Summary of the changes made in the manuscript

In response to the comments of both Reviewers, the manuscript has been further revised. The main changes introduced in the revised version are summarized below:

- The description of the typical particle size (TPS) parameter has been clarified. The proprietary nature of the TPS retrieval within the SPS30 firmware is now stated early in the Methodology and clearly separated from the subsequent conversion of TPS to SAE and R_{eff}, which is defined explicitly by the equations given in the manuscript. It is now stated that the polynomial calibration relationship was determined empirically from the calibration data rather than derived from an assumed analytical form.
- The calibration status of the $PM_{2.5}$ and PM_{10} mass concentrations has been addressed explicitly. The manuscript now states that $PM_{2.5}$ is calibrated by the manufacturer, that PM_{10} is derived from the SPS30 size distribution, and that no independent field calibration was performed because no reference-grade PM mass instrument was available during the campaign. The manufacturer-specified precision is quoted and identified as a between-device specification rather than an uncertainty estimate for the present measurements, and dedicated PM calibration against reference-grade instruments is now identified in the Conclusions as a goal for future work.
- The assumption of a horizontally homogeneous wind field, used to reconstruct the AeroSonde trajectory, is now supported quantitatively. The terrain elevation directly beneath each sounding was analysed along the recorded GPS track, showing variations of less than about 300 m relative to the launch site, and the corresponding figure has been added to the Supplement.
- The description of the instrumentation has been clarified, including an explicit distinction between the remote-sensing and in situ instruments, the specification of the co-location experiment duration, the unambiguous definition of the photon-counting count-rate range used for analog–photon-counting signal merging, and the earlier introduction of the AERONET quality criterion applied to the SSA retrievals.
- The interpretation of the calibration data has been extended: the data points deviating from the regression curve at low PM_{10} concentrations are now explained as a single atypical episode associated with sea-spray aerosol transport, i.e. as a change in aerosol type rather than random measurement outliers.
- A paragraph discussing possible extensions of the system has been added to the Discussion, covering a single-channel aethalometer, an optical particle sizer with extended size range, and integration with the meteorological radiosonde.
- The description of the observed aerosol and thermal layering has been corrected, and passages repeating earlier points or disconnected from the main narrative have been removed.
- The use of citations has been made more consistent: the number of references in the Introduction has been reduced, unpublished works have been removed from the reference list, missing references have been added.

- The figures and tables have been further refined: panel headers and legends have been simplified, redundant caption text has been removed, one table has been moved to the Supplement, and a new figure showing the terrain beneath the AeroSonde track has been introduced. Section titles and terminology have also been harmonized.
- Minor issues related to language, terminology, abbreviations, punctuation, and equation formatting identified by the Reviewers have been corrected throughout the manuscript and the Supplement.

Response to Review #1

The submitted manuscript describes a new airborne system for vertical measurements of aerosol optical properties, along with two measurement examples and uncertainty estimates. The revised version of the manuscript shows significant improvement compared to the previous submission; the overall presentation, structure, and readability have clearly progressed. However, several methodological clarifications, figure enhancements, citation adjustments, and technical edits remain necessary before the manuscript can be accepted for publication.

Re: We thank the Reviewer for the careful and constructive assessment of the revised manuscript, and for acknowledging the improvement in structure and readability.

The use of citations can be improved. In the introduction, an unnecessarily high number of citations are used, even citing two papers for one number (L224), or up to six papers for one platform. One or two citations per platform type would be sufficient.

Re: The number of citations in the Introduction has been reduced to at most two per platform type.

Similarly, the number of citations per instrument used is highly unequal; while there is no citation on the ceilometer (L131-137), other instruments have often 4 citations.

Re: No ceilometer was used in this study; the instrument in the indicated lines is the CIMEL sun photometer, for which a reference has been added.

If the LAGRANTO model is widely applied, can authors cite a more recent work than 2014?

Re: More recent works were added.

Unpublished papers should be removed from the reference list.

Re: Unpublished works have been removed from the reference list.

The terms PBL and mixing layer appear to be used inconsistently throughout the manuscript. Please harmonize this terminology across the text.

Re: Terminology has been standardized to PBL.

In L453: The text mentions two thermal inversion layers, but the data appear to depict one isothermal layer and one inversion layer. Please verify and correct this description.

Re: Corrected to: "On that day, there was one isothermal layer at an altitude of approximately 0.7 and one thermal inversion layer in the middle troposphere at an altitude of 5.5 km (Fig. S7)"

The calculation and background of the TPS parameter are inadequately detailed in the Methodology, and the information on its "black box" nature may be given earlier (around line 80). Furthermore, it is questionable whether showing TPS in Figures 8C and 10C, if it is not clear how it is calculated?

Re: We agree that our previous wording could give the impression that the methodology for deriving SAE and Reff was unclear. We have revised this statement to distinguish between the proprietary calculation of TPS within the SPS30 firmware and the subsequent conversion of TPS to SAE and Reff, which is explicitly defined by the equations provided in the manuscript. We have also introduced the proprietary nature of the TPS calculation earlier in the Methodology.

In the revised manuscript the sentence: "However, since the exact method by which TPS is derived remains unclear, it is difficult to predict the precise functional form of these relationships a priori."

was changed to:

According to the manufacturer, TPS is related to the characteristic mean particle size of the sampled aerosol and correlates with the weighted average of the number concentrations measured in the particle-size bins. However, the detailed algorithm used to derive TPS is proprietary and is not disclosed by the manufacturer. Therefore, the polynomial relationship was determined empirically from the calibration data rather than derived from an assumed analytical relationship between TPS and SAE.

Could the authors clarify why the PM sensors were not calibrated and why their uncertainty was not estimated? Uncertainty bounds should be added to Figures 7 and 10. If this estimation is not possible, the lack of calibration/uncertainty should be explicitly discussed in the text and noted as a goal for future work (L621).

Re: We agree that the lack of independent PM calibration and the resulting limitation in uncertainty estimation should be stated explicitly. We have revised the data calibration discussion section at L621 to clarify that PM2.5 and PM10 were not independently calibrated because no reference-grade PM mass instrument was available during the campaign. We also clarify that the optical and microphysical parameters were calibrated against reference measurements. Finally, we explicitly identify dedicated PM calibration and uncertainty assessment against reference-grade instruments as a goal for future work.

At the end of the Sec. 2.3. such clarification has been provided:

The PM2.5 concentration is calibrated by the manufacturer against a TSI DustTrak DRX 8533, whereas PM10 is derived from the particle-size distribution measured by the SPS30. However, no independent field calibration of the PM2.5 and PM10 mass concentrations was performed in the present study, as no reference-grade PM mass instrument was available during the campaign. For reference, the manufacturer specifies a mass concentration precision of ±(5 μg m^{-3} + 5% of the measured value) for PM2.5 and ±25 μg m^{-3} for PM10 in the concentration range of 0–100 μg m^{-3}. These specifications represent the manufacturer-reported between-device variation rather than an uncertainty estimate established for the present measurements.

In conclusions the need for calibration in the future work has been addressed:

“Additional validation against reference-grade instruments is also required to better quantify measurement uncertainties, including the PM\textsubscript{2.5} and PM\textsubscript{10} mass concentrations, which were not independently calibrated in the present study. Validation covering a range of aerosol types and size distributions would allow a more robust assessment of performance of sensors and measurement uncertainty under atmospheric conditions.”

Minor comments:

L77: An approximate price/cost range would help to understand what authors mean as "affordable"

Re: The approximate unit price of the SPS30 has been added.

Line 262: Specify the duration of the co-location experiment. Hours? Days?

Re: This has been specified in the manuscript: “Measurements were conducted at the Radiative Transfer Laboratory (RTL) of the Institute of Geophysics, University of Warsaw, over 22 consecutive days during the summer of 2025.”

Section 2.3: Consider renaming this section to better reflect its contents (e.g., "Back-trajectory and AOD Models").

Re: Changed as suggested

Section 3 Title: Update the title of Section 3 to mention co-locations and checks?

Re: Title changed to “Lidar retrievals and collocation with AeroSonde profiles”

L270 and 273: Consider replacing "time constant" with "response time" for clarity and to align with standard terminology.

Re: Replaced

L270, 371, 606: Start new paragraphs at these lines to improve visual scanning and narrative flow?

Re: New paragraphs have been introduced at all three locations.

L294: The detailed plot description can be taken out of the main body text, as it is again in the plot legend.

Re: The plot description has been removed from the main text and is retained in the figure caption only.

L539–541, L577–578: These passages repeat earlier points or feel disconnected from the main text; consider removing them to improve the narrative.

Re: Sentence L539–541 has been removed and removed references have been added in the L535. L577-L578 have also been removed.

Figures and tables:

Consider combining Figure 7 with Figure 8, and Figure 10 with Figure 11.

Re: We prefer to keep the figures separate. Combining them would produce nine panels in a single figure, which we believe would reduce rather than improve legibility.

Table 4: Consider moving Table 4 to the Supplementary Information or to the Methods section, as it is not directly tied to the primary results.

Re: Moved to the Supplement (now Table S1).

Figure 5: To clean up subfigure headers, repeated header details on the height can be moved to the right side of the panels, leaving space for LR/AOD above each subplots column.

Re: The altitude range is now given once per row on the right-hand side, and the space above each column carries a single header identifying the retrieval method.

Figure 8: In panel (c) (SAE), a reference line at SAE = 1 could be added, as unity is an important threshold for source estimation. Remove redundant yellow marker information from the legend. It's not mentioned in the other subplots, and it's apparent from the legend directly in the plot.

Re: The SAE=1 line was added and redundant description of plot was removed.

Figure 9: Remove redundant yellow marker annotations from the description.

Re: Figure 9 shows back-trajectories and does not contain yellow markers; we assume the Reviewer refers to Fig. 11, whose caption repeated the description of the yellow marker already given in the caption of Fig. 8 despite stating „same variables as in Fig. 8". This sentence has been removed.

Technical Corrections

L79,121: Define all abbreviations in full at their first occurrence.

Re: Corrected

L103: Typo in "AeroSonde"

Re: Corrected

L155: A word is missing after "total scattering and [___] scattering"?

Re: Missing word "angular" has been added.

L344: Missing word: "and absolute [___], respectively."

Re: Missing word: "calibration" has been added.

L346: Missing punctuation at the end of the equation.

Re: Corrected

L368: Rephrase for clarity: "Only for the highest trajectories, a change in direction is observed"?

Re: Corrected

L376: … displacement from the station …?

Re: Corrected

L513: Remove double parentheses (( )).

Re: Corrected

Table 4 caption: what the wave above h in 3h means?

Re: Typo. Firstly it was intended to be a non-breaking space. Corrected.

Table 5 caption: Additionally, the TABLE includes... rather than column?

Re: Corrected

L634, 641: Insert missing references.

Re: The missing reference in L634 has been added. Equation (A3) in L641 is the standard Ångström power law, routinely used for spectral interpolation of AOD, and we did not consider a specific citation necessary. If the Reviewer prefers, we are happy to add a reference to Ångström (1929).

Eq. A9: Check punctuation between ASC(z) and delta(z)

Re: Corrected

Supplementary Information (SI):

L2–7: Remove duplicate text that repeats the main manuscript text

Re: Corrected

L15: Remove the word "respectively", as only one launch is described.

Re: Corrected

Figure S3: Add missing units on the vertical axis, complete the partially missing x-axis label description, and harmonize its layout with Figure S6, so that the redundant final sentence in the legend can be removed.

Re: Figure has been updated as suggested.The unit was not included on the y-axis because AOD is a dimensionless quantity

Figure S5: Remove the 7 UTC line and change the 15 UTC line from gray to black.

Re: Corrected

Figure S6: Remove redundant mentions of "550 nm" in the legend (already defined on the axis). In the b) part, could the percentage of dust AOD relative to total AOD be added on the right-hand y-axis? And applied the same also to Figure S3?

Re: Figure has been updated as suggested.

Response to Review #2

The manuscript by Broda et al. describes a new balloon-borne system for vertically resolved observations of aerosol optical properties. As the authors explain, this system incorporates low-cost sensors and can be operated on tethered balloons, UAVs, and radiosondes, meaning that the system can be reusable. This study is well aligned with current research needs for cost-effective in-situ observations that can support the validation of ground- and satellite-based lidars. I would therefore recommend publication of this manuscript after considering the following minor comments.

Re: We thank the Reviewer for the positive assessment of the manuscript and for the constructive comments, which we address below.

General comments:

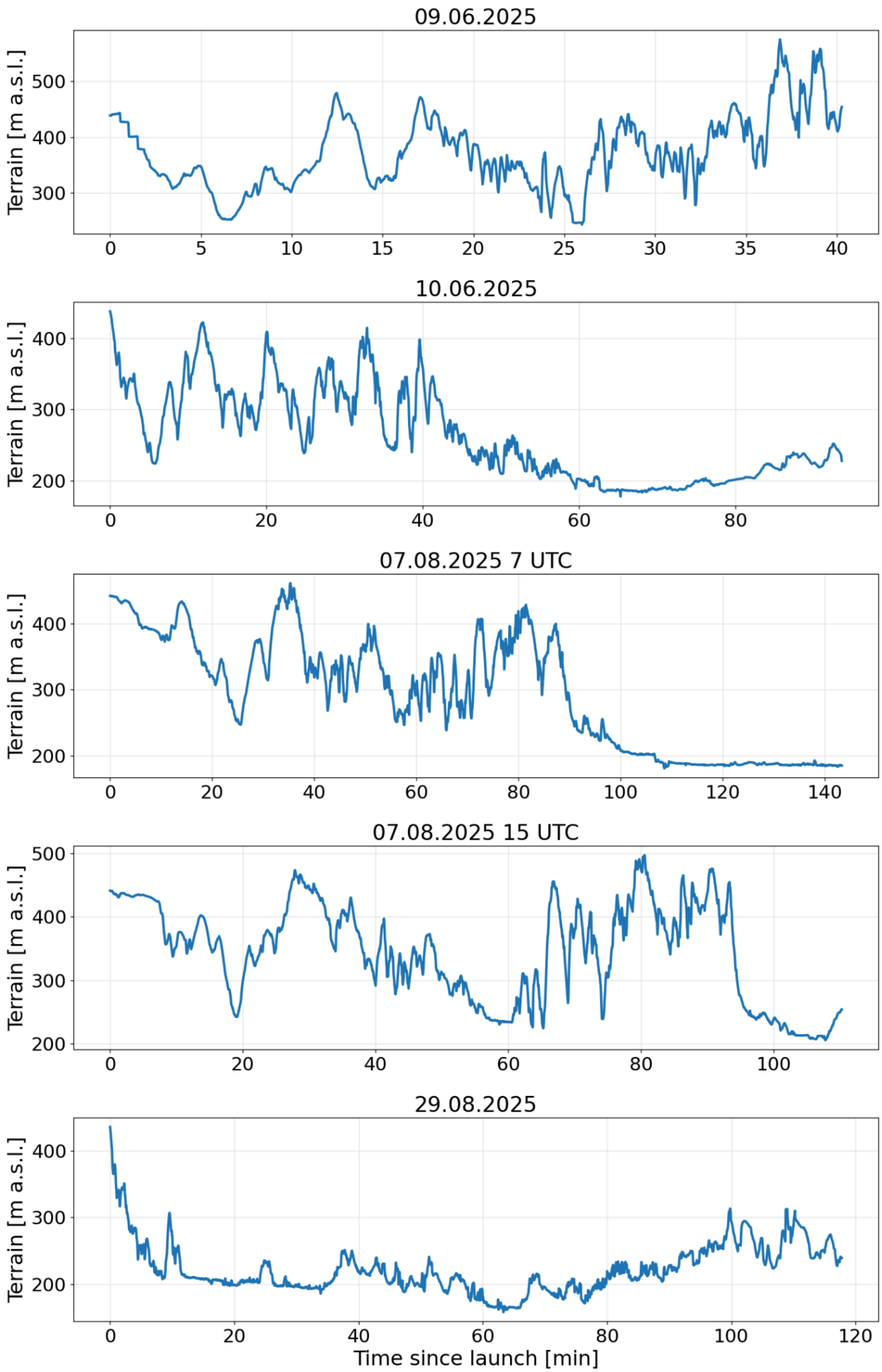
1) Considering the horizontal displacement between lidar and the AeroSonde, I would say that the orography of the region would play an important role on the stratification of elevated layers. Therefore I would recommend that the authors include an orographic map of the region, and further explain why they assume that the horizontal displacement can be neglected. This assumption is also made for the wind field (assumed to be horizontally homogeneous), which based on the orography could be wrong.

Re: We thank the Reviewer for this comment. We have analysed the terrain elevation directly beneath each sounding along the recorded GPS track (Figure below). The launch site is located in the Strzyżów Foothills, a hilly rather than mountainous region: along the trajectories the elevation ranges from 158 to 575 m a.s.l., i.e. it deviates by less than about 300 m from the launch site level (444 m a.s.l.). Given this moderate relief, and since the aerosol layers in the biomass-burning cases are located above 2 km, we consider the influence of the orography on their stratification to be of secondary importance. For the dust case, apart from a short descent in the first minutes after launch, the terrain beneath the trajectory remains nearly flat (elevation variability below ~100 m), which supports the assumption of horizontal homogeneity for this sounding.

Following the Reviewer's suggestion, the following sentences have been added to Sect. 2.3.2, after "It was assumed that the horizontal wind speed was horizontally homogeneous at a given altitude z.":

"To support this approximation, the terrain elevation beneath the AeroSonde was analysed (Fig. S3). The analysis showed that the terrain traversed by the AeroSonde varies between 158 and 575 m a.s.l., i.e. by less than about 300 m relative to the launch site, suggesting that the impact of the orography on the stratification of the layers in which the aerosol is found is secondary."

The corresponding figure has been added to the Supplement.


09.06.2025
10.06.2025
07.08.2025 7 UTC
07.08.2025 15 UTC
29.08.2025
Terrain [m a.s.l.]
Time since launch [min]

2) The lidar processing section has a remarkable level of detail which is really appreciated and provides an example for other research papers dealing with lidars.

Re: We appreciate the reviewer's positive remark on this section.

3) It would be beneficial for the discussion of this manuscript if the authors could discuss whether AeroSonde could be improved with integration of additional sensors e.g. particle sizer for coarser particles

Re: A paragraph discussing possible extensions of the system (a single-channel aethalometer, an optical particle sizer with extended size range, or integration with the meteorological radiosonde) has been added to the Discussion, alongside the other directions for future work.

Specific comments:

1) Lines 26-28: 'Ground-based and satellite-borne lidar systems have been widely employed to retrieve extinction, depolarisation, and absorption profiles (Mulena et al., 2024; Lin et al., 2023; Chen et al., 2024; Mhawish et al., 2020).'. In the cited references there is no evidence that lidar was used to retrieve absorption profiles. It would be great if the authors can provide a reference for this, otherwise remove the absorption profile.

Re: It was clarified. Now it states: "Ground-based and satellite-borne lidar systems have been widely employed to retrieve backscatter and extinction profiles \citep{Chen2024, Lin2023}."

2) Lines 115–117: This section includes both remote-sensing and in-situ instruments. I suggest clarifying this distinction in the introductory sentence, as the lidar and sun photometer are remote-sensing instruments, whereas the Aethalometer, nephelometer, and LAS are in-situ instruments.

Re: The introductory sentence now states the distinction explicitly: "These comprise five instruments: two remote-sensing (the Raymetrics LB10-V-D200 lidar and the CIMEL CE-318 sun photometer) and three near-surface in situ (the AE31 aethalometer, the Aurora 4000 nephelometer, and the LAS 3340)." The subsection has been renamed accordingly.

3) Line 148 and 513: remove parenthesis inside parenthesis

Re: Corrected

4) Line 195: Please clarify the use of MHz in the analog–photon-counting merging procedure. I assume that 5-10 MHz refers to the photon-counting count rate rather than the signal frequency.

Re: We agree, and we have changed the sentence:

The signals were merged within a photon-counting count-rate range of 5–10 MHz (5–10 × 10^6 counts s^{-1}), while the photon-counting channel was used below 5 MHz and the analog channel above 10 MHz, ensuring a continuous dynamic range for further analysis.

5) Lines 254-255: Was this assumption tested by the authors considering the complex terrain (elevation variations) of the region?

Re: Yes. As described in our reply to General Comment 1, we analysed the terrain elevation directly beneath each sounding along the recorded GPS track. The elevation ranges from 158 to 575 m a.s.l., i.e. it deviates by less than about 300 m from the launch site level (444 m a.s.l.), which we consider moderate enough to justify the assumption. The corresponding figure has been added to the Supplement (Fig. S3), and a supporting sentence has been added to Sect. 2.3.2.

6) Figure 2a: Several points between PM 10= 10–20 µg/m3 appear to deviate from the fitted regression curve. Have the authors investigated these outliers and their possible origin?

Re: We investigated the data points falling outside the fitted regression curve in the PM10 = 10–20 µg/m^3 range. We found that these observations occurred during unusual episode when the model (CAMS, ICAP) indicated the presence of sea-spray aerosol (SSA) over Warsaw, despite the site being located approximately 300 km from the Baltic Sea coast. This case may therefore represent aerosol with different physical and chemical properties from the aerosol dominating the calibration dataset. Since the SPS30 response can depend on particle size and composition, the presence of SSA may result in a somewhat different relationship between PM mass concentration and the measured optical scattering signal. We therefore interpret these deviations as potentially related to changes in aerosol type rather than as random measurement outliers.

In the manuscript the clarification has been added:

Several data points deviating from the regression curve at PM10 concentrations of approximately 10–20 µg m^{-3} were associated with a single atypical aerosol episode. During this episode, aerosol transport models indicated the presence of sea-spray aerosol over Warsaw, located approximately 300 km from the Baltic Sea coast. The observed deviations may therefore be related to differences in aerosol type and particle properties during this episode, which can modify the relationship between PM10 mass concentration measured by the SPS30 and aerosol scattering measured by the Aurora 4000.

7) Line 418: Aerosonde > AeroSonde for consistency

Re: Corrected

8) Figure 7b and 10b: Would a logarithmic axis improve the visualization and make the different particle-size ranges more easily distinguishable?

Re: We tested a logarithmic axis but found that it reduced readability. The logarithmic scale emphasises the near-zero contributions of the least abundant size ranges and thereby masks which size range dominates, which is the main feature of these panels. We have therefore retained the linear scale.

9) Lines 467-469: This appears to be the first mention of the AERONET quality criterion applied to the SSA retrievals. I suggest introducing this criterion earlier in the data section.

Re: The discussion of this criterion was moved to Sec 2.2.2. in the paragraph introducing the sunphotometer.