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Interactive comment on "A unified parameterization of clouds and turbulence using CLUBB and subcolumns in the Community Atmosphere Model" *by* K. Thayer-Calder et al.

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The authors wish to begin by thanking the anonymous reviewer for volunteering their time to write this review.

This paper introduces a sub-column variant of the Cloud Layers Unified By Binormals (CLUBB) parameterization and provides a number of key modifications to the previously published formulation to better unify the treatment of cloud types. The manuscript is quite interesting and an important contribution to the literature.

However, the explanation is terse and leaves the reader wanting of many key details relevant to this work. The authors may wish to consider expanding on references to

the background literature on PDF-based parameterizations and sub-column methods, and clarify the description of the methodology.

CLUBB and SILHS have been thoroughly described in a number of prior papers, but those description papers weren't singled out as such in the submitted manuscript. The manuscript now writes that:

"CLUBB's methodology is overviewed in Golaz et al. (2002), and an up-to-date listing of CLUBB's equations is contained in Storer et al. (2015)."

"SILHS' methodology is described in Larson et al. (2005) and Larson and Schanen (2013)."

Nonetheless, I am happy to recommend the manuscript for publication subject to minor revisions as long as the comments and questions below are addressed.

Page 5047, line 2-10: Please provide some brief justification of the given choice of prognostic moments and the marginal distributions. How are mean and variance of these distributions chosen?

The choice of prognostic moments is dictated by the physical quantities that we want to diagnose from them: "The inclusion of r_t and θ_l allows both moisture and temperature fluctuation enter the diagnoses of cloud fraction and cloud water mixing ratio. The inclusion of w allows the buoyancy flux, $\overline{w'\theta_v'}$, to be computed consistently with cloud fraction and cloud water."

The PDF shape is chosen based on agreement with observations and LES: "The marginals of w, r_t, and θ_l are normal mixtures, that is, the sum of two Gaussians. This PDF shape has been shown to compare favorably with aircraft observations and large-eddy simulations of stratiform, shallow cumulus, and deep cumulus clouds (Larson et al., 2002; Bogenschutz et al., 2010). The marginal PDF for r_i and N_i is a delta double-lognormal. That is, the PDF shape for ice is the sum of a delta function representing the ice-free area and the sum of two lognormal distributions. This PDF shape has recently been evaluated against large-eddy simulations (Griffin and Larson, in preparation)."

The means and variances are prognosed rather than diagnosed or prescribed. The relevant papers are now cited: "CLUBB adds prognostic Reynolds-averaged equations for the following moments: $\overline{w'\theta_l'}$, $\overline{w'r_t'}$, $\overline{w'^2}$, $\overline{w'^3}$, $\overline{r_t'^2}$, $\overline{\theta_l'^2}$, $\overline{r_t'\theta_l'}$ (Golaz et al., 2002; Larson and Golaz, 2005)."

Page 5048, line 1: Please provide a brief contrast of MG1 and MG2, and provide an explanation as to why MG2 was not used. Would there be any advantage in adapting CLUBB-SILHS to work with MG2, or any other microphysics scheme for that matter?

The revised manuscript reads: "Each subcolumn is fed into Version 1.0 of the Morrison-Gettelman (MG1) microphysics scheme (Morrison and Gettelman, 2008). MG1 provides a simplified initial test for the subcolumn methodology because MG1 diagnoses rain and snow. Therefore, rain and snow are not inputs to MG1, and hence the subcolumns need not contain rain or snow variates. In the future, we hope to use SILHS with Version 2.0 of Morrison-Gettelman (MG2) microphysics scheme (Gettelman and Morrison, 2015; Gettelman et al., 2015). MG2 prognoses rain and snow, and hence using it will require us to draw subcolumns with rain and snow. Although this will add complexity and expense, the higher-dimensional PDF will offer greater control over processes that involve two or more hydrometeor species, such as accretion of cloud water by rain water."

Page 5048, line 23: Do the tendencies computed from sub-stepped CLUBB feed into the dynamics on the sub-cycled time scale or on the physics time scale?

Line 190 states "The subcolumn-averaged microphysical tendencies are fed back into the host model at the end of the physics time step." There is no sub-cycling of CLUBB and MG microphysics in this version of the model.

Page 5051, section 3: How representative is this cost of operational model perfor-

mance? Has much effort gone into optimizing CLUBB-SILHS? Do you anticipate any performance gains could be made to the present code?

Thanks for the excellent question. We've tried to avoid obvious inefficiencies in the coding, but we don't have a good sense of how much code optimization would help. Unfortunately, it would be a major project to answer your question reliably. So we are reluctant to provide a guess in the paper.

However, we have worked on reducing sample noise. The revised manuscript now includes the following paragraph: "It is currently unknown how much the cost per subcolumn can be reduced by optimization. Another way to reduce the cost is to draw more representative subcolumns, so that fewer subcolumns are needed. In the future, we will evaluate a new sampling method that produces equal accuracy with about half as many subcolumns (Raut and Larson, 2015)."

Page 5058, line 15: In SP-CAM, there is also a sensitivity to the number of sub-columns that is analogous to the response observed in CLUBB. In SP-CAM communication can occur laterally between sub-columns, and consequently when few sub-columns are present there is insufficient area for compensating subsidence, which suppresses convection and drives unphysical results. In the CLUBB-SILHS case I could imagine a similar effect would occur. Namely, is it the case that with insufficient sub-columns the vertical profile will be closely locked to the grid-cell mean and so will be unable to develop convection?

Thanks for the interesting question. We don't think that the vertical profile will necessarily be locked to the grid-cell mean, but it seems likely that, with only a few sampling points, the sampling of the tails of the distribution will be noisy, at best. This might lead to less precipitation formation, which might have similar symptoms to the suppression of convection in SP-CAM. Unfortunately, it is hard to say anything reliable without extensive analysis.

In order to at least touch on the issue, the revised manuscript states: "The 4-subcolumn simulation appears to have a lower precipitation efficiency than the 10-subcolumn simulation. The reason, we speculate, is that use of a limited number of subcolumns leads to poor sampling of the tails of the distribution, which is where precipitation forms and grows."

Page 5060, line 8: How would you anticipate the parameterization will behave as grid resolution is reduced to 0.25 degrees, 0.1 degrees, or finer? Is there a natural mechanism that could be used for deactivating the parameterization as the resolved scales are reduced?

In theory, there is reason to expect that PDF parameterizations will prove to be fairly insensitive to grid spacing. In practice, it would require extensive computer resources to test this thoroughly.

The revised manuscript adds a sentence: "As cloud-resolving resolutions are approached, CLUBB is designed to gradually shut itself off by reducing its turbulent dissipation time scale (Larson et al., 2012). Whether in practice the output of CAM-CLUBB-SILHS proves to be sensitive to significant changes in resolution is left for future work."

Interactive comment on Geosci. Model Dev. Discuss., 8, 5041, 2015.