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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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This paper discusses various aspects related to the convection-parameterization closure issue, from observational, theoretical and modeling point of view. Many interesting points are raised and discussed. It discusses key variables controlling convection in the observations and the contrast between different regions of the globe; it also reviews variables used to control convection in parameterizations. It discusses the limitations of the most commonly used closures, namely the CAPE and moisture convergence closures, the use of CIN, and the problems raised by higher resolution. It discusses aspects from both the process-scale and the large-scale. However, all those aspects are mixed in a way that is sometimes hard to follow. Some key aspects are not discussed from the beginning but in the course of the paper which makes some discussions confusing. The personal point of view of the authors is interesting, however they should be careful to demonstrate their arguments by quoting relevant studies and

results rather than give their feeling. The paper is a review so that results from many previous studies are mentioned, but with no illustration, which makes sometimes the understanding difficult .

Thus I would recommend to profoundly reorganize the paper before considering publication.

Some points should be addressed right from the beginning to help the understanding of the rest of the paper: - The definition of convection in the observations: what are the criteria used? How does that impact the results? How does that relate to convection as simulated by convection parameterization? - The closure versus the triggering question, or the intensity versus the occurrence question. Both are sometimes mixed up in the discussions.

About the figures: This paper is a review referring to many previous studies. However, figures from those studies are only briefly described, and sometimes it is hard to imagine the corresponding figure. Why not reproducing some key figures of previous studies as usually done in reviews?

There are also many repetitions along the text, which confirm that the structure of the paper needs to be improved. Some suggestions in that direction are given below.

More specific comments:

1.Introduction

The introduction starts with a definition of the closure issue, then the outline of the paper is given and then it goes back into definitions. I would suggest to present the full necessary context first and then present the content of the paper. In the context, a clear definition of the triggering question is missing.

2.Observational perspectives

I find the discussion about the fact that different processes may drive convection in the

mid-latitudes and the Tropics very interesting. In fact, I would suggest to extent this discussion here by including the thoughts of section 5 in this section. Indeed, those differences are already partly discussed in section 2.1, and the section 5 only repeats many aspects already discussed in the course of the paper (that could then be removed to avoid repetition).

However, this part is confusing by three aspects:

- There is a mix of physical observed processes and hypotheses made in parameterizations. This is particularly true for the list presented in section 2.2, which presents both variables shown to control convection from observations and variables used in parameterizations to control it. I would suggest to first describe observations to highlight convection controlling factors from observations, and then to discuss the hypotheses made in parameterizations separately.

The paper is supposed to discuss the various variables that control convection. In that respect, I find that the statement at the end of section 2.1: “It follows that the important variables for convection are:” is premature and not demonstrated.

- There is no discussion about how convection is defined in observational studies, how it may influences the conclusions of those studies, and how it relates to convection as represented in parameterization. What about the difference between local and propagative convection? What about the definition of convection: is it only the convective cores, or also anvils and associated cirrus?

- An introduction to the closure versus the triggering issues is missing to follow the entire discussion. For example, the Zhang & Klein study is mostly about comparing situations in which deep convection occurs or not, and not about convection intensity.

3.Perspectives from the tropical large-scale dynamics

This part is really confusing to me. The argumentation is not rigorous enough, and some aspects are not well explained. For example: What is the theory proposed by

Yano et Bonazzola? What is the so-called “moisture mode”? I would suggest to clarify this section.

4.Further theoretical considerations

4.1 Moisture based closures

Again, concepts of triggering and closure are mixed in the discussion. There is also no distinction between physical processes and theories used in parameterizations.

The discussion about the limitations of CAPE and moisture closures is too early, as the CAPE closure is discussed in the next section.

4.3 Parcel-environment CAPE closure

page 25761: why a discussion about triggering versus closure suddenly appears here? This should be discussed earlier.

4.4 PBL based closure

“ CIN is too unreliable to be used for a closure “ ?? What does that mean exactly? Again, the closure and the triggering issues are mixed up here.

In the frame of this “PBL-based closure” part, as well as in the frame of the sub-grid variability of CAPE and CIN mentioned before, a reference to the ALP closure proposed by Grandpeix et Lafore (JAS, 2010) and Rio et al. (GRL, 2009) would be relevant. This closure is now operational in the LMDZ model (Hourdin et al. (2012), Rio et al. (2012), Climate Dynamics). The ALP closure assumes that deep convection is controlled by sub-cloud processes providing energy and power to lift and sustain convection. Boundary-layer thermals and cold pools provide an available lifting power (ALP) which is used to compute the cloud-base mass-flux at the base of convective towers. In addition, the introduction of the cold pool parameterization allows to introduce a sub-grid variabilty of CAPE and CIN within a model grid box, as the convection scheme does not see the mean grid box environment but only the part outside cold

pools. This is an approach that should be mentioned and discussed.

The discussion on CIN is very interesting. But again, is it the right place in the paper to discuss the triggering versus closure issue?

4.5 High resolution limit

This part is the only one not discussing previous literature on the subject and presenting new results. However, neither the model nor the parameterizations and the simulations used are described in a satisfying way. As it is a review, I would suggest to discuss previous studies addressing this point rather than presenting new results here. If there are not enough studies that addressed this point in the past, then this point should be left to the conclusion to justify and encourage future studies but this paper does not seem the right place to present such new results to me.

5. Differences over the globe

This discussion is very interesting, but, as already said, I would put this discussion in section 2, and try to avoid repetition through the text.

6.Conclusions

The discussion about the definition of convection in observations here is much too late.

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 25743, 2012.