Most comments from the access review have not been addressed to a satisfactory level. This is why, following the decision of the editor from 18 Feb 2018, these were repeated in the review of the discussion paper. I can not recommend the publication of this manuscript unless the following major comments have been addressed, most of which have already been raised in the two previous reviews.
The authors address the individual points of the review almost exclusively in their reply. Instead, these should be addressed in detail in the manuscript in order to provide the reader with all necessary information at a glance.
To illustrate the history of reviews and replies, several examples are provided by highlighting the "Access Review, Reviewer", "Access Review, Authors", "RC1, Reviewer", and "RC1, Authors" in color. The most recent comments on the authors' reply and the manuscript ("Reply Reviewer") are shown in black.

Major comments:

1. **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, 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.
   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.

   Reply Reviewer: As mentioned in both previous reviews, the term "scattering phase function" used in the context of this manuscript is misleading. "Scattering phase function" is a standing term describing single scattering properties of particles. It is used here to describe a quantity, which the authors describe themselves correctly, however only in their reply to RC2, as "sky radiance corrected for the relative airmass contribution". This correction does not yield a single scattering property. Hence using the term "scattering phase function" in this context would mean a re-definition of a standing term.
   Later in the manuscript the acronym SPF refers to scattering phase functions calculated from electromagnetic theory ("...modelling studies of SPFs (Macke et al., 1996; Baran and Labonnote, 2007; Um and McFarquhar, 2010; Liu et al., 2013)..."). The authors' notation suggests that these two quantities are the same. It further raises the impression that the

scattering phase function of ice crystals can easily be obtained by simply accounting for geometric, vignetting and air mass correction neglecting multiple scattering and aerosol as stated in the abstract (“We propose to obtain it from the scattering phase function (SPF) derived from all-sky imaging”) and conclusions (page 12, line 25: “The utilisation of the all-sky cameras to transform the measured light intensity into the scattering phase function...”). This causes the reader to raise false expectations. The additional sentence on page 2 line 32 “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” is in my point of view not sufficient to resolve this issue.
Please replace the term “scattering phase function” where it refers to the camera observations by a more correct description, e.g. “sky radiance corrected for the relative airmass contribution”.

2. **Introduction:**

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, 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

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.

Reply Reviewer:
The cited studies observed mostly HR<1, i.e. no visible 22° halo. However, in this study the authors investigate cases with visible 22° halo (HR>1) as well. Therefore it is important to discuss the relationship between HR, asymmetry factor and reflectivity expected from

theory in addition as suggested already in the previous reviews. Otherwise this suggests an over-simplified relationship.

3. **Section 2.2.1: Camera Calibration**
   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, Reviewer:
   2. Section 2.2.1: Please provide some references for the calibration method using the coordinates of stars.
   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.

   Reply Reviewer:
   The camera characterization method using coordinates of stars has been published previously (e.g. Shields et al. 2013: “Day/night whole sky imagers for 24-h cloud and sky assessment: history and overview, Appl. Optics, 52, 1605–1616, doi:10.1364/AO.52.001605, 2013” and others). Please add some references to the manuscript.

4. **Section 2.2.1: Camera Calibration**
   Access review, Reviewer:
   Which kind of distortion is corrected – tangential or radial or both?
   Access 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, Reviewer:
   Which kind of distortion is corrected – tangential or radial or both?
   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.”

   Reply Reviewer: Instead of removing information in lines 25-28 on page 5, please state in the manuscript that lens distortion was not considered for the geometric calibration.

5. **Section 2.5: 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, 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?

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."

Reply Reviewer:

What is the basis of stating that the blue channel of the RGB image is the "best match to a spectral channel of the sun photometer" and has a "narrower spectral width of the response than the green and red channels" (page 7, line 24)? Please provide supporting information in the manuscript.

What is the error introduced by Rayleigh scattering? The RGB camera might detect a larger contribution of Rayleigh scattering than the sun-photometer data due to the different spectral width of their channels. Please provide information in the manuscript.

6. **2.5 Vignetting correction:**

The procedure how the vignetting correction was determined and some justifications are still not clearly explained in the manuscript. The following list is a summary of several points, which were raised already in the previous reviews:

- "The sun-photometer measurement along the solar principal plane, performed every hour..." Please state how many sun-photometer measurements were used to determine the vignetting correction.
- Please provide the corresponding camera image which was used for calculating the vignetting correction.
- Did all data (also from different days) exhibit the shift of the peak by about 8° as shown in Fig. 8?
- Was a geometric average of both sun-photometer channels (0.5015 μm and 0.4403 μm) used for the correction? This might also introduce an error since they should be weighted according to the spectral response of the blue channel of the RGB camera.
- Page 8, line 11/12: AM is not yet defined here. The authors might consider moving this sentence to end of Section 2.6 (Air mass correction).
- "...when geometric, AM, vignetting and mask corrections are included. The latter removes the contamination associated with objects in the field of view, such as trees"

does not explain why the curve in Fig. 6 (black dashed line) is smoothed for sca>40°. If a mask was applied, the measurements close to the horizon should be excluded, not smoothed. In Section 2.4 (Background mask) the authors state that "the measure was limited to images such that the light source z<65°". Does this mean that only images for a solar zenith angle <65° were considered? Should the background mask not rather constrain the scattering angles after the image has been transformed and rotated into the zenith as in Fig. 5?

- Page 8, line 14: "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..." If the shift in the peak of the vignetting correction is due to a misalignment of the camera this offset of 8° should be accounted for in the geometric calibration. Then, the vignetting correction should be determined using the geometrically calibrated image.
- It is not clear how the black dashed line in Fig. 8 was determined from the "original" and the "stretched" (should be "mirrored" like in the text?) curve? It might also seem intuitive to take the "envelope" of the "original" (red) and "mirrored" black curve as vignetting correction.
- With all these approximations it is questionable whether the vignetting correction provided here can serve as a generic correction to other sites as stated on page 8, lines 14-17.

7. **2.6 Air mass correction:**

The following question from the Access Review and RC1 has not been answered:

- Is the applied method of Rapp-Arraras and Domingo-Santos (2011) applicable to cloudy scenes as well? What is the error in this case?

In addition, the following points remain unclear:

- Which equation from Rapp-Arraras and Domingo-Santos 2011 was used in this study? Where does the value "40" (page 8, line 22) come from?
- It is not clear what is meant by "the kink" described in line 24 and how it is removed. Please add a figure to illustrate.

8. **Section 2.7: Cirrus discrimination**

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, 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?

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.

Reply Reviewer: The authors have not addressed the questions from RC1:
→ What is the effect of a decreasing optical thickness on the BT threshold?
→ How sensitive is the presented threshold method on variations of cloud cover in the scene?

9. **3.1 Test case 1: 6th July 2016**

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 increasesfrom 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, 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?

RC1, Authors:

A reply was provided in the access review.

Reply Reviewer: The relative changes described in the text (page 10, lines 15-18) do not match the results shown in Fig. 10. How was the optical thickness determined? The points from the Access Review and RC1 were not addressed in the authors' reply.

10. **3 Results and discussion**

Page 10, lines 6-10: When comparing the scattering angles used to calculate the HR the wavelength or spectral channel used in the respective study should be considered. Which channel is used in this study to compute the HR?

11. **3.1 Test case 1: 6th July 2016**

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, 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.

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.

Reply Reviewer: The citation of Kokhanovsky (2008) on page 10, line 18 is not correct: “...the HR increases with the optical thickness τ (see Fig. 10, middle and bottom plots). This is consistent with the results of simulations (Khokanowsky, 2008) [...] that show a linear increase of halo contrast with increasing τ up to τ =3 and a decrease for τ >3 due to multiple scattering.”.

As the authors correctly cite Kokhanovsky (2008) (not Khokanowsky, as in the manuscript) in their reply (RC1) the “halo contrast decreases with τ”. The authors cite further correctly that the 22° halo first brightens up to an optical thickness of 3 and then decreases for τ>3 – NOT the halo contrast. However, even if the citation of Kokhanovsky in line 18 was corrected, two differently defined quantities are compared: “halo ratio” and “halo contrast”. Both the reviewer and the authors seem to agree in this point. So please correct the sentence in the manuscript accordingly or use a more suitable reference.

12. **3.1 Test case 1: 6th July 2016**

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, 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.

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.

Reply Reviewer: Please include some discussion in the manuscript about the possible reasons of the mis-match between HR and the visibility of the 22° halo in the images. The authors seem to have misunderstood RC1. The reviewer did not intend to suggest using HR as a measure of cloud fraction. Instead the reviewer intended to point out that the information content of the HR, which is calculated by averaging over the whole azimuth angle range, might in fact be influenced by changes in the cloud cover. This might be an explanation of the mis-match between HR and the visibility of the 22° halo in the presented case studies.

13. **9. Conclusions**
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, Reviewer:
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?
RC1, Authors:
We repeat our initial reply.

Reply Reviewer: Throughout the manuscript the authors claim the HR as "quantitative measure characterizing the occurrence of the 22° halo peak" (Abstract). So the above mentioned sentence "Overall the HR is shown to be sensitive to the halo status of cirrus as it is **well correlated** with halo visibility" (now page 12, line 17) should be supported by results. This could be done, for example by counting the fraction of images with visible 22° halo and HR>1 vs. the fraction of images with visible 22° halo and HR<1. If it is not possible to support this statement it is not justified.

14. **9. Conclusions**
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, 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.

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).

Reply Reviewer: Page 12, line 14-16: "We argue that when the 22° halo was visible a percentage of at least 20% of regular ice crystals had to be present. 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."

Please remove or rephrase these sentences. The results presented in this study do not support that "a percentage of at least 20% regular crystals had to be present". This statement also does not seem to appear anywhere in Forster et al. 2017. They only state 10% as minimum fraction of smooth ice crystals in the conclusions by referring to Diedenhoven 2014.

15. **9. Conclusions**

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, 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."

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.

Page 13, line 23: As stated in the previous reviews, the sentence as it is phrased now is not correct. Please change it to "...e.g. the presence of sun dogs and the 46° halo indicates the presence of aligned plates or randomly oriented hexagonal crystals, respectively."

Reply Reviewer: Repeating the comments from both previous reviews, the statement (now page 13 line 23) is not correct. The 46° halo is caused by randomly oriented hexagonal crystals (plates or columns). The sentence should be corrected as follows: "...the presence of sun dogs and the 46° halo indicates the presence of aligned plates and **randomly oriented hexagonal crystals**, respectively."

Minor comments:

1. At some places in the manuscript it is not clear whether the z refers to the zenith angle in world or camera coordinates.
   In Eq. (1) and (2) on page 6 z represents the azimuth angle in camera coordinates.
   In Eq. (4) on page 7 θ is defined as the light source zenith angle.
   → For example: page 7, line 15: "...the measure was limited to images such that the light source z < 65°." If this angle refers to the light source zenith angle, should it be θ instead of z? Similarly on page 8, line 27 ("Furthermore for large solar z...").

2. Fig. 4: Please explain the the red arrow denoted by "u" in the figure caption.
3. Fig. 5: Which quantity is denoted by "R"? Please explain in the figure caption.
4. Fig. 6: What is "PF"?
5. Fig. 10 and 12: For better readability please explain the acronyms in the figure caption.