

Interactive comment on “Vertical profiles of droplet effective radius in shallow convective clouds” by S. Zhang et al.

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This manuscript uses effective radii from LES simulations of shallow convective clouds to evaluate the merit of the Rosenfeld and Lensky (RL; 1998) and Lensky and Rosenfeld (LR; 2003) technique, in which satellite-retrieved cloud-top re are assumed to represent the in-cloud re at that altitude of clouds attaining greater heights.

The major concern I have is that it is not at all clear that the cloud ensembles examined by the authors are representative of the clouds investigated by Lensky and Rosenfeld. Shallow, broken, non-opaque clouds of small dimensions are arguably the most challenging of all clouds for conventional satellite remote sensing. The authors need to make the case that it is fair to apply conclusions drawn from their well-characterized clouds (because they are model output) to those clouds whose satellite-retrieved re

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has some information content - probably larger, thicker clouds than those examined here. Conversely, currently the paper appears to imply, if indirectly, that cloudtop re can be retrieved with reasonable accuracy for clouds in nature with similar properties to those of their simulated clouds. Is this the authors' intent?

My other comments are relatively minor.

Given the importance of the Rosenfeld and Lensky work for motivating this project, it seems valuable to devote more time to explaining the motivating texts (this may also articulate the contribution of the current manuscript more fully).

On p. 30977, we are told "Cloud-top re is used to construct a profile of re and compared to the re profile from all cloud samples for each case". I read the paper a couple of times before I realized what this sentence meant - initially I thought some sort of a vertical profile model of re was implied. It would be worthwhile (and would fit in with the previous comment) to explain this paper's and the RL & LR approach more fully - perhaps even with an example figure.

Section 3.2 and 3.3, on the evolution of re profiles in growing and decaying clouds, needs to be tied in better to the paper's motivation (assessing LR and RL's technique). Do the authors believe their subadiabatic fraction/re values reflect those of the clouds evaluated by RL and LR ?

The abstract needs to mention the motivation for the work.

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