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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Phenomenology of convection-parameterization closure" *by* J.-I. Yano et al.

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Reply to the Referee #2:

We are pleased with great appreciations on our manuscript by the present referee. We also well understand various strong reservations by the present referee on our manuscript. In finalizing our manuscript, we will pay our best attentions to all the aspects raised by the present referee, in a manner detailed below.

However, we also have to note that some of the referee's remarks are originating from misunderstandings. In the following, we carefully explain why we believe these are to be misunderstandings, and also try to explain why the issues in concern led to misunderstandings. In all these cases, we find that misunderstandings stem from our

own misleading statements of the original text. Thus, we provide specific promises to revise those statements below in order to avoid the further misunderstandings.

Note that our following response is almost literally line–by–line to the present referee's comments in the given order.

• In revision, we will do our best to remove all the "mixings" of the issues in order to avoid any confusions by following the referee's specific comments below. Some missing "key aspects" suggested by the referee below will be elaborated as also detailed below.

• Some "relevant studies" will be quoted whenever possible in order to strengthen the arguments. We expect that more than twenty new references will be added in the final manuscript, which is currently under preparation.

• Illustrations: after careful re–reading, we do not find any critical figures to be reproduced in the present review. [Unfortunately, the present referee did not specify any figures to be reproduced]

It is the first author's strong opinion that a scientific review should not be simply consisting of reproductions of the existing results. Rather, any review must clearly set its own view in order to synthesizing all the existing results. From this very point of view, reproductions of figures from the original articles tend to be rather obstructing by interrupting a presentation of a bigger picture that a review tries to present.

We believe that readers should refer to the original articles in order to see these supporting figures, if necessary. For this very purpose, the revised manuscript will carefully list key figures to be referred in association with almost every key reference of the present review [though, unfortunately, more than often there is no single key figure, but you have to read an entire article carefully].

Though ironical, we strongly believe that such an insistent avoidance of reproduction

of secondary figures makes a given review rather more self–contained than otherwise. These secondary figures could, for example, bring up further details that the review would not wish to deal with.

We will also make our best efforts to make our every statement as lucid as possible in revision so that a reader can proceed without looking for the associated figures for a statement.

• The manuscript will be “profoundly reorganized" in its structure as suggested by the present referee. More specifically,

1) The original Sect. 5 will be moved immediately after Sect. 2.1. As a result, the original Sects. 2.1 and 5 will form a new single section 2.

2) As a result of the modification above, the original Sects. 2.2 and 2.3 will form another new section 3. As a further consequence, the original Sects. 3 and 4 are renumbered as Sects. 4 and 5, respectively.

3) A new subsection will be created immediately after the original Sect. 4.2 (to be renumbered to Sect. 5.2), where all the discussions on debates between the moisture and CAPE closures will be gathered.

4) A new subsection discussing the role of vertical wind shear will be added to the end of Sect. 4 (to be re–numbered to Sect. 5).

• Some points to be addressed from the beginning:

1) Definition of convection in observations: this will be clearly stated in the beginning of the subsection discussing the observational data analyses, rather than at the end of the subsection, in revision.

2) Definition of “trigger" (along with suppression) will be given in the revised text as

follows: “Here, triggering and suppression, respectively, refer to technical conditions for turning on and off a given convection parameterization."

However, we also emphasize that the present review does not at all deal with “trigger" as a separate issue from “closure". In order to explain this point, a following paragraph will be added immediately after line 13, page 25745 of the introduction in revision: “The present review essentially hovers round this key question. In course of examining this question, the paper considers the issues of both onset and intensity of convection as a single set of question, rather than as two separate ones, because a process or a physical variable that defines onset of convection would also naturally contributes in defining the intensity of convection. For example, if a positive CAPE (convective available potential energy, cf. Sect. 3.1, 2) defines onset of convection, its magnitude would equally contribute in defining the convective intensity. By taking this perspective, the present review does not consider the issue of trigger (as well as suppression) as a separate issue from a general closure problem, either. In spite of critical importance for defining trigger and suppression in operational implementations, we believe that this restriction is legitimate in the present review by focusing on *phenomenological* aspects of the closure problem. In considering the problem from those perspectives, as it turns out, it is still hard to answer a simpler version of question: what controls convection? A major exception is Sect. 5.5, where the role of CIN (convective inhibition, cf. Sect. 3.1, 2c) in triggering individual convective plumes is discussed. However, the issue of trigger therein is nothing to do with the trigger in standard mass-flux parameterization, as the discussion in this subsection makes it clear. " [In order to make this last point absolutely clear, an explicit statement will also be added at an appropriate place in Sect. 4.4 (to be renumbered to Sect. 5.5 in revision)]

The word “trigger" have appeared in several places in the original manuscript, but mostly for misleading reasons. All those misleading uses of “trigger" are systematically removed in revision by taking a different word or expression. A major exception is the section on PBL-based closure, as specifically stated in the revised introduction as

quoted just above.

• Figures: see our response above concerning the illustrations

• In order to reduce the repetitions, we will remove the following paragraphs: 1) last paragraph of Sect. 2.1 (lines 3–10, page 25749), 2) lines 9–17, page 25771

1. introduction:

The structure is re-organized as requested. Definition of trigger is given with extended discussions (see the item of “trigger" above for more details).

2. observational perspectives:

The original Sect. 5 will be placed immediately after Sect. 2.1 as suggested. The last paragraph of Sect. 2.1 is removed as a consequence. On the other hand, we believe that Sect. 5 (renumbered to Sect. 2.2) serves as important previews for many issues to be discussed in the subsequent part of the paper. For this reason, these repetitions will mostly be retained in revision. Nevertheless, note that a paragraph for lines 9–17, page 25771 will be removed, for example.

• The present review does not make distinction between physical observational processes and hypotheses. The latter must equally correspond to physical observable processes if they are valid notions. Otherwise, they would be meaningless to consider. Also note that all the variables listed in Sect. 2.1 (to be Sect. 3.1 after revision) are physical (nothing “hypothetical") in the sense that they can be evaluated from observations so long as necessary basic variables are measured in proper manner (which is often difficult though). The following paragraph is added for explanation: “Note that all those variables can be evaluated from observations if necessary basic variables are properly measured. In this very respect, all of them reflect certain corresponding physical processes, though some of them are more often employed in data analysis, and others are more often employed in modelling contexts, mostly due to historical

as well as practical reasons. For this reason, the following list is constructed without discriminating between those two major categories."

In revision, short remarks are added to all the physical variables without discrimination so that a reason for listing can always be understood.

Furthermore, in the end of Sect. 2.2 (to be renumbered to Sect. 3.1), we will add a sentence “However, potential importance of the other variables for closure can hardly be excluded *a priori*." immediately after the original last sentence saying “Among those, CAPE and water vapor (moisture) are the two most commonly adopted variables for closures." in order to make it absolutely clear that we should not discriminate between those variables actually used in closures and those not currently used.

• definition of convection in observed studies:

Definition of convection in observed studies was discussed in the last two paragraphs of the original Sect. 2.3 (to be Sect. 3.2 after revision). In order to make this discussion better stand out, these two paragraphs are moved immediately after the leading paragraph of the subsection.

Furthermore in order to answer types of convection considered in these studies, the following paragraph is inserted in a middle: “Even over the tropics, it is clear that either satellite-measured infrared brightness temperature or precipitation rate is a very crude measure of convection. Both variables do not distinguish contributions from either convective core or anvils. All the following analyses do not distinguish even convection is propagating or not."

The following short paragraph is added in the end of the subsection, in order to make the intention in context of the closure problem clear enough: “Overall, those studies may be criticized to be too crude to be useful for defining parameterization closure. However, it is important to recognize that even those simple analyses face difficulties in relating convection–controlling variables listed in Sect. 3.1 with an observational

convection measure."

• An introduction to the closure versus the trigger issues: please refer to our response to the “trigger" issue above.

3. perspectives from the tropical large-scale dynamics:

We agree with the present referee that this section would be rather difficult to follow in a self–contained manner, because its discussion relies on results from quite different domain: convectively–coupled equatorial waves. Probably writing a self–contained description on this subject would be a matter of its own stand–alone review article, clearly beyond a capacity of the present manuscript. [Furthermore these details would clearly obstruct the main threads of the present review in the same manner as addition of many secondary figures would do: see our response to the “illustrations" above]

Nevertheless, in order to enhance readability of this section, extensive references (many of them are newly added) will be more systematically added after every key statement of the section so that interested readers can examine a right paper in order to better understand a specific point of discussions.

• Yano and Bonazzola?: there is no particular theory that is referring to in the sentence in concern. It will be re–written in revision in order to avoid confusion as: “As an example of such an alternative theoretical approach, in the present section, we take a particular theoretical perspective for the tropical large-scale dynamics (cf. Yano and Bonazzola 2009 as review) summarized as follows." We believe that the revised sentence would make it clear that Yano and Bonazzola (2009) is a general reference for basic dynamical regimes for the tropical large–scale dynamics, rather than referring to any particular theory. In revision, we will also add Kiladis et al. (2009) as an observational review for convectively–coupled waves.

• In order to explain the moisture mode, the following paragraph will be added in revision: “Here, a major departure from more classical studies such as those based on

wave–CISK (conditional instability of the second kind: Hayashi 1970, 1971, Lindzen 1974, see also Sect. 5.1), moisture (or any equivalent variable) is explicitly considered as a prognostic variable in addition to standard variables for the dry primitive equation system. As a result, in addition to conventional equatorial–wave modes, another mode called “moisture mode” arises. The moisture mode is, as the name suggests, typically characterized by a dominance of the moisture field relative to the temperature field (Neelin and Yu 1994, Sugiyama 2009a, b, Fuchs et al. 2012)."

• Additionally, the paragraph for lines 11–18, page 256756 will be substantially elaborated after a request of the referee #1 (for more details please refer to our response to the referee #1).

4.1. Moisture based closures:

• The word “trigger" appears in this subsection three times for misleading reasons. Especially, the last one “triggering mechanism" must be replaced by “closure condition" for an obvious reason. The first two will also be replaced by different expressions in revision.

Thus, there is no mixture of triggering and closure, but only the closure issue is discussed throughout this subsection. The original confusion is removed by totally removing the word “trigger" from this subsection.

On the other hand, we emphasize again that the present review never refers to any theory without relating it to specific physical processes. Thus physical processes and theories are clearly linked together throughout the discussions.

• The last paragraph will be moved to a newly created subsection entitled “moisture vs. CAPE closures". The last paragraph of Sect. 4.2 (to be Sect. 5.2 in revision) will also be moved to this new section.

4.3 Parcel–environment CAPE closure:

Here, again, there was a misuse of the word “trigger". The discussion of the paragraph in concern is nothing to do with “trigger" considered as a switching condition of a parameterization. The paragraph in concern will be carefully re-written in revision by removing all the references to “trigger".

4.4 PBL–based closure

• “CIN is too unreliable to be used for a closure"?: the sentence in concern is replaced by following: “ from operational point of view (P. Bechtold, personal communication, 2012) the CIN closure is not reliable being too sensitive to details of the boundary layer. It also does not respond to destabilization in free atmosphere induced by mid–level large–scale forcing (cf. Sect. 5.5)."

Here, we should emphasize that it is not ourselves but a series of papers by Mapes that mix the issues between triggering and closure. This point will be explicitly stated at the end of the paragraph for lines 25-29, page 25762 in revision. It may also be important to emphasize that we should clearly distinguish between Mapes' activation-control trigger mechanism and the trigger condition in standard convection parameterization. This remark will be added explicitly to the end of the paragraph for line 18 page 25763 - line 3 page 25764 in revision.

• The phrase “the role of CIN in convective triggering" in line 15, page 25764 will be replaced by “the role of CIN in convective dynamics" in revision in order to avoid any misunderstanding.

• Finally, the reference to ALP is added to the end of this subsection as follows: “The notion of triggering under lower CIN is more recently further elaborated by introducing the concept of ALP (available lifting potential) as a counterpart for defining the activation threshold for CIN (Rio et al. 2009, Grandpeix and Lafore 2010). The formulation assumes that deep convection is controlled by subcloud processes providing energy and power to lift and sustain convection. Boundary–layer thermals and cold

pools provide ALP, which is used to compute the cloud–base mass–flux. In addition, the introduction of cold pool parameterization allows to introduce a subgrid variability of CAPE and CIN within a model grid box, as convection does not see mean grid–box environment but only part outside cold pools (cf. Sect. 5.4). The formulation has already been implemented into a global climate model (Hourdin et al. 2012, Rio et al. 2012)."

4.5. high resolution limit

In order to discuss the previous studies, the following paragraph is added: “Though some exploratory studies have been performed for high–resolution limit (Gerard and Geleyn 2005, Gerard 2007, Kuell et al. 2007, Gerard et al. 2009), no study systematically focused on the closure issue has been yet reported. The present subsection presents theoretical reflections on this issue with some preliminary results supporting our arguments."

As stated in this paragraph, we believe it important to support the arguments whenever possible with evidences. For this reason, we do not see any reason why the preliminary results should be forbidden to be presented here, as the present referee appears to suggest.

5. difference over the globe:

This section will be moved to Sect. 2 by following the suggestions

6. conclusions

Please note that discussion about the definition of convection in observation found here is merely a re–iteration of a discussion already found in the original Sect. 2.3 (to be renumbered to Sect. 3.2). As stated above, the discussion on the definition in concern will be moved to an earlier part of the subsection in order to make it stand out better.