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# *Interactive comment on* "Comparing calculated microphysical properties of tropical convective clouds at cloud base with measurements during the ACRIDICON-CHUVA campaign" *by* Ramon Campos Braga et al.

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Interactive comment on "Comparing calculated microphysical properties of tropical convective clouds at cloud base with measurements during the ACRIDICON-CHUVA campaign" by Ramon Campos Braga et al.

Anonymous Referee #2

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The Manuscript is a little vague in its objectives, but it appears that it is attempting



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Discussion paper

to validate aircraft observations by performing closure with either different instruments or between CCN measurements made at different supersaturations and cloud droplet numbers measured at different updraft speeds. It does this using below cloud and in cloud measurements made during the ACRIDICON-CHUVA campaign and combining these with activation models. The other reviewers have already made comments regarding the models used so I will focus here mostly on the measurements and the analysis that goes along with those. Unfortunately this paper needs significant extra work in order to make it of publishable quality. However I think the type of analysis that has been performed here is valuable and is not undertaken enough. This is the type of paper that can be used to assure the quality of the measurements being made and that other papers in the project can reference to avoid repeating this analysis by multiple groups and authors. It is also the type of paper that can highlight the limits of the instruments. This is good as it can provide insight to a modeller who is using the data perhaps without an in-depth knowledge of its limits and it also means that it becomes clear what science cannot be performed with the data and therefore where we need to improve our instruments, calibration methods and analysis techniques. However the work is only valuable in this sense if the analysis is performed in an incredibly rigorous manner. I applaud the author's attempt to write this paper, but I would suggest that he needs to pull in more input from coauthors - there are many well respected coauthors on the paper and I am surprised that their instrument knowledge does not show through in this paper. There are certainly other people who work full time within the aircraft instrument community who could have input. I would suggest that the manuscript needs a full rewrite and I would suggest that the author goes back to basics in terms of deciding exactly what the objectives are (are they validating instruments or validating the cloud models), then doing a thorough uncertainty analysis of the instruments. This must include details of calibration methods used and the uncertainty derived from those, plus things that cannot or have not been calibrated and the reason why and what the expected uncertainty for these things might be. Based on the uncertainty analysis the author can then decide if the objectives are achievable and can present appropriate



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uncertainties in the conclusions. Based on the general comments above I recommend the paper be rejected in its current form. Some more detailed comments follow.

General Comments:

The authors thank the referee for the general comments and advices. Furthermore, the advices of the referee are highly appreciated as well as the very valuable and constructive suggestions to increase the quality of the manuscript. We tried to address the points requested by the reviewer to the paper be considered for publication. Overall, we have improved the focus of the paper highlighting our objectives and the novelty of our study.

The availability of the measurements collected by the aircraft HALO provides a unique opportunity to compare the data with model predictions and to test the sensitivity of the results to the differences between the measurements by the cloud probes.

This study is novel in several aspects:

a. It is the first study that validates the methodology of retrieving the adiabatic cloud drop concentrations Na (Freud et al., 2011) from the vertical evolution of re while assuming that re is nearly adiabatic. This is important because it supports the validity of retrieving Na from satellite-retrieved vertical profile of re (Rosenfeld et al., 2014a and 2016).

b. It is the first study that tests with aircraft the measured Nd with its parameterization that is based on NCCN(S) along with cloud base spectrum of updrafts weighted by the updraft speed itself, Wb*. It is done this way to be compatible with the recently developed methodology of retrieving CCN from satellites by means of retrieving Nd and Wb* (Rosenfeld et al., 2016).

c. It is the first study that compares observationally the old Twomey (1959) parameterization of the dependence of Nd on Wb (Eq. 2) versus the recent Pinsky et al. (2012) analytical expression for the same (Eq. 3)."

References:

Freud, E., Rosenfeld, D. and Kulkarni, J. R.: Resolving both entrainment-mixing and number of activated CCN in deep convective clouds, Atmos. Chem. Phys., 11(24), 12887–12900, doi:10.5194/acp-11-12887-2011, 2011.

Pinsky, M., Khain, A., Mazin, I. and Korolev, A.: Analytical estimation of droplet concentration at cloud base, J. Geophys. Res. Atmos., 117(17), 1–14, doi:10.1029/2012JD017753, 2012.

Rosenfeld, D., Fischman, B., Zheng, Y., Goren, T. and Giguzin, D.: Combined satellite and radar retrievals of drop concentration and CCN at convective cloud base, Geophys. Res. Lett., 41(9), 3259–3265, doi:10.1002/2014GL059453, 2014a.

Rosenfeld D., Y. Zheng, E. Hashimshoni, M. L. Pöhlker, A. Jefferson, C. Pöhlker, X. Yu, Y. Zhu, G. Liu, Z. Yue, B. Fischman, Z. Li, D. Giguzin, T. Goren, P. Artaxoi, H. M. J. Barbosai, U. Pöschl, and Meinrat O. Andreae, 2016: Satellite retrieval of cloud condensation nuclei concentrations by using clouds as CCN chambers. Proceedings of the National Academy of Sciences, doi:10.1073/pnas.1514044113.

Twomey, S.: The nuclei of natural cloud formation part II: the supersaturation in natural clouds and the variation of cloud droplet concentration, Geofis. Pura e Appl., 43(1), 243–249, doi:10.1007/BF01993560, 1959.

Specific comments:

Introduction - In general the author should be familiar with the calibration methods used with the instruments used and this should be reflected in the references.

line 75 - Previous analysis (Strapp et al 1992, Journal of Atmospheric and Oceanic Technology Vol 9 p 548) has indicated that the PCASP dries it's sample through ram heating during measurements. The author should familiarise himself with this work, and understand why this drying may not be happening.

A: There is no mention of PCASP near line 75 of the ACPD paper. We are well aware of the claim that PCASP is drying the aerosols in the inlet. The observations show that this assumption is clearly not valid. This disqualified the use of PCASP for this study. This is explained in lines 111-117 of the ACPD paper.

Line 85 - If CCN measured at constant S is not constant then either N0 or k in (1) are changing. I.e., either the total number is changing but everything else remains the same or the hygroscospicity or size distribution of the aerosol is changing. Or of course there could be a combination of these factors. The author must show which are occurring.

A: We highlight that CCN concentration changes as a function of S or CCN load. Therefore, we correct CCN concentrations measured with different S with the CCN loaded observed for measurements with fixed S. The CCN load is calculated by calculating the difference between the measured CCN concentrations with the mean CCN concentration measured for a fixed S.

Line 95-100 - Total number totally cancels from effective radius calculations and adiabatic calculations reveal expectations for mass of condensed water not number. Dividing adiabatic water content by measured mass per particle would give a number concentration but even in an adiabatic regime the uncertainty on this would be larger than the measured droplet number concentration. Calibrations on the CDP operated by FAAM using the method described by Rosenberg et al 2012 (Atmospheric Measurement Techniques vol 5 p1147) provides an uncertainty around 0.5 um in sizing, but typically shows a discrepancy of around 2 um from the manufacturer's specification. If the manufacturer spec is used in this work then we can expect that at 20 um we have approximately 30% uncertainty in mass per particle measurements.

Line 190 - Has the collecting angle of the instruments been measured? This defines the location of the Mie wiggles and where the bins should be merged.

A: Given the uncertainty of the sample area, the Probe Air Speed (PAS), article losses, deviations and maybe coincidence (not negligible but likely not a significant issue) the uncertainty in concentration ranges below 20% and likely approaches or exceeds 20% only in cases of tight curve maneuvers à this might be the most prominent case when the "collecting angle" comes into play. For level flight (straight heading) I would quantify this issue to be comparatively small given the flight speeds of generally larger 170m/s and up to 240m/s – i.e. the direction of particle penetration may be predominantly perfectly in-line with the flight direction (unless there was a systematic deviation of cloud elements due to flow disturbances induced by the aircraft structure or the neighbored instrument, etc. for which at current state no clear evidence is given, yet).

Line 246-260 - As described previously this assumes k is constant, the author needs to provide evidence this is a good assumption. The correction method means that we are correcting to a point where N0 is equal to the average N0 for the scan. This should be made clear and an estimate of how much N0 is varying must be made as this impacts how much confidence we have in a model's estimate of Nd in cloud.

A: The correction method assumes that the variability of CCN1 at each flight step can be corrected by the average measurements of CCN1 (TmCCN1). Indeed, there is a variation on TmCCN1 for each flight segment. The calculated standard deviation for TmCCN1 in all flight segments was up to 24 %, indicating a small impact on the parameterization proposed to fit the Twomey equation (Eq. 1). We calculated the uncertainty impacts from the adjusted N0 and k and it contributes to about 35 % of NdCCN and NDT uncertainties on average. This was added to the text.

Line 261-269 - When I first read this seemed entirely circular. Later it becomes clear that this is the point. We are putting observations into a model and checking for consistency. The author should highlight in the aims of the paper that they intend to do this so that the reader knows to expect this. A better way to represent this may be a plot f Nd vs S with data points taken from measurements and derived through equation 3 (perhaps coloured by w) along with points from the scanning and static CCN instrument. If the model is correct and the obs are consistent then all points should fall on one line.

A: Unfortunately S measurements within clouds are not accurate, and then we could not compare with Smax estimates.

Lines 284-288 This probability matching method assumes droplet number is a monatonic function of w only. I have no issue with the monotonic assumption, but the author should show that there is no other influences upon drop concentration such as entrained dry/clean air and constant aerosol/ccn concentration below cloud or at least state why this is a good assumption.

A: We assume that for a given CCN(S) spectrum below cloud base the droplet number measured at cloud base is a function of W, as stated by Twomey equation (Eq. 2). The analysis show that for most of cases that the theoretical estimates do not reproduce the measured Nd the degree of entrainment should be high (because we have a large dispersion of Nd values). This is an issue that we should highlight in the new version of the manuscript.

New text at manuscript: "A suitable method to analyze the relationship between Wb and Nd measurements is the 'probability matching method' (PMM) (Haddad and Rosenfeld, 1997), which requires that the two related variables will be increasing monotonically with each other. For a set of measurements of Wb and Nd at cloud base, it is expected that larger Wb would produce larger Nd for a given NCCN(S). Therefore, it is assumed also that Nd is produced uniquely by Wb for a given NCCN(S) spectrum as calculated from the measurements below cloud base. It is further assumed that entrainment does not change systematically with Wb in a way that would reverse the monotonic increase of Wb with NCCN(S)."

Lines 307-325 This needs a thorough uncertainty analysis to show its usefulness as described earlier.

A: The line numbers do not match anything that can be relevant to the comment in the manuscript.

Line 350 - You are claiming an uncertainty of 5% in N0, but as described earlier this is in the average N0 over the scan. We have seen CCN number on the constant supersaturation instrument vary from 650 to 950 cm-3 so it seems unreasonable to claim 5% uncertainty in this parameter. . This ambiguity comes from not being clear in the first instance about what you are trying to measure. In reality I think an estimate of k is what you should be aiming for as N0 is clearly changing and is not a constant.

A: We have calculated an uncertainty of about 20% for large NCCN(S) and about 10 % for smaller NCCN(S). On average, NCCN(S) have an uncertainty of 15 %, N0 and k 20 and 23 %, respectively. This is better described at the new version.

Line 380-390 I certainly would not be alone in suggesting that the phrase "agree closely" and similar variations has very little place in scientific work. In this case there is a difference of up to 70% in fig 9a. Phrases such as "agree within the measurement uncertainties," "differ by up to x amount," or "agree to the extent that conclusion y is unaffected" are all appropriate, but "agree closely" is entirely subjective.

A: Ok. changed.

Line 401 - Another "good agreement" statement. Points here deviate from the 1:1 line by up to a factor of 2.

A: We rewrote the sentence.

New text: "...Figure 13a shows the values of Nd* and NdT* for the different cloud base measurements shown in Figs. 11 and 12. The NdT* agrees with Nd* within the measurements uncertainties, as shown by the error bars. The bias of NdT* with respect to Nd* for the CAS -DPOL is 1.00 with a standard deviation $\pm$0.17 around it. The respective result for the CDP is 0.84 $\pm$0.12. A weaker agreement is observed for comparisons between NdCCN* and Nd* (see Fig. 13b), A factor of $\sim$2 can be observed for some cases (AC14 and AC17). The bias of NdCCN* with respect to Nd* for the CAS-DPOL is 0.80 $\pm$0.07. The respective result for the CDP is 0.76 $\pm$0.1. "

Line 440-444 - This difference is almost certainly within the expected uncertainty which as described above is probably 30% from the mass per particle measurement, plus perhaps 10-20% from sample area and air speed through the sample volume estimates. 447-450 and figs 13/14 - I see size distributions like this all the time and often by people who work with these instruments a lot. They are unfortunately not really appropriate styles for plotting size distributions. The following changes should be made. The plot should show points and not lines. It is not appropriate to "join the dots" on a plot that has significant uncertainties. Each point should have an x and y uncertainty. Standard error is not an appropriate uncertainty to use. It assumes that we measure the same thing repeatedly and that the uncertainty is dominated by noise. Here we have concentrations that vary with time during and between the periods that contribute to these average size distributions. So the standard error becomes some combination of noise and variability and omits all systematic uncertainties. Instead the author should do a proper error analysis including contributions from sample area, air speed at the probe, bin width and counting (Poisson) uncertainty for the y error and sizing uncertainty for the x error.

A: The CDP'S sizing uncertainty is calibrated regularly before, during and after flights with mono-sized glass beads or Poly Styrol Latex (PSL) of various sizes. The uncertainty of these calibrations mainly results from the uncertainty in size of the test aerosol and refractive index resulting in an uncertainty for specific particle diameter of at most 10%. The CIPgs sizing may imply an general uncertainty of +/- 15 $\mu$m which is the instruments resolution. In the way we treated the data for spherical bodies, the uncertainty should not be larger for the CIPgs sizing as the particles are mainly in PAS speed or faster (latter causes an image squeezing in flight direction which is compensated by choosing the diameter in diode array direction for the image sizing). Furthermore, in the droplet regime, the reproduction of the Fresnel-diffraction may cause a non-systematic uncertainty in the sizing. However, all droplet sizes may more or less be influenced in the airborne state by deformation or shrinking (very smallest drops - due to congestion heating) in the compressed flow regime upstream of the probes at highest flight speeds which at current state is not quantifiable but may be small. We agree with the referee that the DSDs on Figures 6 should not be presented as line plots. The line between data points suggests a course of the DSDs that is not real. Instead, we have changed the DSDs figures using histograms of binned detection channels. The data points are shown with size bin limits in x-direction (to cover the Mie-ambiguity ranges, providing an approach to have the channel-wise sizing error superimposed by the size-bin limits) and uncertainty in y-direction. The new figures 6a-d are available at supplementary material.

Line 454-560 The sensitivity is probably not the issue, it is more likely to be the bin widths for which we see no calibration. A: OK.

Please also note the supplement to this comment:
http://www.atmos-chem-phys-discuss.net/acp-2016-872/acp-2016-872-AC2-supplement.pdf

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