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Comment on gmd-2021-61

Anonymous Referee #2

Referee comment on "Empirical values and assumptions in the convection schemes of numerical models" by Anahí Villalba-Pradas and Francisco J. Tapiador, Geosci. Model Dev. Discuss., https://doi.org/10.5194/gmd-2021-61-RC2, 2021

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

This study presents a review of convection schemes that have been developed for weather and climate models. An overview is given of the classic types of convection schemes as well as their individual components, such as the triggering function, the transport or cloud model, the microphysics scheme, and closure methods. Figures of results with various convection schemes as implemented in WRF for a single hurricane case are included as illustration. A strong point is this review is pretty comprehensive, also in its referencing. Quite some work has gone into collecting and describing the many variations in formulations that have been proposed in the past few decades. The tables that summarize these formulations can well function as an overview for someone new to the field.

That said, after reading the manuscript I could identify a few significant shortcomings. Firstly, the overview of convection schemes overly focuses on rather classic approaches, and fails to cover some important new developments in the field. These include recently proposed unified approaches, new concepts that address the ever more important grey zone problem, and new schemes capable of representing convective organization and memory. Not fully covering these recent developments, which result from active and intense ongoing research, is a serious omission. In my view this is detrimental for the relevance of this paper for the science community. The manuscript feels rather outdated in that sense. A second concern, and related to this point, is that the science objectives of this study are not clearly defined. What exactly is the purpose of this review? This remains unclear. Third, the main conclusions of this study (as stated in the abstract) are not objectively supported by the content that is presented. This mainly concerns the impacts of convection schemes on weather and climate, and the need for observational datasets. Fourth, the introduction contains statements that are not true, while the introduction is also overly skewed towards motivation and does not explain what is unique about this particular study. Finally, the figures included are not fully described and explained.

To summarize, although I do see the use of another review of convection schemes, the current version is incomplete, somewhat outdated and should be improved at various points. For these reasons I recommend major revision of this manuscript to address these issues, before it can be accepted for publication.

General comments

1. Convective parameterization is a science field that sees active development at the moment. This is driven among others by the realization that these schemes continue to cause significant uncertainty in climate predictions, and also by the shift towards higher resolutions that become feasible in state-of-the-art weather and climate modeling. While these developments are briefly mentioned at various points in the paper, this is not reflected in its organization and structure. Recently proposed new types of convection schemes which show promise in addressing these issues and are "breaking the parameterization deadlock" (Randall et al., 2003) are not discussed in a structural way. These include i) unified parameterizations based on the EDMF approach (Siebesma et al, 2013, and many follow-up papers about EDMF), ii) PDF-based schemes (e.g. Golaz et al., 2003; Larson et al, 2012), iii) schemes that are scale-aware, scale-adaptive and also introduce stochasticity due to subsampling of a convective population inside a GCM gridbox at higher resolutions (Honnert et al, 2020), and iv) approaches that successfully capture convective memory and spatial organization. Not explicitly covering and describing these new approaches, which are now in the stage of becoming widely adopted in operational models, makes the current version of the manuscript not reflective enough of the state of the art in this research field. In that sense the manuscript is somewhat outdated, and repetative of previous reviews of convective parameterization that have already covered the classic schemese and approaches in great detail. To make the review better reflect these new approaches I strongly recommend giving them their own category / subsection in the organization of the manuscript.

On a related note, while the bibliography is indeed extensive, I noticed that at some points the referencing is not accurate. Sometimes the first "breakthrough" studies that proposed a new concept are not referred to, but instead only a somewhat arbitrary selection of follow-up studies are mentioned. One example are new mass flux schemes as mentioned in section 1.2, page 7, of which only one very recent example is mentioned. Another example is the description of spectral mass flux models (section 4.1.2), of which many more have recently been proposed and explored (e.g. Wagner and Graf, 2010; Neggers, 2015; Suselj et al, 2013; Olson et al, 2016; Brast et al., 2018; Hagos et al, 2018). In my opinion the review is by far not complete at these points, which is a serious omission. I recommend going through all sections again and to make sure that key groundbreaking studies (both classic and recent) are properly mentioned.

2. The introduction misses a clear statement of the main science objectives of this study. Is this paper meant to be just a review, or more? Both the introduction and the abstract are misleading in this respect. While the introduction elaborately emphasizes the importance of convection schemes and the need for their careful evaluation, it fails to clearly state that this paper is meant to be a review of all existing schemes out there. It is also not clearly mentioned what is new compared to previous reviews of this kind, how this particular review can help the community. I recommend adding clear statements

about both the objectives and the novel aspects in the introduction.

3. The abstract announces conclusions that are not objectively or adequately supported by the contents of this study. This concerns i) the examination of impacts of choices of parameters in convection schemes on weather and climate, and ii) insights concerning the need for observational datasets for constraining these choices.

Concerning the impacts of parameter choices, no new analyses are included that demonstrate this impact in a statistically significant way. As is usually the case in a review paper, this review refers to previous studies for describing such impacts. However, in my experience these impacts of parameter choices are extremely code-specific, and are not universal. It is often the case that errors in calibration of one parametric component are hidden by errors in another; a good example is the too-few, too-bright problem (e.g. Nam et al., 2014) or errors in cloud overlap masking errors in vertical structure (Neggers and Siebesma, 2013). This means that results from parameter studies with one GCM code do not necessarily translate to another. This danger is not mentioned, but is very relevant for the conclusions drawn in this paper. This should be discussed.

It is also not evident from the content how exactly observations can help in constraining parameter choices, yet is is presented as a major conclusion in the abstract. That convection schemes have many constants that need constraining is not a new insight. Exclusively using observations for this purpose is problematic, due to data gaps and instruments not being capable of sampling key variables in parametric equations. Various recent international efforts have been conducted to make progress in this respect, in the form of field campaigns (e.g. EUREC4A) or long-term deployment of instrumentation at meteorological supersites (e.g. Neggers et al, 2012; Song et al., 2013; Gustafson et al., 2016; Zheng et al, 2020). A thorough discussion of both the problems and opportunities for constraining parameter choices in convection schemes with modern observations is missing, but is needed to support this conclusion. I recommend adding a section on this topic if this conclusion is to be maintained; if not, then I would remove this conclusion from the paper.

4. The introduction contains statements that are factually wrong. Precipitation in general circulation models is not just generated by convection schemes and/or microphysics schemes. Boundary layer schemes can also contribute to both convective and stratiform precipitation, and significantly so. In fact, precipitation in subtropical marine low level stratocumulus and trade wind shallow cumulus is often completely carried by the boundary layer scheme. This error should be corrected. More generally speaking, this point also relates to my first comment, in that new unified schemes have recently been proposed that are less strict in the separate representation between shallow and deep convection, and between convective and stratiform rain. These unified schemes are not properly described. I recommend improving the text in the introduction, and the manuscript as a whole, to more precisely and accurately describe the reality in state-of-the-art general circulation models.

5. Two figures are included in the manuscript that are meant to illustrate the impact of

convective parameterization on the representation of convective phenomena in a circulation model. To this purpose WRF simulations of a tropical cyclone are used. However, at the points in the manuscript that these figures are referred to they are not properly explained. What is the model setup, what are the boundary conditions and forcings, and which data is used to derive these? Because these details are absent, these results can not be independently reproduced. I recommend adding an appendix in which these simulations are adequately described.

Detailed comments

p1, title: ".. in the convection of numerical models". This does not make sense. I guess you mean "in the representation of subgrid-scale convection in circulation models", or something similar?

p1, abstract: The first half (three sentences) is a general description of what convective parameterization is, and why it is important. As this is already generally known and does not reflect the particular contents of this study, I recommend to remove this part and instead add a few lines about what is new about this review compared to previous ones, its science goals, and what aspects are unique and worth remembering.

p5, line 64: ".. generate precipitation through two parameterizations: microphysics... and cumulus parameterizations". This is not true: boundary layer schemes formulated in moist conserved variables also produce significant precipitation. See my 4th main comment above.

p5, line 67: ".. are intended ...". Intended by who? Please be specific.

p5, line 77: "biota". What does this mean?

p7, line 123: please add the key paper by Wyngaard (2004) as well as the recent review paper by Honnert et al (2020) to the list of references about the convective grey zone. See also my 1st main comment above. Also, "grey scales" is a term I have not come across before; I suggest to stick to "grey zone"

p7, line 129: Why only refer to this paper for scale-aware parameterizations? Many more have been proposed by now, also much earlier than the study cited here. Please perform more thorough literature research on scale-aware parameterization, and add the most relevnt papers on this topic here.

p8, line 136: "latest decadal survey". Do you mean the NAS one mentioned in the previous paragraph? This is not clear; also, not everyone is familiar with this survey.

p8, line 143: "crudely". Simple parameterizations are not necessarily crude. This is not scientific language, and is also somewhat insulting towards scientists who have spend significant effort in developing and implementing such parameterizations in GCMs.

p8, line 146: "computing-intensive": do you mean that convective parameterization is always computationally demanding? Then please explain why this is the case.

p8, line 152: ".. Fig. 2". This is the first time the figures are referred to, and would be a good point to explain what exactly we see in them, and why this is relevant in this context. See also my 5th main comment above: these simulations are not adequately described, and can not be independently reproduced. And why is testing convection schemes for a cyclone the most relevant? Usually convective parameterizations are tested for simpler cases (such as locally forced continental convection) in a simpler setting (e.g. Single Column Models combined with a weak temperature gradient approach). Please explain these choices.

p10, line 160: "between cumulus clouds". Parts of convective updrafts are non-cloudy, yet in this state still contribute significantly to the total vertical transport. Please change your labels such as "cloud model" to account for this.

p10, line 169: Please explain what CISK means.

p10, line 190: "no cloud models are needed". I would phrase this differently, because the cloud model is still needed; but it is now hidden in the adjustment procedure.

p11, lines 194-195: "very large precipitation... rarely observed in nature". No reference is provided to back up this statement; please cite a paper that shows this.

p11, line 200 and onwards: For clarity, I suggest to include a simple equation for the adjustment, including a time scale. Different forms of adjustment are possible (e.g. Newtonian); can all be described by the same equation?

p12, equation (1): These equations are not universally applicable to all mass flux schemes. Some are formulated in terms of conserved variables for moist adiabatic motion. So either it should be mentioned that (1) is only applicable to a subset of mass flux schemes, or the formulation should be changed to make it more generally applicable.

p12, line 241: "single entraining plume". Is this a steady state plume? Please explain. Also, I would add some references to the first classic papers about the rising plume model, such as Simpson and Wiggert (JAS, 1969). A relatively recent review study about the rising plume model and its parameter choices that to my great surprise is not mentioned at all in this paper is the one by De Roode et al. (2012). How do the values mentioned in that paper overlap with those summarized in the Tables in this paper? See also my 1st comment about adequate referencing.

p12, line 244: ".. the i-th cumulus cloud". Is this a single cloud, or a sub-ensemble of clouds, such as clouds of a certain size or strength? Please define.

p12, last line. The model described by equations (1) and (2) does not cover various new mass flux approaches, such as EDMF or pdf schemes. These have successfully been applied to precipitating convection, and can actually deal with scale adaptivity. See also my 1st main comment above.

p13, line 252: "produce too little heavy rain and too much light rain". Do all convection schemes suffer from this, or just one implementation in one particular GCM? See also my 3d main comment above.

p13, line 253: "Pritchard et al, 2011": I would also refer to Guichard et al. (2004), who first properly documented this behavior and which paper was also published 7 years earlier (which is a long time in science). See also my 1st comment about adequate referencing.

p16, line 350: "widely used at ECMWF". What do you mean with "widely". Please add a reference to a study that shows this.

p22, line 445: "convective memory". This topic is intensely researched at the moment, yet is only briefly mentioned here. I think it deserves much more attention, even its own section. Doing so would make this review paper a lot more relevant and up to date.

p27, concerning the discussion on entrainment: I think it makes sense in this discussion to also refer to these recent papers, also in the table:
https://doi.org/10.1029/2019JD030889, https://doi.org/10.1175/JAS-D-20-0377.1. Apart from providing new insights, they also use observational data to constrain entrainment

rates, which is relevant for this review.

References

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