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Atmospheric Measurement Techniques
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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.

My main concern is validation of the obtained results. Aerosol parameters, obtained from the AERONET, are recomputed to get input for numerical simulation. This sim-

ulation, 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.

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.

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

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.

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

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

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)).

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?

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).

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

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?

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?)

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?

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).

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.

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.

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?

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

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.

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?

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