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Geoscientific Model Development Discussions

Interactive comment on "The GF Convection Parameterization: recent developments, extensions, and applications" *by* Saulo R. Freitas et al.

Anonymous Referee #2

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Review of manuscript "The GF Convection Parameterization: recent developments, extensions, and applications" by Saulo Freitas et al.

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.

Major comments: 1. In several places the text was almost identical to text from another 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 trimodal 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 realism 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 trimodal 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. 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. 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. 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. 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). 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).

Minor comments: 1. 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$\sim$gdz in equation (11), however, CWF$\sim$-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. 2. Fig. 1: Is this fig-

ure 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). 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. 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. 4. 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 gusswork to the readers. 5. Page 6, lines 19-21: reference should be provided. Is there any difference in diurnal cycle of convection between land and ocean regimes? 6. P. 7, L8. How does the partition vary in the mixed phase temperature range (250.16, 273.16)? 7. 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. 8. 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? 9. 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)? 10. What does the dash line mean in Figure 3? 11. 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? 12. Fig. 5. Add a line for shallow mass flux. 13. 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? 14. Please insert space between an equation and its number. 15. The font size in 3.2 (page10) is different to other parts in the rest of paper. 16. How is the cloud base determined for shallow, congestus and deep convection? 17. Page 12, line 7: what does "Each forecast day comprised a 120-h time integration" mean?

Refs: Baba, Y. (2019). Spectral cumulus parameterization based on cloudâĂŘresolving 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 structures 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 Advances in Modeling Earth Systems, 11, 2290–2310.

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