Response to the Referee Francisco Molero comments for the manuscript "Retrieval of aerosol properties from zenith sky radiance measurements" By Sara Herrero-Anta et al. in AMT

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

Author's answer to Referee Francisco Molero

RC: The manuscript titled "Retrieval of aerosol properties from zenith sky radiance measurements" by Herrero-Anta et al., presents the procedure to extract aerosol properties from zenith sky radiances measured by a newly developed instrument. The inversion algorithm is performed with GRASP and the results are compared with AERONET products. The methodology is well explained, providing details of the calibration, inversion strategy and sensitivity analysis. The manuscript is well written and it presents a substantial contribution to scientific progress within the scope of Atmospheric Measurement Techniques. There are several issues that require a better explanation, and also some minor issues and typos that should be corrected, highlighted in the attached file.

Firstly, the main difference of this measurement strategy respect to AERONET is the use of vertical data instead of that derived by multiple-angles. The AERONET instruments perform principal plane and almucantar measurements to obtain angle information, while the ZEN instrument only measures vertically. Please comment on these different strategies and their effect on the inversions. For instance, would CIMEL vertical measurements (like "Cloud mode" but on clear-sky days) be as reliable as PPL and almucantar?

AC: The reliability of the sky radiance measurements relies on the instrument configuration, not on the geometries. The cloud mode measured by the CIMEL is as reliable as any point of the PPL or the Almucantar scans. However, for the inversion purposes is much more useful to have a wide range of scattering angles to have more information about the aerosol's scattering.

The "cloud mode" but on clear-sky conditions could be implemented as a routinary measurement in the photometers, and these measurements could be used to retrieve AOD and other aerosol properties applying the same method used in this work with the ZEN-R52 instrument. However, we observed that this method is mainly useful to retrieve AOD values, and the photometer is capable to obtain AOD values in a simple way only pointing to the sun. Then, it has not too much sense to configure photometers to carry out only ZSR measurements.

RC: The authors conclude that the GRASP-ZEN measurements produce reliable volume concentration estimations, but not radii estimations. Is it related with the limitation on the measurements (only vertical) or the inversion procedure (limited to five size distributions)?

AC: In this case is related with both. The scarce of scattering angles (only in the zenith), led to the need to apply an inversion procedure very constrained. This inversion procedure limits the retrieved size distribution (radii and standard deviation) to linear combination of five size distribution. It implies that plausible solutions can be rejected if they are not a combination of these five distributions.

This was mentioned in:

L470-476: *"The reason for the observed overestimation could be in the limitations of the GRASP-ZEN method based on the 'models' approach, which only allows to retrieve aerosol properties within the properties of the five aerosol types. It means that, for example, if the real aerosol has a median radius of fine mode bigger than the ones of the five 'models', then the GRASP-ZEN retrieval will underestimate the real median radius of fine mode and this difference will be compensated unbalancing other aerosol properties to fit the measured ZSR and the synthetic ZSR values of the retrieved aerosol scenario (to reduce the residual differences in ZSR values)."*

We could consider a different inversion procedure, as a bimodal size distribution for the inversion, that could improve the retrieval of aerosol properties. But this possibility is not feasible for the current work since this would imply the retrieval of many parameters, increasing the unknowns, and we still have only four ZSR measurements.

The possible alternatives were also discussed in:

L576-588: *"All the results of this paper have been obtained using the GRASP-ZEN methodology based on the 'models' approach, which is a suitable option for the current study due to the reduced number of radiometric observations provided by the ZEN-R52. However, the versatility of GRASP code allows different strategies for the retrieval of aerosol properties. In this sense, we have considered other strategies in this study to choose the one which provides the best results. These strategies are based on the temporal multi-pixel approach offered by GRASP (Lopatin et al., 2021), that constraints the variation of aerosol properties in time, forcing them to vary smoothly. The multi-pixel approach was firstly used in combination with the 'models' approach. In order to avoid the problems derived of having fixed aerosol models with fixed aerosol properties, the temporal multi-pixel was also used assuming the size distribution as a bimodal (fine and coarse modes) log-normal distribution and the refractive indices have no dependence on wavelength. None of these methods significantly improved the retrieval of aerosol properties; but they did reduce the computation time (the data of a full day are inverted all at the same time). Nevertheless, these strategies could be considered for future aerosol retrievals."*

RC: Secondly, the normalization by extraterrestrial spectra is not clear. The authors conclude (lines 578 & 579) that "proposed methodology incorporates the advantage that it includes the normalization used by GRASP and therefore there is not any need to use extraterrestrial spectra to normalize the data when they are used as input in GRASP". However, section 2.2 "GRASP methodology" seems to imply that normalization is required. Please clarify this issue.

AC: The radiances in GRASP are normalized, so we could directly work with the GRASP normalization when using ZEN-R52 measurements which were calibrated using the methodology described in Section 3 (by comparison with measurements simulated by GRASP). When applying the calibration, the ZSR simulated by GRASP are multiplied by the extraterrestrial irradiance to convert them into radiance units, but we could avoid this step, or revert it applying the same factor to recover the normalized GRASP radiance.

If we had to use sky radiances measured by other instruments or methods, then yes, an extraterrestrial normalization must be applied to the measurements if we want to use them as input to GRASP.

It was mentioned also in lines 298-304, which has been rephrased to:

L298-303: "*This fact can increase the relative differences between the two calibration methods, together with the lack of temperature correction in the second one. However, when using the calibration method developed in this study, the same normalization factor applied to the ZSR simulated by GRASP (ZSR_{SIM}) can be applied to the calibrated ZEN-R52 measurements when using them as input to GRASP for the inversion. This way it can be avoided the introduction of a systematic error due to the normalization required by GRASP inversion algorithm.*"

Also a clarification is included on section 2.2:

L158-162: "*The standard ASTM-E490 solar spectrum has been used in this work for the normalization of Eq. (1). This spectrum was calculated for moderate solar activity and medium Sun-Earth distance; therefore, it has been corrected from Sun-Earth distance for each day of the year. This way, the normalization factor must be applied when using data in radiance units as input to GRASP and to transform the output normalized radiances from GRASP into radiance units.*"

RC: Finally, some technical issues:

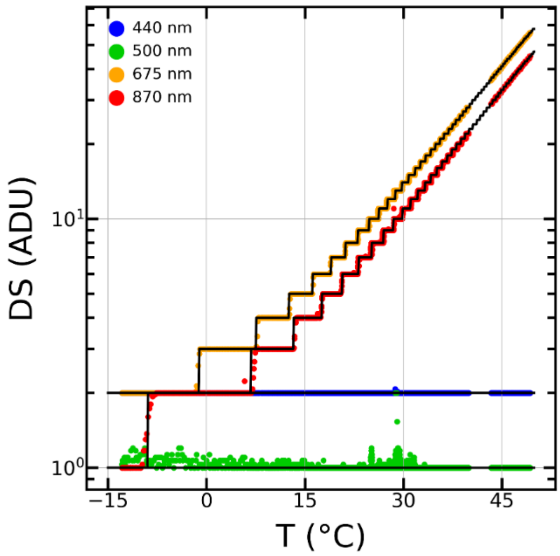
- The procedure described in Section 4.2 AERONET scenarios is not clearly explained. Does the AERONET inversions determine the five aerosol types concentrations? Or the AERONET information is input into GRASP directly?

AC: The AERONET information is input into GRASP directly. This has been added in the new version of the manuscript:

L452-453: *"In this case, the AERONET retrieved aerosol properties (size distribution, refractive indices, etc.) are used directly as input in the GRASP forward module to simulate the ZSR values."*

- Details in Figure 1 are difficult to see. Maybe a logarithm Y-axis would help. Also, the details of the fitting may be added, and a residuals graph, to check randomness.

AC: In order to improve this, Figure 1 has been updated with an y-logaritmic scale as can be seen next.



Regarding the residuals, they have been included in the supplementary material as Figure S2 (see the figure below). The results about residuals have been also discussed in the new version of the manuscript by these new sentences:

L234-236: "*The residuals between the modelled and real DS are shown in the supplementary material (Figure S2); these residual values are within the instrument resolution for all channels.*"

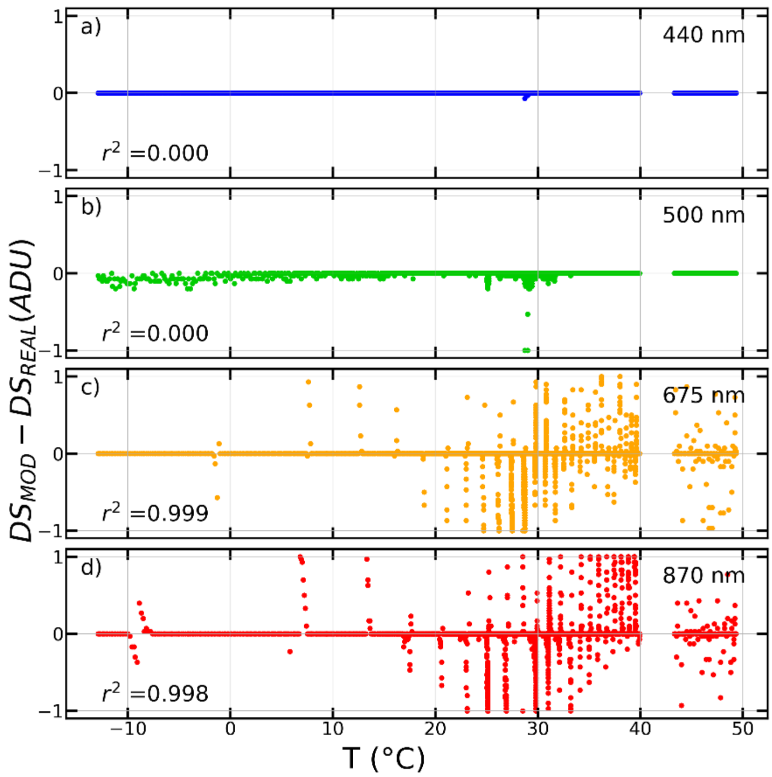


Figure S2. Residuals graph for the dark signal (DS) correction for the thermal chamber period. The differences between the modelled dark signal (DS_{MOD}) and the correspondent real dark signal recorded by the ZEN-R52 (DS_{REAL}) are plotted against the temperature at a) 440nm, b) 500nm, c) 675 nm and d) 870 nm. The determination coefficient (r2) obtained for the direct comparison of the DS_{MOD} against the DS_{REAL} is also included.

RC: L193 How are the fine and coarse modes defined?

AC: The inversion procedure retrieves the proportion of 5 different aerosol size distributions. Each one of these five size distributions is defined for fine and coarse modes, as can be observed in Figure S1. Then, the retrieved fine parameters are the combination of the fine mode of the five aerosol models; the same for the coarse.

To clarify this issue, we added the next in the new version of the manuscript:

L195-199: "*The size distribution of the five models is defined for fine and coarse modes, hence the retrieved parameters are also calculated for these modes. Then, the obtained size distribution parameters are volume median radius of fine (RF) and coarse (RC) modes, standard deviation of lognormal distribution for fine (σF) and coarse (σC) modes, and aerosol volume concentration for fine (VCF) and coarse (VCC) modes and the total value (VCT).*"

RC: L199 But non-convergent cases don't produce simulated ZSR. How is the residual information calculated for those cases?

AC: The non-convergence issue was not clear. All the retrievals provide simulated ZSR. However, we assume that if the residual is higher than a threshold, the retrieval does not show a good convergence (non-convergence cases). Regarding how the residual is calculated, we add the cite of Román et al. (2022), where the residual formula is written. We have tried to clarify these issues about convergence, thresholds and residuals adding the next text in the new version of the paper:

L199-205: "*Each output, one per retrieval, provides the relative residual differences between the measured ZSR (input) and the ones generated after the inversion (simulated by GRASP forward module under the retrieved scenario) for each wavelength (Román et al., 2022). This residual information will be used to evaluate the goodness of the retrievals; if the residual at one or more wavelengths is above an established threshold, the inversion is rejected (assumed as non-convergent). This threshold, which varies with the wavelength, has been set as the absolute value of the accuracy plus the precision for each channel of the ZEN-R52 (see Section 3.5.2).*"

And also the next:

L372-373: "*These accuracy and precision values will be used in the convergence criteria mentioned in Section 2.2.2.*"

RC: L203-204 Is this study published? In such case, add reference

AC: It is not published, it is just a simple monthly climatology calculated for this study. It has been obtained using all data available in AERONET in Valladolid for the 2012-2021 period as it is mentioned in the manuscript.

RC: L225-226 Add details of fitting to the figure. Also, figure 1 may be more interpretable with y-axis in logarithm scale.

AC: These changes have been applied as has been commented above.

RC: L309-310 Why is it necessary to download them from AERONET webpage? Aren't they available from the instrument memory?
& L334-335 Again, why download from webpage is required? Shouldn't these data be available within the instrument?

AC: The memory within the instrument is limited, especially older instruments, which are the ones measuring at these geometries. Anyway, the raw data is frequently stored and sent to CAELIS and AERONET databases. These instruments record only raw data, it implies that we cannot obtain the sky radiance directly from the instrument memory. Then, the radiances cannot be directly obtained from instrument memory since we need to know the calibration coefficients. The data of all photometers belonging to AERONET is sent to AERONET server; AERONET also knows the calibration coefficients since these photometers are calibrated following the AERONET procedures. AERONET provides the well calibrated photometer products like sky radiance. Therefore, it is more comfortable and simpler to download the data directly from AERONET (warrantying the data quality assurance), specially for researchers that do not participate in the photometer calibration tasks. Fortunately, our group, within ACTRIS/AERONET-Europe branch, participates in the AERONET calibration tasks of part of the photometers of the network. It helps to know the calibration coefficients, which are stored in CAELIS; this makes that we have access to sky radiance values in the principal plane directly from CAELIS.

RC: L316 Only the sun, or the sky?

AC: Clouds in the sky may affect the ZSR measurements, especially if they are in the zenith. For that the ZEN error (now renamed as ZEN variability as suggested by the reviewers) mentioned in section 3.2 is used, since clouds strongly affecting the measurement would produce a high ZEN variability, due to the high variability of the measurements within the minute, and therefore the consequent high standard deviation. But in this case, we are referring to the observed outliers for SZA<30º in Figure S3, that are due to sun stray light as confirmed here since we do not observe those outliers when there is an obscured sun like in the Cloud Mode observations. Therefore, we assume it is only the sun.

RC: L380 This sentence isn't clear. An extreme AOD event should be AOD@440nm above 0.7, not below. Please rephrase more clearly.

AC: This sentence has been rewritten as next:

L404-407: "*Nevertheless, for lower SZA conditions (Figure S5; panels a-d) there is a clear sensitivity to type and aerosol load for AOD at 440 nm, at least for values below*

0.7; values above 0.7 are assumed for extreme AOD events (Mateos et al., 2020) and therefore are unusual".

RC: L410 Are all the scenarios real, in terms of aerosol concentration of all types? Would a classification of the scenarios (Saharan intrusions for those with high dust concentration, Urban pollution event for those mostly influenced by urban aerosol type etc...) assist in the interpretation of convergent inversions?

AC: This should not be a problem for the convergence. In this section we have used the same scenarios for different SZA and the amount of convergence values has changed, so it should to be related with the SZA. Nevertheless the 'models' strategy from GRASP does not really care about the amount of each model, it just uses common aerosol properties and tries to replicate the observed measurements adjusting the fraction of each model and the total concentration. If it is able to reproduce the forward, it should be able to conduct the inversion.

RC: L425-427 This procedure is not clearly explained. Does the AERONET inversions determine the five aerosol types concentrations? Or the AERONET information is input into GRASP directly?

AC: AERONET information is input into GRASP directly. It has been clarified in the text with this new sentence:

L452-453: "*In this case, the AERONET retrieved aerosol properties (size distribution, refractive indices, etc.) are used directly as input in the GRASP forward module to simulate the ZSR values*".

RC: L443-445 But if the five aerosols types can explain the AERONET measurements, the scenario should be reproducible. Please describe this procedure in more detail, as mentioned in the previous comment.

AC: When we do the forward simulations with a RTM we are in a direct problem, that has only one solution. This correspond to the sentence added in the comment before: "*In this case, the AERONET retrieved aerosol properties (size distribution, refractive indices, etc.) are used directly as input in the GRASP forward module to simulate the ZSR values*".

But when we do the inversion there might be a high number of scenarios that would reproduce the same measurements, hence the importance of constraining the solutions. In this case we can reproduce the sky radiance measurements and even the AOD, but the microphysical properties are not reproducible because we are employing only five fixed aerosol types for the inversion. For example, if the coarse radius of the AERONET scenario is out of the achievable values using the combination of the five models, this under or overestimation could be compensated with a under or overestimation of the refractive index (or any other combination of aerosol properties providing an effective solution); this retrieved scenario could reproduce the measurements, even the AOD, but the aerosol microphysics properties are only effective, not necessarily the real. Anyway, the five aerosols are not completely explaining the AERONET measurements since an overestimation of the inverted AOD values can be seen.

This limitation is explained in:

L470-476: *"The reason for the observed overestimation could be in the limitations of the GRASP-ZEN method based on the 'models' approach, which only allows to retrieve aerosol properties within the properties of the five aerosol types. It means that, for example, if the real aerosol has a median radius of fine mode bigger than the ones of the five 'models', then the GRASP-ZEN retrieval will underestimate the real median radius of fine mode and this difference will be compensated unbalancing other aerosol properties to fit the measured ZSR and the synthetic ZSR values of the retrieved aerosol scenario (to reduce the residual differences in ZSR values)."*

RC: L474 Please relate with dates and number of measurements

AC: Dates are the correspondent to the total available period of ZEN-R52 measurements, April 2019 to September 2021. It has been added in the new manuscript version:

L500-504: "*Once the ZSR_{ZEN} measurements have been calibrated, and the GRASP-ZEN method has been proved in Section 4 as capable to retrieve aerosol properties, the GRASP-ZEN methodology has been applied to the whole available dataset of ZEN-R52 measurements at Valladolid at the moment of the study. As result, a total of 222663 GRASP-ZEN retrievals have been obtained between April 2019 and September 2021.*"

RC: L525-531 The AERONET instruments perform PPL and almucantar measurements to obtain angle information, while the ZEN instrument only measure vertically. Please comment on this different strategies and their effect on the inversions. For instance, would CIMEL vertical measurements (like "Cloud mode" but on clear-sky days) be as reliable as PPL and almucantar?

AC: This comment has been answered above.

RC: L577-579 This is not mentioned before. It is not clear why no extraterrestrial spectra is needed since it is mentioned in section 2.2. Please clarify this statement

AC: This comment has been answered above.

RC: L583 On section 4, lines380, says below 0.7. Why has it changed?

AC: On section 4 it referred to SZA = 70º, and in this sentence it is for SZA≤50º. It was not clear then now it is detailed:

L603-606 "*An analysis with synthetic data has concluded that ZSR measurements are useful to derive aerosol optical depth (AOD), since these measurements are sensitive to aerosol load and type for the ZEN-R52 channels, at least for AOD at 440 nm below 1 for SZA ≤ 50º*".

RC: L606 Expanding from the previous comment about the difference in strategies between the AERONET (PPL and almucantar measurements to obtain angle information) and ZEN (only measure vertically), can the authors comment on the properties (volume concentrations, refractive indices, asymmetry parameters) that can be obtained and possible limitations?

AC: As mentioned on section 4.2, the retrieval of aerosol properties is limited to the properties of the five types of aerosols used for the inversion. They possess fixed properties, so the resulted aerosol must be a linear combination of them. For the volume concentration there should not be more limitations than the one derived from the fact of using fixed types of aerosol that can influence the inversion as mentioned on lines 469-475.

RC: General typing errors:

AC: All these typing errors have been corrected.

Response to the Anonymous Referee #2 comments for the manuscript "Retrieval of aerosol properties from zenith sky radiance measurements" By Sara Herrero-Anta et al. in AMT

First of all, we would like to thank the time and effort of the referee for their 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 #2

RC: 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 modelling 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:
RC1. 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.
AC: Following the referee comment, the English of the manuscript has been reviewed and improved in the new version of the manuscript.

RC2. 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.
AC: This work presents three well established targets: calibration of the instrument, to study the capability of the inversion strategy, and to apply the method to a real measurement database. For that, we decided to divide them in three sections. As referee says, all the three sections present results, then we consider that a section called "Results" has not sense. Then, the Section 5 has been renamed as "GRASP-ZEN application to

ZEN-R52 database" instead of "Results". With this change, we think that has not sense to add Section 3.5 to Section 5, since the comparison of sky radiances in the "ZEN-R52 vs. CE318 photometer comparison" does not fit with "GRASP-ZEN application to ZEN-R52 database".

RC3. 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.
AC: The total period of ZEN measurements in this work is used for the calibration and also for the retrieval of aerosol properties. It has been added at the beginning of Sections 3 and 5 adding: *"April 2019 to September 2021"*

This is possible because different information is used for each part, so it should not affect; calibration is based on almucantar/hybrid retrievals while Section 5 is mainly based on AOD data (sun measurements instead of sky radiances).

Specific comments:
RC: Line 20: AERONET has been named as Aerosol Robotic Network or AErosol RObotic NETwork, please homogenize.
AC: It has been homogenized to Aerosol Robotic Network.

RC: Line 25: pre-calculated.
AC: Done

RC: Line 38: no significant.
AC: Done

RC: 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.
AC: The referee was totally right. It has been corrected, the correct reference is: Forster et al., 2021:
Forster, P., T. Storelvmo, K. Armour, W. Collins, J.-L. Dufresne, D. Frame, D.J. Lunt, T. Mauritsen, M.D. Palmer, M. Watanabe, M. Wild, and H. Zhang, 2021: The Earth's Energy Budget, Climate Feedbacks, and Climate Sensitivity. In Climate Change 2021: The Physical Science Basis. Contribution of Working Group I to the Sixth Assessment Report of the Intergovernmental Panel on Climate Change [Masson-Delmotte, V., P. Zhai, A. Pirani, S.L. Connors, C. Péan, S. Berger, N. Caud, Y. Chen, L. Goldfarb, M.I. Gomis, M. Huang, K. Leitzell, E. Lonnoy, J.B.R. Matthews, T.K. Maycock, T. Waterfield, O. Yelekçi, R. Yu, and B. Zhou (eds.)]. Cambridge University Press, Cambridge, United Kingdom and New York, NY, USA, pp. 923–1054. https://doi.org/10.1017/9781009157896.009

RC: Line 47: … processing and data(?) distribution.
AC: Done

RC: Line 48: manufactured by Cimel Electronique SAS.

AC: Done

RC: Line 50: (or lunar, if available).
AC: Done

RC: Line 51: I will include a full stop here: This is achieved by applying…
AC: Done

RC: 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.
AC: This sentence has been changed following the referee suggestion.

RC: 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…
AC: This sentence has been rephrased as:

L61-62: "*In this regard, some authors have utilized GRASP to retrieve aerosol properties using as input, among others, data from:*"

RC: Line 66: manufactured by …
AC: Done

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

RC: 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.
AC: This sentence has been rephrased as:

L72-74: "*One advantage of this instrument is that it does not have moving parts and is cheaper than more complex photometers. This affordability enables the installation of multiple instruments, thereby achieving a higher spatial coverage*".

RC: Line 80: The following paper?
AC: "following" has been changed by "current".

RC: Line 81: study location?
AC: "as well as the study location" has been changed by "as well as a description of the site".

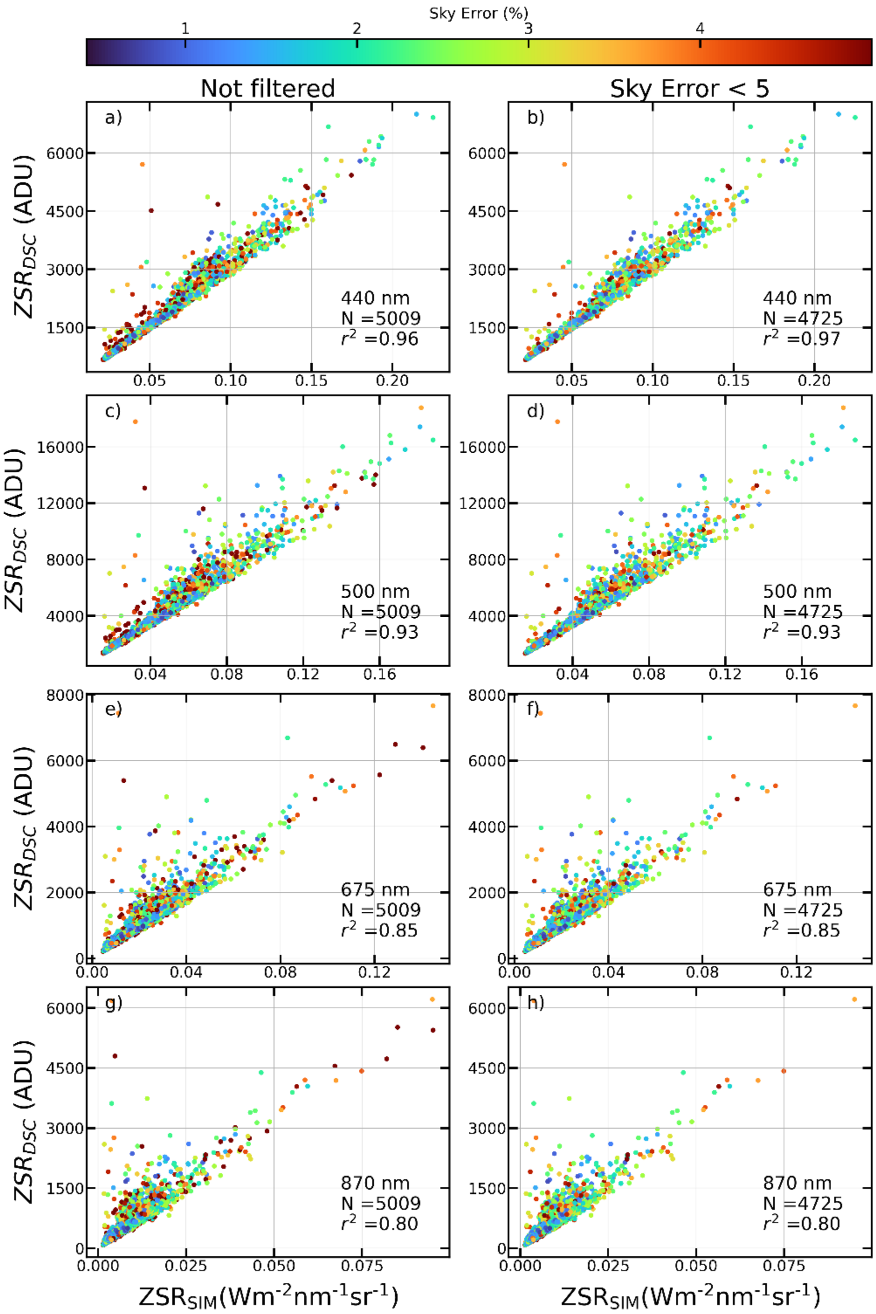
RC: Line 106: almost always?
AC: Replaced by only "always".

RC: 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?

AC: When level 2.0 is available in AERONET, level 1.5 is directly updated to 2.0, but if it is not, it remains at 1.5. The difference between 1.5 and 2.0 is only the application of the last calibration in the latter, which occurs once a year, so usually last year of measurements are only available at level 1.5 as it occurred by the time of the study with year 2021, but the rest of the years correspond to 2.0. This 1.5 level is also quality assured.

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

AC: Actually it is 5%, it has been changed it. As a preliminar option it was selected the limit 10%. After that, a visual analysis as the ones done for SZA or ZEN variability in Section 3.2 was carried out for the sky error. We could see most points had a good correlation except with some points with sky error > 5% (red points), so we stablished the final limit at 5%. This part has not been included for the simplification of the section.



RC: Line 131: Each filter is placed over…
AC: Done

RC: 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…
AC: As mentioned in the paragraph, it can be calculated from field measurements using the temperature information provided by the ZEN, using night-time measurements or even a full-day measurements using a dark piece to cover the instrument. The paragraph has been rewritten as next to clarify these issues:

L214-217: "*A methodology for the ZEN-R52 calibration is proposed in this Section. This methodology can be developed using only field measurements, so it would not require laboratory measurements. It is based on four steps: dark signal correction, quality data filtering, temperature correction, and a final comparison against simulated values to convert the output signal from ADU into radiance units ($Wm^{-2}nm^{-1}sr^{-1}$)*".

RC: 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?
AC: It is recommended to include both corrections.

RC: Line 233: The colour of the points…
AC: Done

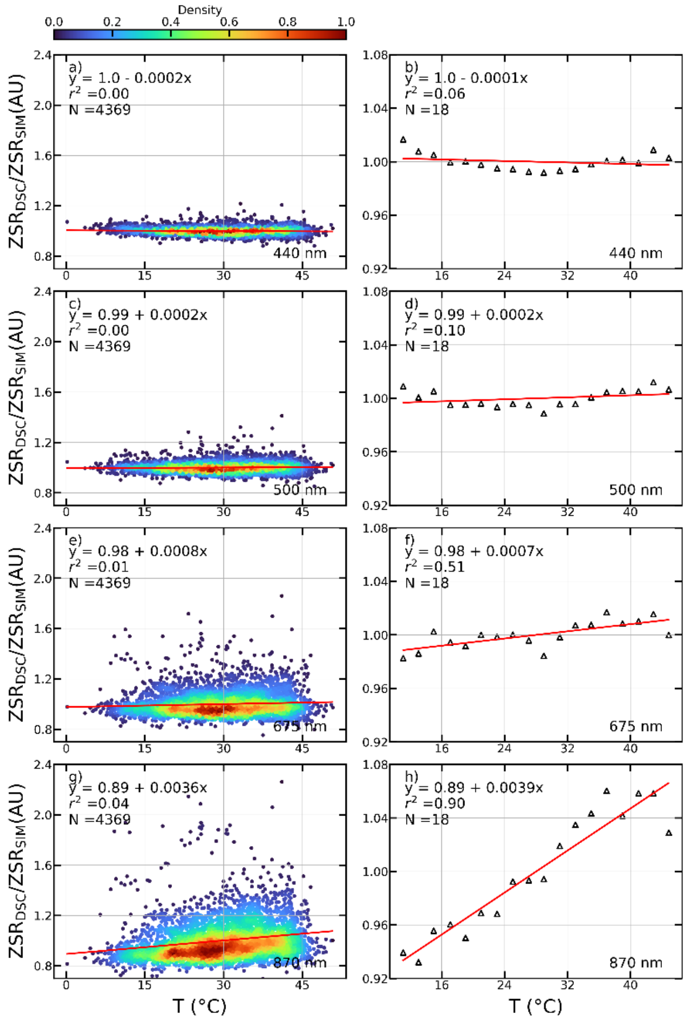
RC: Section 3.3: I do not understand how the temperature correction has been done. The authors have the information of real ZSR measurements and simulated values when the ZEN system is inside the thermal chamber? Could you please clarify?
AC: The thermal chamber is only used for the dark signal evaluation in section 3.1. For the temperature correction the ZSR_{DSC}/ZSR_{SIM} ratio normalized to the mean value is plotted against the temperature in Figure 3. It is explained in the new manuscript:

L269-270: "*In order to check the dependence with temperature of each channel the ratio ZSR_{DSC}/ZSR_{SIM} normalized by the mean ratio has been plotted against the temperature in Figure 3.*"

Where ZSR_{DSC} are the ZSR raw signal from the zen with dark signal corrected (DSC) and ZSR_{SIM} are the ZSR simulated by GRASP. As mentioned, we also are excluding those points which do not satisfy the quality control filtering calculated in section 3.2:

L307-309: "*From now on ZSR_{ZEN} will stand for the calibrated zenith sky radiances measured by the ZEN-R52 satisfying the stablished quality controls (30º < SZA < 80º; ZEN variability < 4%).*"

RC: The different scales of the plots in Figure 3 prevents us for discerning the trends described in the paper.
AC: This figure has been updated as referee suggests. It is shown next:



RC: 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.

AC: It has been changed by the next:

L270-272: "*In the left panels (a, c, e and g) of Figure 3 all data points are represented together with the linear fit, showing a negligible dependence on temperature for 440 and 500 nm*"

RC: Line 269: Please remove indent.

AC: Done

RC: Line 272: Please remove "λ-wavelength". It is redundant.

AC: Done

RC: 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.

AC: The sentence including the "definitive comparison" has been simplified, removing the term 'definitive', with:

L287-288: *"The calibration factors can be directly obtained by comparing the dark and temperature corrected ZSR from the ZEN-R52 against the values simulated by GRASP"*

As mentioned above the whole dataset is used all the time.

RC: Lines 287-290: Could the authors clarify why systematic errors are expecting in case of sphere calibration is used (provided the same E0 for normalization is used)?
AC: This section has been updated since it looks to be not so clear. Due to the normalization used by GRASP, if radiance data is being used to GRASP it needs to be normalized, therefore including a systematic error due to this normalization. But if the measurements are obtained from GRASP, we are not including are using the same normalization or not normalization at all if we already use the normalized radiances from GRASP.

This paragraph has been rephrased as:
L296-303: *"The proposed calibration method uses the standard ASTM-E490 solar spectrum to transform the unitless output radiances from GRASP, as indicated in Equation 1. This fact can increase the relative differences between the two calibration methods, together with the lack of temperature correction in the second one. However, when using the calibration method developed in this study, the same normalization factor applied to the ZSR simulated by GRASP (ZSR_{SIM}) can be applied to the calibrated ZEN-R52 measurements when using them as input to GRASP for the inversion. This way it can be avoided the introduction of a systematic error due to the normalization required by GRASP inversion algorithm."*

RC: Section 3.5: Similar problem with the time period.
AC: In the new manuscript it is clearer that all the available dataset has been used with different purposes, including the time period *"April 2019 to September 2021"*.

RC: Line 296: Please correct the typo in "whole".
AC: Done

RC: Line 308: Please remove the final comma.
AC: Done

RC: Line 316: Can you add more information about the considered "wrong" values? Some statistics can help to understand why the values are wrong.
AC: "Wrong" meaning that these points (SZA<30º) do not correlate with the reference values. This sentence has been rephrased as:

L331-334: *"Hence, the ZSR_{ZEN} values do not correlate with reference values for SZA<30º when the sun is cloud-free, which confirms the suggested explanation that ZSR_{ZEN} measurements are contaminated by stray sun light under cloud-free conditions when the sun elevation is high (SZA<30º)"*

RC: Line 332: Please add a point missing.
AC: Done

RC: Line 340: Why this paragraph has a different indent?
AC: This paragraph has been homogenized with the others.

RC: 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?
AC: In the new version of the manuscript it has been clarified the period (April 2019-september 2021). As suggested by the referee, the same QC is used here. To avoid confusion this paragraph has been rephrased as:

L504-505: *"This dataset has been obtained using ZSR_{ZEN} measurements which satisfy the filtering criteria determined in Section 3.2"*

RC: Line 526: Please include a comma after because.
AC: Done

RC: Line 532: VCT, VCC and VCF.
AC: Done

RC: Line 546: …a r2 of about …
AC: Done

RC: 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.
AC: We acknowledge the comment, but we believe that including this paragraph provides relevant information on how to address the same problem we have faced using alternative strategies. Moreover, it briefly presents interesting results that, we consider, do not distract the readers from the main work.

RC: Line 584: A couple of tests?
AC: It has been changed by "Two different tests".

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

RC: 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…
AC: Referee is totally right. It is added in the conclusions:

L634-635: *"This methodology also represents a major advance over the former ZEN-LUT proposed by Almansa et al. (2020) for aerosol properties retrieval, since it is not linked to the place of study."*

RC: 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?

AC: We have assumed that during the period of study the calibration has not decayed, since it is not a long dataset. However, a recalibration must be considered, especially if there is any maintenance or repair task.

It has been included in:
L305-307: *"For this work, it has been assumed that during the period of study the calibration has not decayed, since it is not a long dataset. Nevertheless, a recalibration must be considered, especially if there is any maintenance or repair task."*

RC: Figure 4: The x-labels should be "ZSR_DSC_T"
AC: Done

RC: General things in the text: Please add a space before "nm" and correct the degree sign.
AC: Done

Response to the Anonymous Referee #3 comments for the manuscript "Retrieval of aerosol properties from zenith sky radiance measurements" By Sara Herrero-Anta et al. in AMT

First of all, we would like to thank the time and effort of the referee for their 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

RC: 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:

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

AC: A short indication has been added in the abstract:

L34-36: '*The comparison against independent values from AERONET presents r^2 values of 0.57, 0.56 and 0.66, and uncertainties with values of 0.009, 0.016 and 0.02 $\mu m^3/\mu m^2$ for VCT, VCF, VCC respectively*'.

RC: Line 263: It is stated that a "clear trend" is shown in Figure 3 e and g. However, those fittings only present r2 = 0.01, so in my view this is not a clear trend at all. I agree that 675 nm and 870nm 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.

AC: We agree with the referee, and we consider that it is not a clear trend. The new sentences are:

L270-275: "*In the left panels (a, c, e and g) of Figure 3 all data points are represented together with the linear fit, showing a negligible dependence on temperature for 440 and 500 nm. For 675 and 870 nm channels this dependency presents slopes of the linear fitting of 0.008 ºC^{-1} and 0.0036 ºC^{-1}, respectively. These values are higher than the 0.0002 ºC-1 obtained for the other two channels, which led us to consider a temperature correction for 675 and 870 nm.*"

RC: Figure 4 and line 861: Here the acronym ZSRDSC_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 ZSRTC. Please homogenize the acronyms to avoid confusion.

AC: It has been homogenized to ZSRTC following the referee comment.

RC: 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 ZSRSIM will be then applied to the calibrated ZSRZEN 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.

AC: This sentences have been rephrased to make it more understandable as:

L299-305: "*However, when using the calibration method developed in this study, the same normalization factor applied to the ZSR simulated by GRASP (ZSRSIM) can be applied to the calibrated ZEN-R52 measurements when using them as input to GRASP for the inversion. This way it can be avoided the introduction of a systematic error due to the normalization required by GRASP inversion algorithm. It means that this calibration method is better suited when using the ZSRZEN values as input for GRASP to retrieve aerosol properties, since we could work directly with the normalized radiances from GRASP.*"

RC: Section 3.5: it is stated that the ZSRZEN 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.

AC: This comment is entirely correct. We primarily used the CM comparison to verify the source of errors for some SZA when the sun is not obstructed by clouds and observe the calibration behaviour qualitatively. However, to accurately quantify the uncertainty of the ZEN measurements we believe it is essential to exclusively consider measurements taken in the absence of clouds. This is mainly because, in this study, we are only utilizing measurements under cloud-free conditions for the retrieval of aerosol properties. In addition, the ZSR measurements strongly vary in short time periods under the presence of clouds due to the variability of clouds. This fact Implies that for a good comparison between ZEN and CIMEL measurements under cloudy conditions, the time of both measurements should be exactly the same. Unfortunately, it is not possible, the ZEN measurements are an average along one minute, while CIMEL measurements are quasi-instantaneous.

To clarify this, the following paragraph has been added in section 3.5.1:

L338-344: *"This comparison against the cloud mode measurements will not be used to quantify the uncertainty of the ZEN measurements; it is because clouds are very variable and, therefore, the recorded signal. Therefore, we should need to compare both measurements carried out at exactly the same time; but this is not the case since ZEN measurements are 1-min averages while CE318 photometer measurements are quasi-instantaneous. In addition, for the retrieval of aerosol properties, it is necessary to employ measurements under cloud-free conditions, therefore, the results obtained in following comparison will be the reference ones."*

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

AC: Yes, they are correct, a mistake was in the figure. It has been changed in the new version of the manuscript.

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

AC: The correct value is the one appearing in the Fig. 8. This error has been corrected in the new version of the manuscript.

RC: 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%).

AC: Regarding extensive properties, such as volume concentration, we are focusing on absolute differences rather than relative differences. This is because these magnitudes can reach very small values, leading to significant percentage differences even when the actual difference is relatively small. Thus, if these values are considered, the results are acceptable. However, for the radius and standard deviation, the possible values are bounded, making it more appropriate to analyse the results considering percentage differences.

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

AC: This has been removed in the new version of the manuscript.

RC: 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.

RC: 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.

AC: We understand the comments, but the uncertainties obtained through the synthetic analysis may not correspond to the actual uncertainties. In the synthetic analysis, for instance, the same radiative transfer model is used in both the forward and inversion parts. As a result, the uncertainty in the radiative transfer model itself is not taken into account in the synthetic study, but this uncertainty is propagated in the results when real measurements are inverted. Therefore, we consider it more appropriate to discuss about 'uncertainty' when comparing against AERONET products, which are realistic, especially AOD. To differentiate between the uncertainties in both cases, we have renamed the uncertainty obtained in the synthetic study by "theoretical uncertainty" and we have tried to add comments about if the obtained results are within or without the theoretical uncertainty.

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

AC: This sentence has been modified as next in order to be more clear:

L585-587: "*None of these methods significantly improved the retrieval of aerosol properties; but they did reduce the computation time (the data of a full day are inverted all at the same time).*"

RC: 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.

AC: We agree with the reviewer, hence we have renamed 'Results' Section as 'GRASP-ZEN application to ZEN-R52 database'.

RC: 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 μm3/μm2 according to the plot

AC: All these typos have been corrected.

Response to the Anonymous Referee #4 comments for the manuscript "Retrieval of aerosol properties from zenith sky radiance measurements" By Sara Herrero-Anta et al. in AMT

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

<u>Author's answer to Anonymous Referee #4</u>

RC: This manuscript describes the retrieval of aerosol properties with the synergetic use of zenith sky radiance measurements at 4 wavelengths and GRASP.

The instrument used ZEN-R52 radiometer, is a simple radiometer with uncertainties arising mainly from the temperature dependencies. The authors have done a laboratory characterization of the instrument, introduced corrections and accessed their uncertainties, which were further used in the inversion algorithm.

This paper provides detailed information for the normalization, validation and inversion strategies followed.

This work provides new insights in the possibility of retrieving aerosol properties (AOD, VCT, VCC and VCF) using a simple measurement geometry and skipping the laboratory radiance calibration (or using it for stability monitoring purposes). The results are very promising, and the authors provide information to the community of possible improvements through their validation against AERONET products and uncertainty budget.

The manuscript is clear, but it would improve, if a more concisely writing style was used.

I propose that this article is accepted for publication, after minor revisions.

RC: L131 Silicone diode sensitivity range: 180 nm to 1100 nm

AC: The clarification of the sensitivity range is irrelevant for the paper and it has been removed in the new version of the manuscript.

RC: L134 is this the plateau of the FOV or the FWHM?

AC: It is the FOV.

RC: L142-143 the software name is misleading. I would suggest to rename it to combined variability since it describes both the atmospheric variability and the noise of the ZEN.

AC: The software does not have a specific name, it is just the software of the instrument. This software gives the parameter as ZEN error, so we used it like that, but we agree that

ZEN variability is more appealable. To clarify it we have modify the description of the parameter as follows:

L144-146: "*For each measurement, it is also provided a variability parameter (ZEN variability) that describes both the atmospheric variability and the noise of the ZEN-R51 within the minute of measurement, which is calculated as the standard deviation of the 30 samples.*"

RC: L157 and?

AC: We think "Therefore" fits better than "and".

RC: L164 (NO2 and O3)

AC: The GOD includes the gases given by AERONET: O3, NO2, CO2, CH4 and Water Vapor.

RC: L207 (Section 3 calibration) A comparison to a RT model is not a calibration procedure. This section should be renamed to "Normalization to GRASP forward model"/ "Responsivity to GRASP forward model radiance" to. A calibration would be the comparison of co-located, synchronous zenith radiance measurements to a laboratory calibrated instrument (eg section 3.5), accounting for the uncertainties of differences in wavelength, FOV, extrapolation,...

AC: Although this calibration method may not be conventional, we think it can be considered a calibration method since, in this case, model data are used as a reference or standard instead of another instrument, and these model data do are co-located and synchronous. In fact, as demonstrated in this study, by applying this methodology, the raw signal from the ZEN instrument is transformed into physical units of radiance ($Wm^{-2}nm^{-1}sr^{-1}$), which is equivalent to what is achieved with a traditional calibration. Therefore, we consider the method well-defined as calibration, even though we understand it may generate potential confusion due to its more unconventional nature as a calibration method.

RC: L208-212 This normalization methodology requires dark, temperature corrected and quality assured signals over the analysed period. There is limited effort in the laboratory characterization, since the radiance calibration is replaced by the RT model. However, laboratory test are done and presented in 3.1, 3.3.

AC: Laboratory measurements have been conducted only for Section 3.1, but it is clarified that it can also be realized using nigh-time measurements or even measurements during day using a dark cover for the instrument. No laboratory measurements have been conducted in Section 3.3 for the temperature correction, it is used the normalized ZSR_{DSC}/ZSR_{SIM} ratio.

To clarify that, the next sentence has been modified:

L238-240: "*In this work, the DS has been characterized in the laboratory to cover a wide range of temperatures, but it could be calculated from the night-time measurements (dark sky) or even from day-time measurements (covering the instrument with a black piece), when a thermal chamber is no available.*"

RC: L223 negligible temperature dependency (<1%)

C: It has been replaced by "negligible dependence on temperature".

RC: L224 steep?

AC: "Steeped" has been replaced by "staggered".

RC: L228-230 Is the dark signal level of the instrument constant over time? Is this monitored through the night-time measurements?

AC: Here it can be seen a residuals graph for the differences between the modelled (using the laboratory data) and the raw real ZEN signal for night-time measurements for the whole study period, April 2019 to September 2021. The residuals are consistent for the whole dataset, being mainly within +-1 DC, so we can consider the modelled dark signal represents well the dark signal for the all the period. It means that dark signal did not vary over the period.

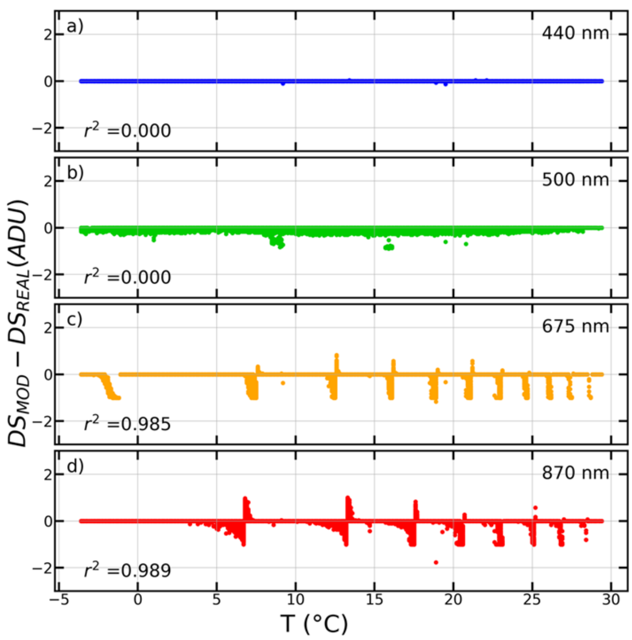


Figure: Residuals graph for the dark signal (DS) correction calculated for the whole study period, April 2019 to September 2021. The differences between the modelled dark signal (DS_{MOD}) and the correspondent raw real dark signal recorded by the ZEN-R52 (DS_{REAL}) are plotted against the temperature at a) 440nm, b) 500nm, c) 675 nm and d) 870 nm. The determination coefficient (r^2) obtained for the direct comparison of the DS_{MOD} against the DS_{REAL} is also included.

To point out that, the next sentence has been added in the new version:

L236-238: '*It has also been verified that the dark signal behaviour has remained constant over time, comparing the modelled DS against the nigh-time measurements*'

RC: L234 λ is used later as wavelength, please consider changing he letter or even skipping the information about the smoothing.

AC: This has been removed to avoid confusion.

RC: L241-242 It would be easier for the reader if the physical parameters are described here and explained later. e.g. atmospheric variability, stray-light, uncertainties in temperature correction.

AC: These sentences have been rewritten following the reviewer's comment as follows:

L250-255: '*The ZEN-R52 measurements can be affected in different ways. For example, the possible sun stray-light intromission when sun is very elevated can increase the measured signal, clouds presence can also alter it, or the variation in temperature can introduce some dependency. To identify and reject the cloud-contaminated or wrong measurements, different thresholds have been identified after the visual analysis of some parameters in scatter plots. For the SZA, the signal of the instrument is higher than expected for SZA values below 30º, which could be explained by sun stray-light intromission.*'

L263 typos

This has been corrected changing 'Despise outliners' by 'disregard outliers'.

RC: L266-267 Please rewrite it clearer eg: are used for the temperature dependency correction following Equation 2

AC: Done

RC: L274-276 I would suggest simplification of the sentence. eg the "calibration" factor are obtained by comparing the dark and temperature corrected QA signals of ZEN to ...

AC: It has been simplified by:

L287-288: '*The calibration factors can be directly obtained by comparing the dark and temperature corrected ZSR from the ZEN-R52 against the values simulated by GRASP*'.

RC: Section 3.5.1 The geometry of the "Cloud Mode" is identical to the ZEN, introducing less uncertainty in the comparison with respect to PPL method. However, the high variability of the data set used results in high uncertainty in the comparison. It would be worth while to perform the analysis using the strict QA criteria or limit the discussion in the validation of the cloud screening thought the std of the 30.

AC: If we applied the QA criteria at this comparison, we would eliminate most of the ZEN data, since we are under the presence of clouds and that results in a high ZEN variability associated to those measurements. Furthermore, the CIMEL measurement is quasi-instantaneous, while the ZEN measurement represents a one-minute average, so in the presence of clouds, conditions can vary significantly within that minute. Therefore, it is expected to observe a high level of uncertainty in the comparison between both datasets.

In order to clarify that, the next paragraph has been added:

L338-344: "*This comparison against the cloud mode measurements will not be used to quantify the uncertainty of the ZEN measurements; it is because clouds are very variable and, therefore, the recorded signal. Therefore, we should need to compare both measurements carried out at exactly the same time; but this is not the case since ZEN measurements are 1-min averages while CE318 photometer measurements are quasi-instantaneous. In addition, for the retrieval of aerosol properties, it is necessary to employ measurements under cloud-free conditions, therefore, the results obtained in following comparison will be the reference ones.*"

RC: L348 it seem that the distributions start deviating from the assumed normal one for 675 nm and especially 870 nm. A better stimulation of the distribution would give more representative values for these wavelengths.

AC: It is true that these distribution looks like a distribution with positive skewness, especially for 675 and 870 nm. But we consider the values in the tail are due to occasional malfunctions of this instrument at those wavelengths. Then, if we neglect these outlier values, we can assume a normal distribution. In fact, the use of the median instead of the mean is to neglect these values. Regarding the standard deviation, it is calculated with these outliers, which provides a higher value, but we prefer to be conservative assuming a lower precision than the expected.

RC: L349-350 when was the IARC calibration performed and at which temperature? it is erroneous to apply a calibration factor applying different corrections than those in the calibration procedure. This paragraph doesn't add some information since the calibration and "normalization" factors can be directly compared.

AC: We are comparing the calibration factors obtained using two independent calibration methods, the one proposed here, and the one described by Almansa et al. (2020) which has been called IARC in this work. IARC calibration method has no additional corrections (no temperature correction, no dark signal removal), the calibration factors obtained by IARC are directly applied to the ZEN-R52 raw signal. Maybe the IARC calibration could be improved considering dark and temperature corrections, but it is out of the scope of the paper.

Anyway, the similarity of the results between our calibration method and the IARC method demonstrates the quality of the calibration method proposed.

RC: L358-365 Information of the impact of the uncertain of each channel, is repeatedly mentioned. Please consider simplifying the paragraph.

AC: The paragraph has been simplified as follows:

L384-390: "*These results indicate that the ZEN-R52 measurements are more reliable at shorter wavelengths and, therefore, should be given more importance than those corresponding to longer ones in the retrieval of aerosol properties. The inversion module from GRASP code considers the importance of each measurement through the so-called 'noises'; allowing to associate a different 'noise' or reliability to each channel. The standard deviations collected in Table 2 (using the calibration proposed in this work), associated with the ZSR_{ZEN} uncertainty, are used to this end in the GRASP-ZEN method.*"

RC: L384-385 of ZEN-R52 for these scenarios (ZSRSYN).

AC: This sentence has been modified as follows:

L409-411: "*For both tests, synthetic aerosol scenarios have been created and used as input to the GRASP forward module to simulate the ZSR of the ZEN-R52 under these scenarios (ZSR_{SYN}).*"

RC: L527-531 Not relevant to the publication

AC: It has been removed.

RC: L534 'overstimates'

AC: It has been replaced by 'higher than'.

RC: L537-538 'a match-up has been done. In this case, the GRASP-ZEN values closest to the AERONET values within 5 minutes are chosen,'

AC: The next sentence has been added instead:

L559-561: "*For a more quantitative analysis of the correlation between VCF, VCC and VCT from GRASP-ZEN and AERONET datasets a synchronization with a time window of ±5 min was done, obtaining a total of 4356 coincident points for each volume concentration.*"

RC: L552 'offered by'

AC: It has been changed by “of the AERONET products”.

RC: L554 ‘issue’

AC: It has been changed by ‘study’.

RC: L568 retrieve a normalization factor converting the ZEN-R52 signal to radiance

AC: We prefer to maintain the calibration word as expressed above.

RC: L573 a substantial amount of

AC: Done

RC: L601 lower

AC: It is low, since we are referring to the fact they are indeed low, not a comparison with anything else.