Review of revised GMD manuscript https://doi.org/10.5194/gmd-2021-61

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.

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.

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.

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).

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.

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.

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 modeling 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.

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 can not be strictly separated anymore into unique and single modules, as was classicaly done.

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.

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 reproducable, otherwise it is meaningless.

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 modeled, using a single bulk plume and a modular approach. The schematic certainly does not accommodate unified or spectral approaches in modeling 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.