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Interactive comment on "A-Train estimates of the sensitivity of warm rain likelihood and efficiency to cloud size, environmental moisture, and aerosols" *by* Kevin M. Smalley and Anita D. Rapp

Anonymous Referee #2

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This paper is a useful analysis of the production of warm rain in cumulus clouds based primarily on cloud and rain water measurements from the CloudSat and MODIS satellite datasets. The main new result is that the efficiency of production of warm rain appears to increase with the horizontal size of the cloud, even when controlling for variations in cloud depth and sea surface temperature. The results imply that dilution of cloud updrafts due to entrainment is less effective in larger clouds than smaller clouds which are presumably better protected by the larger scale of the clouds. This is a plausible hypothesis supported by some prior modeling. The paper shows consistent results between an examination of the ratio of precipitation water to cloud water and the vertical gradient in CloudSat reflectivity. I have some comments about the resolution of the measurements used, the quantification of "warm rain efficiency", and the conclusions the authors draw about the aerosol sensitivity of warm rain efficiency. The paper should be suitable for publication in ACP subject to some revisions.

Some aspects of the scales of the clouds in this investigation are left unanswered, but are potentially critical because of the resolution of the measurements employed. The CloudSat rain water data used here has a footprint of 1.4 x 1.8 km. The cloud water path data from MODIS has a nominal resolution of $\sim$1 km at nadir. According to the methods, the cloud water path is based on a 9-pixel average, which suggests that the horizontal scale of the cloud water measurements are on the scale of 10 km. Nevertheless, clouds are shown varying from about 1.7 km to greater than 18 km. So, one question is: are the cloud water values really representative of the true values for clouds smaller than 10 km? Can we then be certain that the strong dependence of the ratio of precipitation water to cloud water on cloud scale shown in figure 2 for clouds smaller than 10 km is not influenced by the resolution of the cloud water quantity?

The authors state that "prior studies [of biases in MODIS cloud water] have found them to be small in comparison to other satellite retrievals". I suspect that this result may be resolution dependent and that in fact uncertainties for cloud smaller than several km in scale may be quite significant. For example, Cho et al. (2015) find that the MODIS cloud property retrievals from which the cloud water path is derived can have substantial errors in cumulus cloud fields because of partially cloudy pixels and horizontal inhomogeneity of cloud properties within the satellite footprint. Can the authors provide some greater support for the notion that the cloud water values are representative of the true value at the scales on the small end of the spectrum shown in this analysis?

Fine resolution satellite imagery indicates that warm cumulus clouds substantially smaller than 1.7 km are common and in fact may be more prevalent than clouds larger than 1.7 km (e.g. Mieslinger et al. 2019). Presumably some of these clouds may be precipitating. Obviously, comparable data to the CloudSat data are not available at smaller scales from satellite. Nevertheless, do the authors expect that there may be a substantial population of precipitating cumulus clouds that are not captured in their analysis? Furthermore, one might expect that warm cumulus clouds might be limited in scale. Assuming crudely that cumulus clouds typically have an aspect ratio of around 1, one might presume that cumulus clouds broader than 5-10 km might also be tall enough to contain ice or mixed phase microphysical processes occurring. What characteristics ensure that the clouds included here are both warm liquid phase and truly cumulus clouds, or is the analysis expecting to include some stratocumulus clouds as well?

The authors use the ratio of precipitation water to cloud water as their measure of "warm rain efficiency". Although, as the authors note, this quantity is just a proxy for the true efficiency. I think the authors are correct to make this point clear. I also think that perhaps it would be helpful for the authors to clarify what defines a proper quantitative measure of the warm rain efficiency. Presumably, it is not so easily observed, which is why they have chosen a proxy, which is fine. Given the brevity of this paper, however, I think a short elaboration on this point would be helpful. Furthermore, if the ratio used in this paper is merely a proxy for the true efficiency, is it really appropriate to be using "warm rain efficiency" throughout the manuscript to refer to this quantity? I suggest that the authors perhaps consider a different name so that readers are not confused about what is the true measure of the efficiency and what is the approximation of it. Alternatively, if there is a quantitative comparison of the ratio to the true efficiency, perhaps from a theoretical study, then it might be appropriate to refer to the proxy value as a measure of the efficiency with some quoted uncertainty value.

The corroboration of the inferences based on the ratio of precipitating water to cloud water with the inferences from the vertical gradient in reflectivity (VGZ) is a valuable contribution of this paper and certainly strengthens the case that the authors are making. In lines 174 to 180 the authors argue that the dependence of VGZ on cloud-top height supports the notion that updrafts in larger clouds are protected from entrainment. Why would this dependence on cloud-top height not simply result from colli-

sion/coalescence happening through a deeper cloud layer independent of any difference in entrainment? Presumably the taller clouds are provide a broader distance from cloud base to cloud top through which raining drops can fall and collect cloud drops. Likewise, perhaps a stronger updraft that yields a taller cloud is better at promoting the coalescence of cloud drops through turbulent collisions. Could these similarly explain the differences between clouds of differing heights?

Finally, the authors explore the dependence of their proxy for warm rain efficiency on the aerosol optical thickness in the vicinity of the cloud. They conclude that there is little dependence of the efficiency on aerosols, which is an interesting result. I suggest, though, that the authors remove the word "surprisingly" from the abstract where this result is reported. As noted by the authors, by excluding non-precipitating clouds from their analysis they are likely missing the expected dominant effect, which is the suppression of rain formation. Is there not a CloudSat study looking at the dependence of the occurrence of rain in CloudSat retrievals upon AOD? I think that a citation to such a study would be appropriate in the discussion of the results presented in this paper. If not, I think the authors should point out that this might be the more fruitful path to quantifying aerosol effects.

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