Initial Review of "Improving the representation of shallow cumulus convection with the Simplified Higher-Order Closure Mass-Flux (SHOC+MF v1.0) approach" by Maria J. Chinita, Mikael Witte, Marcin J. Kurowski, Jaoa Teixeira, Kay Suselj, Georgios Matheou, and Peter Bogenschutz

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1 General Comments

This manuscript presents a nice discussion of the EDMF and PDF-based HOC PBL and shallow convection literature, describes a new combination of physics schemes to improve on SHOC, and presents relatively preliminary results of using this combination in a SCM to simulate two shallow cumulus cases. It contains excellent writing and grammar and the authors are able to convey their main points in a concise manner, appropriate for the chosen journal. My recommendation would be to accept the paper after minor revisions despite what I perceive as a few major weaknesses of the manuscript, described below. The reason for my recommendation is based on the demonstrated efficacy of the described approach and a recognition that it has *potential* to make a substantial improvement to future GCM/NWP models if further developed.

2 Specific Comments

1. I'm concerned that this approach is conceptually "double-counting" effects due to the largest, coherent turbulent eddies. My understanding of the SHOC scheme is that one of the Gaussian components of the underlying PDF is already supposed to account for the type of subgrid-scale PBL-spanning updrafts that the MF scheme is also trying to represent. If I remember correctly, although the complexity of SHOC is boiled down to a K-theory implementation (as evidenced by the first term on the RHS of Eq. 3, 4, 9, 10), several of its underlying assumptions, notably its length scale and the "updraft" portion of the trivariate binormal PDF, are formulated to try to represent the same physical phenomenon that the MF scheme is. I.e., in regions of the column where PBL-scale convective eddies are present, K is substantially increased in SHOC in order to try to represent the effects of those eddies. The need for an additional turbulent transport "boost" from a MF component potentially speaks to the inherent limits of SHOC's approach of maintaining first-order closure (neglecting the TKE component that gives it a 1.5-order closure) at its heart. The manuscript comes close to touching on this point in several places, but never explicitly discusses it. For example, on lines 83-84 where it mentions SHOC represents "local mixing", on lines 197-199 mentioning the down-gradient term from SHOC, and lines 246-248 that describes the modifications to SHOC to achieve satisfactory results. In my reading of this paper, this thought was a through-line that I feel needs to be addressed/discussed. The fact that lines 246-248 are put in the paper lead me to believe that the authors are aware of this issue, but the lack of detail (how was SHOC's length scale reduced, which constant was increased and why, sensitivities to these changes) elicits an impression of "sweeping this under the rug", so to speak. A discussion/explanation doesn't even have to be scientifically/phenomenologically-motivated necessarily. It may suffice just to say that this is a pragmatic approach to patching a "weakness" of the SHOC formulation, etc.

2. The applicability of the paper is limited by the chosen cases. The readers are definitely left "wanting more" in the sense that no indication is given for how the new scheme performs for stratocumulus, deep convection, frontal cloudiness, clear/dry convection, stable PBLs, mixed-phase cloudiness cases, etc. The manuscript as presented is fairly typical in its scope in this regard, and this criticism applies generally to other similar papers, but it is worth pointing out. I'm not saying that the authors need to expand the scope for the particular paper, just that it is much less exciting/convincing without more meteorological regimes (higher "N") to go on.

3. There are components of the scheme that seem arbitrary to the reader. For example, line 144 describing which part of the PDF is used for the MF model with $[1.5\sigma_w - 3\sigma_w]$, or 6.65%. Can this be justified? E.g., why not use the entire part of the PDF with $w > 0$?

4. I'd like a more physical explanation for the formulation of L_ϵ, which I have interpreted as the "mean free path" between entrainment events. It seems like this should be related to the local strength of turbulence, e.g. TKE. I realize that h_{CBL} might be a proxy for TKE, but how it is used seems to be opposite to my intuition. I would think that high TKE (and high h_{CBL}) would lead to more frequent entrainment/mixing events, creating a *lower* L_ϵ. This leads me to think that this formulation was "tuned" to get the desired scheme behavior/results rather than using physical reasoning. A more thorough explanation (perhaps it is in the Suselj 2019 paper) for Eq. 8 would be beneficial to the reader.

5. Lines 53-67 discuss EDMF schemes more broadly and the last couple sentences imply that the SHOC + MF approach is on par computationally to existing EDMF implementations. Is this claim accurate? It would be nice to have some numbers to back this up if so.

6. Lines 98-99: Why is C++ code better than Fortran code in this case? Seems strange to make this claim in this paper.

7. Line 115: What are "fluctuations" with respect to? Time, space, both? I'm assuming just space, e.g. fluctuations from the spatial, grid mean values.

8. Line 169: It is perhaps awkward to have two different length scales. How does L_ϵ related to L used in SHOC?

9. Line 213: I'm confused by this. Doesn't SHOC produce tendencies for u, v like other PBL schemes? Are these not applied too?

10. Line 217: Would it be correct to say that physics then is ONLY using SHOC + MF?

11. Figure 1: Aren't there observations for these case studies? Why are they not plotted alongside LES results?

12. Figure 2: Doesn't SHOC have a binormal PDF? Could you also plots means from the "updraft" component of the PDF? Also, it's explained why there are no data points for SHOC + MF at $z >= 1.5km$, but why wouldn't you plot all lines returning to 0? Shouldn't they all do that at some height?

13. Figure 3: It's pretty hard to see the LES curves. It might be better to plot the bias (difference from LES) for clarity.

14. Figure 6a,b: There are discussions of oscillations like this in the literature. IIRC, Anning Cheng mentions cloud water oscillations related to IPHOC, so it might be worth mentioning that these oscillations are not unique. Also, why do these oscillations not show up in the means in figures 4a,b?

15. Figures 8,9: Same comment about potentially plotting biases for clarity since all lines are on top of each other.

3 Technical Corrections

1. Line 77 Instead of “PDFs”, it is more appropriate to say Gaussian components or modes. The PDF should mean the entirety (weighted sum of Gaussian components).

2. Line 121: The second approximation is worded strangely in my opinion. You’re talking about a small updraft area but the mentioned term doesn’t even contain updraft area. I think it would be better to say that the environmental fraction is large such that $w_e \approx \overline{w}$, etc.