Reviewer 2:

Journal: GMD

Title: "The GF Convection Parameterization: recent developments, extensions, and applications" by Saulo R. Freitas et al.

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Anonymous Referee #2

We thank the reviewer for his/her insightful and helpful comments. The paper is now much improved by his/her comments and corrections. The reviewer's comments are in blue color.

In this paper the authors extended various aspects of the Grell-Freitas convection scheme. These include using a trimodal representation of shallow, congestus and deep convection, inclusion of a non-equilibrium closure to account for boundary layer forcing to better represent the diurnal cycle of convection, and the use of three pdfs for normalized mass flux profiles. In addition, the microphysics in convective updrafts is extended to include ice phase and associated latent heat release. Both single column and GCM simulations are performed to evaluate these changes. The results are quite interesting. However, the presentation has much to be improved. A serious effort is needed to fix many sloppy descriptions of the GF updates. A major revision is required before I can recommend it for publication.

Thanks again for your work on reviewing the manuscript and making recommendations. We did our best to accomplish all of them and hope it is now suitable for publication.

Major comments:

1. In several places the text was almost identical to text from an- other paper of the authors. While I believe this is unintentional and will refrain from calling it "self-plagiarism", it does reflect the sloppiness of the writing. For example, In the abstract of Freitas et al. (2018): "Recently, we extended the scheme to a tri- modal spectral size distribution of allowed convective plumes to simulate the transition among shallow, congestus, and deep convection regimes. In addition, the inclusion of a new closure for nonequilibrium convection resulted in a substantial gain of real- ism in the model representation of the diurnal cycle of convection over the land." In the abstract of current manuscript: "The parameterization has been extended to a tri- modal spectral size to simulate the interaction and transition from shallow, congestus and deep convection regimes. Another main new feature is the inclusion of a closure for nonequilibrium convection that resulted in a substantial gain of realism in the simulation of the diurnal cycle of convection, mainly associated with boundary layer forcing over the land." Lines 7-9 on page 3 of this manuscript: "Each of the modes is distinguished by different lateral entrainment rates that strongly control its vertical depth and, consequently, the height of the main detrainment layers." Lines 10-11 from bottom on page 1268 of Freitas et al. (2018): "Each of the modes is distinguished by different lateral entrainment rates that strongly control its vertical depth and, consequently, the height of the main detrainment layers." Such a practice of copy-and-paste from one paper to another is unacceptable.

Those expressions are no longer present and sorry about that.

2. Many specifics are missing in the description of the GF updates. Providing accurate information is important since a main objective of such work is to document the changes of physical schemes for interested readers. For example, P. 5, L5-8. "The mass flux profiles are given by a Beta PDF, statistically representing the normalized statistical average mass flux of deep and congestus convection in a grid

box. The effective vertical entrainment rate and detrainment rate profiles are derived from these mass flux profiles." Please provide the beta pdf in the form of a mathematical equation. Also, provide the equations for entrainment and detrainment. There are a number of such omissions.

We rewrote the description of the use of the PDF's and gave much more details, including a description of how this approach could be used for stochastics and/or tuning for operational applications. Section 2.2 is a full description of the PDF approach of the GF scheme as it is available on github, and used operationally in the RAP forecast system at NCEP/U.S.

3. In the abstract, authors states that one of recent extensions is in cloud microphysics: "Finally, the cloud microphysics has been extended to include the ice phase to simulate the conversion from liquid water to ice in updrafts with resulting additional heating release, and the melting from snow to rain within a user-specified melting vertical layer." However, there is no analysis, no figure, and no conclusion about the impact and performance of this change. If the impact is significant, please show it.

The main feature in extending to include the ice phase is additional heating at upper levels associated with the phase change from liquid to ice. We did not see a significant impact overall. However, including this extension makes the formulation physically more realistic.

4. The authors showed the performance of GF shallow scheme only with a 2-day model simulation. Are there any observations to evaluate the shallow cumulus simulation? The mass flux shows that shallow cumulus can reach to 5.5km height, is it reasonable? Authors list three shallow convection closures in the manuscript. However, it is not clear that what closure is actually used in GF shallow scheme. What are the performance differences among these closures? A figure showing the performance of each option would be desirable.

R. The figure 1 was replaced by the results of the SCM run over the Amazon basin. The text was completely rewritten (section 3.2), and the Figure 8 shows the results.

5. In the single column model evaluation (Figs. 7&8), the authors should evaluate the simulated heating and drying tendency with available observations, for example, the analyzed diabatic heating (Q1) and drying rate (Q2) over the sounding domain during TWP-ICE described by Xie et al (2010).

We added Q1 and Q2 profile discussions. Shape is usually very similar, but magnitudes are somewhat different, even for a comparison of profiles calculated over the sounding domain from Xie et al (2010) and observed profiles supplied by the SCM.

6. Trimodal formulation is based on the observational analysis for tropical environment (Johnson et al., 1999). Is there any observational analysis for middle latitudes that supports this classification of convective modes? Is the scheme sensitive to the choice of entrainment rate for three mode of convection? It would be helpful to discuss this in the context of the full spectral representation recently used by Song and Zhang (2018) and Baba (2019).

We don't think that congestus convection is limited to the tropical regions. We modified the sentence in the manuscript to clarify this a little. In addition, while we are not representing a smooth spectral representation of all convective cloud types as is the intention in Song and Zhang (2018) and Baba (2019), the PDFs used here are to represent a statistical average of three cloud types, but that does not mean they are always the same size. A PDF for deep convection may represent several cloud types. The top of those cloud type is given and the location of the maximum mass flux in the vertical, but that does not mean that cloud types contribute that are not reaching the top of the PDF. A similar assumption is valid for congestus and shallow convection. This is also why mass flux of shallow convection may reach up to 550mb.

Minor comments:

The calculation of cloud work function (CWF) using Equation (12) is problematic. The equation (11) shows that CWF is in units of m-2s-2, which is equivalent to Jkg-1. However, equation (12) shows that CWF is in units of kg2m-4s-2. The problem is that CWF~gdz in equation (11), however, CWF~-rho*dp in equation (12). Based on the hydrostatic equation (dp=-rho*gdz), the equation (12) should be divided by air density instead of being multiplied by air density.

Sorry for the typo and thanks for point it out.

Fig. 1: Is this figure diagnosed using GF from a high-resolution simulation? which shallow scheme is used? How did the authors calculate mass flux in units of kg m-2s-2 in Fig.1? Authors show mass flux in several figures but with three different units: kg m-2s-2(Fig.1), m s-1(Fig.3), kg m-2s-1 (Figs. 5 and 6).

The figure 1 was removed. The units in Figure 3 are wrong, they are kg m^{-2} s^{-1}. The units in Figs 5 and 6 are correct.

Fig.9 even does not provide the units of mass flux. Authors should clarify this and use the same correct units for mass flux so that the readers can easily compare and understand these figures.

The figure was replaced and the total mass flux from the 3 modes is now shown in units of kg m^{-2} s^{-1}.

3. P.5, L10-11. "For congestus, the closures BLQE and based on W* described in Section 2.1.1 are available, besides the instability removal using a prescribed time scale." You mean eqs. (1) and (2)? If so, state explicitly. Also, in these equations no instability removal timescale is involved. Please clarify and be specific.

The text reads now: "For congestus, the closures BLQE (Eq. 1) and based on W* (Eq. 2) described in Section 2.1.1 are available, besides the instability (measured as the cloud work function) removal using a prescribed time scale of 1800 seconds (see Section 2.3 for further details)."

Figure 3. What is the shading on the left and dashed lines on the right? I assume they are standard deviations. But the authors should not leave the guesswork to the readers.

As asked by the reviewers, the Section 2 was completely redone, and this figure no longer is present in manuscript.

Page 6, lines 19-21: reference should be provided. Is there any difference in diurnal cycle of convection between land and ocean regimes?

A reference was added. A brief discussion about the diurnal cycle of convection in both regimes is present in Section 3.3.

P. 7, L8. How does the partition vary in the mixed phase temperature range (250.16, 273.16)?

R. The partition is now explicitly informed in 2.4. Thanks for asking.

Page 8, line 31: “GF slightly underestimates the heavy precipitation in the active monsoon period”. The underestimation of about 30% is not a slight underestimation for me.

Rewrite in paper as:
Compared to single location precipitation data the maximum amount appears underestimated. However, the average when using GF is over an area that covers a much larger domain.

P. 9, L30: Authors explain the convective cooling near cloud top by the evaporation of detrained liquid water at cloud tops. Since the cloud liquid water is detrained into environment, its evaporation cooling in the environment should not be considered convective cooling. It is usually treated as grid-scale evaporation cooling in the model. So why is there convective cooling near the cloud top? OR GF counts it differently?

The reviewer is correct, that the resolved microphysics will evaporate the detrained cloud water, which may lead to cooling. However, in GF impacts from convection are from both, detrainment of water vapor, detrainment of moist static energy, and subsidence impacting both. Since much water vapor is detrained this will lead to cooling, especially for shallow and congestus clouds, since the amounts of water vapor are larger, and the subsidence impact on moist static energy is smaller. The equation below shows those impacts:

$$\frac{\partial T(k)}{\partial t} = \frac{1.}{cp}\varrho[h(z)] * m_{b(CU)} - \frac{L_v}{cp}\varrho[q(z)] * m_{b(CU)}$$

Here the ϱ is the change of moist static energy (h) or water vapor (q) per unit mass, and $m_{b(CU)}$ is the cloud base mass flux for deep, congestus, or shallow convection.

Figure 2 shows the updraft mass flux with cloud base at model level 5 and cloud top at level 50. Why large mass flux exists below cloud base (model level 1-5)?

The figure 2 was redone. The cloud base height is about ~ 1.2 km and the mass flux increases from that level.

What does the dash line mean in Figure 3?

This figure is no longer in this manuscript version.

Figure 6. The caption says that downdraft mass flux is in green, however, there is no green line in Figure 6. It shows shallow cumulus can reach to 600hPa. Again, are there cloud observations (cloud depth, fraction) to qualitatively evaluate the simulations?

Considering we already show the trimodal cloud characteristics in Fig 5b, and also got the AMS permission to use the observational Figure 13 from Kumar et al. 2016 as Fig 5a. We deleted the original Figure 6, and added the validation of diabatic heating source (Q1) and drying sink (Q2) in the revised manuscript.

Fig. 5. Add a line for shallow mass flux.

Following the reviewer’s suggestion, a line in blue for shallow mass flux was added in Figure 5b.

Figure 13. It's difficult to discern much useful information from this figure. It would be better to plot the 24-h phase dial. (e.g. Fig.9 and Fig.12 in Xie et al. 2019). Also, the results of one month (January 2016) are not enough. It can be easily done with multi-year data. How is the performance of model simulation for summer months?

R. We included more results and discussion based on the approach shown in Xie et 2019, and the analysis was also performed for July 2015. Using this approach, we focused on the CONUS, Amazon, tropical north of Africa, and the Tropical Pacific Ocean. Please, take a look at the new Section 3.3.

Please insert space between an equation and its number.

Done, thanks.

The font size in 3.2 (page10) is different to other parts in the rest of paper.

Done, thanks.

How is the cloud base determined for shallow, congestus and deep convection?

This information is now much more detailed in the Section 2.

Page 12, line 7: what does "Each forecast day comprised a 120-h time integration" mean?

The text now reads: "Each forecast day covered a 120-h time integration, with output available every hour."

Refs:

Baba, Y. (2019). Spectral cumulus parameterization based on cloud resolving model. Climate Dynamics, 52, 309– 334.

Song, X., and G. J. Zhang, 2018: The roles of convection parameterization in the formation of double ITCZ syndrome in the NCAR CESM: I. Atmospheric processes. Journal of Advances in Modeling Earth Systems, 10, https://doi.org/10.1002/2017MS001191

Xie, S., T. Hume, C. Jakob, S. A. Klein, R. B. McCoy, and M. Zhang, 2010: Observed large-scale struc- tures and diabatic heating and drying profiles during TWP-ICE. J. Climate, 23, 57–59, https://doi.org/10.1175/2009JCLI3071.1. Xie et al., 2019:

Improved diurnal cycle of precipitation in E3SM with a revised convective triggering function. Journal of Ad- vances in Modeling Earth Systems, 11, 2290–2310.