Comments for: "Aerosol Vertical Distributions Shaped by Boundary Layer Dynamics in a Coastal Urban Environment: Insights from the TRACER Campaign"
Chen et al., egusphere-2026-1748

General comments:

Overall, this is an interesting study that develops a useful framework for connecting aerosol vertical profiles to boundary-layer dynamics using lidar observations and thermodynamic profiles. The manuscript combines physical interpretation, empirical fitting, and case-study analysis in a coherent way. They also account for hygroscopic growth effects before interpreting aerosol vertical structure is particularly valuable. The introduction of a compact fitting framework for aerosol profiles, together with the distinction between locally mixed aerosol layers and long range transported aerosol layers is useful and potentially provide constraints for model parameterizations or remote-sensing retrievals.

The case studies are generally convincing and clearly demonstrate the influence of convective growth, stable nocturnal layers, residual layers, and sea-breeze circulation on aerosol vertical structure. The discussion appropriately recognizes that the fitted aerosol transition height is not necessarily identical to the thermodynamic boundary-layer height and the manuscript does a good job avoiding overinterpretation of this relationship. The statistical analysis further shows that BLH and stability contribute to aerosol profile structure, although they explain only a limited fraction of the variance.

At the same time, several aspects of the manuscript would benefit from clarification and stronger justification. The fitting framework and the interpretation of its parameters should be described more concisely and with clearer physical meaning. The methodology for identifying elevated aerosol layers and selecting Gaussian components could also be made more objective and reproducible. Some interpretations, particularly regarding aerosol type attribution and possible aerosol radiative feedback, should be stated more cautiously unless additional supporting evidence is provided.

The manuscript would also benefit from a deeper discussion of aerosol characteristics beyond bulk concentration alone. Again, this is useful but might be beyond the scope of the current work, but it is worth considering in section 3.6. Several of the analyzed profiles appear to be influenced by elevated long-range transported aerosol layers, and comparison with regional reanalysis products such as MERRA-2 aerosol products could help separate local boundary-layer effects from synoptic aerosol transport. It may also be useful to discuss whether the relationships found here would differ for accumulation-mode and coarse-mode aerosol populations individually, since different aerosol types may respond differently to boundary-layer evolution and transport processes.

Overall, the manuscript presents a promising framework for studying aerosol vertical structure in relation to boundary-layer dynamics. With clarification of the fitting methodology, more careful treatment of transported aerosol influences, and additional discussion of the physical interpretation and limitations of the derived relationships, the study would make a valuable contribution to aerosol-boundary-layer interaction research. These are some specific comments:

Major comments:

Comment 1. Page 1 Line 14-15: "*We introduce a new parameterized fitting function that captures the characteristic S-shaped aerosol profiles associated with boundary layer processes*."

The current sentence sounds a bit too technical and vague for an abstract because it does not clearly say why the fitting function matters. Can you include a phrase that mentions justification on the physical meaning of the fitting parameters?

Comment 2. Page 7 Line 153-176: "*Equation 1 is well suited for representing aerosol vertical profiles,… to the transition zone.*"

This is scientifically reasonable, but this section reads a bit too repetitive. The main idea is strong: the fitting function gives a compact way to describe aerosol profiles when aerosols are surface-originating and no elevated transported layer is present.

A few clarifications would make this section stronger. First, the text could more clearly state the range of conditions where the function is valid, especially excluding cases with elevated transported aerosol layers or multilayer structures. Second, the physical interpretation of each parameter could be shortened and connected more directly to observable features of the profile. Third, the choice to fix $\gamma = 0.3$ is reasonable for fit stability, but it should be justified with a brief sensitivity test or goodness-of-fit comparison (e.g., some numbers instead of 'good fits'). Finally, the section would benefit from a concise statement explaining how this fitted transition-zone height will be used later in the analysis.

Comment 3. Equation 2: This is a reasonable extension of Equation 1. It clearly separates the background surface-driven aerosol profile from elevated layers caused by long-range transport or in-situ formation aloft. The Gaussian terms are a practical way to capture distinct layers without changing the interpretation of the original surface-anchored profile.

A few points could make the description stronger- The text could clarify how elevated layers are identified before fitting, for example using local maxima in the lidar profile, threshold exceedance above the fitted background, or visual/manual selection. Otherwise, the Gaussian terms could appear somewhat subjective.

It is good that the method limits the number of Gaussian terms to avoid overfitting. However, the manuscript should briefly explain the criterion for choosing $N = 0,1,$ or 2, such as improvement in residuals, physical interpretability, or a minimum layer strength/width.

Comment 4. Page 9: Line 219-221: "*The pronounced relative humidity dependence of the depolarization ratio in these layers, together with the HYSPLIT results, suggest a major contribution from hygroscopic sea salt aerosol.*"

The sea-salt interpretation should be stated more cautiously. The RH dependence, low-level Gulf trajectories, and depolarization behavior are consistent with hygroscopic aerosol such as sea salt, but they do not uniquely identify sea salt. Other hygroscopic aerosol types from marine or coastal sources may also contribute.

Comment 5. Page 10: Line 238-240: "*The thermal structure could also limit vertical mixing and help shape the observed aerosol layering. Aerosol radiative effects may, in turn, reduce solar radiation at the surface and therefore surface heating, strengthening the inversion and helping to maintain the layering*."

Does this imply to aerosol direct radiative impacts? The sentence suggesting aerosol radiative effects may strengthen the inversion feels speculative. Unless this is supported by radiation measurements or model sensitivity tests, it may be better to say this is possible feedback rather than an inferred mechanism.

Comment 6. Line 246-250: "*BLH-LL, is 140 m in the early morning (10:59–11:59 UTC) and 320 m around midnight … that develops near the surface during nighttime.*"

The discussion of BLH-RiB versus BLH-LL is useful, but it should more clearly explain why they differ: BLH-LL captures the shallow nocturnal stable layer, while BLH-RiB and the fitted transition height may correspond more closely to the top of the residual aerosol layer or inversion-capped mixed layer.

Comment 7. Page 11. Line 271-272: "*Figure 6b, which shows larger depolarization ratios in the 0-2 km and 2-4 km ranges, separated by a layer of smaller depolarization ratio in between.*"

Looks like there is some separation between the two layers instead of 0-2 and 2-4, I see more like 0-2 and 2.5-4. (between the low-level aerosol layer and the elevated layer). Please clarify.

Comment 8. Figure 7: Could you please explain why BLH-RiB and BLH-LL donot agree in the first and 4th panels?

Comment 9. For both the cases: For all cases, it would be helpful to discuss the surface aerosol concentrations together with the vertical mixing and boundary-layer dynamics in more detail. The elevated layer above ~2.5 km appears to be associated with long-range transported aerosols. How much of this aerosol is mixed downward into the boundary layer, and how deep does the homogeneous mixing extend?

Comparing Figures 7 and 10, the aerosol vertical profiles are clearly different, which is useful for the case-study comparison. The case in Figure 10 appears to show cleaner aerosol conditions at higher altitudes. Can these profiles capture differences in synoptic aerosol regimes? For example, if there is synoptically driven dust loading, which is common in summertime, would these vertical profiles be able to identify and represent that elevated dust layer? For. e.g., Figure 7 could be compared with the vertical dust distributions from the columnar dust MERRA-2 analysis products.

Comment 10. Page 14, Line 331-332: "*The gradual change in 1-2 km backscatter around 16:00-02:00 UTC (18–19 May) may indicate some degree of vertical mixing.*"

This is hard to see in the plot. It looks more like a clean and vertically well mixed in the types of the aerosols as indicated by the depolarization ratio, so it would help if the aerosol concentrations in the ground are also stable and have less variation in the diurnal cycle. Can you discuss more on the diurnal cycle of the aerosol concentration measured in the ground?

Comment 11. Page 17, Line 394-396: "*A profile was classified as having an elevated layer when it showed a distinct local maximum in aerosol concentration above the low-level aerosol layer and was separated from it by a relative local minimum*."

This is a very impressive. Out of the 144 cases, how many days were classified as having elevated aerosol layers? This information could be useful for future model simulations, particularly for understanding how elevated aerosols contribute to the regional and seasonal aerosol distribution in the vertical dimension. Perhaps, there could be statistically relevant vertical profiles with the averages/stds in concentrations for different weather/aerosol regimes.

Comment 12. Section 3.5. Could elevated transported aerosol layers or humidity-driven aerosol growth bias the inferred relationship between stability and k? This is important because aerosol layering may not always be controlled by local thermodynamic structure alone, especially during long-range transport or strong hygroscopic growth conditions. How sensitive is the relationship between k and $d\theta/dz$ to the choice of ±100 m and the 100 m Gaussian smoothing? This directly tests whether the result is physically robust or partly an artifact of methodological choices.

Comment 13. Section 3.6. This question might be out of the scope of the current work; however, it could be important to help understand some of the discrepancies in predicted profiles.

These analyses are based on bulk aerosol concentrations. Do you think the results, or the prediction of the vertical profiles, would change if aerosol size distributions were considered? For example, would the relationships differ if accumulation-mode and coarse-mode particle number concentrations were analyzed separately? It would be interesting to see.

Comment 14. Abstract: The abstract could be strengthened by presenting the main results more directly. While it provides a helpful overview of the motivation and scope of the study, the specific findings are somewhat understated. I recommend revising the abstract to clearly state the key results, including the observed links between aerosol vertical profiles, BLH, static stability, and the predictive capability demonstrated in the study.

Minor comments:

Comment 1. Page 6. Figure 3. Replot the figure with proper y-axis labeling. Please show the y limits and major and minor ticks in both the axes.

Comment 2. Figure 10: Can you please explain what was the motivation behind choosing a very wide variations in the averaging window and the frequency (even in Figure 13)?

Comment 3. Figure 13: Is the dark gray line showing aerosol concentration? Could you please clarify that in the legend?