Atmos. Chem. Phys. Discuss., 14, C3295–C3296, 2014
www.atmos-chem-phys-discuss.net/14/C3295/2014/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "The relative dispersion of cloud droplets: its robustness with respect to key cloud properties" *by* E. Tas et al.

Anonymous Referee #1

Received and published: 6 June 2014

The paper discusses the variability of relative dispersion of cloud droplet distribution in convective clouds. Results are based on in-situ measurements performed in 2007-2008 during the Cloud and Aerosol Research in Istambul (CARI) experiment. Five flights are analysed. Data were collected during traverses of a research aircraft through a field of cumulus cloouds. As can be inferred from Figure 1 the aircraft ascended from the cloud base to the cloud top over a horizontal distance over 100 km long. Clouds were 1 000 to over 2 000 m deap. We don't know where it is known from; obviousely not from the in-situ measurements, because the aircraft didn't fly close to the cloud tops. Relative dispersion is analysed with respect to the location in clopud (with respect to the cloud base), mean concentration and Liquid water content.

In general I have two major comments about the paper: the first one concerns the

technical quality, the second concerns the scientific robustnes.

The technical quality of all figures is very bad. In all figures the vertical dimension, where the discussed parameter (relative dispersion) is presented is so small, that it makes impossible to see, understand and appreciete discussion of results. The vertical scale changes from one flight to the other making results shown very coonfusing.

Those critical remarks come together with those more serious that concern the overall approach. As I stated at the beginning, authors use the notion of cloud depth (in the suplement) atributing the same cloud depth for the whole cloud field measured during a given flight. They should comment where they know this value from, and secondly why the attribute the same value for all clouds. This issue brings also a confusion while loohing at Fig 2, where the vertical scale end for some flights at 'expected' cloud top (TR5, TR4, TR2), but not fro two others. Clouds are heavily diluted. Although it is not so easy to see it from the colour scale in Fig. 3 and 4 it is very likely that LWC very rarly rises up to 2 g/mˆ3 for those flights were the maximum adiabatic value goes up to 3,8 g/mˆ3. Division of cloud points into 'inner' and 'boundary' parts doesn't seem sound for me. The procedure doesn't provide a real division, because as I stated before all clouds are very diluted. A comment on inhomogeneous mixing as l. 20, p. 11161 is unjustified. I don't understand the reason of submition of a suplement, that is discussed also in the main body of a paper. If the results are discussed in the paper and references to suplement's figures are provided, this text should be merged to the main paper.

In my opinion the paper is not ready to be published. My 'major' remarks are not exhaustive, because I was not alble to understand and appreciate all discussion and conclusions drown due to the illegibility of figures.

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 11153, 2014.