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Comment on acp-2021-113

Anonymous Referee #2

Referee comment on "Tracking the influence of cloud condensation nuclei on summer diurnal precipitating systems over complex topography in Taiwan" by Yu-Hung Chang et al., Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2021-113-RC2>, 2021

I have reviewed "Tracking the influence of cloud condensation nuclei on summer diurnal precipitating systems over complex topography in Taiwan" by Chang et al. The manuscript describes a study using a large-area, high-resolution model to investigate the effect of cloud condensation nuclei on orographic precipitation. The use of an ensemble of cases and of an object-tracking algorithm represent a significant advance over the typical case study setup. I recommend publication after my concerns below have been addressed.

(1) By and large, I think the focus on a fairly tightly constrained weather system ("orographically locked" precipitation with weak synoptic forcing) and the use of a reasonable-size ensemble of cases are good first steps in the direction of robust conclusions. I would like to see more discussion in the manuscript of remaining uncertainties, however. For example, microphysics uncertainties can still have a very large impact even when the dynamics is fairly well constrained (e.g., White et al., <https://doi.org/10.5194/acp-17-12145-2017>).

(2) The authors describe their results as "consistent" with the Rosenfeld et al. (2008) hypothesis and leave it at that. I consider this a missed opportunity to test the Rosenfeld hypothesis -- which is highly controversial -- more deeply. If I am not mistaken, previous work by Grabowski with the same microphysics scheme concluded that the latent heat of freezing is not sufficient to counter the potential energy expenditure of lofting the liquid water mass above the freezing level. Thus, I am a bit surprised by the authors' conclusion, and I think they are doing themselves a disservice by not digging deeper. For example, if they disable freezing processes, do they still obtain the same invigoration signal (i.e., would warm rain invigoration suffice as a mechanism to explain the model behavior)? One of the advantages of modeling studies over observations is that they allow these types of process denial studies.

(3) This might be more an indication that I am braindead than anything wrong with the manuscript, but I did not follow the northern/southern locked/not locked argument. I can

see that there are two regimes, one where the strongest precipitation occurs in the northern region and one where it occurs in the southern region (though I would be really curious how sharp this distinction is in the two ensemble members closest to the regime split). But I don't understand why precipitation in region N during a southern case can't still be orographically locked.

(4) I think the authors could make a stronger case for their method by describing the advantage that object tracking conveys. For example, what conclusions does their object tracking analysis permit that they could not have drawn from an old-school, area-mean analysis?

Minor comments:

- Perhaps I missed it, but how interactive are the CCN? I.e., are they depleted by wet scavenging?

- l. 29: Albrecht (1989) actually discusses both stratocumulus and shallow cumulus. Perhaps replace "stratocumulus" with "warm clouds"?

- l. 29: The Quaas et al. (2020) review is mostly concerned with the Twomey effect, not precipitation. Other reviews, perhaps Mulmenstadt and Feingold (2018, <https://doi.org/10.1007/s40641-018-0089-y>), provide a more general discussion of regime dependence in ACI.