Interactive comment on “Halo ratio from ground based all-sky imaging” by Paolo Dandini et al.

Response to Anonymous Referee #1

We thank the Referee for many insightful comments and helpful suggestions. We list them below, together with our clarifications and changes to the manuscript, where relevant.

First of all, we must point out that Referee #1, perhaps misunderstanding the purpose of the initial, pre-discussion “access review”, provided a similar, detailed review prior to the discussion paper being published. In response the manuscript underwent substantial changes and was then published as the discussion paper. However, in several instances in the latter parts of the present comment the Referee has not taken these changes into account and instead appears to refer to the initial manuscript, which unfortunately is not made available as part of the public discussion. This makes our response and its interpretation difficult and potentially confusing, so to help the readers where this occurs we quote the initial review (labelled “Access review, Reviewer”) and response (showing the initial changes and labelled “Access review, Authors”, in red) followed by the final reply (labelled “RC1, Authors”, in red).

RC1, Reviewer:
• Abstract, Page 2, line 32 and especially Page 6, line 20:
The term “scattering phase function” should not be used in this context. It seems that the analysis in this study is based on the measured brightness distribution of the camera which includes multiple scattering rather than the actual scattering phase function, which is a single scattering property.
Access review, Reviewer:
◦ Page 2, line 31 and especially Page 6, line 19:
The term “scattering phase function” is confusing in this context. It seems that the analysis in this study is based on the measured brightness distribution of the camera which includes multiple scattering rather than the actual scattering phase function, which is a single scattering property.
Access review, Authors:
This is intentional: the corrections applied to the measured radiance are intended to provide an approximation to the unnormalized (1,1) element of the scattering matrix. We use the term phase function as shorthand for this property.
RC1, Authors:
We have responded in the access review. We define our meaning of the term phase function early in the text. However, on page 2, after “*Nevertheless, sky imaging is effective for recording the optical displays sometimes associated with cirrus called halos and for measuring the angular distribution of scattered light.*” for greater clarity we continue as follows “*We will henceforth refer to this quantity Scattering Phase Function as the corrections applied to the measured radiance are intended to provide an approximation to the angular dependence of the unnormalized (1,1) element of the scattering matrix*”. Further explanation is given in the reply to RC2.

RC1, Reviewer:
· Page 3, line 9:
“...the reflectivity is inversely proportional to the HR.” and line 14: “The asymmetry parameter […] is expected to be positively correlated with HR”

These statements are based on observations (Ulanowski et al., 2006; Gayet et al., 2011; Ulanowski et al., 2014) with most HR values < 1, i.e. no 22° halo, which has to be emphasized in this context. Since it is questionable whether differentiating values HR<1 is meaningful at all, the relationship between HR and asymmetry parameter should be explained here first without assumptions:
In general, the reflectivity is primarily determined by the asymmetry parameter which depends on the ice crystal surface roughness and aspect ratio in a U-shaped manner. The HR of the scattering phase function increases for ARs ranging from plates over compact crystals to columns. This implies an ambiguity in the relationship between HR and asymmetry parameter and thus also on the reflectivity.
See

◦ van Diedenhoven 2012: Remote sensing of ice crystal asymmetry parameter using multidirectional polarization measurements – Part 1: Methodology and evaluation with simulated measurements

◦ van Diedenhoven 2014: The prevalence of the 22° halo in cirrus clouds

◦ van Diedenhoven 2014a: A Flexible Parameterization for Shortwave Optical Properties of Ice Crystals

Access review, Authors:
Such ambiguity is indeed expected to exist for geometrically regular, hexagonal ice crystals. However, recent work (see the cited references) indicates that cirrus clouds are dominated by irregular crystals. Ulanowski et al. (2014) show that the halo ratio is inversely proportional to the broadly defined crystal roughness, which in turn is inversely proportional to the asymmetry parameter. As already cited, Gayet et al. (2011) show from in situ observations that the halo ratio is strongly positively correlated with the asymmetry parameter. We now repeat this citation on page 3.
RC1, Authors:
This comment is nearly a verbatim copy of the one given in the access review, too long to be quoted here and to which we believe to have provided adequate response in the initial reply.

· Many abbreviations and acronyms, especially in Sections 2.7 and 3, make the paper difficult to read.
In the following more detailed comments:

1. **Introduction:**

RC1, Reviewer:
The Introduction mainly focuses on cloud detection using all-sky imagers. Are these methods relevant for the presented study?
Access review, Reviewer:
◦ The Introduction mainly focuses on cloud detection using all-sky imagers. Are these methods relevant for the presented study? I would expect a more detailed review of the literature related to camera observations of halo displays, which is only briefly mentioned (Lynch et al., 1985, Sassen etal. 1994, Forster et al. 2017).
Access review, Authors:
No further literature on all-sky cameras observations of halo displays was found. If we take Forster et al. 2017 as the most recent review, aside from some more amateur-like observing networks of volunteers keeping track of halo phenomena as in (Verschure, P.-P., 1998.) and in (Pekkola, M., 1991), they do not mention any further work in this respect.
RC1, Authors:
We have provided a response in the initial reply to the nearly identical access review. However we can add that it is important to mention the various fields of investigation in which sky imaging finds application: cloud detection is one of these. Also this allows us to

point out widespread use of all-sky cameras and ease with which they could be extended to recording halos and measuring the angular distribution of scattered light.

RC1, Reviewer:
2. **Section 2.2.1:**
◦ Please provide some references for the calibration method using the coordinates of stars.
Access review, Reviewer:
3. **Section 2.2.1:** The description of the calibration method seems a bit incomplete:
◦ Please provide some references for the calibration method, e.g. Kannala and Brandt 2006: "A Generic Camera Model and Calibration Method for Conventional, Wide-Angle, and Fish-Eye Lenses
Access review, Authors:
This is the calibration method for estimating the parameters of the equidistant projection model. This work is not about accurate geometric modelling of real cameras aiming for fine correction of radial and tangential distortions. Described in this section is the calibration procedure to find the camera parameters by assuming the equidistant projection model ($r=f\theta$, "f-theta system"). This assumption is tested in section 2.2.2., and found to be satisfactory.
RC1, Authors:
We have responded to a nearly identical comment in the access review – see above. For clarification, our camera characterization method using the coordinates of stars is not taken from previous work. It is a novel aspect of this study.

RC1, Reviewer:
◦ How does the fit to a bi-cubic function work? Please provide the complete camera model used for the calibration.
Access review, Reviewer:
◦ How does the fit to a bi-cubic function work? Please provide the complete camera model used for the calibration
Access review, Authors:
The only camera model used for the testing of the lens projection is the one identified by eq. 1 and 2, the equidistant projection model.
RC1, Authors:
The bi-cubic function was not used in the final instance for the correction. We have clarified this by replacing the sentences on page 5, lines 25-27: "*This was found by plotting stars using the preliminary projection and determining the difference between the plotted stars and their corresponding background star. The errors as a function of the x, and then y directions were fitted to a bi-cubic function and these added to the coordinates.*" with the following: "*With the preliminary f-theta model so modified the camera parameters were manually adjusted until the difference between the plotted stars and their corresponding background star was minimized.*"

RC1, Reviewer:
◦ Which kind of distortion is corrected – tangential or radial or both?
Access review, Reviewer:
Which kind of distortion is corrected – tangential or radial or both?
Preliminary review, Authors:
There is no distortion correction implemented here. Section 2.2.1 is meant to explain the procedure adopted to measure the camera parameters, namely f, Δ, x_0, and y_0.
RC1, Authors:
For greater clarity we have replaced the phrase in the Abstract "*geometric correction of lens*

distortion" with: "*geometric camera characterization*".
Also, on page 5 we replace "*Finally the empirical scaling factor was applied to the coordinates in the x and y direction to compensate for the distortion created by the lens.*" with "*Finally an empirical scaling factor was applied to the coordinates in the x and y direction. With the preliminary f-theta model so modified the camera parameters were manually adjusted until the difference between the plotted stars and their corresponding background star was minimized.*"

RC1, Reviewer:
◦ How many pictures are used for the calibration?
Access review, Reviewer:
◦ How many pictures are used for the calibration?
Preliminary review, Authors:
While one image was used for the camera calibration, for the testing of the lens projection more than 8000 images were processed, roughly one for each point (blue/red dot, fig. 2) of the star/planet's trajectories.
RC1, Authors:
For greater clarity we add on page 5 after: "*Geometric calibration of the camera is done by detecting the position of specific stars and planets in a night-time image and implementing a minimization procedure.*" the following sentence: "*This was achieved by using four images, taken at different times of night so that bright stars were available in all quadrants of the image.*"

RC1, Reviewer:
◦ What is the effect of increasing the lens aperture on the accuracy of the geometric calibration?
Access review, Reviewer:
What is the effect of increasing the lens aperture on the accuracy of the geometric calibration?
Access review, Authors:
The tests had to be done with the aperture fully open, so there is no way to determine this effect, if present. However, we do not expect it to be significant. Moreover, the errors that were determined must be the worst case, as agreement with the f-theta projection can be expected to be similar or better for the closed aperture.
RC1, Authors:
A reply was provided in the access review.

RC1, Reviewer:
3. **Section 2.2.2:** The authors state a reprojection error of "mostly <0.1° aside from portions of […] trajectories for which larger discrepancies, up to 0.38°, were observed". How large is the error in the region of interest, i.e. where the 22° halo occurs? Is the accuracy sufficient for this study?
Access review, Reviewer:
4. **Section 2.2.2:** The authors state a reprojection error of "mostly <0.1° aside from portions of […] trajectories for which larger discrepancies, up to 0.38°, were observed". How large is the error in the region of interest, i.e. where the 22° halo occurs? Is the accuracy sufficient for this study?
Access review, Authors:
Such error cannot be stated, as the sun, and hence the halo, can be present at a broad range of zenith angles.

RC1, Authors:
A reply was provided in the access review.

4. **Section 2.5:** Using sun photometer measurements to estimate the vignetting effect of the camera is an interesting approach, but some issues are not discussed.
RC1, Reviewer:
◦ Page 7, line 20: What is the spectral response of the blue channel? The influence of Rayleigh scattering in the blue channel is much stronger compared to the red channel and small deviations in the considered wavelengths might cause larger errors in the radiance distribution. Why not use all 3 (RGB) channels? What is the required accuracy of the vignetting correction?
Access review, Reviewer:
◦ Page 8, lines 3-4: What is the spectral response of the blue channel? The influence of Rayleigh scattering in the blue channel is much stronger compared to the red channel and small deviations in the considered wavelengths might cause larger errors in the radiance distribution. Why not use all 3 (RGB) channels? What is the required accuracy of the vignetting correction?
Access review, Authors:
We use the blue channel as it provides the best spectral match with the sun-photometer operating wavelengths and also because it has a larger quantum efficiency when compared to the green and red channels. Furthermore the quantum response of the red sensor has a larger spectral width.
RC1, Authors:
On page 7 for greater clarity we rephrase "*The blue channel of a clear sky daytime RGB image was extracted, being the best match to a spectral channel of the sun photometer*." as follows: "*The blue channel of a clear sky daytime RGB image was extracted, being the best match to a spectral channel of the sun photometer and having larger quantum efficiency and narrower spectral width of the response than the green and red channels*."

RC1, Reviewer:
◦ Page 7, line 23: "...by neglecting pixels such that z is greater than approximately 20° over the meridian containing the sun...". Why are these pixels neglected?
Access review, Reviewer:
◦ Page 8, line 6: "...by neglecting pixels such that z is greater than approximately 20° over the meridian containing the sun...". Why are these pixels neglected?
Access review, Authors:
Vignetting is assumed to be symmetric under rotations around the camera zenith [(x_0,y_0) or equivalently z=0°]. Hence there is no need to use both halves of the principal plane, the one containing the light source (z<0°) and the one that does not contain the light source (z>0°), to extract the ratio of the two polynomials, one is enough. We decided to use the half such that z>0° and here is why: with respect to fig. 7, pixels such that z <-24° are not taken into account because in this half of the principal plane pixels suitable for measuring the devignetting coefficient are fewer. In fact the camera "sees" the occulting disk while the sun-photometer "sees" the light source, and therefore over the pixels where the occulting disk is located the devignetting coefficient cannot be measured. Nevertheless, pixels such that z<0° but outside the occulting disk region could have been used to take the ratio of the two fitting polynomials. However, this was not done as it was not needed. In fact from the above symmetry argument the devignetting coefficient could be measured using pixels such that z>-24°, as we did.
RC1, Authors:

On page 7 for greater clarity we replace "*by neglecting pixels such that z is greater than approximately 20° over the meridian containing the sun*" with "As *vignetting is assumed to be symmetric under rotations around the camera zenith, pixels such that z is greater than approximately 20° over the meridian containing the sun were neglected and ...* ."

RC1, Reviewer:
◦ Page 7, line 27: "...the presence of a peak located roughly 8° from the zenith was investigated...To form a correction function symmetric about the zenith, the original curve was "mirrored" about the zenith.." Based on which assumption? Is this peak is related to the camera or the sun photometer data? Was this peak observed at a different time as well?
Access review, Reviewer:
◦ Page 8, line 11: "...the presence of a peak located roughly 8° from the zenith was investigated...To form a correction function symmetric about the zenith, the original curve was "mirrored" about the zenith.." Based on which assumption? Is this peak is related to the camera or the sun photometer data? Was this peak observed at a different time as well?
Access review, Authors:
The vignetting correction is intended to be generic, so that it could be applied to many other sites using the same combination of camera and lens. Since the correction is nearly rotationally symmetric, and we concluded that the residual asymmetry is the outcome of a small misalignment of the sensor, which is likely to vary between cameras, the generic, symmetric correction can be applied to sites where deriving a camera-specific correction is not possible due to the absence of a sun photometer.
RC1, Authors:
For greater clarity at the end of section 2.5 we add the following: "*The vignetting correction so obtained is intended to be generic. Since the correction is nearly rotationally symmetric, and we have concluded that the residual asymmetry is the outcome of a small misalignment of the sensor, which is likely to vary between cameras, the generic, symmetric correction can be applied to sites where deriving a camera-specific correction is not possible due to the absence of a sun photometer*."

RC1, Reviewer:
◦ Are the measurements interpolated to the same time? How long did the sun photometer scan take?
Access review, Reviewer:
Are the measurements interpolated to the same time? How long did the sun photometer scan take?
Access review, Authors:
The sun-photometer principal plane measurement is performed every hour, over the various wavelengths, one at a time, about 35 s apart.
RC1, Authors:
On page 7 for greater clarity we replace "*The image brightness along the principal plane (containing the zenith and the sun) and the corresponding sun photometer output were compared;*" with the following sentence: "*The sun-photometer measurement along the solar principal plane, performed every hour, over the various wavelengths, one at a time, about 35 s apart, were compared to the corresponding image brightness from the closest camera measurement,;*"

RC1, Reviewer:
◦ Is the vignetting correction determined in the principal plane applied to the whole camera image? If yes, under which assumption?

Access review, Reviewer:
Is the vignetting correction determined in the principal plane applied to the whole camera image? If yes, under which assumption?
Access review, Authors:
See the comment above.
RC1, Authors:
See the comment above.

RC1, Reviewer:
◦ Fig. 6: Why is the calibrated brightness distribution (black dashed line) smoothed compared to the original data (red dashed line)?
Access review, Reviewer:
◦ Fig. 6: Why is the calibrated brightness distribution (black dashed line) smoothed compared to the original data (red dashed line)?
Access review, Authors:
This is due to the fact that the red dashed line is obtained by applying only the geometric correction to the raw data without the inclusion of the background mask. Therefore contamination associated with background objects is not screened out.
RC1, Authors:
At the end of section 2.5 after "*Fig. 6 (black-dashed line) shows the SPF corresponding to the raw image from Fig. 5 when geometric, AM, vignetting and mask corrections are included*." we add the following sentence: "*The latter removes the contamination associated with objects in the field of view, such as trees*."

RC1, Reviewer:
5. Section 2.6: What is the purpose of the air mass correction? Is the applied method of Rapp- Arraras and Domingo-Santos (2011) applicable to cloudy scenes as well? What is the error in this case?
Access review, Reviewer:
5. **Section 2.5:** What is the purpose of the air mass correction? Is the applied method of Rapp-Arraras and Domingo-Santos (2011) applicable to cloudy scenes as well? What is the error in this case?
Access review, Authors:
The purpose of the air mass correction is obvious: it is intended to remove the effect of variable slant path in the single scattering approximation. And the context is thin cirrus, so by definition the scene is cloudy (but the optical depth is low).
RC1, Authors:
The purpose of the air mass correction is simple, as stated at the beginning of section 2.6: the intention is to remove the effect of variable slant path in the single scattering approximation. Otherwise, averaging along lines of constant scattering angle could not be carried out. On page 8 to clarify this we have inserted an explanatory sentence "*With this correction in place averaging of sky brightness along lines of constant scattering angle becomes possible*." after the text "*the corresponding AM(z).*".

RC1, Reviewer:
6. Section 2.7:
◦ The cirrus BT threshold is based on an optically thick cirrus. 22° halos are only visible in thin cirrus. What is the effect of a decreasing optical thickness on the BT threshold?
Access review, Authors:
We have substantially extended section 2.7, given further detail about the cirrus

discrimination algorithm and highlighted where a further description of it can be found in the literature. Additionally, in Conclusions we have stated that the method has not been tested yet, vis: "*In the future these cirrus discrimination methods should be compared to techniques such as microwave radiometry or lidar which would allow us to assess their relative merit*."
RC1, Authors:
We have responded in preliminary review and have already substantially changed and expanded the manuscript (the changes in section 2.7 comprise 15 lines of text). Testing the BT threshold method, which is only a subsidiary topic in this work, is well beyond the scope of the article, as stated clearly in the manuscript.

RC1, Reviewer:
◦ How sensitive is the presented threshold method on variations of cloud cover in the scene?
RC1, Authors:
See the comment above.

RC1, Reviewer:
◦ The DFA threshold was estimated empirically to 0.02. This value is much lower than the values found by Brocard et al. 2011 (~0.1 for clear sky, ~0.5 for stratiform cirrus, ~1 for broken cirrus), why?
RC1, Authors:
While in Brocard et al. (2011) data were resampled into 5 s bins our data have a time resolution of 1 second. Moreover, while Brocard's FC is calculated over a time interval ranging from 60 to 300 seconds, our FC is fitted over interval lengths ranging from 20 to 60 seconds. In this respect on page 8, line 28, we rephrase "*This threshold was found empirically to be 0.02 on the basis of the DFA output calculated every 20 minutes and over a time scale ranging between 60 and 150 seconds*." as follows: "*This threshold was chosen empirically to be 0.02 on the basis of the DFA output calculated, unlike by Brocard et al.(2011), every 5 minutes, over data sampled with 1 s resolution and for time intervals ranging between 20 and 60 seconds. The intervals were chosen as the time range over which the DFA function was relatively stable and the slope of the function (the FC) was as sensitive as possible to the presence of clouds.*"
Furthermore, we wish to point out that like in Brocard et al. (2011) the initial analysis step of cumulative summation (integration) of the time series was not carried out. We note that cumulative summation of a self-affine series shifts the FC by +1 (Heneghan and McDarby, 2000), so the threshold applied here would have a value close to 1 if a standard DFA procedure was followed. We place this explanation in the main text.

RC1, Reviewer:
7. Section 3.1:
◦ Page 9, line 28: "...between 8 and 10 am… Over this time window […] the behaviour of the BT and the solar irradiance suggests that the HR increases with the optical thickness τ (see Fig. 10, middle and bottom plots)" This is not clearly visible in Fig. 10. The HR increases from 8:12 until 8:30 am but clearly decreases towards 10 am. How is the optical thickness derived from the displayed data?
Access review, Reviewer:
◦ Page 9, line 15: "...between 8 and 10 am… Over this time window […] the behaviour of the BT and the solar irradiance suggests that the HR increases with the optical thickness τ (see Fig. 10, middle and bottom plots)" This is not clearly visible in Fig. 10. The HR increases

from 8:12 until 8:30 am but decreases towards 10 am. How is the optical thickness derived from the displayed data?
Access review, Authors:
We are discussing optical thickness changes - in this context it is legitimate to consider relative changes in brightness temperature or solar irradiance (the latter opposite in sign), which is done here.
RC1, Authors:
A reply was provided in the access review.

RC1, Reviewer:
◦ Page 9, line 31: Kokhanovsky 2008 showed that the halo contrast is linearly decreasing with increasing optical thickness since molecular and aerosol scattering are neglected. Only **the radiance distribution is increasing up to τ=3 and decreasing for τ>3.**
Access review, Reviewer:
◦ Page 9, line 18: Kokhanovsky 2008 showed that the halo contrast is linearly decreasing with increasing optical thickness since molecular and aerosol scattering are neglected. Only the radiance distribution is increasing up to τ=3.
Access review, Authors:
From Kokhanovsky it follows that there are two regimes with respect to the behaviour of the 22° halo brightness with respect to cloud optical thickness τ. For τ<3 halo contrast increases with τ. Analogously, we observe cirrus whose optical thickness is increasing as testified by the increase of BT and a decrease in solar irradiance, and correspondingly an increase in the halo ratio.
RC1 review, Authors:
The reviewer is confusing our definition of “halo ratio” with Kokhanovsky’s definition of “contrast”. The reviewer seems to be referring to Fig. 3 of Kokhanovsky’s paper showing that “contrast” decreases with τ. However, our definition of the halo ratio differs from Kokhanovsky’s definition of “contrast”. With respect to the radiance distribution shown in Fig. 2a and 2b of the same paper, page 2, Kokhanovsky states: “*It follows from the analysis of Fig. 2 that basically there are two regimes with respect to the behaviour of the brightness of the halo located around 22° observation angle with respect to the cloud optical thickness. The first effect is the brightening of halo with τ. This occurs till cloud optical thickness of 3 (see Fig. 2a) both in the internal dark halo circle and also in the bright ring. For values of τ>3, the increase in the optical thickness leads to the decrease of halo central ring brightness.*” The behaviour of Kokhanovsky’s transmission functions (Fig. 2a and 2b) suggests that, with respect to our definition of the halo ratio, the ratio of the transmitted light at 23° to the transmitted light at 20° increases with τ for τ<3. Analogously, we observe cirrus whose optical thickness is increasing as testified by the increase of BT and a decrease in solar irradiance, and correspondingly an increase in the halo ratio.

RC1, Reviewer:
◦ Page 10, line 2: “Between about 8 and 8:20 am the HR, mostly <1, shows a maximum and a minimum at about 8:06 and 8:12 am, respectively, when a relatively faint 22° halo is visible”
How can the HR be <1 but the image shows a 22° halo? The 22° halo in the image at 8:54 appears brighter than at 8:06 but the HR is smaller. Why? Is this an effect of averaging over the complete 22° halo scattering angle region? If so, the HR might be primarily a measure of cloud fraction in this case.
Access review, Reviewer:
◦ Page 9, line 21: “Between about 8 and 8:20 am the HR, mostly <1, shows a maximum and a

minimum at about 8:06 and 8:12 am, respectively, when a relatively faint 22° halo is visible" How can the HR be <1 but the image shows a 22° halo? The 22° halo in the image at 8:54 appears brighter than at 8:06 but the HR is smaller. Why? Is this an effect of averaging over the complete 22° halo scattering angle region?

Access review, Authors:

Indeed - the reviewer has provided the most likely answer to the question, but it would be speculative to comment further.

RC1, Authors:

As to the original suggestion of the reviewer to use the HR for measuring cloud fraction, it should be pointed out that there are far better ways of doing so. Anyway, this is not the aim of this work. The ultimate goal is deriving reliable HR statistics. The reviewer's scepticism over whether HR is sensitive to the halo status of cirrus is not justified by the overall behaviour of the HR seen over the test cases discussed.

RC1, Reviewer:

8. **Section 3.2:**

Page 11, line 11: "... we conjecture that in order to observe the HR maxima measured for the two cases discussed here a certain minimum fraction of smooth hexagonal ice columns had to be present. If multiple scattering were to be accounted for, such a fraction could be an underestimation of the actual one." The second sentence is now redundant.

Access review, Reviewer:

Page 10, line 31: "According to previous radiative transfer calculations (Forster et al., 2017), we conjecture that in order to observe the absolute HR maxima measured for the two cases discussed here a fraction of at least 20% of smooth hexagonal ice columns was present. If Rayleigh and aerosol scattering were to be accounted for, such a fraction could be an underestimation of the actual one." Why 20%? The simulations in Forster et al. 2017 account for Rayleigh and aerosol scattering.

Access review, Authors:

We changed the manuscript and rewritten this sentence as: "*According to previous radiative transfer calculations (Forster et al., 2017), we conjecture that in order to observe the absolute HR maxima measured for the two cases discussed here a certain minimum fraction of smooth hexagonal ice columns had to be present. If multiple scattering were to be accounted for, such a fraction could be an underestimation of the actual one*."

RC1, Authors:

We removed the redundant phrase "*If multiple scattering were to be accounted for, such a fraction could be an underestimation of the actual one*."

RC1, Reviewer:

9. Conclusions:

◦ Page 11, line 29/30: "Overall the HR is shown to be sensitive to the halo status of cirrus as it is well correlated with halo visibility." Can you provide a quantitative statement?

Access review, Reviewer:

◦ Page 11, line 17: "Overall the HR is shown to be sensitive to the halo status of cirrus as it is well correlated with halo visibility." Can you provide a quantitative statement?

Access review, Authors:

Unfortunately, we do not know of a quantitative measure of halo visibility - it is a subjective one.

RC1, Authors:

We repeat our initial reply.

RC1, Reviewer:

◦ Page 12, line 3-9: Discrimination between cloud and clear sky seems to work well using the halo ratio alone, without additional DFA threshold test. Discrimination between cirrus and broken cumulus or other clouds seems necessary. How (well) does it work?

Access review, Reviewer:

◦ Page 11, line 24-30: Discrimination between cloud and clear sky seems to work well using the halo ratio alone. Discrimination between cirrus and broken cumulus or other clouds seems necessary. How does the presented method perform compared to the methods presented in the Introduction for cloud type discrimination and halo detection? (e.g. Shields et al. 2013, Forster et al. 2017)

Access review, Authors:

It is not possible to answer this question - we have pointed out in the conclusions (page 12 lines 3, 4 and 5) that such method "*should be compared to techniques such as microwave radiometry or lidar which would allow us to assess its relative merit.*"

RC1, Authors:

See the access review Authors' response here and "Section 2.7" above - the same manuscript modifications apply. Thus the required changes are already in the current version of the manuscript.

RC1, Reviewer:

◦ How does the presented method perform compared to the methods presented in the Introduction for cloud type discrimination and halo detection? (e.g. Shields et al. 2013, Forster et al. 2017)

Access review, Reviewer:

How does the presented method perform compared to the methods presented in the Introduction for cloud type discrimination and halo detection? (e.g. Shields et al. 2013, Forster et al. 2017)

Access review, Authors:

See the comment above.

RC1, Authors:

See the access review Authors' response here and "Section 2.7" above - the same manuscript modifications apply. The required changes are already in the current version of the manuscript.

RC1, Reviewer:

◦ Page 12, line 26: "We argue that when the 22° halo was visible a percentage of at least 20% of regular ice crystals had to be present if molecular, aerosol and multiple scattering in addition to surface albedo are accounted for, together contributing to a reduction of the halo contrast." What is the basis of this argument? Please explain.

Access review, Reviewer:

◦ Page 12, line 14: "We argue that when the 22° halo was visible a percentage of at least 20% of regular ice crystals had to be present if molecular, aerosol and multiple scattering in addition to surface albedo are accounted for, together contributing to a reduction of the halo contrast." What is the basis of this argument? Please explain.

Access review, Authors:

Radiative transfer calculations (Forster et al. 2017) not accounting for multiple scattering suggest that roughly 20% is the fraction of smooth particles needed for the halo to be visible. Given that the all-sky camera does not screen out the effect of multiple scattering, it is reasonable to think that such fraction could be larger, as multiple scattering reduces halo visibility.

RC1, Authors:
Page 12, line 26, we replaced the following sentence "*We argue that when the 22° halo was visible a percentage of at least 20% of regular ice crystals had to be present if molecular, aerosol and multiple scattering in addition to surface albedo are accounted for, together contributing to a reduction of the halo contrast.*" with: "*We argue that when the 22° halo was visible a percentage of at least 20% of regular ice crystals had to be present.*"
This is based on sensitivity studies of light scattering simulations (Forster et. al., 2017) showing (see Fig. 11 of Forster et. al.) that HR>1 is associated to smooth crystal fraction larger than 20%, as already cited (end of section 3).

RC1, Reviewer:
◦ Page 12, line 28: "We have also conjectured that when the HR reached its absolute maxima the fraction of such pristine crystals is likely to have been much larger than 20%." See previous comment.
"The remaining fraction could have been composed of irregularly shaped, complex, rough or small ice crystals." In Sections 3.1 and 3.2 fluctuations of the HR are explained only by variations of the cloud optical thickness, now the argument is based on the ice crystal microphysical properties. This is not consistent.
Access review, Reviewer:
◦ Page 12, line 16: "We have also conjectured that when the HR reached its absolute maxima the fraction of such pristine crystals is likely to have been much larger than 20%. The remaining fraction could have been composed of irregularly shaped, complex, rough or small ice crystals." In Sections 3.1 and 3.2 fluctuations of the HR are explained only by variations of the cloud optical thickness, now the argument is based on the ice crystal microphysical properties. This is not consistent.
Access review, Authors:
We do not understand the Reviewer's objection: the halo ratio depends on both optical thickness and microphysical properties. We make this point several times in the text. Perhaps the Reviewer was confused by the emphasis in the observation sections on optical thickness, as we have its indirect measures (via solar irradiance, BT etc.) hence we can draw conclusions on its impact on HR, while the later discussion also takes in modelling results that account for microphysics.
RC1, Authors:
We can only repeat our initial response.

RC1, Reviewer:
◦ Page 13, line 1: should be "...e.g. the presence of sun dogs and the 46° halo indicates the presence of aligned plates and **randomly oriented hexagonal crystals**, respectively."
Access review, Reviewer:
◦ Page 12, line 23: "...e.g. the presence of sun dogs and the 46° halo indicates the presence of aligned plates or solid columns, respectively." The 46° halo is formed by randomly oriented hexagonal crystals, not aligned solid columns.
Access review, Authors:
We did not intend to say that the 46° halo is caused by aligned columns, but solid columns. Perhaps our meaning will be clearer if we replace the conjunction "or" with "and".
It now reads: "*Furthermore, by extending the method to additional halo displays further information on ice crystal geometry could potentially be obtained - e.g. the presence of sun dogs and the 46◦ halo indicates the presence of aligned plates and solid columns, respectively.*"
RC1, Authors:

We have already replied to this and changed the phrasing accordingly; moreover we believe our statement to be more precise than the suggested one.

RC1, Reviewer:

◦ Page 13, line 3: "The utilisation of the all-sky cameras to transform the measured light intensity into the scattering phase function and, on a limited extent, the cirrus detection algorithm, are the particularly novel aspects of this work; this has not been done previously to the best of our knowledge." The term "cirrus detection algorithm" appears only in the Abstract and Conclusions. Where is this algorithm explained? The method described in Section 2.7 is not new and was already published by Brocard et al., 2011.

Access review, Reviewer:

◦ Page 12, line 24: "The utilisation of the all-sky cameras to transform the measured light intensity into the scattering phase function and, on a limited extent, the cirrus detection algorithm, are the particularly novel aspects of this work; this has not been done previously to the best of our knowledge." Which cirrus detection algorithm? The used method was already described in Brocard et al., 2011.

Access review, Authors:

Our cirrus detection algorithm does not rely solely on the method of Brocard et al. but includes thresholding of the level, as opposed to the fluctuations, of brightness temperature.

RC1, Authors:

Section 2.7 has already been substantially extended, further detail about the cirrus discrimination algorithm was given and relevant literature cited, as described in "Section 2.7" above. The Reviewer appears not to have re-read the manuscript.

RC1, Reviewer:

◦ Page 13, line 10: " The combined use of these two methods, allows relatively inexpensive halo observations and the retrieval of information pertaining to ice particles size and texture." How can particle size an texture be retrieved?

Access review, Reviewer:

◦ Page 12, line 31: " The combined use of these two methods, allows relatively inexpensive halo observations and the retrieval of information pertaining to ice particles size and texture." How can particle size an texture be retrieved?

Access review, Authors:

We are not saying that particle size and texture can be retrieved, certainly not individually.

RC1, Authors:

We stand by our initial response.