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Comment

## ***Interactive comment on* “Evaluation of a new convective cloud field model: precipitation over the maritime continent” by H.-F. Graf and J. Yang**

**J. Yano (Referee)**

yano@cnrm.meteo.fr

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Comments on "Evaluation of a new convective cloud field model: precipitation over the maritime continent by H.-F. Graf and J. Yang" by Jun-Ichi Yano CNRM, Meteo-France, Toulouse yano@cnrm.meteo.fr

Evaluation:

This is a sequel of their work on CCFM (convective cloud field model). I did not find any serious defects in their manuscript, thus I recommend its publication. The description is overall fine, and the paper can be published almost no change to the text as far as it is published side-by-side with my own comments, as a basic policy of ACP, so that the readers can evaluate the value of this work more objectively by comparing it with my

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second opinions.

#### General Remarks:

With a lack of a well-defined characteristic time-scale for convective adjustment (Yano et al., 2000), the convective quasi-equilibrium hypothesis (cf., Arakawa and Schubert 1974: hereinafter AS) appears to be not well established from observational evidences. Existence of a  $1/f$ -noise spectrum in tropical convective variability (Yano et al., 2001) further indicates that tropical convection is at self-organized criticality rather than at quasi-equilibrium as traditionally believed (see also Yano et al., 2004).

The present series of papers may be considered as a first attempt to construct a convective parameterization under the principle of "self-organization". For this reason, I would like to much welcome their efforts.

On the other hand, I have wide ranges of reservations, but more from philosophical, conceptual, and methodological perspectives. Unfortunately, I did not have a chance to comment on their first paper (Nober and Graf 2005, hereinafter NG05). Thus my criticism are also naturally directed to the latter.

The present parameterization suffers from various arbitrary choices, which could be avoided by more careful considerations, as indicated more specifically below. Nevertheless, the degree of arbitrariness does not exceed standards for current parameterizations, thus I do not think that this constitutes a reason for a rejection.

The present paper, a sequel to NG05, attempts further verifications of their parameterization (CCFM). Here, I'm afraid, the work suffers from the same problem as most of the current verifications of the parameterizations. My main comments are focused on this aspect. Nevertheless, their verifications methods are not any more defected in current standards. Again, for this reason, I do not think that this constitutes a reason for rejecting the present paper.

Misleading interpretations on the backgrounds of the current cumulus parameterization

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problem:

Problems found in their approach appear to be stemming from their misunderstandings on the current status of the cumulus parameterizations.

As indicated by the present authors, a majority of the current convective parameterizations is based on the mass flux formulation. However, they rather arbitrarily separate them into those "determine the overall mass flux of all cumulus clouds in one AGCM grid column" (is that simply a paraphrase for mass flux schemes?) and those "based on cloud models". Note that in NG05, it is more clearly indicated that the latter is a different category than the mass flux approach. In reality, all the mass flux formulation uses a certain "cloud model" in order to define a vertical profile for the mass flux. For this reason, these remarks are rather misleading.

I believe, a more important categorization is those based on a bulk mass flux and on a spectrum of mass fluxes. Their remark that "One problem with current mass flux schemes is that they describe the variety of convective cloud by an effective mean convective cloud...." only applies to the first category.

Furthermore, in NG05, they propose their parameterization as a one based on the idea of "self-organization", but at the same time, they also state that the model follows the idea of "quasi-equilibrium". This is an obvious contradiction, because the "self-organization" and the "equilibrium" are two extreme limits for the possible thermodynamic states (see references in Yano et al., 2001, 2004).

Formulational problems for CCFM:

The most unfortunate aspect of the original publication (NG05) is that it is coincidental with a publication of Yano et al. (2005a: hereinafter YRGB), which outlines a general systematic methodology for deducing a full convective system into a parameterization. Unfortunately, the formulation of CCFM is based on an arbitrary combination of arbitrary chosen components without taking such a systematic approach.

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Here, the three arbitrary chosen components are: Weinstein and MacCready's plume model, a CAPE budget, and Lotka-Volterra's population-dynamics equation. These choices are probably not the best ones, and these three are not well logically combined neither.

No doubt, a certain "cloud model" must be introduced in order to construct a cumulus parametrisation, but it is reasonable to ask why they have chosen a plume model by Weinstein and MacCready for this purpose, and not a one by Joanne Simpson and co-workers (e.g., Simpson et al., 1965; Simpson and Wiggert 1969), for example. No remark is made in NG05. There is a substantial difference in their treatments of the equation for the vertical velocity. The main problem is that these equations are totally phenomenological based on the "small-scale" laboratory experiments (most of them without density stratification), that is naively applied to atmospheric plumes. A more specific problem is the neglect of the pressure term (see Holton 1973), which is hardly justifiable.

A more fundamental problem is an assumption of a steady state for the convective system. That means that a vertical profile for each cloud type is evaluated afresh at every time step by a vertical integration, totally dismissing its previous state. In other words, each cloud type completely loses its identity after upgrading its state at every time step of integration. This is an important point to keep in mind considering the consistence with the other components.

The second choice is the CAPE budget. Here, I much appreciate that they frankly admit the arbitrariness of this choice in NG05. However, the main problem is that they do not pursue to construct a CAPE budget equation systematically to the end. Instead, they only estimate some selective coefficients for the CAPE budget, then they decide to arbitrarily substitute these coefficients into the Lotka-Volterra equation.

A closed CAPE budget equation is, in fact, carefully derived in the Appendix B of AS. Due to a different cloud model and definition for CAPE adopted in the present study,

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the final formulas would not be identical, but nevertheless, it would reduce to a form comparable to Eq. (142) of AS. Here, it is striking to note a similarity of their Eq. (9) in NG05 and AS's (142). It is even tempting to imagine that they would recover the same Lotka-Volterra equation by following such a more formal approach, but I strongly doubt it.

The time evolution of CAPE for a given type of cloud is essentially controlled by an evolution of vertical profiles for both the in-cloud and the environmental thermodynamic variables (cf., Roff and Yano 2002). I do not see any possibility of replacing the temporal CAPE tendency by a temporal tendency for a cloud type population, as postulated in NG05, for this reason.

In fact, the CAPE budget does not provide any additional information than those contained in the original cloud-model equations, because the former is essentially obtained by "energy integrating" the latter set of equations (Nevertheless, note that CAPE is not an energy in a strict sense: see Yano et al., 2005b). At this point, we also have to point out an implicit contradiction in their approach: they appear to assume that CAPE for each cloud type evolves prognostically, but they evaluate the vertical profiles of each cloud type only diagnostically at each time step on the other hand.

We should realize that it is extremely difficult to derive an equation (either prognostic or diagnostic) for defining the cloud type populations (or the fractional area occupied by each cloud type, in a more proper mass flux formulation terminology). The main problem (see YRGB for more details) stems from the fact that the segmentally-constant mode decomposition, on which both the classical plume model and the mass flux formulation are based, does not constitute an orthogonal complete set as more standard mode decompositions, as Fourier. As a result, it is only possible to define a spatial distribution of segmentally-constant modes initially, but it is difficult to activate and deactivate the modes in an obvious manner, corresponding to the evolution of cloud populations in their terminology.

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These various awkward inconsistencies in the formulation can, in fact, be easily avoided by constructing a plume dynamic model ("cloud model") from a first principle, more precisely, from a full anelastic system. Such a systematic methodology is outlined in YRGB, and more concrete derivations for thermodynamic variables are given in AS (see their Eq. 43-50). The corresponding equation for the vertical velocity can also be derived by the same principle (cf., Eq. 4.1 of YRGB, see also Holton 1973). I believe, they can reconstruct their population dynamics model more logically from this basic set of prognostic equations, by making the introduced hypotheses behind more explicit.

However, my own recommendation is to directly integrate this set of equations prognostically by assuming a certain spectrum of clouds initially. Then a population of clouds would evolve spontaneously without additional assumptions. This would also be conceptually closer to the principle of "self organization", in which the individual subgrid-scale elements evolve far more spontaneously than at an equilibrium state.

I should also point out that both the traditional plume model as well as the prognostic system (4.1 of YRGB) introduced here still exclude a direct interaction between the clouds, but only through the environment (see Fig. 11 of AS, which elucidates the principle of NG05 more vividly than their own Fig. 4), due to the auxiliary conditions posed (cf., Sec. 4.b, YRGB). This is a possible generalization that the authors may also wish to consider, because at the self organization, nonlinear interactions between the subgrid elements are expected to become critical.

Problems with verification strategies:

I believe, the so-called "model verification", which refers to a process of verifying a model as a whole, on the one hand, and the verification of a parameterization on the other hand, should be clearly distinguished. The goal of the former is to diagnose the model performance as a whole in comparisons with various observations. On the other hand, the goal of the latter is, legitimately speaking, to verify the validity of each

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element in a parameterization. Thus, it must be somehow more detailed than simple diagnoses. However, in practice, these two are rarely distinguished. The same is also the case in the present work.

In NG05, essentially global monthly-mean precipitation fields are compared with observations. In turn, in the present paper, the focus is shifted to regional modellings, but the main verification variable still remains the monthly-averaged precipitation field. Certainly, this is a field of practical interests, but it measures the performance of convective parameterizations only in an indirect manner. By taking only monthly means, an improvement of predictability of the model is hardly evaluated.

Moreover, why they decided to shift from a global model to a regional model, instead of performing further details analyses over a global domain? For example, improvement on a representation of Madden-Julian oscillations would be a key question to address.

Here, I would also like to question the validity of CCFM in applying to a regional model with a finer resolution. Recall that CCFM is based on a population dynamics. Thus, a basic assumption behind is that a population of convective clouds always exist within a grid box. In other words, the grid size of the model must be large enough in order to accommodate enough number of convective clouds. For this reason, applying CCFM to a fine-resolution regional model is obviously not a best idea.

Obviously, the authors have faced certain difficulties, and they have decided to limit the number of cloud types within the scheme for the present purpose (cf., p. 4). However, no comment is made about a possible need to control a number of clouds for a given type in this set of simulations.

Recall that the standard procedure for testing a convective parameterization is to implement it into a single column model before it is tested in a full global model. In spite of various shortcomings associated with this approach, it allows to examine the detailed performance of the parametrisation more closely than otherwise. Strangely enough, this key step is totally skipped in their testings.

As a whole, the performance of CCFM is, albeit showing some trends for improvements, not at all dramatic, especially considering its quite elaborated formulation. In this respect, their statement in the abstract that "the use of CCFM clearly improves the simulated precipitation patterns...." is clearly an overstatement. They should examine the details of its performance in order to better understand lack of performances.

Technical corrections:

p. 2, 2nd paragraph, beginning (l. 20-): I believe, the authors are talking about the 'bulk mass flux' scheme rather than the mass flux schemes in general.

p. 2, l. 32-33: It is misleading to quote Naveau and Moncrieff (2001) here. This paper does not provide any concrete formulation for convective parameterizations, but they simply perform a statistical analysis on vertical velocity distributions in convective systems, and "propose" a certain "possibility".

Post Script:

Page numbers and the line numbers for my comments are based on those for the original submitted manuscript, because local printers do not allow me to print an electric file provided from the editor. The local system manager believes that a provided electric file is defected, whereas an editorial assistance insisted that the file had no defect.

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