## Response to Review 1

The reviewer is thanked for their general comments and careful reading of the manuscript. In our opinion, we have addressed the major concerns of this reviewer in a revised version of the paper. A point-by-point reply to each statement made by reviewer is provided below.

*1a). How does the capability for PARASOL to discriminate different crystal distortion (or habit in general) depend on the cloud optical thickness? As discussed below in more detail, directionality of the reflection should be reduced by increasing contributions of multiple scattering, i.e., increasing optical thickness. I would expect results to depend on optical thickness.*

**Reply:** This comment was recently addressed by Zhang et al. (2009) on page 7123 **[**Zhang, Z., Yang P., Kattawar G. W, Reidi J., Labonnote L.-C., Baum B. A, Platnick S., Huang H.-L.: Influence of ice particle model on satellite ice cloud retrieval: lessons learned from MODIS and POLDER cloud product comparison. Atmos. Chem. Phys., 9, 7115-7129, 2009]. In the paper, it is physically explained why there is still observed structure in the reflected light at top-of-the-atmosphere (TOA) even at large values of cloud optical depth. They explain that the shape of the phase function is still retained at TOA due to scattering in the cloud being dominated by forward scattering, due to ice crystals having a well known very strong diffraction peak at those scattering angles. This single-scattering information is still retained at large values of cloud optical depth. This point is illustrated by the two figures below, which contrasts the reflected light at TOA, at cloud optical depths from 0 to 5, with the same but at cloud optical depths from 35 to 40. In the figures, the hexagonal non-randomized hexagonal column phase function is assumed. The purple line in the figures shows the best-fit straight line through the data, and the correlation coefficient, in the case of the first figure is 0.09 and in the second it is 0.26. Clearly, in both cases, the correlation coefficient is very low and so clearly the shape of the phase function is still retained to allow discrimination against pristine phase functions, even at cloud optical depths of 35 to 40.

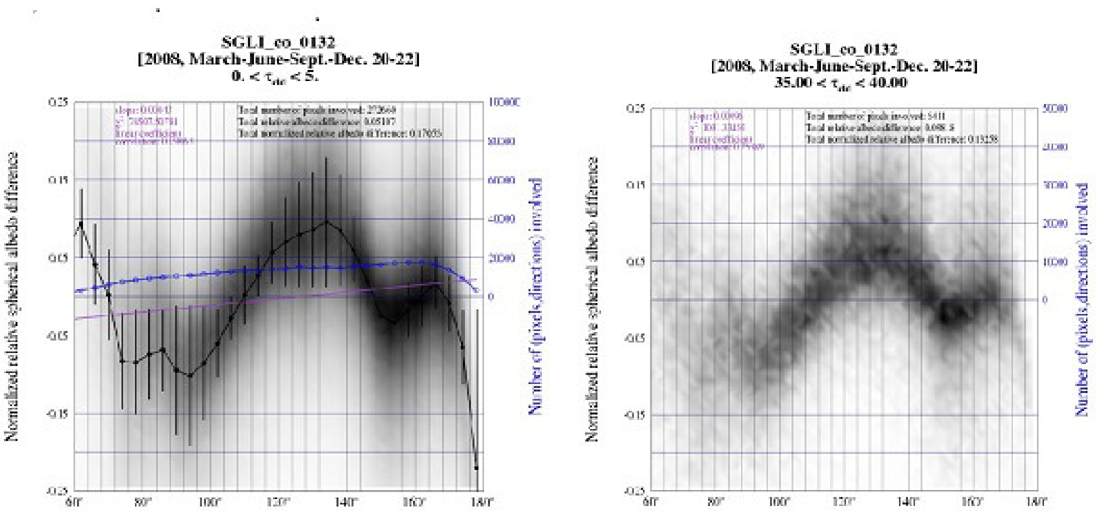


Figure 1.

As an explanation has already been given by Zhang et al. (2009) we do not show the above figure in the revised paper but cite Zhang et al. (2009) instead. Stating the following in the revised paper:

“The physical reason for this was recently given by Zhang et al. (2009). In the paper, they physically argued that even if the optical thickness is increased to large values, the shape of the phase function is still retained at top-of-the-atmosphere. This is because scattering within the cloud is dominated by forward scattering, which results from strong diffraction in the forward direction (Macke et al. 1995), and this single-scattering information is still retained in the presence of strong multiple scattering.”

*1b) How does this capability vary with the angular range and sampling? The phase functions in Figure 4 show only minimal differences at the sampled scattering angle range of 80-130 degrees. I would expect the technique to be better suited for pixels sampling between, e.g., 120-170 degrees.*

**Reply:** We agree with the reviewer, Figure 4 by itself is not sufficiently clear to show that discrimination between randomizations is possible using the range of scattering angles presented in the paper. To clarify this further, we introduce a Fig. 4b, which shows the ratio of randomized scattering phase function to the pristine phase function plotted at scattering angles from 50$^{o}$ to 150$^{o}$. This figure is re-produced below:

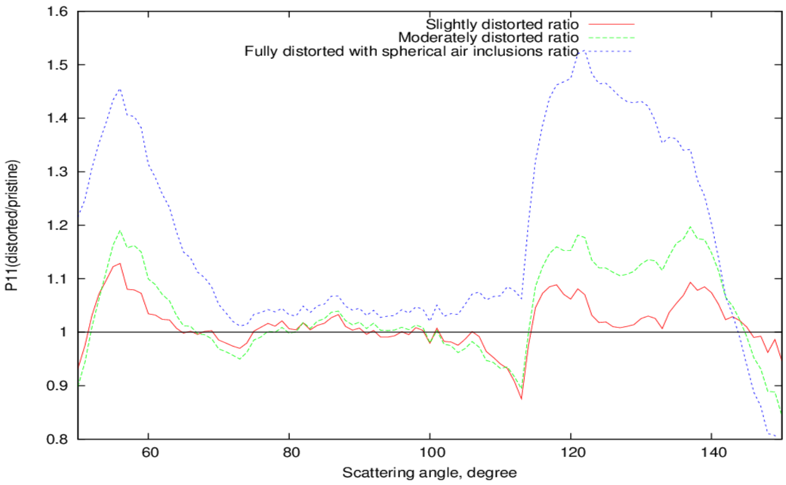


Figure 2.

At the back scattering angles relevant to this paper, the most randomized phase function ratio increases by 50% relative to the pristine case, and the least randomized is increased by up to about 10%. At scattering angles between 80$^{o}$ to 100$^{o}$, the ratio of most randomized phase function is still increased by up to 10%. Whilst the lesser randomized phase functions would be more difficult to discriminate against if these were the only scattering angles available. The paper therefore relies on the discrimination at the back scattering angles of up to 130$^{o}$, which from the figure is possible.

In the revised version of the paper Figure 2 above is described as follows:

“To demonstrate the feasibility of PARASOL to discriminate between the different ensemble model randomizations, Fig. 4 (b) shows the ratio of the randomized to the pristine phase functions plotted against scattering angle. Figure 4 (b) shows that at scattering angles between about 80$^{o}$ to 130$^{o}$, the ratio between the most randomized and pristine phase functions can reach values of about 1.1 to 1.5. At the distortion value of 0.15 (slightly distorted), and at scattering angles greater than 115$^{o}$, the ratio can still reach values of 1.1. However, at scattering angles between about 80$^{o}$ to 100$^{o}$, the values of the ratio between the pristine and slightly distorted, and moderately distorted phase functions are only slightly greater than unity, which means that discrimination between those models may not be possible at those particular scattering angles. However, due to the increasing values of the ratio at scattering angles between approximately 100° to 125°, it should be possible to discriminate between models on a pixel-by-pixel basis at those particular scattering angles."

*1c) Is the use of a single habit mixture with only varying distortion sufficient? The scattering phase function not only depends on distortion, but also on habit (Um and Mc-Farquhar, 2007, 2009; Macke et al., 1996a; Yang and Liou 1998). Here only a single selection of habits and a single PSD is used. The ensemble model might generally fit in situ volume extinction measurements, but that does not constrain the scattering phase function. Furthermore, a very large variation of habits is possible in natural clouds. The dependence of the scattering phase function on crystal shape and the implications for the method needs to be discussed. For example, the ensemble model consists of columns, bullet rosettes and aggregates of columns, but what if a real cloud contains mainly thin plates or columns with very different aspect ratios than used in the model?*
*References:*
*- Um, Junshik, Greg M. McFarquhar, 2007: Single-Scattering Properties of Aggregates of Bullet Rosettes in Cirrus. J. Appl. Meteor. Climatol., 46, 757–775.*

**Reply:** The scattering phase function depends on habit if the ice crystal exhibiting that phase function is idealized, meaning without distortion or randomization applied to that ice crystal. The optical features exhibited by ice crystals of an idealized hexagonal nature will remain the same, such halos, ice bows, scattering peaks and depressions, see the figure below taken from Baran (2009), which is composed of different crystal shapes, but in the case of columns and plates the aspect ratios vary across the particle size distribution according to Auer and Veal (1970).

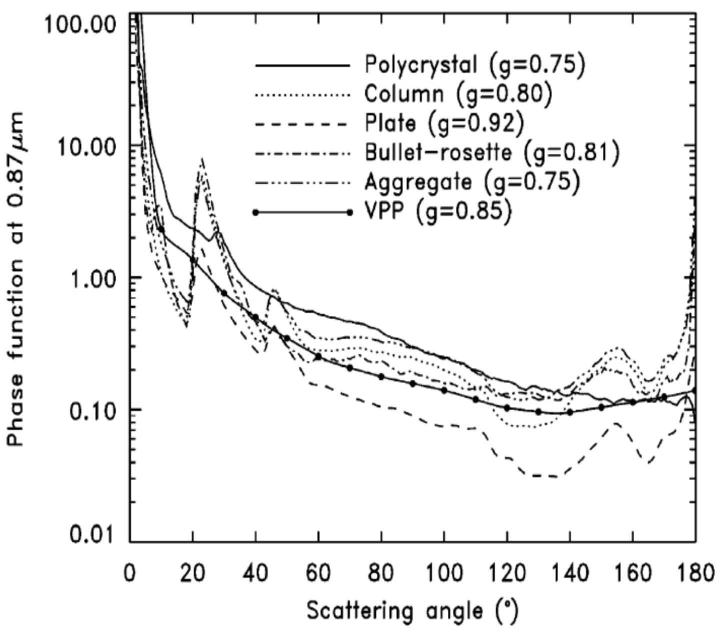


Figure 3. (Baran 2009, Figure 4).

However, the positions of optical features, shown in Figure 3 above, may be altered, but are not removed outside the backscattering angle range used in this paper. Therefore, this paper is not about which particular aspect ratio or shape, but whether the cloud exhibits optical features not explained by completely randomized phase functions. This may not mean that the pristine (i.e., non-randomized) ensemble phase function is a perfect fit to the measurements but the fit is nonetheless better than a completely featureless phase function. Examples of this are given further below in answer to another question posed by this reviewer. As for aggregating ice crystals, as shown by Westbrook et al. (2004), the monomers making up those crystals tend to be well separated so that multiple scattering between monomers become unimportant, and so the optical features remain in almost the same scattering angle positions (see, for example, Figs 5 and 6 of Baran, 2009). Moreover, this fundamental aggregation process is independent of assumptions about the initial shape of the monomer (Westbrook et al. 2004). The reviewer refers to Um and McFarquhar 2007 and 2009. Those papers show that in the case of compact ice aggregation, the phase functions are shifted to higher positions, but the positions of the optical features are hardly changed. Both of these papers have now been cited in the revised paper, which now states the following:

"Of course, the phase functions derived from the ensemble model shown in Fig. 4 may not cover the entire range of possible cirrus phase functions as there are many possible cirrus habits that might occur at particular environmental temperatures (see, for example, Baran 2012 and references therein). However, in the case of aggregates of hexagonal plates or hexagonal columns, it was shown by Baran (2009), using the ice aggregation model of Westbrook et al. (2004), that after three monomers were attached to the ice aggregate the asymmetry parameters and phase functions asymptote to their limiting values. This asymptote occurs because the ice aggregation model predicts that the ice monomers making up the ice aggregate are well separated from each other. This separation is sufficient to reduce the effects of multiple scattering on the phase function, resulting in only slight modifications to the scattering angle positions of optical features (see, for example, Fig. 5 and Fig. 6 of Baran 2009). This aggregation process is fundamental, and the same behaviour would be observed independent of the shape of the initial monomer (Westbrook et al. 2004). Therefore, in the case of pristine aggregates, the position of optical features on the phase functions would not be expected to be fundamentally different to those shown in Fig. 4a. If, on the other hand, the monomers that make up the ice crystal aggregate are sufficiently close to each other, by arbitrarily attaching them, then multiple scattering between monomers becomes important, as the scattered energy is increased and so therefore is the phase function. However, the positions of the optical features exhibited by the ice aggregate phase functions do not significantly change position with respect to their scattering angles as these are principally determined by the hexagonal geometry (Um and McFarquhar 2007; 2009). As discussed in the introduction to this paper, the observational evidence indicates that pristine ice crystals are a rarity in nature and so the phase functions of highly complex ice crystals exhibiting inclusions, cavities and surface roughness will produce featureless phase functions and the featureless nature of the phase function is invariant with respect to ice crystal habit."

*2) As stated in the paper, "this paper reports a positive correlation between the scattering phase function and RHi." This conclusion is based on 12 pixels in total, which in my opinion is a too small number to justify any conclusions. Furthermore, low RH values are also associated with pixels indicating severely distorted crystals. The fact that the 12 pixels with less distorted crystals are 1) on the edge of the area were the data was within the selection criteria and 2) their adjacency of null-results to the 12 pixels with less distorted crystals also raises concerns. For example, could contamination of lower lying liquid water clouds be excluded? Finally, as further argued below, the criteria for the null-results are not given, while these remove the majority of the field with low RH from the analysis. Only by inclusion of more data and more robust statistics the present conclusions could be reached.*

**Reply:** The paper presents one case of cirrus. There were other cases during the "Constrain" observations campaign. However, these other cases were unsuitable. This is because the take off time was not co-incident with the PARASOL overpass, the cloud was not cirrus, or there were many cases of underlying broken cloud or underlying sheets of cloud, or the cloud was optically too thick in the case of flying around the Chilbolton 94GHz cloud radar. To address this point we state the following in revised paper:

"In this paper, one case from the Constrain field programme is presented. The conditions required for this paper are that the cirrus should be sufficiently optically thick to allow discrimination between various randomizations of the ensemble model using PARASOL retrievals, the aircraft and satellite should be co-incident, and there should be no underlying cloud or broken cloud fields. It is practically very difficult to obtain all these necessary conditions at the same time. There were several other Constrain cases but these did not meet the conditions necessary for this paper. This is because the other cases were either optically too thick, as these cases were associated with radar reflectivity studies, or there was no co-incidence between PARASOL and the aircraft. Furthermore, the condition of no substantial underlying cloud was not met, and the cloud studied was not cirrus, the other cloud types studied were either altostratus or stratocumulus."

The null results have now been defined in the revised paper as follows:

"The reason for the null results at those pixels is because the retrieved spherical albedo at each of the scattering angles was the same for all ensemble models. The similarity of retrieved results in the null cases is because the retrieval conditions stated in section 4 were not met."

The reviewer requested a figure showing the PARASOL measurements plotted against the scattering angle. This new figure is shown below as Figure 8, shown in answer to one of the reviewers other points. If sheets of water cloud were underneath the cirrus, then the fits to the spherical albedo differences would have been much worse than is shown in Figure 8 below. This is because the scattering phase function of water spheres is orders of magnitude lower than non-spherical particles at scattering angles between $80^{o}$ to about $105^{o}$, which is in the region of Alexander's dark band. In this scattering angle region non-spherical particles would be much more divergent from the zero line, if the scattering phase function of the sphere was better suited. The reason for the proximity of the null results to the phase functions exhibiting optical features could be due to the location of broken cloud fields below the null pixels. This is shown by the lidar image shown in figure 4a below, which shows the range corrected lidar data. In this figure, it can be seen that there are broken cloud fields below the cirrus at altitudes less than 2 km, at the times around 13.2 hrs, and at 13.8 hrs. The null pixel results are likely to correspond more to the underlying cloud cases at around the times of 13.2 hrs to 13.30 hrs when the aircraft flew over those pixels. The aircraft track as a function of time is shown in Figure 4b below. The non-null pixels are more associated with no underlying cloud at the times after 13.35 hrs. Although these times do not coincide exactly with the PARASOL overpass, the figure does show that there was underlying broken cloud in the vicinity of the null results, even though the cirrus was semi-transparent. The revised version of the paper now includes the possibility that the null results in the region mentioned by the reviewer are more likely to be associated with underlying water cloud. The following is stated in the revised version of the paper:

“Further analysis of lidar data, not shown here for reasons of brevity, shows that the null results adjacent to the pixels associated with structure in the phase function are likely to be associated with broken cloud fields in the boundary layer.”

(a)

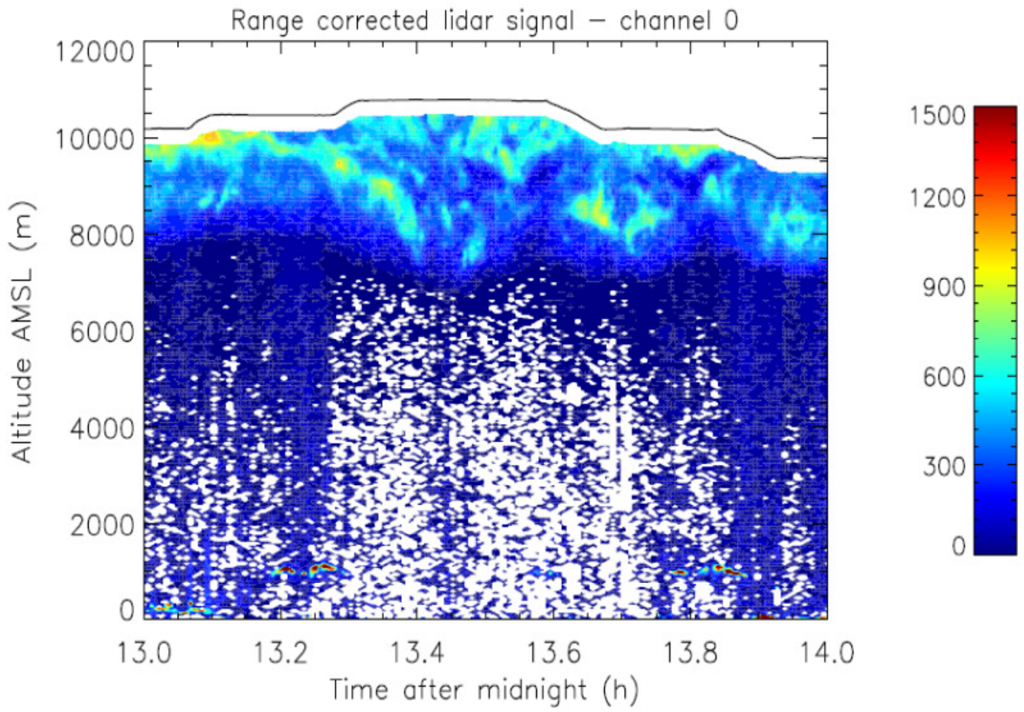


Figure 4a

(b)

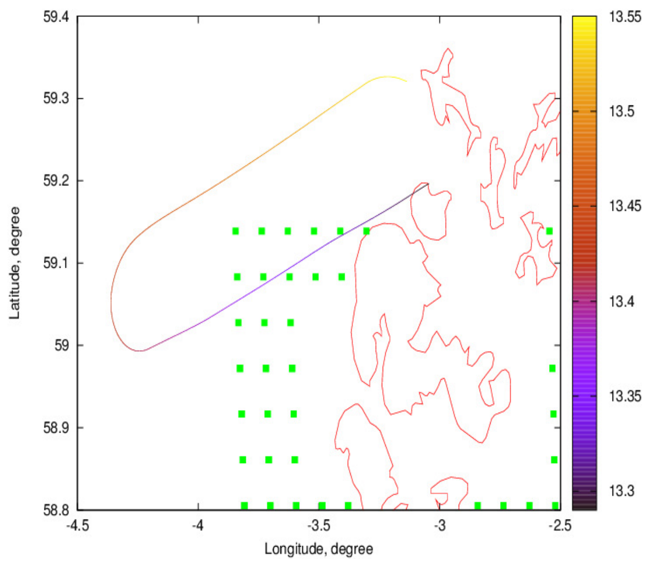


Figure 4b

With regard to there being too few data points to reach conclusions. For the reasons stated above we could only use one case from the complete Constrain observations program. We do not actually specify conclusions, we merely report that for the one case that did meet the specified conditions there is the possibility of a correlation between $RH_i$ and shape of the scattering phase function. We are of the opinion that this is an interesting result alone and the paper should encourage colleagues to investigate whether such relationships do generally exist. As pointed out by the second reviewer, if such a relationship does exist then this has rather important implications for the parameterization of the asymmetry parameter in global climate models. The resources required to produce this work are significant and can only be achieved through international collaboration, which is why we wish to point this finding out to other colleagues so they too can investigate this finding using global data and to also think of combining NWP modelling, satellite and aircraft data in the way that is presented in this paper. We also wish to encourage colleagues to think more about relationships between fundamental ice scattering properties and the state of the atmosphere in which the ice resides. This paper encourages that link to be further investigated. The paper also shows that using new cirrus microphysics obtained from the Constrain data to parametrize the cloud scheme used in the NWP model resulted in a good NWP prediction of the cirrus compared to lidar, retrievals and measurements. This result is also worth pointing out to colleagues that well chosen cirrus microphysics in an NWP model at high-resolution can produce realistic cloudy humidity fields.

*Specific comments:*

*Page 14111, second paragraph: I suggest also to include the new results by Magee et al. (ACPD, 2014) in the discussion: Magee et al., "Mesoscopic surface roughness of ice crystals pervasive across a wide range of ice crystal conditions", Atmos. Chem. Phys. Discuss., 14, 8393-8418, 2014.*

**Reply: Done.**

*Page 14112, line 29: The reduction of the halo features by increasing roughness was recently shown by Van Diedenhoven (2014). That paper also explores the presence of halo features in mixtures of rough and pristine ice crystals, which may be relevant for the discussion on page 14113.*
*Reference:*
*Van Diedenhoven, B., 2014: The prevalence of the 22_ halo in cirrus clouds. J. Quant. Spectrosc. Radiat. Transfer, in press, doi:10.1016/j.jqsrt.2014.01.012.*

**Reply: Done.**

*Page 14113, line 28: Please also include the recent paper by Cole et al.: Ice particle habit and surface roughness derived from PARASOL polarization measurements, Atmos. Chem.Phys., 14, 3739-3750, doi:10.5194/acp-14-3739-2014, 2014.ole, B. H., Yang, P., Baum, B. A., Riedi, J., and C.-Labonnote, L.*

**Reply: Done.**

*Page 14117, line 1: Please mark the location of the used data (i.e. where the aircraft was above cloud) on the map in figure 6. Were there lidar measurements at the location at which the retrievals suggest pristine crystals?*

**Reply:** Done. The figure is reproduced below so that the reviewer can see that the location, marked by X in Figure 5, was outside of the area covered by the PARASOL pixels.

Figure 5.

*Page 14119: paragraph 2 and further: In my opinion, the discussion about the comparison of the area ratio of the used ice model and in situ measurements is out of scope of this paper. The paper aims to relate the scattering phase function with variation of RH. The area ratio is not uniquely related to the scattering phase function, which is mainly determined by the overall shape of the crystals, the aspect ratios of their components and the level of distortion. As the authors already noted, the capability of the ensemble model to replicate in situ estimates of volume extinction and other cirrus properties is already demonstrated in several papers (Baran et al., 2009, 2011a, 2014a). I suggest removing this part of the paper, including figure 3.*

**Reply:** We disagree with this, as it is important to show that the ensemble model predicts area ratios that are consistent with microphysical observations. This is because as ice aggregates elongate with ice crystal size, the elongation has important consequences for the scattering phase function and asymmetry parameter and as such Figure 3 will remain. The other references cited by the reviewer are mostly about the volume extinction coefficient.

*Page 14122, line 27: Please indicate here which definition of distortion parameter is used. Is this using the uniform distribution of Macke et al. (1996)?*

**Reply:** Yes it is, and this has been stated in the revised paper.

*Page 14125, line 12: I believe "total reflectance" should be "spectral albedo" here.*

**Reply:** Thank you. This has been changed in the revised paper to "spectral albedo".

*Page 14125, line 15: What are the assumptions for the aerosol and are they realistic for this particular dataset?*

**Reply:** The aerosol assumed in the PARASOL global product has previously been described by Buriez et al. (2005) [Buriez J.-C., Parol F., Cornet C., and Doutriaux-Boucher M.: An improved derivation of the top-of-atmosphere albedo from POLDER/ADEOS-2: Narrowband albedos. J. Geophys. Res., 110, DOI: 10.1029/2004JD005243, 2005]. The aerosol is maritime-based and the optical depth of aerosol of this type will be very small compared to the cirrus optical depth and is confined to the boundary layer as shown in the lidar image below. The lidar image shown in Figure 6 below was obtained in clear air with no cloud beneath the aircraft, and the aerosol optical depth in the boundary layer is estimated to be ~0.05. This value being much lower than the PARASOL retrieved cirrus optical depths, which were generally 1 or greater.

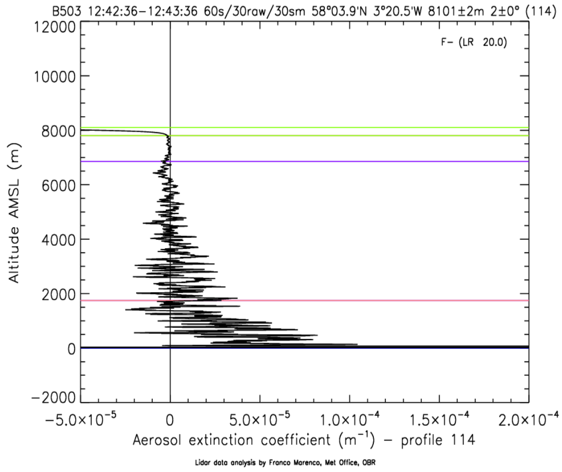


Figure 6.

In the revised paper the following is stated about the assumed aerosol.

"The aerosol model assumed in the PARASOL retrieval has been previously described by Buriez et al. (2005), and so a description will not be repeated here. However, the aerosol is principally maritime-based and so its optical depth will be much smaller than the cirrus optical depth, and as such, it will not be of any significance for the purposes of this paper."

Buriez J.-C., Parol F., Cornet C., and Doutriaux-Boucher M.: An improved derivation of the top-of-atmosphere albedo from POLDER/ADEOS-2: Narrowband albedos. J. Geophys. Res., 110, DOI: 10.1029/2004JD005243, 2005.

*Page 14125, line 19: I assume a Cox and Munck model is used. Please add the reference. Is the reflectance value of 0.000612 an addition to the reflectance predicted by the Cox and Munck model? These details are not given in the Buriez et al. (2001) paper.*

**Reply:** Unfortunately the Buriez et al. (2001) was an incorrect citation. It should have been Buriez et al. (2005) as above. The reflectance value cited can be derived from Appendix A in Buriez et al. (2005). We do not go through the derivation of the reflectance value in the paper but state it here for the benefit of the reviewer. The reflectance value of 0.000612 is the foam contribution (outside of the glint). Found in Appendix A is the general expression for the sea surface reflectance and in that expression at 865 nm the Runderlight is assumed to be 0 and outside of glint Rspecular=0. Therefore, the only contribution is the foam, given by the following expression:

Wfoam(V)*Rfoam

Where V is the wind speed assumed to be 7 m/s and Rfoam=0.22, and Wfoam = 0.00000295*V^3.52, Which gives Rsurface= 0.000612...

In the revised paper the following is stated:

"At the wavelength of 0.865 µm, the PARASOL retrieval algorithm assumes that the sea surface has a reflectance value of 0.000612 (the foam contribution outside of sun glint) and a wind speed of 7 $ms^{-1}$. See Appendix A in Buriez et al. (2005) for a detailed derivation of the assumed PARASOL sea surface reflectance value."

*Page 14124, line 4: Please note that aerosol scattering, Rayleigh scattering and glint on the ocean surface also add to directional variation of measured reflectance.*

**Reply:** This has been noted and in the revised version of the paper and the following is stated:

"The scattering by aerosol and Ocean glint all contribute to the directional variation of the retrieved cloud optical depth, and these effects are taken into account in the PARASOL retrieval algorithm."

*Page 14126, line 15 and line 27: Please note that the directional dependence can also be caused by inhomogeneity in the cloud, as discussed by Buriez et al. (2001). Strictlyspeaking, the assumption of a perfectly homogeneous cloud is unphysical in itself, so please rephrase these sentences.*

**Reply:** This point has been noted in the revised paper and reads as follows:

"However, inhomogeneity in the cloud can also affect the directional reflection as shown by Buriez et al. (2001), but this effect is not currently accounted for in the PARASOL retrieval algorithm due to its highly variable nature."

*Page 14127, line 3: I would expect that the level of directional variability of cloud reflectance depends also on the cloud optical thickness. For optically thin clouds, single scattering is contributing significantly to the total reflectance and I would expect the shape of the phase function to be of greater importance here. Directionality should be reduced by increasing contributions of multiple scattering. The importance of the optical thickness of the cloud on the analysis should be discussed here.*

**Reply:** This point has already been addressed in answer to 1(a) above.

*Page 14130, line 15: The figure shows retrievals over land, although one of the selection conditions was for the measurements to be over ocean, which is also consistent with the radiative transfer model. Please clarify and remove the land pixels.*

**Reply:** The land pixels have been removed from the figure to be consistent with the radiative transfer model.

*Page 14130, line 15: It would be very illustrative to add a plot to the paper showing the retrieved optical depth. How do the retrieval results correlate with optical thickness?*

**Reply:** Agreed, a figure has been included of the retrieved optical thickness and this figure is shown below for the benefit of the reviewer, alongside the estimated cirrus randomization at each pixel.

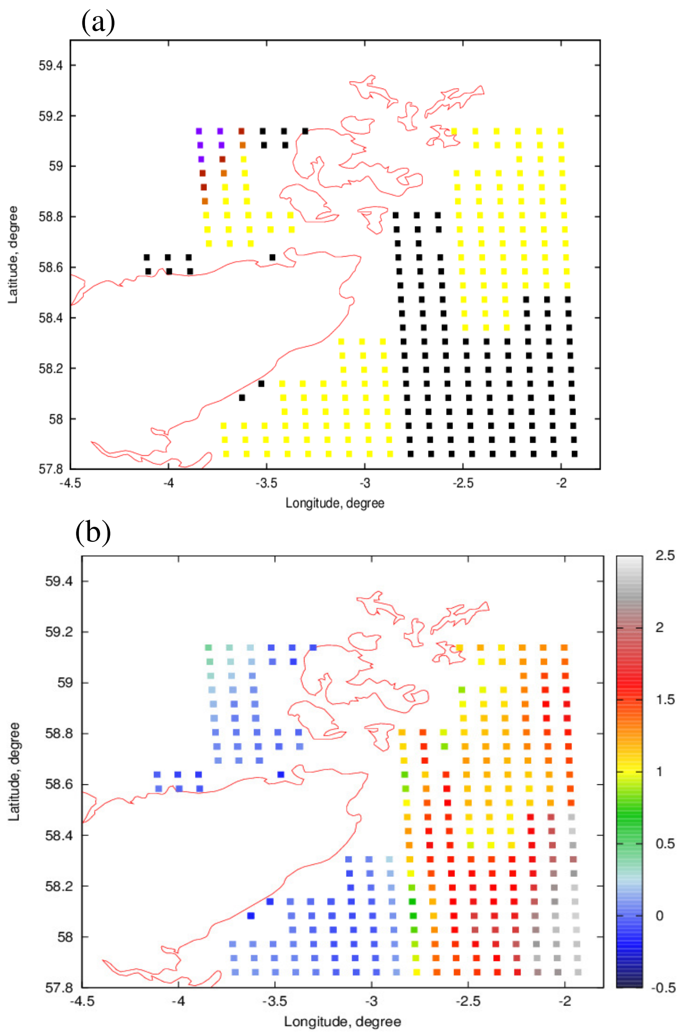


Figure 7.

Some of the pixels showing null results shown in Fig. 7(a) are associated with the largest retrieved optical thicknesses shown in Fig. 7 (b), the optical thicknesses shown in 7(b) are plotted as the decadal logarithm.

In the revised paper the above figures are described as follows:

"Moreover, Fig. 9 (b) shows the averaged retrieved PARASOL decadal optical thickness (averaged over all available scattering angles) at each of the pixels shown in Fig. 9 (a). The figure shows that the retrieved PARASOL optical thickness ranged between less than 1 and up to about 250. The largest optical thicknesses retrieved by PARASOL are associated with the broken frontal cloud shown in Fig. 1 (right-hand side of the figure), and the positions of the broken frontal cloud fields are also predominantly associated with the positions of the null results shown in Fig. 9 (a)."

*Page 14130, line 16: It is stated that 190 pixels contained no discrimination between ice models. What criteria are used here to define "no discrimination" and what is the basis for these criteria? The method seeks the lowest rmse (Eq. 5) produced by the different models and theoretically one model should lead to the lowest rsme. It is extremely unlikely that two or more models yield exactly the same rsme. Please clarify.*

**Reply:** The no discrimination between models means the null result and the definition of a null result was previously explained in answer to 2 above. The text in the revised paper has been clarified and the term "no discrimination" replaced by "null result" with the definition of null result immediately following.

*Page 14130, line 21: What are the indications for multi-layered clouds? Please show that the results are not affected by multi-layered clouds. The pixels with more pristine crystals are adjacent to pixels with null-results, which raises concerns about possible contamination.*

**Reply:** The explanation for this is given in answer to question 2 above.

*Page 14131, line 8: Please show differences in PARASOL measurements in the region with pristine particles and those in regions with distorted crystals to illustrate the difference in backscattering features.*

**Reply:** This is a good suggestion and to answer this point we have included a new figure 10 (a) and 10 (b). The new figure is shown below (as Figure 8), and this shows for two pixels, the differences between the averaged directional and directional spherical albedos as a function of scattering angle. The two pixels chosen were from the "pristine" and "fully randomized" regions.

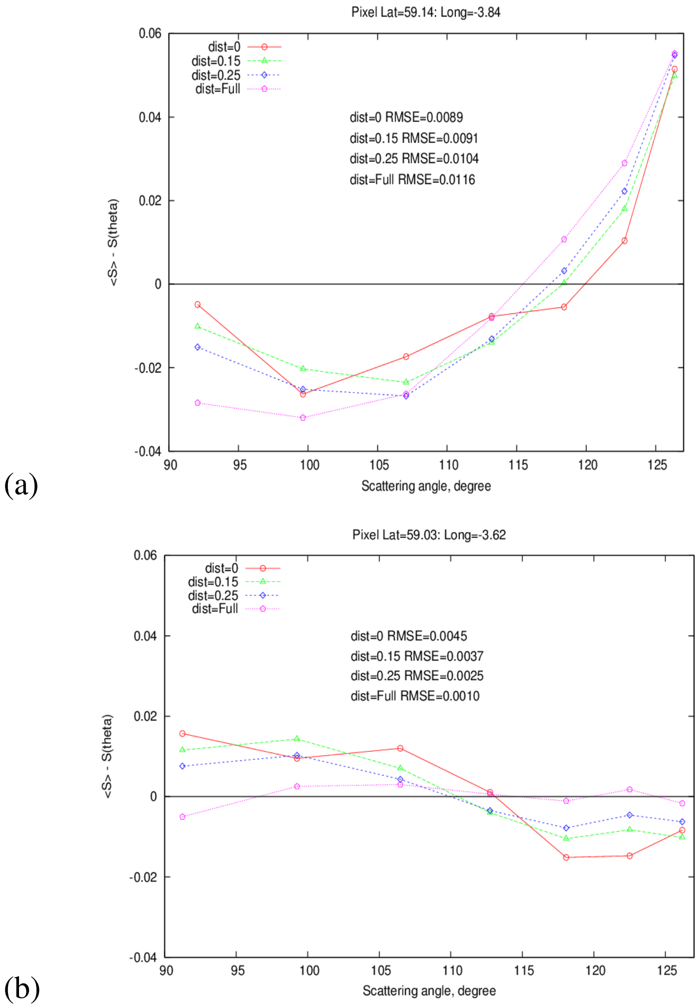


Figure 8.

In both figures the key that defines the symbols is shown in the top left-hand side. Where the pristine ensemble model (dist=0), are open red circles; the slightly distorted model (dist=0.15), green filled triangles; the moderately distorted model (dist=0.25), open blue diamonds; and the fully randomized model (dist=0.4 with spherical air bubble inclusions), open purple pentagons. For each model the RMSE value is embedded in the figures. Figure 8 (Figure 10 (a) and (b) in the revised paper) is described by the following text in the revised paper:

"The figure shows the spherical albedo differences plotted as a function of scattering angle for each of the two pixels, and in each of the figures, the rmse values are shown that were derived from the spherical albedo differences assuming the four models. The first pixel shown in Fig. 10 (a) is located at latitude 59.14$^{o}$ and longitude -3.84$^{o}$, and this pixel is associated with the pristine model phase function, since that had the lowest rmse value out of all the models. However, at some scattering angles, such as at 99$^{o}$ and 113$^{o}$, the pristine phase functions predicted spherical albedo differences similar to the moderately and fully randomized phase functions, respectively. At the scattering angles of 92$^{o}$, 107$^{o}$, and 123$^{o}$, the pristine phase function is closer to the zero line than the other models, but over all the rmse found for the pristine model is lower than the other models. It is interesting to note from the figure that at the backscattering angles, between about 90$^{o}$ to 100$^{o}$, and 117$^{o}$ to 125$^{o}$, the most randomized phase function is furthest from the zero line, and the rmse value found for the most randomized model is 31% higher than the value found for the pristine model. This finding is consistent with Fig. 4 (b)[**note this is Figure 2 above**], which showed that the greatest divergence between the fully randomized and pristine model phase functions occurred at the latter scattering angles shown in Fig. 10 (a). This figure shows that although the pristine model phase function may not be a perfect fit to the PARASOL data, it does, however, demonstrate that the more featureless the phase function becomes, the fit to the PARASOL data becomes progressively worse at the latter scattering angles.

In contrast to Fig. 10 (a), Fig. 10 (b) shows the same but choosing a pixel, located at latitude 59.03$^{o}$ and longitude -3.62$^{o}$, which is associated with the fully randomised model phase function. It can be seen from the figure, that in this case, the spherical albedo differences predicted by the fully randomized phase function are much closer to the zero line than the other models for all scattering angles considered. In this case, the rmse value found for the fully randomized model is a factor of 4.6 smaller than the value of the rmse found for the pristine model. Figure 10 (b) shows that when the most randomized model phase function is the best representation of the spherical albedo differences, the discrimination between models is at its strongest. In Fig. 10 (a), ensemble models exhibiting optical features on their phase functions generally better minimised spherical albedo differences better than completely featureless phase functions. Other examples are not shown here, but the results are similar to the cases shown in Figs. 10 (a) and 10 (b), and are not re-produced here for reasons of brevity."

*Page 14131, line 16: If I understand correctly, the PN measures over nearly the full scattering angle range, so please clarify the remark about the need "in situ observations to sample the scattered angular intensities over a more complete range of scattering angle than is currently possible".*

**Reply:** The scattering angle measurement range of the PN is currently between 15$^{o}$ and 162$^{o}$ and this range was stated on page 14114 on line 3. This range cannot be regarded as "nearly full" as the phase function at scattering angles between 162$^{o}$ to 180$^{o}$ is still not understood. This is why the statement is contained in the paper and the phase function described by Baran et al. (2012) is given as an example of this inadequacy [ Baran A. J., Gayet J-F., Shcherbakov V.: On the interpretation of an unusual in-situ measured ice crystal scattering phase function. Atmos. Chem. Phys., 12, 9355-9364, 2012].

*Page 14132, line 15 and figure 10: In my opinion, the arbitrary x-axis is not very illustrative. Why not use the distortion value itself?*

**Reply:** Agreed, the distortion values are now used in the revised figure and the figure is shown below for the benefit of the reviewer.

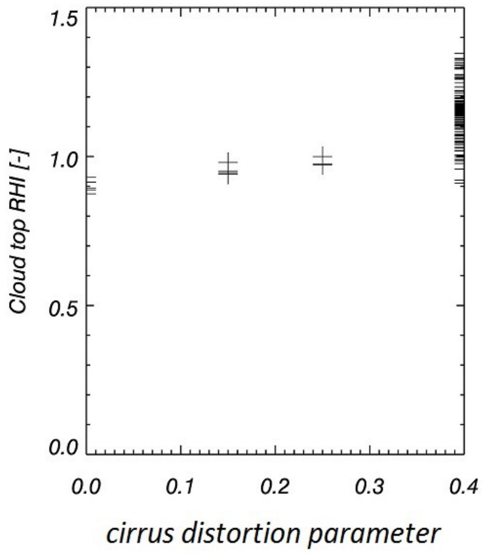


Figure 9.

*Page 14133, line 1 and further, figure 11, and appendix A: I am puzzled by these figures and their explanation. At 865 nm, where ice is essentially non-absorbing, most light will penetrate through the whole cloud for cloud optical depths below about 8-12, where the reflectance is below 50*
*References:*
*- Liou, 2002, An Introduction to Atmospheric Radiation, book, ISBN: 978-0-12-451451-5*
*- V.V. Rozanov, A.A. Kokhanovsky, The average number of photon scattering events in vertically inhomogeneous atmospheres, Journal of Quantitative Spectroscopy and Radiative Transfer, Volume 96, Issue 1, 15 November 2005, Pages 11-33, ISSN 0022-4073, [http://dx.doi.org/10.1016/j.jqsrt.2004.12.026](http://dx.doi.org/10.1016/j.jqsrt.2004.12.026).*

**Reply:** The previous figures were not sufficiently clear and they were the cumulated probability as a function of distance from the cloud-top. This is why the probabilities were zero at the cloud-bottom irrespective of optical thickness. However, these figures have now been replaced by the % probability of penetration at 0.865 µm, which is defined as the last position (distance from the cloud-top) of the photon before leaving the cloud to reach the sensor. The revised figures are shown below for the benefit of the reviewer.

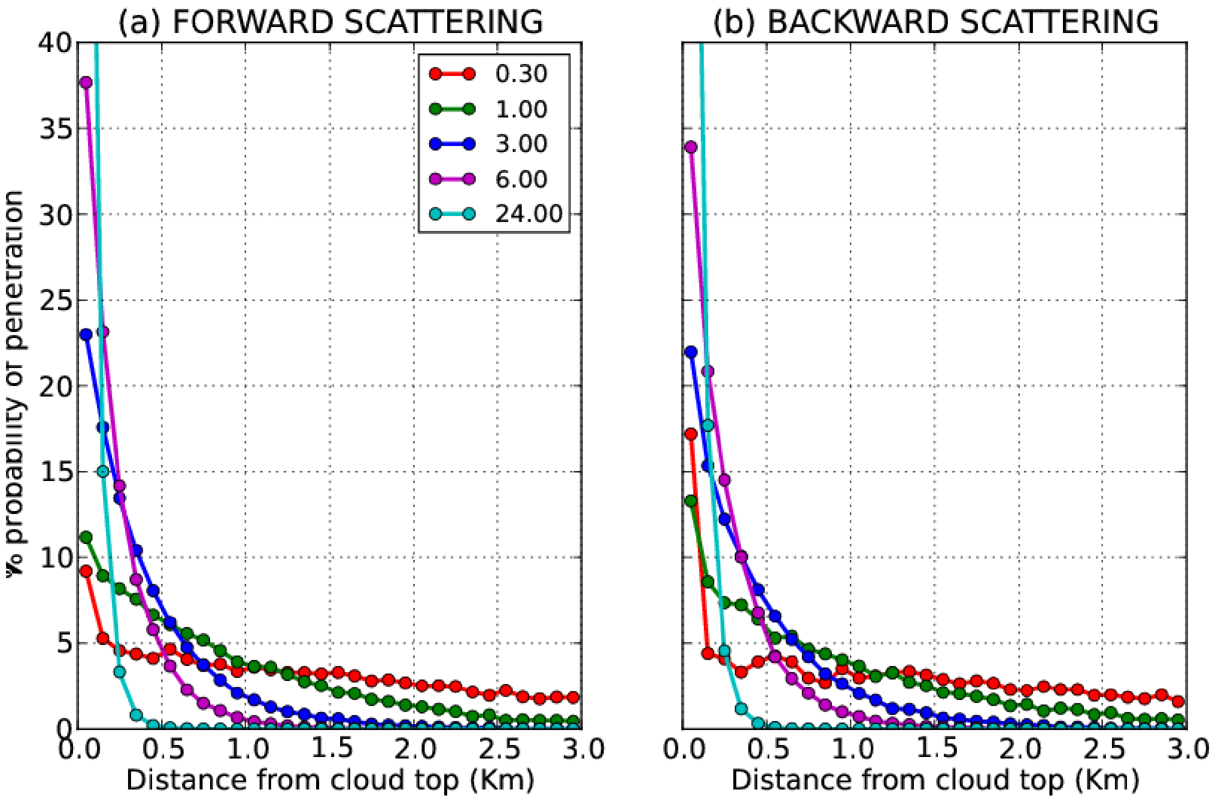


Figure 10.

The Key in the top left of Figure 10 (a) gives the assumed cloud optical depth. The reviewer cited Rosanov and Kokhanovsky. The parameter of interest in that work is the average number of scattering event (N) profile. That paper shows that for optical depths greater than 6, N is smaller than one (which means that almost all reflected light comes from the top of the cloud (tau<6)). It is difficult to compare both calculations because the geometry, microphysics and wavelength are not the same, but they both lead to the same conclusion, the reflected light mainly comes from the top of the cloud.

In the revised paper the figure shown above is described as follows:

"Figures 13 (a) and (b) show that by a depth of 1 km from the cloud-top, the probability of penetration has been approximately more than halved, for optical depths greater than 0.3. By 1.5 km from the cloud-top, the probability of penetration is generally less than 5%. The percent probability of penetration shown in Fig. 13 (a) and (b) is similar."

*Page 14134, line 7: Figures 10 and 12 seem almost identical. I suggest noting that the weighting of RH does not make a difference instead of showing a new figure. However, the weighting of RH over the cloud depth needs to be corrected.*

**Reply:** The latter figure has been removed in the revised paper and we simply state that weighting the profile of $RH_i$ does not make any difference to the results already shown at the cloud-top.

*Page 14134, line 11: The correlation between RH and crystal distortion is far from convincing. The sample size for the low distortion pixels is very low (12 out of 297).*

**Reply:** The results presented are consistent with Gayet et al. (2011), a point that the reviewer did not dispute in their review, and in our opinion are worthy of publication as argued in our response to point 2 above.

*Pages 14135-14136: Please adapt the conclusions to reflect all the changes made accordingly.*

**Reply:** The conclusions have been adapted in light of the comments made by both reviewers.

*Technical corrections:*

*Page 14111, line 18: "Aspect ratios" should be "Area ratios" here.*

**Reply:** Corrected.

*Page 14111, line 20: I believe "make-up" should not contain the hyphen.*

**Reply:** Corrected.

*Page 14112, line 2: Please remove brackets around the citations.*

**Reply:** Corrected.

*Page 14113, line 7 and page 14136, line 26: Please change "Van de Diedenhoven" to "Van Diedenhoven"*

**Reply:** Corrected.

**Response to review 2**

The reviewer is thanked for their general positive and helpful comments. The reviewer has carefully read the manuscript and has helped to improve the quality of our paper. In our opinion, we have addressed the major concerns of this reviewer in a revised version of the paper. A point-by-point reply to each statement made by reviewer is presented below.

*One flaw in this work is the limited character of the data: the conclusions are based entirely on one data set covering one co-incident flight. The study would have much greater value if other data sets were examined in the same way. If this is not possible or practicable, we should be told why, and the point that this is essentially a single case study should be emphasized and suggestions made for other future studies addressing this issue.*

**Reply:** We were careful not to make bold conclusions, we merely report on our findings for this one case. There were a number of other Constrain flights that sampled cirrus, but these other flights were unsuitable for the purposes of the paper. The reason for this is because our paper requires the following flight conditions to be met. The cirrus must be largely free of underlying cloud and over the sea, the aircraft must be coincident with the PARASOL overpass, and the cirrus must be sufficiently optically thick to allow discrimination between models. Practically, it is quite rare that all these conditions are simultaneously met. In the revised version of the paper we state the reasons why more flights were not used and the revised text reads as follows:

"In this paper, one case from the Constrain field programme is presented. The conditions required for this paper are that the cirrus should be sufficiently optically thick to allow discrimination between various randomizations of the ensemble model using PARASOL retrievals, the aircraft and satellite should be co-incident, and there should be no underlying cloud or broken cloud fields. It is practically very difficult to obtain all these necessary conditions at the same time. There were several other Constrain cases but these did not meet the conditions necessary for this paper. This is because the other cases were either optically too thick, as these cases were associated with radar reflectivity studies, or there was no co-incidence between PARASOL and the aircraft. Furthermore, the condition of no substantial underlying cloud was not met, and the cloud studied was not cirrus, the other cloud types studied were either altostratus or stratocumulus."

In the revised paper we do emphasise that there is only one case presented for the reasons given above. We believe that the results presented are sufficiently interesting to encourage others to examine these more fundamental relationships between the atmospheric state and the shape of the scattering phase function. In the conclusions, we already call upon colleagues to investigate these relationships but using global observations.

*Another flaw concerns the single scattering model that is used, namely geometric optics (GO). While the GO includes facet distortion via facet tilt (the Cox and Munk 1954 model), it is only a surrogate for describing the scattering phenomena that are the expression of ice crystal surface distortion, as the GO model does not even include diffraction. One consequence may be that the discontinuity or nonlinearity observed in the RHi vs. distortion relationship, may be due to the fact that the facet tilt approach inadequately describes the scattering properties of ice, as it is essentially non-physical. That is not to say that the inclusion of air bubbles is physical - more likely, it is another ansatz that happens to produce results that match observations. Furthermore, the calculated asymmetry parameter values cannot be expected to be accurate. These points* *should be discussed.*

**Reply:** We were careful to emphasize in the original submitted paper on page 14122 lines 25-26 that the method of distortion does not necessarily represent real surface roughness but is instead a method used to create featureless scattering phase functions. The inclusion of spherical air bubbles is yet another approximation, which is used to further smooth the phase function, so that extreme values of distortion are avoided. We already pointed out that the tilted facet approach has been investigated by Liu et al. (2014) and we clearly stated the results from that paper on page 14122 lines 22-26. The wavelength of interest here is 0.865 μm and as already shown in Fig. 5, the maximum contribution to the scattering cross section occurs at a size parameter of about 182, which is well outside the range of current electromagnetic methods that can applied to complex randomly oriented ice crystal geometries. Therefore, we have no option but to use geometric optics, and to apply this method, necessitates an approximation to represent ice crystal complexity through the distortion method introduced by Macke et al. (1996). The model provided by Macke et al. (1996) does include diffraction at the projected cross section, but not internal diffraction. At the size parameters used in this study we do not know of any other readily available method that can approximate ice crystal complexity in a more realistic way. However, with regard to the asymmetry parameter, we do have to be more careful here, as the reviewer is correct to imply that just because featureless phase functions are predicted by using the above methods, this does not necessarily mean that the asymmetry parameter predicted is accurate. To address the points raised by the reviewer we state the following in the revised version of the paper:

"As stated previously, the methods adopted throughout this paper to represent ice crystal complexity have been applied to generate a spectrum of phase functions that retain and remove optical features that may be exhibited by naturally-occurring cirrus phase functions. It is not as yet possible to simultaneously fully represent actual ice crystal complexity (i.e., surface roughening and internal hexagonal cavities) using electromagnetic methods at the size parameters considered in this paper (Baran 2012). Therefore, approximations to ice crystal light scattering properties are required at such size parameters. This is achieved, principally, through the method of geometric optics, and as such, approximations are required to represent surface roughness and ice crystal complexity. Here, both of these complexities are represented through the application of distorting ray paths (Macke et al. 1996) and spherical air bubble inclusions (Macke et al. 1995; Labonnote et al., 2001). When applied to the ice crystal model, both randomizations lead to featureless phase functions, which are the phase functions that are generally observed (Baran 2012; Cole et al. 2013; 2014). However, although the methods applied in this study result in featureless phase functions, this, however, does not necessarily mean that the resulting asymmetry parameter values shown in Table 1 cover the actual range of those values. Recent observations by van Diedenhoven et al. (2013) of the asymmetry parameter derived from global polarimetric space-based remote sensing suggest median values in the range 0.76 to 0.78. Whilst Ulanowski et al. (2006) reported that laboratory estimates of the asymmetry parameter, assuming highly surface roughened laboratory grown rosette crystal analogues, could be as low as 0.61. On the other hand, the same study reported that smooth aggregate crystal analogues had asymmetry parameter values of 0.81. It is yet to be determined as to whether the asymmetry parameter values of actual cirrus ice crystals are as low as 0.61, and the values tabulated in Table 1 are in the upper range of van Diedenhoven et al. (2013) and Ulanowski et al. (2006)."

*While good use is made of the co-incident aircraft flight through the use of ARIES and the dropsondes, no advantage has been taken of in situ microphysical or humidity measurements. Why?*

**Reply:** In the revised paper we have included in situ measurements of $RH_i$ obtained during a profile ascent nearest to the dropsonde launch, PARASOL overpass, and ARIES retrievals and NWP model run. The measurements were obtained using the General Eastern and FWVS, and these results are now included in a revised figure 8 shown below for the benefit of the reviewer.

(a) (b)

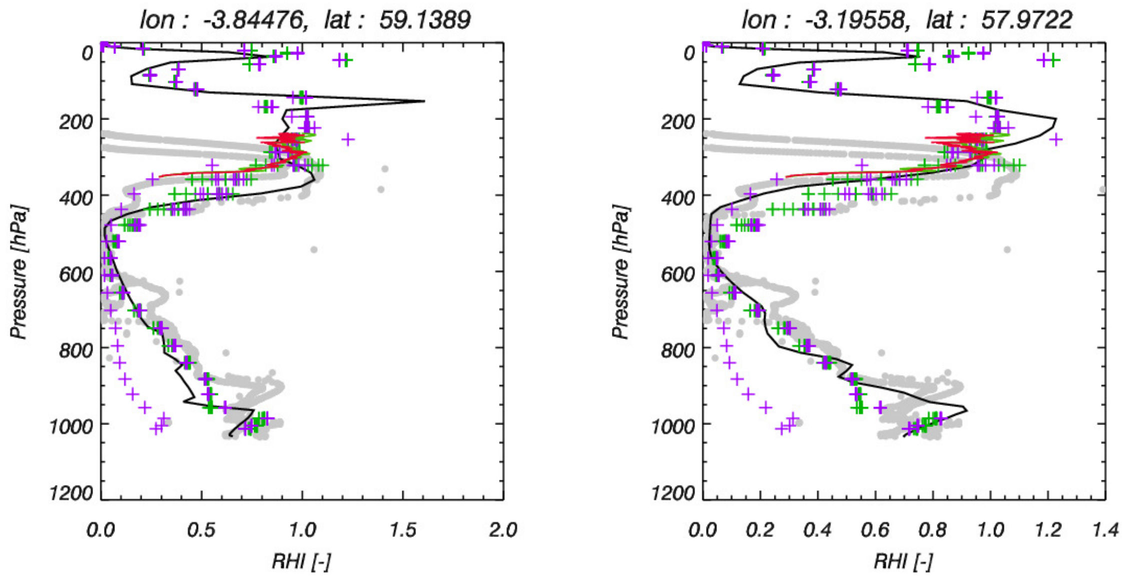


Figure 1.

The in situ $RH_i$ measurements are shown as the full red and green lines, NWP model full bold line, the plus signs are the ARIES retrievals and the dropsonde measurements are shown as the grey lines, and filled grey circles. There is generally a good correspondence between all comparisons at the pressure levels, where measurements were available. In the revised paper the new figure 8 (figure 1) above is described as follows:

"The NWP model prediction of the vertical profile of $RH_i$ is compared against the ARIES retrievals, dropsonde measurements, and in situ aircraft measurements from the GE and FWVS instruments of $RH_i$. The various comparisons are shown in Fig. 8 (a) and Fig. 8 (b) for two different locations. The in situ vertical profiles of $RH_i$ shown in the figures were obtained during an aircraft ascent from about 350 hPa to about 240 hPa, and the ascent started at 12:45:58 UTC and ended at 13:18:52 UTC. The dropsonde shown in the figure was launched at 13:30:00 UTC. The ARIES retrievals of $RH_i$ took place whilst the aircraft was on a straight and level run above the cloud between the times of 13:19:00 UTC and 13:32:13 UTC.

Figure 8 (a) and 8 (b) show that the two in situ $RH_i$ measurements are in good agreement with each other, whilst the dropsonde took some time to adjust to the prevailing atmospheric conditions. After this adjustment time, the infrared retrievals of $RH_i$, in the presence of cirrus, are in good agreement with the dropsonde and are within the range of $RH_i$ measured by the two in situ instruments. The figure demonstrates that the retrieval of $RH_i$ using high-resolution passive infrared measurements is sufficiently accurate and can be obtained, in the presence of cirrus, on a global scale using space-based high-resolution instruments such as the Infrared Atmospheric Sounding Interferometer (IASI). Furthermore, below the cloud, the dropsonde and retrievals are in very good agreement in the drier regions of the atmosphere, down to pressures of about 600 hPa. Moreover, the retrievals and dropsonde are in good agreement, down to pressures of about 1000 hPa. The two different retrieval colours represent the retrievals based on the two aircraft runs above the cirrus that were previously described. Each of the runs was 10 min in length. There were approximately eight ARIES retrievals per run. Figure 8 demonstrates that the retrievals, dropsonde measurements and in situ measurements are sufficiently consistent to compare against the NWP model. Figure 8 (a) shows the various comparisons at the latitude of $59.14^{o}$N and longitude $3.85^{o}$W, which corresponds to the upper left of Fig. 6. At a pressure of about 150 hPa there is a spike in the NWP model $RH_i$ field, but this is not supported by the retrievals and is probably due to numerical instability at that level, caused most likely, by the very low values of the water vapour mixing ratio at that level. The figure shows that the NWP model prediction of the vertical profile of $RH_i$ is consistent with the retrievals and measurements. Figure 8 (b) is similar to Fig. 8 (a) but for the location $57.97^{o}$N and $3.20^{o}$W, which corresponds to the lower left of Fig. 6. In this figure, the NWP model and retrievals can reach values of $RH_i$ of up to about 1.20. Figure 8 (a) and (b) validates the NWP model prediction of $RH_{i,}$ and, thus, this model can be used to compare against the PARASOL estimates of ice crystal randomization. Moreover, the model predicted cloud-top and base are consistent with the lidar results shown in Fig. 7 (a). Figure 8 (a) and (b) show that the NWP model predicted cloud-top is at about 200 hPa (~10 km), and the cloud-base is at about 400 hPa (~7 km), respectively."

In situ PSDs were not used in the PARASOL retrievals because we wish to be consistent with the PSDs assumed in the NWP model. This point is made in the revised paper. It is often the case that on comparing remote sensing results with climate or NWP models, the assumed microphysics is often inconsistent, especially with regard to PSD parameterizations.

*A further general comment is that the figures have been prepared somewhat carelessly, and both the graphics and the captions should be improved before the article is resubmitted for ACP.*

**Reply:** This has been done through the use of original postscript files and figure captions have also been generally improved where appropriate.

*Specific comments:*

*I do not like the statement in the Abstract that "This paper reports a positive correlation between the scattering phase function and RHi". Even though the statement is qualified in the next sentence, it still jars: something like "This paper reports a correlation between the shape of the scattering phase function and RHi" would be better.*

**Reply:** Agreed, we have used the change suggested by the reviewer in the abstract and actually throughout the revised paper.

*Fig. 1: to aid a comparison between Fig. 1 and Fig. 9 that we are encouraged to do later, geographical coordinates should be shown in Fig. 1. Likewise in Fig. 6. Also, what does "composite" mean, briefly, in Fig. 1 caption?*

**Reply:** Agreed, the lat-long bounds have now been incorporated into Figure 1 but we prefer to just state the lat-long bounds of figure 6 as the map projection between model and satellite are not the same. Figure 1 is re-produced below for the benefit of the reviewer. The lat-long bounds of Figure 6 are 57.8$^{o}$ and 59.7$^{o}$ and longitudes -5.3$^{o}$ and -1.8$^{o}$. We re-produce the new Fig. 6 as Fig. 3 below for the benefit of the reviewer. In Fig. 3 below, X mark the altitude at which the aircraft was above the cirrus, this was requested by the first reviewer.

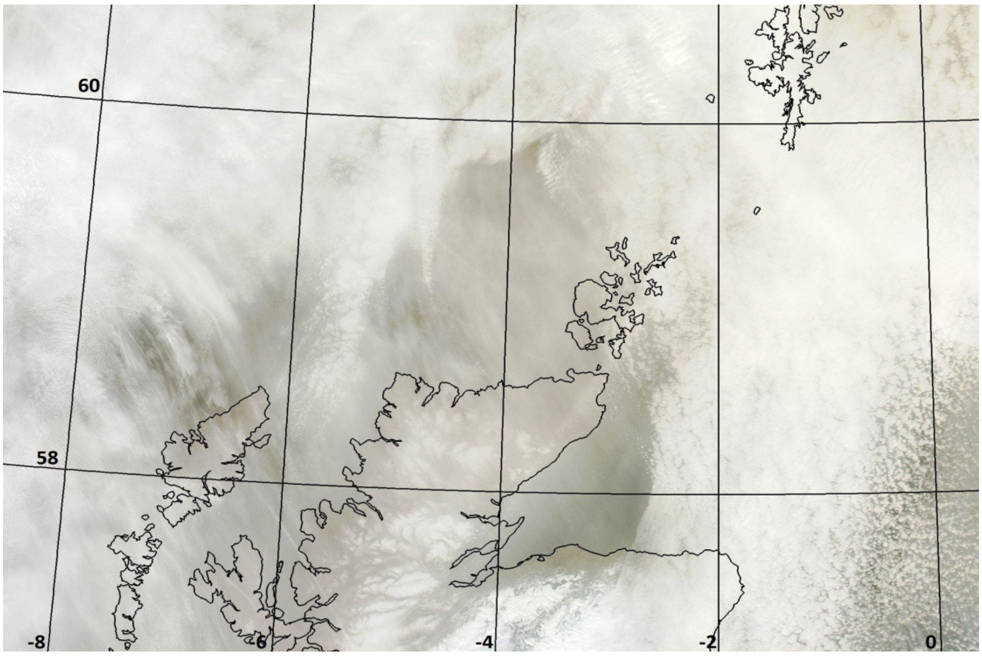


Figure 2.

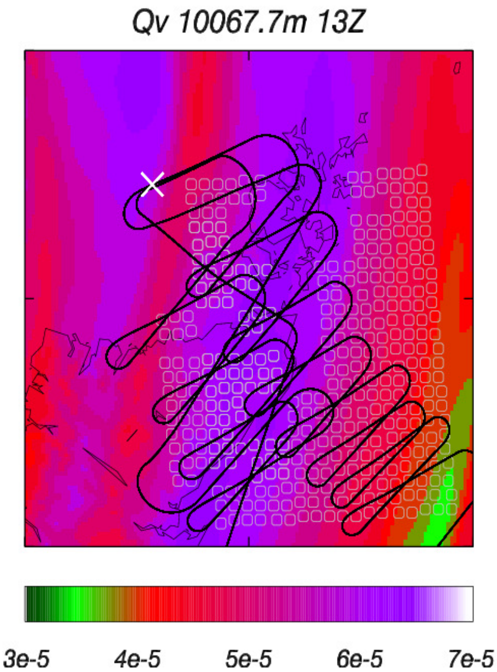


Figure 3.

In the caption of Figure 1, in the revised manuscript, now includes a definition of the word "composite" and the caption now reads as:

"The composite image was formed by combining the MODIS red, green and blue channels to obtain the closest "true" colour image."

With regard to the other figures, we have for instance improved the resolution of the coastline around Scotland in all the retrieval figures, an example figure is shown below in Figure 4a, which compares estimates of the shape of the phase function against the NWP predicted $RH_i$ field in Figure 4b, and Figure 4a can be compared against the MODIS image shown in Figure 2.

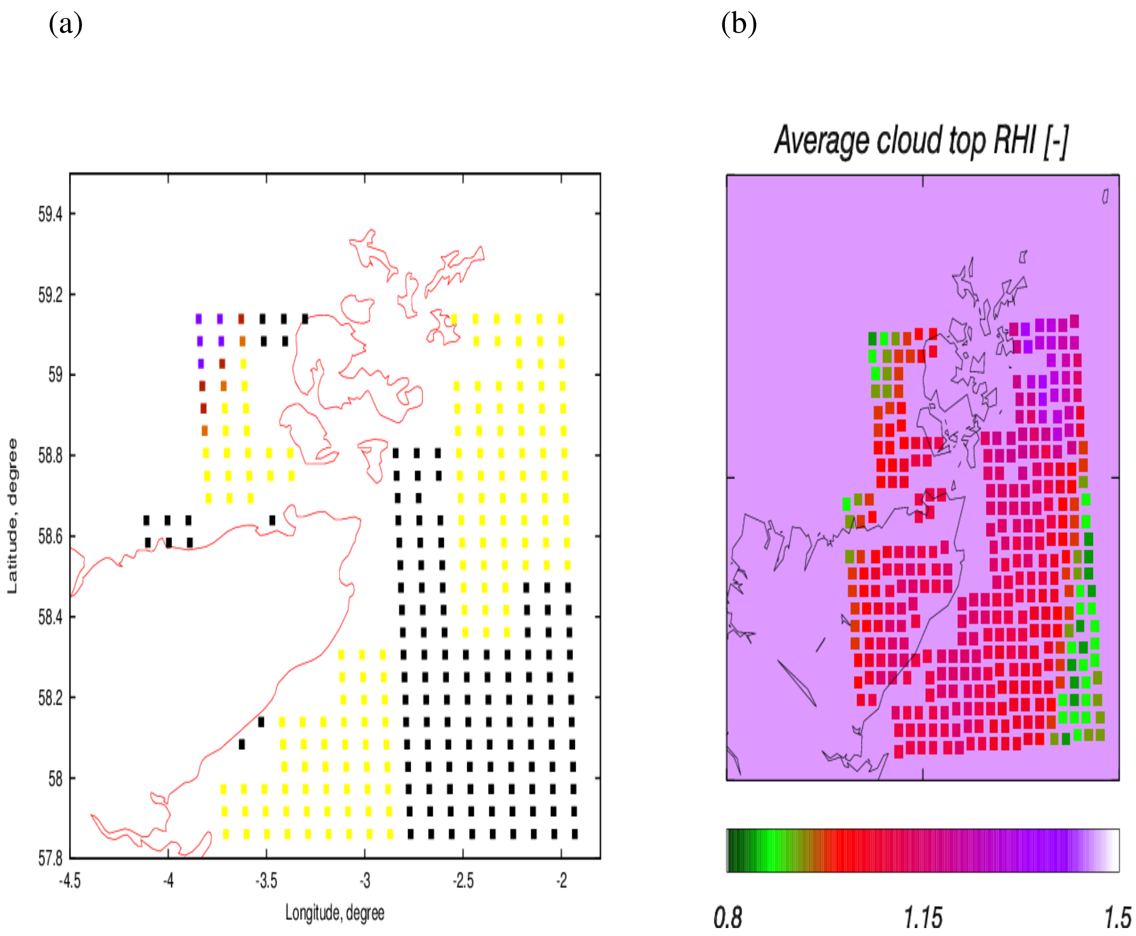


Figure 4.

The coloured boxes shown in Fig. 4a represent estimates of the shape of the phase function and these are defined as follows. The null results are the black squares; the most randomized phase functions (distortion=0.4 with spherical air bubble inclusions), yellow squares; and the pristine phase functions (distortion=0), purple squares; dark and light brown squares represent the slightly distorted (distortion=0.15) and moderately distorted (distortion=0.25) phase functions, respectively.

*Fig. 8 caption could be more descriptive: i.e. the retrievals are ARIES, and correspond to different runs. In (a) where was the sounding? Also I would drop the "percentage", what is meant is RHw expressed in %. In (a) RHi should ideally be shown, not RHw.*

*Lastly, the units in (b) and (c) should be hPa, as in (a).*

**Reply:** Agreed, Figure 8 has now generally been improved, see Figure 1 above. All units are now consistent as can be seen in Figure 1 above. The details of timings of the dropsonde and aircraft ascents are now given in the revised paper. Firstly, the caption to figure 8 now reads as follows in the revised paper:

“A comparison between the retrievals, dropsonde measurements, in situ measurements, and NWP model predictions of $RH_i$ plotted against the pressure (hPa) for two different locations. (a) The pixel located at longitude -3.84$^o$ and latitude 59.14$^o$ and (b) the pixel located at longitude -3.20$^o$ and latitude 57.97$^o$. Where in (a) and (b) the retrievals are represented by the purple and green plus signs, dropsonde measurements are the full grey line and filled grey circles, the General Eastern is the green full line and FWVS is the full red line.”

In the main text of the revised paper the timings are described as follows:

“The in situ vertical profiles of $RH_i$ shown in the figures were obtained during an aircraft ascent from about 350 hPa to about 240 hPa, and the ascent started at 12:45:58 UTC and ended at 13:18:52 UTC. The dropsonde shown in the figure was launched at 13:30:00 UTC. The ARIES retrievals of $RH_i$ took place whilst the aircraft was on a straight and level run above the cloud between the times of 13:19:00 UTC and 13:32:13 UTC.”

*Figs. 10 and 12. These figures are nearly identical, and one has to struggle to see any differences. Either a different graphical representation should be used (possibly combining the two plots), or the difference should be quantified, or both. Quantitative descriptors could include a correlation coefficient; to remove a bias due to the large number of data points corresponding to large distortion, correlation could be calculated for mean RHi values for each of the four distortion levels. Also, it is not clear why the authors introduce a new variable, the "cirrus randomization parameter". Would the "distortion" variable not be better?*

**Reply:** The latter figure has now been removed leaving only the results at the cloud-top and the distortion values are now plotted along the x-axis. The revised figure is re-produced below as Figure 5 for the benefit of the reviewer.

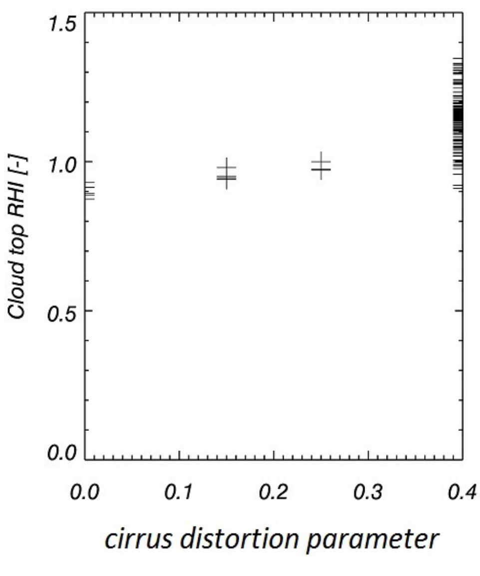


Figure 5.

At this stage we are of the opinion that a statistical analysis on the data presented may be incorrectly used by others as it does not include a global analysis. By this we mean that modellers may be tempted to use simple statistical models for parameterization, which at this stage would be premature. A full statistical analysis will be done once sufficient global data have been collected and if the results prove to be general.

*Technical corrections:*

*Page 14127, the index "j" has been omitted from the formulas.*

**Reply:** Corrected.

*Page 14141 line 20 and 14148 last line, change "Milosshevich" to "Miloshevich".*

**Reply:** Corrected.