**Atmospheric
Chemistry
and Physics
Discussions**

Interactive comment on "Resolving both entrainment-mixing and number of activated CCN in deep convective clouds" *by* E. Freud et al.

Anonymous Referee #1

General Comments

This paper presents a method to infer the number concentration of activated CCN at the cloud base (Na) in convective clouds, by using in-situ (1Hz) measurements of droplet size distribution at different heights within a cloud or cloud field. The proposed iterative method first estimate Na from the vertical profile of Rv (or Re) by assuming extreme inhomogeneous mixing, and then allowing corrections to this first estimate by relaxing the constrain of extreme inhomogeneous mixing, and deriving a theoretical environmental RH profile such that the it minimizes the residual of a linear fit. The methodology is applied to a large collection of data. For the samples analyzed, the proposed algorithm tends to converge to RH_best values that are close to 100%. The derived Na is shown to closely correlate with below cloud CCN concentrations. The

ideas put forward by the authors in the manuscript are interesting, but many and important parts of the manuscript are completely lacking or require substantial revisions. Following are some general comments on the most important issues that should be addressed by the authors:

1. Remarkably little discussion on the susceptibility of the method to measurement uncertainty is included in the manuscript. No error estimates are included in the graphics (no for measurements or for the inferred values). The authors fail to address how uncertainties, for example, in cloud base estimates and in the sizing of the droplets by the cloud probe translate into uncertainty of their inferred RH_best or Na values. In general, the description of the measurements is overall inadequate.

2. The authors do not show how their proposed method (which involves direct measurements of droplet concentration, N) compares to simpler estimation methods, for example, estimating Na as the average N measured near the cloud base (where entrainment effects are expected to be weak), or from relatively undiluted data points trough the cloud (i.e., selecting a subset of samples with an AF higher than a given threshold). The authors emphasize the importance of the derived Na values, but nowhere in the manuscript do they perform comparisons of this number with the actual measured N profiles trough the sampled clouds. Are simple averages of the measured droplets concentrations also strongly correlated with below cloud CCN?

3. The lack of thermodynamic measurements of environmental air (relative humidity and temperatures profiles) harms the strength of the conclusions reached by the authors in the manuscript. It would be also interesting to see a more thorough discussion of the impact that has the use of 1Hz data (corresponding roughly to 100 m) to study effects of entrainment on droplet sizes. As stated by Lehman et al. 2009, there exists a scale above which entrainment will be mostly inhomogeneous and under which it occurs homogeneously.

4. Despite the authors efforts in making the proposed algorithm clear, important de-

tails in the calculations were left out of the description, and this makes following the proposed method difficult.

5. To improve the clarity of the manuscript, I suggest the authors to reserve section 3 to the detailed description of the proposed algorithm (including specifics of the calculations involved), and to remove or displace some of the paragraphs that are devoted to speculations on possible uses and applications of these steps to other sections.

Specific Comments

1- Introduction

Line 26 - p9675: "... as the supersaturation has to be controlled and adjusted". I suggest the authors to either extend their explanation of this or remove it from the manuscript. What do the authors mean by controlling and adjusting the supersaturation?

Line 24 – p9676: "here we introduce a methodology for deriving Na of convective clouds, regardless of the exact knowledge of the Earth's surface radiative properties" – This phrase should be removed. The manuscript only makes use of in-situ measurements with a cloud probe, and no remote-sensing techniques are used, so the relevance of this phrase is unclear.

2- Entrainment-mixing process

Line 6 – p9677: St and Sc are not defined in the manuscript.

Line 13 – p9677: Remove "until it is saturated". This is not always possible and it is not always the case.

3 – Methods

The 5-step algorithm described in this section would be much clear to the readers if the exact equations applied to the measurements were shown. The authors devote some time to rather trivial equations (1, 2, 3 and 4), but do not include the equations leading

to the mixing diagrams of Figure 1 (of great importance in the proposed approach). It would be definitely helpful to include these equations and to show exactly how they were applied to the data. Before discussing the algorithm to estimate Na, it would be useful to first describe the available data and instrumentation used.

Line 4 - p9682: "Re_a" is not defined yet. It would perhaps be useful to explicitly state that the sub-index "a" will in general denote adiabatic values of the cloud parameters.

Line 21 - p9682: The research flights and available data are not described yet.

Line 23 - p9683: "In theory [...] Na is sensitive to LWC_a". This should be rephrased since it has no physical meaning. Perhaps the authors meant that the inferred value of Na depends on the (also inferred) adiabatic liquid water content (lapse rate?). In this case and some others in the manuscript the authors tend to confound physical variables with inferred quantities.

Line 25 to 30 - p9683: The authors imply here that the pressure and temperature at cloud base could be inferred from a linear fit to equation 4 by "prescribing different cloud base properties until the fit crosses the origin". However, no support for this is included in the manuscript and this assertion should be removed. Are the linear fits included in figure 2 one-parameter fits in which only the slope of the fitting line is allowed to change while the intercept with the axis is set to zero? In line 26, where the authors explain how this fitting is done, it is difficult to tell if they are suggesting a possible use for their method or if they used this method in their calculations.

Line 16 – p9684: I found this part of the manuscript confusing. In line 18 the authors assert that the derived Na would be too sensitive to errors in Re and LWCa for a too high AF threshold, but immediately after this, in line21 it is mentioned that the final derivation of Na is not sensitive to the AF threshold chosen. This is a little confusing for the reader. Is the AF threshold only used in step 3.1 or are those data points with AF values lower than the threshold also dropped out of the calculations of RH_best in the following steps?

Line 21 – p9684: "... the mean RH_best is smaller than 100%". Perhaps this phrase should be more elaborated. I assume the authors restrict the fitting parameter RH_best to be at most 100%, so this is not very meaningful since the only way the average RH_best could be 100% is if every penetration gives as a result the maximum allowed value.

Line 20 – p9685: It is not clear to me exactly of which best fitting curve are the MPR minimized for. Is it for the Dv-AF diagrams similar to those in Figure 3? It would be useful to state this explicitly.

4 – Results and discussions

As a general comment for this section, no discussion of how much improvement is attained with RH_best, i.e., what is the average change in the MPR. Also, the sensitivity of RH_best to sizing errors of the probe is not discussed.

Line 21 - p9687: "... therefore, it is important to correct this drift", It is unclear to me if the authors did this to their data or if they are suggesting others to follow this procedure (or both).

Technical corrections

Line 13 – p9674: "adiabatic water" should be replaced with "adiabatic liquid water mixing ratio".

Line 23 – p9679: "...although parts of it ..." – It is difficult to know to what exactly are the authors referring to. Is it the tendency towards extreme inhomogeneous mixing? Or is it the intermediate features between homogeneous and inhomogeneous?

Figure 2:

The plot in general is difficult to read. - "Adiabatic Water" probably refers to "Adiabatic Liquid water content". - Assuming there was indeed a typo, I do not understand what the authors exactly mean by "adiabatic liquid water content for different thresh-

old adiabatic fractions". This seems inaccurate, since they seem to be referring to the dependence of the "Best linear fit to equation (4)" to different threshold adiabatic fractions. - The labeling of the ordinate should be changed to "adiabatic liquid water mixing ratio".