Geosci. Model Dev. Discuss., 7, C2175–C2182, 2014
www.geosci-model-dev-discuss.net/7/C2175/2014/

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

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Received and published: 30 October 2014

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.

Thank you for these comments. Responses are provided below to each individual comment.

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.

We have added an appendix with the basic prognostic equation set from CLUBB for reference - this should hopefully help to address any confusion a reader may have about the terms in the equation we are referring to throughout the methods section. In addition, we have expanded the caption for Table 2 to define the terms and reworded the section on correlations that you refer to, which hopefully has improved the clarity.

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.

CLUBB has been tested within CAM5 as a boundary layer/turbulence parameterization (e.g. Bogenschutz et al., 2013). Testing CLUBB as a deep convective parameterization within CAM5 is currently in the early stages. If CLUBB were used as a fully unified cloud parameterization, the large scale forcing from the host model would provide forcing to the CLUBB model, which would then calculate cloud fractions, liquid water, precipitation, etc. which could be then available for the host model to use (e.g. for radiation or dynamics). In this way the local processes and the large scale would directly feed back on each other.

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.

Thank you for pointing out how this text was unclear. To add clarity, and some detail on

the Davies et al. study, we have changed the text as follows:

Previous: “Despite these advantages for parameterizing deep clouds, to date PDF parameterizations have been applied only to shallow clouds, except in higher-resolution cloud- resolving models (Cheng and Xu, 2006; Bogenschutz and Krueger, 2013) and except in Davies et al. (2013). The present paper takes a PDF parameterization, the Clouds Layers Unified By Binormals (CLUBB) parameterization, that has been used to simulate shallow clouds, and extends it in ways designed to better represent deep convection. In particular, the representation of the subgrid distribution of ice and precipitation is improved.”

New: “Despite these advantages for parameterizing deep clouds, to date PDF parameterizations have been applied only to shallow clouds, except in higher resolution cloud resolving models (Cheng and Xu, 2006; Bogenschutz and Krueger, 2013) and except in Davies et al. (2013). Davies et al. (2013) includes a single-column model (SCM) simulation of a tropical del convective case by the Cloud Layers Unified by Binormals (CLUBB) PDF parameterization. However, that study was an intercomparison of several SCMs and hence did not include details of the formulation or evaluation of CLUBB. CLUBB performed comparably to the other SCMs in Davies et al. (2013), but here we take the CLUBB parameterization and extend it in ways designed to better represent deep convection. In particular, the representation of the subgrid distribution of ice and precipitation is improved.”

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).

One reason we chose SAM as our “truth” for model validation is the fact that SAM was included in model intercomparisons as cited for all 3 of the deep convective cases, and performed well in general characteristics such as precipitation and liquid water path. A detailed comparison of the microphysical variables would be beyond the scope of our study. To mention the general agreement, we've changed:

"We configure the deep convective simulations as per previous model intercomparisons of those cases."

to:

"We configure the deep convective simulations as per previous model intercomparisons of those cases. SAM simulations examined here are comparable to previous simulations and observations in general characteristics such as timing, precipitation, and liquid water path. We conclude, therefore, that they provide good reference simulations for evaluation of CLUBB-SILHS."

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?

Using prognostic cloud drop number concentrations leads to small differences in the microphysics profiles, but no significant differences in the mean fields. The decision to prescribe the concentration was made to simplify the microphysical budget calculations and remove a simple source of possible differences - however the overall results would not be largely different with prognostic cloud number.

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?

We have run simulations using the BUGSRAD interactive radiation (Stephens et al., 2001). The radiative fluxes were sensitive - however the mean fields varied little. The CLUBB simulations used in Davies et al. (2013), which produced comparable results

to other SCM simulations, utilized BUGSRAD as well.

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.

To remove confusion, we changed "constrained by surface evaporation rates." to "constrained by prescribed large-scale forcings."

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?

Generally, a too-high liquid water path with a reasonable rain water path implies that the precipitation efficiency is too low. In combination with the low cloud ice that you point out, it is possible that some of this is due to an issue with the correlations between cloud and ice hydrometeors. We have stated this in the text.

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

This is a good point. As mentioned above, we corrected this in the text.

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

Yes. The large nucleation rates mean that there are large numbers of smaller ice crystals produced, leading to less efficient snow production. We added a sentence about this.

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

Thank you for pointing this out. We have made this change.

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.

Yes, though the wording was confusing here. It should have read non-unity, rather than non-zero. The precipitation fraction allows for a region of the PDF that is hydrometeor free - hydrometeors including cloud ice, snow, graupel, and rain - whereas the previous formation essentially had a precipitation fraction equal to one (with all hydrometeors occurring througout the entire region). We have changed the wording in this paragraph to be more consistent with the description in 2.2.1.

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.

We have added the following sentence further clarifying what we mean by precipitation efficiency: "That is, the amount of precipitation produced in CLUBB-SILHS for an amount of condensate is too low in these cases - leaving too much cloud water remaining compared to the SAM simulations."

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

Thank you for pointing this out. We have fixed this.

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.

Thank you for pointing this out - we have added the extra column and defined the variables in the caption for clarity.

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.

Thank you for these suggestions. We have re-done the figures and legends to make them clearer to see.

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.

We have made this change, thank you for mentioning this confusion.

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

They have been averaged over the same time periods as used in previous figures - we added a note of this to the captions.

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