. Evolution of the mass–flux parameterization is probably the best example to make this point. Ooyama's original article (1971) on mass–flux parameterization can be best considered a sketch. Arakawa and Schubert (1974) is supposed to provide a full formulation for the problem, but we also have to realize that it took another decade and publication of another four parts, before this scheme becomes operational. It is fair to say that the present paper merely marks the state of Ooyama (1971).

Main criticisms:

(1) The present manuscript *does* make comments and suggestions in Sect. 4.3 on the key parameters of entrainment and detrainment that are introduced. This original Sect. 4.3 concludes with a very clear proposal: "The best available approach to answer all these questions would be to employ cloud-resolving modelling and large-eddy simulations in a systematic manner. A standard methodology already exists for estimating the entrainment and detrainment rates (Siebesma and Cuijpers 1995), which can easily be generalized for a matrix formulation."

In revision, this original subsection is completely re–written as Sect. 4.4 for a better clarity.

Here, unfortunately, the referee makes a major misunderstanding on the present paper: it *does not* intend to "improve" any existing parameterization schemes, but proposes a completely new type of parameterization schemes. This point is explicitly stated in the revised Introduction.

(2) The proposed system, essentially, consists of a set of primitive equation systems, each describes the time evolution of each subgrid-scale component. Note that each subgrid-scale component is advected by their own large-scale flows, u_j. This is a natural consequence under an absence of an approximation, $\sigma_j \ll 1$. As a result, there is no longer such a thing as an "environment" that can be approximately equated with a grid-box average. As a further consequence, there is no longer such a thing as a large–scale model equation independent of a subgrid–scale parameterization. Rather, with an absence of environment, each subgrid-scale component must be evaluated in its own way almost like a large-scale variable by itself. That is the key feature of the present formulation. This key feature is best understood in analogy with the multi–component flow problem.

Unfortunately, this key point has not been well emphasized in the original manuscript, leading to various misgivings of the present referee. An extensive elaboration is made in the revised text, especially in the introduction as well as in Sect. 4.1. More specifi-

cally, whenever the subgrid–scale component flow is referred in the text, the adjective "large-scale" is added systematically in order to make it unambiguously clear that it refers to a large-scale flow.

(3) A unified approach for the subgrid-scale physical parameterization in terms of the mode decomposition is already presented in Yano et al. (2005a). I sincerely request the present referee to read this reference carefully before making further comments on this issue. Under this unified approach, the mass-flux parameterization can be re-interpreted as a formulation under the segmentally-constant mode decomposition. Yano et al. (2010) further elaborate and generalize of the mass–flux parameterization under SCA. Those background is more carefully and extensively discussed in the revised introduction.

The mode decomposition provides a basis for subgrid-scale physical representations, because the method can in great deal "compress" a full physical system when an appropriate set of decomposition modes is chosen. SCA loosely corresponds to when the Haar wavelet is chosen as a set of decomposition modes. The efficiency of this choice, along with the other choices of wavelets, is well demonstrated in Yano et al. (2004b). See their Figs. 5 and 8 as graphical demonstrations for the efficiency of SCA.

The present paper correctly emphasizes that implementation of this proposal would be a major exercise. Unfortunately, in the original text, I chose an expression "need for development". That was a mistake. It is not simple a matter of technological development at all, as suggested in the original text, but a full-scale research is required before this scheme becomes implementable. This point is better emphasized in the revised Sect. 5. Most importantly, a systematic CRM/LES analysis outlined in the original Sect. 4.3 (renumbered to Sect. 4.4 in revision) would be crucial before any development work can begin, especially, in order to define a solid formulation for the entrainment–detrainment rate. This point is also re–iterated in the final section.

The decision would fully depend on how critical and how much seriously we should consider the self–consistency of the physical parameterizations. In my very personal opinion, for example, the cloud scheme can be constructed in a more consistent manner under the present formulation, as argued in Sect. 4.8. Furthermore, we are facing with the issues of subgrid–scale physical parameterizations under "gray zone" where the approximation, $\sigma_j \ll 1$ is no longer valid. The present paper simply presents, though partial, a formal answer to this question, and urges to consider this need seriously, if the consistency of the subgrid–scale parameterizations as well as the "gray zone" problem are to be considered seriously at operational research centres.

General Comments:

Introduction:

Yes, from my own personal background, I always inevitably have tropical convective flows in mind. Nevertheless, it is emphasized that the present formulation is given in such manner that it can also be applied to the mid-latitude baroclinic systems. This point will be clear if the basic principle adopted in the present formulation as outlined in responding to the item (3) of the main criticisms is well understood. The point is better emphasized in the revised text: this generality is guaranteed by a generality of the mode decomposition approach on which also this SCA formulation is also based on.

Section 2:

Yes, Eq. (2.1) *does* mix up the two issues: only the first inequality is valid in this equation. The second inequality is not at all a requirement. I explicitly state this point in the revised text. The equation is also corrected.

The main point I should have made more clearly here is that a confusion associated with the common terminology "grid-box mean". The mean should be, in fact, taken against a typical large-scale, ΔX, rather than the grid-box size, L. This remark is also added in the revised text.

Nevertheless, the ensuing discussion rather decides to follow this traditional notion, just for a sake of a comfortableness. In order to maintain it, the second inequality in the original Eq. (2.1) must rather be artificially recovered, as presented as a separate unnumbered equation in the revised text. Such a grid–box size, L, may better be considered a virtual one as explicitly stated in the revised text.

What happens when the small fractional area approximation is removed from the Arakawa ans Schubert's formulation is exactly the key question addressed in the present paper: no, this is *not at all* trivial how the mass flux approximation applies under this situation as the referee correctly points out. That is exactly why this paper is written. So please read carefully, if you are interested with this answer.

Recall the generality of the notion of SCA. As long as a boundary layer process (such as a well-defined cold pool) can be represented under SCA, the present formulation equally applies to this boundary layer process.

The "digression" to the multi-component flow is absolutely a key here: the proposed parameterization formulation is perfectly in analogy with the multi-component flow system under the primitive equation, as already emphasized in responding to the item (2) of the main criticisms. Especially, the subcomponent horizontal velocity, u_j, must be understood exactly as that for a component in a multi-component flow. A more emphasis is placed on this point throughout the revised text in order to make it almost impossible to escape from the importance of this analogy.

(2.2): of course, the choice of the parameters such as entrainment and detrainment rates must reflect the non-hydrostatic nature of the subgrid-scale physical processes. This point is explicitly stated in the revised text.

(3.1.1): No, the original manuscript has totally failed to discuss the issue how the proposed scheme determines which subcomponents are in contact each other. A subsection is inserted as a new Sect. 4.3 in order to discuss this issue exclusively.

(3.1.3): Yes, the upstream approximation is purely a numerical approximation and not at all a fundamental aspect of the present formulation. This point is clearly stated immediately after Eq. (3.6) in the revised text. Here, we simply follow a traditional approximation adopted in the standard mass-flux formulation. However, I should also emphasize an advantage of this choice guaranteeing a numerical stability.

Eq. (3.14): ϕ is the geopotential as introduced by Eq. (2.2) above. A short reminder is added in the revised text immediately after Eq. (3.13).

(3.4): I would like to thank to the present referee for pointing me out a mistake in the original derivation. A key for a more consistent derivation is, as it turns out, to re–write Eq. (3.18) into a more explicitly Galilean invariant manner, as presented by Eq. (3.19) of the revised text. It is clear that the revised pair equation for the mass continuity (Eqs. 3.21 and 3.22) is Galilean invariant.

(4.2): Allusion to the data assimilation in the original text was ambiguous, thus is removed in the revised text.

Note that $\bar{\nabla} \cdot (\sigma_j u_j)$ is not the subgrid-scale divergence, but that of the large scale. Note a bar added to the nabla operator. The meaning of the bar on the nabla operator is explicitly stated in the revised text.

The forcing term, F, includes the two major processes: 1) non-conserved processes such as diabatic heating, phase change, chemical reactions, and 2) the eddy transport that is not going to be considered under the mass–flux approach. Especially, most of the surface processes are considered as a part of the forcing term, F, introduced in Eq. (2.4). In the revised text, it is explicitly stated that this term also includes the surface processes (unless a process is represented as one of the subgrid–scale component). The remarks are added immediately after Eq. (2.4) as well to discussions on the eddy contributions in Sect. 3.1.

(4.3): Yes, I frankly admit that the definition of the entrainment-detrainment parame-

ters is the major weakness of the present approach. However, I also emphasize, this difficulty is in the same sense as it is already a major weakness of the current parameterizations, as reviewed by de Rooy et al. (2012). As this review clearly suggests, currently there is no agreed general principle for defining the entrainment-detrainment rate even under the current conventional framework. The review strongly advocates for a need for performing massive CRM/LES analysis for obtaining the better estimates numerically. In this vein, we should realize that we do not loose much by moving to a new formulation framework, thanks to a lack of guiding principle. Adoption of a new framework could be more beneficial, if it is more realistic.

For example, the current mass-flux formulation only lets the updrafts and the downdrafts interact with the environment. However, the updrafts and the downdrafts do not interact each other in terms of the entrainment-detrainment processes. As presented herein, such a generalization is straightforward, and all we need is a good estimate of parameters by CRM/LES analysis.

All these points are more clearly stated in the revised text by extensive rewordings.

I also emphasize that the manuscript explicitly points out a possibility for a methodology going beyond the entrainment–detrainment formulation as remarked in the original Sect. 3.1.1. The point is further emphasized in the conclusion section in the revised text.

(4.4): Here, the referee clearly raises an open question that cannot be answered in the present manuscript: yes, if you refuse to accept a very small, but finite fractional area as “unphysical”, you have to introduce a triggering condition. However, if we insist on such a very small fractional area (albeit unphysical may be) to be maintained within each grid box, the issue of the triggering can be avoided. Here, however, I emphasize that there is no clear principle for introducing “trigger”. In this respect, keeping all the subgrid–scale component with a very small, but nonvanishing fraction is probably a good idea.

Note that a similar issue is encountered with the UM cloud scheme, PC2. Under the current PC2, the cloud fraction occasionally turns into zero, as a result, it suffers from an ill-posed "triggering" problem (Cyril Moncrette, personal communication, May 2011).

It may also be important to note that there is no need to set $\sigma_j = 0$ in order to "deactivate" a j-th subcomponent under the present formulation, but the subcomponent may simply become "inactive" even with $\sigma_j > 0$. For example, a large–scale descent, explicitly included as the second term in Eq. (3.12), may eventually "dry out" a stratiform cloud without setting $\sigma_j = 0$ in the scheme.

(4.5): I *do not* say deep convection can be excluded from parameterizations. I simply say that it may be a better idea to *retain* a standard mass-flux parameterization, rather than taking a new approach proposed in the present paper based on a nonhydrostatic limit. The discussion is expanded for a better clarity in revision.

(4.9): I absolutely agree with the present referee that "the problem of resolution dependence in current parameterizations arises because there is no clear definition of what the scale of the large scale flow represented by the model". That is the issue that I intend to point out in the discussions of Sect. 2.1. For this reason, the remark is explicitly added in the revised text.

I also absolutely agree with the referee that under the "gray zone", the nonhydrostatic nature of the system becomes crucial. This point was already clearly stated in the last paragraph of the original Sect. 4.10.

Nevertheless, I also emphasize that the present formulation already includes the two major ingredients crucial under the "gray zone": a fully prognostic formulation and an explicit inclusion of the lateral communication between the grid columns. As further emphasized in the revised text, for this very reason, the present formulation can be considered a good initial operational implementation in order to *seriously* tackle with the "gray zone" problem. As also emphasized in the text, though an equivalent formulation can be developed for a full nonhydrostatic system, it will be much involved, and it is

very likely to be condemned by the operational modelling community as "impractical".

References:

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Interactive comment on Geosci. Model Dev. Discuss., 4, 3127, 2011.