Reviewer 1:
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
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.

("general comments")
This manuscript surveys some aspects of the latest version of the GF convection scheme, whose code is offered in an open source repository. I suppose the world1 is fortunate to have a narrative record from busy hard-working developers about what they have been doing. But I found it fairly unclear, despite my appetite for understand- ing it, and in places a bit congratulatory or justifying where the same column inches could be spent on more information. Major revision is needed if this is meant to reach the standards of a formal "scientific publication" in a traditional sense.
Since a lot of prior knowledge of the subject is assumed, and since the text has little pretense to a physical rationale for the algorithms being described, the paper could be very short and to the point: what is the informatic mapping from inputs and assump- tions, to internal variables, to outputs? Unfortunately, a careful read of this manuscript is required to fish out many of these mappings and assumptions, and then many details are left ambiguous, for instance about what kinds of choices a user must make (and the authors DID make in the examples presented).
In short, it could use a tightening-up in style, and a completeness check for crucial details. Here is an indented bulleted list of the inputs, internals, and outputs as I see them. The formatting system seems to mess them up, I find, but this is what I offer as an unpaid volunteer; the reader can guess the hierarchy.
Inputs:
- T and q and wind and tracer profiles
- the Tv tendency averaged over the PBL from a separate BL scheme
- an aerosol-related input to autoconversion efficiency in the height domain Assumed parameters:
- what level to start entraining parcels/plumes from, never explained
- 3 "initial" entrainment rates {2, 0.9, 0.1} /km , buried in different places
- something about the undetermined 4th parameter of the Beta profile curve
- closure choices for shallow convection - for 3 different approaches; and rules for how to select or combine them
- closure choices for middle and deep convection -assumed timescale for convection to damp the work function - the strength of a diurnal delay trick, misleadingly cast as a timescale tau_b
- temperatures for freezing and melting (melting should not begin at -3C, should it?)
Intermediate variables:
- 4 parameters of the parametric steady-state mass flux profiles for each of the 3 kinds of updraft plumes - 2 params: how to map Beta's [0,1] domain into levels, z, log-p, or p (various figures use all four)

- 2 params: shape of the profile - cast as the altitude of the maxiumum? - cast as the "mean cloud base"? - inverted from the closure somehow, along with a detrainment profile? - is an entrainment profile different from constant also backed out?
- All of the above for downdrafts too? downdrafts are mentioned, but never specified
- Finite area fraction profiles for minor "scale aware" adjustments of eddy flux from MF?
- Special assumptions for momentum vs. other tracers? (reflecting "pressure effects")

Outputs:
- profiles of tendencies of state variables (or the final result of successively updated profiles?)
- surface rainfall rate

Unfortunately, the manuscript is not clear at this level. It should be, since that seems to be its one job.

The manuscript was completely rewritten following the concerns and recommendations of the two reviewers. Thanks for taking your time to advise us on how to improve it. We understand this version is now much clear, complete, and polished; we hope it is now acceptable for your and GMD standards.

("specific comments")

Allowing for its very high-level narrative intent, the paper still needs major clarifications. At the outset, the gross characterizations of the GF scheme's type and intent should be given. It is a steady-state updraft plume model, evidently with no downdrafts - oh wait, page 5 line 19 mentions them, presumably as inverted saturated plumes of the same kind? Precipitation removal is not mentioned, except to say that it can be made "aerosol aware" via an autoconversion of some sort, and can optionally be disabled entirely for the plume with 2/km initial entrainment rate. Momentum flux is mentioned in the abstract, but never in the paper, so presumably all quantities are simply transported identically, based on plume mass flux, is that correct? Or is there some pressure treatment for momentum? How does the scheme define its cloud base, for one or all plumes (LCL of surface air?) and its parcel properties (mixture up to that LCL, or does it entrain its way through the subcloud layer?) How about cloud top - is it the highest or lowest level of neutral buoyancy for the initial entraining-only instability- probing parcel calculation with its specified entrainment rate? (2, 0.9, and 0.1 /km; these three numbers are buried in the text).

We detail the information about how the parameters for the three types of convection are defined and improved the description Please, take a look at the new section 2.

The 3 plumes are said to be successive in time; does this mean the scheme updates the state profile within a call? Merely for its own internal accounting, such that it re- turns only one tendency profile to the mother model, or is this entangled inseparably with a model's time-marching scheme? Must the host model use the same conserved variables? (presumably with some fixed constant reference values for Cp, Lv, etc.?)

The three convective types are called successively in the same time step. At his point they do not update the state successively, since we were not able yet to make a comprehensive assessment of the model performance using this approach. So far, they all use the same state of the host model and the net feedback is simply the sum of three individual tendencies produced by each one. The scheme provides tendencies for temperature, water vapor and condensates mixing ratio.

Once the cloud base and top are determined, these are somehow (it is very ambiguous) used to fit a Beta Distribution in the height (or level? or log-pressure? or pressure? Figures include all four) domain, as the mass flux (MF) profile. Presumably that defines the eddy flux of all scalars and the condensation rate? Although lines 1-7 of Page 6 make it sound like the heating and moistening rates can ALSO be specified, or that they TOO can be made smooth because MF is, that is not how tendencies work out for a smoothly varying mass flux through a sharp inversion. Beta is a two-parameter curve family on [0,1] so we are left to guess how THREE of Beta's FOUR parameters are set by (1) cloudbase cb, perhaps the LCL of some undefined parcel origination level?, and (2) one of its neutral buoyancy levels?, and (3a) perhaps something that is different from cb called "average cloud base" (line 21 of page 5) or (3b) "the average height of mass flux maximum" (line 1 of page 6)? Very unclear. Read the code, I suppose. Beta's FOUR parameters are: the mapping of [0,1] onto height, and Beta's two param- eters, leaving only one parameter "free", to be defined by ?the closure? From Fig. 3 (why are there 3 curves?), it appears that the [0,1] domain of Beta is not the same as some clipping or masking at cb and ct. From the mass flux profile, entrainment and detrainment are backed-out, although how TWO profiles are backed out of ONE is un- clear. Is the buoyancy of the new entraining-detraining plume used to revisit the initial cloud base and top, or are these retained once fixed by the constant-entrainment-rate parcel buoyancy?
Quite unclear, all of this.
The section 2 was completely redone to make clear the formulation for defining the cloud properties, entrain/detrainment rates and the normalized mass flux profile (Beta function). We also added a section for a better understanding of how the PDFs function, and how this might be used for stochastic applications or training methods.

Freezing and melting are now accounted for, which seems fine and necessary, but for some reason melting begins at -3C, which makes no physical sense to me.
Done.

("technical corrections": typing errors, etc.).
Abstract:
stochasticism, "temporal and spatial correlations", but of what? One of the poorly de- fined parameters of the beta function? This is left for futrue work, says the conclusions, so it does not belong in the abstract as a claim about this manuscript.
This feature is no longer present in the Abstract.

p3, line 21: "inversion" layer means negative buoyancy of parcel? dT/dz >0 is what an inversion means to this reader.
This expression is now clarified, thanks.

p4, cloud base and air parcel source are both mentioned with no indication of how they are defined or chosen. ine 10: w* PBL should be PBL w*
Done, thanks.

p5: Beta PDF is not really a probability distribution. Clarify that it is a distribution in the height domain.

We are following the nomenclature given in https://en.wikipedia.org/wiki/Beta_distribution, and using the PDF defined in this web page.

Lines 7-8: how are both entrainment and detrainment profiles derived from an assumed mass flux profile? Seems like an underdetermined problem.
The determination of the entr/detrainment profiles is now better informed. Please, take a look at the new Section 2.

p5, line 17-18: "set the vertical distribution of heat and mass" Huh? It is just a mass flux, right? Then tendencies of all scalars flow from there in the usual way.
That expression is no longer present in the manuscript. However, changes in the Beta parameter and the height of maximum normalized mass profiles imply changes in the tendencies. See a short discussion at the end of the new Section 2.3.

p5, line 25-26, Fig. 2: the beta distribution has TWO parameters on [0,1]. What is being shown here exactly?
We show the normalized mass flux profile in terms of the beta parameter for a given cloud base, cloud top, and maximum mass flux height. The cloud base may be determined by the boundary layer height (shallow and congestus convection) or through determination where the level of free convection is located. The assumed statistically averaged cloud top for deep, congestus, or shallow convection is determined by environmental conditions in addition to the assumed average characteristic size, given by an initial gross entrainment rate. The height of maximum mass flux is also given by environmental conditions (explained in section 2). An option exists to also supply this level as an input variable. Please, revisit Section 2 for a new, more detailed explanation.

p6, line 1: Who sets the "average height of mass flux maximum", the user or the scheme via its parameters? How exactly is a beta for fitted? This is all quite unclear, more touting of supposed virtues than explaining of algorithms.
Please, revisit Section 2 for a new, more detailed explanation.

p7, are equations (4-5) redundant with (12-13) below? Units appear to be (workfunc- tion/time) in (4), but (pressure/time) in (5). Is there something missing?
Equations 4-5 are the original formulation by Bechtold et al (2014), whereas Equations 12-13 represent the adaptation of those equations in the GF scheme. Section 2.3 was rewritten to make this clear. Thanks.

Eqs. (4) and (12) make clear that tau-bl is the STRENGTH of this term, while its tempo- ral structure is the derivative of the BL Tv (6 hours, for solar heating effects in a 12-hour daytime). So although it naively appears one is specifying a timescale with the symbol tau-bl, it is really the maginitude of a temporal quadrature component that inherits its delay timescale from the frequency-weighted frequency spectrum of partial tendencies of Tv in a boundary layer (whose upper bound pb's definition is incidentally not given). I can probably imagine some of the contortions of logic behind this choice, but let's not pretend one is specifying a delay time, like a convection organization timescale which is more what one observes as the reason for the delay of rainfall into afternoons.

The reference below describes the original development of this approach:
Bechtold, P., N.et al.: Representing Equilibrium and Nonequilibrium Convection in Large-Scale Models. J. Atmos. Sci., 71, 734–753, doi: 10.1175/JAS-D-13-0163.1, 2014.

page 12, eq (11): this must be SATURATION hbar, not hbar, correct? (so that line 22 is incorrect in words)
Done, thanks.

p14, lines 20-22. Clarify how three (out of 4) Beta parameters are sufficient to define profiles of detrainment AND entrainment. Do you mean that, for a fixed entrainment profile (constant), a detreinment profile is uniquely defined from the outcome (Beta- shaped MF profile)? That I could see, if the source of the 4th parameter of the beta function were stated. Is it part of the closure, somehow?? This is frustratingly ambiguous about the information flow.
Please, revisit Section 2.2 for a clear discussion about how those profiles are derived.

Despite the frustration of ambiguities, the brevity of the text is appreciated. These are clearly authors with important real coding work to do, above and beyond and arguably more important than writing papers. Still, this really should be improved to at least a point of clarity. The English could use a polishing edit as well. I did not enter all the typos and word misuses that I found, I too have other jobs.

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.