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Discussions

Interactive comment on "Cloud-microphysical sensors intercomparison at the Puy-de-Dôme Observatory, France" *by* G. Guyot et al.

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This study is a useful comparison of several instruments for making measurements of the microphysical properties of clouds, in particular the size distributions, bulk water content, particle surface area (PSA) and visibility.

The stated objectives in the abstract are to study the effects of wind direction and speed on ground based cloud observations, to quantify the cloud parameters discrepancies observed by the different instruments, and to develop methods to improve the quantification of the measurements; however, in the introduction the objectives are stated as 1) to investigate the variability amongst the measurements from the different instruments and 2) to investigate the sensitivity of FSSP measurements to orientation with

respect to the wind direction and speed. These are distinctly different and need to be rectified. The ones in the abstract more match what is actually done in the study.

I think that the title is too vague and does not describe very well the actual study. I suggest something like "Quantitative evaluation of eight optical sensors for cloud microphysical measurements at the Puy-de-Dôme Observatory, France".

The manuscript, in general, needs better organization in order to be useful to those that are interested in applying the results to their own measurements, i.e. in understanding how to interpret measurements with cloud probes within expected uncertainties, variabilities and limitations.

The description of the instruments and their uncertainties is hard to follow and misleading at times. The description of the results in some places is confusing, there are some figures that do not contribute much to the understanding of the comparisons, and there is a lack of solid, statistical analysis that can quantify the observed differences. I will address these issues section by section in my detailed comments below. These same comments are annotated in the PDF of the manuscript that I am also including as supplementary material. As part of that annotation I have also made an attempt to improve the wording in a few places.

Abstract

"However, the cloud properties derived from these different instrumentations have rarely been compared." This is an exaggeration. There have been many studies in wind tunnels, cloud chambers and aircraft that compared different instruments. Some of these were explicit studies that were published as instrumentation paper whereas there are many science oriented papers that contain comparisons among instruments in order to evaluate sensor performance. What the current study fails to do is explain how it is providing new information that is not already known.

Introduction

The intercomparisons that have been conducted previously are inadequately documented in the introduction as I mention above. The CDP intercomparisons by Lance are not discussed and those comparisons that are mentioned need to either have their main conclusions briefly summarized here, or compared with the present results in the discussion section. The Spiegel results are particularly important since they are clearly relevant to the issue of probe orientation with the wind. In addition these need to be brought up again later in the manuscript when discussing the observations with the FSSP versus wind velocity.

In the introduction there needs to be a clear explanation about how the current study adds to the body of knowledge that has already been published.

2.2 Cloud Instrumentation and Sampling Methodology

Missing: Any discussion of how the instruments were calibrated and quality assured before the measurements were made.

This section needs to be rewritten and reorganized. All of the single particle, light scattering instruments use the same technique for measuring and these techniques have the same uncertainties with respect to the Mie scattering theory. These need to be discussed together, and only instrument by instrument with respect to any particular differences that are only associated with that particular instrument. Coincidence needs to be discussed as a general problem then differences between the FSSP-100 (electronic delays and activity correction), SPP-100 (no electronic delays but activity correction still needed) and CDP (no electronic delays but activity correction still needed).

The VAR needs more explanation and how it and activity correction needs to be explicitly discussed, particularly with respect to when the activity correction begins to be important.

There are two FSSPs shown in the wind tunnel (one with sample tube rotated) and both an FM-100 and FM-120 shown on the platform. Why are two of these instruments

not used in the evaluation? Since they are shown in the figure they should either be used in the study or an explanation given why they are not.

The differences between the two CDPs should be discussed. They have different arm tips. Has either been modified with the additional pin hole on the sizer that reduces coincidence?

The measurement uncertainties are not adequately discussed and at times are erroneously reported. The maximum errors of 60% oversizing and 50% undercounting reported by Lance (2010) are worst case at very high concentrations while the 100% sampling losses are also under extreme conditions when the instrument is not properly oriented. These should not be reported in the table unless reported as extremes but with another column that shows the average errors.

The term “effective diameter” is misused when reporting not only uncertainties in sizing with the FSSP but also later on when reporting comparisons with the sizing by the different instruments. The PVM does report an actual “Effective Radius” or “Effective Diameter” that is proportional to the ratio of the LWC and PSA; however, nowhere in the manuscript is the calculation of the effective diameter from the FSSP, FM or CDP reported.

The calculation of the effective diameter should be reported as equation 4 when reporting the other calculated bulk parameters.

Equations 1-3 (and now 4) should be moved to the end of this section. In addition, these equations are wrong in both the dimensions and the parameters used in the calculations. N(D) is used correctly in equation (1) as the number of detected droplets in the size interval D and time interval delta T. It is used incorrectly in (2) and (3) if the sample volume is not included in those equations. The summation limits are not given and units are not given for any of the variables. The equations as written by Manfred (1998) are correct. Since the VAR and activity are both corrections to the sample volume, they should be shown explicitly here. For the CDP and FM the VAR is

1.

The TAS needs further explanation for the rooftop instruments, especially for the FM that has a pitot to measure the airspeed (was this used in the calculations?) and the aspirated FSSP that is given as both 9 and 15 ms-1. Which one is it?

The FM has an inlet horn with the prerequisite 7° angle to minimize flow separation and turbulence whereas it appears that the FSSP is aspirated with no flow straightener in front of it. This can lead to excessive turbulence as well as distortion of the size distributions as the effective sample volume is greater than the optical volume when the flow is anisokinetics, i.e. corrections have to be made for the fact that the inlet velocity is higher than the wind velocity (particle velocity) and hence he concentrations are overestimated. This is clearly why the FSSP is usually much higher than the other instruments. The wind speeds are between 2-6 ms-1 but the pump speed, depending on which part of the manuscript you are reading, is either 9 or 15 ms-1. In either case it means that the flow is anisokinetic and the FSSP is oversampling. This is a major issue that has to be discussed.

Was the effective diameter from the PVM the direct output from the PVM or was it calculated separately from the LWC and PSA? Gerber reports accuracy of 10% but Manfred shows that when the MVD exceeds 25 μm, these errosr are much larger. This needs to be reported as extreme value in the table.

Where does the definition of equation (4) come from. The Koschsneider equation uses 3200 not 3000.

What does the velocity and droplet size distribution across the tunnel look like? Are corrections needed for instrument location? Are there no airflow interferences between the probes? How was the profile checked? Was an attempt ever made to switch instrument positions?

Although Lance (2010) states that the multimode smooths out the Mie ambiguities, I

don't think that this is the primary reason that Knollenberg selected a mulitimode. I don't think Kollenberg in his 1976 paper says that this is the reason for the multimode. The multimode was designed to provide a more uniform intensity across the laser beam cross section, not to smooth the Mie resonances. The calculations used to derive scattering cross sections for particles do not take the multimode into account. Lance also states, immediately after the comments on single versus multimode that "However, the single-mode CDP diode laser (658 nm) avoids the greater spatial intensity and/or phase inhomogeneity of a multi-mode laser, which can result in a greater broadening of the measured droplet size distribution (Baumgardner et al.,1990) in addition to a shift in the measured mean size (Hovenac and Lock, 1993).

"During the campaign, measurements were performed with 1 Hz acquisition frequency Instruments". The instruments do not set the acquisition frequency, the data system does.

Results

Confusion: Why is the FSSP on the roof top being used as what seems to be a reference when it clearly will have the largest measurement errors?

I don't understand why PVM-2 is used at all in Fig. 2 since it was identified as having problems. Why not PVM-1. This is never explained.

The correlation plots with the best fit coefficients are good as a first step in comparing the instruments but not sufficient to help explain any differences or to evaluate if the differences are within the expected uncertainties. At a minimum, on each of the scatter plots there needs to be dashed lines that show the envelope of uncertainty for the two instruments being compared. Although not statistically robust, this at least shows if the differences are more than expected.

Another recommendation is that al the best fits except effective diameter should be forced through 0 as there is no reason to expect offsets in the LWC, Concentration or

Extinction.

The least squares slopes need to be statistically tested to see if they are significantly different than 1:1.

Discussing Fig. 2, the FM-100 is noted as being lower than all the other instruments but actually it is not that much different than CDP-1; however, from the PSD comparison, given that the FM is the right shape but much lower in overall concentration, I question the TAS that is being used to compute the sample volume. The PSDs in Figure 3 should be only when the FSSP and FM-100 are co-axial with the wind and this figure should be moved to section 3.2. In addition, there should be a Figure 3a, 3b and 3c: Number, surface area and LWC as a function of size, with the scales linear on all three graphs. The reason is that number concentration tends to be dominated by small particles, surface area by medium sized and LWC by larger so in order to understand discrepancies in these three bulk quantities, the PSDs will show if it is due to size sorting or not. Log scales hide the differences while linear scales will highlight them. For example in Fig. 3, it is clear that the reason the FSSP is so much higher in concentration (Fig. 2) is because most of the particles are in the size range below 6 μm. This is not obvious on a log scale but on a linear scale we see that below 6 μm the FSSP concentration is 2-3 times greater than the other instruments. This in turn is because of the problem that I referred to earlier of anisokinetic sampling.

Figure 4 while a colorful graphic is not very illustrative as it would be difficult to see any changes with airspeed even if there were changes. Why would changes be expected with tunnel speed? The better graphic would be to compare the LWC and extinction from the tunnel probes with the LWC and extinction from the platform PVM and PWD as a function of tunnel speed. This would show if changing the tunnel speed affected the sampling of the cloud.

The discussion about the results shown in Figure 5 is very difficult to follow unless one is intimately familiar with the definition of effective diameter being the scaled ratio of

LWC to extinction. This brings me back to my original request to include the equation of the effective diameter and secondly, to compare the effective diameter directly output by the PVM to the effective diameter derived by calculating the ratio of LWC and PSA using an appropriate scaling factor, the same used when calculating the effective diameter from the FSSP and FM-100. I understand what the authors are saying here, i.e. that since the relationship between the FSSP and FM-100 LWC and Extinction and those from the PVM are the same slopes, then the difference in effective diameter is not because of different slope. This, however, is not well presented for the less experience observationalist to understand.

Figure 6 should have the best fit through 0. The large difference between FSSP and FM-100 is puzzling and suggests that the FM is somehow under-sampling or that the flow velocity is much less than is being used to calculate the TAS in the sample volume.

I don't understand to what the authors are referring when they state "Pearson Principal Component Analysis" related to Fig. 7 and the statistical significance of the correlation coefficient. There certainly is an analysis technique that Pearson and Hotteling developed but it does not relate to testing the significance of the Pearson Correlation Coefficient.

Figure 8 panels are much too small and need enlarged.

"This bias may be attributed to the assessment of the probe sampling speed/volume. In particular, it is known that the Depth Of Field (DOF) of an instrument can be significantly different from the value given by the manufacturer. This uncertainty may exceed a factor 2" This brings me back to my original question about how the instruments were calibrated prior to the experiment. Were the DOFs measured? How uniform is the velocity and droplet concentration across the tunnel? Is there any reason why these differences aren't just due to velocity and droplet concentration inhomogeneities?

Figure 10 – Why aren't comparisons shown with CDP-2?

Figure 11 (need an a and b) – The large overestimation of extinction by the FSSP at low wind speeds is due to the anisokinetic sampling that I referred to earlier and has to be discussed in this manuscript because it is the main factor causing the differences with this probe.

Section 3.4

It is my recommendation that this section should be completely removed as all it really demonstrates is that measurements with aspirated probes in ground based applications should always be made isoaxial to the wind. The detailed study by Spiegel (2012) is a much more thorough investigation of this issue and I don't see that the measurements here add to what was done by Spiegel.

I think that one of the biggest factors is the lack of an inlet for the FSSP, similar to what is used by the FM-100, to bring particles in isokinetically. The biggest correction that can be made to the FSSP measurements, without taking into account the size sorting, would be to correct for the difference between the aspiration velocity and wind velocity in the calculated sample volume.

Conclusions

This section will need modifying once the paper has been edited to take into account the issues that I have raised.

Please also note the supplement to this comment:
http://www.atmos-meas-tech-discuss.net/8/C1771/2015/amtd-8-C1771-2015-supplement.pdf

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