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Atmospheric Chemistry and Physics Discussions

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

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We thank the referees for their helpful suggestions and comments on improving the manuscript. Below are our point-to-point responses to the referees' comments.

Responses to anonymous referee #1:

The paper highlights the importance and robustness of the relation between vertical profiles of cloud drop effective radius and aerosol concentrations, using LES. It tests the validity of satellite observations and their inferences that were first presented by Rosenfeld and Lensky (1998), and generally confirms them. The paper is well written. The authors should consider the following remarks:

Page 30975, lines 3-5: The text reads: "they assumed that cloud-top properties ob-

served for clouds at different stages of their vertical growth are similar to the properties of a single cloud as it grows through various heights". In this regard, it should be mentioned here that Lensky and Rosenfeld (2006) have already validated this hypothesis by comparing the ensemble properties of a snap shot of a large number of clouds in different stages of their development to the properties of individual clouds tracked along their lifecycle, using 3-minute rapid scans of the METEOSAT-7 geostationary satellite.

Our reply:

We thank the referee for pointing out that Lensky and Rosenfeld (2006) have validated the hypothesis. The profile of droplet effective radius for clouds with various heights in a snapshot was compared with the composite effective radius profile for individual clouds tracked along their lifecycle, using the 3-minute satellite images in their study. They confirmed that the composite properties from tracking the cells should reproduce the properties in the snapshot. We have added the above discussion in the manuscript. We have also added the reference Lensky and Rosenfeld (2006) in the manuscript. Nevertheless, we feel that the use of a process-based numerical model to investigate the method provides new and important insights into the strengths and limitations of the method.

Section 3.1: Using the highest cloudy grid point from the model produced a bias of about 10% lower effective radius, as compared to a grid point that is well in cloud at the same height. This is likely the result of the following reasons:

a. The uppermost grid point of the simulated cloud has much smaller adiabatic fraction as compared with the lower points. Because of the limited model resolution (despite being rather high), the transition between the cloud and cloud free air is not depicted very well. Therefore the cloudy portion of the uppermost bin in reality is only partially field with cloud, and this contributes to decreasing the adiabatic fraction to well below the real value. Because the model assumes extreme homogeneous mixing, inevitably the effective radius must decrease and be underestimated.

Our reply:

We agree with the reviewer that at the cloud boundary (for example, the cloud top), the model tends to over-dilute the cloud because of the limited model resolution. It is possible that an uppermost grid point of the cloud is considered as cloudy in the model, while it is only partially filled with cloud in reality. The modeled cloud would then have lower adiabatic fraction compared to cloud in reality. However, regardless of the model performance on this issue, the uppermost grid point would have smaller adiabatic fraction as compared with the lower grid points because of the mixing. We have added the above discussion to the manuscript. We agree with the referee that the lower adiabatic fraction in the highest grid point, and the homogeneous mixing assumption, are the reasons for the bias of about 10% lower effective radius in that grid point as compared to a grid point well in cloud at the same height.

b. The application to satellites must take into account the portion of the cloud that contributes the indicated effective radius. This is more than 40 m in most circumstances, and definitely so when a thin layer at the very top portion of the cloud is highly diluted, as the model simulates. Again, the model is likely over-diluting the highest grid-point of the cloud.

Our reply:

When applying our results to satellites, we need to bear in mind that the cloud top portion is the region where satellites retrieve droplet effective radius. At the same time, the cloud top portion is the region where the adiabatic fraction and effective radius are affected by mixing. The effective radius at cloud top is smaller than the in-cloud effective radius at the same height for the modeled clouds because of the model assumption of homogeneous mixing. As will be seen in the reply for the next comment, for clouds that have inhomogeneous mixing (or have both homogeneous and inhomogeneous mixing), the bias in estimating the in-cloud effective radius is likely less than 10% using the method in Rosenfeld and Lensky (1998).

Conclusions: When discussing the applicability to satellites, the authors have to take into account the comments above. In addition, they should discuss the bias due to the low adiabatic fraction, even well within the clouds, that incurs an underestimate of the effective radius due to the extreme homogeneous mixing assumption of the model. In this context, the authors should discuss the findings of Freud et al. which they mentioned earlier in the manuscript, about the clouds being closer to undergoing extreme inhomogeneous mixing.

Our reply:

In reality, mixing is neither extremely homogeneous, nor extremely inhomogeneous but somewhere in between. In addition, the nature of the mixing may change as the cloud evolves. Freud et al. (2008) suggested that the mixing is inhomogeneous, which causes nearly no change in droplet effective radius. Therefore, our study reflects an extreme case of underestimating the in-cloud effective radius using the method in Rosenfeld and Lensky (1998). In reality, for clouds that have inhomogeneous mixing (or have both homogeneous and inhomogeneous mixing), the bias in estimating the in-cloud effective radius is likely less than 10% using the method in Rosenfeld and Lensky (1998). We have added the above discussion in the manuscript.

Finally, the authors state in the introduction: "The assumption that cloud-top re acquired by satellites is representative of in-cloud re at the same height for convective clouds, as used in Rosenfeld and Lensky (1998), will be tested here". However, the authors never explicitly state in their conclusions whether their study confirmed that assumption. This point should also be reflected in the abstract, as this provides the context of the importance of the findings of this study.

Our reply:

We have added the following statement in the conclusion: “The assumption used by Rosenfeld and Lensky (1998) has been confirmed for shallow convective clouds in this study.” This point is also added in the abstract.

A minor point: Page 30984, line 1: The text should read: ". . . 76–90% of the re at the top of the vertically inhomogeneous cloud. . ."

Our reply:

The text has been changed in the manuscript.

Responses to referee P. Zuidema:

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 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?

Our reply:

The referee is mainly concerned whether the modeled clouds in this study are representative of the clouds investigated by Rosenfeld and Lensky (1998). Although the modeled (smaller and shallower) clouds are not necessarily representative of the (larger and thicker) clouds studied by Rosenfeld and Lensky (1998), we use the modeled clouds to test whether the Rosenfeld and Lensky (1998) method works for this cloud type, i.e. we test if the cloud-top effective radius from shallow cumulus cloud en-

sembles with different heights can be used to construct the profile of effective radius. Our work is therefore a specific case of the Rosenfeld and Lensky approach. We confirmed that their method can lead to a low bias of about 10% in estimating the in-cloud effective radius for the shallow (modeled) clouds. We also concluded that the 10% low bias is caused by the lower adiabatic fraction at cloud top due to mixing, and also by the model assumption of homogeneous mixing that inevitably leads to smaller effective radius at cloud top. Note that a homogeneous mixing would lead to smaller effective radius as compared to inhomogeneous mixing.

When applying the conclusion of this study to these smaller clouds, we need to keep in mind that the vertical profile of the adiabatic fraction may differ from those in larger clouds. For example, the core regions of deeper clouds may be able to preserve adiabatic LWC. In addition, the mixing in reality is probably not extremely homogeneous as assumed in the model in this study. Freud et al. (2008) suggested that the mixing is inhomogeneous, which causes almost no change in droplet effective radius. Therefore, our study reflects an extreme case of underestimating the in-cloud effective radius using the method in Rosenfeld and Lensky (1998). For the cores of bigger and thicker clouds that have larger adiabatic fraction, and for clouds that have inhomogeneous mixing (or have both homogeneous and inhomogeneous mixing), the bias in estimating the in-cloud effective radius is likely less than 10% using the method in Rosenfeld and Lensky (1998).

We agree with the referee that shallow, broken, non-opaque clouds of small dimensions are arguably the most challenging of all clouds for conventional satellite remote sensing. Our conclusions imply that, if the cloud-top effective radius of a broken cumulus cloud field can be measured with satellites, we can use the method in Rosenfeld and Lensky (1998) to infer the in-cloud effective radius with reasonable accuracy (about 10%). A caveat here is that the cloud sizes would have to fill a remote sensing pixel for these techniques to be useful. Also, in reality, the accuracy would be diminished by instrument and other measurement uncertainties. Our study also provides information on cumulus cloud vertical profiles (and the effects of aerosols on them) that can be useful in understanding the radiative properties and in the retrieval of this kind of clouds.

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).

Our reply:

We have added more discussion on the method of Rosenfeld and Lensky (1998) in the manuscript: Because of the difficulty of measuring profiles of effective radius from satellites, Rosenfeld and Lensky (1998) used the cloud-top height and the corresponding cloud-top effective radius from the cloud ensembles in the snapshot to construct a profile of effective radius that can be used to represent the effective radius profile well in clouds.

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.

Our reply:

We have added more explanation to the manuscript regarding the method of constructing the profiles of effective radius: We use the cloud-top effective radius and the cloud-top height from the modeled cloud ensembles in 48 snapshots to form a constructed effective radius profile. The constructed profile is then compared with the effective radius from all cloud samples in the 48 snapshots. We intend to investigate if the

cloud-top effective radius, which may be measured from satellites, can represent the effective radius well in clouds at the same height for the modeled clouds.

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?

Our reply:

The evolution of effective radius profiles is important because the method used by Rosenfeld and Lensky (1998) is based on the assumption of time-space exchangeability (Rosenfeld and Lensky 2006), i.e., cloud ensembles in the snapshot have similar profiles as compared with the individual clouds tracked along the life cycle. It is worth looking at the effective radius profiles as individual clouds develop. In sections 3.2 and 3.3, we investigate whether the profiles change during the development of the clouds, and the reasons for the change. As discussed in the manuscript, mixing plays an important role in changing the in-cloud effective radius. We conclude that decaying clouds have smaller effective radius because of the mixing.

The subadiabatic fraction/re values do not necessarily reflect those of the clouds evaluated by RL and LR. As discussed in the reply to the first comment, smaller and shallower clouds may have different adiabatic fraction as compared with bigger and thicker clouds. In addition, the model assumption of homogeneous mixing in this study may also be different than the mixing process in clouds studied by Rosenfeld and Lensky (1998), and in clouds in reality.

The abstract needs to mention the motivation for the work.

Our reply:

At the beginning of the abstract, we have added the following to mention the motivation: Conventional satellite retrievals can only provide information on cloud-top droplet effective radius. Given the fact that cloud ensembles in a satellite snapshot have different cloud-top height, Rosenfeld and Lensky (1998) used the cloud-top height and the corresponding cloud-top effective radius from the cloud ensembles in the snapshot to construct a profile of effective radius representative of that in the individual clouds.

In Line 10 of the Abstract, we added the following sentence: Therefore, the method used by Rosenfeld and Lensky (1998) is validated for the modeled nonprecipitating clouds in this study.

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