Geoscientific Model Development Discussions

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Comment on gmd-2022-162

Anonymous Referee #2

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-RC2, 2022

This manuscript describes the implementation of a stochastic, multi-plume Eddy Diffusivity Mass Flux (EDMF) boundary layer parameterization in the SCREAM GCM, using the Simplified Higher Order Closure (SHOC) to calculate eddy diffusivity and cloud properties. Single column model experiments are used to evaluate the scheme against large eddy simulations for marine shallow cumulus and continental convection cases. Improvements are shown relative to experiments using SHOC alone.

The topic is certainly of scientific interest, as boundary layer clouds remain one of the largest sources of uncertainty in future climate projections, and boundary layer parameterization is a topic of active research. The paper is well written and logically organized. However, a few details are missing that are needed for readers to interpret and reproduce results, as noted in comments below. I believe these can be addressed with minor revisions.

Minor Comments:

1) There are some discrepancies in cloud top height in the continental convection case between this study, Bogenschutz and Krueger (2013) describing SHOC, and Brown et. al. (2002) documenting the continental case. The two previous papers show LES cloud top heights around 2800 m, while in Fig. 7 here the deepest cloud tops are close to 2400 m. I am wondering if there are differences in case specification that might explain the LES difference, or if it may be due to the use of different LES codes and grid spacings? This has implications for the conclusion that SHOC-MF matches the LES, while SHOC by itself produces too-deep clouds. The original SHOC (Bogenschutz and Krueger, 2013) appeared to match LES in this case fairly well, so the different behavior here warrants some discussion.

2) Additional details are needed regarding the SHOC length scale and how it has been changed since Bogenschutz and Krueger (2013). The length scale formulation can have a major impact on PBL behavior, and should be described in the paper.

3) It is not entirely clear how convective clouds are treated in this study. Is there any special treatment of cumulus cloud detrained from the mass flux? Or do the updrafts impact clouds only indirectly, through the mean state? This could be noted in Section 2.

4) The experiments here used N=40 updrafts, while previous papers (e.g., Suselj et al 2013) have typically used a smaller number (N=10). Was N=40 chosen to reduce the effects of stochasticity, and do the results show any sensitivity to the value of N?

5) Are the simple updraft microphysics mentioned on lines 61-64 included in this implementation? If so, do they have a non-negligible impact? This is relevant to the comparison with LES, for which precipitation was disabled.

6) On Line 247, which constant in SHOC was increased to reduce the mixing length, and by how much?

L113: Typo: Eqn 1 is missing a “partial” symbol.