



Interactive comment on “Water droplet calibration of a cloud droplet probe and in-flight performance in liquid, ice and mixed-phase clouds during ARCPAC” by S. Lance et al.

Anonymous Referee #3

Received and published: 21 September 2010

General Comments

This paper addresses droplet size distribution measurements of the Cloud Droplet Probe (CDP). Similarly to Brenguier et al. (1998) for the Fast-FSSP, it explores sizing and counting biases for the CDP, although not to the same level of detail as Brenguier. In contrast to the Brenguier paper, the present paper capitalizes on calibration of the single droplet counter with water droplets, a method that was previously used by, e.g., Wendisch et al. (1996) and Nagel et al. (2007). To simulate coincidences (and thus undercounting of water droplets), a Monte Carlo method was used to fully account

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



for the instrument response under near-real conditions. This is slightly different from the simulation of the Fast-FSSP's response by the Brenguier group (thesis by Afranio Coelho).

The paper is very compelling in tying together various aspects which were previously mostly addressed in separate papers: (1) Sizing bias and its removal by use of a water droplet generator; (2) effects of coincidences; (3) modeling of the instrument's response; (4) explanation of real-world data and biases using the model results. Figures 9 and 10 are very impressive. They show that their model of the instrument's response reproduces both the sizing and the counting bias for measurements in liquid and mixed-phase clouds. A by-product of the paper is that the authors can explain all biases without resorting to particle shattering (at least for this particular probe and for the cases shown). The paper is well-structured, figures illustrate the content of the manuscript nicely, and the paper is relatively short (sometimes a little more detail would be helpful, see below).

There appears to be one inconsistency in the paper. If the CDP size bins were shifted by 2 microns to account for intensity heterogeneity within the laser beam, shouldn't that remove the bias that is related to under-sizing? Why then does an additional negative bias occur at low concentrations in Figure 9a? If the shift was already applied, the error for D should be zero for $C=0$. This problem occurs throughout the paper (details below in specific comments). I believe though that it could be addressed by minor revisions. I also believe that the title understates the achievements of this work which consists precisely in the consistency of calibration, instrument response modeling, and field data. As to my knowledge, this was never achieved before, and the title and/or abstract could be a little more self-confident in that respect.

Specific Comments

Most comments concern the aforementioned inconsistency. First of all, the manuscript could use a little more detail about the instrument. For example, you should state

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



upfront that we are dealing with a mono-mode Gaussian laser here (we only learn that quite late in the text). A subsection with a general description of CDP might help. Sections 2.2-2.4 don't quite cover it. Explain how the sizing is done (pulse height analyzer or AD converter?), give more details about the slit used by CDP as opposed to the FSSP annulus, state upfront that this instrument internally accumulates drop size distributions and does not store data for each individual drop. (This is in fact an issue, I believe. Nowadays, with DA systems like NI cRIO around, it shouldn't be a problem to store individual droplets.) Furthermore, explain what the laser intensity looks like within SAQ. If it's a Gaussian laser - shouldn't it vary a lot? Or is the SAQ limited to such a small fraction of a relatively broad Gaussian that there are only a few percent variability? From looking at Figure 7, one cannot tell.

Looking again at Figure 7: Your SAQ contour (small area) encompasses normalized sizer amplitudes ranging from about 0.85...0.95 (if my eyes are properly calibrated). Unfortunately, the SAQ is **not** centered around the maximum intensity of the laser beam within the lateral and longitudinal extent. The red areas (amplitude=1) are a few pixels left. If you aligned the droplet stream such that it maximizes the signal on the oscillator, you were about 0.2 cm away from the center of SAQ. If you had centered your droplet stream in the center of the SAQ instead, you would have had smaller signal amplitudes. Can you discuss the consequences of the mismatch between center of SAQ and maximum intensity of the laser beam on the calibration? Perhaps that's the explanation for the mis-sizing right there.

I don't like the "brute-force" shift of the instrument response by 2 μ m. As you state yourself, the undersizing occurs because droplets don't just pass through the maximum intensity section of the laser beam but also through regions latitudinally and longitudinally away from the center. The fact that Nagel et al. (2007) did a calibration only for the maximum intensity of the laser beam shouldn't keep you back from calibrating the probe all across SAQ. You already did that in Figure 3 and 4 (i.e., you measured 22 and 12 micron droplets at various locations). Why didn't you use these all-SAQ measure-

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

ments to perform a calibration that takes the intensity distribution into account? Strictly speaking, you should hardly ever measure the "greatest CDP sizing response" (p3134, l6) in real-world measurements since droplets cross the laser beam everywhere and the signal will always be below the possible maximum.

I would encourage you to make the following additional plot after Figure 4: Within SAQ, plot a histogram of D_V/D_{true} . Make sure you identify the pixel where you originally did the calibration. At this pixel, D_V/D_{true} should be equal to 1. However, following Figure 7, I am not even sure that the maximum intensity lies within SAQ!

Page 3150 (line 23-26) is slightly confusing. You say that undersizing of 74% and oversizing of 25% may occur within SAQ, but in the same sentence you say that the "most likely" bias is -1.2% (-8.6% on average). The "most likely" vs. "average" vs. "maximum" (-74%...+25%) would be far better illustrated by adding the aforementioned additional figure. Also, how does that relate to the bias shown in Figures 2 and 3? Is Figure 4 based on the CDP response that was shifted by 2 microns? The fact that the "maximum" bias is +25% suggests that indeed it was already shifted - why else should you get a *larger* size across the sampling volume if you already positioned your calibration droplets at the intensity maximum!

Around the discussion of Figure 5, 9, 10, 11 you sometimes don't make a distinction between the two different biases you describe: sizing and concentration. We do not necessarily need coincidences to get undersizing (otherwise, why would we get an LWC bias at $C=0$?). Although coincidences do contribute to part of the sizing and LWC bias, especially at large concentrations, don't forget to mention SAQ heterogeneities as well, especially in the summary/conclusion where this distinction is completely blurred. It is also not mentioned in the abstract (line 9) - biases are not just due to coincidences!

Minor / technical comments

p3136, l13-15: run-on sentence

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



p3139, l25: replace "precise" with "precisely"

p3141, l10: insert "the" between "with" and "long"

p3142, 2.4.1: Doesn't a Gaussian laser do just the same (dampen the Mie oscillations?). After all, we have an intensity distribution within SAQ - that's all we need to smooth out the Mie structure! You could use this section to describe the laser a little better (e.g. percent variability within SAQ)

p3149, l30: "for reasons that are not known" - I don't understand this statement. First of all, the response of CDP in Figure 2 is *not* completely linear, secondly, it does correspond to the theoretical CDP response curve. I assume the CDP pulse analyzers are binned non-linearly, to counteract the $\sim D^2$ signal amplitude.

p3151, 4.2: How do you deal with ice crystals in CDP measurements: Mie code? T-Matrix?

Section 5: This is a great idea. You might want to reference Coelho's thesis, or at least Brenguier et al. (1998). Also: the Brenguier paper lists more possibilities that can occur with coincidences, you only list the (most relevant) cases.

p3156, l5: This section (description how you simulate the response) comes too late, it should be introduced earlier.

p3156, l24: Replace "droplets" with "droplet"

Interactive comment on Atmos. Meas. Tech. Discuss., 3, 3133, 2010.

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

