The manuscript presents a system for aerosol vertical profiling, but the current form suffers from some structural, methodological, and interpretative weaknesses that prevent the scientific results from emerging clearly. A recurring issue throughout the text is the imbalance between extensive descriptive passages and the lack of corresponding scientific analysis. The new system was tested with only five vertical profiles. More comparisons with lidar would be expected to assess optical properties, and comparisons with aerosounding data would also be useful to support the presented results. Many sections, particularly Section 4.1, read more like a campaign or field-log report than a scientific study. Long situational descriptions are repeated multiple times, while essential methodological information (e.g., uncertainties, calibration procedures, justification of regression shape, model references, data quality criteria) is missing or insufficiently explained.

The manuscript is also very difficult to navigate. Figures and tables are placed far from the referencing text, many figures duplicate information already shown elsewhere, and several figures or panels are not referenced. The appendices and supplemental materials are similarly hard to follow, with unclear placement and redundant content.

Overall, the manuscript requires major restructuring, removal of redundant descriptive content, clearer methodological explanations, improved figure organization, and stronger scientific interpretation.

Re: We thank the reviewer for the thorough and constructive assessment of the manuscript. We acknowledge the concerns regarding structure, clarity, and the balance between descriptive and analytical content.

The presented study is intended as a first deployment and proof-of-concept of the newly developed AeroSonde system. The limited number of vertical profiles results from the specific meteorological conditions required to capture biomass burning events during the campaign period.

In response to the reviewer’s comments, the journal’s scope, and our own reassessment of the manuscript, we have substantially shortened and restructured the Results section, reduced repetitive descriptive content, and improved the clarity of the methodological descriptions. We have also revised the figure organization and removed redundant material to improve readability.

We clarified uncertainties, data processing steps, and interpretation of the results. The revised manuscript places stronger emphasis on the technical validation and capabilities of the system, consistent with its proof-of-concept character.

**Specific comments**

L33: Is there any link or reference available for the cited measurements?

Re: The statement in line 33 is supported by several references to in situ aircraft measurement studies (Harshvardhan et al., 2022; Denjean et al., 2020; Pistone et al., 2019; Cai et al., 2022; Wang et al., 2019b), which describe the measurements in detail. We therefore consider that no additional references are needed.

L47–49 could be moved to the Methods section.

Re: We decided to retain this sentence in its current location, as it offers a brief overview of the topic and facilitates a smoother transition into the detailed Methods section.

Section 2.2.1: The description would be clearer if the sensor were described first, followed by Vaisala, then data acquisition, then the GPS, and finally the detachment system. The current order is misleading.

Re: We revised this section according to the reviewer's suggestion. However, we retained the description of the Vaisala radiosonde at the end of the section, as it is not part of the AeroSonde system but rather a separate sensor included in the measurement payload.

L75: The power consumption is given only for the GPS sensor; why is the power consumption of the other system components not provided?

Re: We removed this sentence. Instead, we added information on the whole AeroSonde system: "The AeroSonde operates with an average power consumption of approximately 125 mA."

L85: Can the height of the lower and middle atmosphere be estimated in kilometers? Can the distance from the launching site also be given in kilometers? What happens to the sample if it cannot be retrieved? Are there any height restrictions or approvals required to operate the new system?

Re: We have revised this paragraph to address the reviewer's comments. Specifically, we removed two sentences ("This system will be used only when the aerosol event is expected in the lower and middle troposphere, because only in such cases can the AeroSonde be found close to the launching site. For the upper-layer event, the particle counter and the weather sensor will allow profiling of aerosol particles up to the lower stratosphere; however, in this case, the AeroSonde will not be retrieved."), as they did not sufficiently clarify the operational constraints. We also:

- modified info on data transmission "...which also handles wireless data transmission over LoRaWAN with the LoRa SX1262 module to the ground-based receiver.";
- confirmed vertical distance: "Proper data reception was confirmed even when the horizontal distance between the probe and the receiving station exceeded 100 km" changed to: "Proper data reception was confirmed

even when the horizontal distance between the probe and the receiving station exceeded 100 km, and the vertical distance surpassed 25 km."

Section 2.2.2: An introductory explanation of the use of the presented systems could be added. Citations to studies that used the lidar and sunphotometer could be included. Bernardoni et al. (2021) is not an original citation describing the working principle.

Re: We added the following paragraphs to section 2.2.2:

In addition to in situ measurements from the aerosonde, ground-based remote sensing instruments were used to provide complementary information on aerosol properties and their vertical distribution. Lidar observations provide insight into the vertical structure of the atmosphere and support the interpretation of aerosol layering observed by the aerosonde. Sun photometer measurements provide column-integrated aerosol optical properties, such as aerosol optical depth, which are used together with lidar signals in the Klett–Fernald–Sasano inversion algorithm to retrieve aerosol optical properties (Klett, 1985; Fernald, 1984; Sasano et al., 1985; Sanchez-Barrero et al., 2026).

It should be noted that lidar-derived quantities (e.g., AEC) and in situ measurements (e.g., ASC) represent different optical properties and are associated with different uncertainties. Therefore, lidar observations were used for qualitative comparison and consistency assessment of aerosol vertical structures rather than for direct validation of the aerosonde data. Additional in situ instruments (e.g., nephelometer, aethalometer, Laser Aerosol Spectrometer) were used to characterize aerosol scattering and absorption properties at the surface, but also to calibrate the SPS30 sensors.

We substitute the Bernardoni et al. (2021) with:

Hansen, A.D.A., Rosen, H., Novakov, T. (1984). *The Aethalometer: An instrument for the real-time measurement of optical absorption by particles*. Science of the Total Environment, 36, 191–196.

L129 What mass absorption coefficient was used for the equivalent black carbon calculations?

Re: In this study, the equivalent black carbon is not discussed; therefore, the mass absorption coefficient is not provided.

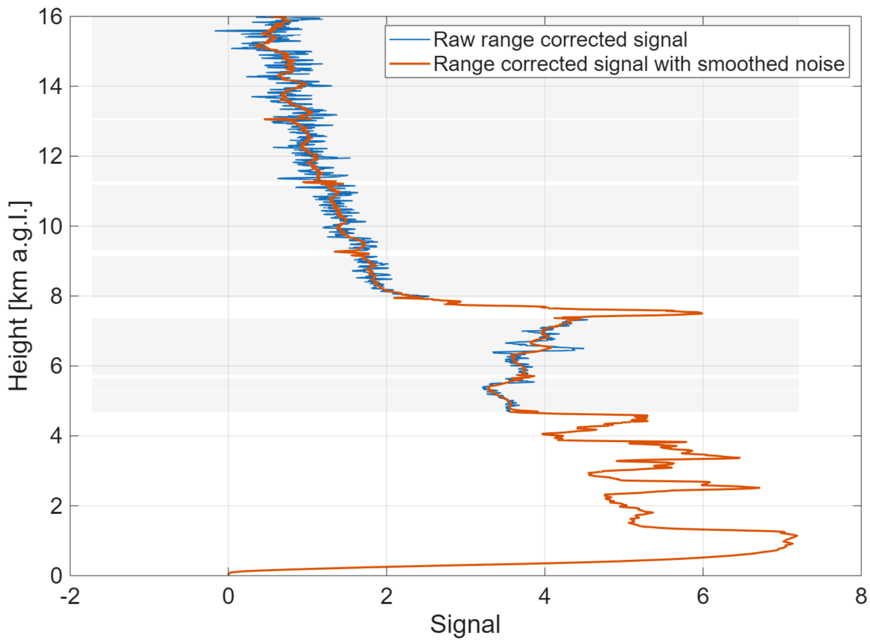
Section 2.2.3: An introduction explaining the purpose of the presented models could be provided. L159: Are there any recent works on the LAGRANTO model that could be cited?

Re: We have added a brief introductory sentence to Section 2.2.3 to clarify the purpose of the models: "We employed the LAGrangian ANalysis TOol (LAGRANTO) model to compute back-trajectories for identifying source regions and the International Cooperative for Aerosol Prediction Multi-Model Ensemble (ICAP-MME) to analyze dust and smoke AOD along these trajectories."

Regarding the LAGRANTO model, we are not aware of any more recent peer-reviewed publications. Therefore, we retained this reference and added a sentence: “The current version of the model is available online at the official project website: https://iacweb.ethz.ch/staff/sprenger/lagranto/”.

L172: Why is the correction not introduced first, followed by the description of how the corrected data are used? Is there a citation for the noise detection algorithm? Why was the flagging threshold set at 45% and not, for example, 50%? How many data points were removed due to flagging?

Re: The noise correction is introduced before its application. The workflow is as follows: the raw lidar signal is first converted to a range-corrected signal, then the noise detection and smoothing algorithm is applied, and finally, aerosol optical properties are retrieved. The noise detection algorithm is an in-house method developed for this study. Given the variability of lidar noise and the supporting role of lidar data here, the aim was to apply a simple, fully automated approach that avoids subjective manual smoothing. The 45% threshold was selected empirically based on inspection of multiple profiles, as no independent reference or objective quality metric for noise identification was available. No data points were removed; flagged values were replaced with a moving-average filter.




To further illustrate how the algorithm operates, a representative example is provided on the left (7 August 2025 case, 15:20 UTC), showing the raw range-corrected lidar signal (blue) and the signal after noise smoothing (red). The flagged noisy region is coloured in grey. The profile corresponds to the time of the Aerosonde launch. For this case, approximately 9% of points were flagged in the 0.7–3 km range, 68% within the aerosol layers (3–9 km), and 94–95% above 9 km. A representative example of the smoothing procedure has been added to the manuscript.

Regarding the reviewer comment, we decided to add at the end of this paragraph a sentence clarifying how the threshold was chosen: “The threshold of 45\% was chosen empirically, based on inspection of multiple profiles and iterative testing.”

What is meant by the “transported layer”? L191 How is the transported layer defined within the atmosphere?

Re: We agree that the term “transported layer” may have been unclear. In the manuscript, this term simply refers to a layer of aerosol originating from long-range transport. We have clarified this wording in the revised manuscript to avoid ambiguity.

Sentence changed from: “The assumption of a constant LR introduces uncertainties, particularly because aerosol mixtures within the PBL can differ from those within the transported layer in the free atmosphere. ”

to: “The assumption of a constant LR introduces uncertainties, as aerosol properties within the PBL may differ from those of long-range transported aerosols in the free troposphere.”

L227: What is the origin of the horizontal wind speed data?

Re: The horizontal wind speed data were obtained from the Vaisala radiosonde (RS41) measurements conducted simultaneously with the AeroSonde launches. We have clarified this in the revised manuscript.

L235: What is the correlation at the original time resolution of the aerosol data? The correlation at the original time resolution would be more relevant for high-frequency measurements than hourly averages.

Re: This sentence was modified to: “The average Pearson correlation coefficient across all instrument pairs was 0.97 for PM10 and PM1, and 0.99 for TPS when 1-hour averaging was applied. In contrast, for the original 1-second data (without averaging), the correlations decreased to 0.89 for PM10 and 0.90 for PM1.”

L238: Can the term “very high concentration” be quantified?

Re: We agree that this term was not sufficiently precise, and we added value in the text: “In the first experiment, the sensor was placed in a chamber with a very high aerosol concentration (PM10 in the order of 200-300 μg/m3).”

L243: Could the 5-second sensor response time be a limiting factor for atmospheric measurements?

Re: Given a typical vertical velocity of approximately 3–4 $ms^{-1}$, the less than 5 s time response of the sensor corresponds to an effective vertical resolution of better than 15–20 m. This scale is smaller than the characteristic thickness of the observed aerosol layers (typically on the order of hundreds of meters). Therefore, the response time is not expected to be a limiting factor for resolving the main vertical aerosol structures. However, we note that very sharp gradients at smaller vertical scales may be smoothed due to the sensor response time.

We added sentence: “For typical balloon ascent rates of 3-4 m/s, this corresponds to a vertical resolution of no worse than 15-20 meters”.

Why are all regressions non-linear? What is the physical reason for the chosen regression shapes? Can the regression choices be supported by references? It is unclear where Equation 5 comes from.

Re: The non-linear form of the regressions arises from the fundamental relationship between aerosol optical properties and particulate mass, rather than from an arbitrary empirical choice. The aerosol scattering coefficient (ASC) is proportional to the integral of particle cross-section weighted by the scattering efficiency, i.e.:

$ASC \propto \int r^2 Q(r, \lambda) n(r) dr$,

whereas PM10 is proportional to the particle mass:

$PM10 \propto \int r^3 n(r) dr$.

Therefore, ASC and PM10 depend on different moments of the particle size distribution. As long as the size distribution remains strictly constant and only particle number concentration changes, a linear relationship between ASC and PM10 can be expected. However, in real atmospheric conditions, variations in PM10 are often accompanied by changes in particle size (e.g., due to hygroscopic growth, coagulation, or mixing of aerosol types) and chemical composition (refractive index). In such cases, the relationship between ASC and PM10 becomes inherently non-linear.

The regression form used in this study,

$ASC = a \cdot PM10^{(1/2)} + b \cdot PM10$,

should therefore be interpreted as a semi-empirical representation of this behavior. The linear term reflects variability dominated by particle number concentration, while the sublinear term captures situations where changes in particle size contribute to the observed variability.

We added two paragraphs in revised version of manuscript:

“The proposed relationship between ASC and PM10, specifically the inclusion of both a linear and a square-root term, is supported by simulations conducted as part of this study. Lorenz–Mie simulations for spherical particles (see Fig. X in the Supplement) show that this functional form provides a good approximation of both the simulated and observed data, effectively capturing the combined effects of particle number concentration and particle size evolution. This suggests that the non-linear regression reflects the underlying physical processes governing aerosol behavior, including changes in particle size due to hygroscopic growth and coagulation. This interpretation is consistent with the findings of Hand and Malm (2007), who showed that mass scattering efficiency (i.e., the ASC to PM10 ratio) is a non-linear function of particle size. However, if the particle size distribution remains constant and only the particle number concentration varies, a linear relationship between ASC and PM10 would be expected."

and:

"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. Despite this uncertainty, the polynomial fits provide reasonable approximations, capturing the general trends observed in the data. This non-linear behavior is consistent with previous studies, which have shown that key optical parameters, such as the Ångström exponent, are non-linearly related to particle size (Schuster et al., 2006). Nevertheless, further studies are needed to refine these relationships and better understand the underlying physical processes."

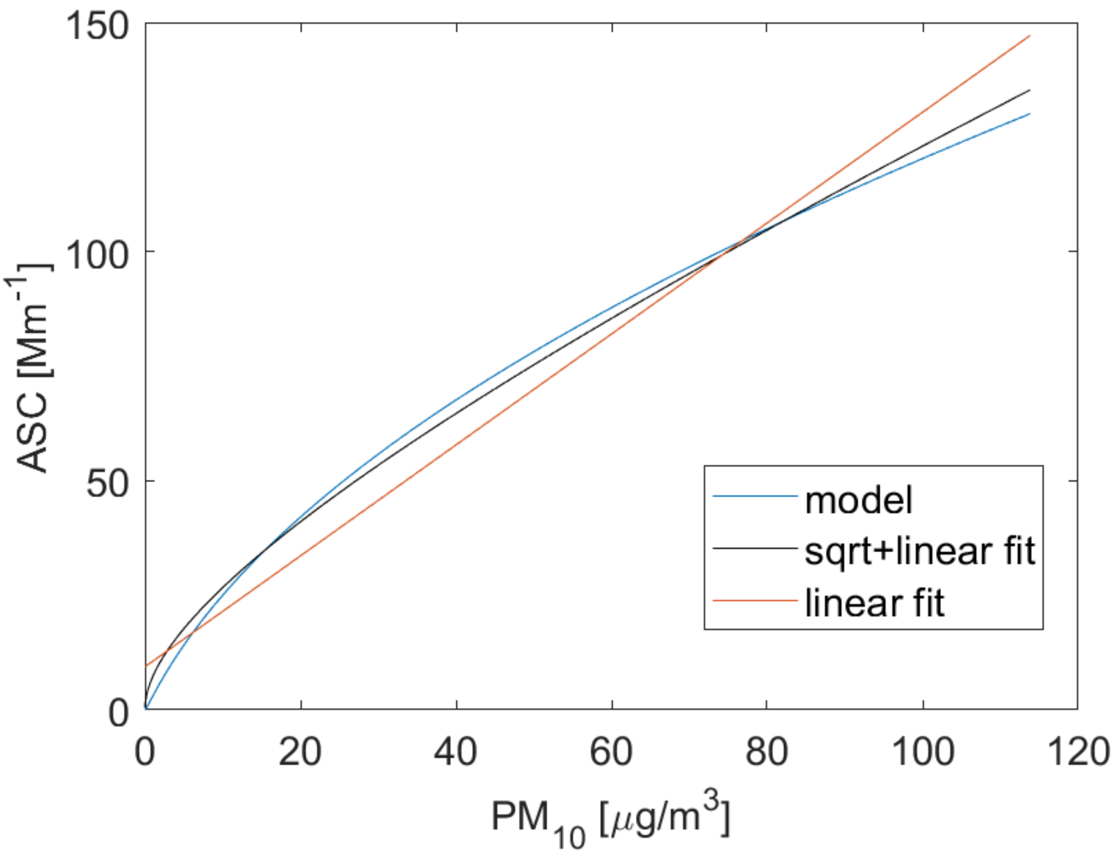



**Fig. 2.** Aerosol scattering coefficient [$Mm^{-1}$] as a function of $PM_{10}$ [μg $m^{-3}$] (blue line), derived from Lorenz–Mie simulations for spherical particles with a refractive index of (1.50 + 0.004i), representative of brown carbon from biomass burning (Sumlin et al., 2018). Calculations were performed assuming a log-normal size distribution with a fixed geometric standard deviation of 2.0 and a mode radius varying from 0.01 to 0.25 μm. The green line denotes a linear fit ($ASC = c \cdot PM_{10}$), while the black line represents a combined square-root and linear fit ($ASC = a \cdot PM_{10}^{1/2} + b \cdot PM_{10}$).

L270: The SPS30 does not appear to be very useful for typical particle sizes below 0.5 µm. Therefore, the SPS30 may not be very useful for biomass burning aerosol?

Re: We respectfully disagree with the concern raised. Although the Sensirion SPS30 has a nominal lower detectable particle size of approximately 0.3 µm, published evaluations show that its effective size discrimination is limited and that the sensor performs best in the submicron range, particularly for PM1 and in part PM2.5 (Kuula et al. 2020, Tryner et al. 2020, Sousan et al. 2021). This is still highly relevant for biomass-burning aerosols, which are typically dominated by fine and accumulation-mode particles, with many studies reporting that the majority of emitted particles are smaller than 1 µm and that the number mode often peaks around 0.1–0.2 µm.

Therefore, while the SPS30 should not be interpreted as a high-resolution particle sizer, it remains suitable for capturing the variability of biomass-burning aerosol in the fine-particle range. In our study, its use is therefore justified.

We added the above-mentioned references in the text.

Why are the data averaged across all instruments in Figure 3 instead of being shown individually? Each sounding uses only one unit.

Re: The data in Figure 3 were aggregated across all measurements to derive a robust calibration relationship between the AeroSonde and reference instruments. While each individual sounding uses a single unit, combining the datasets improves statistical robustness and reduces the influence of case-specific variability. However, calibration for each sensor is performed independently, and the assigned coefficients are shown in Table 1.

June 10: The near-surface inversion is not acknowledged in the text (L318).

Re: The corresponding parts of the Results section have been removed as part of the restructuring.

Why are cloud presence and cloud descriptions so emphasized (e.g., L346–349), if they do not play a role in the results?

Re: We added a paragraph to the method section: “Aerosol layers were identified based on the analysis of lidar backscatter profiles at 532 nm. In the absence of depolarization measurements, the distinction between aerosol and cloud layers was based on signal intensity and temporal continuity. Cloud-contaminated parts of the lidar profiles were replaced using linear interpolation between adjacent cloud-free layers. Cloud presence was additionally assessed using an all-sky camera and vertical pointing infrared radiometers integrated within the microwave radiometer system, based on brightness temperature measurements (PolandAOD, 2025).”

A large part of the text is devoted to describing trajectories, while the key information is simply whether they originated in Canada or not. Therefore, most of this text could be shortened.

Re: We agree that parts of the trajectory description were overly detailed. However, we still consider this information important for providing context on the transport pathways and understanding the vertical layering of aerosols. Nevertheless, the corresponding sections have been shortened, and the text now focuses on the key information regarding air mass origin.

Why were the vertical profiles from the system not compared with WMO station data?

Re: Thermodynamic profiles were obtained from the co-launched Vaisala RS41 radiosonde, which is a reference-grade instrument providing high-quality in situ measurements of atmospheric conditions. The use of a co-launched system ensures full collocation in time and space with the AeroSonde observations.

In contrast, standard WMO radiosonde data are typically not collocated with the measurement location and time, which would introduce additional uncertainty. Moreover, the primary objective of this study was not the validation of meteorological profiles, but the assessment of aerosol measurements.

For these reasons, a comparison with WMO station data was not included.

L399: It is not possible to claim that the stratosphere was unaffected because no measurements were taken there.

Re: We thank the reviewer for this comment. We agree that this statement was too strong, as no direct measurements were performed in the stratosphere. Since the intention was only to suggest a possible interpretation rather than make a firm claim, we have removed this sentence from the revised manuscript.

It is not clear why two different Klett approaches were used. One would expect a methodological suggestion at the end of the MS?

Re: We thank the reviewer for this comment. We agree that the rationale for using two Klett-based approaches was not sufficiently explained in the original manuscript. To address this, we have clarified this aspect in the methodology section by adding the following explanation:

"*Two Klett-based approaches were applied to account for different observational constraints. The Klett LR method does not require measurement information and can therefore be used under all conditions, including periods without AOD measurements (e.g. nighttime), but it depends on the assumed constant lidar ratio, which introduces uncertainty. In contrast, the Klett AOD approach constrains the aerosol extinction coefficient using measured AOD, therefore limiting occurrences of unrealistic values.*"

Referring to the reviewer's suggestion to provide a recommendation at the end of the manuscript, we respond as follows: based on our analysis, a slightly better agreement between ASC and AEC was obtained when using the Klett LR approach. However, we emphasize that the ASC parameter does not account for aerosol absorption properties, and we lack an independent reference that would allow us to objectively assess which retrieval is more accurate. For this reason, we consider the use of both approaches as a means of evaluating the robustness of the lidar-derived signal, rather than favouring one method over the other. Moreover, the lidar retrieval is not the primary focus of this study, but serves as a supporting tool for the interpretation of aerosol properties. Therefore, we intentionally avoid recommending one approach over the other.

It is unclear how the measurement uncertainty was calculated in Figure 10.

Re: The uncertainty shown in Figure 10 is derived from the methodology described in Section 3. In particular, it includes both the calibration-fit uncertainty of the SPS30-derived quantities and the uncertainty of the reference instruments (Aurora 4000 and LAS), combined using standard error propagation (Eqs. 4–7).

Was the temporary reduction in Reff artificial due to abrupt atmospheric changes and the low spatial response of SPS30, or do the authors expect it to represent real behavior?

Re: This sentence has been removed as part of the restructuring of the Results section. However, it has been emphasized that both SAE and Reff are associated with large uncertainties. Although the calibration of the SPS30 sensor for different aerosol types clearly indicates the presence of a dominant fine or coarse aerosol fraction, drawing more detailed conclusions remains questionable.

L500: The upper limit of the aerosol layer appears to be clearly connected to the existence of the inversion layer, as AOD decreases from 20 to 3 $Mm^{-1}$. Values above the inversion would likely not be connected to dust.

Re: We agree that the aerosol signal observed above the inversion layer cannot be directly associated with the dust plume. To clarify this point, we have revised the manuscript to explain that the contribution above ~5.5 km can be attributed to background free-tropospheric aerosol. The corresponding sentence has been updated accordingly.

L626: If the new system used Raman lidar, what additional information would it provide beyond using the lidar alone?

Re. To add this information, we replaced the sentence: "Future work should focus on extended deployments under a wider range of aerosol conditions, improved calibration strategies for coarse-mode particles, and integration with Raman lidar, which would allow aerosol extinction profiles to be retrieved without assuming LR or AOD, thereby enhancing closure studies. "

with:

“The additional integration of the AeroSonde system with a Raman lidar would provide complementary information by enabling the retrieval of aerosol extinction profiles without assuming LR or AOD, which are typically required when using conventional lidar alone. Raman lidar, in particular, enables the determination of AE, which can be compared with the data obtained from the AeroSonde, offering deeper insight into the particle size distribution. Moreover, combining Raman lidar's AEC with the AeroSonde's ASC would allow estimation of the single scattering albedo. This combination would not only improve the accuracy of vertical aerosol profiles but also provide more reliable data for closure studies, as the dependence on assumptions such as LR and AOD would be minimized.”

**Figures:**

Figure 1 could be combined with Figure 2 if rotated by 90 degrees.

Re: We modified the figures as suggested.

Figure 3C is not referenced in the text.

Re: Reference to this figure is added in line 317: As in the case of SAE, the Reff can be estimated, albeit with relatively high uncertainty (Fig. 2c; RMSE = 0.019 µm). The figure numbering has been updated as part of the manuscript restructuring.

In Figure 3, the Pearson correlation, RMSE, and equation are not necessary because they are already given in the text.

Re: We thank the reviewer for this suggestion. We agree that these metrics are also reported in the text; however, we intentionally retained the Pearson correlation coefficient, RMSE, and regression equation in Figure 3 to ensure that the figure remains self-contained and can be interpreted independently of the main text. This approach improves readability and allows readers to directly assess the quantitative agreement without the need to cross-reference the manuscript. We therefore chose to keep these elements in the figure.

Figures 5 and 6 could be combined into a single figure for better comparison.

Re: The corresponding Figures have been removed as part of the restructuring.

Figure B3: Why is the “anthropogenic” category written in capital letters?

Re: Corrected

Figure 5B is not referenced in the text.

Re: We thank the Reviewer for this comment. If the Reviewer was referring to Fig. B5 (Appendix), this figure was indeed cited in the manuscript (line 525). If the Reviewer was referring to Fig. 5b, it was cited in line 329. Both Fig. B5 and Fig. 5b have now been removed as part of the manuscript restructuring.

The information in Figure 4 is completely duplicated in Figures B4, C4, C3, and D3. Figure 4 could therefore be removed.

Re: We agree that the information presented in Figure 4 is redundant. Therefore, Figure 4 has been removed from the revised manuscript.

Figure 7C presents the same information already included in the C3 plot and therefore duplicates the information. This duplication also applies to other figures with trajectories.

Re: Figure 7C is intended to provide contextual support for the trajectory analysis by showing that the backward trajectories, initialized at the altitude of the ASC peak, intersect regions of elevated AOD. This visualization helps to directly link the observed extinction peak with potential source regions along the air mass pathways.

In contrast, Figure C3 serves a different purpose: it presents the ASC vertical structure in relation to the thermodynamic conditions of the atmosphere. Its role is to interpret the vertical profile in the context of atmospheric stability and structure rather than transport pathways.

Therefore, while both figures include related information, they address different aspects of the analysis (transport vs. thermodynamic context) and are not considered duplicative.

Figures 10 and 11 could be combined into a single figure. Part of the Figure 10 label would be more appropriate in the Methods section rather than in the figure label.

Re: The corresponding Figures have been removed as part of the restructuring.

Figure 13: Why is the agreement of SAE and RR with the reference instruments and ground measurements so poor?

Re: Thank you for pointing this out. During data verification, an issue with the zero-offset measurement in the Aurora 4000 was identified. The data were corrected by using zero measurements from another day.

Figure 15 could be moved to the Supplementary Information because the information it contains is already described in the text.

Re: We have chosen to retain Figure 15 in the main manuscript, as it provides a concise visual summary of the key quantitative results and complements the textual description. While the main findings are discussed in the text, the figure allows the reader to directly assess the relationships and uncertainties in a clear and accessible way.

Figure A1b: The resolution of the y-axis should be improved because the values are currently impossible to read.

Re: The figure has been updated; however, the exact values are not important here. It is evident that the RPM does not change with variations in air pressure. The figure was moved to the Supplement.

Figure B1 could be combined with Figure B2. Figure B2: The starting height of the model trajectory could be highlighted. The same highlighting could be applied to other similar figures.

Re: The corresponding Figures have been removed as part of the restructuring.

Figures D2 and C2 atc. appear more relevant to flight planning than to the results and may not be necessary.

Re: We moved both Figures to the Supplement.

**Tables:**

Table 1 could be moved to the Supplementary Information. In the main text, the information from Table 1 could be summarized in a sentence.

Re: We thank the reviewer for this suggestion. However, we have retained Table 1 in the main text, as it provides essential calibration parameters necessary for the interpretation and reproducibility of the results. We believe that keeping Table 1 in the main text improves readability and facilitates interpretation of the results.

Table 3: Does it mean that there is a maximum three-hour time difference? This is unclear from the description.

Re: We have clarified the wording to make the temporal reference explicit. The sentence has been revised from:

"In addition, the contributions of smoke AOD (AOD_smoke) and mineral dust AOD (AOD_dust) to the total AOD from ICAP are provided for the times closest to the measurements (max. 3h)."

to: "In addition, the contributions of smoke AOD (AOD_smoke) and mineral dust AOD (AOD_dust) to the total AOD from ICAP are provided for the closest available times, with a maximum temporal offset of ±3 hours relative to the measurements."

**Minor comments**

The manuscript does not describe single scattering properties but rather bulk scattering properties of the aerosol; therefore, the term "single scattering" should be removed from the entire manuscript, including the title.

Re: Title of the manuscript changed to "A new balloon-borne system for measuring the vertical variability of aerosol optical properties using low-cost sensors."

Abbreviations are not defined at their first appearance (L3, L9, 29, 48, 58, 61, 88, 154, 244, etc.). If an abbreviation is not used later in the text, it is not necessary to define it.

Re: We defined abbreviations where appropriate.

L39: It is unclear what is meant by “high rise installations.”

Re: By “high-rise installations,” we refer to measurement setups deployed on tall buildings or towers that enable quasi-vertical observations of atmospheric properties within the boundary layer. To improve clarity, we have revised the wording accordingly.

Changed from: “... and high-rise installations (Hao et al., 2022; Quan et al., 2025; Chili´nski et al., 2025).”

To: “... and measurement systems deployed on tall buildings or towers (Hao et al., 2022; Quan et al., 2025; Chiliński et al., 2025).”

Note 1 is not necessary and could be removed.

Re: Removed as suggested.

L191: The description of mean and standard deviation could be written as mean ± standard deviation instead of a full sentence. The same issue applies to L193, L194, L206, L207, etc.

Re: Corrected.

L218: The abbreviation AEC stands for aerosol extinction coefficient.

Re: Corrected.

L238 and L309: Why is the mean difference given in micrometers rather than nanometers?

Re: The mean difference is expressed in micrometers to maintain consistency with the notation used for effective radius and typical particle size throughout the manuscript.

L398: The inversion does not increase with height; it disappears.

Re: We have corrected the wording as suggested. The sentence has been revised from:

“The observed temperature inversion near the surface explains the enhanced ASC values close to the ground and the subsequent decrease in ASC as the inversion weakens with height.”

to: “The observed temperature inversion near the surface explains the enhanced ASC values close to the ground and the subsequent decrease in ASC as the inversion disappears.”

Lines 433–438 and 486–492 repeat similar information and could be omitted.

Re: Those sentences were rewritten as a part of the restructuring.

Lines 438–443 repeat the layer positions already described in Section 4.1 without a clear purpose.

Re: We removed this sentence.

Sentences starting at L445 and L548 convey the same information; one of them could be removed.

Re:  We would like to clarify that the sentences mentioned appear not to be directly related to each other in terms of content or logical flow. Could the reviewer please confirm whether these lines were indeed the intended references? This will help us address the comment.

RMSE has a unit, and this unit should be included in Figure 15.

Re: We have added the appropriate unit for RMSE in Figure 15 to clarify the scale of the values.

L576: The uncertainties mentioned in the text have units, but the units are missing.

Re: We have added the appropriate units.

L581: The PBL itself does not contribute to AOD, but the aerosol trapped within the PBL does.

Re: We have corrected the wording as suggested. The sentence has been revised from:

"During the analysed long-range transport events, the PBL contribution…"

to: "During the analysed long-range transport events, the contribution from aerosol found within PBL…"

In the appendices, both text and supplementary figures are included in the references, which makes navigation very difficult.

Re: We have revised the layout of the appendices and supplementary figures to improve readability and navigation. In particular, we ensured that all figures are properly placed within their respective sections and no longer interfere with the reference list. Please note that the final layout will be further adjusted during the production stage to conform to the journal's formatting standards.

Appendix D is not necessary because the situation description is already fully given in the text.

Re: We moved Appendix D to the Supplement.