The study by Mei et al. presents tethered balloon measurements complemented by surface-based and remote sensing observations and ancillary calculations conducted during the TRACER campaign, offering a highly valuable contribution to the field. The work builds upon previous research and draws on multi-platform measurements and derived data techniques. The detailed analysis effectively demonstrates the necessity of integrating distinct observation types to gain a more comprehensive understanding of the atmospheric processes involved. In particular, the aerosol concentrations and estimated CCN profiles provide new insights into the vertical distribution of aerosol properties as influenced by air mass origin. The extensive set of tethered balloon profiles enables the identification of distinct clusters and provide compelling statistics spanning the summer through early fall seasons. The case study from September 6–7 further illustrates the complexity of the atmospheric processes at play and reaffirms the critical role of complementary observational approaches in constructing a temporally and spatially coherent narrative. The authors demonstrate a clear awareness of the study's limitations, including the absence of vertical profiles of particle number size distribution and CCN measurements, the restricted temporal scope of the dataset, and the constraints on the tethered balloon's ability to operate above the boundary layer more frequently. Overall, the manuscript is of high scientific quality, is well-structured, and reads clearly. I recommend accepting the paper subject to minor revisions.

Response: Thank you very much for the encouraging comments.

Minor comments:

The description of how the back trajectories are obtained might be better suited to the Data & Methodology section (lines 205-214), after which the results can be directly introduced in section 3.1 (from line 215 onward). A similar consideration might apply to the description of the Principal Component Analysis, which could also be relocated to the methods section.

Response: Thank you for this suggestion. We have moved both the back trajectories and PCA description to the main "Data & Methodology section" as section "2.4 ARM Air Mass Back-trajectory Analysis"

Ground-based Scanning Mobility Particle Sizer, POPS, and CCN counter instruments appear to have been operated during the deployment, though the specific location remain unclear. For this study, it would be valuable to incorporate ground-based particle number size distributions from 10 nm to 3000 nm. Presenting monthly medians alongside 25th–75th percentile ranges would help visualize concentrations

for each mode and assess seasonal variations, providing surface-level measurements representative of the tethered balloon deployment periods. A similar figure could be envisioned for CCN, enabling a direct comparison between measured and estimated CCN at the ground. Additionally, markers representing surface measurements should be displayed at the base of the profiles in Figure 4 for each cluster. If data are available, markers corresponding to measured CCN at the surface should also be added into Figure 7.

Response: Thank you so much for your suggestions. We added additional information about other ground-based instruments at the ANC site to Table 2. Additionally, the ground-based particle number size distribution plot, similar to the reviewer suggested, has been published before in one of our referred paper Li et al. 2025 (included below). We also agree that it will be useful to include the ground data with the TBS data. However, both Figure 4 and Figure 7 are very crowded now. Thus, we included an additional supplemental Table S2 to present that information (included below).

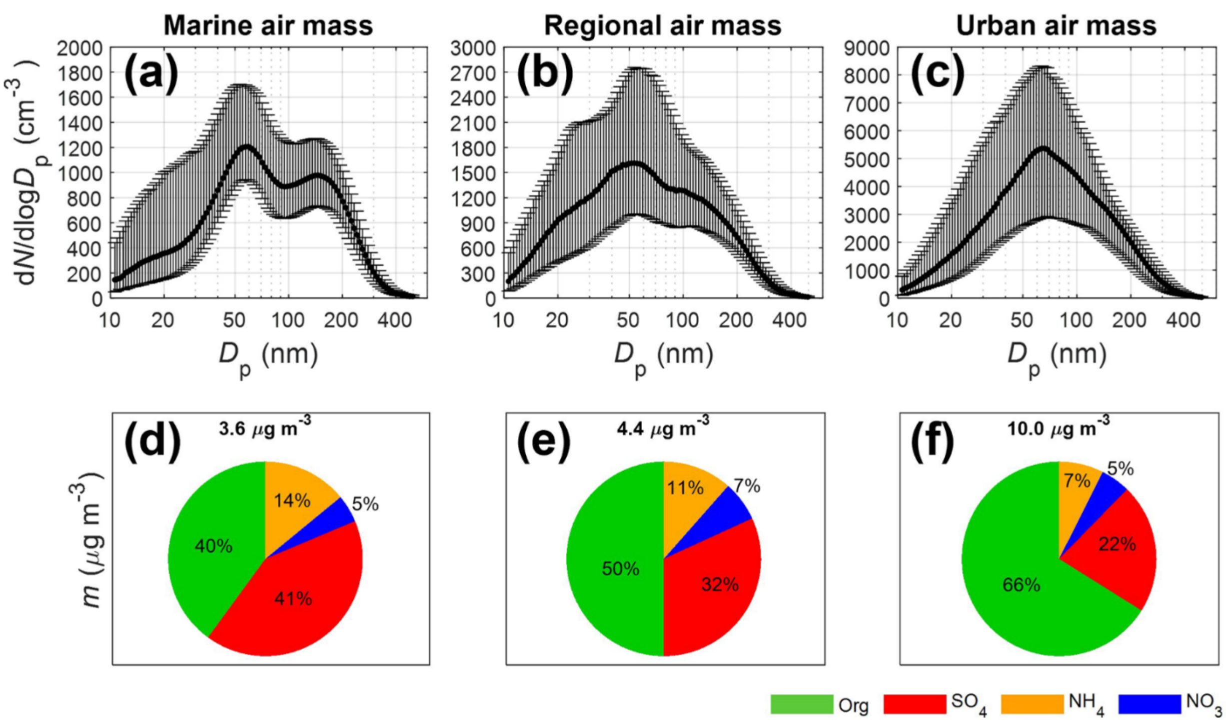

Figure 3 (from Li et al. 2025) First row (a–c): averaged aerosol size distributions during IOP in (a) marine, (b) regional, and (c) urban air masses. The solid line represents the median values, and the error bars indicate the 25th and 75th percentiles. Second row (d–f): averaged NR-PM1 mass concentrations and fractions in (d) marine, (e) regional, and (f) urban air masses.

Table S2. Ground-based measurements compared with the TBS measurements for the periods under different cluster influences.

	Cluster 1 - Marine		Cluster 2 - Mixed influence		Cluster 3 - Continental	
	Ground	TBS	Ground	TBS	Ground	TBS
T (C)	27.86±4.07	31.81±2.82	28.25±4.68	30.62±2.99	26.55±4.63	29.65±2.96
RH (%)	78.18±18.72	50.85±23.32	74.37±20.10	57.06±22.00	71.26±20.16	58.98±17.6
CPC ($\times 10^3$ cm^{-3})	2.21 ± 2.72	2.14±2.75	2.71 ± 2.44	3.77±3.20	6.87 ± 7.83	6.08±3.88
CCN (cm^{-3}) SS = 0.63%	647 ± 552	796±753	834 ± 819	1530±1490	2395 ± 2551	2379±1720

Can you please clarify how the flights were grouped into cluster in section 3.2? It appears that the air mass origin defining each cluster influenced entire days or extended periods, allowing individual flights to be categorized accordingly. However, as illustrated by the case study on September 6, a vertical profile was influenced by two distinct air masses associated with Custer 1 and 2. In the case of such decoupled air masses, how were the corresponding flights classified? Alternatively, given that decoupled profiles were rarely encountered during the deployment, were they excluded from Figure 4? Including a figure, table, or calendar displaying the tethered balloon flight times alongside their identified clusters would help visualize the temporal distribution of the air masses sampled throughout the deployment period.

Response: Thank you for the reviewer's question. For the flights we carried out during the TRACER campaign, we didn't observe any other decoupled air mass cases other than September 6. Thus, we excluded it from Figure 4 (now Figure 6). We have clarified that in the text. "Note that September 6 flights were influenced by both Cluster 1 and Cluster 2 and were excluded from the cluster-influenced figures. "

Given the estimated CCN concentration is based on particle hygroscopicity, which is influenced by the volume fraction of each chemical species (Section 2.3), were the volume fractions adjusted to reflect the chemical composition observed aloft in the upper part of the profile on September 6 (Figure 9)? If so, did this yield a different κ_{CCN} value, and, consequently, distinct estimated CCN concentrations compared to those derived for the lower part of the profile? Alternatively, are changes in estimated CCN seen above 1000 m in Figure 8a solely attributable to variations in aerosol number concentration?

Response: We thank the reviewer for this careful and important question, which has prompted us to clarify our approach and to quantify its associated uncertainty. To answer the reviewer's final question directly: no, the volume fractions were not adjusted with altitude, and the variability in estimated N_{CCN} above 1000 m in Figure 8a therefore arises solely from the measured aerosol number–size distribution.

We fully agree with the reviewer that the decoupled air masses aloft differed in composition (Figure 8) and, hence, in density, mixing state and hygroscopicity from the air within the surface layer. To assess how strongly this affects our conclusions, we have conducted another study led by Li et al. (submitted to the Journal of Geophysical Research – Atmospheres and is currently under review). We found a critical regime shift governing CCN activation: at *SS* below 0.3%, vertical variability of CCN concentration is primarily driven by aerosol chemical composition. At *SS* above 0.3%, the vertical differences are increasingly governed by particle size distribution and total number concentration, with chemical composition playing a secondary role. Thus, we kept the current figure and added the text below in the manuscript. "Because size-resolved chemical composition was not measured continuously along the vertical profiles, N_{CCN} was computed using a single, height-invariant hygroscopicity parameter for each profile. This leads to greater uncertainty in the estimated NCCN, especially for SS values less than 0.3% (Li et al. under review). "

A compelling case is presented in Section 3.5.3 , differentiating between what might have been anticipated from the case study and the conditions that were actually observed by combining aerosols, and thermodynamic variables. The importance of integrating multiple observation types to fully capture such an event is clearly demonstrated. Could you elaborate further on the broader implications of these findings from a climate perspective? What insights does this case study offer within a larger climate context? What does it reveal about our understanding of the climate processes for this region?

Response: Thank you for your suggestions. We have added additional paragraph to elaborate further on the broad implications of these findings, "This case study reinforces a fundamental hierarchy of controls on deep convection, in which thermodynamic environment and mesoscale dynamical forcing act as first-order controls while aerosol effects serve only as second-order modulators: on September 7th, the combination of enormous CAPE, deep moisture, and robust lifting from colliding cold pools and a bay-breeze front virtually guaranteed convection regardless of aerosol loading, meaning aerosols may have shaped droplet size, precipitation efficiency, or intensity but not whether storms occurred. This carries direct significance for forecasting—accurately representing boundary-layer circulations and cold-pool dynamics matters more than resolving aerosol fields—and offers a broader cautionary lesson that dramatic clean-versus-polluted day contrasts can also reflect variation in underlying meteorology. At the regional scale, the primary finding is that aerosol character and convective favorability are not independent but dynamically coupled through the same circulation systems: the similar mesoscale processes that imported moisture, eroded inhibition, and enhanced instability also delivered a high-CCN aerosol population, such that clean marine air tends to arrive with stable, moisture-limited conditions while polluted continental inflow coincides with convectively primed environments. Because the region's aerosol and convective climatologies co-evolve with the prevailing flow, understanding—and ultimately projecting—convection in this coastal setting requires treating the circulation–thermodynamics–aerosol system as an integrated whole, particularly as variability at the decadal time scales alters land–sea thermal contrasts, boundary-layer moisture, and aerosol transport pathways in ways no single-variable analysis could anticipate."

Specific comments:

Line 105: " a co-located microwave radiometer", which model was used? Is there a publication associated with this instrument?

Response:

We revised the sentence as: "a co-located 3-channel RPG-G5 microwave radiometer provides time-series measurements of brightness temperature from three channels centered at 23.8, 30, and 89 GHz (Cadeddu et al., 2013), which are used to retrieve precipitable water vapor (PWV) and liquid water path (LWP) (Turner et al., 2007)."

Reference: "Turner, DD, SA Clough, JC Liljegren, EE Clothiaux, KE Cady-Pereira, and KL Gaustad. 2007. "Retrieving liquid water path and precipitable water vapor from Atmospheric Radiation Measurement (ARM) microwave radiometers." IEEE Transactions on Geoscience and Remote Sensing 45(11): 3680−3690, https://doi.org/10.1109/TGRS.2007.903703 " and "Cadeddu, M. P., Liljegren, J. C., and Turner, D. D.: The Atmospheric radiation measurement (ARM) program network of microwave radiometers: instrumentation, data, and retrievals, Atmos. Meas. Tech., 6, 2359–2372, https://doi.org/10.5194/amt-6-2359-2013, 2013."

Line 141: Table 1. Would it be possible to replace the "instruments" column with two separate columns: one listing the name and model of each instrument, and a second one providing the manufacturer's name? The current column includes some model names but does not consistently identify the manufacturers.

Response: Thank you. Revised.

Line 168: For clarification, where were the ground-based SMPS, POPS, and CCN counter located? Were they deployed at La Porte?

Response: Thank you. We have revised the manuscript, added Table 2 and clarified the location of the ground-based measurements at the ANC site.

Line 183: The calculation of CCN using the κ-Köhler theory assumes a uniform aerosol chemical composition from the surface up through the vertical column. Based on the meteorological profiles obtained from the tethered balloon, were any decoupled layers aloft identified during the campaign beside the case study? If so, do you have any statistics on their frequency of occurrence?

Response: Thank you. September 6 is the only decoupled layer aloft we observed during the TRACER, based on the back-trajectories.

Line 190: Has the uncertainty introduced by the assumption of vertically uniform chemical composition been quantified in previous studies?

Response: We agree that this uncertainty is worth further study. However, we didn't find any quantitative study for this region. We revised the sentence to, "These missing species, together with the assumption of a vertically uniform chemical composition, can influence the bulk hygroscopicity and introduce additional uncertainty into CCN concentration estimates - a source of uncertainty that warrants further experimental study.(Schulze et al., 2020; Ren et al., 2026)" The last two references provide informative approaches for further investigation.

Line 236: Could you provide a reference describing the Principal Component Analysis methodology? This would be helpful for readers unfamiliar with this method, allowing them to access further details on the approach and potential examples of its application.

Response: Thank you for this suggestion. We have added the paragraph below to the method section. "Principal Component Analysis (PCA) was employed to reduce the dimensionality of the dataset while retaining the maximum amount of variance present in the original variables. PCA is a multivariate statistical technique that transforms a set of possibly correlated variables into a smaller number of uncorrelated variables known as principal components (Jolliffe & Cadima, 2016). These components are linear combinations of the original variables and are ordered such that the first principal component accounts for the largest possible variance in the data, with each successive component capturing the maximum remaining variance under the constraint of being orthogonal to the preceding components."

Line 280: Please rename N_{pops} and N_{cpc} using the corresponding particle size ranges, $N_{135\text{-}3000}$ $N_{>10}$ in both the text and figures.

Response: Thank you for your suggestions. We revised the text to N_{cpc} (>10 nm) and N_{pops} (>135 nm) and also updated the figures.

Line 289: Subtle temperature differences are described at the surface between the clusters. Are these differences significant? Could they be explained by the topography and/or the wind direction? If these differences cannot be attributed to air mass origin, presenting the numbers without further context may be misleading to the reader.

Response: Thank you for pointing this out. We have revised the text, "Subtle differences in the vertical profiles of temperature and relative humidity were observed among the three air-mass clusters. However, these differences may also reflect local cloud cover, solar heating, wind, soil moisture, and boundary-layer mixing effects. Thus, the small statistically detectable difference does not establish that air-mass origin alone caused the difference. "

Line 300: It seems that DMS has not been introduced before.

Response: Replaced DMS with "volatile dimethyl sulfide"

Line 316, Figure 4: Could the surface measurements be added at the bottom of the vertical profiles, using a distinct maker or other visual differentiation? It would make it insightful to display the tethered balloon observations and ground-based reference instrument measurements in the same figure. This comment is associated with previous general feedback on displaying surface-based observations.

Response: Thank you. We revised the manuscript and added Table S1.

Line 458: Could you specify the take-off and landing times for the flights on September 6 (and September 7)? Although the flight is represented in Figure 6 between 15:30 and 18:30, a reminder in the text would help the reader follow the discussion more easily.

Response: Thank you. We have added the take-off and landing times to the text.

Line 465: Have you examined 5-day back trajectories for September 6 and 7? Do the longer back trajectories support the assumption of long-range transport for the overlying aerosol layer? Additionally, have you considered visualizing the back trajectories with time on the x-axis, altitude on the y-axis, and an indication of whether the air mass traveled below or above the boundary layer during its transport? Potential supplementary figures presenting these analyses could further reinforce the narrative of the case study.

Response: Thank you for your suggestions. We examined 5-day back trajectories for September 6 and 7, and the longer back trajectories support the assumption of long-range transport for the overlying aerosol layer. In this study, we focused on 24-hour back trajectories because longer trajectories become increasingly uncertain, especially for higher-level starting points (due to the much stronger winds and frequent wind shear). One of the references about ARMTRAJ published in the ESSD (https://essd.copernicus.org/articles/17/29/2025/) discusses this in detail, e.g., in its Sect. 3.1 and the first conclusions section (Sect. 5).

We have included additional plots visualizing the back trajectories as supplementary figure S7 and S8.

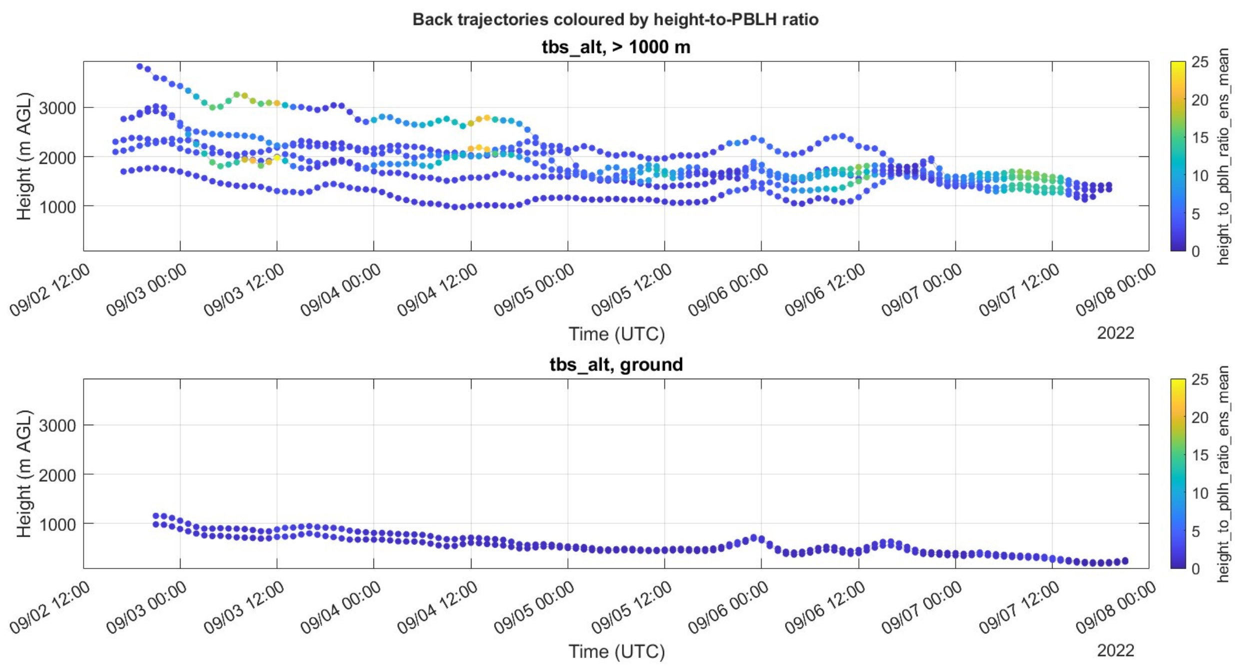

Figure S7. The time series plot shows the temporal evolution of air-mass parameters along trajectories initialized at the center coordinates together with the ensemble mean of the height-to-pblh ratio for September 6 flights.

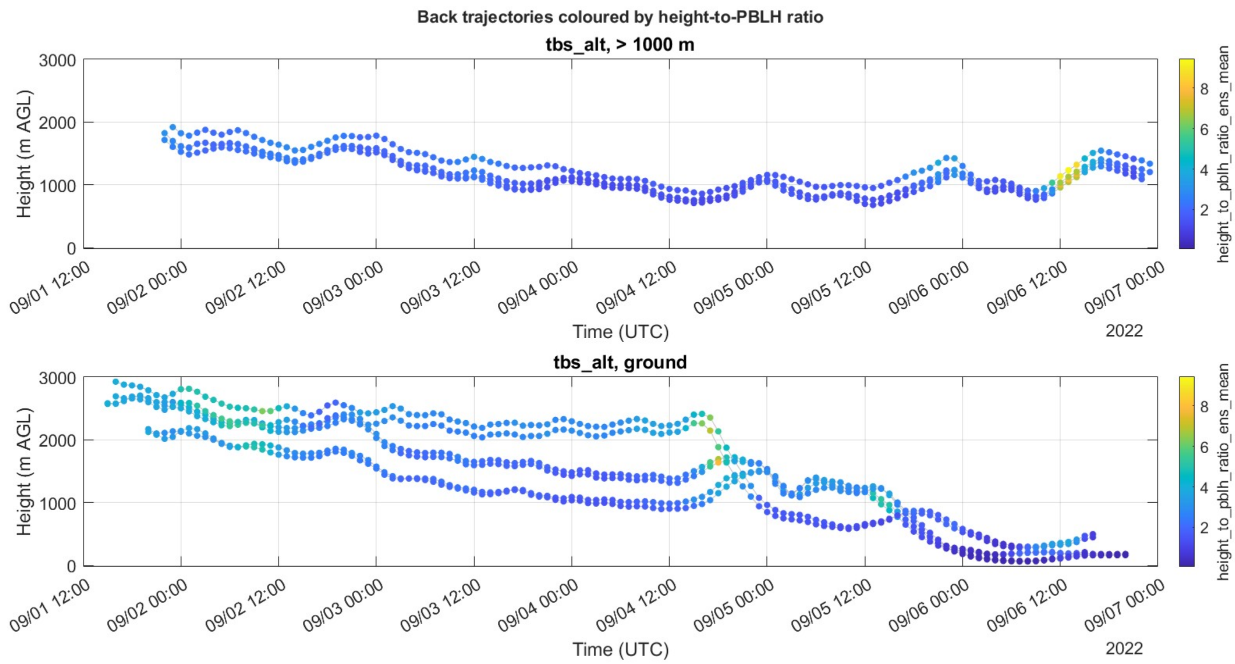

Figure S8. The time series plot shows the temporal evolution of air-mass parameters along trajectories initialized at the center coordinates together with the ensemble mean of the height-to-pblh ratio for September 7 flights.

Line 475, Figure 8: Could the pressure levels in Figure 8c and 8d be converted to altitude? It would facilitate a more direct comparison with Figure 8a.

Response: Thank you for your suggestion. We updated Figures 8 and 10.

Line 520: CAPE has not been introduced before.

Response: Revised. Convective Available Potential Energy (CAPE)

Technical comments:

Line 62: A period is missing between “and chemical composition” and “Airborne platforms”.

Response: Thank you, corrected.

Line 397: When referring to Figures (a)–(c), do you mean Figures 7 and S2? Please clarify and ensure the figure references are correct.

Response: Thank you, corrected. I have updated the text and also Figures 7 and S2.

Line 460: a period is missing between “increase with altitude" and "These trends”.

Response: Thank you, corrected.

Line 471: Figure S8 is introduced before Figure S4 in the reading order. Please consider renumbering the supplementary figures to follow the sequential order in which they are first cited in the text.

Response: Thank you, corrected.

Line 483 and following paragraphs: mb and hPa are used throughout. Please harmonize the units to SI standards by using hPa consistently throughout the manuscript.

Response: Thank you for catching this. The manuscript has been corrected to hPa.

Figure S6: The legend appears to be incorrect. Please update it with the appropriate and correct legend.

Response: Thank you for catching this. The manuscript has been corrected.