Response to the Referee #1 comments for the manuscript "Relative sky radiance from multi-exposure all-sky camera images" By Juan Carlos Antuña Sánchez et al. in AMTD

First, we are grateful for the effort of Referee #1 and her/his review in detail. Reviewer comments are in black font (RC), and author comments (AC) in red font.

<u>Author's answer to Anonymous Referee #1</u>

RC: This manuscript presents a generally well-written study on the relative radiance calibration of a sky imager using HDR images. The study presents a comprehensive method in order to take into account all possible sources of uncertainty in obtaining the HDR image and, finally, the relative sky radiance. I recommend this study for publication in AMT after some minor revisions.

On section 2, it is not specified the origin of the spectral response of the camera filters shown in Fig. 1. Is it from the data sheet from the CCD manufacturer? Are they calculated somehow? Please specify. The exact setup of the RGB triband filter is not clear. How are these filters coupled to the camera body or CMOS sensor? In addition, the same as before, where does the spectral response of the filters come from?
AC: The spectral response of each RGB channel was obtained from data sheet of CMOS manufacturer, while triband filter response was provided by the sky camera manufacturer. The manuscript has been changed in order to add this information:

"*The spectral response of these filters, obtained from the data sheet from the CMOS manufacturer, is shown in Fig. 1a. An additional RGB triband filter (Fig. 1b; spectral response provided by the manufacturer) is over the full mosaic in the SONA202-NF in order to reduce the width of the colour filters*"

Regarding the setup of triband filter, it is coupled to the camera body, it is not part of the sensor, but its position is in front of the CMOS sensor.

RC: On section 3.2, what I'm understanding here is that the camera provides the images with a white balance gains of 1.1 and 2.1 for the G and B channels respectively. The white balance is reversed by dividing the pixel values by these gains (as in Eq. 1) and the everything is calculated with the image without the white balance, correct? If that is the case, the effect of having saturated pixels due to the white balance still exist. Is it possible to manually set the white balance off (or white balance gains to 1) in the camera?
AC: Yes, it is correct. This is the case, the pixels still are saturated by the white balance problem. The used camera was a prototype and it had the white balance fixed. The white balance can be set off now, but the option was not implemented when the images used in this paper were captured.

RC: On section 3.3 (paragraph starting on line 173), the M_DFS and SIGMA_DFS are average and standard deviation of the sum of the three channels? Or is it one channel (which one?)

AC: It is for all channels together (the sum of the three) since the readout noise (or dark signal) must be not dependent on the colour filter when there is not light incoming to the sensor.
"*All the dark frames have been corrected by Eq. (1), and the mean (M_{DFS}; mean dark frame signal) and standard deviation (σ_{DFS}) of the signal of all pixels (including the three channels) has been calculated for each dark frame.*"

RC: On the same section, finally, a temperature correction of the dark signal is not applied, right?
AC: Exactly, a temperature correction is not applied. We assume the main signal recorded under dark conditions is the black level, which is corrected. In fact, the mean of dark signal is below the pixel signal resolution of 1 DC. Hot pixels present a higher dependence on temperature, but they are also removed.

RC: On section 3.4, why the cloudy day (18th August) is used instead of a clear day (the 17th) to show the signal value at one exposure time vs other exposure times? Why adding an Fig. A1 when that could the Fig. 6? In addition, on Fig. A1, the description says 18th august and should be 17th.
AC: The original idea was to show two different cases, one full clear day and other with cloud presence, to observe that the results are similar and, hence, we can use all available days for the slope t_j/t_i calculation. Unfortunately, just one figure for one day is too big and hence we decided to attach one of them as supplementary. We think the importance of both figures is equal, and it is true that for the days with clouds the figure looks worse due to the dispersed data, but these data are very low frequent because the colour density scale is logarithmic. We were not sure what figure should be as supplementary and what as Fig. 6, but finally we decided to put the clear day figure as supplementary since it needs less explanation (it does not present high deviation data).
The description of the mentioned figure has been changed.

RC: On page 8, line 233, after readout noise it should say "(N_r)" just for clarification.
AC: Yes, it is true, but N_r is defined before at the end of the Section 3.3, and hence the definition of N_r again could be redundant.

RC: On section 4.1 it is introduced that the time window for comparison is 10 minutes. Having almost 2 years of data, it might be possible to narrow this window. This could probably have an impact on the deviation of radiance values at small scattering angles (<10) besides other possible effects. As shown on Fig. 9, the slope of the radiance for small scattering angles is very steep, and a difference in sun position between image and photometer might have an impact. At least it could be quantified.
AC: We found that 99.7% of the camera-photometer data pairs chosen with the time window of 10 min were within ±2.5 min, which was expected since HDR images are recorded every 5 minutes (0.3% of the cases with more than 2.5 min are due to some interruption in camera capture). We have changed the time window threshold to ±2.5 min instead of ±10 minutes in the new version of manuscript. The results are similar to the obtained in the previous version.

RC: As a final comment, it if very surprising that the effect of the reflection of the lens on the dome is not higher. It is clearly visible on the images and, even though the almucantar and hybrid configurations explore very specific angles, I would expect a higher impact, especially for the hybrid configuration where the angles measure pass transversally the reflection (as shown on Fig. 9).

AC: We think the effect of the reflection on the dome is unfortunately high, especially at longer wavelengths since the sky signal is lower. It is true, the effect has more impact on the hybrid scan, but the effect is high since it can be even observed in the radiance plot of Figure 9 (bottom panel), where a jump in radiance can be observed from 40º to 45º scattering angle because the radiance at 45º is inside the problematic area (reflection on the dome). The comparison between camera radiances and simulated ones revealed that the reflected area affects the radiance, and hence it is not recommended to use the sky radiance in this area (zenith angles from 48º to 65º).