We thank D. Baumgardner for his detailed review and valuable comments. The manuscript has been modified according to the suggestions proposed by the reviewer. The remainder is devoted to the specific response item-by-item on the reviewer's comments.

RC=Reviewer Comments
AR=Author response
TC=Text Changes

RC: 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.

AR: We agree with the reviewer; the objectives in the introduction have been revised according to the reviewer's comment.
Line 184: The first objective of this work is to quantify the discrepancies between the some of the cloud microphysical probes available for the scientific community to this date. The peculiarity of this intercomparison consists in the fact that the set of instruments was operating in two different conditions simultaneously. In other words, we compare data recorded in ambient conditions and in the wind tunnel. The latter simulates to some extent airborne measurements. The second objective is to propose an approach to correct some potential biases between those instruments. And, the third objective is to assess the effect of wind speed and direction on ground-based FSSP and Fog Monitor probes.

RC: 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.

RC: 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.

AR: Abstract and Introduction were revised according to the reviewer's comment.
Line 30 : Several intercomparison studies have been carried out in the past to assess the reliability of cloud microphysical properties inferred from various measurement techniques. However, there still is considerable need for reducing the observational uncertainties and providing better measurements for droplet size distribution for instance.

RC: 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.

AR: We added references on published studies that compared different instruments. Main conclusions of the works by Lance et al. (2010) and Spiegel et al. (2012) are briefly summarized.
Line 131: Lance et al. (2010) used glass beads to study calibration accuracy of the Cloud Droplet Probe. They found that the calibration was consistent with the theoretical instrument-response provided by the manufacturer. On the other hand, their laboratory experiments with water droplets originated from a piezo-electric drop generator showed a 2 µm shift in the size assessment for the diameters between 12 and 23 µm. The shift was attributed to misalignment of the optical system. In flight comparisons with measurements of liquid water content (LWC) suggest a bias in the droplet size and/or droplet concentration. This bias was found to be strongly concentration dependent and was a result of coincidences, when two or more droplets pass through the CDP laser beam within a very short period of time of each other (Lance et al., 2010).
Line 147: In addition, the sampling efficiency formula by Hangal and Willeke (1990 a,b) were applied with the Fog Monitor characteristics, the results showed that the efficiency decreased quickly for droplets larger than around 10 µm and angles larger than 30° in sub-kinetic regime, and the efficiency is nearly independent of the sampling angle in super-kinetic regime (Spiegel et al., 2012).

And, we underscored the following. The peculiarity of this intercomparison consists in the fact that the set of instruments was operating in two different conditions simultaneously. In other words, we compare data recorded in ambient conditions and in the wind tunnel. The latter simulates to some extent airborne measurements.

We pointed out at the end of Section 3.4 that the experimental data presented in Table 5 corroborate with the modeling results by Spiegel et al. (2012).

RC: 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.

AR: Section 2.2 was reorganized and almost totally rewritten according to the comments of both reviewers.
The instruments that detect and size individual particles are discussed together. Several major sources of uncertainties are explained and discussed; a special attention is paid to the Velocity Acceptance Ratio (VAR). And, we provided information on the date of the calibration for each instrument.
Line 308: Changing Velocity Acceptance Ratio (VAR) (Wendisch, 1998): it stems from the fact that only a part of the laser beam diameter is used to calculate the sampling volume because drops passing the laser beam near its edges are undersized. Theoretically, by electronic procedure consisting in a threshold in the transit time, only 62% of the laser beam diameter is used to accept a particle. This value has to be taken into account in the sampling-surface calculation and it can change with time. Thus, the VAR has to be measured and the actual value has to be used in the data processing.

RC: 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.

AR: The FSSP positioned at the rear of the wind tunnel did not function during this campaign. So only, the SPP-100 with the sample tube rotated was operating. The FM 120 (with the swivel) didn't work at all (NaN or 0 in the data), they have been completely removed from the text of the manuscript.

RC: 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?

AR: There is a CDP version 1 and one version 2, no pin hole were installed.
Line 390 : Two types of CDP were used during the campaign: the first version (CDP1) with original tips and the second version (CDP2) with Korolev tips against possible shattering effects. To match with the version, the CDPs were renamed; the ex-CDP1 is now CDP2. For both versions no pin hole used to reduce coincidence effects was added on the sizer of the CDP.

RC: 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.

AR: The Table 2 has been improved with two additional columns for the uncertainties, one for “normal” conditions and one for extreme conditions, the corresponding references were also added.

RC: 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.

AR: The equations to compute the effective diameter has been added (equations 3 and 5).
Line 266:

$$D_{eff}\ [\mu m] = \frac{\sum_D n(D) D^3}{\sum_D n(D) D^2} \qquad (3)$$

Line 411:

The first filter converts scattered light to a signal proportional to the particle volume density (or *LWC*) of droplets; the second filter produces a signal proportional to the particle surface area density (*PSA*) (Gerber et al., 1994). From the ratio of these two quantities, r_{eff} can be derived:

$$r_{eff} = \frac{LWC}{3\ \sigma} \qquad (5)$$

These two filters guarantee a linear relationship between scattering intensity and *LWC* or *PSA* for droplets diameter from 3 to 45 µm for the PVM-100 (Gerber, 1991). The extinction coefficient σ is directly proportional to the *PSA*:

$$\sigma\ [km^{-1}] = 0.05 * PSA\ [cm^2.m^{-3}] \qquad (6)$$

RC: 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.

AR: The equations have been revised, the units have been added. At the same time, we prefer to put the equations where they are because we will need some definitions just below for the description of the instruments and uncertainties.

Line 261: The total concentration *N*, *LWC*, effective diameter D_{eff} and extinction coefficient σ are computed using the following equations (Cerni, 1983):

$$N\ [cm^{-3}] = \sum_D \frac{n(D)}{V_s} = \sum_D \frac{n(D)}{S*TAS*\Delta t} \quad (1)$$

$$LWC\ [g.m^{-3}] = \frac{\pi}{6} * \rho_w * \sum_D \frac{n(D)}{V_s} D^3 \quad (2)$$

$$D_{eff}\ [\mu m] = \frac{\sum_D n(D) D^3}{\sum_D n(D) D^2} \quad (3)$$

$$\sigma\ [km^{-1}] = Q_{ext} * \frac{\pi}{4} * \sum_D \frac{n(D)}{V_s} D^2 \quad (4)$$

where *n(D)* is the number concentration measured for the size bin of diameter *D*, ρ_w is the density of the water. V_s is the sampling volume defined as the product of the speed of the air in the inlet *TAS* (True Air Speed), Δ*t* the sampling duration and *S* the sampling surface. S is computed as the Depth of Field (*DOF*) multiplied by the width of the laser beam. The extinction efficiency Q_{ext} is considered to be equal to 2 within the droplet size and laser wavelength range.

RC: 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?

AR: We tried to clarify these issues. A speed of 9m/s is used for the calculations. The text below has been added in Section 2.2:

Line 343: During the intercomparison, a commercial pump was employed to aspirate a constant air flow through the FSSP-100. The flow through the pump was monitored with a hot wire; the measured air speed was around 15 m s^{-1}. Theoretically, this flow leads to airs peed through the FSSP-100 inlet of 9 m s^{-1}; that value was employed to compute corresponding data.

Line 378 : The FM-100 has a Pitot tube to measure the air speed used in the sampling volume computation. However, as it didn't work during the campaign, the sampling speed was set to the constant theoretical value of 15 m s^{-1}. This assumption adds uncertainties to the FM sampling volume.

RC: The FM has an inlet horn with the prerequisite 7º 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 the 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.

AR: This issue is now discussed in the section Results and in the appendix.

Line 737: As the FSSP is aspirated with no flow straightener in front of it, turbulent flow and distortion of the size distributions can be expected. Anisokinetic sampling and errors in the sampling volume can explained the concentration overestimation.

Line 845: This confirms the hypothesis of the FSSP anisokinetic sampling with potential turbulent fluxes, leading to a bad correlation with the PWD. A correction which would be proportional to the concentration is thus not possible for the FSSP.

RC: 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 errors are much larger. This needs to be reported as extreme value in the table.

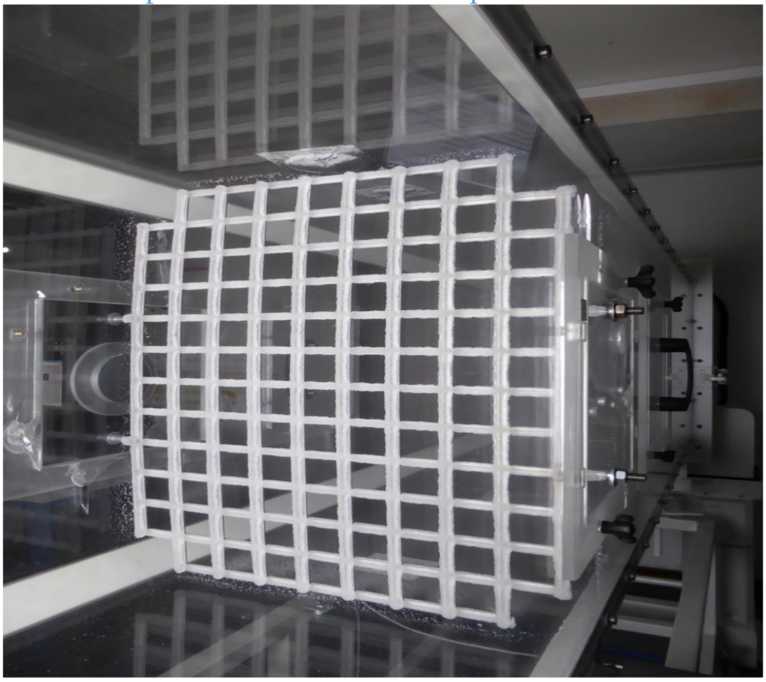
AR: The used effective diameter comes from the direct output, however, it has been checked that the computation with the LWC and PSA give the same values. Extreme uncertainties have been reported in Table 2.

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

AR: The coefficient of 3000 comes from the documentation of the PWD22 (see p.25, Vaisala, 2004). The corresponding reference has been added to the manuscript.

RC: 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?

AR: This text below has been added to Section 2.2.
Line 456 : The method of an icing grid (see, e.g., Irvine et al. 2001) was used for airflow uniformity measurement. The tests were performed at the maximal airspeed available in the wind tunnel. According to the preliminary results, the variations of the thicknesses and widths of the iced bands were lower than 5 %, i.e., of the order of the uncertainty of the method. Thus, we can reject the hypothesis of the airflow heterogeneity as the cause of the differences between data.
The tests of the airflow uniformity were conducted at winter time (see picture below, P. Personne and C. Verhaege personal communication). There was no possibility to perfume the same kind of tests in May 2013 because of the weather conditions. The instruments were placed to avoid airflow interferences as far as possible. The instrument positions were not switched.



RC: 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).*

AR: We agree with reviewer's opinion. The corresponding statement has been deleted.

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

AR: We agree, the corresponding sentence has been revised.

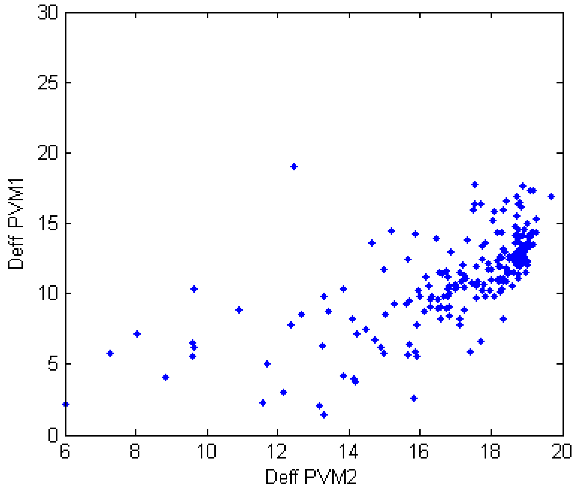
RC: Results

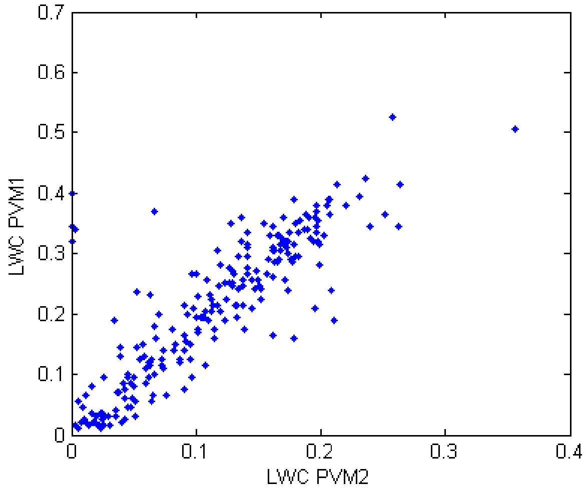
Confusion: Why is the FSSP on the roof top being used as what seems to be a reference when it clearly will have the largest measurement errors?

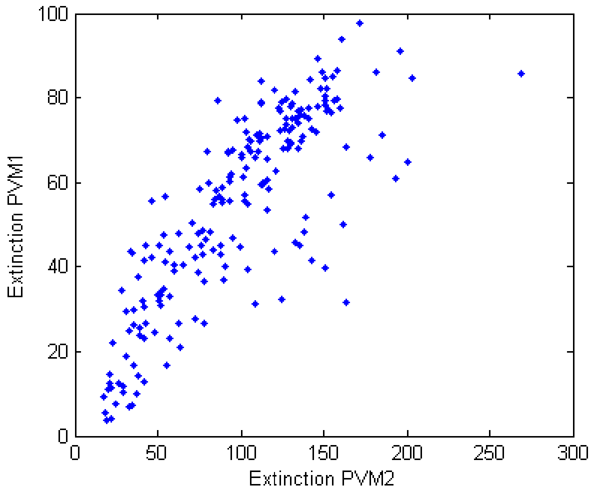
AR: The PWD is the reference and not the FSSP; however FSSP deserves particular attention because it was the instrument which has the most discrepancies in our experiments. The section Results has been revised to avoid this misunderstanding.

RC: 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.

AR: The PVM2 was shown as it provides an example of instrumental disfonctionning which leads to its removal of the comparison. But, as it doesn't show new results, we choose not to talk about this instrument to avoid misunderstanding; the PVM2 has been removed from the study. Here is the comparison with the PVM1, the dispersion is very high and lead to bad retrieval of the effective diameter for the PVM 2:







RC: 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.

AR: We agree with the reviewer's comments. Dashed lines showing the envelope of uncertainty have been added to the concentration and extinction figures. A confidence interval has also been calculated

RC: Another recommendation is that all 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.

AR: We agree, the best fits (LWC, Concentration and Extinction) were recomputed so that they pass through 0; the corresponding figures were revised.

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

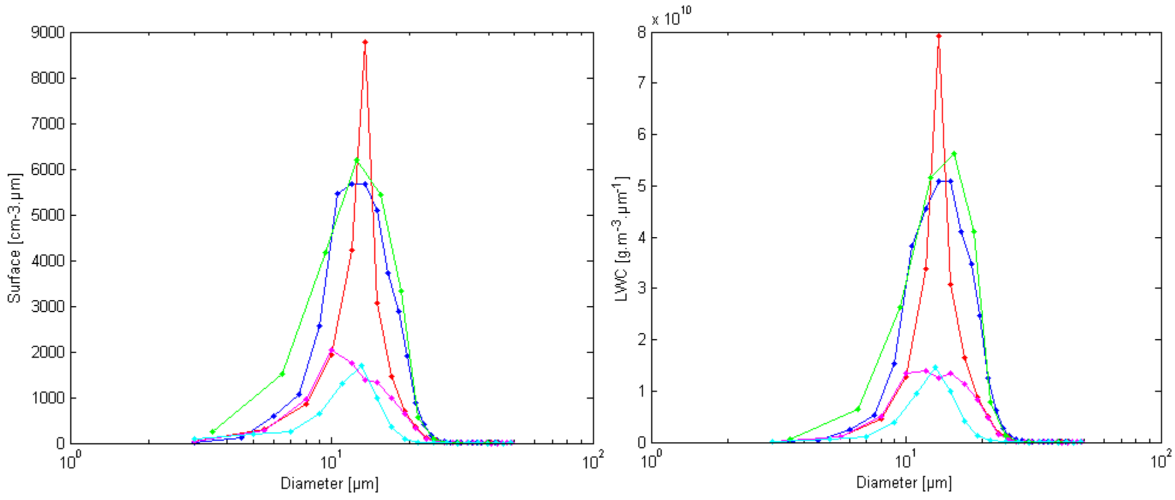
AR: statistical tests of a confidence interval with a confidence level of 99% have been added to the scatter plots.

RC: 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.

AR: The corresponding paragraph has been revised with additional explanations. We agree that the TAS belongs to the major sources of uncertainties. A misestimation of the TAS can lead to the observed effect. At the same time, we prefer the approach to standardize the measurements with instruments which are based on the measurements of an ensemble of particles.

RC: 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.

AR: Figure 3 was revised: The surface area and volume distributions are given along with the Number distribution. It is underscored that the data are selected for the co-axial with the wind conditions, a sentence was added to precise this.
We prefer Figure 3 in the manuscript given in the log-log scales for the surface and volume. Its version in the linear- linear scales is given below.



RC: 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.

AR: We agree. The asked corrections of the ex-figure 4 correspond to the figure 9 (for the entire campaign), so we deleted this paragraph and put the conclusions in the analysis of the figure 9

RC: 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.*

AR: the definitions of the effective diameter have been added for the different types of instrumentation.

RC: 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.

AR: The Figure was revisited. Indeed, as explained in the section 2.2, the FM TAS was set to a constant theoretical value; errors on absolute values can be high.

RC: 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.

AR: This has been deleted and replaced by a statistical analysis of confidence interval.

RC: Figure 8 panels are much too small and need enlarged.

AR: The Figure was revisited

RC: "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?

AR: The date of the calibration has been added in the section. Indeed, the DOF is not the only uncertainty that affects the number concentration. All errors in the sampling-volume estimation can lead to strong errors. The text was changed:
Line 733: To summarize this section, the comparisons showed good correlations between the deduced parameters, that is, good sizing for all the instruments. At the same time, the instruments displayed large discrepancies in their capability to assess the cloud droplet number concentrations. Except for the FSSP, which is shown to have anisokinetic sampling, the biases were constant during the campaign. They are attributed to the assessment of the sampling volume. That assessment includes errors in the sampling speed, the laser width, the DOF. The listed uncertainties are very difficult to quantify and they can reach rather high values. Thus, it seems to be more productive an approach to correct measured data without computation of all the errors related to the sampling volume. The approach is discussed in the following section.

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

AR: The results of the CDP1 (ex-CDP2) have been added, however, the data of the 22 May are not available for this instrument.

RC: 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.

AR: This is indeed the main issue for the FSSP and has thus been added in the text:
Line 716: On the contrary, anisokinetic sampling of the FSSP leads to higher discrepancies when this instrument is compared to other ones.
Line 807 : As a consequence, the FSSP seems to be very sensitive to the wind conditions, and this confirms the hypothesis that anisokinetic sampling affects the FSSP measurements whereas FM-100 inlet system avoid as much as possible this effect. Indeed, the FM-100 has a transport tube, which allows an aperture angle more important and ensure a more laminar flow than in the FSSP.
The discussion of this FSSP anisokinetic sampling is the subject of the appendix.

RC: 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.

AR: On the one hand, we pointed out at the end of Section 3.4 that our data corroborate with the modeling results by Spiegel et al. (2012). On the other, we believe that Tables 4-5 and Figure 12 (revised manuscript) provides new experimental data that are of importance. In particular, it is seen that the concentration loss is a highly nonlinear function of the particle size and the wind speed. Thus, there exists no approach that would be able to correct misalignment with the wind direction.
That is why we prefer to keep Section 3.4 in the manuscript.

RC: 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.

AR: The absence of a horn leads actually to bigger uncertainties.
The appendix shows that the sampling speed depends on the particle size and the wind velocity. Moreover, high dispersion is found in the SPP transit time (so the sampling speed) due to turbulent flow inside the inlet. That makes impossible a correction like for the other instruments.
Line 1073: The results of the Figure 13.a show that droplets smaller than 20 µm have the range of the SPP transit time between 6 and 14 µs, whereas the range is between 7 and 10 µs for droplets larger than 20 µm. Small particles tend to be driven by streamlines. This highlights anisotropy in the sampling suction, which is potentially turbulent. Indeed, in the non-isokinetic conditions and for high Reynolds number (about $2 \cdot 10^4$), turbulent flows are expected inside and near the FSSP inlet.
Line 843: In addition, average transit speed was found to be dependent of the droplets diameter (see Figure 13.a), with a larger dispersion for small particles (≤ 18 µm). This confirms the existence of a FSSP anisokinetic sampling with potential turbulent fluxes, which leads to a bad

correlation with the PWD. A correction that is proportional to the concentration is thus not possible for the FSSP.

RC: Conclusions

This section will need modifying once the paper has been edited to take into account the issues that I have raised.
AR: The conclusion has been revised according to the new issues.