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Comment on gmd-2022-162

Anonymous Referee #1

Referee comment on "Improving the representation of shallow cumulus convection with the simplified-higher-order-closure-mass-flux (SHOC+MF v1.0) approach" by Maria J. Chinita et al., Geosci. Model Dev. Discuss., <https://doi.org/10.5194/gmd-2022-162-RC1>, 2022

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, Joao Teixeira, Kay Suselj, Georgios Matheou, and Peter Bogenschütz

The goal of this paper is to test whether the addition of a mass flux scheme, which represent non-local transport by updrafts, to the turbulence scheme called SHOC improves the representation of shallow cumulus convection by the SCREAM climate model.

For this purpose, the authors use a single-column model with the SCREAM model physics, and with or without the multiplume mass flux scheme developed by Suselj et. al (2013, 2019a,b). By simulating two cloudy boundary layers and comparing them with large-eddy simulations, they show that the addition of this mass flux parameterization improves the representation and evolution of cloud features and turbulent transport.

Knowing that uncertainties in the representation of low clouds by climate models remain important, it is still very valuable to continue working on improving these clouds. The paper presents interesting results, a relevant methodology, and is well written. Therefore, the article is very suitable for publication. I'm nevertheless concerned about the potential implementation of the SHOC+MF in the parent 3D climate model for several reasons: the adjustment of tunable parameters, the relevance of the EDMF scheme in a model that aims to simulate at high resolution (scale awareness), and the coupling with other parameterization such as radiation, and microphysics. These points are not addressed in this study. More information about would be very valuable for the community.

Major comments:

- I know that it could be complicated, but I really find it problematic that the authors avoid the question of tuning when testing their new parameterization. Here there are several parameters that need to be adjusted when combining SHOC and MF schemes, e.g. number of plumes N , ϵ_0 , 'a' for the entrainment length scale, ... The authors justify the tuning parameters' values by the original studies that set them. However, this study highlights a coupling between parameterizations that might suggest that these values may be outdated. Without asking the authors to test all tunable parameters, I would like to know if (1) values they use remain physically consistent in their new SHOC-MF framework, and (2) if their conclusions remain similar when some important tunable parameters are modified. I guess that the second point is feasible given the simple SCM framework the authors use.

- The authors show improvements when implementing the MF scheme in SHOC. However, the heat and moisture transports remain biased low (Fig 8-9). Could the authors describe improvements to reduce this bias? Would it be possible to reduce it by a better tuning strategy or switching on radiation scheme?

Minor comments:

- I understand that removing some parameterization schemes is useful to highlight the novelty of SHOC+MF. Yet I'm surprised that the authors removed the radiation schemes. I would assume that some large-scale cooling and drying forcing are imposed in large-eddy simulations. Removing these schemes would also compromise the ability of the model development to be used in the parent global climate model.

- Figures 1+5: I'm confused with the notation. You have two simulations SHOC (A), and SHOC+MF (B). Therefore, you plot results from A and B experiments, but also the relative contribution of SHOC and MF in turbulent fluxes (we can call it $B[\text{SHOC}]$ and $B[\text{MF}]$). MF in Figure 1 seems to be $B[\text{MF}]$, but this is confusing because MF could also be understood as the difference $B-A$ in Figure 1. I guess that non-linear interactions could make these contributions different (i.e. $B-A \neq B[\text{MF}]$). Could you clarify this a little bit (differences between experiments vs the MF contribution that is saved from SHOC+MF) ?

- Line: 311: Could you use a single time notation, either "hour XX" or "+YYh" relative to the start of the simulation (as done in Fig 4).

- Figure 8: I don't think the dashed and the dotted-dashed lines are described.