# Empirical values and assumptions in the convection schemes of numerical models

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## Response to referee

Referee comments shown in black
Authors' responses shown in blue

The authors have done well in improving the manuscript at various points, in response to the feedback provided by both reviewers. This includes a more complete coverage of all types of convection schemes that have been proposed, including more modern and unified approaches. The authors have also put convincing effort into getting the references right, where in the first submission a few key publications were ignored. All of this has made the paper more complete and concise, which is recommendable. As I already stated in my first review, I really do appreciate the significant amount of work that has gone into scanning all convection schemes and summarizing their essential assumptions and settings.

Thank you very much for the comments.

That said, some of my main concerns have not been adequately addressed. A few specific points I raised and some questions I asked remain unanswered or were side stepped in the response. These still open issues, which are also important, are summarized below. I remain of the opinion that these concerns need to be adequately addressed before publication is possible.

We address this comment in the Main concerns section below.

In some scientific journals a failure to address major concerns first time round automatically leads to rejection. I would still recommend a major revision, mainly because I do see merit in this work. So, I leave that decision to the editor.

Thank you for suggesting a revision of the manuscript. We have carefully addressed all comments and we believe that the manuscript has been sufficiently improved for its acceptance.

**Main concerns**

1) In response to my first major comment, and at various other points, the authors state that "Please note that as stated in the title and in the abstract, the paper is a review of the empirical values and assumptions. It is not a review of convection schemes". I fully disagree, for the following simple reason: these (parametric) assumptions are the defining parts of convection schemes, and what makes them differ from each other. This implies that one cannot separate the two. When the objective is to provide a review of empirical assumptions, then this in effect comes down to reviewing (differences between) convection schemes. This might be a disagreement on semantics. Still, it is important to clarify this in the manuscript, to avoid any confusion with the reader (including myself).

Indeed, reviewing the empirical values and assumptions implies reviewing convection schemes, but what we mean is that we focus on the values and assumptions and not on reviewing each particular convection scheme separately.

We have deleted line 95 ("This is briefly and schematically done, as the focus of this paper is not reviewing the convection schemes in themselves but to identify the assumptions and empirical values embedded in them").

I also disagree that this review is the first of its kind ever, as for example stated in the introduction (line 75, "To the best of our knowledge, there is no such extensive review..."). I know of at least one previous study. De Roode et al (2012, doi.org/10.1175/MWR-D-11-00277.1) discusses empirical assumptions and values as feature in the updraft kinetic energy equation and includes a thorough literature review. In structure and content, their Table 1 is very similar to, say, Table 6 on entrainment rates in this manuscript (among others). For this reason, I think this statement should be softened, to properly acknowledge previous work.

We agree that other reviews, such as the work of De Roode et al. (2012), thoroughly discuss the empirical assumptions and values used in convective models. However, these reviews usually focus on one particular parameter. For example, De Roode et al (2012) mostly focus on the vertical velocity equation and does not include a revision of the detrainment or the closure, among others. To make it clearer, we have added the following sentence in line 80: “There are indeed several reviews thoroughly discussing the empirical values and assumptions in convective models (e.g. De Roode et al. 2012), but they are generally focused on a particular parameter.”

2) In my second main comment I asked to provide a clear statement of what is the overarching science objective / higher goal of this review, or in other words, what is the added value of this review. The response is as follows: “The goal of the present paper is to provide a comprehensive account of the empirical choices and assumptions behind the representation of convective precipitation in models.” But this is not an answer to my question. I ask what we learn from reviews like this. Is it just a collection of long tables with many values and references, acting as a library index? Or does it yield new insights? This remains unclear, also in the revised version. Most scientific review studies provide a vision like this, so I was expecting this as a reader.

We have added the following sentence in the abstract:

Such information can assist satellite missions focused on elucidating convective processes (e.g. the INCUS mission) and the evaluation of those model output uncertainties due to spatial and temporal variability of the empirical values embedded into the parameterizations.

And also added this paragraph in the introduction:

The scientific interest of our endeavor is twofold. First, it can assist dedicated satellite missions such as the Investigation of Convective Updrafts (INCUS) mission, a new Earth Venture Mission-3 (EVM-3) of three SmallSats expected to be launch in 2027 that aims to increase our knowledge of precipitation processes, and specifically on the many nuances behind convection (Stephens et al. 2020). Indeed, INCUS aims to advance our present understanding and modeling of convection on the directions identified in the ‘decadal survey’ (cf. Jakob, 2010; National Academies of Sciences, Engineering and Medicine, 2018, hereafter ‘decadal survey’). The precise description and rationale behind the empirical parameters in the parameterization of convection can help INCUS and similar missions to focus on the key parameters, and to analyze their impacts on weather and climate models.
Another science goal of our review is to pinpoint the more relevant empirical values so systematic sensitivity studies can be readily carried out. We exemplify the latest goal showing that the spread of a perturbed ensemble of just a few parameters can be substantial. Thus, we have used the European Centre for Medium-Range Forecasts (ECMWF) Integrated Forecasting System (IFS) to perform a sensitivity experiment with seven parameters (organized entrainment, entrainment for shallow convection, turbulent detrainment, adjustment time, rain conversion, momentum transport, and shallow vs deep cloud thickness). While this is a small subset of the many parameters we have identified in this review, and the experiment is intended as an illustration of the spread in the simulations for two tropical storms, the case invites to more systematic runs in both space (global coverage) and time (decadal simulations) over the whole empirical set of parameters of any given model. The spread of the results will help to gauge the uncertainties due to the empiricisms embedded in the convection modules, and to constraint those through dedicated campaigns and targeted observations.

3) The response provided does not adequately address my concern. The response is: "... we refer to numerous convective parameterizations that were developed based on observations, ...", and "We state that observations are needed to improve the current understanding of the physics of convection". All convection schemes are based on at least a few observations; that is common knowledge, and not my point. Instead, my question is what your tables can tell us about what more we need in terms of observations to make progress, for example to break the ongoing "parameterization deadlock" (Randall et al, BAMS, 2003). Has the use of observations by the convective modelling community so far sufficient? Or do we need to find new ways to adequately constrain assumptions and calibrate parameterizations, in a statistically significant way? And if so, how can we most efficiently use modern extensive big datasets to this purpose? Having put so much work into delving through all these schemes in detail and listing all the key components (which I find really impressive), you are now in a unique position to make a statement about that. The reader expects that vision, and accordingly, I thoroughly recommend adding it. Not doing so is an omission. Hence my advice to add a section dedicated to this topic. This advice still stands.

In section 6 we have already mentioned that "…observations suffer from data gaps and the instruments used are not able to sampling key variables in parametric equations." Therefore, other techniques to improve parameterizations were proposed, such as the use of CRMs, LES or SCMs. However, these techniques suffer from drawbacks, such as the ability of idealized simulations to represent the actual climate.  More recently, a combination of observations and LES simulations is used, such as the one proposed in Neggers et al. (2012). We have added the following explanation in line 1683: "Despite the increase of observational supersites worldwide, data gaps still remain. A statistically process-level evaluation has been proposed by authors such as Neggers et al. (2012) or Gustafson et al. (2020), among others. This new approach consists in combining LES outputs with observations. Indeed, high resolution models provide additional information in 4D that is not possible to be obtained from point-based measurements (Gustafson et al., 2020). Another complementary approach to fill observational gaps and provide scientists with more information about the physics of convection is dedicated satellite missions such as INCUS. Although observations have long been used to tune parameters in convective schemes to reduce errors, it is still unclear whether these tuned parameters based on particular datasets can improve model skills across different locations, model resolutions or atmospheric events. Spaceborne sensors can help to palliate the situation through global, homogeneous and time-extended observations. INCUS and forthcoming missions can shed new light on the empiricisms and help characterizing the adequate values for the many empirical parameters in models. As described above, it is known that model results are sensitive to the empirical values in convection."

4) Judging from the response, I think there is some confusion about what is meant by "boundary layer scheme". This is not always the same in each model. Some interpret the boundary layer as only representing dry (non-saturated) turbulence and convection; others consider cloud layers as intrinsic part of the boundary layer, thus including shallow cumulus and stratocumulus. So, to avoid unnecessary confusion with the reader, I recommend to clearly define early on in the manuscript what exactly is meant by "boundary layer scheme", and then to consistently use this definition throughout the manuscript. This template may sometimes not be applicable to more unified schemes, in which microphysics, shallow transport and deep transport are interwoven and cannot be strictly separated anymore into unique and single modules, as was classically done.

We have added an explanation on boundary layer schemes in section 1: "While other schemes, such as planetary boundary layer (PBL) parameterization used to parameterize turbulence within the PBL without accounting for moist convection …"

That said, I know of quite a few boundary layer schemes that do generate precipitation. For example, in contrast what you say, the IFS EDMF scheme makes use of plume equations that do include a source/sink term representing precipitation. See IFS documentation C47R3 chapters 3.2 and 6.3.1. So the EDMF scheme does produce rain in case the EDMF plume condensates. Second, when the IFS Tiedtke scheme is in shallow cumulus mode, it is in effect generating boundary layer precipitation, and can thus be classified as a “boundary layer scheme”. This rain can be significant, as we have learned from field campaigns on Trade wind cumulus such as RICO and EUREC4A.

The IFS scheme is just one example; there are more boundary layer schemes that directly generate precipitation. The EDMF scheme of Neggers (2009, doi.org/10.1175/2008JAS2636.1) also produces rain. The CLUBB scheme as implemented in CAM (Larson et al., GMD, doi.org/10.5194/gmd-8-3801-2015) also generates precipitation when in boundary layer mode; see their Section 2.4 and Fig. 1.

Thank you very much for the insight into boundary layer schemes that directly produce precipitation. We have added these examples in section 2.7 of the manuscript.

5) "A detailed list of parameters is not included". I do not understand; which parameters do you mean? In the figures? In my opinion, all aspects of figures should be fully explained in a scientific publication, even if they are just meant to be illustrative. This is just good scientific practice: all science should be reproducible, otherwise it is meaningless.

We meant that in the first version of the manuscript we did not include detailed information about the setup used to perform the simulations, e.g., microphysics scheme or radiation scheme, among others. However, we added these details in the revised version of the manuscript after the referee recommended to do so.

I also find new Figure 3 somewhat simplistic. For example, it depicts shallow convection as exclusively non-precipitating, which by now we now is totally untrue (see the many studies based on RICO, NARVAL and EUREC4A data and simulations). Second, it conforms to the old idea of how convection should be modelled, using a single bulk plume and a modular approach. The schematic certainly does not accommodate unified or spectral approaches in modelling convection. See for example Fig. 1 in Arakawa and Schubert (1974), which is a much more realistic example of how a convective population works. If this review is to be comprehensive, as is claimed in the introduction, the figure should accommodate all approaches, not just the classic bulk one.

We have changed the figure accordingly.