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**Geoscientific
Model Development
Discussions**

Interactive comment on "A dynamic probability density function treatment of cloud mass and number concentrations for low level clouds in GFDL SCM/GCM" *by* H. Guo et al.

Anonymous Referee #2

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This paper presents initial tests for the new joint treatment of boundary layer turbulent and cloud processes (CLUBB) in the GFDL AM3 single column model (SCM). The performance of the new scheme, or more precisely, the droplet activation part of the scheme, is demonstrated using three cases with different cloud types and cloud fractions, with an LES model serving as a benchmark. The tests include simulations with two aerosol loadings as well as CLUBB runs at low and high resolutions. While treating subgrid vertical motions is clearly necessary for any realistic SCM simulations of clouds, the goals and benefits of the specific approach needs to be identified more clearly to be useful for the modeling community. The outlined model development seems viable, but a major revision of the manuscript is needed to bring it to the

publication level.

General comments:

1. The study is motivated by the need to have a droplet activation scheme driven by the sub-grid turbulent motions. Other models have use pdfs of vertical velocity to predict droplet activation. As pointed out in the manuscript, such pdf often take a form of a Gaussian distribution with a width related to some measure of turbulence intensity (e.g., TKE). The CLUBB treatment discussed here is different because it uses a pdf which is bi-modal and multi-variate. Unfortunately, neither feature is discussed in the context of droplet activation. A double Gaussian vertical velocity pdf is quite apparent in figure 4 but never mentioned in the paper. A multi-variate nature of CLUBB's pdf is mentioned and reflected in Eq. 3, but its role in treating droplet activations is not discussed. These are the two unique aspects of the new treatment, which this work should focus more instead of concentrating on a comparison with a somewhat artificially simplified parameterization with a prescribed updraft.

2. The main conclusion of the paper, that the proposed implementation is promising and feasible, is rather weak. What aspects of the simulations were improved using the new scheme? What is the reason for these improvements? Does the bi-modality or the use of a joint vertical velocity – temperature – moisture pdf plays a larger role? In the introduction it is mentioned that the droplet number transport is also handled by CLUBB. Does this have any effect on the results?

3. Adopting a higher order turbulence closure parameterization obviously requires extra computations. How much does the CLUBB slow down the SCM?

4. The sensitivity of the simulations to CLUBB's vertical resolution is an interesting aspect of the study but needs to be put into context. The changes appear to be not that large – much smaller than the difference between the SCM and LES benchmark. Does this improvement worth extra computing power? Also, since one would expect the simulations to improve at higher resolution, should the high resolution CLUBB be

compared with the high resolution SCM with a diagnostic sigmaw treatment?

Specific comments:

1) Consider a more specific title since the manuscript covers only one aspect of the number concentration treatment (i.e., droplet activation). Also GCM could be removed from the title; otherwise readers may expect to see results from global simulations.

2) The meaning of ”dynamic pdf” or ”dynamics-pdf” in title and text is not clear. Is it the same as “multi-variate”?

3) p. 551, ln. 4: A plot of time series of cloud fraction or liquid water path could be useful to illustrate the “quisi-steady states” of the cloud fields.

4) p. 552, last paragraph: Aerosol activation spectrum, or, at least, a size distribution spectrum would be helpful to show in addition to providing the mass loadings.

5) p. 553, lns. 20-25: I am not convinced that it is justified to abandon a more realistic diagnostic treatment for the sigmaw in favor of a constant sigmaw for the sake of simplicity. Is this what is used in GFDL GCM? If not, then why not use a TKE-diagnosed sigmaw?

6) p. 555, ln. 5: Do you mean the positive skewness is indicative of turbulent structure of a convective boundary layer?

7) p. 556, lns. 3-5: Are there any global models that use a constant velocity for droplet activations? If so, a reference is needed here.

8) Figure 3: The two dark-colored lines are hard to distinguish. Consider changing color or using markers to make these lines more easily identifiable.

Interactive comment on Geosci. Model Dev. Discuss., 3, 541, 2010.