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Interactive comment on "Parameterizing deep convection using the assumed probability density function method" *by* R. L. Storer et al.

Anonymous Referee #1

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General Comments:

This article presents single column model results from several cases of deep and shallow convection using a new version of a convective parameterization (originally intended only for shallow convection) that uses the assumed probability density function method. The single column model results for these cases, which are cases that have been used for intercomparisons of convection-permitting models and single column models in the literature, are compared with 3-D cloud resolving simulations of the same cases using the System for Atmospheric Modeling (SAM). The new parameterization scheme simulates the environment and cloud properties fairly well compared with SAM, although there are some issues with cloud liquid amount, cloud ice amount, and precipitation efficiency. The sensitivities to some new aspects of this version of the

parameterization, as well as to time step, vertical resolution, and sub grid sampling, are also shown.

The paper is well written and well motivated, since it presents this new version of the parameterization as a step towards a unified convection parameterization for all cloud regimes. My main concerns are: 1, a few more details about methodology should be included in this paper rather than just referred to from previous works, 2, the limitations of using single column models to test convective parameterizations should be addressed, 3, the extent to which this parameterization has already been used and described in previous studies should be made clearer, and 4, some mention of observations for these cases, and how they compare with the single column model and SAM results, should be included. I address these specific comments, as well as some minor technical comments, below. I recommend publication after these points are addressed.

Specific comments:

1. I recommend a bit more explanation of the assumed probability density function method in section 2.1. Specifically, a few examples of the equations for the vertical turbulent fluxes would be helpful. The quantities listed in table 2 could be defined in section 2.1 (some of these are never defined in the paper) and a bit more information on how correlations are estimated could be included so that the reader is better prepared to understand changes in methodology that are introduced in section 2.2.1 and elsewhere.

2. Single column models can be useful to test local processes such as conversion rates from cloud droplets to precipitation (for example). However, they do not allow feedback from local processes on to the large-scale circulation. For deep convection, precipitation is usually balanced mainly by advective moisture convergence, which is prescribed in typical single column model experiments such as these. This paper does not use surface precipitation as a metric for comparison for this very reason, which is appropriate, but the profiles of environmental properties shown here will certainly be affected

by changes in the large-scale circulation in a fully three-dimensional model. It would be useful to address this and to mention any plans for testing this parameterization in a fully 3-D weather or climate model.

3. This paper should be more clear about the use of CLUBB for deep convection in Davies et al. 2013. In the introduction, it is mentioned that Davies et al. 2013 did include a PDF parameterization for deep convection, but it appears that it was actually CLUBB that was used for deep convection in that paper. This is not clear in the current manuscript. I think that the results from that paper deserve at least a paragraph describing how the version of CLUBB used in that paper compare with the version presented in the current paper. The performance of that version of CLUBB in Davies et al. 2013 compared with observations and other models should also be discussed here in this paper.

4. Since these case studies include some observational information, it should be discussed at least to some extent how well both the single column model with the PDF parameterization and the SAM results compare with the observations. This would be especially useful for quantities in which the two models disagree with each other (although it would be good to know in general).

Minor (or technical) comments:

Page 3816, line 9: do you have any idea how SAM and CLUBB-SILHS might perform with prognostic droplet number concentration rather than prescribed values?

Page 3816, line 18: have you tried interactive radiation in this model, and how does it look? How does it affect radiation fluxes? Or, if you haven't tried this, do you have plans to do so?

Page 3818, line 14: usually for deep convection, precipitation is mostly balanced by moisture convergence (by advection), not surface fluxes, I would expect this to be the case here as well. In other words, it is probably not the prescribed surface fluxes that

are constraining your surface rain rates but rather the prescribed moisture convergence from advection.

Page 3818, line 16: it looks to me like liquid water path is too high especially in the early part of the simulation, might there be a reason for this?

Page 3818, line 18: the cloud ice is not completely accurate, it is somewhat lower than SAM and slightly out of phase.

Page 3819, line 17: the models also disagree on snow, is this for the same reason?

Page 3819, line 27: here and elsewhere, including in some of the figure captions, be careful with terms like "cloud fraction" or "cloud water mixing ratio" because you are only including liquid cloud for these terms, so I would replace these with "liquid cloud fraction" "cloud liquid water mixing ratio" throughout

page 3820, line 8: is "including non-zero precipitation fraction" the same as "allowing a hydrometeor free region" as described in 2.2.1? I'm not sure how these could be the same thing, but they seem to be used interchangeably.

Page 3823, line 20: it would be nice to have a little more discussion and explanation of how these biases are symptoms of low precipitation efficiency. This is not necessarily obvious, and I think it would enhance the paper to go into a little more detail.

Table 2 caption: "an another" -> "another"

table 2: I recommend listing the variable names downwards along the left (or right) side of the table to make it easier to find corresponding correlations. Also, as mentioned in the main comments, at least six of the variables listed here are never defined in the paper or in this caption.

Figures: in general, I would make the lines thinner because some of the detail is lost and it is sometimes difficult to compare the two lines (this is especially bad in some panels of Figure 7 and Figure 15). I would also use days (for longer cases) or hours (for shorter cases) instead of minutes for the x-axes since most people are not used to interpreting large numbers of minutes (e.g. to look at the phase of the diurnal cycle). The legend boxes only need to appear once per figure, not in every panel; this will help with issues such as the fact that the legend box is blocking the curve in at least one panel of figure 1. This will also allow legend boxes to be made larger for figures 11-16, because currently the legend information is too small in those figures.

Figures 10-11: in the captions, I would refer to these figures as showing the "sensitivity to the inclusion of the new xxxx" rather than "the effect of including (or boosting) xxxx". Or alternatively, these could be labeled as "the effect of not including (or boosting) xxxx". Otherwise, it makes it confusing whether these new additions to the scheme were included in the parameterizations used for the previous figures or whether they are only being tested in versions in these figures.

Figures 11-14: please include the time range for each experiment during which the mean profiles have been averaged.

Interactive comment on Geosci. Model Dev. Discuss., 7, 3803, 2014.