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Interactive comment on "A new subgrid-scale representation of hydrometeor fields using a multivariate PDF" *by* Brian M. Griffin and Vincent E. Larson

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In this response to the reviewer's comments, the reviewer's comments are italicized, and our responses are in roman font.

Anonymous Referee #2

Overall comment

This paper provides an overview of the development of an extension to a PDF-based microphysics and cloud paramaterization method. The key development being the inclusion of flexibility to allow for cloud-free regions where there are no hydro-meteors. Although sections 2 and 3 are mathematically pretty heavy going, they are required to

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fully document the method being described.

Thank you for your review.

Minor comments:

1) Presumably the LES simulations used here do not have time-evolving forcing. I presume they are runs for a period of time until some form of equilibrium is reached. If that is case, it may be worth clarifying that these are run with non-evolving forcing.

The shallow cumulus (RICO) and marine stratocumulus (DYCOMS-II RF02) cases have steady-state forcing, but the shallow-to-deep-convection transition case (LBA) evolves rapidly. It goes from no cloud to deep convection in 6 hours.

2) If the simulations are indeed for steady-state conditions, could you discuss how applicable your method may be to time-evolving situations. Although the Tompkins scheme is dealing with a different problem, that scheme discusses the ill-posed nature of that mathematical framework at the point when cloud first appears in a previosuly cloud-free environment. Could you discuss whether your approach could deal with simulating the transition from clear to partially cloudy and the transition from precipitation free to precipitation in a portion of the domain. Is all of the math well-posed for these scenarios.

Yes, the math is well posed during the transition from precipitation-free to precipitation. The problem with the Tompkins scheme is that it uses cloud water mixing ratio as a predictor for cloud fraction, and cloud water is zero in clear skies. CLUBB doesn't do this; it uses moments instead, and the moments are always predicted, even in clear skies. Hence the information about variability that is needed to initiate cloud is always available.

The revised manuscript adds an explanation: "Although f_p is provided by the LES for this study, it can be diagnosed based on the cloud fraction using a method such as that of Morrison and Gettelman (2008). If the cloud fraction, in turn, is diagnosed

based on the omnipresent prediction of means, variances, and other moments — as in higher-order moment parameterizations such as CLUBB — then the onset of partial cloudiness is well defined and indeterminacy about the time of cloud initiation is avoided. In contrast, parameterizations that diagnose cloud fraction based on, e.g., cloud water mixing ratio, lack crucial information in cloudless grid boxes, as discussed in Tompkins (2002) . The well-defined onset of CLUBB's cloud fraction is inherited by the precipitation fraction."

3) Although there is mention of horizontal correlation between species, for example for calculating accretion, there is no mention of vertical correlation and vertical overlap. Presumably the LES simulations have some vertical coherence and precipitation formed in cloud is more likely to be in a cloudy part of the domain as it falls to the next level down. Is this effect considered in your present work, or do falling precipitation particles experience a randomly selected portion of the layer they fall into. Please clarify whether and how you take this into account.

The present work does not consider sedimentation, only instantaneous snapshots of PDFs and local processes like accretion and evaporation. Hence vertical overlap doesn't enter our calculations. However, Larson and Schanen (2013) have developed a method to parameterize vertical correlations for CLUBB, and this manuscript is cited in the revised manuscript: "Although the multivariate PDF allows for the calculation or specification of the (horizontal) correlation between any two variables at the same grid level, the PDF does not contain information about vertical correlations. Vertical correlations can arise in calculations of radiative transfer, diagnosed hydrometeor sedimentation, or other processes that involve the correlation of a variable with itself at different vertical levels. Such processes are excluded from this study, and hence information about vertical correlations is not needed here. For one possible method to parameterize vertical correlations, see Larson and Schanen (2013)."

Typographical comments:

4) Line 75 and elsewhere. Instead of "in precip." and outside precip." I suggest using the un-abbreviated for "in precipitation" to avoid some periods appearing mid sentence. Suggest changing this through-out document.

In the revised manuscript, “in-precip." has been replaced everywhere by “in-precipitation", and similarly for “outside-precip."

*5) Line 331 need rephrasing "The value of * can now be solved for through Eq 27."*

We have rephrased "The value of * can now be solved for through Eq 27." to "The value of * can now be found using Eq 27."

6) Line 385. What was the model top?

We’ve added the following sentence to the revised manuscript: “The model top was located at 4000 m in altitude."

7) Line 398. Suggest "and covering a domain of DEPTH 159.3 m." Similarly line 406 "a domain of DEPTH 27500m."

The wording has been changed as suggested.

8) Line 455. Suggest "much closer match subjectively. A quantitative assessment will follow in the next section."

The section now reads “The DL and DDL PDF shapes provide a much closer match qualitatively to the SAM data. A quantitative assessment of the quality of the fit will follow in Section 5.1."

9) Line 495. Perhaps remind reader that a less subjective assessment will follow.

We have added the sentence “The fit will be quantified in Section 5.1."

10) Line 493. Presumably this height was chosen as the hydro-meteors are all liquid-only at this height?

Yes, this height contains only liquid. The revised manuscript adds the sentences “In

order to assess how well the PDF shapes are able to capture ice PDFs as well as liquid PDFs, we turn to the LBA case. In LBA, liquid and ice appear at different altitudes and times."

11) Near line 570, why are different time-averaging windows being used?

The ice-phase hydrometeors appear only later in the simulation, and when they do appear, their values increase and decrease with time differently. This calls for the use of different time-averaging windows. The revised manuscript now states: “The LBA case contains both liquid and frozen-phase hydrometeor species that evolve as the cloud system transitions from shallow to deep convection. The various hydrometeor species develop and maximize at different altitudes and times, so different periods and altitude ranges are chosen for averaging test scores for each species."

12) Line 565, no need for period after abbreviation of meter.

The period has been removed, as suggested.

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