***Interactive review of* " A comparison of light backscattering and particle size distribution measurements in tropical cirrus clouds" by F. Cairo et al.**

We would like to thank the Referees for their careful and proficient review of our paper. In the following, answers to their remarks – the latter reported in *italics* - are provided. The sentences we intend to add to our manuscript, in response to the Referees' suggestions, are marked in **bold**.

<u>Response to Referee 1</u>

In the general review, a weakness is pointed out, namely the use of Mie calculation – which is appropriate for particles of spherical shape – to model the optical response of aspherical scatterers. As the Referee acknowledges, this choice has been discussed in our manuscript, and is hereby more detailed.

The authors have already used (see for instance Scarchilli et al., "Determination of polar stratospheric cloud particle refractive indices by use of in situ optical measurements and T-matrix calculations", Appl. Opt. 44, 3302-3311, 2005) mathematical tools more sophisticated than the simple Mie theory to model the scattering from aspherical particles and compare it to optical measurement. In their previous works, particle counter and backscattersonde data were acquired during a series of stratospheric balloon flights. In the present work they have considered that approach – which would have led to a tenfold increase in computing time - not worth the effort because the comparison of the two dataset was already affected by other sources of uncertainties that would have spoiled the attempt to improve the optical modeling. More precisely, in the present case uncertainty not only arises from approximating the size spectrum with an histogram, but also from the larger distance between the particle counter and backscattersonde on board the aircraft and – even more important - the reduced time resolution and the higher speed of the platform, leading to a greater uncertainty in the synchronization of the two instruments, that likely increased the discrepancy arising from the lack of spatial homogeneity of the cloud. The authors have estimated in their manuscript the uncertainty of an analysis based on the Mie approach, and judged them of minor importance with respect to those arising from the measurement conditions.

Response to specific comments:

p.4067, ln.2. ". . . the discrepancy remaining on average within a factor of 2". Can it be seen on panel F of Fig.2? What does the linear fit on that panel mean? Is it "perfect" case?
It may be seen, but maybe not so clearly. The straight line is the y=x "perfect" correspondence between the computation with the two approaches.
To make our statement clearer, a gray band highlighting the region of the "factor 2" agreement between the two computations has been added to Fig.2 panel F, and the caption corrected accordingly.

p.4067, ln.5. "This result is likely due to the fact that in the present study the scattering is dominated by particles whose diameters are above 4 μm, and allows us to rule out any appreciable dependency from the particular binning used, on our optical computations." This sentence is not clear for me.
What is meant is the following: The dependence of the Mie backscattering efficiency Q on particle size is rather complicated, showing an extremely oscillatory behavior. When Q it is averaged over a finite particle size window, these sharp oscillations are smoothed out. However, lower frequency variations in Q are still present, and taper off to the geometric optics value of 2 only at large values of the particle size (see for instance the classical Twomey and Howell, Appl. Opt. 4, 501-505, 1965). These oscillations, which are larger at smaller particle sizes, make the result of an integration of the backscatter coefficients over a particle size spectrum, dependent on the particular binning used to render the size spectrum. This binning dependence became less and less important as the particle size spectrum shifts to larger size values.
Hence, our remark that in the presented case, the lack of a significant binning dependence in the results of our integration is attributable to computations on particles "large enough" to neglect such problem.
We acknowledge that our quoted value of 4 μm, as the threshold value to consider negligible the binning dependency, has been chosen rather arbitrarily, only based on the fact that is close to the lower particle size detecton limit of the FSSP.
In our manuscript, in pg. 4067 ln. 4, we rephrased our sentence as:

"This result is likely due to the fact that in our computation the scattering is dominated by large enough particles, for which the oscillation of the Mie backscattering coefficient is small, hence allowing us to rule out any appreciable dependency from the particular binning used on our optical computations."

Please note that in the present formulation we do not state that "…the scattering is dominated…", but that "…IN OUR COMPUTATION the scattering is dominated…"

p.4067, ln.20. Just want to mention that for the real part of refractive index 1.35 and particle radius of several mcm the backscattering by spheres is about 2.5 times higher than backscattering by randomly oriented spheroids of the same volume (Veselovskii, (2010), Application of randomly oriented spheroids for retrieval of dust particle parameters from multiwavelength lidar measurements, J. Geophys. Res., 115, doi:10.1029/2010JD014139, to be published).
We acknowledge the difficulty of giving definite numbers for quantifying the Mie backscattering overestimation, compared to real world. Unfortunately – when it comes to cirrus particles – the real world is neither spherical nor made of oblate or prolate spheroids… as instance, the backscattering can vary as much as a factor 10, from equal sized hollow to solid columns!, see Liou and Takano, Atmos. Res. 31, 271-298, 1994. Large uncertainty would arise whichever mathematical tool you may use to model the scattering by aspherical particles of unknown shape. A prudential statement may affirm that in our case an overestimation may be expected, and that may be as high as a factor 4. We rephrased the sentence on p. 4067, ln. 17-20 as follows:

"An educated guess of such overestimation can be provided by looking at studies comparing the phase function of aspherical vs spherical scatterers, which suggest an average overestimation of the Mie backscattering coefficient by a factor 2, which may possibly get as large as a factor 4 or more, depending on particle sizes and shapes (Mishchenko et al., 1996)."

p.4070, ln.2. "This correspondence suggests that the portion of size distribution sampled by the FSSP is sufficient to account for the overall optical properties of tropical high cirrus clouds at 532 nm." Mie calculations overestimate backscattering, but if part of the particles stay undetected by FSSP, these effects may be partly compensated. . .
The Referee is here posing a correct remark on an important issue. It is true that there might be a partial compensation, and in fact the scope of our paragraph 2.3 was to try to quantify how important could it be. Our results shows that even a relatively little amount of undetected small particles would have a major effect on the backscattering. Quantitatively, an undetected volume as small as 20% of the volume detected by the FSSP, could account for a doubling of the backscattering (since the results of the paragraph show that a doubling of the volume creates a tenfold increase fo the backscatter, and these results scales linearly with the volume, see our response to Referee 2 *p.4068 l. 14* hereafter). This already poses a limit to the amount of undetected particles. Moreover and more important, it is arguable that the amount of (undetected) small particles may depend on the different processes that led to the cirrus formation.
The fact that the fraction of the size distribution detected by the FSSP well reproduces the observed backscattering, with no biases over five orders of magnitude, i.e. from subvisible cirrus to thick anvil outflows, seemed to us a good hint that, if ever the suggested partial compensation is present, it should not play a major role.

Fig.6-Fig.9 ". . . measured by the backscattersonde (vertical). . . " Vertical or horizontal? Because surface, volume etc are shown for vertical. I don't understand also the capture on the lower panel : "aerosol backscattering coefficient –FSSP". Does it mean that backscattering is calculated from FSSP data?
The captions and axes titles have been changed. Moreover the aerosol backscattering coefficient – from MAS is now displayed (see answer to Referee 3, *p.4071 and Fig. 6-9)*

The authors mention possibility depolarization measurements, but no depolarization data are given. Since we used Mie theory, no depolarization have been presented or discussed. The comparison was carried out between total backscattering (by summing its parallel and cross component) as measured by MAS, and as computed from FSSP observations. A line has been added at pg. 4063, ln. 14, explicating that:

"A total Backscatter Ratio has been computed by summing its parallel and cross components, with respect to the polarization of the emitted laser light."

<u>Response to Referee 2</u>

In the general comments, the Referee addresses two topics of relevance:

1) The request to explicit the different ways the relevant geophysical parameters, namely aerosol volume backscatter coefficient and depolarization, are retrieved from backscattersonde and lidar raw data.
In fact, as pointed out by the Referee, lidar measurements are affected by the effect of molecular and particle extinction along the optical path, and a suite of inversion technique is reported in literature to properly account for that effect (Klett, 1981; Fernald, 1984; Young, 1995; among many). Conversely, such effect is not relevant in backscattersonde measurement, since that instrument performs in-situ measurements unaffected by attenuation along the optical path.
Obviously, once the effect of attenuation has properly been taken into account in the lidar inversion, the retrieved backscattering coefficient should attain that same value possibly measured in situ by a backscattersonde.
However, the details of lidar inversion to get to a correct aerosol backscatter coefficient have no practical concern with the aim of the paper, that starts ahead of that, to relate microphysical particle measurements with the backscatter coefficient.
To meet the Referee suggestion, the following lines have been added in the introduction of our manuscript to explicitly describe the different ways backscattersonde and lidar measure the optical coefficient (pg. 4062 ln. 2).

"In fact, one of the geophysical observables of the elastic lidar technique is the particle volume backscattering coefficient β. The backscattersonde offers an advantage with respect to the lidar, whose measurement of backscattering is affected by the molecular and particle extinction along the laser optical path. In fact, the molecular extinction can be evaluated by the Rayleigh scattering theory from measurement of air density, or from a suitable atmospheric model, while the particle contribution to backscattering and extinction coefficients is unknown. Both quantities are present in the lidar equation and have to be retrieved from measurements. Different inversion techniques have been reported in the literature to properly account for that effect (Klett, 1981; Fernald, 1984; Young, 1995), all assuming some a priori relationship between particle backscattering and extinction. Such assumption is itself a source of uncertainty for the lidar derived particle backscatter coefficient (Russell et al., 1979) . Attenuation is, on the contrary, of no concern in backscattersonde measurements: the instrument performs optically unattenuated in-situ measurements. Obviously, once the effect of attenuation has properly been taken into account in the lidar inversion, the lidar retrieved backscattering coefficient should attain – within its limits of accuracy and precision - that same value possibly measured in situ by a backscattersonde."

Russell, P. B., Swissler, T. J., McCormick, M., P.: Methodology for error analysis and simulation of lidar aerosol measurements, Appl. Opt., 18, 3783-3797,1979.

Klett, J. D., Stable analytical inversion solution for processing lidar returns, Appl. Opt., 20, 211-220,1981.

Fernald, F. G., Analysis of atmospheric lidar observations: some comments, Appl. Opt., 23, 7113-7131, 1984.

Young, S. A., Analysis of lidar backscatter profiles in optical thin clouds, Appl. Opt. 30, 7019-7024, 1995.

2) We claimed to measure the backscattering at 1064 nm as well, but although such measurements would be valuable to the paper aims, we did not present any of them.
The backscattersonde do measures particle backscattering at 1064 nm. Unfortunately such measurements are hampered by the feebleness of the molecular backscattering so that often a calibration of the signal and a quantitative determination of the backscatter coefficient is impeded or questionable.
That is why we often use the 1064 nm measurements – which in principle are far more sensitive to particle presence - only as an internal consistency check for the more accurate 532 nm measurements, and we do not want to present them in the present context.

Response to specific comments:

p. 4063 l. 23 What is the uncertainty for the 1064 channel ? What is the uncertainty on C532 and C1064 ?
We answer to the first part of the question for the sake of completeness, and possibly to have a feedback from the Referee for directions to future work, although we do not intend to present in our manuscript any 1064 nm data, for what detailed hereabove.

As reported, the lack of a reliable molecular signal at 1064 nm prevent us from performing an absolute calibration. Incidentally we note that the same problem is also affecting the 1064 nm channel of the satellite-borne CALIOP lidar.
One way to circumvent such difficulty would be to assume a Color Index value on some clouds (i.e. to impose the 1064 nm backscatter coefficient value), then gauge everywhere the 1064 nm observations to that value (see for istance what proposed for CALIOP in Reagan et al., IEEE Trans. Geosci. Remote Sens., 40, 2285-2290). The choice of the “reference cloud” should be driven by the reliability of the C. I. assumed for it. If such a procedure is applied, the relative error on the 1064 channel would be the sum of the one on the 532 channel, and the one to be attributed to the choice of the C. I. reference value . For this latter, likely values and variability could be found in the literature (see for instance Tao et al., Appl. Opt. 47, 1478-1485, 2008). A similar approach is feasible if the system’s characteristics do not vary appreciably in the course of its usage. Unfortunately, during two of the three campaigns whose results are here presented, our system has often been realigned between flight, this operation changing the intercalibration between channels.
For what concern the second part of the question, the values of C532 and C1064 came from optical scattering theory (see eq. 4.7 in Collis, R. T. H. and Russell, P. B.*: Lidar Measurement of Particles and Gases by Elastic backscattering and Differential absorption*, in “ Laser Monitoring of the Atmosphere”, edited by: Hinkley, E. D., Springer Verlag, Berlin, 1976) and therefore there’s no uncertainty attributable to them.

p 4064 l.1 Discuss here also the time resolution of the FSSP. Is it comparable to the 5s of the backscatter sonde ?
We have added the following line on 4064 ln 4

“The time resolution of the FSSP can be made as small as 2s, if sufficient counting statistics is available, i.e. in thick clouds.”

Then, as reported in pg. 4065 ln. 7

“The time series of the two instruments were interpolated to a common 10 s resolution time grid, corresponding to a spatial average over 2 km along the aircraft trajectory.”

The authors do not say anything about the shattering effect related to the particle sampling. This may underestimate the number of large crystals and overestimate the number of small crystals seen by the FSSP. What is the expected error?
We have reported the following considerations in our manuscript, on pg. 4071 ln. 4.

“In discussing these dataset, we have to take into account the possibility of shattering effects, which is surely an issue in aircraft in situ particle detection. The issue for the present FSSP dataset has been extensively discussed in De Reus et al., (2009). There, the authors were able to rule out shattering effects for cloud IWC smaller than 10 ^-4 g m^-3, but – although there were good indications that the influence of shattering was small over the whole dataset - they were not able to definitely exclude it for the denser clouds. The reported IWC number, according to the V-β relationship suggested in our work and discussed later on, translates into particle backscattering coefficients smaller than approximately 10^-6 m-1 sr-1, roughly in the middle of our magnitude range.
Shattering effects are most prominent in clouds containing ice particles with sizes above several hundred microns. We selected cloud periods where the particles mostly were smaller or where the likelihood of having significant numbers of (too) large particles was low. As instance, the cloud cases displayed in Figure 2 only contain particles with maximum sizes below 200µm and small number concentrations.
Therefore, shattering is thought to have a minor or negligible effect (Lawson et al., 2008; Jensen et al., 2009). Moreover, these high tropical clouds seem to contain only simply structured particles (Lawson et al., 2010). This means plates, elongated spheriods, droxtals and not complex aggregates of crystals, rimed particles, complex hydrometeors. Such large particle aggregates cause considerable shattering effects. As a matter of fact, if there had been low concentrations of large particles present in the sampled air masses, then these would not have cause too much of a signal in the backscattersonde in comparison to the more abundant small particles. However these particles would have generated many shattered fragments which would have been detected as small particles by the FSSP. Then the FSSP derived backscatter ratios would strongly disagree (being consistently higher) with the MAS results. This is not the case.
In fact, the size distribution detected by the FSSP well reproduces the observed backscattering, with no significant biases over its range of magnitude, i.e. from thinner to thicker clouds. This seems a

good indication that an underestimation of large crystals and overestimation of small crystals has no leading effect, and shattering is not a major "player" in the sampled cloud volumes."

Jensen, E. J., Lawson, P., Baker, B., Pilson, B., Mo, Q., Heymsfield, A. J., Bansemer, A., Bui, T. P., McGill, M., Hlavka, D., Heymsfield, G., Platnick, S., Arnold, G. T., and Tanelli, S.: On the importance of small ice crystals in tropical anvil cirrus, Atmos. Chem. Phys., 9, 5519–5537, http://www.atmos-chem-phys.net/9/5519/2009/, 2009.

Lawson, R. P., Jensen E., Mitchell D. L., Baker B., Mo Q., Pilson B., Microphysical and radiative properties of tropical clouds investigated in TC4 and NAMMA, J. Geophys. Res., 115, D00J08, doi:10.1029/2009JD013017, 2010.

**Lawson, R. P., Pilson, B., Baker, B., Mo, Q., Jensen, E., Pfister, L., and Bui, P.: Aircraft measurements of microphysical properties of subvisible cirrus in the tropical tropopause layer, Atmos. Chem. Phys.,
8, 1609–1620, 2008.**

p. 4065 l.21 typo in "variabilities"
This has been corrected.

l. 14 I do not the added value of figure 1 to document the characteristics of cirrus clouds encountered during the 3 campaigns. Table 1 serves this purpose much better and is probably enough.
Such figure came from a suggestion during the editor pre-review. We are ready to drop it out, if the Editor shares the Referee's view.

p. 4067 l. 6 How do the authors know that scattering is dominated by particles above 4 μm?
Please refer to our response to Referee 1's comment on p.4067, ln.5.

l.21 Panel F of Fig. 2 shows little differences between the raw binning and the use of lognormal fits. I am not sure why it is necessary to come back again to the error related to the binning. If this is kept it should be discussed early in section 2.2.
Basically the whole 2.2 is devoted to discuss the validity of the binning approximation so it is unclear to us where to replace such discussion. About the aim of such a discussion, please refer to what responded to Referee 1's remark on p.4067, ln.5.

p.4068 l. 14 What is the reason for using a factor of two in the number of undetected particles? Why not more or less?
There's no particular reason to double the total particle surface or particle volume in our sensitivity test. It simply gives a gauge to the whole discussion, whose results scales linearly with both parameters. We tried to clarify this by modifying pg. 4068 ln.4 to:

**"To quantify the effects of undetected particles on optical parameters on one side,
and on bulk size distribution parameters on the other, the particle number density *n*
in these additional bins was defined in order to allocate in such bins a volume, or surface, equivalent to those actually detected by the FSSP.
This choice allows to easily extrapolate the effects of an arbitrary amount of undetected particles, just noting how, for particles uniformly distributed in these bins, their contribution to the optical parameters would scale linearly with their number, or volume, or surface."**

l. 21 I am not sure how we must interpret the expected difference by a factor of 2 or 10 in the backscatter coefficient from this study. A difference by a factor of 10 is a real problem to discuss results from the comparison of FSSP with the backscatter sonde. A lower limit of the expected difference would be more appropriate.
As espressed hereabove, and in the manuscript albeit evidently in a confused way, the sensitivity results scales linearly.
The aim of the sensitivity study was to quantify what contribution should possibly be expected from missing particles. A factor 10 disagreement in backscattering would be caused by 100% of undetected volume. Since we do not see a a factor of 10 difference, but it rather stays around a factor 2, we have a good indication that we are not missing small particles for a volume greater than 20% of the detected one.

These aspects are clarified and reaffirmed in the conclusion of our manuscript (see our response to the Referee comments on page p. 4073 hereafter).

p. 4069 l. 25 What is the added value of the JPDF? It is not really used in the discussion of the differences between the two instruments. The scatter plot seems to be the main source of information. Specify what i and j are. Do the authors take the JPDF into account when calculating the linear fit between the bulk parameters N, S, V and the backscatter?
There's no real added value apart from the sake of clarity in the figure. A scatterplot looked more confusing to us, but the linear fit has been performed on the scatterplot. (We note incidentally that a linear fit on the JPDF would have provided the same result although at a higher computational cost).
Since there's no added value but also no harm to use JPDF, we would prefer to keep the JPDF in the figures, unless we receive strong indications against.

p. 4070 l. 13 the AMMA data are probably dispersed because they are sparse. Are there any changes in the results if you remove the AMMA data?
We tried that, and the answer is negative.

l. 21 I am wondering if the right panel of figures 5 brings really new information compared to the scatter plot in the left panel where the linearity problems are already clearly visible.
We acknowledge that the right panel may be redundant. We leave to the Editor to suggest whether to leave or remove that panel.

p.4072 l.1 and l.3 typos "between" and "parameters"
Corrected.

l.5 Not sure how you derive uncertainties of 5 for N from figures (which figures ?). Specify values for S and V.
The number of 5 was a rough estimate from a visual inspection of figures 6, 7, 8. These latter have been updates with the MAS derived backscattering replacing the FSSP derived one. Moreover, a linear fit has been added to the plots, together with a factor 2 tolerance band surrounding it, to facilitate the interpretation of the figures.
The lines (Pg. 4072 ln. 1-4) in our manuscript have been modified as:

"Linear fits can be assessed between the aerosol bulk parameters and optical observations. A linear regression procedure has been applied in order to find the best fitting line passing through the origin. The fit was limited to the central part of the backscattering values, i.e. between 10^-9 and 10^-5 m-1 sr-1, where the linear dependence seemed more robust. Such fits are reported in figures 6, 7 and 8, with a factor 2 band surrounding the fitted line, while the values of the angular coefficients, together with the R-squared of the fit, are reported in Table 2.
By inspecting the figures, the uncertainty to be attributed to the bulk parameters inferred from such fits can be as large as a factor 2 for *N*, at least in the central part of the backscattering variability range, while they increase both for the largest values – this probably due to a lack of linearity in the response of the backscattersonde – and for the smallest values of the backscattering, where the β-N relationship became more scattered.
The uncertainty increases for S and, even more so, for V ."

l.21 Estimated uncertainties are larger for N, but we expect more problems with the relation between backscatter and S or V according to De Reus. This means that we know that FSSP cannot derive S or V. Is it worth then discussing the use of backscatter for an estimate of S or V if we do not start with a good reference?
We agree with the Referee's remarks and we think we have expressed that in pg 4073 ln 4-10 where we discourage the inference of S and V, based on β values. To further underline such point, we have added to our conclusion the following lines (pg.4073 ln.24):

"The results of our study suggest a robust linear relationship between particle concentration and backscattering coefficient. Similarly, relationships may be established also between backscattering and surface area and volume concentrations. However, some caveats must be put on the possibility to use backscattering measurements to infer particle surface or volume concentrations, due to the relatively scarce sensitivity of backscattering techniques for detecting particles above the FSSP

upper detection limit. Hence such relationships should probably be regarded as only providing a lower limit for particle surface or, even more so, volume concentrations."

p. 4073 In the conclusions, say something about (i) the limits of the relation established and (ii) the possible difficulties to use the results for a "true" lidar measurements where extinction terms play also a role.
(i) see hereabove, and (ii) we have discussed that in the response to the Referee's general comment, where we have modified our introduction accordingly (see at the beginning of our response to Referee 2) . We would prefer to not repeat in the conclusion what already exposed there.

Fig. 5 Draw the diagonal of the scatter plot to highlight the non linearities. It is not mentioned in the caption that the JPDF is plotted on the left panel. Exponential are difficult to read in x and y axis.
This has been corrected.

Caption in Fig. 6 to 9. Backscatter sonde data are not plotted on the vertical axis
This has been corrected.

<u>Response to Referee 3</u>

In the general comment, Referee 3 shares one of the remarks of Referee 2, expressing the need to further detail the differences between the backscattersonde measurement and that performed by a lidar. Please refer to what already responded to Referee 2.

Specific comments :

The authors mention that the backscattersonde allows to measure the backscattering coefficient and depolarization ratio at two wavelengths, as with a ground lidar system, but all parameters is not used here to compare to those that can be derived from microphysical measurements. Why? Is it because the authors use the approximation of spherical particles that becomes irrelevant to simulate the depolarization ratio of complex particles ?
That is correct. Please see also our response to Referee 1's last comment.

The term "aerosol" has been changed to "particle" everywhere in the manuscript.

p.4066, l.20 : Explain how are fitted the measured size distribution. What is (are) the constraint(s) ?
On pg. 4066 ln.18 we have added the following lines:
"The Levenberg Marquardt algorithm was used in order to search for the coefficient values that minimize the chi-square. For size distributions with less than six points (number of fit coefficients) initial guesses for the coefficient values are required and have been adapted after visual inspection."

p.4067 and p.4068 : The approximation of spherical particles underestimates the backscattering coefficient and the measurement uncertainties, including the undetected particles, lead to an overestimation of the coefficient. Does this not create a fortuitous error compensation that should be analyzed more precisely in the text ?
There may be such compensation. However we think it is not playing a major role: An expected overestimation from Mie computations might be up to a factor 2, that would be compensated by non-detected small particles accounting for up to 20% of the total particle volume, but to a much higher fraction in terms of total particle number. The fact that the size distribution detected by the FSSP well reproduces the observed backscattering, and a good linear relationship is established between backscattering and particle number concentration, with no significant biases over its range of magnitude, would imply that this compensation, if present, would have the same efficacy over five orders of magnitude from thinner to thicker clouds, irrespective of their different environment and formation processes. This seems unlikely.
These ideas have been reported in our manuscript. However, ours are order of magnitude computations. We do not think we could do better than that.

p.4068, l.3 : To increase of 1 micron the radius of large particles seems to small considering the dimension of large particle observed in cirrus clouds (see for example Baran, JQSRT, 2009).
It IS small. However it gives the upper limit – as we wanted to - for the contribution of large undetected particles to the backscattering coefficient. In fact, keeping the undetected volume constant, the larger the particles, the smaller their effect.

p.4069, l.22 and 23 : "horizontal" and "vertical" are not coherent with the caption of Fig.5. Please check this point.
We do apologize for that. Captions and axis titles are now coherent.

p.4071, l.5 : Are Fig. 6-9 for a typical observation or all of the observations ?
They represent the whole dataset.

p.4071 and Fig. 6-9 : The text states that it shows the measured backscattering coefficient while the figures show the calculated one (from FSSP). It is a major mistake that you must check and correct - if it is a mistake - because it is not really surprising that the calculated coefficient is linearly related to the other bulk microphysical parameters.
The Referee is right.
Since we proved the correspondence between MAS and FSSP optical coefficients, we allowed ourselves to plot the relationships between the FSSP optical coefficient and the other bulk microphysical coefficients. But it is true that the MEASURED backscattering comes from the backscattersonde, so we agree is probably more correct to plot the relationship between the latter and the FSSP bulk microphysical coefficients. So we have redone the plots as the Referee suggested. As should be expected, the plots are more scattered.
Note that the use of MEASURED backscattering data has also changed the parameters of the linear fits, that have been changed accordingly in table 2.

p.4071, l.19 : ". . .although more scattered". It is not clear on the figures.
Now – after the substitution of FSSP with MAS backscattering coefficients - this can be seen clearly.

p.4072, l.7 : Authors should also argue "to what extend measurements with backscattersonde are similar or close to that performed with a ground-based or airborne lidar system ?"
Please see our response to the general comments of referee 2.

Technical corrections
p.4061, l.18 : "peak at 10 microns,. . .", please precise radius or diameter
p.4061, l.22 : same remark
p.4065, l.21 : typo "variabilities"
p.4067, l.25 : 1.35 microns and 15.5 microns, . . . add "radius" in order to be coherent with table 1.
p.4069, l.4 : change 0.85 by 0.35
p.4070, l.13 : typo "small"
p.4072, l.3 : typo "parameters"
C2039
p.4072, l.7 : typo "answer"
p.4079, l.1 : typo "cm-3"
p.4080, l.2 : typo "expressed"
p.4081 : characters in figure panels are too small
p.4082, Fig. 2 : indicate the labels (A-F) on each diagram.
p.4085 : complete figure caption to explain right part.
All these correction have been implemented in the revised manuscript.

<u>Additional corrections we want to make</u>

On page 4072 ln14: CIP size range is quoted to be between 26 µm and 1.5 mm. Actually it's 25µm and 1.6mm.

On page 4070 ln11-26 (This could be a possible add in line 17 or 18)
"Also, during SCOUT-AMMA very thick MCS anvil clouds have been observed and there, a different shattering effect might have contributed to the FSSP data set compared to the data sets from the other campaigns."