

Referee comment on “Retrieval of aerosol properties from zenith sky radiance measurements”

Anonymous Referee

This paper describes the retrieval of aerosol properties using zenith sky radiances (ZSR) measured with the ZEN-R52 radiometer. Although a ZEN-LUT methodology was previously published for this purpose, this paper presents a new alternative for aerosol properties retrieval based on the GRASP inversion strategy (ZEN-GRASP). This new strategy has the advantage of not being dependent on the study location, allowing it to be applied to any instrument worldwide without the need for compiling a specific lookup table (LUT). Another advantage is that the ZEN-GRASP is capable of retrieving extensive aerosol properties, including volume concentration (VCT, VCC and VCF). Considering that this paper introduces new insights to expand aerosol characterization with a robust instrument capable of operating in remote regions, it can play an important role in reducing the current lack of ground-based information in key areas for aerosol modeling or assimilation.

The authors provide a detailed explanation of the inversion strategy in this work, along with a sensitivity analysis to ensure the method's suitability.

I consider that this manuscript aligns well with the scope of AMT, and the presented results are relevant. However, there are some general and specific comments that this referee believes should be taken into account, particularly to enhance the readability of the paper.

General comments:

1. This referee strongly recommends that the authors revise the English language used in the paper. There are certain parts of the text that are difficult to understand.
2. Section 3 and 4 (Calibration and Sensitivity Analysis) constitute a significant portion of the document. They comprise 5 pages compared to the 2 pages dedicated to listing the results. I must acknowledge that while these sections contain useful information for presenting the results, they tend to distract the reader from the study's main objective. Additionally, in my opinion, section 3.5, which includes the comparison with the Cimel instrument in terms of radiance, should be included in the results section. This would considerably streamline Section 3.
3. In line with the last comment, the lack of information regarding the periods considered for calibration (Section 3) and the application of calibration for obtaining AOD and VC products (Section 5) makes it challenging to follow the authors' temporal sequence in presenting the results. Is this the reason why the validation in radiance is included in Section 3 instead of Section 5? Are you using different period for calibration (training) and application of the calibration (validation)? Please provide clarification on this matter.

Specific comments:

Line 20: AERONET has been named as Aerosol Robotic Network or AErosol RObotic NETwork, please homogenize.

Line 25: pre-calculated.

Line 38: no significant.

Line 39: Could you please check the reference Cissé et al. (2022). The DOI does not work and I can not find a reference about aerosols in this text.

Line 47: ... processing and data(?) distribution.

Line 48: manufactured by Cimel Electronique SAS.

Line 50: (or lunar, if available).

Line 51: I will include a full stop here: This is achieved by applying...

Line 52-54: As a reader, I would appreciate shorter and more direct sentences, like, for example, this one: AERONET also employs an inversion algorithm to retrieve more intricate aerosol properties, such as aerosol size distribution and refractive indices. This algorithm takes into account sky radiances at different angles and wavelengths, along with the AOD values, as input.

Lines 59-65: This sentence appears to be written in a confusing manner. I propose something like: In this regard, several authors have utilized GRASP to retrieve aerosol properties using various measurements as input, including: satellites...

Line 66: manufactured by ...

Line 69-70: dedicated to the retrieval of water vapour).

Line 70-72: I suggest to re-write this sentence as follows: One advantage of this instrument is that it does not have moving parts and, in general, it is cheaper than more complex photometers. This affordability enables the installation of multiple instruments, thereby achieving a higher spatial coverage.

Line 80: The following paper?

Line 81: study location?

Line 106: almost always?

Line 119: The authors are using level 1.5 data. Is not the level 2.0 available in the period 2019-2021 at your site?

Line 121: Why the 10% limit? Is there a reference?

Line 131: Each filter is placed over...

Lines 208-209: The authors stated here that "This methodology is a field campaign" with no need of laboratory measurements. I do not understand the point of referring it as a field campaign. Furthermore, temperature characterization also needs the use of a thermal chamber in the laboratory...

Section 3.1: "...but it could be calculated from the night-time measurements (dark sky) when a thermal chamber is no available." Is it recommended by looking at the important T dependence of some ZEN filters not including DC correction (T dependent) and T correction on the ZEN systems?

Line 233: The colour of the points...

Section 3.3: I do not understand how the temperature correction has been done. The authors have the information of real ZSR measurements and simulated values when the ZEN system is inside the thermal chamber? Could you please clarify?

The different scales of the plots in Figure 3 prevents us for discerning the trends described in the paper.

Line 263: When the authors state the presence of a trend with temperature, is “trend” the correct wording? I suggest to talk about temperature dependence.

Line 269: Please remove indent.

Line 272: Please remove “ λ -wavelength”. It is redundant.

Line 275: “Definitive comparison”. Why this comparison has been labelled as definitive? As mentioned in the general comments, I consider that including here the time period will help the reader to clear the time sequence.

Lines 287-290: Could the authors clarify why systematic errors are expecting in case of sphere calibration is used (provided the same E_0 for normalization is used)?

Section 3.5: Similar problem with the time period.

Line 296: Please correct the typo in “whole”.

Line 308: Please remove the final comma.

Line 316: Can you add more information about the considered “wrong” values? Some statistics can help to understand why the values are wrong.

Line 332: Please add a point missing.

Line 340: Why this paragraph has a different indent?

Section 5: I can read here the number of data included in the analysis but not the period covered. Are the authors using here a different quality-controlled analysis than the one presented in the Section 3.2? I do not expect so, since this specific ZEN method includes the measurement errors. Maybe I have misunderstood the text, but I don’t see the point of using different QC methodologies. Could you please clarify?

Line 526: Please include a comma after because.

Line 532: VCT, VCC and VCF.

Line 546: ... a r^2 of about ...

Lines 553-566: I understand that it can be challenging to leave unpublished results that may be considered scientifically interesting. However, it is important to assess whether doing so would come at the expense of reducing the comprehension of the text or making it less appealing to the reader.

Line 584: A couple of tests?

Lines 615-616: It seems redundant with the information provided at the beginning of this paragraph.

Conclusions: I’m surprised it is not included in the conclusions the fact that this new strategy is not linked to the place of study as the former ZEN-LUT. It is actually one relevant improvement of the method...

Do the authors have an estimation of the decaying period of the calibration proposed in this paper? Is it expected the instrument to be recalibrated against a Cimel instrument every a certain period of time?

Figure 4: The x-labels should be “ZSR_DSC_T”

General things in the text: Please add an space before “nm” and correct the degree sign.