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Atmospheric Measurement Techniques Discussions

AMTD

3, C1818–C1853, 2010

Interactive Comment

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.

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GENERAL COMMENTS

"I think the work done here is of very high quality and of great importance to the field. There's no question that it's worthy of publication. That said, I'll have to admit that reading it was more frustrating than I think it should have been. One of the main issues seem to be that many of the key figures aren't as clearly explained as they should be. Details below. Interpreting the figures was also difficult for me sometimes, as I wasn't really sure what I was supposed to compare on a given figure. The figure captions are very sparse and could be a lot more helpful to the reader. But more than this, I think there are parts where the text is lacks explanation and clarity. I've tried to point out the main spots below. Definition of symbols in some places also lacking (especially n_D: you MUST be consistent with the usage of n_D!)."

We now include more information in the figure captions and more explicit (and consistent) use of symbols.

"A second big issue is the author's use of "ideal CDP" instrument. The comments below dig into this issue somewhat, but I'll summarize here: the calibration system "calibrates" the instrument at maximum signal. The authors then conclude that drops passing through less intense parts of the beam leads to "undersizing" since the pulse amplitude is smaller. The result is a 2 um shift between "ideal" and "realistic" CDP isntruments. I'm not going to force the authors to change this interpretation, but I disagree with this thinking. . . I really do think this transfer function approach would be more useful, too, as the transfer function width gives important information about the instrument resolution, and knowing the transfer function might also be used as the basis for an inversion to better recreate the parent or true distribution (as in DMA inversions)."

By defining the maximum CDP response as the "correct size", we are following the precedent of previous studies (e.g. Nagel et al., 2007; Wendisch et al., 1996). However, since this issue has sparked confusion for several people, including many of the reviewers, we now define the "correct size" as the average CDP response instead of the maximum CDP response. A type of transfer function (which has been called a "distortion matrix" by Wendisch et al [1996] and Nagel et al [2007]) is now specifically discussed in the paper. We discuss this in more detail in response to specific questions below.

Wendisch, M., A. Keil and A.V. Korolev, FSSP Characterization with Monodisperse

AMTD

3, C1818–C1853, 2010

Interactive Comment

Water Droplets, J. Atmos. Oceanic Technol., 13, 1152-1165, 1996.

Nagel, D., U. Maixner, W. Strapp and M. Wasey, Advancements in Techniques for Calibration and Characterization of In Situ Optical Particle Measuring Probes, and Applications to the FSSP-100 Probe, J. Atmos. Oceanic Technol., 24, 745-760, DOI: 10.1175/JTECH2006.1, 2007.

"The comments below don't necessarily reflect this, but I think the importance that the qualifier and sizer signals take on later in the paper require a lot more careful explanation of how this works in the instrument. A figure illustrating this would go a long way to helping the reader out."

The basic operation of the two detectors (qualifier and sizer) are essentially the same as in the CAS and the FSSP, which have been written about in detail in many papers (e.g. Baumgardner et al, 2001; Burnet and Brenguier, 2002; Hovenac and Lock, 1993). However, since the optical geometry is central to our discussion of coincidence errors, and since it seems that there was confusion about certain aspects of this, we now add a figure to illustrate the basic optical configuration of the CDP (Figure S1).

Baumgardner, D., H. Jonsson, W. Dawson, D. O'Connor, R. Newton, The cloud, aerosol and precipitation spectrometer: a new instrument for cloud investigations, Atmos. Res., 59-60, 251-264, 2001.

Burnet, F. and J.-L. Brenguier, Comparison between Standard and Modified Forward Scattering Spectrometer Probes during the Small Cumulus Microphysics Study, J. Atmos. Oceanic Technol., 19, 1516-1531, 2002.

Hovenac, E. A. and Lock, J. A., Calibration of the forward-scattering spectrometer probe: modeling scattering from a multimode laser beam, J. Atmos. Ocean. Tech., 10, 518–525, 1993.

——— SPECIFIC COMMENTS ———

"page 3136, line 1: I prefer in situ in italics since it's Latin. NY Times agrees. You may not, however."

We are following the AMT/ACP formatting guidelines.

"line 4: does this include sources of uncertainty? (random, not biases)"

yes, optical cloud probe measurements are subject to uncertainties, in addition to biases and limitations. We now include this word here.

"5: i'm not sure drop counting uncertainty is the same as view volume uncertainty. i'd make them separate. "

You are correct – these are not the same. Neither is sufficient for characterizing the uncertainty in droplet concentrations (since uncertainty in the aircraft velocity is also important). Now, we instead say "Uncertainties in droplet concentration and mean diameter..."

"5: "greater uncertainty": not necessarily - if uncertainty is bigger for small drops than big drops, then higher order products should be better off. yes, it's a bit pathological but possible..."

A given uncertainty in mean diameter will always lead to a greater uncertainty in CDP-LWC. We are now more specific in the paper.

"14: at some point might want to point out this sample volume is drop size dependent for many instruments, even if it is treated as a constant."

Good point. We now do this.

AMTD

3, C1818–C1853, 2010

"20-23: this sentence seems a bit obtuse. it needs to make its point more directly, i think."

We have modified this statement slightly...

"32: “standardized... sample area” that sounds a bit odd... like there’s a tube goes right up to the laser."

Actually – yes that’s correct. There is a tube that goes right up to the laser, similar to what is done in this paper (See Figure 1b of the paper).

"3137, 1: this doesn’t seem like a very useful sentence. i’d take the next sentence and divide into two: 1. response depends on ref index then 2. thus response to water must be calculated..."

This sentence has been reworded.

"22: it’s not the flight that matters, though, is it? it’s more that it’s a population measurement at high speed rather than drop-by-drop at low speed, right?"

The “in-flight” performance of the probes is what matters in the end, since the probes are designed for flight operation (although they can be modified for use on the ground or on tethered balloons). There are a variety of issues that can arise in-flight that are not evaluated with typical calibration procedures (such as differences in speed and coincidence errors, in addition to particle shattering on the probe housing), which are discussed in this paragraph. There may be other issues, such as changes in droplet trajectory as the airflow is accelerated around the cloud probe housing or around the

AMTD
3, C1818–C1853, 2010

Interactive Comment

aircraft itself. Using the general term “in-flight performance” includes any measurement artifact that arises during flight, which may not occur during ground based calibration.

"32: “continuously... transmitting”: a bit roundabout - don’t you really mean that it’s difficult to consistently generate a population of known concentration and size distribution (and, for ice crystals, crystal shape)."

We change “continuously generating and uniformly transmitting” to “consistently generating and transmitting” in this sentence. Both the generation and transmission of particles is important.

"3141, 5-6: I’m still a bit unclear how this works. This sentence makes it sound like the total light scattered is then divided between the qualifier and sizer, whereas later I infer that at least the intensity as a function of scattering angle information must be preserved. I think a picture would help this out immensely. I also think it’s such an important part of what happens later that you should take the time to explain this well."

You are correct that the scattered light is collected and split between the qualifier and sizer. It is unclear why you would infer that the intensity as a function of scattering angle is preserved, since that is not written here or elsewhere, so we cannot respond about this. To assist the reader, we now add a figure to illustrate the basic optical configuration of the CDP (Figure S1).

"10: Does the qualifier algorithm depend on drop size? If not, should it? Seems to me like it should (can’t use the same intensity threshold for 4 and 40 micron drops). If it does, does it make the sample area drop size dependent?"

The qualifier algorithm depends on the relative intensity measured by the two photode-

AMTD

3, C1818–C1853, 2010

Interactive Comment

tectors, as written in the preceding paragraph: “When the qualifier voltage is larger than the sizer voltage, the particle is considered inside the DoF and is therefore counted”. Since both the qualifier and sizer signals increase with particle size, the threshold also changes with particle size. This is why the sample area is not strongly droplet size dependent (we were unable to discern a difference in SAQ between 12 and 22 um droplets).

"I recommend you add approximate dimensions (lateral and longitudinal) for both the SAQ and SAE to give people a sense for the values. I was surprised to see the difference between these in Fig 7. Are the qualified values substantially different from that for the FSSP?"

We now quantify the lateral and longitudinal dimensions of SAQ and SAE in the text. Yes, the values are comparable between the CDP and FSSP for SAQ [Schmidt et al, 2004].

Schmidt, S., K. Lehmann and M. Wendisch, Minimizing Instrumental Broadening of the Drop Size Distribution with the M-Fast-FSSP, J. Atmos. Oceanic Technol., 21, 1855-1867, 2004.

"24: “diffracted” why just diffraction being considered in this sentence? i think "scattering" is better."

In previous versions of the paper, there was more description about reflected and refracted light interfering with the diffracted light, resulting in a nonmonotonic relationship between droplet size and scattered light collected over a given angle. Since this is well established in the scientific literature, and can be adequately summarized simply as “Mie theory”, we removed these statements. We now remove this first sentence as well.

AMTD

3, C1818–C1853, 2010

"24: “proportional...” not true for all drop sizes - only geometric scattering, definitely not Mie or Rayleigh scattering"

See above.

"3142, 2: and as you point out earlier, a model of the optical setup."

Good point. We add this here.

"3145, 26: might this velocity change cause a temperature of the air? if so, any chance the drop size responds to this?"

Yes, the temperature of the air increases as a result of airflow acceleration. However, the drop size is not expected to respond significantly to this increase in temperature given the short duration that the droplets are exposed to these changing conditions prior to detection (< 1 ms, given a generous 10 cm boundary layer upstream of the laser beam and an aircraft velocity of 100 m/s). Even if ram pressure heating could bring the temperature to 300K and the relative humidity to 0% (unrealistically hot and dry conditions), 10 um droplets would shrink due to evaporation by less than 1% if exposed to those conditions for 1 ms (See Figure S2).

Seinfeld, J.H. and S.N. Pandis., Atmospheric Chemistry and Physics: From Air Pollution to Climate Change”, John Wiley and Sons, Inc., 1998.

"3147, 30: “10% greater” any sense for the uncertainty in this correction?"

Figure 2A and Equation A7 in the paper by Wendisch et al [1996] shows the nonlinear relationship between the ratio Dglares/Dtrue and the viewing angle. At a viewing angle of 130 degrees, a change of 10 degrees results in a change of < 1.6% for Dglares/Dtrue. The uncertainty in viewing angle for our calibrations is actually much less than 10 degrees. We now add this to the paper.

Wendisch, M., A. Keil and A.V. Korolev, FSSP Characterization with Monodisperse Water Droplets, J. Atmos. Oceanic Technol., 13, 1152-1165, 1996.

"3148, 8: what is uncertainty in drop velocity measurements using this method?"

For velocities < ~45 m/s, the velocity can be determined to within 2 m/s (this is the uncertainty in the slope of the glares length versus the shutter speed) for 20um droplets. However, at greater velocities (up to ~70 m/s), with our metrology system, this slope must be determined from only two shutter speeds, and the uncertainty becomes indeterminate. Also, for smaller droplets, the uncertainty increases because the glares are much dimmer.

"3149, 5: Is “Fundamental” needed in the section title?"

Since it is not clear to all reviewers why the word “fundamental” was used to describe these calibrations (because single droplets are directly injected into the sample area of the CDP), we now leave the word out.

"7: “initially calibrated” at what velocity?"

The velocity of PSL spheres and glass beads during calibration is not known. This is typically not reported for this type of calibration (we’ve never seen it reported). However, the pulse widths (2x the Gaussian standard deviation of the pulses) were on the order of 5-30 us, which indicates that the velocities were slower than the water droplet calibrations (and also more variable).

"10: this phrase is a bit confusing. does a "sizer" have a "pulse amplitude"? i found this section very confusing. i read this paragraph at least six times and still can't totally understand it. the topic sentence talks about psl and glass beads. this third sentence doesn't seem to follow from the first two (nor does the fourth). then discussion goes back to the calibrations."

The “sizer pulse amplitude” corresponds to “the maximum amount of light collected by the sizer as a droplet transits across the laser beam”. This is quite explicit. We should have written “particle” instead of “droplet” in this sentence, however, because the sizer pulse amplitude was recorded for all of the calibrations (with PSL, glass beads and water droplets, alike).

We have modified this paragraph to try and make it easier to understand:

“The CDP was calibrated with PSL spheres, glass beads and water droplets. For each of these calibrations, the standard CDP binned size distributions were recorded. In addition, for a subset of particles, the waveforms of electronic pulses were recorded using an oscilloscope to sample both the sizer and qualifier signals. The amplitude of electronic pulses recorded by the sizer (which we refer to as the “sizer pulse amplitude”) corresponds to the maximum scattered light intensity detected by the sizer as a particle transits across the laser beam. The sizer pulse amplitude for these calibrations is plotted on the left axis of Figure 2. Plotted on the right axis are the theoretically determined response functions of the CDP for different particle refractive indices, calculated from Mie theory. The range of collection angles for the theoretical curves illustrates the expected sensitivity of the CDP response to changes in the droplet position within the qualified sample area.”

“Glass beads were aspirated from a small vial and through a tube positioned over the sample area of the CDP using dry compressed gas. The PSL calibrations were performed using a nebulizer followed by a diffusional dryer to evaporate the water from the

PSL particles. The PSL particles were then transmitted across the sample area of the CDP using the evaporation flow-tube. For both the PSL and glass bead calibrations aggregation of generated particles is possible, which would result in a bias in the measured pulse amplitude. Coincidence is also possible, but is extremely unlikely for the PSL calibrations, since particle count rates were less than 0.1 Hz."

"Leads to Fig 2 issues as well - it's not clear what is supposed to agree with what (like the red symbols, the way the cal was done, shouldn't actually correspond to the blue "curve", for example). left and right axes reversed in caption? also, if units are Watts, add to axis label. I like the uncertainties for the calibration points, but they're hard to see - I'd suggest making them darker (esp. for the PSL)."

The red symbols represent single droplets that have been transmitted across SAQ at the center of the DoF. We mistakenly wrote in the paper (Page 3149, Line 25) that these points represent the maximum sizer pulse amplitude, when, rather, the qualifier pulse amplitude is maximum at this location (See Figure S3). We made this mistake again, by incorrectly stating (Page 3150, Line 4) that "…the calibration using water droplets is constrained to the center of the DoF where the scattered light signal is highest…" In actuality, the scattered light signal (recorded by the sizer) is greatest at the edge of the DoF, not at the center of the DoF (again see Figure S3).

We now include in Figure 2 of the paper the sizer signal averaged over all positions within SAQ (Figure S4), obtained from the calibrations reported in Figure S3 (for 12 and 22 um droplets, respectively). The average response agrees well with the response reported for droplets at the center of the DoF. Again, this contradicts the statements above that we are now retracting from the paper.

Thus, the reason postulated (Page 3150, Line 4) for a greater than expected scattering signal for the water droplet calibrations is not valid (i.e. the red symbols really should correspond to the blue curve, according to our optical model of the instrument). We

AMTD

3, C1818–C1853, 2010

have removed these incorrect statements from the paper, and offer a new hypothesis for the discrepancy between the modeled and calibrated instrument response for water droplets (See our response to the next question).

This also means that the 2um shift is actually appropriate for correcting the droplet volume mean diameter for the ambient observations, and the CDP-LWC bias should be close to zero at low droplet concentrations. We suspect then that the negative CDP-LWC bias observed at low droplet concentrations results from a positive bias (∼36%) in the King-LWC measurements in mixed-phase conditions. We did not correct for the ice bias on the King-LWC measurements, because we do not know of a correction procedure during sustained sampling in mixed-phase conditions that does not require a good deal of manual manipulation, which we felt was subjective, especially given the possibility of measurement hysteresis. However, we estimate that the presence of ice can shift the King-LWC baseline by as much as 0.08 g mˆ-3, since LWC can remain elevated at this level for several minutes after exiting a cloud. When the King-LWC is in the range of 0.1-0.2 g mˆ-3, which is common for the mixed-phase clouds sampled, this results in as much as 40-80% error. Further evidence in support of this theory is the linear fit to the liquid-only observations, which has an intercept much closer to zero (Figure 5 of the paper).

Yes, the caption has the left and right axes reversed – that has been fixed. We also darken the error bars for the PSL and glass bead calibrations.

Figure S4 Caption: Figure 2 from the paper with additional water droplet calibrations added. The average sizer pulse amplitudes (as well as the standard deviation, minimum and maximum) are shown for calibrations with 12 um and 22um water droplets, respectively, over all positions within SAQ. Also shown are water droplet calibrations of various sizes near the center of the DoF on another day (about 6 months after the calibration that was originally reported in the paper). These added data illustrate that calibrations at the center of the DoF are representative of the average instrument response.

AMTD

3, C1818–C1853, 2010

"30: I'm also very surprised by this - and I think it deserves more discussion than this. Other people have been able to reproduce this behavior in other optical instruments, and I'm assuming it's been done for the FSSP though I don't know for sure. But in any case, it must say something important about either the optical model used for the instrument or the calibration method or the instrument itself. Is there some way to address this further?"

This is a very good point – and we have spent a good deal of time attempting to address it. We calibrated another CDP in the lab and were able to reproduce Mie resonance structure. Therefore we do not believe that there is a problem with the calibration method. We have also duplicated the monotonic response with the CDP reported in this paper (Figure S4).

Pinnick et al [1982] showed calibrations of the FSSP with glass, aluminum and latex spheres, which followed Mie theory well (including the resonances). Knollenberg [1976] suspected that Mie resonance structure could be dampened with the use of a multi-mode laser, and Korolev [1985] used this idea to explain the lack of observed resonance for their FSSP when calibrated with water droplets. However, Pinnick et al [1982] stated that "...the theoretical response calculations adequately predict the FSSP response for spheres, regardless of effects that may be caused by multimode operation of the instrument laser source that might render the plane wave assumption in Mie theory invalid". Also, simulations by Hovenac and Lock [1993] did not show significant suppression of Mie resonances for a multimodal FSSP laser beam. The CDP does not use a multimodal laser, however, other non-idealities in the instrument performance could potentially result in a response that differs from the Mie calculations.

We speculate that the optical model is insufficient for this particular instrument due to optical misalignment (beyond the symmetrical ± 0.5 degree shift modeled). If the qualified sample area is out of alignment with the center of the laser beam, this would result

AMTD

3, C1818–C1853, 2010

in a different range of collection angles for qualified droplets than the 4-12 degree specification. Incomplete blocking of the primary forward scattering lobe can result in a dampening of the Mie resonance structure (if the misalignment is nonsymmetrical) and also an increase in scattered intensity (Figure S5). The result of this type of misalignment was determined by integrating the Mie solution over different collection geometries. A lateral displacement of $\sim$1 mm for the qualified sample area relative to the center of the laser beam, results in a change in collection geometry for qualified droplets as depicted in Figure S6. We now add a paragraph in the paper describing the simulated effect of this type of nonsymmetrical optical misalignment and also an appendix describing the technical details of these simulations.

Figure S5 Caption: The calibrated response of the CDP to PSL and glass beads, and the calculated response for PSL, glass beads and water droplets assuming that all forward scattered light 4-12 degrees is collected by the sizing detector. The light blue dots show the modeled CDP response for water droplets given a 1 mm misalignment in the y-dimension (laterally across the laser, and perpendicular to the droplet trajectories). The dark blue dots similarly show the modeled CDP response for water droplets given a slightly greater misalignment. The fit to the water droplet calibrations is shown for comparison. The CDP response to PSL and glass beads, given these misalignments, are also shown, and often agree well with the PSL and glass bead calibrations.

Figure S6 Caption: Calculated scattered light intensities for 10 um (left) and 40 um (right) droplets, respectively, as projected on a flat plane at 4 cm distance from the droplet (the approximate location of the dump spot, which nominally collects light 0-4 degrees off-axis, Fig. S1). Image intensity represents the logarithm of scattered irradiance normalized by the incident laser intensity, for an unpolarized light source with lambda = 658 nm. The solid white circles represent 4-12 degree symmetric collection angles. The dotted white circles represent the CDP light-collection geometry accounting for a lateral misalignment of 1 mm in y (top) and 1mm in both x and y (bottom) for qualified droplets relative to the intended optical alignment.

Hovenac E.A. and J.A. Lock, Calibration of the Forward-scattering Spectrometer Probe: Modeling Scattering from a Multimode Laser Beam, J. Atmos. Oceanic Technol., 10, 518-525, 1993.

Knollenberg, R. G. The use of low power lasers in particle size spectrometry, Practical applications of low power lasers, Soc. Photo-opt. Instru., 92, 137–152, 1976.

Korolev, A. V., Makarov, Yu. E., and Novikov, V. S.: On the calibration of photoelectric cloud droplet spectrometer FSSP-100, TCAO, 158, 43–49, (in Russian), 1985.

Pinnick, R.G., D.M. Garvey and L.D. Duncan, Calibration of Knollenberg FSSP Light-Scattering Counters for Measurement of Cloud Droplets, J. Appl. Meteor., 20, 1049-1057, 1981.

"3150, 1-2: This is confusing. It kind of sounds like you're claiming that the red dots on Fig 2 represent smaller sizes for given pulse amplitude than the green or grey curve, but clearly you mean red dots vs blue curve. The point, I think, is that the blue curve is the green + grey curves shifted using some model. So the problem doesn't *have* to lie with the green/grey curve- it could be the model, right? And the model might be flawed since no Mie resonances show up it the red dots?"

Sorry for the confusion. Yes, the valid comparison is between the red dots and blue curve. We never meant to imply that the green + grey curves were problematic in some way; we believe that PSL and glass bead calibrations are valid (although we have now reanalyzed the PSL calibrations, which reduced the uncertainty in pulse amplitude). We agree with your assertion that the optical model may be inappropriate for this instrument, and we expect this is due to optical misalignment, as explained in our reply to your previous comment. We have rewritten this paragraph, in light of our new results.

AMTD

3, C1818–C1853, 2010

Interactive Comment

"3: Is the cal constrained only to the center of the DOF (depth being along the laser path) or is it also constrained to the middle of the beam, meaning passing from 3o'clock to 9 o'clock vs from 2 o'clock to 10 o'clock? I'm not 100% sure of the terminology, but I think of DOF as the former only."

The calibrations with glass beads and PSL particles are not constrained to the center of the DoF. We now state this explicitly in the paper.

"What I'm confused by is why the calibration was done like this in the first place. Since the drops pass through lower intensity parts of the beam, isn't it necessary to calibrate based on all trajectories that will be accepted by the CDP, and not just the ones with max amplitude? Why should this 2 um shift be needed? Couldn't one just traverse the cal drops through the beam until they're rejected by the instrument and then weight each stream by its likelihood?"

Yes, the instrument response at different locations within the sample area needs to be accounted for. The calibration method suggested by this reviewer has been implemented, with the results reported in Figure 4 of the paper (and Figure S3). We assume that all streams within SAQ are equally likely. At this point we would obtain additional calibration points for other droplet sizes (averaging the response over all positions within SAQ, as we did for 12 and 22um droplets), however the instrument has been optically modified since the date of publication. Because the average response for 12 and 22 um droplets was consistent with the response for various droplet sizes at the center of the DoF, as calibrated on two separate days (Figure S4), we believe that the reported results are sufficient for our analysis.

"8: In the first sentence where you introduce Fig 3 (above), you say "no averaging was performed and each data point represents a single droplet..." which is not consistent with the idea that Fig 3 represents Dv, which is a population measure."

Good point. No averaging was performed for the pulse amplitudes reported in Fig 2. However this statement does not apply for Fig 3 where the binned CDP observations are reported. We have corrected this in the paper.

"Again, the issue of why one expects Fig 3 standard measurement data to line up along the 1:1 curve is odd - you selected only a small subset of possible trajectories and thus there's no reason to think that the CDP standard measurement is valid here. It only works as an average if you send drops along all trajectories, right?"

We do have reason to suspect that these observations (with droplets of various sizes transiting across the center of the DoF) are a valid representation of the average CDP response. We have now added the average CDP response for 12 and 22 um over all positions within SAQ to Figure 2 of the paper (See Figure S4).

"23: It seems like the assumption here (and one that would clarify some of the above remarks) is that the ideal CDP has a transfer function that is a delta function. Obviously it's not. Although this would certainly completely change the way this paper is structured, I would suggest changing it so that you describe the transfer function instead. The valuable part about this is that one could then use an inversion routine (in the same way that DMA data is inverted, say) in order to account for the transfer function. One could fit the transfer function to different functions (lognormals? gamma functions?)and then describe the parameters of this with size. If you choose not to go this route, then I think you need to be upfront about your assumption of the transfer function, and noting that no one, obviously, expects it to look that way. In any case, the breadth of the transfer function (whether you ignore it or not) could be useful since it is another constraint on the size resolution of the instrument. Is there a regime where these sizing uncertainties dominate relative to Mie resonance uncertainties?"

We do not assume that the CDP transfer function is a delta function. In the Monte Carlo simulations, we use the measured response at all positions within SAQ (termed the "distortion matrix" by Wendisch et al [1996], and the "response matrix" by Baumgardner and Spowart [1990] and Nagel et al [2007]), which results in broadening of the simulated droplet size distributions. We state in the paper (Page 3154, Line 19) that "The measured response of the sizer and qualifier to individual droplets within SAQ and SAE during the laboratory calibrations constrains the simulated sizing and counting errors of the CDP. In the simulations, droplets are individually allowed to transit randomly across SAQ, and the pulse amplitude is then modified depending on the position of the droplet within SAQ." This is how the distortion matrix is applied in our simulations.

We do not explicitly report the distortion matrix in tabular form, as some authors have done (e.g. Wendisch et al, 1996), because we expect this matrix will be different for other instruments and will also change with changes in optical alignment. However, the distortion matrix for this instrument is revealed in two separate figures. Figure 4 of the paper shows this via a spatial map and Figure 10a shows its effect on the droplet size distribution. The procedure for inverting the measurements using the distortion matrix has been previously described by Baumgardner and Spowart [1990] and a more complex and precise inversion method has been described by Brenguier et al [1998] when individual pulse widths and droplet interarrival times are known.

In the inversion procedure it is important to know whether the distortion matrix is normalized by the maximum instrument response or the average response. This is where we have made an error. As stated in reply to previous comments, we incorrectly assumed that the water droplet calibrations at the center of the DoF (in Figure 2 of the paper) represent the maximum sizer response. This assumption was not necessary, since we had obtained observations to show that it was not true (Figure S3). We have now corrected this error.

AMTD

3, C1818–C1853, 2010

Interactive Comment

Baumgardner, D. and M. Spowart, Evaluation of the forward scattering spectrometer probe, Part III: Time response and laser imhomogeneity limitations, J. Atmos. Ocean. Tech., 7, 666–672, 1990.

Brenguier, J.L., T. Bourrianne, A. de Coelho, J. Isbert, R. Peytavi, D. Trevarin and P. Weschler, Improvements of droplet size distribution measurements with the fast-FSSP (Forward Scattering Spectrometer Probe), J. Atmos. Ocean. Tech., 15, 1077–1090, 1998.

Nagel, D., U. Maixner, W. Strapp, and M. Wasey, Advancements in techniques for calibration and characterization of in situ optical particle measuring probes, and applications to the FSSP-100 probe, J. Atmos. Ocean. Tech., 24, 745–760, doi:10.1175/JTECH2006.1, 2007.

Wendisch, M., A. Keil, and A.V. Korolev, FSSP characterization with monodisperse water droplets, J. Atmos. Ocean. Tech., 13, 1152–1165, 1996.

"3151, 6: Can't you be more quantitative? Can you take the Fig 4 data and, assuming randomly distributed drop trajectories, come up with a good estimate for the count rate uncertainty? That would certainly be helpful since it directly affects concentration! Even cooler would be to do it as a function of drop size..."

This is what was done, and that is how the average counting rate was determined. "Although the counting rate varies significantly at the edges of SAQ, the average counting rate within SAQ for both experiments is within 5% of the rate that droplets were generated." (Page 3151, Lines 4-6)

"3152, 21-22: Move this sentence ("The ice-only... probe.") to previous paragraph."

OK.

"26-27: Isn't the real test whether or not you see small particles at concentrations on the order of 10 to 100 times that of the precip-sized particles (as in Alexei Korolev's recent measurements)? So if you have 2 per liter of large particles, then you might expect shattering artifact concs on the order of 0.02 to 0.2 per cmËĘ3, right? And this is well within what was observed for concentration, right? So I'm not convinced this is a good case for testing shattering. Am I wrong here?"

I suppose this would depend on what you are interested in. Since many scientists use droplet concentrations > 1 cmˆ-3 or even > 10 cmˆ-3 as a criteria in the definition of a liquid or mixed-phase clouds [Hobbs and Rangno, 1998] , ice shattering artifacts resulting in concentrations of small particles on the order of 0.02 to 0.2 cmˆ-3 are insignificant. However, if you are trying to detect droplet concentrations on the order of 0.1/cc, then clearly you will have a problem. One may be interested in small ice crystals with concentrations of this magnitude, however the CDP is not designed for measuring ice crystals.

Hobb, P.V. and A.L. Rangno, Microstructures of Low and Middle-Level Clouds over the Beaufort Sea, Q. J. R. Meterol. Soc., 124, 2035-2071, 1998.

"3153, 20: Can you explain the implications for choosing an inter-arrival distribution that's uniform and limited to 2 * tao? In the absence of any small-scale clustering, one would expect the distribution to be Poisson with mean (and standard dev) of tao, which means there's a long tail to larger tao values than you use here. Would using a more realistic distribution change the results?"

Great question! We reran the simulations (with the same input size distribution and pulse widths), but instead of using a random distribution of interarrival times between 0 and 2*tau, we used a Poisson distribution of interarrival times [Field et al, 2003].

AMTD

3, C1818–C1853, 2010

Interactive Comment

Figure S7 shows a comparison between these interarrival time distributions. Although the probability distributions are clearly different, there was no significant change in the coincidence errors when using the Poisson distribution (Figure S8). We now briefly describe these additional simulations in the paper here.

Figure S7 Caption: Interarrival times randomly distributed between 0 and 2*tau (blue) and a Poisson distribution of interarrival times (red) for droplet concentrations of 10 cmˆ-3 (top) and 400 cmˆ-3 (bottom), respectively. Figures on the right show random sampling from each of these distributions and figures on the left show the probability distributions.

Figure S8 Caption: Simulated CDP-LWC bias assuming a random sampling of interarrival times between 0 and 2*tau (in black), compared to assuming a Poisson distribution of interarrival times (in red). The range of observations from all flights during ARCPAC is also shown (in grey), in addition to a linear fit to the observations in liquid-only clouds (solid black line).

Field, P.R., R. Wood, P.R.A Brown, P.H. Kaye, E. Hirst, R. Greenaway and J.A. Smith, Ice Particle Interarrival Times Measured with a Fast FSSP, J. Atmos. Oceanic Technol., 20, 249-261, 2003.

"24: this notation is inconsistent with Fig 6, where n_D is a concentration rather than a count rate. make it consistent!"

Thanks for pointing this out! We have fixed the notation in Figure 6.

"3154, 1: I think this paragraph deserves a much better topic sentence!"

We now add a few general statements to introduce this paragraph: “Coincident droplets can influence the scattering signal from qualified droplets even when the coincident and qualified droplets do not arrive at exactly the same time. This is because, in addition to the interarrival time between droplets, coincidence errors depend on the finite time that droplets spend transiting the laser beam, or the transit time. However, transit time is actually a proxy for the laser beam width, which is the parameter of real importance. This is important to understand, because it means that the aircraft velocity does not directly influence coincidence errors. ”

This last sentence was added because we have recently realized that this is not common knowledge in our field, and we believe it is important. Flying the same instrument on another, slower, aircraft will do nothing to solve the coincidence problem!

"28: I think Fig 8 needs to be explained better. I think "sizer sum" and "qualifier sum" are the total scattered light from all drops to each of these detectors. "Sizer signal" is, I'm assuming, the signal just from the drop of interest. These aren't labelled anywhere."

We now label these in the text.

"30: “inhomogeneous instrument response”: i won't preach any further on this, but i'm not terribly fond of using this terminology - inhomogeneous response is built in to the design of this instrument and there's no way around it unless the optics are changed dramatically. i guess i'd view a "perfect CDP" as one that works as well as can be expected within the hardware design parameters. to be less ambiguous, maybe you could refer to this as an instrument with a delta function as its transfer function? i didn't pick up what was meant by "inhomogeneous instrument response" on first reading."

We now refer here to Section 2.4.2, which describes what is meant by this terminology.

"3155, 8-9: “scatters additional light into the sizer” why isn't this additional light seen

AMTD

3, C1818–C1853, 2010

by the qualifier?"

Because the qualifier has an optical mask (see Figure S1).

"12-13 “droplets are undercounted”: This sounds funny to me (the only way to undercount a single event is to make it at zero - strictly it’s undercounting but it’s really missing it entirely!). Maybe "undercounting of droplets occurs"?"

that’s true... We replace this with: “...when this constraint is exceeded, qualified droplets are not counted.”

"25: I think you need to better explain Fig 9. Here are some questions I have about it that seem unexplained (the caption is exceptionally short and the text only helps a bit):
- Is this a monodisperse distriibution or is there some width to it? If former, why is D_v used in the text (since D_v suggests other types of diameters, but if monodisperse, they’re all the same)? If the latter, why isn’t the distribution width described?"

Droplet size distributions are used in the simulations (examples are shown in Figure 10 of the paper). We now add the standard deviation of the prescribed size distributions used in the simulations to the caption.

Even if a monodisperse size distribution were used, however, we would use D_v to describe these results since the measured response would still be spread over several bins.

"- What is this pulse width associated with? The sizer or qualifier? Why is it permitted to be a free parameter? Isn’t it constrained by the optics of the instrument? [OK, this is clarified in the next page by saying it’s treated as independent: so why is it allowed to be independent when it’s not? What’s gained in doing so, and how would a more realistic simulation be different from these idealized ones?]"

The pulse width is associated with both the qualifier and the sizer. We used it as a free parameter because it is "…not known precisely since it depends on multiple factors including the width of the laser beam at a given location, the droplet size, and the aircraft velocity" (page 3154, Line 10). During the water droplet calibrations we recorded pulse widths and droplet sizes. However, the width of the laser beam (in the x-direction = the direction of motion) is not known precisely. Therefore, the velocity of calibration droplets is required to interpret the pulse widths measured during calibration, and the aircraft velocity is then required to estimate the pulse widths during ambient sampling. We feel that the velocity of the droplets was not sufficiently constrained (especially for the calibrations where the droplet size was varied, since droplet velocity and our ability to measure velocity are drop-size dependent). Therefore, we used the measured "average transit time" to constrain "the pulse widths that can be used in the simulations" (page 3154, Line 13).

"- What does "sub-100m variability in n_D" refer to? What is this variability? (It doesn't help that I don't know which n_D you mean as you use it for two different parameters). [explained much much later - I suggest removing this as described in more detail later]"

As suggested, for clarity, we remove these simulations from the figures, and simply summarize these results in the text.

"- Apart from all the questions, what hurts my head the most is seeing (a) different kinds of simulations (homogeneous and inhomogeneous) and (b) simulations where both drop size and pulse width vary among the curves (oh, and having some of them fit to a line and others not). I don't really know how to interpret the figure - the text focuses on the change in drop sizes but pulse width changes and not in any obvious

AMTD

3, C1818–C1853, 2010

way (like smaller drops have smaller pulse widths)."

You've made some good points here. We now show only a few simulations, each with their own best fit line.

We picked a range of droplet sizes that show a range in errors due to coincidence, using a prescribed pulse width for each that results in an average transit time consistent with the ambient observations.

"This figure (9a and 9b) is also too small - I had to blow it up to 200% to make it comfortably legible. My eyes aren't great but I don't think they're that bad!"

We now split the figure into two separate figures.

"29: "The oversizing bias.." Are these supposed to be quantitative, i.e. representative of oversizing in the actual instrument, or qualitative, i.e. showing the approx. magnitude of the error and its variability with drop size? Again, I'm not sure what to take away from these simulations."

Since the pulse widths and interarrival times of individual droplets are not known, we would not say that these are quantitative results, but rather show the expected range of droplet sizing errors, given our uncertainty in the width of the CDP laser (in the direction of motion). The laser width is parameterized by varying the pulse widths, which are constrained by the average transit times observed. Thank you for helping us to clarify this point.

"3156, 6: "Figure 10" You don't mention at all that you have plotted the prescribed and simulated distributions on totally different y-axis scales. I assume this is because of the undercounting issue. Why aren't they plotted on exactly the same scale to show the magnitude of this issue as well? Or if you keep them on different scales, at least make it clear. In either case, it seems worthy of discussion, no?"

The purpose of these size distributions is to show changes in the shape of the size distribution. The concentration error already has its own figure (Figure 9b of the paper). We now clearly state the two scales used.

"And in constructing this, what is the assumption about pulse width? In Fig 9 it's an independent parameter. Presumably something was assumed here? Or not?"

The Figure 10 caption lists the pulse widths used.

"10: “variable... SAQ”: This is again, I believe, specifically referring to the fact that the drops see varying intensities in the sizing region, right? I find this sentence doesn't really say this - it just says the response varies but seems to lack a specific explanation. If I can offer a suggestion: can you come up with a specific term for this effect? Explain it carefully once and use it for the rest of the paper. For example, you could call it the "non-uniform illumination effect"."

We call this “2) Spatial heterogeneity in instrument response” from Line 23 on Page 3142 where we describe sources of the uncertainty for the CDP. In this section, we briefly outline the many different reasons why the instrument response is not spatially homogeneous, including inhomogeneous laser intensity and finite DoF of the optics.

"17: Fig 11: The shaded area is, I assume, the same shaded area from Fig 5? You don't make it clear."

Yes it is the same. We now say this explicitly. Now, however, the shaded area for both figures refers to the standard deviation of all the ARCPAC observations (as requested by another reviewer).

"Is this Figure really a fair comparison? The simulations choose very specific values of size and the (to me, confusing) pulse width. Are these consistent with the data used for Fig 5?"

The sizes were randomly chosen. We chose pulse widths that resulted in average transit times consistent with the average transit times observed (Figure 12 of the paper).

"3157, 2: Figure 12: I think if this were placed earlier (i.e. before Fig 9) it would have helped convince me that the simulations were representative of realistic conditions."

Since the pulse widths used in the simulations are constrained by the average transit time, we now re-order the figures so that this causality is accurately represented.

"11: “We ran additional simulations...” I don’t get why these simulations don’t match any of the homogeneous (L = 100 m) simulations. They seem random."

It is not clear to us what is meant by “. . .these simulations don’t match. . .” The results are different, as expected, since the inputs are different. The coincidence error is greater for these simulations (all else being equal). Another way of looking at it is the same coincidence error can be obtained with larger prescribed droplet sizes and/or smaller pulse widths.

"If the only point here is that smushing all the drops into a smaller time period instead of distributing them homogeneously over 1s causes more coincidence, I’d suggest to remove these points. They confused me terribly when I was looking at Fig 9, and the

AMTD

3, C1818–C1853, 2010

point is rather obvious."

Yes, this was our primary point. We have removed these points from the figures and simply summarize the results in the text.

"Figure 6: Define n_D."

This term is now defined in the text as droplets/s passing through the qualified sample area.

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

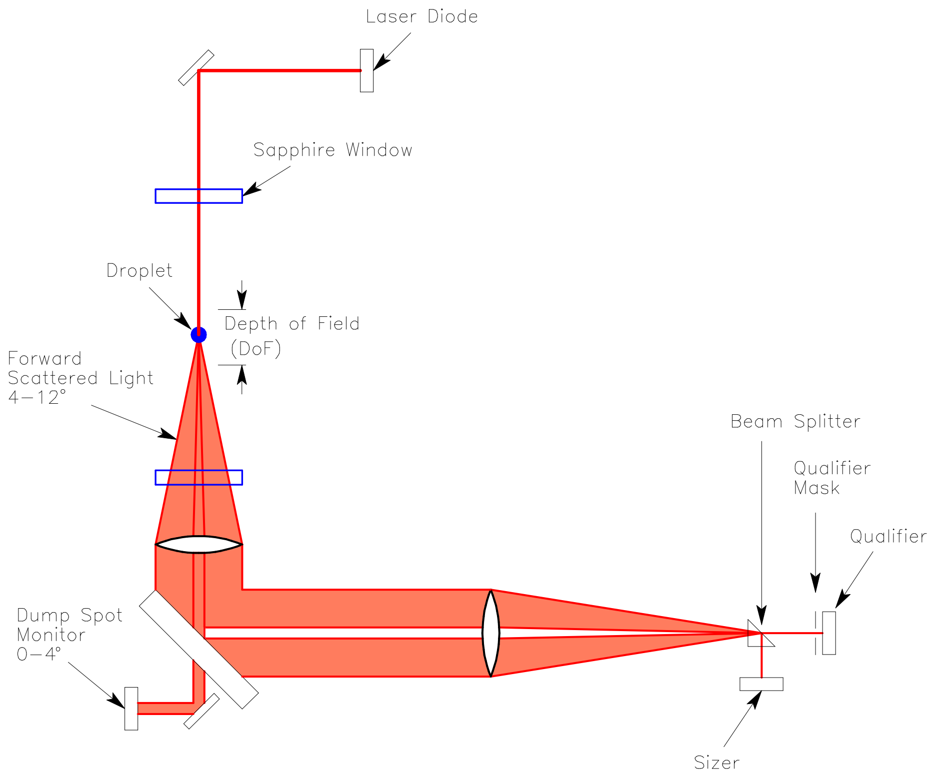

Fig. 1. (S1) Optical Schematic of the CDP

AMTD
3, C1818–C1853, 2010

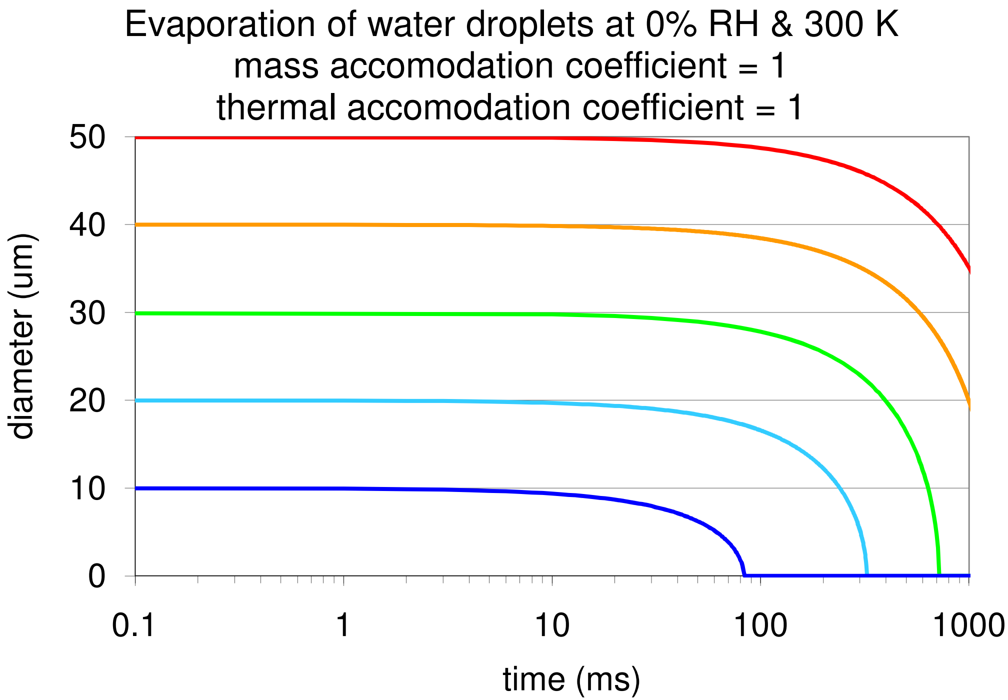

Fig. 2. (S2) Simulation of evaporating water droplets

AMTD
3, C1818–C1853, 2010

Interactive Comment

AMTD

3, C1818–C1853, 2010

Interactive Comment

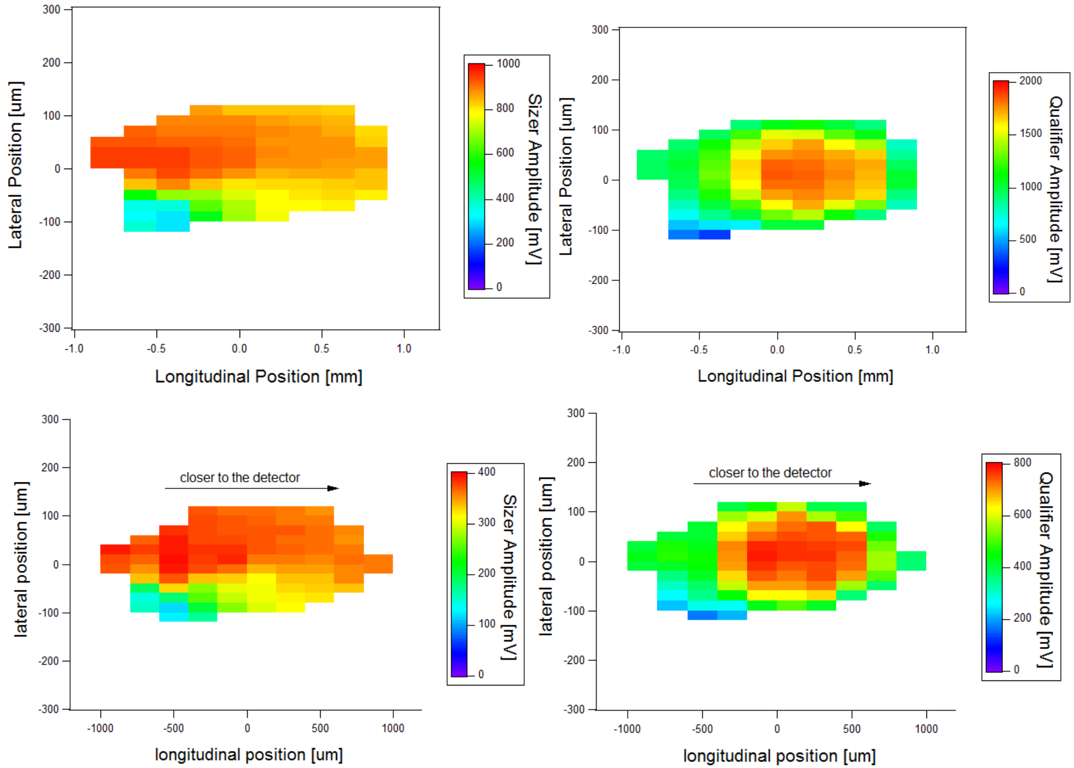

Fig. 3. (S3) Calibrated sizer and qualifier signals as a function of position within SAQ for 22 um (top) and 12 um (bottom) droplets

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AMTD

3, C1818–C1853, 2010

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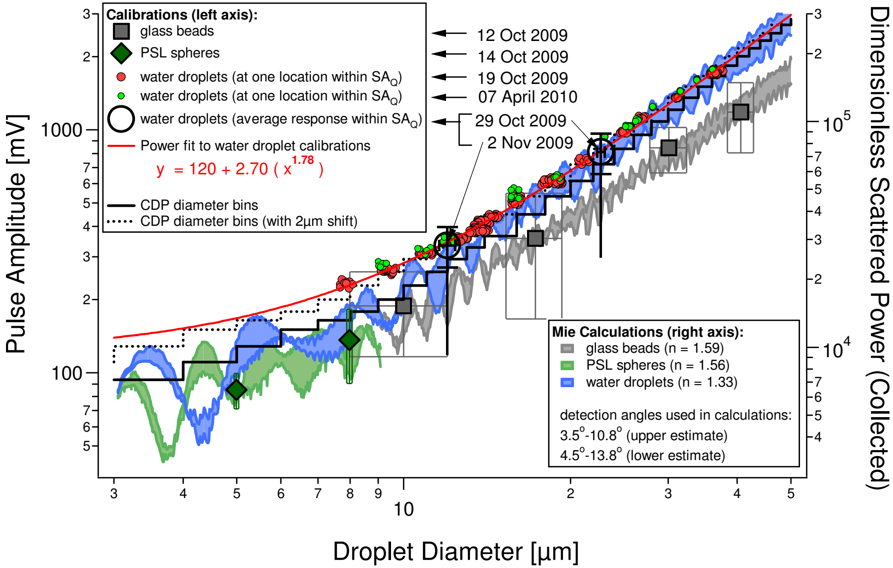

Fig. 4. (S4) Updated version of Figure 2 from the paper (see full caption in the text)

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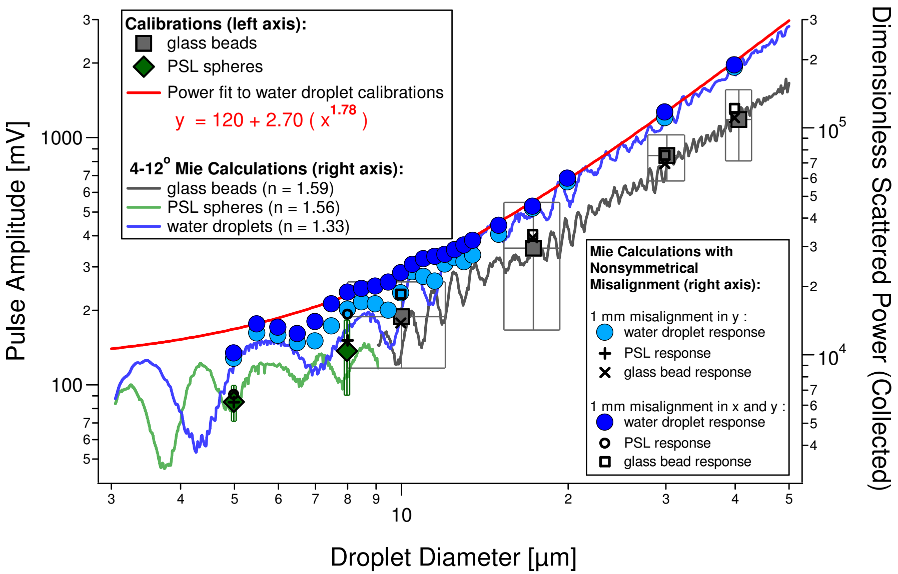

Fig. 5. (S5) Simulated CDP response function assuming various degrees of misalignment (see full caption in the text)

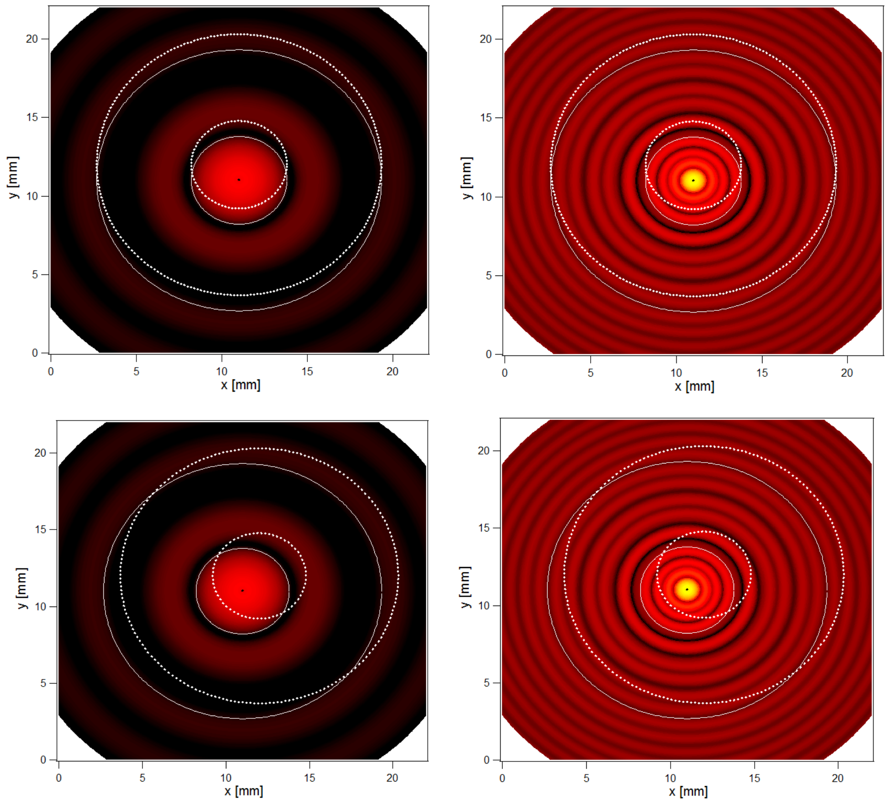

Fig. 6. (S6) Calculated scattered light intensities for 10 um (left) and 40 um (right) droplets (see full caption in the text)

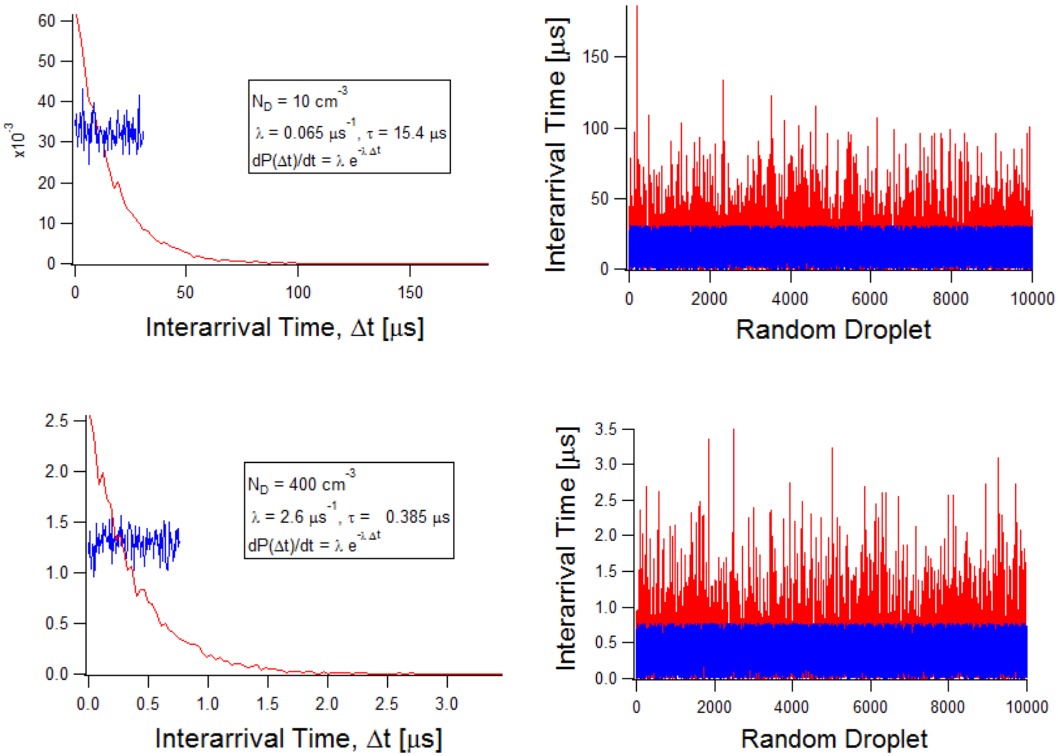

Fig. 7. (S7) Interarrival times randomly distribution between 0 and 2*tau (blue) versus a Poisson distribution (red). See full caption in text.

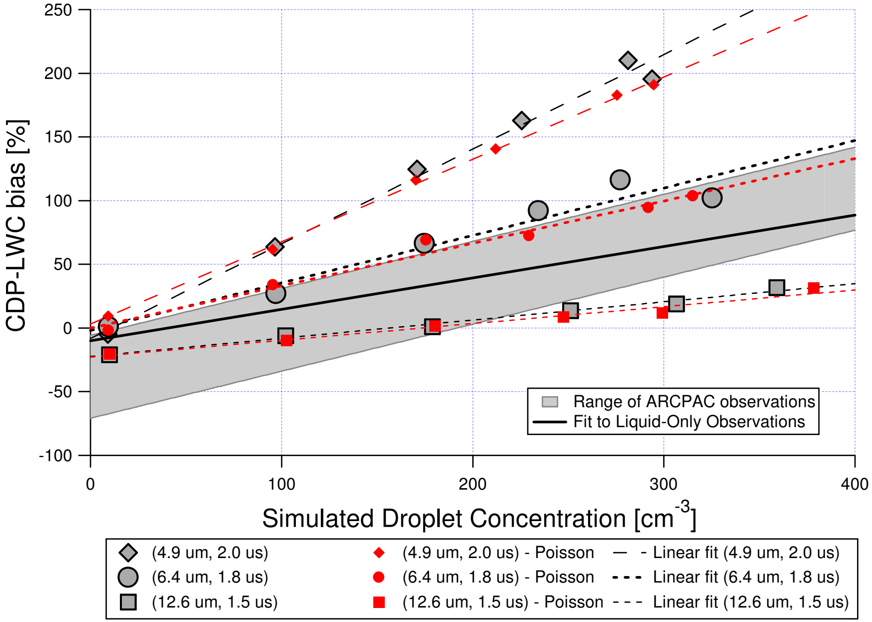

Fig. 8. (S8) Simulated CDP-LWC bias with different assumptions about interarrival time probability distributions (see full caption in text)