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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.

Anonymous Referee #1

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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 observed for clouds at different stages of their vertical growth are similar to the 5 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.

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.

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.

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.

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.

A minor point:

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

Reference:

Lensky I. M., D. Rosenfeld, 2006. The time-space exchangeability of satellite retrieved relations between cloud top temperature and particle effective radius. Atmos. Chem. Phys. 6, 2887-2894.

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 30971, 2010.