Interactive comment on "Laboratory and In-flight Evaluation of a Cloud Droplet Probe (CDP)" *by* Spencer Faber et al.

Anonymous Referee #2

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This is a thorough and concise manuscript which I recommend for publication after a couple of changes have been considered. Laboratory and airborne field characterizations of a commercial cloud droplet sizing spectrometer (CDP, Cloud Droplet Probe) are reported. As such it fits well into the scope of Atmos. Meas. Tech.

I have three general and a number of specific comments/suggestions:

We appreciate the feedback from the anonymous reviewer. Below, inline, we provide responses to the reviewer's general and specific comments.

General comments

Some discussion about the general applicability of the presented test results of a specific instrument of the CDP type to other instruments of the same type than tested here is required. To which extend the reported sizing deviations are systematic problems of the CDP, and to what extend they hold for the specific probe used here only?

We strongly agree with the sentiment that 'to what extent our findings, for this specific probe, are more generally applicable to other CDPs' is an important question'. In the original manuscript, we briefly mention at the end of paragraph 1 in the summary that the lateral dependence of sizing was also measured in two other CDPs tested on our system. It turns out, that those measurements also indicate a 1-2 um oversizing through much of the sampled region for drops larger than about 25 um. We will add the results of these measurements to the summary in the revised manuscript. However, to some extent, these measurements are anecdotal only. But they do show at least some consistency across different CDP instruments.

We also note in the original summary that a similar lateral dependence on sizing was reported by Lance et al (2010); although (and not discussed in our original manuscript) their limited resolution and test sizes did not reveal how this dependence changed with droplet size. The position dependent-sizing described by Lance et al (2010) shown in their figure 6a & 6d differed from our results. In particular, where it occurred within the sample volume--in all of our tests, the mis-sizing of droplets was rather symmetric with a lateral dependence across the sample volume (laser beam), and a small region near the detector where droplets were severely undersized. It may be that the specific probe described by Lance et al was not well-aligned (as suggested in their paper), and for later generations of the CDP, the optical alignment is better positioned/characterized by the manufacturer. Regardless, the missizing, in response to sample location, appears to have much better symmetry with the sample volume than reported by Lance. This discussion is added to the summary section of the revised manuscript (now revised as Section 6: Summary and Discussion)

It should be made clearer which progress has been achieved over the results reported previously (in particular with respect to Lance et al., 2010, 2012). Is there more than the incorporation of computerized position stages, instead of manual positioners as used by Lance et al. (2010)?

We discuss some of this above in our response to general comment #1. In terms of the droplet generator system, yes, only the addition of computerized stages has progressed over the system originally designed and described by Lance et al (2010). However, this manuscript is meant to focus not on the droplet generating

system, but rather how using that system enables further evaluation of CDP performance. To that end, the UW droplet generator provides a more thorough set of the experiments covering a broader range of droplets with higher spatial resolution across the sample volume. Lance et al.'s 2010 work tested CDP response at a grid of location throughout the sample area using only 12 and 22 µm droplets. This study includes similar experiments for seven droplet sizes, conducted at a much finer spatial resolution.

The revised manuscript elucidates how these additions provide a greater understanding of instrument response across its entire sample volume over the range of particles it was designed to measure. Much of this addition is in the summary and discussion in the revised manuscript. We also lay the groundwork for this in the introduction (Section 1) and discussion of the UW droplet generating system (Section 3).

It should be discussed that the two major parts of the paper (laboratory and field observations) actually don't have much to do with each other. In the lab the sizing was tested, in the field LWC data were evaluated. Also, droplet sizing spectrometers are not made to derive higher order moments of the size distribution, deriving LWC from this type of measurements is originally not intended by these droplet sizers. The immanent sizing uncertainties amplify (cube) in LWC. For LWC measurements different operating principles are much more appropriate, such as the well tested hotwire probes (e.g., the Nevzorov probe). Therefore, it is kind of unfair to compare the extremely error prone LWC data from the droplet sizing spectrometer to the much more straight-forward hotwire bulk probe LWC measurements. Why should you use a droplet spectrometer for LWC measurements if a bulk hotwire instrument is cheaper and much more accurate? I don't say that it makes no sense to compare both LWC measurements, in an ideal world both LWC measurements would agree. However, in the real world I am not surprised at all, that there is a huge discrepancy between the two approaches as illustrated in Figure 6 of the paper. I suggest to include a short discussion on this subject into the manuscript.

In terms of cloud property measurements, closure studies are very important. One way we accomplish this is by comparing different types of measurements--for example, those that respond to different aspects of the particle size distribution such as the 2nd moment (effective radius), 3rd moment (mass), or 6th moment (Rayleigh reflectivity). The point of all of this is that under the appropriate conditions, these closure tests should agree to within the uncertainty of the individual measurements after allowing for error propagation. The fact that we do not find complete agreement between the bulk LWC and the CDP-derived measurements is not a surprise (as you state). But rather, that the disagreement between the two is greater than what is able to be quantified by our best estimate of uncertainty in *both* measurements is disconcerting. This tells us that our error quantification for one (or both) of the measurements is incorrect. In the revised manuscript we add discussion in this regard. For the case presented here, one must accept uncertainty in LWC greater than that which is suggested based strictly on the Hotwire (or CDP) uncertainty estimates alone resulting from this closure argument. In the revised manuscript, we add discussion to this regard in section 5.4.

Specific suggestions

Title: The wording in the title is inaccurate in the sense that not the CDP probe itself, but its measurement uncertainties in terms of droplet sizing and LWC are evaluated. Maybe the title could be changed to something like "Laboratory and In-flight Evaluation of Uncertainties of Droplet Size and Liquid Water Content Measurements of a Commercial Cloud Droplet Probe". BTW: I always try to avoid acronyms in the title.

We have changed the title in the revised manuscript to: "Laboratory and In-flight Evaluation of Uncertainties of Measurements from a Cloud Droplet Probe (CDP)"

We acknowledge that the <u>measurements</u> are being evaluated and we spell-out the name of the probe. However, we suggest maintaining CDP within a parenthetical because it is well-established in the airborne cloud physics community (such as FSSP).

Abstract: Please quantify the seven droplets sizes generated in the lab; otherwise it is unclear what you mean with "For the smallest diameters" (lines 5-6 of abstract) and with "For all larger diameters" (line 7).

In the revised abstract we are explicit about the drop sizes tested. We state: "...for 9 um drops the CDP undersized drops by 1 - 4 um…Droplets of 17 to 24 um were sized to within 2 um of the correct diameter… ...For droplets 24 um and larger …"

1. Introduction

I always try to avoid mentioning specific companies (PMS, DMT) in scientific papers, we should not advertise. That should be replaced by appropriate references to reviewed papers.

Fair enough. While this is difficult in such a small field and (because of that) we often have probes with similar acronyms (for instance the DMT CDP and the SPEC F-CDP which are two different probes)...we have revised the manuscript to remove manufacturer's name wherever possible. However, in some instance, no other references to certain probes exist and we provide links to online instrument manuals.

I am glad you appreciate the major contribution of Lorenz and call it the "Mie-Lorenz theory" instead of mentioning Mie only. However, to be consequent you should call it "Lorenz-Mie theory", Lorenz was first! And please stay consistent, later in your text you forget Lorenz and use "Mie-theory" only.

Instances of "Mie" have been standardized to "Lorenz-Mie" throughout the document.

I suggest to include the following additional reference when discussing the FAST-FSSP and instrumental broadening of size distribution: Schmidt, S., K. Lehmann, and M. Wendisch, 2004: Minimizing instrumental broadening of the drop size distribution with the M-Fast-FSSP. J. Atmos. Ocean. Tech., 21, 1855–1867.

This reference will be included in the discussion of OPC sizing uncertainty sources.

Page 3, first paragraph: You imply that Lorenz-Mie theory only holds for small droplets, which is not true. It is just not appropriate to apply Lorenz-Mie theory for larger particles; it is much more efficient to use geometric optics for larger particles. But in principle Lorenz-Mie theory is not restricted to small droplets, please would you make respective changes to the text.

Of course, you are correct...all scattering follows Lorenz-Mie theory. In the revised manuscript we state: 'due to the relationship between particle size (d~10-50 um) and wavelength of incident radiation (lambda~658 nm), full Lorenz-Mie theory calculations must be considered to accurately relate droplet size to scattered intensity.

2. CDP Operating Principle

A schematic drawing of the CDP would greatly help readers not familiar with this type of instrument to follow your explanation of the principal of operation of the CDP. Alternatively, you may drastically reduce the description of the operational principal and refer to respective literature.
I mostly avoid to refer to gray-literature manuals provided by companies in scientific texts. If unavoidable, just give the web site address.

We chose not to include a schematic of the CDP because this is included in both Lance et al. (2010) and the DMT manual (2014). However, we felt a brief description was appropriate here with the appropriate references at the end of the paragraph.

In principle, we agree with your comments regarding 'gray literature'. However, very little is available in the peer-reviewed literature regarding 'relatively' new instruments such as the CDP. Perhaps, even more important, is that in-production probes are constantly changing. By capturing manufactures' snapshots of designs at a specific time (based on a specific manual revision, 2014 in this case) we can help alleviate confusion resulting from comparisons of differing probe generations.

The differences between FSSP and CDP should be emphasized, progress achieved by CDP in addition to the revised electronics should be highlighted.

The revised version of the manuscript will include discussion about other improvements made to the CDP (in addition to revised electronics and the exclusion of the laser shroud) including the unimodal laser intended to provide more homogenous laser intensity within the sample area and the differences in qualifier mask design.

Last line in paragraph 20 on page 4: Please quantify "high droplet concentration".

Standard coincidence depends upon flight speed (faster speed, greater probability for two particles being in the sample volume at same time). But at most research aircraft flight speeds (~80-120 m s-1), standard coincidence is anticipated at less than 5% for concentrations on the order of 500 cm-3 based on a qualified sample volume of 0.3 mm2 (Lance et al. 2010).

3. University of Wyoming Droplet Generating System
You need to make clear why you partly repeat work done already by Lance et al. (2010). A schematic illustration of the droplet generator setup might be helpful.

As noted above (and elucidated in the revised manuscript), the focus of this work is not to expand on the droplet generator setup (from Lance et al. 2010) but rather to expand on the measurements they provide. The revised manuscript describes how the UW setup allows a greater range of testing, across the full dynamic range of droplet sizes measured by the CDP and the full volume sampled.

Droplet speed measurements are discussed, at this point the reader asks why this is important. Later it becomes obvious that this is needed to compare with droplet velocities occurring in real aircraft measurements. Just make sure the reader understands here that these measurements have some meaning for the later text.

In the revised manuscript, we add text to describe to the reader that velocity measurements are needed to ensure that droplets are traveling at speeds within the operational airspeed of the CDP and to compare droplet velocity with typical aircraft airspeeds.

Maybe you briefly discuss about droplet evaporation during generation and transport to the sampling volume of the CDP.
How about mechanical distortions of the droplet generation setup and its impact on droplet generation, is there a problem with small-scale wind turbulence?

We, in part, control droplet size through evaporation. Increased residence time within the sheath flow increases evaporation and allows us to tune our experiment to a specific drop size (i.e. increasing residence time in the sheath flow reduces droplet diameter...therefore we can increase/decrease diameter by ejecting the droplets later/earlier in the flow field). The range of residence time in the sheath flow is provided in the revised manuscript.

Turbulence in the vicinity of the flow tube exit is an important consideration and is the main factor limiting droplet velocity. It turns out that turbulence at the point of droplet generation does not appear to be important (based on high speed photography of droplet generation events). However, the acceleration of those droplets (once generated) may induce distortions in the droplets as they are ejected from the accelerated flow into the CDP sample volume. Because we measure (independently) the size of droplets as they pass through the sample volume of the CDP (through the glare technique) we assume any impact of distortion is accounted for; however departures from spheroidal shape may result in slight mis-sizing...this is acknowledged in the revised manuscript; although the data collected does not allow for quantification.

4. Results of Droplet Generator Tests on the CDP

Second paragraph in 4.1: Quantify “Smaller droplets”, “shorter test periods”, and “less stable”.

In the revised manuscript we better describe the constraints of the experimental setup. In particular, for the smallest droplet produced (9 um) we needed to complete the test runs in roughly 25% of the time required for larger droplets. This was because we found that after a certain length of time, the size of the droplets (and/or the position of the droplets) was no longer predictable. Because of the shorter test periods, the resolution of the measurements was reduced and the number of droplets at each location was also reduced. The same was true for the 17 um resolution test, although the test was conducted over roughly 75% of the time required for larger droplets; so that the statistics are significantly improved. All other tests (for drops 24 um and larger) were completed at the same temporal and spatial resolution.

Figure 1: Maybe you add a similar figure just including droplets passing through the center of depth of field. The axis labels are way too tiny. Coloring is not optimal in my point of view.

The font size of axis labels for Figure 1 will be increased for better readability. The semi-transparent bars in Figure 1 are blue and a neutral grey so that overlapping regions don’t appear as a third color. Changing the grey bars to a more obvious color could complicates interpretation of the figure.

Center of DOF measurements can be quite misleading. While this may (or may not) represent how the manufacturer calibrates the CDP, it does not represent what the probe itself measures in a cloud. In fact, we

assert that the range of droplet mis-sizing quantified in Figures 1 and 3 is key to understanding how best to calibrate the probe. As we discuss in the summary, determining whether to calibrate based on particle modal diameter or mean diameter has impacts across the droplet spectrum. Because the degree of mis-sizing depends on the droplet location within the sample volume, we believe that plotting the droplet size within a specific location (such as the center of DOF) is misleading and can result in wrong conclusions. For this, we maintain, that describing the response of the probe throughout the entire sample volume using a (nearly) monodisperse distribution is the best way to represent measurement sizing accuracy.

I don’t see any reason why the pure counting efficiency is not exactly 100 %, please comment on that.

In general, we agree. Of course, on the very edges (within one 10 X 10 um sample bin) a droplet might fall on the edge resulting in reduced counts...but the more curious points in our study result from the over counting in the small region near the detector. Our results suggest something is occurring that is not well accounted for in the model of how the probe responds. We have no suggestion as to what might cause this result. While the overall effect (impact on total number counted) is small, it is curious that it occurs in the region located between the ‘expected’ normal sample volume and the ‘region outside of the expected sample volume, where particles are severely under-sized’. The revised manuscript discusses this in sections 4 and 6, but at this time we don’t offer any concrete explanations.

We find it curious that Lance et al (2010) found significantly larger mis-counting at different locations for their two tests (Figs 6b & 6e). Their results appear much more difficult to reconcile than our findings. This is discussed in the revised manuscript.

5. Comparison of . . . (please use capital letters for the section title, as you did before)

Some words in the section title are not capitalized in order to follow AMT guidelines: ‘titles and headings should follow sentence-style capitalization standards (only first words and proper nouns are capitalized)’. Section titles with unnecessary capitalization will be corrected.

As discussed above already, you are entering a new world here. Anyway, these data cannot be compared to your lab studies. Just discuss this, I don’t want you to delete this section.

Agreed, our discussion above (and included in the revised manuscript) makes this point.

Sometimes you duplicate/repeat figure captions in the text.

The revised manuscript removes these duplications.

The discussion of the differences in Figure 6 is kind of speculative. This is unavoidable, at least partly. A reader could ask why you mostly use data collected in complicated super-cooled conditions with quite some chance to encounter mixed-phase clouds. The matter is already complicated enough in pure liquid water clouds.

Discussion of Figure 6 is quite speculative. Perhaps the main takeaway from this section is that further work using different data and additional lab experiments is needed to get a better understanding of exactly what is leading to the greater than expected difference between LWC estimates provided by the two instruments.

A majority of data used in this analysis were taken in super-cooled conditions because there were very few cloud penetrations with temperatures greater than 0 C encountered during SNOWIE and PACMICE. Comparing measurements from the two instruments in conditions that could contain liquid and ice particles certainly isn't ideal but IWC estimates from the Nevzorov were used to exclude periods that contained a significant number of ice particles. There are surely some mixed phase penetrations still included in the analysis but we believe ice particles minimally affected results because the Nevzorov LWC sensor has been shown to only be sensitive to 12% IWC (as demonstrated by Korolev et al., 1998), with IWC typically being small compared to LWC in mixed phase conditions. Also, Lance et al (2010) and Khanal (2018) have shown that the CDP is not greatly affected by natural or shattered ice particles.

The discussion on the droplet speed influence is not satisfying. No way to at least roughly test something in this regard?

Ideally, additional droplet generator tests using greater velocities would have been conducted to test how droplet speed influences the results. This research was conducted to satisfy requirements for a masters degree so there unfortunately was not time to make necessary modifications to the laboratory setup and run additional experiments.