

Review for manuscript “Retrieval of aerosol properties from zenith sky radiance measurements” by S. Herrero-Anta et al.

The manuscript presents a new methodology to calibrate ZSR from a ZEN-R52 instrument and a new method based on GRASP to retrieve aerosol optical properties from the calibrated ZSR values. The new methodologies present advantages with respect to previous works cited in the manuscript, namely the calibration without laboratory measurements and the retrievals without the need of local lookup table. The obtained results show a strong potential of both methodologies to make this instrument useful in different locations. Therefore, I clearly agree that this study fits within the scientific innovation, quality and the scope of Atmospheric Measurement Techniques.

The manuscript presents high scientific level, significance and potential for application of the work. It is well written and well structured, the objective and applicability is clear and the approach is technically well justified and validated. The abstract is accurate and concise, the introduction properly presents the topic background, previous works on the subject are properly cited and the new points are clearly indicated. The data and methods are well explained, and the rest of the sections are comprehensive and detailed. To my view, sections 3 and 4 have the same weight as section 5 and contain technical results as it is expected in this kind of works, therefore I do not consider that the results are only in section 5.

I propose that this article is accepted for publication, after improving some minor aspects that, to my view, will make the work more robust.

Minor aspects:

-Line 34: please give a short indication on the conclusions obtained for volume concentrations.

-Line 263: It is stated that a “clear trend” is shown in Figure 3 e and g. However, those fittings only present $r^2 = 0.01$, so in my view this is not a clear trend at all. I agree that 675 nm and 870 nm channels show such a trend once the median values are taken for the 2°C-bins, i.e., in Figure 3 f and h. Please rephrase those sentences to make this clearer.

-Figure 4 and line 861: Here the acronym ZSR_{DSC_TC} is used for the Dark signal and Temperature corrected ZSR. However, in the text (e.g. in Equation 2 and lines 269 or 278) this is simply called ZSR_{TC} . Please homogenize the acronyms to avoid confusion.

-Line 291: This seems a bit confusing here. I agree that the proposed calibration method is better in the sense that the same introduced ASTM-E490 solar spectrum to calculate ZSR_{SIM} will be then applied to the calibrated ZSR_{ZEN} before using them as input for GRASP inversion. Therefore, the statement “since there is no need for extraterrestrial spectrum normalization” may be misleading. Please rephrase those sentences.

-Section 3.5: it is stated that the ZSR_{ZEN} observations are compared against two different scenarios of CE318. However, the comparisons in 3.5.1 and 3.5.2 are not “treated” equally, or used for the same purposes. Only the bias statistics (MBE, Md, SD) from PPL comparison are considered and actually used as GRAPS “noise” input for each channel, while the same statistics (Md and SD) from CM are not used later. It seems that the CM comparison was actually more used to confirm the that the SZA and ZEN error criteria are successfully filtering out cloudy cases, but the deviations are not used as the reliability of each channel. If this is the case, please rephrase some parts of this section so that this is clearer.

-Line 404: are the values of MBE 0.23 to 0.11 correct? These are not the values shown in Figure 7.

-Line 418: SD value of 33.2% is not matching with the value appearing in Fig. 8

-Lines 466-468: is this statement correct? In the previous paragraphs, you explain that the AOD values are clearly overestimated by GRASP-ZEN and also in Figure 10 the VCF, VCT and VCC show the highest deviations in % (12 to 70%).

-Lines 502-504 and Figure 12e: I do not think this is needed, it is redundant plot.

-Lines 516-520: since the sensitivity study in section 4 was used to set the accuracy and precision of the proposed method, I would emphasize here in the results (section 5) whether the observed differences are within those accuracy and precision values or not.

-Line 550: again, I would not speak here about “uncertainty”, as it is understood that you obtain such uncertainty from the sensitivity study in section 4. I would better say that the dispersion of the differences are (or not) within the uncertainties obtained in section 4 and the uncertainties offered by AERONET.

-Line 563: the sentence is not clear, what did the methods reduce?

-Section 5: because of the strong importance of sections 3 and 4, that already contain technical results within the scope of the work, I will not call section 5 “results”. I propose to change the name of the section to something more describing the application of the methodology to measurements database.

Other corrections or typos:

-Line 111: “an uncertainty” instead of “and uncertainty”

-Line 224: “On the contrary”

-Line 234: “represents”

-Line 296: “whole”

-Lines 324-325: “at the same fixed angles regards the SZA” could be removed, it seems redundant.

-Line 426: “with a”

-Line 467: “but not”

-Line 517: maybe better “sensitivity study” as it was previously called like that.

-Line 551: $0.020 \mu\text{m}^3/\mu\text{m}^2$ according to the plot