Journal: GMD

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

MS No.: gmd-2020-38

Anonymous Referee #1

The manuscript is much improved in response to first reviews. It is important that schemes have their documentation papers, so I recommend publication (whatever that means for copernicus journals like GMD, I remain confused by this whole scholarship model).

A few points of clarification will help with readability, but I focus my comments and suggestions only on the Abstract, since any reader who will take the time to wade into the internal structure will have committed the time to parse the subtleties.

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.

Line 11 "Abstract: We detail recent developments" — it will be clearer if the phrase AND OPTIONS is added. There are multiple treatments, for instance multiple closures and now (line 16) “we also added a new closure”. In the first reading I was confused about how these closures all connect or relate. Now I understand that these are various options within the code, all of which are being described here for completeness. In the applications section 3 (which I did not read carefully), the parameter values and choices should be specified, preferably in a Table. There are no tables currently.

Done.

Line 14: “we assume that Probability Density Functions (PDFs) can be used to characterize the vertical mass flux profiles...” This is incorrect and confusing language! Probability is not used. Instead, the Beta Function is assumed to be the shape of mass flux profiles. While the beta function is a normalized function, such that it can be sometimes used in probability theory, the functional form here is NOT a probability: rather, mass flux M(p) for each plume type is assumed to be a beta function of the pressure coordinate. The symbol Z is used for mass flux rather than M, for unclear reasons. The symbol r is used for a (relative) pressure (5), for unclear reasons. So this rather strong assumption that M(p) is smooth is somewhat obfuscated as a Z(r) smoothness assumption. In any case, it seems quite a drastic physical assumption, as stable layers and other features of stratification often induce blips in mass flux profiles through entrainment and detrainment (for instance, buoyancy sorting). That process is important in making the stratification moist adiabatic in convecting regions, ironing out the kinks and inversions in a sounding. None of the figures shown indicate whether this key job of buoyant convection is actually performed under this profile assumption. With 3 smooth profiles and time intermittency it probably works itself out, but that physical assumption remains a debatable one, and mustn’t be covered up by calling it a probability density assumption!

We thank the reviewer for the comments. We changed the expression, and now we are naming it as ‘Beta Function.’ The text was altered throughout to reflect the new term.

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Anonymous Referee #2

2nd review of "The GF Convection Parameterization: recent developments, extensions, and applications" by S. Freitas et al.
The manuscript is significantly improved although there are still areas that need further improvement. I have a few additional comments.
We thank the reviewer for his/her insightful and helpful comments. The reviewer's comments are in blue color.

Major comments:
1. The authors added 4 more figures (Figs. 14-17), all on diurnal cycle. Unfortunately, this makes the application section extremely lopsided, with 10 out of 17 figures on various aspects of the precipitation diurnal cycle associated with the use of boundary layer cloud work function generation. As such, the current title is not quite fitting. Alternatively, the authors can consolidate the diurnal cycle part and make the material more balanced.
We understand that including the diurnal cycle closure is a significant advance in this parameterization. That explains the larger number of figures and discussions. However, the model version still has several other new features that justify the current title.

2. The writing is much improved. However, there are still many places where the text reads quite rough. I suggest the authors go over the text thoroughly and correct the errors/misuse of words and polish the writing.
Additional English proofreading was performed. Also, during production, Copernicus Publications applies typesetting and language copy-editing. We understand that the final version of the manuscript will have an acceptable level of language quality and correction.

Minor comments:
1. P. 5, L12-13. By "thermal inversion" do you mean you still look for dT/dz>0 near 500 hPa to define congestus cloud top as you do for shallow convection? If so, this would be unrealistic, as in the free troposphere on a spatial scale of a GCM grid box size it would be hard to find dT/dz>0.
The thermal inversion is defined following the two criteria below:
- the first derivative (del T/del Z) must have a local maximum
- the absolute value of the second derivative must be zero.

2. Eq. (16), what is the value of tau_BL?
Sorry about that. It is now defined.

3. It appears zt (cloud top height in eq. (15)) is defined by the neutral buoyancy and/or inversion layer. But in Figs. 11 and 12, there are negative values of total CWF in the composite diurnal cycle. Is this largely from the negative buoyancy contribution below the level of free convection? If so, it's worth mentioning. If not, where do the negative contributions come from? The global average CWF is very small; there must be large negative contributions from mid- and high latitudes.
It seems to be related to the negative buoyancy contribution below the level of free convection. That is included in the text.

4. Regarding Fig. 12, it seems the composite over all grid points globally (plus land and ocean separately). This may not be meaningful since most of the grid points outside the tropics will have stable atmosphere. I suggest dropping this figure.
Thanks for the suggestion, but we think it is still beneficial to report the characteristics of the time evolution of the cloud work function and the boundary layer production.

5. Figs. 8-10 and 11-12 used time inconsistently. Please use either local time or UTC, but not mixed use of both.

We believe that even using the local time and UTC, the specification is clear for the reader. Additionally, for the figures with UTC, a mark denotes when the sunrise happens, giving the reader a sense of local time.

6. Fig. 15. Left column is not labeled.
Done, thanks.

7. Subsection 2.4. I asked about the effect of including freezing heating and the authored responded, stating the effect is minimal. It should be mentioned in the manuscript. Otherwise, readers may wonder what effects this modification has.
Done, thanks.

List of a few typos/grammatic errors (there are many more):
Additional English proofreading was performed by a native English speaker. Also, during production, Copernicus Publications applies typesetting and language copy-editing. We understand that the final version of the manuscript will have an acceptable level of language quality and correction.

1. P. 8, L14. "implies, for example, in a more evenly detrainment" should be "implies, for example, a more even detrainment".
Done, thanks.

2. P. 16, L25, "averaged areal", make a correction.
Done, thanks.

L28, "does not do much". To what?
Done, thanks.

3. P. 18, L20-21. "Both simulations were not able to...". Change to "...were unable to...".
Done, thanks.