Interactive comment on “Can AERONET data be used to accurately model the monochromatic beam and circumsolar irradiances under cloud-free conditions in desert environment?” by Y. Eissa et al.

Anonymous Referee #3

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The topic of the papar matches the scope of AMT. Based on the comments (below), i recommend "major correction". Some of my comments are related to unclear explanation or disputable statements, while the others are just minor corrections. My overall impression: the paper is missing several important steps, necessary for the research and the derived conclusion, awkwardly written, and provide a lot of excessive details.

REPLY:

We thank you for your review of our manuscript. We respond to each one of your comments brought up. The manuscript has been updated accordingly.

My main concern is validation of the obtained results. Aerosol parameters, obtained from the AERONET, are recomputed to get input for numerical simulation. This simulation, to the best of my understanding, is compared with measurements taken by another instrument, SAM. An essential step is missing. Before switching to another instrument, the authors should have compared their numerical simulation with the AERONET measurements in order to make sure that numerical simulation works as expected. AERONET measurements and numerical simulation would coincide with some error. If the error is high, the subsequent comparison with SAM makes absolutely no sence. Source of the disagreement must be found and probably published.

REPLY:

In the revised manuscript we no longer ‘correct’ the AERONET data. The AERONET data is directly used as input to the radiative transfer codes. The data sets, results, figures, tables and text have been revised accordingly.

We do keep the comparisons, though not the correction, as it confirms the reliability of measurements from both SAM and AERONET. And it helps in removing possibly cloud-contaminated measurements, i.e. it may be cloudy in the field of view of one instrument but not the other.

CHANGES IN MANUSCRIPT:

The comparison now goes as:

“The AERONET AOD is not provided at the specific wavelength of the SAM instrument of 670 nm. Therefore, the AERONET AOD at this specific wavelength was computed using a second order polynomial fit of AOD versus wavelength using the AERONET measurements of AOD in the interval [440 nm, 675 nm] (Eck et al., 1999) as:

$$\ln(\tau_{a,\lambda}) = a_0 + a_1\ln(\lambda) + a_2\ln(\lambda)^2. \qquad (5)$$

This method to compute the reference AOD at 670 nm was selected because the fine mode pollution aerosols, mainly produced by the petroleum industry in the UAE, affect the linear fit of $\ln(\tau_{a,\lambda})$ versus $\ln(\lambda)$ (Eck et al., 2008).

5024 pairs of coincident observations remain, for which the maximum difference in time stamp of both instruments is 1 min. Similar to the cross-comparison of the radiance measurements to remove potentially cloud-contaminated measurements, the standard deviation of the differences between these remaining pairs of observations was computed. All coinciding samples with a difference greater than three times the standard deviation were filtered out. 150 pairs out of 5024 pairs of samples were excluded.

The Fig. 2 exhibits the density scatter plot of the 4874 pairs of SAM versus AERONET AOD at 670 nm. The relative RMSE is 10% and the relative bias is +7% meaning that the SAM $\tau_{a,670\text{ nm}}$ is greater in average than the AERONET $\tau_{a,670\text{ nm}}$. The R^2 value is high at 0.990. Even though AOD values sometimes exceed 0.8, the limits of the axes have been set to have a maximum value of 0.8 in order to better examine the regions with higher sample densities.

There are several interpretations for the discrepancies observed between the SAM and AERONET $\tau_{a,670\text{ nm}}$. The difference in the field of view of both instruments may partially explain such discrepancies, where the AERONET Sun photometer has an aperture half-angle of 0.6°. This implies a portion of the circumsolar radiation is intercepted within the field of view of the instrument, hence a smaller AOD than that observed by SAM. Although in Sinyuk et al. (2012) the error due to the field of view is quantified to be less than the uncertainty in the AERONET AOD retrievals, being 0.01 for $\lambda > 440$ nm.

Another possible cause for such discrepancies is how the Rayleigh scattering and small atmospheric absorption is accounted for at 670 nm in the SAM AOD retrievals. A fixed correction of −0.0556 is used, which was derived empirically by cross calibrations between SAM and AERONET using measurements collected in Oklahoma, USA (Pers. Comm. with J. DeVore and A. LePage, 2015). This fixed correction may induce errors in the SAM AOD retrievals, but it is stated by the team at Visidyne Inc. to be less than the uncertainty of the SAM AOD, being 0.03. Indeed, the bias of 0.02 between AERONET and SAM AOD retrievals is less than the reported uncertainty of the SAM AOD."

Besides that, i have several other comments:

1. Title of the paper. It makes sense to title a paper with a question (too long, by the way) only if the very first sentence in Conclusion and the very last sentence in Abstract provide clear answer "Yes" or "No" with just a few words of explanation. Instead, the first sentence in conclusion is "The work ... demonstrates that the AERONET data may very well be used .... with a certain degree of accuracy". 100% error is a certain degree of accuracy as well. The sentence does not provide a quick and clear answer (honestly, it has many words, but no useful information). Thus, i am convinced, the title should be reformulated.

REPLY:

We prefer to keep the title as is, with the following changes to address your comments.

CHANGES IN MANUSCRIPT:

The following sentence is added to the sentence before last in the abstract:

“Therefore, AERONET data may very well be used to model the monochromatic DNI_S and the monochromatic CSNI.”

The first line of the conclusion answers the question, with the following lines giving the specific numbers:

“AERONET data may very well be used to accurately model the monochromatic beam and circumsolar irradiances under cloud-free conditions in desert environment. In modelling the DNI_S at 670 nm both libRadtran and SMARTS provide very similar results. The relative RMSE is 6% for both RTMs, while the relative bias is +2% for libRadtran and −1% for SMARTS, and $R^2$is 0.972 and 0.964 in the same order. For modelling the CSNI at 670 nm in the interval [$\delta = 0.52°$, $\alpha = 6°$] five different configurations of inputs have been tested, two using SMARTS and three using libRadtran. Of the tested configurations, the most accurate is that using libRadtran when the aerosol phase function $P_{a,675\,\mathrm{nm}}(\xi)$ is defined as a 3-parameter TTHG phase function. In this case, the relative RMSE is 27%, relative bias is −24% and R^2 is 0.882.”

2. Abstract: i would recommend to avoid using acronyms. Before explanation, acronyms make the Abstract unclear.

REPLY:

We agree that acronyms should be avoided in abstracts. In this case, using acronyms is a way to prevent repetitions. One example, is the root mean square error (RMSE). Another more complex example is DNI_S whose name is "the irradiance originating from within the extent of the solar disc only" and which is used three times. Nevertheless, we have removed two acronyms: SAM and DNI. The last sentence of the abstract has been changed from "The results are promising and pave the way towards reporting the contribution of the broadband circumsolar irradiance to standard DNI measurements." to "The results are promising and pave the way towards reporting the contribution of the broadband circumsolar irradiance to standard measurements of the beam irradiance."

3. p.7700, line 16: "The objective of this article ...". Please start a new paragraph with this sentence, because objective is important. Otherwise, it is lost in the middle of the text. Also consider moving this sentence closer to the beginning of Introduction for clarity.

REPLY:

We start a new paragraph for the objective.

We have considered moving it closer to the beginning of the Introduction. However, that may confuse some readers who are unfamiliar with AERONET data, and the definitions of the beam and circumsolar irradiances.

We do mention “This paper deals with the modelling of the DNI_S and CSNI.” at the end of the second paragraph of the introduction. This at least gives the reader an idea on where we are headed with the paper.

4. p. 7701, line 1: "The article is organized..." - please start a new paragraph for the structure of the paper.

REPLY:

Done.

5. p. 7701, line 20: "...plane normal to the Sun ..." - consider reformulation: "...plane normal to the Solar beam direction ..."

REPLY:

Done.

6. Eq.(4) is a particular case of transmitted radiation, scattered once, in the direction of propagation of the Solar beam. At any other direction, a complete form of single scattering should be used. The authors failed to analyze the error caused by using Eq.(4) instead of exact single scattering. The explanation is "...diffuse radiance can be computed with a certain level of accuracy ...", which is absolutely not sufficient (reference are given, but no estimation of error from the references is given for Eq.(4)).

REPLY:

You are right, this is an approximation. In any case we do not present this formula anymore. Now the sensitivity analysis has been removed and the following analysis appears instead.

CHANGES IN MANUSCRIPT:

"Table 1 presents the mean, minimum, maximum and standard deviation of $\tau_{a,675\ \mathrm{nm}}$, $\omega_{a,675\ \mathrm{nm}}$ and $P_{a,675\ \mathrm{nm}}(\xi)$ for both the 1068 samples (excluding $\omega_{a,675\ \mathrm{nm}}$) and the 491 samples. These statistics are presented for $P_{a,675\ \mathrm{nm}}(\xi)$ for the three ξ smaller than 6° reported in the AERONET Version 2 Inversion product, i.e. 0°, 1.71°, and 3.93°.

The relative standard deviation of $\tau_{a,675\ \mathrm{nm}}$ for the 1068 samples is very large at 69% of the mean value, indicating its great temporal variability and its significance in modelling both the monochromatic $\mathrm{DNI_S}$ and diffuse radiance. The relative standard deviation of $P_{a,675\ \mathrm{nm}}(\xi)$ is also large, ranging between 18% and 24% for the three smallest ξ for the 1068 samples, again implying its significance in modelling the diffuse radiance.

On the contrary, the relative standard deviation of $\omega_{a,675\ \mathrm{nm}}$ is small at 0.019 (2% of the mean value) for the 491 samples. The uncertainty of the AERONET $\omega_{a,675\ \mathrm{nm}}$ retrievals is not provided, it is reported at $\omega_{a,440\ \mathrm{nm}}$ and is 0.03 (Dubovik et al., 2000). If the multiple scattering effects are ignored, the diffuse radiance is linearly proportional to the single scattering albedo (Dubovik and King, 2000; Liou, 2002; Wilbert et al., 2013). A practical consequence is that a mean value of $\omega_{a,675\ \mathrm{nm}}$ can be used with an acceptable loss of accuracy. In addition, using a mean value of $\omega_{a,\lambda}$ is a means to tackle the issue of the missing $\omega_{a,\lambda}$ values at instances when $P_{a,\lambda}(\xi)$ data are available. The AERONET retrievals of $\omega_{a,\lambda}$ are not provided under small aerosol loading situations and this causes the gaps in $\omega_{a,\lambda}$ (Dubovik et al., 2000; Yin et al., 2015).

The mean value of $\omega_{a,675\ \mathrm{nm}}$ for the available 491 observations over this study area and for this study period is 0.954, this number is fairly close to the monthly mean values of $\omega_{a,675\ \mathrm{nm}}$, which range from a minimum of 0.917 in December 2012 to a maximum of 0.974 reached in March 2013. In the extreme case of the minimum observed value (0.881), an error of 8% will be induced on the diffuse radiance by opting to use a mean value of $\omega_{a,675\ \mathrm{nm}}$. However, this is a rare situation. Indeed, 67% of the $\omega_{a,675\ \mathrm{nm}}$ samples lie within the mean ± 1 standard deviation and 96% lie within the mean ± 2 standard deviations."

It is worth noting the diffuse radiance is still linearly proportional to the phase function and the single scattering albedo for directions other than that of the solar beam.

7. p.7702, line 6: "Ignoring the multiple scattering ...". This section is misleading. Further in the text, the authors use radiative transfer (RT) codes, which include multiple scattering. So, what approach is used: single or multiple scattering? If single, than why using complicated RT codes? If multiple, that why section 2.2 is necessary at all?

REPLY:

This is only to provide an indication on the importance of the aerosol optical properties. In any case, the text has been modified as in the reply to the previous comment.

8. p.7702m Eq.(5): The circumsolar diffuse radiance, L, possess high dependence on scattering angle. As mentioned further in the text, there are only three measured points in the phase function (p.7705, line 23), available from the AERONET data, and hence only three points for the radiance distribution, L, in the integral. More points are needed for accurate evaluation of the integral, Eq.(5), but it is absolutely unclear where these points come from. Far from Eq.(5), the authors mentioned a log-log interpolation of the phase function (p.7708, line 6), but it is not clear how this log-log interpolation relates to integration in Eq.(5).

REPLY (a):

There is some confusion here, the aerosol phase function from AERONET products is provided at 83 angles. In p. 7702 to compute the radiance at a specific ξ we of course used the AERONET aerosol phase function at that angle. We are only interested at $\xi < 6°$, which is why we only used the three values of the phase function reported for such angles. As mentioned in the previous two comments this equation no longer appears in the text.

CHANGES IN MANUSCRIPT:

To further avoid any confusion we modify the text in Sect. 3 in the list of AERONET Version 2 Inversion products used as:

"the monochromatic aerosol phase function provided at 83 scattering angles, where the scattering angle is approximated by ξ (Wilbert et al., 2013), noted $P_{a,\lambda}(\xi)$;"

REPLY (b):

Regarding p. 7708, you are referring to point v of the quality checks performed on the SAM measurements. Those are not related to the AERONET aerosol phase function in any way. Besides there no interpolation is performed in point v, we compute the correlation coefficient between the circumsolar radiance measurements of SAM and ξ in the log-log space to check their linearity, which according to the literature they should exhibit a linear relationship

9. Eq.(6): it looks like Rayleigh scattering is ignored. Why?

REPLY:

Eq. (6) is no longer used.

10. Eq.(7): in many places across the paper relative error is mentioned (numbers are given). What is the acceptable range of errors? What error is considered unacceptably high?

REPLY:

We only refer to the error now when defending the use of a mean value of the single scattering albedo.

CHANGES IN MANUSCRIPT:

"The mean value of $\omega_{a,675\ \mathrm{nm}}$ for the available 491 observations over this study area and for this study period is 0.954, this number is fairly close to the monthly mean values of $\omega_{a,675\ \mathrm{nm}}$, which range from a minimum of 0.917 in December 2012 to a maximum of 0.974 reached in March 2013. In the extreme case of the minimum observed value (0.881), an error of 8% will be induced on the diffuse radiance by opting to use a mean value of $\omega_{a,675\ \mathrm{nm}}$. However, this is a rare situation. Indeed, 67% of the $\omega_{a,675\ \mathrm{nm}}$ samples lie within the mean ± 1 standard deviation and 96% lie within the mean ± 2 standard deviations."

11. p.7707, line 1: Quality control of an instrument definitely deserve a separate publication. I am not convinced (in part, because of my main concern mentioned above) that the quick quality control described in Section 4 is sufficient (well, maybe it is, but what proofs that?)

REPLY:

We understand your point and we partly agree. Actually, what we did is to set up a series of tests that permit to retain the high quality measurements. Some tests are more general than the others, some are maybe specific to our work.

CHANGES IN MANUSCRIPT:

We have softened the tone and changed the text from

"Therefore, a set of quality control procedures are defined herein to retain only the high quality measurements:"

into

"A series of tests have been applied herein to retain only the high quality measurements and possibly remove cloud-contaminated ones:"

We also changed the text in the Conclusion (Page 7721 – Line 20) from

"This article presents several tests that may contribute to a well-accepted quality control procedure."

into

"Several elements were developed here that may further contribute to a quality control procedure, whose design requires more work."

12. p.7710: Eq.(10) seems to be crucial for the study. But coefficients in the correction equation are poorly explained: why these values (0.992, 0.016)? Why 3 significant digits?

REPLY:

As per the comments of the other reviewers this correction is no longer used. The data sets, figures, tables and text have been updated as such.

13. Section 5: it looks like this section describes research in chronological order. If so, this is not the best way because it provides to many unnecessary information. I would recommend to characterize the final set of data points and list the (main) criteria only (instead of step-by-step description).

REPLY:

As we mention the SAM instrument is a fairly new instrument. Such comparisons presented in Sect. 5 provide an insight on the reliability of the measurements. We prefer to keep the comparisons, with some changes to them following the comments of other reviewers.

14. Section 6: why 2 RT codes are used? Why libRadtran only is not sufficient? If the second code is use to validate libRadtran (just in case) and nothing suspicions is found, then the role of the 2nd code is unclear.

REPLY:

Both RT codes offer different flexibilities on the inputs that can be used. We prefer to present the results of both codes and their flexibility in the inputs. This should be interesting for many readers interested in the subject.

15. Eq.(11) is complicated and not necessary for the paper. Simple explanation "Legendre polynomials" is sufficient both for those who are familiar with them, and for further googling by those who might be interested.

REPLY:

We prefer to explain everything clearly, there is no harm in keeping it.

16. p.7714, line 11: there are 3 unknown coefficients and 3 points for the phase function (p.7705, line 22). Why is the least-squares technique used for the case when the number of unknowns coincide with the number of measured data points, instead of solving a simple system of 3 equations?

REPLY:

We are using all the 83 scattering angles of the AERONET aerosol phase function to solve for these three coefficients.

17. p7714, lines 19-21: "... not tested ..." - if so, why mentioning this "possible solution"?

REPLY:

This sentence has been removed.

18. The DISORT solver from the libRadtran subroutine uses truncation of the phase matrix with single scattering correction as postprocessing. It is not clear from the text, if the correction was off or on. If off, then 16 streams (p.7715, line 14) does not sound sufficient to simulate light scattering by dust (large) particles. If on, then truncation of the phase function leads to high errors in the aureole (circumsolar) area even with the single scattering correction. This truncation error might or might not greatly affect the result of numerical simulation.

REPLY:

The correction was on (the *disort_icm* option in libRadtran v1.7), where we provided the Legendre moments of the TTHG phase function for the correction.

19. By the end of the paper i was absolutely confused regarding the value (applicability) of the Henyey-Greenstein phase function. There are two opposite statements: p.7718, line 26 "the HG phase function is a very bad representation ..." and p.7719, line 12 - " ... a very significant improvement when using the TTHG ...". Which statement is correct and what figure or number confirms that?

REPLY:

The HG and TTHG phase functions are discussed in Section 6.1 (Eqs 12-14 in the original manuscript, or Eqs 7-9 in the revised one). The TTHG is an improvement of the HG function. Hence, both statements are correct:

"It is clear from the results that the HG phase function is a very bad representation of $P_{a,\lambda}(\xi)$ and its use is not recommended when modelling the CSNI, in a desert environment at least."

and: "All statistical indicators show a very significant improvement when using the TTHG phase function determined from the AERONET measurements."

We understand that the wording may be a bit confusing and we rephrase the last statement as

"When using the TTHG phase function determined from the AERONET measurements instead of the HG function, all statistical indicators show a very significant improvement."

Interactive comment on Atmos. Meas. Tech. Discuss., 8, 7697, 2015.