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Comment on acp-2021-757

Anonymous Referee #2

Referee comment on "Technical note: Parameterising cloud base updraft velocity of marine stratocumuli" by Jaakko Ahola et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2021-757-RC2, 2021

This is an interesting study that attempts to improve the parameterization of cloud-base updraft speed for marine stratocumulus using a large ensemble of large-eddy simulations (LES) driven by a GCM. Three methods are examined: (1) a linear physics-based scheme developed by a previous study, (2) a machine learning method that incorporates the (1), developed by the authors, and (3) a purely machine-learning-based method without incorporating physics, also developed by the authors. By comparing these three methods, it is found that (2) and (3) markedly outperform the (1), suggesting their potential usage for improving cloud-base updraft simulation in more coarse-resolution models. Some analyses about which predictor is more dominant were conducted.

Overall, the manuscript is clearly written and straightforward to understand. The conclusion that the new parameterizations improve the previous one is robust and convincing. Also, the data of the large ensemble of LES, which are released publically, should be very useful datasets. However, there are two major gaps that undermine the scientific quality, with one regarding the sampling issue and one regarding a lack of physical interpretation of the results. If these two issues can be well addressed, the manuscript should be a good fit for the ACP. See my detailed comments below.

Major comments:

(1) Marine stratocumulus only occurs under certain large-scale environments (e.g. see Wood (2012)'s review article). Your samples include many cases with delta_thetal of only several K degrees (Fig. 2b). Under this condition, an overcast stratocumulus deck can hardly sustain because of the strong cloud-top entrainment under a weak temperature inversion. Moreover, under weak inversions, the strong entrainment of warm air from above into the boundary layer, stably stratifying (or decoupling) the boundary layer so that your assumption of well-mixedness becomes invalid. Although you removed some of these cases (in Lines 244-251), the standard seems arbitrary to me and it cannot guarantee the remaining samples are physically reasonable since they are sampled based

on a purely statistical method. I would suggest plotting a map showing where those samples are, so that the readers can have a sense of what kinds of stratocumulus we are looking at: eastern subtropical stratocumulus deck? or stratocumulus in the postfrontal region of midlatitude cyclones? or polar stratocumulus under an unperturbed environment? The point is to build a physical context for understanding those samples. Actually, Zheng et al. (2016)'s study is limited to only two regions, which does not necessarily guarantee a universal relationship.

(2) I feel the discussion of the results is somewhat superficial. For example, entrainment is a crucial process governing the boundary layer dynamics. Stronger entrainment of warm air above can stabilize the boundary layer, reducing the turbulence. Such an important process, however, is not even mentioned in the discussion. Another important process is evaporative cooling that can also promote instability and enhance turbulence. Discussions should include these fundamental processes that contribute to the TKE budget of the boundary layer.

Minor comments:

L335: "developed three xxx". I think the first method is developed by previous work. The wording should be revised.