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Atmospheric Chemistry and Physics
Discussions

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 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 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

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.

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.

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.

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.

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.

Lines284-288 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.

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

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 $\sim$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. 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.

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

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.

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.

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