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Atmospheric Measurement Techniques Discussions

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

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

"**Response to Anonymous Referee #2**

We thank the Referee for many insightful comments, questions and helpful suggestions that allowed us to improve the manuscript. We list them below, together with our clarifications and changes to the manuscript.

Major points:

- The term "scattering phase function" is misleading, as I already mentioned in the quick review. The scattering phase function a clearly defined single scattering property, in particular the probability of scattering into a certain direction. What the authors deter-

mine here is a normalised radiance distribution. In case of optically very thin clouds (tau<<1, single scattering), the radiance distribution would be close to the scattering phase function if there was no additional molecular scattering. Please avoid the term because it is misleading!

Authors: We do not claim that what we observe is due solely to cirrus, and a phase function remains a phase function even if it is due to a mix of scattering components. It is appropriate to acknowledge that in some cases the observations depart from the single scattering assumption. However, a term like "sky radiance" is not suitable. This is because our results focus on radiance that is corrected for the relative airmass contribution, so is not simply sky brightness. Our use of the term phase function is intentional: 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. We use the term phase function as shorthand for this property. There is no other term in common use that we are aware of that describes the angular dependence of scattering in this context. We define the meaning of our usage of the term phase function early in the text.

However, for greater clarity, 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.*" we now 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.*".

- After the two case studies, I am not sure if the brightness temperature fluctuation threshold provides any useful information. Nearly all shown data points are clearly above the threshold. It would be good to see examples where the values are actually below the threshold.

Authors: Cirrus discrimination is only a subsidiary topic of the present article, the main aim being the retrieval and the behaviour of the halo ratio. Therefore, for brevity, we focus almost exclusively on cloudy skies. Brightness temperature fluctuation as a means to detect cirrus has been the subject of the previous studies.

- Conclusions, page 12, line 26: Where do the numbers (20%) concerning smooth crystal fractions come from? I do not see any evidence in the manuscript, neither any reference. If there is no quantitative evidence, please remove the statement.
Authors: 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).

- How was the halo ratio determined from the images? Maybe I missed it, but it isn't clear to me if the average over the full circle around the transformed image center was taken, or if only a sector was used, or ... And how does the (admittedly small) angular error affect the halo ratio?
Authors: The halo ratio is determined from the phase function, as already described in the introduction and then extended at the beginning of section 3 with the definition of scattering angles in use. So the Reviewer's question boils down to how the phase function is obtained; this is described at the beginning of section 2.3, as follows: "*The SPF is obtained by averaging the image brightness over pixels which are equidistant from the light source in terms of scattering angle*", and then reinforced at the end of this section with reference to the example image as "*The interpolated image is ultimately used to calculate the brightness as a function of the scattering angle*". For greater clarity we now insert at the end the words "*by averaging over the entire azimuth angle range.*".
As to the effect of the angular error on the halo ratio, we have not studied this aspect, beyond determining that the angular error is small, as the reviewer states.

Minor points:

Page 2, line 20: This is not really a sentence and the level of detail could be reduced, since it is anyway not sufficient to understand the technique without having to read the referenced paper.

Authors: The sentence
"*Field images, a red, a blue and a red and a blue trimmed with neural density were calibrated for the non-linearity of the basic sensor and for differences in the pass bands of the spectral filters and then used to obtain the corresponding blue to red ratio images before a threshold was set for determining the presence of thin clouds.*" has been as modified as follows: "*Field images were used to obtain the corresponding blue to red ratio images before a threshold was set for determining the presence of thin clouds.*".

Page 3, line 23: "previous" findings are contrasted to more recent results. However, these two papers describe observations in completely different locations on the globe which might be one cause for differences.
Authors: While these observations are from different geographic locations they both have been recorded in the northern-hemisphere for a mid-latitude scenario. Here we are not trying to give reasons as to why different studies show different results. We only want to stress that differences exist and should be resolved. Here we propose enabling this by the introduction of an inexpensive and easy to implement technique.

Page 4, line 3: Since the reader might now be familiar with the details of image vignetting at this point in the text, this section might be moved after the "all-sky-camera" section.
Authors: we think that there is a need for a short introduction to the instrumentation used, prior to a more detailed description of the cameras.

Page 4, line 16: What are these "2.52%"? Fraction of the solid angle? Please define.
Authors: We are referring to area only in this sentence: 2.52% is the fraction of the area of the sky. But to remove ambiguity we rephrase the end of the sentence to read: "*2.5% cut off at the top and at the bottom*" (note that now 95%+2.5%+2.5%=100% exactly).

Page 4, line 21: I am not sure how many readers are still familiar with kbaud. One might use kbit/s instead. Authors: we have altered this as suggested.

Page 4, line 25: The shadow disk for the daytime camera is meant to reduce stray light. Isn't the same necessary for the night time camera? The moon is certainly six orders of magnitude darker than the sun, but the lunar halo is as well. Or in other words, the ratio of the brightness lunar halo / moon should be the same as the ratio solar halo / sun. The straylight problem should be the same during day and night.
Authors: The measurements of the halo ratio used the daytime camera, and the night-time camera provided only star positions for testing the lens projection. Hence we show the shade used for the daytime camera only. The referee is probably correct that the stray light effect should be accounted for at night too, but we do not suggest otherwise!

Page 5, line 27: Could you provide the formula of the bi-cubic function and the order of magnitude of the deviation from the ideal camera model? From statements in the following section I infer that it was actually not used? If so, why mention it at all?
Yes, it is correct that the bi-cubic function was not used in the final instance for the correction. To remove the ambiguity, we have replaced 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.*".

Page 6, line 8: I assume, the test image here was different from the one in the previous section which was used for the geometric calibration?
Authors: Yes, they are not the same. Fig. 2 shows the image used for the testing of the lens projection model, as stated 3 lines later.

Page 7, line 16: Could you briefly (one sentence) explain what vignetting is and how it

is caused?
Authors: the additional text
Optical and natural vignetting are associated with smaller lens opening for obliquely incident light and the cos4 law of illumination falloff, respectively, both inherent to any lens design (Ray, 2002;"
has been inserted after
"*...in particular wide-angle and ultra-wide-angle lenses like fisheye lenses*", with the new reference: Ray, S. F.: Applied photographic optics, Focal Press, Oxford, 2002.

Page 7, line 24: Explain acronym LOWESS.
Authors: we have inserted the explanatory text "*(Locally Weighted Scatterplot Smoothing)*".

Page 7, line 27: with "the presence of a peak located roughly 8°" you mean that the maximum of the function is at 8° rather than at 0° where you would expect it?
Authors: Yes, that is correct. To clarify the meaning, we have replaced the text with "*the shift of the maximum from the zenith to the position at roughly 8°*".

Page 8, line 1: Could you please explain why the average between the original and the mirrored curve was used, rather than shifting the curve by 8°?
Authors: As is explained later in the text, the purpose is to form a rotationally symmetric devignetting function, centred on the zenith.

Figure 8, caption: This is confusing and contradicts the text: The caption says "stretched", while the curve is obviously shifted. The text on the other hand says "mirrored" rather than shifted.
Authors: The red curve is not shifted, it is indeed stretched, i.e. the value at the margin of the image is kept constant. To achieve that, the curve is simply mirrored about the zenith, as explained in the text.

Page 8, line 8: The purpose of the airmass correction is not clear to me. Is it to make the brightness distribution more "flat"?

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. 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).*".

Page 8, line 19: To assume single scattering the slant optical thickness has to be smaller than 1. For a viewing zenith angle of 70° the slant optical thickness would be about three times the vertical optical thickness, and thus the assumption of single scattering would only be true for clouds with vertical optical thickness smaller than 0.3. I am sure many of the observed cirrus clouds had a larger optical thickness. Also, the vertically integrated Rayleigh optical thickness in the blue is about 0.2 which adds to the cirrus optical thickness. This once more confirms that you don't observe the scattering phase function but the sky radiance which is caused by cloud scattering plus molecular scattering.
Authors: Please see the first comment for our explanation and justification of our use of the term phase function. Concerning optical thickness, the Reviewer is correct that in some instances a departure from the single scattering approximation is likely have occurred, especially at higher zenith angles. But the purpose of this work was not to obtain the single scattering phase function in the strict sense. The aim was to retrieve the halo ratio, and for that purpose a number of corrections was applied, one of which was the airmass correction, which implicitly assumed the single scattering approximation. So it is a compromise, but one that allowed us to average sky brightness over most of the sky, with the advantages that this brings.

Page 8, line 27: How was the threshold determined empirically? From "manually" deciding if an image is cloud/cloudless and looking at the corresponding fluctuation?
Authors: With reference to Dandini (2016) and Fig. 3.19 and 3.20 therein, the DFA algorithm was applied to one year of continuous BT data. Then the all-sky images

associated with minima of the fluctuation coefficient were selected. From this subset of images a manual cloud screening was done by visual inspection before taking the average FC, which was found to be 0.02. For additional explanation, we now add after "*The DFA algorithm was applied to one year of data and the corresponding FC time series was averaged over clear sky periods to set the DFA threshold.*" the following sentence: "*This was obtained after manual cloud screening of images associated with minima of the fluctuation coefficient.*".

Page 9, line 8: With "irradiance due to the direct emission of cirrus corrected for the atmospheric attenuation" you actually mean after correction to brightness temperature assuming Stephan-Bolthmann? The large deviation from -38°C results from the fact that your broadband instrument integrates over water vapor absorption as well as the atmospheric window? At first glance I expected it to be closer to -38°C, but that would only be the case for a narrowband instrument measuring in the atmospheric window.
Authors: The Stephan-Boltzmann does not apply to this case, as we approximate the narrow-band spectral response of the pyrometer KT15.85 (in the atmospheric window) to be monochromatic, at the central wavelength.
As to the text "*...the irradiance due to the direct emission of cirrus corrected for the atmospheric attenuation, was obtained (see Fig. 10 and 12, middle plot, black dashed line).*", it means that the direct cirrus emission is attenuated by the water vapour layer below the cloud before entering the instrument; this quantity is then added to the modelled water vapour direct emission and the final result is turned into brightness temperature by inverting Planck's law. The large deviation of the Ci threshold from -38° is mainly due to the fact that the model accounts also for direct emission from the water vapour layer below the cloud. For greater clarity we have rephrased the text to read "*...the irradiance due to the direct emission of cirrus corrected for the atmospheric attenuation and emission was obtained, and converted to brightness temperature by inverting Planck's law (see Fig. 10 and 12, middle plot, black dashed line).*".

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