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)

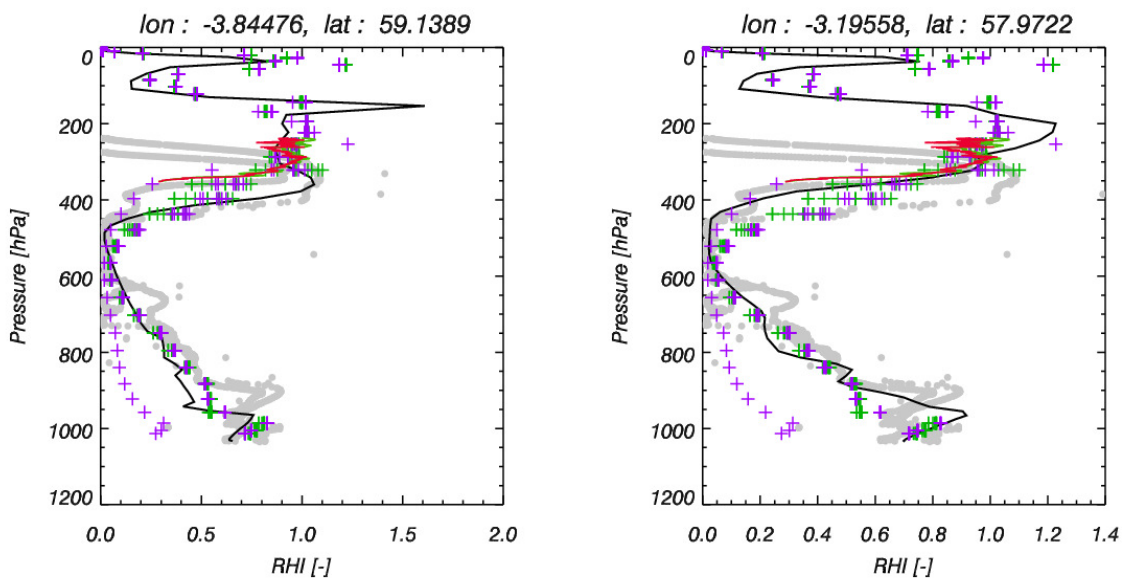

Figure 1.

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° and 59.7° and longitudes -5.3° and -1.8°. 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.

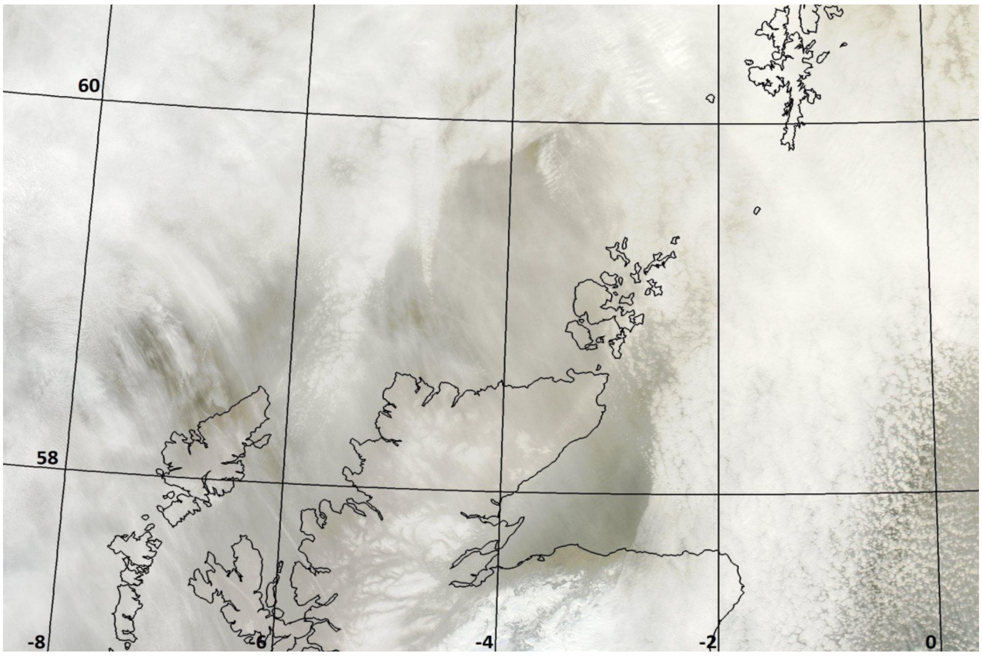

Figure 2.

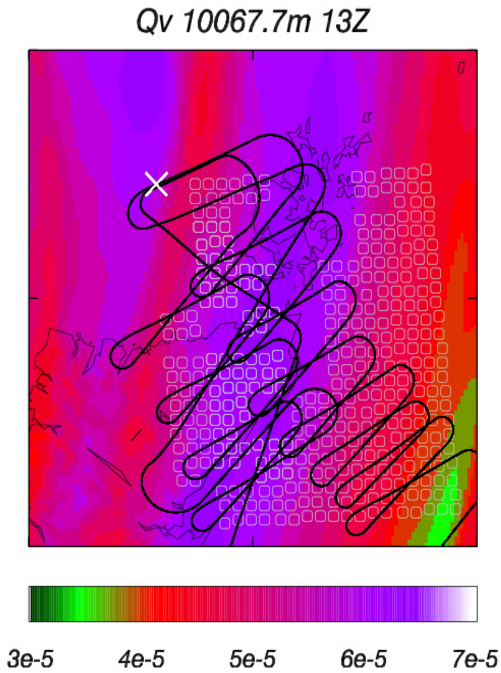

Figure 3.

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.

(a) (b)

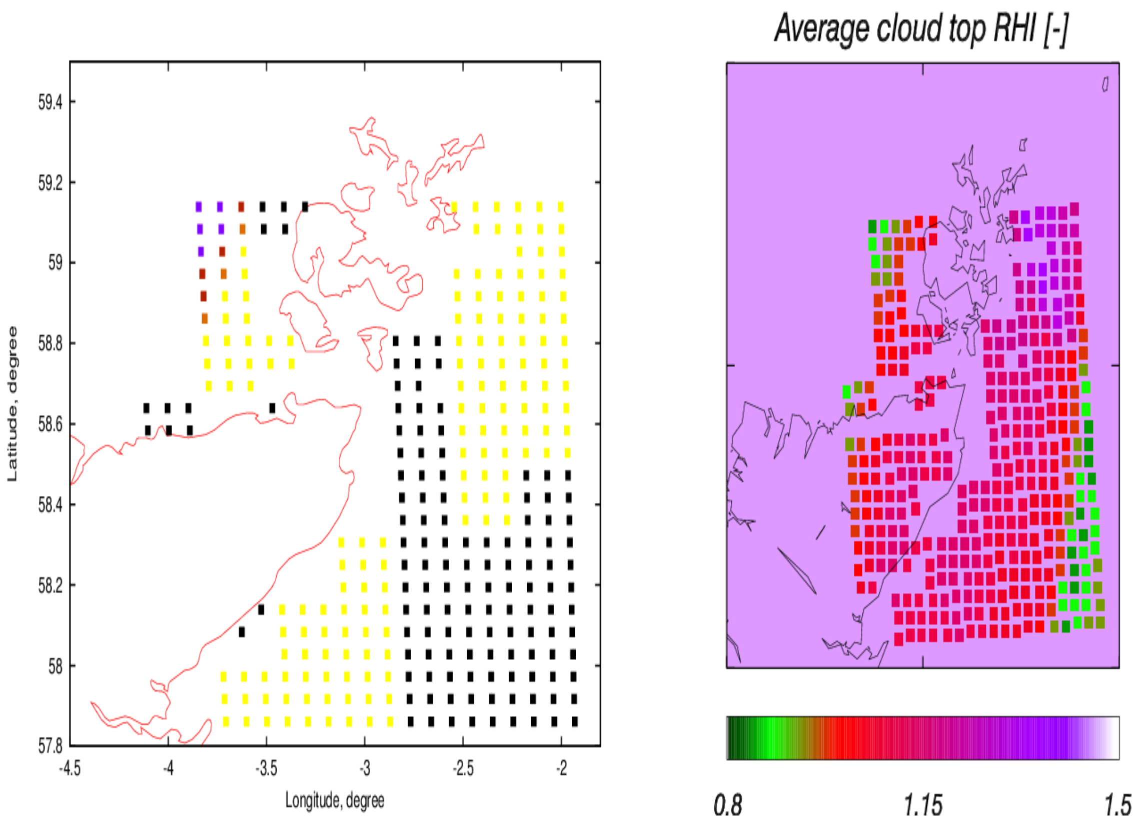

Figure 4.

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.

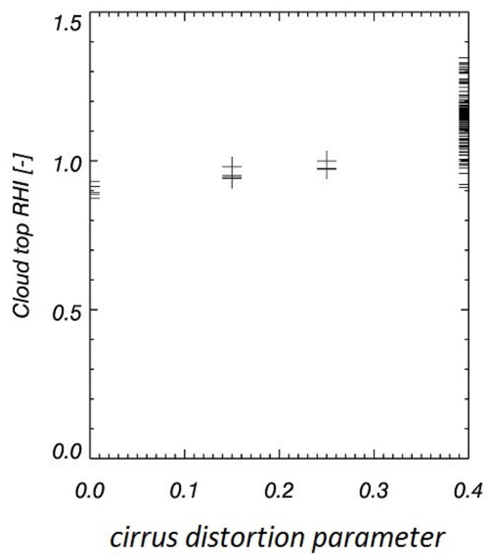

Figure 5.

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.