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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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Anonymous Referee #3

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Title: Comparing calculated microphysical properties of tropical convective clouds at

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Discussion paper

cloud base with measurements during the ACRIDICON-CHUVA campaign.

Author(s): Ramon Braga et al.

The paper compares calculated with measured microphysical properties of convective liquid clouds in the tropics. Unfortunately, calculations are not performed within a microphysical model taking into account important spatiotemporal fluctuations of dynamical and thermodynamical properties, turbulence, entrainment, etc... In this study solely a comparison of calculating cloud properties from analytical equations and respective measurements has been performed, which represents a considerable work, however with rather limited outcome. Conclusions of this comparison study are disappointing and do not gain new insights in liquid convective cloud microphysical processes. The paper barely presents new and noteworthy concepts. As it stands, the work is solely a rather qualitative affirmation of existing parameterizations. Taking these issues into account, the study may better carve out the uncertainties of used cloud parameterizations (equations 1, 2, 3? . . .) based on the uncertainties of measured cloud parameters from the ACRIDICONCHUVA dataset. Also taking into account missed features and uncertainties stemming from turbulence (and more complex droplet activation) and entrainment not captured in this study. Would this be possible at all? The uncertainties of your instruments and derived measurements have been discussed rather honestly in this manuscript. This is why I encourage authors to develop this manuscript into that direction. Otherwise, I would recommend rejection of this manuscript due to its poor contribution to scientific progress.

The manuscript shows some striking and unexplained differences between calculated and measured microphysical parameters (Nd versus NdT, NdT* versus NdCCN* for a series of flights). Is this a principal problem of performed measurements within an environment of complex processes and limited degree of complexity of calculations that are hardly comparable: calculations do not capture measurement data features like turbulence, entrainment, etc...? At least above mentioned differences are more important for higher Wb values!

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. However, we disagree with the referee affirmation which highlight that the study have a poor contribution to scientific progress. As mentioned before, the objective of the paper is to validate the physical parameterizations connecting between CCN(S), Wb, Nd and Na, when tested over the important convective regime of the Amazon. This is the first paper that tests all parameterizations proposed for convective clouds regime against each other with the same dataset.

Furthermore, the parameterizations are far from being old. Rather, as now stated in the first paragraph of the abstract: "The objective of this study is to validate novel parameterizations that were recently developed for satellite retrievals of CCN(S) at cloud base alongside with more traditional parameterizations connecting CCN(S) with cloud base updrafts and drop concentrations."

The calculations of the effective updraft speed at cloud base (Wb*) provide a new capability for test the parameterizations proposed in the study and to ascribe the capability of Na estimates for convective clouds developed at different aerosol conditions over Amazon. The role idea of using Wb* is already accounting for the air turbulence (see Rosenfeld et al., 2014a) and the Na estimates for convective clouds also account for entrainment as is described at section 4.4 (also discussed at Freud et al., 2011). The study supports the use of Na estimates using satellite data of effective radius vertical profiles at convective clouds over the Amazon basin.

Regarding the possibility of use other parameterizations in our analysis, we have already highlight at introduction section some issues due to the unreasonable measurements of low hygroscopic factor values below cloud base, which prevent us to test other types of parameterizations (e.g. k-Köhler model estimates) to validate Nd at cloud base.

In summary, the following text was added to the introduction: “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 supersaturation spectrum of CCN along with cloud base spectrum of updrafts, 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).”

We agree to better carve out the uncertainties of used cloud parameterizations, which is available at the new version of the manuscript.

The uncertainties of Wb of HALO were recalculated for all campaign data. In the new version of the manuscript we ascribe the uncertainties of theoretical estimates assuming 0.3 ms-1 as the Wb uncertainty. This new number changes the values of variables uncertainties used for NdT, Smax and NdCCN estimates.

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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.

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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

Line 36: What is the impact of Wb uncertainty of 0.2 ms-1 on Nd calculation?

A: The Wb uncertainty impacts on average for about 65% on NdT uncertainty.

Line 98: What is the cumulative impact of Wb and Nd uncertainties on Smax calculation and then N0 and k?

A: The cumulative impact on Smax is 22 % on average. N0 and k is not used on Smax calculation, just Wb, Nd and temperature and pressure of cloud base (used on coefficient C estimate at equation 2).

A: We have changed the manuscript to address these questions as follow: “The uncertainties regarding the Smax, NdCCN and NdT estimates for measurements at cloud base with both probes (CCP-CDP and CAS-DPOL) are on average about 22, 20 and

38 % for all flights, respectively (the uncertainty method adopted for these theoretical estimates are available at Appendix A). The Wb uncertainty of 0.3 m s-1 impacts on average for about 65% (60 %) on NdT (Smax) uncertainty, and the uncertainty from the estimated Smax contributes for most of NdCCN uncertainty (∼70% on average)."

Line 194: CDP sample area has not been calibrated before, during, after flight campaign? In this case you may not claim only 10% of uncertainty in SA?

A: The CDP sample area has been frequently measured revealing by the way 0.27mm^2 (not 0.22mm^2, which should by a typo or an outdated value and must be corrected in the text) with an uncertainty of 10%. The uncertainty +/- 0.03mm^2 results from repeated measurements. Unless there is no massive manipulation/disarrangement of the CDP's optics or a detectable aging of the laser diode, the sample area remains stable even if the instrument experiences regular handling during, e.g., field campaign operations.

Do you correct King probe LWC (seems not to be the case), knowing that sensitivity below 10 μm and above may be 30-40 μm is reduced. You are using this probe for LWC reference, however Strapp (2003) demonstrated large deviations of King probe LWC also for larger drop diameters of 40 μm (may be already 30 μm?). Your effective drop diameters reach 26 μm .... Uncertainty of solely 5% in LWC is difficult to believe.

A: The uncertainty of 5 % was referenced with the original paper by King et al.. In order to account for the particles <10um that are not fully detected by the hotwire, we only consider size distributions with an effective radius above 5 um to reduce the contributions from smaller particles. Since the agreement between the Hotwire and CAS-DPOL and the Hotwire and CDP doesn't change with effective diameter, this is a good indicator that the uncertainty of the measurement doesn't change with the growth of the detected particles. Figure 6d shows the size resolved contribution of LWC for effective radii of 26 um. The fraction of CWC from particles above 30 um is one to two orders of magnitude less than the maximum CWC. Therefore we believe that the uncertainty of 5% is justified for the size range considered in this analysis.

Line 309: And what if King probe and CAS DPOL are both wrong and CDP is right?

A: We believe that as they measure the same cloud volume and have good agreement the measurements are correct. This does not exclude the possibility of CDP works fine as we were measuring at very inhomogeneous convective clouds. Also, perfect agreement between both probes is not expected due differences at cloud volume and singular characteristics of each instrument (e.g. sample area, inlet configuration etc.)

Line 339: Why don't you correct CAS DPOL data for your calibrations? Consequently, in your data the CAS DPOL instrument undersizes large droplets! (40 μm in diameter appear as 35 μm drops?). In case your effective diameter droplets of 26 μm would have been 30 μm droplets in reality, you are underestimating LWC by 50% for these droplet sizes: : : Likewise, the King probe is underestimating LWC for other reasons as mentioned above.

A: In Figure 6d (available at supplementary material), we show size distributions and CWC distributions of both cloud probes for large effective diameters. Even for the large effective diameter, only few drops > 40 μm were measured by both instruments CDP and CAS-DPOL. The difference between CAS-DPOL and the CDP above 35um is less than a factor of two, though at very low concentrations. Thus, the difference/error in diameter of these large particles does not contribute significantly to the calculated effective radius. The large droplets contribute an order of magnitude less to the total water content than droplets in the size range around 25 um. Changing the calibration would change the result of the intercomparison of CWC insignificantly. According to Strapp et al. 2003, hotwire probes in general compared well to other LWC instruments up to MVDs of about 32 um, which comprises the largest fraction of the CWC measured during ACRIDICON-CHUVA. To sum up, the disagreement between the CWC of hot wire and the cloud probes represent an upper limit of the cloud probe inaccuracy.

Line 386-394: What is the uncertainty in N0 and k calculation and finally the uncertainty in equation (2) calculated droplet number (calculated each second) when averaging

CCN2 per time step normalized by FA (with two other averages of mCCN1 per time step and TmCCN1 average of all mCCN1 time steps or may be even all CCN1 data)?

A: We have changed the manuscript to address these questions as follow:

“ The calculated NCCN(S) errors for these flight segments are a function of the measured particle number (i.e. 10% of NCCN(S) for large concentrations and the mean of the error is around 20% of NCCN(S)). The estimated standard error (STDE) for the N0 and k parameters and CCN estimates were calculated (as described in Appendix B) for each flight segment and are shown in Table 2. The table shows that the STDE associated to the Twomey’s equation fit is about 5% for the N0 and k parameters. The changes in the air mass assumed to correct the CCN2 for FA during the flight segments were up to 24 % for all flights. As long as the cloud segment compared with this data are not at exactly the same location as the measurements was performed, the mean (i.e. TmCCN1) is a good measure for this comparison. The standard error was used for the error propagation calculations and the resulting error in NCCN(S) is 15 % of NCCN(S) estimates on average. The resulting error of N0 (k slope) was also calculated and is 23 % (20 %) of N0 (k) values on average, associated to the Twomey’s equation fit and the NCCN(S) error.”

“The uncertainties regarding the Smax, NdCCN and NdT estimates for measurements at cloud base with both probes (CCP-CDP and CAS-DPOL) are on average about 22, 20 and 38 % for all flights, respectively (the uncertainty method adopted for these theoretical estimates are available at Appendix A). The Wb uncertainty of 0.3 m s-1 impacts on average for about 65% (60 %) on NdT (Smax) uncertainty, and the uncertainty from the estimated Smax contributes for most of NdCCN uncertainty ($\sim$70% on average).”

Line 400: equation (3) does not pretend Smax depending on NCCN(S). Please detail how Nd can be used to achieve a closure for NdCCN estimate.

A: Ok. The following text was added: " The value of Smax at cloud base can be estimated from Eq. 3 based on the vertical velocity at cloud base and Nd values measured with the cloud probes CCP-CDP and CAS-DPOL (Ncdp and Ncas, respectively). Therefore, the estimated Smax near cloud base can be used in Eq. 1, producing the NdCCN estimates to achieve a closure for Nd measurements at cloud base."

Section 5.2.1.: Gray solid/dashed lines difficult to see in Figs 11 and 12!

A: Ok. We made it thicker.

Fig 11a & 11c show very weak overlap of NdCCN and NdT including both uncertainties. In addition, real Nd measurements can be considerably outside NdT uncertainties and particularly outside the overlap region. Why? What is the value of this study when measurements are not better matching the calculations with their uncertainties? Are the already large uncertainties still underestimated? Are measurements and calculations comparable in their complexity of the respective environments? I don't think so. . ..

A: NdT show a great overlap at Figures 11a and 11c, whilst NdCCN indeed have a weaker overlap (see Figures 11a and 11c at supplementary material). The following text was added to the manuscript: "Both values of Ncas and Ncdp are within the range of the theoretical expectation of NdT and NdCCN, except for occasional deviations at the extreme percentiles. For example, the maximum NdT versus maximum Nd are outside the error interval for Nd. This is so because extreme percentiles are much more prone to random variations than the middle range, such as the median. The lines of NdT mostly agreed quite well with the lines of Nd with only small deviations. The NdCCN mostly underestimates Nd by down to a factor of 0.5 for reasons that we could not identify. Entrainment is not a likely cause, because it would dilute Nd and thus incur NdCCN to be biased positively with respect to Nd. It appears that measuring S in clouds is still a great challenge, even indirectly by using Eq. 3. Remarkably, Eq. 2 (Twomey, 1959), which avoids an explicit usage of S, still performs better when limited within the observed bounds of Wb and k within the cloud."

Fig 12: Color difference of Nd curves (red) and Nd in legend (blue). Can you also show results for AC13 and AC16? Fig 12b and 12c as well as 11c show Nd that significantly exceed NdT for higher Wb. Explanation? The problem stems basically from NCCN2 calculation?

A: The response to the previous comment addresses this comment too.

Line 513: Change 10% to at least 15% if not 20% (AC14!).

A: Ok. Changed.

Line 513-517: and a factor of 1.5 for other cases AC11 and again AC17. Solely AC 13 and AC16 data points ok. Therefore I don't agree with that improper statement. Line 566-567: I would call a factor of 2 in NdCCN* to Nd* comparison a pretty bad result rather than a good agreement.

A: The following text was added to the manuscript regarding these comments: "...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 ± 0.17 around it. The respective result for the CDP is 0.84 ± 0.12. A weaker agreement is observed for comparisons between NdCCN* and Nd* (see Fig. 13b), A factor of ~ 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 ± 0.07. The respective result for the CDP is 0.76 ± 0.1."

Please also note the supplement to this comment:
http://www.atmos-chem-phys-discuss.net/acp-2016-872/acp-2016-872-AC3-supplement.pdf

Interactive comment on Atmos. Chem. Phys. Discuss., doi:10.5194/acp-2016-872, 2016.