Response to the Anonymous Referee #3 comments for the manuscript "Star photometry with all-sky cameras to retrieve aerosol optical depth at night-time" By Roberto Román et al. in AMT

First of all, we would like to thank the time and effort of the referee for the detailed review of the manuscript. Reviewer comments (RC) are in black font and author comments (AC) are in red.

Author's answer to Anonymous Referee #3

General comments

RC: The manuscript is about a relevant topic in aerosol remote sensing, specifically exploring the method of star photometry with all-sky cameras. The method yields interesting results, the text is well written, and so from my point of view, it is suitable for publication in AMT. Below, I have added some comments, and I leave it to the authors to decide if/ how to consider them for the manuscript.

AC: We appreciate very much this comment from the reviewer.

Specific comments:

RC: L272, Eqn. 2. It's probably pretty standard, so it could be omitted in case the manuscript needed to be streamlined.

AC: We agree that this equation is well known. It has been removed in the revised version of the manuscript.

RC: L382pp. While you can of course use a RT model, for the direct irradiance it would be a bit of an overkill, when you can use a simple equation (as in the Beer-Lambert law)?.

AC: We agree that for estimating direct irradiance under these conditions, the Beer-Lambert law may suffice. However, although the use of a complex radiative transfer model may not be necessary for this task, we opted to use the libRadtran package because it was more practical for us, given our experience with its use. Furthermore, this approach allowed us to incorporate the concentrations of gases in a standard atmosphere and the extinction coefficients for different gases at each wavelength, which are already included in the libRadtran package. This facilitated the calculation in a simpler way for us, and ultimately, it is as valid a method as directly using the Beer-Lambert law.

RC: By the way, also try to avoid the common confusion with libRadtran: it is not a "model" but a software package that includes multiple models (or solvers) like DISORT and MYSTIC (Monte Carlo code).

AC: Totally agree. We have rewritten the sentence as next:

"*To calculate λ_{eff}, and to ensure realistic values of direct irradiances, the spectral direct transmittance of the Earth's atmosphere is simulated under different conditions using the DISORT radiative transfer model (Stamnes et al., 1988; Buras et al., 2011) included in the libRadtran 2.0.4 radiative transfer package (Mayer and Kylling, 2005; Emde et al., 2016)*".

RC: L417pp. Again, is it probably not necessary to use a RT model here? Basically it's about the cross sections that you use, no? Also, while Fig. 7 is of course relevant in general, it is maybe more appropriate in a textbook about remote sensing. To me it seems a bit distracting in a paper focusing on a specific implementation of star photometry with a camera.

AC: As in the previous case, it is not strictly necessary to use an RT model, but it was more practical and easier for us, and we were able to take advantage of the cross-section coefficients that are included in the package.

Regarding Figure 7, it might be the one that least fits with the rest of the figures in the article. However, we believe that the method proposed for retrieving the optical depth of gases (including Rayleigh) is somewhat complex, particularly when dealing with spectral bands broader than usual. Therefore, we consider that this figure can help the reader better understand the method. That is the reason why we have chosen to keep it in the manuscript for now.

RC: Fig. 9. I could think that there are more interesting or representative cases to be shown?.
AC: The reviewer is correct in noting that there are more interesting cases. But while it is true that there may be other, more interesting cases, this particular case was chosen somewhat arbitrarily. We believe that there are likely more representative days, but these are already addressed in Section 4.2, which focuses on case studies. In the case of Figure 9, the main interest lies in showing how the AOD remains relatively stable during the night and discussing how and why some data points do not pass the established filters.

RC: Fig. 10. I found myself a couple of times trying to relate camera number and location (for simplicity sometimes only number or location is used in the discussion). I know, the figure is quite busy already with text and numbers, but maybe there is a way to include the locations or generally have a more efficient labeling?
AC: We are aware that the labeling can sometimes be somewhat confusing due to the large number of used cameras and in several locations, with some cameras being in multiple sites and some sites having multiple cameras. While we could separate the data by location, this would require mixing data from two cameras in Lindenberg and three in Valladolid (with two different camera models in the latter case). Alternatively, we could separate by cameras and subdivide by location, for example, by separating C005 in Valladolid, Izaña, Fuencaliente, and Andoya, as shown in Tables 5 and 6. However, this would result in too many panels in Figure 10, which already contains a significant number. Moreover, creating a new figure with this subdivision could be redundant, as most of the information is already provided in Tables 5 and 6.

Despite the potential confusion, and in order to avoid mixing data from different cameras, we believe that the current way of presenting the results is the most appropriate.

RC: L563pp. So how exactly would an error in the Langley show in the correlation scatter plot? Considering the airmass dependence of the AOD error it would cause some scatter, but only in one direction (above or below the 1:1 line)? If yes, would it be radiometric instability or sth else that causes the low correlation?
AC: We believe the reviewer has made an insightful observation in this comment. If the constant calibration log(USI0) –ln(USI0) in the new manuscript– obtained through the Langley method, is higher than its true value, then the AOD will always be overestimated, and vice versa. However, in our case, we do not rely on a single star with a unique value of log(USI0), but instead, the AOD is obtained at each moment by averaging the AOD values from different stars. These individual AOD values may be either overestimated or underestimated if the value of log(USI0) is not perfectly calculated, which is more complicated in high-latitude stations when using the Langley method. This is because the optical mass variation is slower in those locations, making it take longer to perform a Langley, and consequently, there is a higher likelihood of atmospheric changes during that longer period.

Therefore, the final AOD is an average of AODs that may be overestimated, underestimated, or well estimated. However, the individual AODs available change throughout the night and also during the year. As a result, we can have situations where the AOD could be sometimes overestimated and other times underestimated due to inaccuracies in the calibration constant (logUSI0) of the individual stars.