

Interactive comment on “Effects of ice crystals on the FSSP measurements in mixed phase clouds”

by G. Febvre et al.

First off all, we are very grateful to the two referees for their comments and their pertinent recommendations. Many thanks for your time and for your contribution to the present version of our paper.

In order to try to answer each remark specifically, we list below the complete recommendations using a color code (randomly chosen), D Baumgardner (Referee) in black, second anonymous Referee in green and our answers in blue.

This manuscript would probably be better suited as a submission for Atmospheric Measurement Techniques, rather than ACP, and perhaps once it has been revised, the editors can choose to move it to AMT. As I was a reviewer who did the original technical assessment, it should have been my responsibility to make this recommendation at that time.

We have no objection.

The study concerns the interpretation of measurements made with single particle light scattering spectrometers (OPC), equipped with inlets, when operated in clouds that contain ice crystals. This evaluation contributes to the ongoing evaluation that has seen a number of contributions that focus strictly on the potential for ice crystal fragments to be sampled as natural ice crystals or water droplets. The current study adds a very valuable component to this ongoing assessment of the level of uncertainty that is contributed by having ice crystals in the environment, i.e. in the absence of shattering artifacts, how does an OPC respond to an ice crystal when it has been designed to interpret the intensity of scattered light with respect to spherical water droplets? As far as I know, only a single paper has addressed this issue, i.e. Borrmann et al. (2000) and much more is needed in order to use OPCs more effectively in clouds with ice.

There are certainly cases where the spurious particles produced by ice shattering will swamp the signal such that no amount of data processing can recover useful information. There are, however, probably many other instances where ice is present but with very few crystals of sufficient size to cause problems with shattering. In these cases, much more information can be extracted once the response of the OPC is better characterized.

The current paper make a good start in the direction of characterizing the optical response of FSSPs to ice crystals, but falls short in a number of ways that I feel are too important to neglect and should be addressed before this paper moved to either the ACP or AMT stage.

First of all, there has yet to be a balanced paper written on the potential response of OPCs in clouds with ice. Given that this paper is entitled “Effects of ice crystals on the FSSP measurements in mixed phase clouds”, it is important that it presents as many of the effects as possible related to how the FSSP responds to ice crystals. There are several different effects that need to be addressed in the current paper, in addition to improving upon the analysis of the effect that is discussed, i.e. missizing larger ice crystals as smaller water droplets.

The topic of our paper is not to carry out a review of all possible effects of ice crystals on OCP but it is to demonstrate for the first time the idea that the presence of ice crystals induces a bimodal FSSP-PSD.

In order to take into account the referee’s remark, we propose to change the title of our paper to a less general one : i.e. : Some effects of ice crystals on the FSSP measurements in mixed phase clouds.

So, aside from ice shattering, what are the effects that need to be addressed?

1) In mixed phase clouds, as a result of the Wegener–Bergeron–Findeisen (WBF) process, ice crystals will grow faster than water droplets, depending on the available water vapor and the relative humidity with respect to ice. The growth of the size distribution into a bimodal shape can be a result of frozen water droplets or small ice crystals growing more rapidly than the droplets. Can this be ruled out in the data set that is shown? The

comparison with the PN of phase function derived from the FSSP, assuming some fraction of water droplets and ice crystals with mode between 25-35 μm would help confirm or throw out this possibility

In order to evaluate the Wegener–Bergeron–Findeisen (WBF) process, a very good knowledge of the fraction of ice/water in a cloud is needed. Such an evaluation is not reliable with our set of data.

Before proposing this paper to ACP, we have explored the correlation between the FSSP PSD and the phase function in mixed cloud as shown by Kokhanovsky in his 2007 paper.

Indeed, our Fig. 4 shows simultaneously the ice crystal effect on the phase function (PN) and on FSSP PSD. This effect is stronger when the ratio between the two fractions, namely ice and water, is larger.

In the case shown on Fig. 4d, the measured Polar Nephelometer Phase function could be associated with a crystal population without water droplets.

In the mixed cloud cases (Fig. 4 b, Fig. 4c), the measured phase functions (ϕ) are the sum of the phase function of the ice population and the phase function of water droplets.

$$\phi_{\text{mixed-measured}} = \phi_{\text{liquid}} + \phi_{\text{ice}}$$

ϕ_{liquid} is calculated using the Mie theory with the FSSP PSD, especially for diameters smaller than 20/25 micron.

So :

$$\phi_{\text{ice}} = \phi_{\text{mixed-measured}} - \phi_{\text{liquid}}$$

The correlation between ϕ_{ice} and the second mode of the FSSP PSD has been established (consequence and the purpose of our paper). This phase function (ϕ_{ice}) is very close to those measured by the polar nephelometer in ice conditions (4d). However, the problem consists of explaining the relationship between ϕ_{ice} and second mode of the FSSP PSD.

Indeed, with our set of data, we have no means of evaluating precisely the quantities and the qualities of ice crystals smaller than 50 μm . The precision of the CPI within this size range (0-50 μm) is not sufficient. The ice crystal shattering effect complicates the problem further.

This consideration (correlation of the FSSP-PSD second mode with a part of the NP phase function due to ice crystals) is our main concern.

Nevertheless, we do not believe that the bimodal shape of the size distribution can only be a result of frozen water droplets or small ice crystals growing more rapidly than the droplets. Of course the Wegener–Bergeron–Findeisen (WBF) process could be very efficient in mixed-phase clouds. Nevertheless, because the data presented in this paper address a representative sample of measurements in mixed-phase clouds at different stages of evolution, the subsequent second mode of the size distribution should be observed on a broad size range, i.e. small diameters at the onset of the Wegener–Bergeron–Findeisen process (Fig. 4b for instance) and larger diameters at further cloud evolution (Fig. 4d for instance). But our observations clearly show that the second mode is always found in a rather narrow size range (25 – 35 μm). Moreover in cirrus clouds the Wegener–Bergeron–Findeisen process does not exist. The corresponding FSSP-100 cirrus measurements (see Fig. 6b) also showing a second mode in a similar size range, confirm that the second mode of the size distribution is not a consequence of the WBF process.

Nevertheless, in order not to deceive the reader, we specify our point of view in the introduction and discuss the WBF process in new section 5.

2) The ray tracing calculations produce “average” scattering cross sections yet the OPCs do not measure ensembles of particles in random orientations but measure individual particles in individual orientations. Consider the following: the results of the Borrmann study and those of the current evaluation show that ice crystals are generally undersized with respect to a water equivalent size. This is because the average orientation presents a geometric cross section somewhere between the maximum and minimum cross section. What if the flow through the FSSP inlet produces a velocity gradient that rotates the plates or columns into a preferential orientation so that as they pass through the sample area they aren’t randomly oriented but all more or less showing the same geometric cross section? This is not at all out of the realm of possibilities. King (1986) showed conclusively that the shear in front of wingtip mounted PMS probes led to the preferential orientation of ice crystals so that plates appeared as columns. If this is the case as air flows into the FSSP inlet, it means that

some fraction of the higher concentration, smaller ice crystals appear as larger particles that fall in the 25-35 bin because they present their largest cross section. This would explain why there is a secondary mode with higher concentrations than in the one or two channels lower.

As King (1986) explains in his paper, the air flow around the aircraft can induce turbulence near the probe. King shows very clearly an example of the effect of the turbulence on a preferential orientation of ice crystals. The example described by King shows that plates can be viewed as columns with a 2D-C probe.

Concerning the air flow, two perturbations can be distinguished:

- 1) The air flow around the aircraft,
- 2) The air flow disturbance due to the probe itself.

In the case of the air flow around the aircraft, we believe that no particular perturbation may be noted.

Firstly, the aircraft, used in this study, was used during several cloud *in situ* measurement campaigns. The problem of particle orientation has never been noted from an examination of viewer probe images.

All probes (FSSP and viewer) were mounted on wings pods at a distance from the leading edge calculated so as to be outside the flow perturbations.

Secondly, Fig. 6 shows bimodal FSSP PSD for three different campaigns: Polarcat 2008 with an ATR42, ASTAR 2007 with a Do228 and AEROCONTRAIL with a Falcon20. In others words, we show cases for three different aircrafts with three different nominal air-speeds.

For these three experiments, the analysis of 2D images does not show preferential orientation of crystals.

It seems reasonable to extrapolate this observation to FSSP measurements by assuming no preferential orientation due to aircraft flow.

Concerning the second point, i.e., the perturbation of the probe itself.

The flow distortion around PMS canisters (King, 1986 and McPherson and Baumgardner, 1988) may explain the preferential orientation of ice crystals of two-dimensional regular shape structure (i.e., plates, stars, columns, ...). In our mixed-phase clouds, the ice crystal shapes are largely dominated by irregular patterns with 3D-structure, mainly due to vapor deposition during the Wegener–Bergeron–Findeisen process (see examples of ice crystals in Figs. 4). Therefore the effects of the preferential orientation of such ice crystals are unlikely to be significant.

This section has been added to our new section 5.

This explanation seems like a more likely explanation for the secondary mode, given that the natural size distributions tend to decrease exponentially with size, i.e. if this mode was coming from 55-80 um particles, the concentration of these would have to be of order 50 per liter, according to the distributions shown in Fig. 4. Yet from the CPI data, shown in the same figure, the concentration of crystals in this size range are on the order of 0.1 per liter. It doesn't seem consistent that this mode is being produced by larger particles that are much lower in concentration.

We agree, this strong argument has been introduced in our text in the new Part -5 where we explore the origin of this mode.

3) What is the sample volume for ice crystals versus water droplets? There have been suggestions that the depth of field for ice crystals larger than the nominal size of 50 um is much larger than the 2.5-3 mm for water droplets, due to the way that the FSSP qualifies particles. The bimodal peak could possibly be out of focus, larger ice particles, that are being qualified but since they are in the much larger, but less intense, portion of the beam, they are undersized. This can be tested in a number of ways.

If this is the case, the concentration of particles in the 25-35 um range will be proportional to the slope of the distribution from 35-50 um.

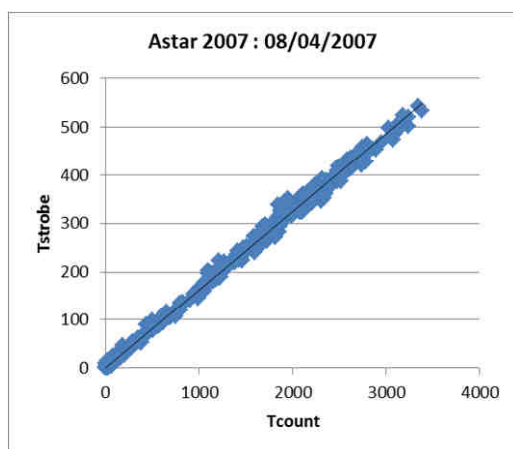
This suggestion is pertinent. However, with FSSP measurement-size cut-off at 45 microns, and the second mode set to 25/30 microns, the available class range is too small to calculate precisely the slope of the PSD.

Secondly, all FSSP-100s have auxiliary channels of housekeeping information that can be used to test this hypothesis. The ratio of accepted to DOF rejected particles, will indicate if a larger than normal fraction of particles are being accepted or rejected. Secondly, the velocity accepted fraction will also indicate a larger than normal fraction of particles being accepted as within the most intense region of laser sample area. These are two parameters that need to be utilized in the current evaluation.

The ratio of accepted particles to DOF rejected particles is not available on the FSSP version used in this study.

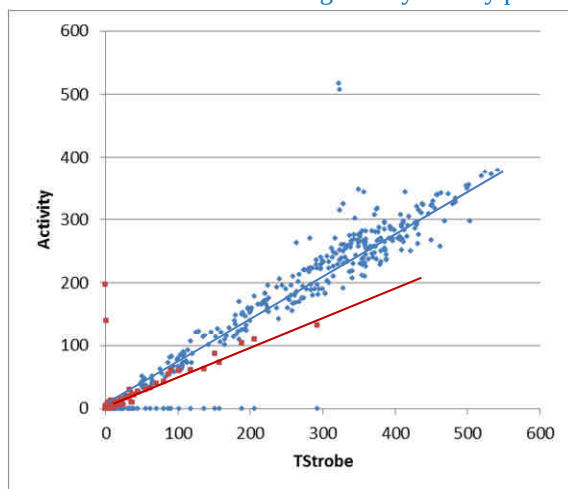
Nevertheless we have standard outputs named TStrobe and activity. The TStrobe is the total number of particles passed through the laser beam in the DOF. The total number of counted particles is equal to the TStrobe minus the fraction of particles passed through the laser edges (the rejection criterion is done with the measurement of the transit time of the particle in the laser cross section). The Activity parameter corresponds to the mean time used in the FSSP for processing the information. During this time, we consider the FSSP blind.

The figure (below) shows the Tstrobe as a function of the Total count for all the sequences described in the paper (Fig1, 8 April 2008 case study). In this set of data, water cloud, mixed cloud and ice cloud were sampled.



We observe a perfect straight line between these two parameters and thus, we are not able to identify the phase of cloud particles. The fraction of the rejected particles due to transit time in the laser beam is independent of the phase.

The next figure is more interesting. This figure shows the activity as a function of the TStrobe for the same set of aforementioned data. The water situations are plotted using blue and the ice situations using red. The discrimination was done using the asymmetry parameter of the Polar nephelometer.



The Activity is the working time of the probe. If we consider the DOF for a particular type of particles (i.e. ice crystals) to be bigger than for water droplets, for a same measured TStrobe, the Activity would be smaller. This is due to an underload of the FSSP. Indeed, the time needed to count a particle is longer than the time needed to reject a particle. Considering the following:

- Activity as the sum of the time needed to count particles (T_c) and the time needed to reject particles (T_r) :

$$\text{Activity} = T_c + T_r$$

- two samples with the same measurements of T_{Strobe} , but one sample is comprised of water droplets and the other of ice crystals
- for our demonstration, let us state arbitrarily that the DOF is bigger for ice.

In this case:

$$T_c(\text{water}) = T_c(\text{ice})$$

$$T_r(\text{water}) > T_r(\text{ice})$$

Indeed, the bigger the DOF the smaller the number of reject particles.

$$\Rightarrow \text{Activity}(\text{water}) > \text{Activity}(\text{ice})$$

We observe the expected position between ice and water.

The best regression lines give a factor 1.5 between the two slopes. The direct conclusion would be that the DOF is 1.5 bigger for ice than for water...

But, we consider that this set of data is not consistent enough and our maturity is insufficient (regarding this specific problem) to publish this for the moment...

In our paper, we state that the complementary parameters do not allow the variability of the DOF to be estimated due to the shape and size of cloud particles.

It is very important to emphasize here that the FSSP does not ever miss-size particles as long as the relationship between measured, scattered light intensity has been properly established. What the FSSP measures is the light scattered by a particle over a solid angle of +/- 4-12 degrees. This scattered light is related to an equivalent optical diameter of a water droplet.

When the FSSP measures the light scattered by an ice crystal and places this in a size bin, it is essentially classifying the ice particle with respect to the light scattered by a droplet with an equivalent optical diameter. For studies related to climate, i.e. to the evaluation of how cloud particles interact with radiation, there is no error in the measurement since, for example, a hexagonal ice crystal, with a major dimension of 80 μm and minor dimension of 20 μm , may have an optical diameter equivalent to a 35 μm water droplet. Clearly its phase function will be quite different than a water droplet but from the perspective of forward scattering, the two are equivalent. If the goal is to derive the water content then this is a different issue, although it is very likely that the volume of a plate with these dimension would not differ significantly from that of an equivalent volume water droplet. To reiterate, there has been no "error" when an ice crystal is classified into a water equivalent size bin since the definition of "size" is nebulous to begin with when measuring irregular particles and classifying them by equivalent optical diameter has obvious advantages.

Very few papers address forward scattering for a single oriented particle in relation to FSSP optics.

Kokhanovsky's 2008 paper suggests that in different conditions (water, ice, mixed) phase functions are significantly different. This is also the case for forward scattering. When phase functions are different, if for a solid angle (ie : 4° to 12°) the scattering energies are equal, optically (for this solid angle), particles are similar. We agree with this concept. If these scattering energies are used for particle sizing (ie FSSP probe) with a water droplet calibration, the particles are of the same equivalent size as a water droplet. We agree. This notion is introduced in the recent paper by Gayet et al. "The evolution of microphysical and optical properties of an A380 contrail in the vortex phase" submitted to ACP.

This notion of equivalent optical diameter/size is correct for an optical angle aperture and thus correct for a probe. Nevertheless, this notion should be generalized with precaution. The use of equivalent optical diameter of a water droplet in climate modeling can be hazardous. Hypothesizing that an ice crystal has the same forward scattering as a droplet with the same equivalent optical diameter (from the point of view of FSSP) seems to be a little hasty in our context.

The idea of equivalent optical diameter seems to be a very pertinent remark. We introduce this notion whilst specifying that this is unambiguous if the solid angle is mentioned.

With the notion of equivalent water diameter, we reformulated also our “error, contamination...”

This is a nice study that effectively demonstrates the importance of complimentary instruments such as the FSSP-100 and the Polar nephelometer to increase understanding of mixed-phase and glaciated clouds, and to isolate potentially serious measurement and interpretation errors. I recommend that the article be published after mostly minor modifications.

I do believe that the authors have understated the potential effect of ice crystal shattering in their introduction and in subsequent analysis. In particular, Korolev et al. 2011 have shown that the response of the FSSP-100 in ice clouds can be almost entirely due to ice crystal shattering on the inlet tube, and this can be almost greatly eliminated by removing the sample tube, and using deflecting probe tips.

The aim of this paper is not to understate the effect of shattering. It is largely accepted that FSSP response can be entirely due to ice crystal shattering. But, we would like to show the influence of real crystals on the shape of the FSSP-PSD. In the rest of our paper, we explore ways of interpreting measurements. We conclude with the fact that ice crystal shattering is the most likely explanation for the bimodal shape of FSSP PSD.

I also strongly recommend that the authors eliminate the paragraphs related to the estimation of the number of particle fragments produce by shattering crystals. The quantitative arguments are very weak and arbitrary in my opinion. For example, the concentration of particles $> 100 \text{ m}$ on the CPI is used to estimate the number of impacting ice particles. This is totally arbitrary, and the size distribution shown in Figure 10 shows many more CPI particles below 100 m .

This recommendation has been followed. The subject of paragraph 5 has been changed. We have removed the calculation of the efficiency, but have kept a simple evaluation of the ice crystal shattering risk by counting the number of crystals that the FSSP inlet edges encounter. We hope to demonstrate that in our set of data, ice crystal shattering is the main cause of ice crystals observed by FSSP.

The MVD size of 310 m is arbitrarily used to estimate the number of fragments per particle, where the concentration is lower than at any other arbitrary smaller size. And this all assumes that the CPI does not suffer itself from shattering. The inlet design of the CPI would lead the average reader to believe such shattering is likely to exist. Do the authors believe that the CPI is immune from shattering?

No, we do not. CPI and Polar Nephelometer probes have sampling inlet designs that may induce the shattering effect. The difficulty is to estimate the fraction of ice crystals broken by the probe itself. We underline that this is not the purpose of this paper.

If they do, they should state this in the paper with their justification. These shattering estimates detract from a paper where every other argument is reasonably convincing and supported by data. As a minimum the authors should greatly simplify this shattering argument, if they can, and provide realistic error estimates. I believe to do otherwise is misleading, and these estimates tend to get used in subsequent papers without the benefit of seeing how they were derived.

We accept this criticism.

It is not always clear what the authors' objective is in identifying the effect of ice crystals on FSSP distributions. For example, they refer to the ‘contamination’ of the measurements by ice crystals. This is a bit tricky. In one earlier study referring to ‘contamination’, the intention was to quantify the liquid portion of the spectrum, so that ice crystals in fact ‘contaminated’ the liquid PSD. In this paper, the authors determine a technique to identify PSDs that are mixed-phase or glaciated, and the ‘contamination’ of mixed phase measurements they refer to is presumably not the simple presence of ice crystals, but their potential erroneous sizing and/or the augmentation by artifacts. For real crystals, the authors state that the sizing errors can be large if the crystals are smooth, but

almost insignificant if they are rough. In figure 4 they report on concentration, area, and volume PSD errors for combined ice+liquid spectra, so I presume that the estimation of composite PSDs are considered of primary importance to their evaluation.

As mentioned in the response to D. Baumgardner's review, we agree with the ambiguity of "contamination". We have clarified this aspect in our paper by removing the word "contamination".

The subject of paragraph "4.2 effect of ice crystals on FSSP distributions" has been revised in order to clarify our demonstration.

In the specific comments, I have suggested some re-wording of certain sentences to address the points in this paragraph that I hope the authors will consider. My first interpretation in early reading into the paper was that any measurement of ice crystals by the FSSP, even if it was corrected somehow, would be considered 'contamination'.

The text would benefit from editing by someone whose first language is English. In the specific comments below, I have only made such corrections if I think the meaning of the sentence can be misinterpreted. I have also added some more optional editorial suggestions at the end of this review that I believe would help improve the text.

Thanks for all your suggestions; English is not our native language. The text was reread by Kate. We hope her corrections give sufficient fluency for the English native reader.

Abstract: Here and throughout the paper there needs to be a change in how the effects on the FSSP measurements are described. From the beginning the word "contamination" is used, i.e. "In this paper, we show that in mixed phase clouds FSSP-100 measurements may be contaminated by ice crystals : : :". Yet the word contamination, according to the American Heritage dictionary, means "to make unpure or unclean by mixing". This would possibly be a correct description of the effect on the measurements of spurious particles from ice crystal shattering but does not apply to the effect caused by mis-sizing due to asphericity. I think that a much better, and clearer, description would be "contribution to measurement uncertainties" , i.e. "In this paper we show how the presence of ice in cloud contributes to the uncertainties of measurements made with the Forward Scattering Spectrometer Probe (FSSP).

We agree with this precision. Our text has progressed in this sense.

Abstract: Here and throughout references are made to shattering on the FSSP tip but actually most of the fragments measured by the FSSP are from shattering on the inlet.

We agree.

Page 7910: lines 2-4 "In this paper, we show that in mixed phase clouds FSSP-100 measurements may be contaminated by ice crystals, inducing wrong interpretation of particle size and subsequent bulk parameters." I think the use of "contaminated" is ambiguous here. Please consider the following: "In this paper, we show that in mixed phase clouds FSSP-100 measurements may include both spurious measurements of shattered ice crystals and potentially incorrectly sized natural ice crystals, inducing improper interpretation of particle size and subsequent bulk parameters."

This comment about the ambiguity of the word « contamination » is pertinent. As the other referee has suggested, we have removed this word from our text.

Page 7910: lines 16 "but likely corresponds to bigger aspherical ice particles". Why likely? Could they not, according to your model calculations, be equally likely from rough or irregular ice particles with little size error? You imply the latter in the next sentence, in fact.

We have removed "likely".

Page 7910: lines 25- end of paragraph I suggest you take out the section on the ice crystal shattering efficiency, or propose something less arbitrary. See general comment.

This has been done.

Page 7911: lines 7-9 Small change at end of sentence: “In situ measurement science uses quantitative types of probes in order to perform the particle size analysis of hydrometeor range going from a few microns to a millimeter or more.

This has been corrected.

Page 7911, line 10: Spell out FSSP (and all acronyms) the first time.

This has been changed.

Page 7911: line 13. “especially when accurate measurements of cloud liquid water content (LWC) and droplet spectra are required”. I would suggest that you not make a statement implying that that FSSP provides accurate measurements of LWC. I would just state “especially when accurate liquid droplet spectra are required”.

This has been changed.

Page 7911: line 14. Should also add here: Cober, S.G., and G.A. Isaac, 2012: Characterization of Aircraft Icing Environments with Supercooled Large Drops for Application to Commercial Aircraft Certification. J. Appl. Meteorol. and Climatol., 51. 265-284.

One sentence has been introduced in the text with the reference. Reference list has been updated.

Page 7911: lines 21-24: “The scientific community of cloud physics (see the recent review on cloud in situ instruments by Baumgardner et al., 2011) seems to agree that the FSSP is a suitable probe only when the liquid phase is present, even if the discussion is not closed concerning the quantification of uncertainties in the evaluation of the LWC.” Do you mean above: “if only liquid phase is present” ?

Page 7911, line 22: “: : seems to agree that the FSSP is a suitable probe only when the liquid phase is present..”. I don’t think this is quite correct. The community agrees that the FSSP is an accurate instrument for all water clouds but, given the lack of an alternative, accepts that the FSSP and similar instruments can be used in clouds with ice crystals with clear caveats that should be understood before interpreting the measurements.

These two recommendations have been added to our text.

Page 7912: line 2: Change “the shattering effects” to “the ice crystal shattering effects”

This has been changed.

Page 7912, line 8: “spherical” I think should be “near-spherical” or “quasi-spherical”.

This has been corrected.

Page 7914: line 8 I don’t think the problem is isolated to shrouded inlets. I think it is best to reword as following: Change: “on probes with shrouded inlet” to “by ice particle impacts on surfaces upstream of the sample areas”

This has been changed.

Page 7914: line 9: “Heymsfield, 2007”. I think you should put a full list of the references on ice crystal shattering at this point. Heymsfield (2007) is neither the original reference in this regard, nor the most encompassing. Some of them are already elsewhere in your paper, but there are several missing.

Gardiner and Hallett, Field et al. (2003) Korolev et al. (2005), Heymsfield (2007), Jensen et al. (2009) have been added

Page 7914, line 9: “Experimental evidence shows that for particle diameters larger than about 100 μ m, the number of shattered particles increases with the concentration of large particles”. .Actually, the 100 μ m threshold has never actually been established and is a number that is too often used with no hard evidence. This should not continue to be propagated in this paper unless the authors are aware of a study or publication that I don’t know about.

The sentence has been removed.

Page 7914, line 10: “The new generation of cloud instruments (CDP, CIP, 2DS,. . .) are equipped with innovative shrouded inlets specially designed to reduce the shattering effects (Korolev et al., 2011) and provide information to separate real and artifact-shattered crystals (Field et al., 2003, 2006).”

The new designs are not for shrouded inlets, and I am not aware of a new design for the CPI. I would recommend the following: “The new generation of cloud instruments (e.g. CDP, 2DS) are being equipped with innovative arms and leading edge tips especially designed to reduce the shattering effects (Korolev et al., 2011) and provide inter-arrival time information to further help separate real and artefact-shattered particles (Field et al., 2003, 2006, Lawson 2011).”

Lawson, R. P., 2011: Effects of ice particles shattering on the 2D-S probe. *Atmos. Meas. Tech.*, 4, 1361-1381

This has been changed.

Page 7914: line 16: Change “The FSSP-100 instrument” to “The standard FSSP-100 instrument”. There is also the extended range instrument that you mention later in the paper.

We have introduced in this section the FSSP300 as asked by D. Baumgardner.

Page 7914, paragraph starting line 17: Korolev et al. (2011) show clear evidence that the FSSP number concentrations can be decreased by 2 orders of magnitude by removing the sample tube. This is not reflected in this paragraph. Obviously bulk parameters could be overestimated by more than 15-20% in this case. Maybe the Korolev examples are more extreme, but I don’t think you want to minimize the effect of FSSP shattering here (with a sample tube installed).

Our text has progressed in this sense.

Page 7914, last paragraph on CPI accuracy: Based on the results of Gayet et al., can the authors give a clear estimate of the shattering effects of the CPI, given that the 2DC is subject as well to shattering in this size range? You state that the CPI and 2D-C/2D-P have similar accuracy, but shattering on the 2DC at 100 μ m can still be quite significant, according to Korolev et al. 2011.

This paragraph needs to be re-written so as to clearly state what uncertainties the authors believe are in the CPI, and to what degree they think it suffers from shattering. The CPI results are used extensively later in the paper, and this reviewer believe that the there is no good evidence that CPI concentrations at 100 μ m are immune from shattering effects.

We do not have the means to evaluate quantitative errors of CPI instruments due to shattering effects. Comparisons between particle size distributions measured simultaneously by the 2D-C and CPI instruments do usually show a rather good agreement leading to state that these two instruments have similar basic accuracies as reported by Gayet et al. (2002). Of course this does not prevent additional errors due to shattering effects on both instruments. We may refer to the results by Field et al. (2006) which suggest that measured concentrations by 2D instruments could be affected by up to a factor of 4 where the mass-weighted mean size was in excess of 3 μ m,

while the estimate of ice water content was most affected for narrow size distributions and could be overestimated by 20%–30%. In extreme cases the errors could be larger.

Page 7914, line 19: “: : as 2 m and 30 %, respectively.” These are only for waterclouds.

This has been changed.

Page 7915: line 15: “Indeed, Sassen et al. (1979) proposes an identification of phase clouds on the basis of their side scattering differences with water or ice particles.”

Do the authors mean ““Indeed, Sassen et al. (1979) proposed identification of cloud phase on the basis of side scattering differences of water and ice particles””?

This has been corrected

Page 7916, line 17: Why are the angles of 3-15 used here and throughout? The nominal values for the FSSP-100 are 4-12 degrees.:

That is a misprint, 3-12 degrees are correct (Dye et Baumgardner 1984, Field 2003), and this mistake was also propagated in the fig. 7.

Page 7916, line 24: The word “power” should probably not be used here since were are not talking about scattering per unit time. maybe “energy” would be more appropriate.

This has been changed.

Page 7916, line 26: Change “lighted” to “illuminated”.

This has been changed.

Page 7917: line 13: “Discrimination of liquid water clouds and mixed or iced cloud is a great challenge for a correct FSSP measurements analysis, but a hazardous process if only the FSSP probe is available.”

Do you mean: “Separation of the liquid and ice components of the particle size distribution (PSD) in a mixed phase cloud, as well as quantitative estimate of the PSD of a glaciated cloud is a great challenge based on the FSSP probe alone.” ?

Not exactly. For us, the analysis of FSSP measurements requires knowledge of the particle phase. In liquid cloud, we use the classical approach with Mie theory. In mixed conditions, we need to separate in the FSSP PSD the liquid and ice crystals parts, in order to calculate, for example, the LWC. In ice conditions, the optical features of ice crystals in the optical aperture of the probe are necessary for result interpretation.

When the FSSP is the only *in-situ* measurement probe, the interpretation in mixed and ice cloud is tricky.

We propose rewriting the sentence in the following way.

To resume, during FSSP measurement analysis, the presence of ice crystals can induce a wrong interpretation if the used scattering model of particles is not appropriate. In this condition, when the FSSP is the only *in-situ* measurement probe, the interpretation in mixed and ice cloud is unsafe.

Page 7917: line 20: “Literature sometimes describes a typical behaviour of the FSSP in the presence of ice (mixed or iced clouds).” I don’t understand this sentence. Can you please re-write?

Page 7917, line 21: “Literature sometimes describes a typical behaviour of the FSSP in the presence of ice (mixed or iced clouds).” What does this mean?

I propose rewriting this paragraph.

Conversely, as Gardiner and Hallett (1985) claim, the shape of the spectra in the presence of ice particles may be a good indication of the presence of ice crystals. In this study, we will demonstrate below that the bimodal spectrum is undoubtedly the signature of ice crystals in mixed-phase clouds.

Our observations of this bimodal PSD in the presence of ice crystals have been previously reported. Some examples exist in the literature without a specific interpretation; see Lawson 2011, Ivanova et al. (2004).

Page 7917, Line 26: “: : altitude, droplet concentration, liquid water content (LWC) : :”. concentration and LWC are reversed in the figure.

This has been done.

Page 7918: line 3: “Without bulk water probe information during ASTAR, the consistency of the FSSP measurements was verified in liquid water clouds (i.e. $g > 0.83$) by comparing the extinction coefficient derived from the Polar Nephelometer data.”

What does this sentence mean?

Does it simply mean that the FSSP agreed with the PN in what was thought to be LWC clouds? If so, I would suggest substituting with: "The consistency of the FSSP and PN data was determined by comparing extinction estimates from both probes in cloud sections thought to be dominated by liquid (i.e. $g > 0.83$)."

This has been done.

Page 7918: line 13: “The C100 profile indicates that ice particles are found even near the cloud top with rather a low concentration (~ 1), which then significantly increases (up to ~ 40) at lower levels, with g -values of about 0.77.”

Are you saying that all, or the majority of particles > 100 μm (C100) are identified as ice particles from the CPI imagery?

Absolutely.

It should be stated in the text, because it is not necessarily obvious to all readers that all large particle are ice particles.

This has been done.

Page 7919, line 3: The phase diagrams are in the right not the left panels.

Yes, it was a mistake.

Page 7919, line 6: “The PSD: :”. However the CPI shows large ice crystals. I don’t think that the CPI images are very useful shown as they are. There is a need a quantitative assessment of the fraction of water to ice, concentration of large ice crystals, etc

The quantitative assessment of the water to ice ratio, concentration of large ice crystals is indicated in the manuscript with the use of the REX ratio.

Page 7919: line 7: “The close agreement between the PN measurements and the theoretical FSSP-100 values confirms this statement (seen as already on Fig. 2).” How does it confirm that this is liquid cloud? Do you mean that the close agreement between the PN and the FSSP extinction supports that the PSDs are accurate? I don’t see the requirement for liquid cloud. Maybe I missed something here.

We would like to demonstrate here that the case concerns only water droplets. To do that we have two arguments:

- a) The shape of the measured PN phase function.
- b) The good correlation between this phase function and those calculated from the FSSP PSD by assuming Mie Theory.

We propose to rewrite this sentence without reference to Fig2.

“The close agreement between the PN measured and the theoretical FSSP-100 phase functions confirms the statement that this cloud is dominated by water droplets, as reported by Gayet et al. (2009) concerning this type of cloud. Nevertheless, the presence of some ice crystals is noted.”

Page 7919: line 7: “Near the cloud top the particle phase is dominated by (spherical) liquid water droplets even if some ice crystals are detected (see Table 1 and Fig. 4a) as reported by Gayet et al. (2009). What was reported by Gayet et al. 2009? Was it that the tops of these clouds are often dominated by liquid cloud with some presence of ice crystals? If so, please re-write.

The sentence has been rewritten.

Page 7919, line 14: “: : seem to be correlated ; when the latter increases, the former shows a similar tendency.”. Is this shown somewhere? “

We suggest calculating the fraction due to ice crystals in the calculation of the extinction coefficient. These values have been added in table I.

The following sentence has been added.

“In order to illustrate the variability of the FSSP PSD second mode, we calculate the extinction coefficient from the FSSP-PSD (Ext_{total}) and the fraction of this coefficient calculated using only the second mode (Ext_{sm}). Here, if we neglect errors due to the implicit water droplet hypothesis, the ratio between these two values (Ext_{sm}/Ext_{total}) gives us the contribution of the second mode in the total extinction coefficient, these values are reported in Table 1.”

Page 7919, line 14: change “ration” to “ratio”.

This has been corrected.

Page 7919, line 15: “We define REX as the ratio of extinction due to ice particles alone (CPI data) to the total extinction (water droplets and ice crystals, PN measurements).” Move this definition before the use of REX.

This has been done.

Page 7919: line 17: “Conversely, the difference between the measured (PN) and theoretical (FSSP-100) phase functions at sideward scattering angles increases with REX values (see Table 1).” This is not clear, and I am not sure what you are saying. Please re-write.

This has been done.

Page 7920, line 22: “Our results clearly show that the second mode in the range 20–35 m of the FSSP-100 size distribution is related to the presence of ice particles”. No, there is an association not a relationship. There is a very large difference between a relationship and an association with respect to cause and effect.

We have replaced “related” by “associated

Page 7920, line 26: This is the first mention of the FSSP-300. This should be introduced in the section on instrumentation.

This has been done !

Page 7921, line 6: “: : smaller secondary mode..”. The secondary mode is not that much smaller than "mode" in first channel. These distributions look quite different than those shown in Figure 4.

This has been rewritten in order to explain our idea shown on Fig 6b

Fig. 6b displays typical FSSP size distributions measured during ASTAR and AEROCONTRAIL experiments. It shows different positions of the secondary mode on size axis as a function of the airspeed ($25 \mu\text{m}$ for 200 m.s^{-1} against $35 \mu\text{m}$ for 100 m.s^{-1}).

Page 7921: line 10: “Therefore a common feature is observed in the presence of ice crystals regardless of the probe version and airspeed” Not exactly. Korolev et al. (2011) provide convincing data that this second mode is dominated by shatters created by the sample inlet on the FSSP. So, FSSPs without a sample tube probably have a different behaviour, as would the CDP. Please reword.

This has been reworded

The set of data presented by Korolev et al. 2011 probably provide important elements to clarify the problem. It would be interesting for us to work with these data.

Page 7923, Figure 7: Use a more meaningful legend for the crystal types. The figure caption does not sufficiently explain the curves. These are averaged over all different orientations.

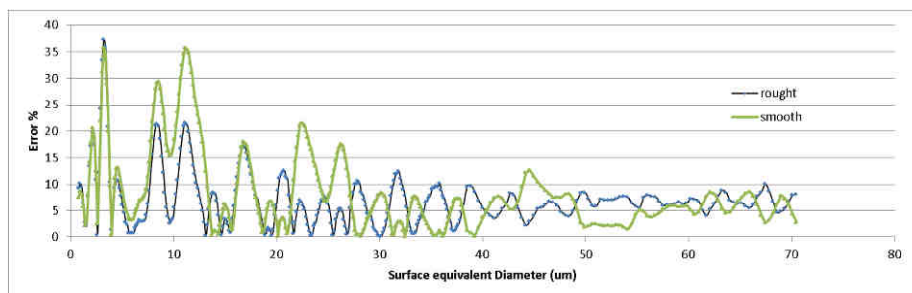
We agree, figure caption has progressed.

What type of variation is there, i.e. there should be vertical bars indicating the range of scattering cross sections.

We have added on the figure the scattering cross section corresponding to different classes of the FSSP for standard and extended range versions.

Page 7923, Line 21: “..no more than 15%..”. On average, perhaps, but when looking at variation, there could be a very different outcome.

The figure below presents the error calculated as the absolute value of the difference between values of scattering cross sections for form factor 2 and 0,5 normalized by the mean value of scattering cross sections.



This figure shows the error is never larger than 40%. The larger values of the error are for diameters smaller than 30 microns, they are due to the oscillation of the scattering cross sections in this diameter span.

We have specified « on average » in the text.

Page 7924, line 17: How is Delta calculated? : explain

The ratio value (DELTA parameter) is calculated as follows :

$$\text{Delta} = 100 * \text{XI} / (\text{XW} + \text{XI})$$

Where : XI is the sum from FSSP class 8 to 15 of $\text{Ni} * A_i$,
XW is the sum from FSSP class 0 to 7 of $\text{Ni} * A_i$,
 Ni the concentration of the class i ,

A corresponds to the diameter, the surface or volume of the particles as function of the considered curve.

Page 7924, line 22: Under or overestimated?

When delta is equal to x% it corresponds to the part of ice crystals in the bulk parameter calculation. It is not like an error where +/- sign informs of the over or under estimation.

Page 7924: line 23: "At the same time the asymmetry parameter remains within a deviation of 0.01 (Fig. 8b)." It is not 100% clear to this reviewer how Fig. 8b that tells me this? REX below 0.2 is 25% of the way between b and c. I see a deviation of 0.02 for the same range on 8b. It is difficult to understand your point here. Maybe you should reword, and correct the 0.01 for 0.02 if I am correct.

This has been corrected.

Page 7925, Section 5: I would remove this section entirely. First of all, the focus of the paper should be on identifying and evaluating all the effects of ice crystals on the FSSP except for shattering. Secondly, attempting to derive a shattering efficiency is fruitless given all the uncertainties related to this process. Why would the fragments all fall into the 25-35 um category? Why would you assume the fragments will spread uniformly across the sample volume? Why would you assume that 50% fall in and out of the inlet? A very detailed modeling study is needed to examine these issues in detail and have no relevance in the current study.

I would remove this section entirely.

We agree to remove a part of this section. But it is necessary to introduce the problem of ice crystal shattering because it is the most probable hypothesis for a correct interpretation of the bimodal FSSP PSD.

We have revised all this section accordingly.

Page 7925: line 17: Regarding the reference to Heymsfield (2007), if you want to quote the extreme situation for the FSSP, you need to quote Korolev et al. (2011), who found that ice crystal concentrations could be increased by up to 2 orders of magnitude by the sample tube of the FSSP. I am not sure if there are estimates of extinction increase or mass in that reference, but their data showed that there was almost no registration on the FSSP without a sample tube, and quite high concentrations with the simultaneous second probe with the sample tube. You need to consider this work in this paragraph.

Page 7925: line 20: You need to add Korolev et al. (2011) to the reference list here.

This has been added.

Page 7925: line 24: "There are no means of discriminating real and artefact ice particles related to the FSSP-100 secondary mode." Some FSSP probes have been modified to measure interarrival times, which helps in eliminating artefacts. This should be mentioned somewhere in the text, because the statement above is misleading. Also, the CDP has the particle-by-particle option, which gives the interarrival time, and it might be more commonly used than the FSSP in the near future.

This has been done.

Page 7925: line 26: "with new inlets specially designed" I think you mean "new arms and leading edge tips specially designed"

This has been corrected.

Page 7926: line 2: "Figure 9 gives convincing arguments showing that the number concentration of particles larger than 20 m (hypothesized to be ice shattered-fragments measured by the FSSP) is related to the concentration of (natural) ice particles larger than 100 m"

Note that some readers could argue that particles measured in the size range smaller than 100 μm may be positively correlated to concentrations larger than 100 μm for natural reasons. For example, if the PSDs at any given point in cloud change only by mixing, you would just have scaled PSDs across the cloud, and a natural positive correlation. I think that the authors should make the point here if they think this correlation should not exist in natural clouds. In my opinion it is ‘evidence’, but not a ‘convincing argument’.

This has been done.

Page 7927: line 11 to end of section: I suggest that the authors remove the entire argument on the estimation of the number of shattered particles. See argument in ‘general comments’

Ok...

Page 7927: line 22: “We show that in mixed phase clouds the FSSP measurements could be contaminated by ice crystals, inducing a wrong interpretation of the particle size and subsequent bulk parameters. Conversely, this contamination is revealed by a bimodal feature of the particle size distribution which could be a relevant indication of the presence of ice particles in mixed-phase clouds.”

This comment is related to my discussion of the term ‘contamination’ in the ‘general comments’. I find the use of the word ‘contaminated’ in your sentence is not clear. I prefer something like the following:

“We show that in mixed phase clouds FSSP measurements could contain errors, resulting from ice crystal artefacts or improper measurement of natural ice crystals, which affect the PSD and its bulk parameters. The presence of ice crystals in the mixed-phase PSD is identified by a characteristic bimodal feature.

This has been corrected.

Page 7928, Line 3: “The larger the amplitude of the second mode, the greater the ratio (REX) of extinction carried by ice particles to the total extinction (water droplets and ice crystals).” This is never shown quantitatively or even in a table or figure.

Same remark has been made above.

Page 7928, line 15: “The results suggest that the second mode peaked between 25 μm and 35 μm does not represent true size responses but likely corresponds to bigger aspherical ice particles.” No, as I discussed at the beginning, if the second peak is not a result of shattered particles, then it is a correctly measured equivalent optical diameter for a water droplet.

Page 7928: line 25: “The results suggest that the second mode peaked between 25 μm and 35 μm does not represent true size responses but likely corresponds to bigger aspherical ice particles.”

Does this sentence contain an error? Did you not state in the text that rough ice crystals have almost the same size response as spherical drops? Could you change this to: “The results suggest that the second mode peaked between 25 and 35 μm would not represent the true sizes of aspherical particles if they were present, especially if they were smooth” (and delete the following sentence)

This has been done.

Page 7928: line 20: “As for the number concentration measurements they are hampered by the unknown definition of the depth of field to aspherical randomly oriented ice crystals.” Was this discussed earlier in the text? If not, it should not appear here first in the conclusions.

This sentence has been removed.

Page 7928: line 22: “There are no means of discriminating real and artefact ice particles related to the FSSP-100 secondary mode”

My comment is again to make the reader aware that inter-arrival time may help, and some modified FSSPs and some CDPs are equipped with this option

How about: “There are no means of discriminating real and artefact ice particles related to the FSSP-100 secondary mode without additional data such as interarrival time that is not commonly available, and even then results may not be conclusive.”

This suggestion has been added to the text.

Page 7928: line 22: I suggest you delete all the end of the paragraph starting with “We define the shattering efficiency ..”

This has been done !

Other notes:

I did not see figs. 9a and 9b, just fig. 9.

Real ice crystal size-distribution is presented on Fig. 10 (not 9a)

Editorial suggestions:

Page 7911: lines 17, 18. “The LaMP’s activities in the area of aircraft icing and the implication of the FSSP in these studies give motivation to explore any situation capable of increasing the knowledge of FSSP behaviour.”
Change to: “The LaMP’s activities in the area of aircraft icing and the importance of the FSSP in these studies motivates exploration of any situation capable of increasing the knowledge of FSSP behaviour.”

This has been changed.

Page 7911: lines 18-20: “This is useful mainly in order to calculate bulk parameter such as the liquid water content (LWC) and mean volume diameter (MVD) with minimum errors.” Change to:

“The objective is to calculate bulk parameter such as the liquid water content (LWC) and mean volume diameter (MVD) with minimum errors.”

This has been changed.

Page 7912: line14: “the size response” Change to: “the analysis of the size response”

This has been changed.

Page 7912: line 24-25: “The conclusion always seems to be similar namely that it is difficult” to Change to: “They conclude that it is difficult to”

This has been changed.

Page 7913: line 2: “measurements in Arctic mixed-phase clouds” Change to: “measurements using a data set for Arctic mixed phase clouds”

This has been changed.

Page 7913: line 13: “to measure cloud particle properties” Change to: “, to measure cloud particle properties” (comma added)

This has been changed.

Page 7913: line 16: “completed the ATR42” Change to: “were additional to the ATR42”

This has been changed.

Page 7913: line 23: “extended” Change to: “additional”

This has been changed.

Page 7914: line 2: Change “We recall that it includes” Change to: “The instrumentation package includes”

This has been changed.

Page 7914: line 7: “could be hampered by” Change to: “may be compromised by”

This has been changed.

Page 7914: line 15: “study, the effects of resulting” Change to: “study, but the potential effects of resulting”

This has been changed.

Page 7916: line 1: “affected” Change to: “adversely affected”

This has been changed.

Page 7916: line 6: “was yielded” Change to: “was present”

This has been changed.

Page 7916: line 12: “were planned” Change to: “were conducted”

This has been changed.

Page 7916: line 22: “Usually, water liquid spheres are used in Mie calculations” Change to: “Mie calculations assume liquid water spheres.”

This has been changed.

Page 7916: line 22: “As a consequence, particles” Change to: “Particle “

This has been changed.

Page 7916: line 22: “of the lighted particle” Change to: “of the light-scattering particle”

This as been changed by “Illuminated” proposed by D. Baumgardner

Page 7917: line 1: “For the same geometric volume, aspherical particles scatter between 3_ and 15_ a light power that differs from those predictable using Lorentz-Mie theory (Borrmann et al., 2000).” Change to: "For the same geometric volume, light scattered by aspherical particles between 3 and 15 degrees differ from that predicted by Lorentz-Mie theory (Borrmann et al., 2000)."

This has been changed.

Page 7917: line 4: “size ranging can be affected” Change to: to “instrument sizing can be affected”

This has been changed.

Page 7917: line 5: “leading to uncertainties in the bulk parameter calculation and mean diameter” Change to: “leading to uncertainties in the bulk parameter calculations (e.g. mean diameter, MVD, LWC etc.).”

This has been changed.

Page 7917: line 9: “usable” Change to: “possibly effective”

This has been changed.

Page 7917: line 12: “To resume, FSSP” Change to: “In summary, FSSP”

Page 7917: line 17: “Conversely, the shape” Change to: “However, the shape”

Page 7917: line 23: “in the mixed-phase stratiform cloud layer and yielded precipitations” Change to: “in the mixed-phase precipitating stratiform cloud layer”

This section has been rewritten

Page 7918: line 17: “In order to evidence” Change to: “In order to reveal”

This has been changed.

Page 7918: line 19: “addresses the top of the cloud layer” Change to: “corresponds to the top of the cloud layer”

This has been changed.

Page 7918: line 21: “ (d) relates precipitating ice crystals” Change to: “ (d) corresponds to precipitating ice crystals”

This has been changed.

Page 7919: line 21: “the second mode of the PSD is strongly marked whereas simulated” Change to: “the second mode of the PSD has a strong identifying feature, and simulated”

This has been changed.

Page 7919: line 26: “ice crystals are more dominating the” Change to: “ice crystals increasingly dominate the”

This has been changed.

Page 7922: line 17: “different realizations whereupon” Change to: “different scenarios where”

This has been changed.

Page 7922: line 20: “We recall that surface roughness” Change to: “Surface roughness”

This has been changed.

Page 7922: line 24: “domain of the rough influence” Change to: “domain of the roughness influence”

This has been changed.

Page 7923: line 18: “an assessment of the size response” Change to: “an approximation of the size response”

This has been changed.

Page 7923: line 22 “The rough aspect seems to play the crucial role in scattering studies. The difference between water and smooth ice crystal calibration is extremely large with an influence on the channel width. On the contrary, the scattering properties of a crystal with a deep roughness” Change to: “The crystal roughness seems to play the crucial role in its scattering properties. The difference between the water and smooth ice crystal size calibration is extremely large with an influence on the channel width. On the contrary, the scattering properties of a rough crystal”

This has been changed.

Page 7924: line 5: “crystal pictures” Change to: “crystal images”

This has been changed.

Page 7924: line 8: “Sensitivity studies deserve to be carried out in, this domain, although remain outside the scope of this paper” Change to: “Sensitivity studies should to be carried out in the future in this regard, but are beyond the scope of this paper”

This has been changed.

Page 7924: line 26: “a subsequent g-decrease” Change to: “a corresponding g decrease”

This has been changed.

Page 7924: line 28: “the ice crystals control” Change to: “the ice crystals highly dominate”

This has been changed.

Page 7925: line 5: “an adequate modelisation” Change to: “an appropriate consideration”

This has been changed.

Page 7925: line 7: “are in this case precious allies of” Change to: “provide crucial additional information for proper interpretation of”

This has been changed.

References :

King, W. D. « Air Flow and Particle Trajectories around Aircraft Fuselages. IV: Orientation of Ice Crystals ». *Journal of Atmospheric and Oceanic Technology* 3, n° 3 (1986): 433-439.

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MacPherson, J. I., et D. Baumgardner. « Airflow about King Air Wingtip-Mounted Cloud Particle Measurement Probes ». *Journal of Atmospheric and Oceanic Technology* 5, n° 2 (1988): 259-273.