Article review comments for: "In situ vertical observations of the layered structure of air pollution in a continental high-latitude urban boundary layer during winter", Atmospheric Chemistry and Physics, Manuscript ID: egusphere-2024-2863

We thank the editor and reviewers for the handling and commenting of our manuscript. Please find below our point-by-point responses to each of the reviewers' comments, including the modifications we have made to the manuscript. Reviewer comments (RC) are in black text and the answers to reviewers (AR) are given in blue text, and excerpts of the revised manuscript are given in *blue italic text.*

Reviewer 1:

Review of "In situ vertical observations of the layered structure of air pollution in a continental high latitude urban boundary layer during winter" by Pohorsky et al. (2024)

The authors present in-situ vertical measurements of aerosols and trace gases using tethered balloon system during wintertime January – February 2022 in Fairbanks, AK, USA. The manuscript is suitable for publication in EGUsphere, the conclusions are solid, valuable and clear, but only of local impact as authors point out themselves. Even though I am marking it as: accept with minor revisions, all below are mostly suggestions to improve the manuscript from the point of potential reader.

We would like to thank the reviewer for the positive and constructive comments on the manuscript. Answers to individual comments are provided below.

General comments:

RC1: While reading the manuscript it is obvious that a lot of work and subsequent analyses were done. However, the manuscript is extensive in length, and I would encourage the authors its shortening. It is not a critical point, but the authors in many places dive into unnecessary details with very low impact on conclusions and results, e.g. in the section 4.3 they present in detail method, models, analysis and the result of all that effort is that the models do not work (low correlations) and would probably perform better at higher winds. The authors made in-situ measurements and determined the mixing height, the models do not work very well in this particular case, it is ok, models are just models. It could be compressed into one paragraph and the interested reader you can direct to supplementary materials (SM), which are very extensive already. Similarly, the section 3, there is detailed discussion on radiative balance, all that would make very good sense and great value if the Helikite payload included 3D anemometer, and radiation sensors.

AR1: Thank you for your feedback. Following your suggestion, we put efforts in shortening the overall manuscript. More specifically, section 3 has been shortened to discuss only the essentials of the effect of the SCF on the structure of the temperature profile and Fig. 4 has been removed. Section 4.3 has been moved to the SI and is referred to at the end of Sect. 4.2. Section 6 on the plume analysis has been shortened and some parts have been moved to the SI following suggestions from reviewer 2.

RC2: The authors could easily divide the manuscript into two separate manuscripts a) definition of multi-layered structure and b) its implications for air pollutants.

AR2: While we agree that the manuscript is extensive, we believe that the pollution composition analysis is linked to the first part and constitutes a comprehensive message. Nevertheless, based on comments from both reviewers, we made efforts to reduce the overall length of the manuscript (see AR1).

RC3: For any vertical profiling measurements, it is very important to persuade the reader your measurements are correct. Since there was a surface reference available, I would encourage the authors to provide a figure and a paragraph, where it is clear all profile measured variables when payload is on Helikite are reasonably correlating with the surface reference. The lab calibration of instruments or inter-comparison when the payload is measuring from the same inlet at surface is not enough, especially when the profiling is done in such an extreme environment.

AR3: Following the reviewer's suggestion, we added a short section after section 2.2 in the methods to describe the comparisons between Helikite measurements at the surface with measurements performed in the blue hut or from the weather station. A figure has been added to the SI (Fig. S3).

Lines 269-278:

2.3 Comparability between vertical and ground-based measurements

To assess the comparability of measurements conducted with the MoMuCAMS system with those obtained at the ground, data measured below 2 m for each profile were averaged and compared to simultaneous measurements from the blue hut or the weather station. Figure S3 presents the results for number concentrations measured using (a) the POPS ($N_{186\text{-}3370}$) and (b) the SEMS and mSEMS ($N_{8–270}$). Figure S3c shows the comparison of temperature measurements. The measurements demonstrate good agreement, with regression slopes of 0.92, 0.90 and 0.98 respectively. The coefficients of determination (R^2) equal 0.95, 0.97 and 0.99, respectively. Since aerosol light absorption coefficients, as well as CO and O_3 mixing ratio measurements, were not duplicated on the ground during the flights, direct comparisons were not possible.

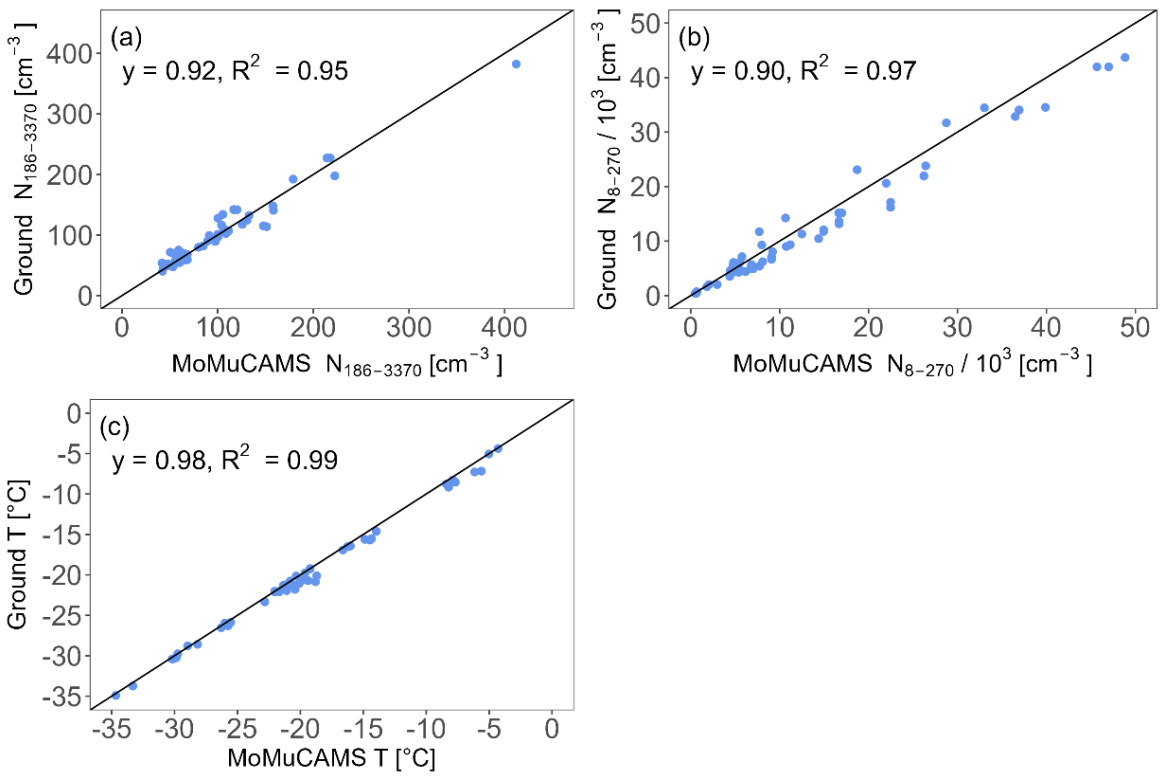

Figure S3 Scatterplots of MoMuCAMS measurements in the lowest 2 m of each profile (average) and corresponding ground-based measurements for (a) $N_{186\text{-}3370}$ (cm^{-3}), (b) $N_{8\text{-}270}$ (cm^{-3}), and (c) temperature (°C). The black diagonal represents the 1:1 line. The regression slope of a linear fit and the coefficients of determination (R^2) are indicated in each panel.

RC4: The authors describe and define the complexity of Arctic boundary layer and its detailed structure. The definitions are mostly based on temperature measurements, however cross validated by pollutant concentrations in vertical column. All the complex layering in boundary layer and lower mixing height has strong implications for local air quality, however the manuscript even though having "air pollution" in the title is not using any of AQ nomenclature or comparison to EPA limits considering aerosol load – particulate mass concentrations PM1, PM2.5, etc., or gaseous pollutants.

AR4: Thank you for pointing this out. As the reviewer notes, our study does not use standard air quality metrics like particulate mass concentrations (PM_1, $PM_{2.5}$) or EPA-defined pollutant thresholds. This decision was intentional, as our study site, located in the suburbs, typically experiences air quality well within EPA's National Ambient Air Quality Standards for both particulate matter and gases. Furthermore, our instruments are not necessarily standard instruments for AQ measurements. Therefore, applying these metrics was less relevant to our objectives.

Instead, our focus is on the mechanisms governing pollution layering and its link to the stable boundary layer structure. These findings can be extrapolated to more polluted regions, including central Fairbanks, where other ALPACA studies (see Simpson et al., 2024), have specifically investigated pollutant levels. By concentrating on the processes driving pollution stratification, our work complements such studies, offering insights that are transferable to urban areas with higher pollutant loads.

Technical comments:

RC5: Page 8, line 204: Please add what inlet was used for surface measurements, what cut-off, what flows, heated/not heated, any losses accounted for? Inner diameter is more important for definition of laminarity in the sampling lines.

AR5: The text has been modified to provide more details on the inlet, the particles transmission efficiency and corrections applied to the data as requested. We have included a figure in the SI showing the calculated inlet transmission efficiency.

Line 217-225: *A **heated** 1.8-m long stainless steel aerosol sampling line **(8 mm inner diameter)** sampled total suspended particles (no cut-off diameter). **The nominal flow rate was 3.48 lpm (liters per minute).** The inlet was equipped with a custom-made silica gel column (similar to a TSI 3062 model) to ensure relative humidity below 40 % according to the Global Atmosphere Watch aerosol measurement recommendations (Wiedensohler et al., 2014). **Behind the dryer, the sampled air was distributed to the different instruments through an isokinetic flow splitter. Conductive silicon tubing was used to connect the different branches of the flow splitter to the instruments. Instruments were placed to minimize the tubing length (~ 60 cm on average) and bends. Losses in the inlet were characterized using the Particle Loss Calculator (PLC) (von der Weiden et al., 2009). Figure S2 shows results of the calculated transmission efficiency in the inlet. The transmission efficiency was above 90 % within our measurement size range. The PNSD data were corrected for particles losses.***

RC6: Page 10, line 276: just for consistency through the manuscript use …gradient (22.7C .100 m^{-1})

AR6: The temperature gradient notation has been modified as suggested throughout the manuscript.

RC7: Figure 7. The appearance of geometric standard deviation of number size distribution used here is quite surprising, since there was not mentioned anything about the measured size distribution, the shape, modality, etc., yet. Why not use, at this point, the total particle concentration from the mSEMS, moreover the difference between POPS and SMPS would indicate presence of nanoparticles (the harmful ones) in each layer.

AR7: We have adapted the figure (now Figure 6) to include the integrated number concentration between 8 and 186 nm as well, in the same panels as the number concentration measured with the POPS. We believe however that the geometric standard deviation is a valuable information to distinguish layers constituted mainly of fresh emissions from the background layers or layers where pollution is already more diluted. It offers a new and complementary approach to identify the pollution layers and is therefore worth showing. Although the PNSD has not been addressed in detailed at this stage, the reader is referred to the appropriate section at lines 458 and 468 and the PNSD measurement has also been addressed in the method section (2.2.1).

RC8: Figure 13. How do the surface measurements of PNSD fit into this figure? Also, are the surface measurements satisfactory indicator of local AQ?

AR8: To address the reviewer's comment, we have included the SEMS measurements from the blue hut during the discussed flight periods. The particle number and volume size distributions have been added to Fig. 13 (now Fig. 11) to illustrate the good comparison between the ground measurements and

measurements in the MsL. We mention this in the text. Hence, the MsL averaged concentrations and distributions are typically representative of ground-based observations.

To answer the second question, we would refer the reviewer to Sect. 5.1 (lines 650-655), where we discuss the representativeness of the measurements performed at the UAF farm site. The measurements performed at the site are indeed not necessarily representative of the AQ in the larger Fairbanks area but as for the local conditions of the site. That said, ground-based measurements are indeed a satisfactory indicator as they represent the AQ at breathing level, where most people live. The aim of this manuscript is however not the evaluation of the AQ in Fairbanks but the vertical layering of pollution and its link to the thermodynamic structure of the atmosphere (see answer to RC4).

RC9: Figure 14. The plume particle concentration enhancement (Nc of POPS) of 30/ccm is not very significant, suggesting that the edge of the plume was measured. It seems that there are high dynamics in the plume itself (condensation and coagulation) due to high temperature gradients. There is not mentioned the dominant size of the measured particles, 2 or 3 micron particles would have probably a good chance of deposition at low winds. Do the different power plants (diesel or coal) plumes have any specific signature in number size distribution?

AR9: We agree that the enhancement is relatively low, although it still represents a relative increase of ~ 50 % compared to the WPBL concentration. It is however likely that we measured the plume edge given that the plume was not measured on the previous profile. As indicated in the text, the plume was advected above the measurement site because of a wind direction change, which probably just allowed to measure the edge of the plume. As for the deposition of 2 or 3 micron particles, we believe that it is unlikely that gravitational settling would be fast enough at these timescales, based on Stoke's equations. The main driver for wet deposition is therefore likely related to turbulence at higher wind speeds and deposition by contact with the surface, which is not the case here.

We have modified the text at lines 841 to 843 to indicate that sampling likely occurred on the edge of the plume.

These enhancements are, however, only indicative, as it is uncertain how the ΔX of a certain tracer evolves from the edge to the center of a plume. Given the observed dynamics described above, it is likely that the observations were made on the edge of the plume where the enhancements are expected to be lower compared to the center of the plume.

Regarding the dominant particle size, we unfortunately lack sufficient mSEMS measurements within plumes to derive significant statistics distinguishing coal-fired from diesel power plants. From the limited observations available (three plumes), the particle number size distribution (PNSD) appears to be dominated by Aitken mode particles, primarily between 25 and 35 nm. However, due to the limited sample size, we cannot draw definitive conclusions from these measurements. As a result, we focus on the tracer-tracer analysis in Sect. 6.2.

RC10: Page 43 in Conclusions, have not found Fochesatto et al. in references.

AR10: Thank you for spotting it. We have added the missing reference.

Reviewer 2:

The paper address a very important and interesting topic connected to vertical profiles in the Arctic. It is quite long and any effort to shorten it should be done.

We would like to thank the reviewer for the constructive comments on the manuscript and acknowledge the suggestion to shorten it. More specifically, based on the comments of the two reviewers, section 3 has been shortened to discuss only the essential effects of the SCF on the structure of the temperature profile. Section 4.3 has been moved to the SI. Section 6 on the plume analysis has been shortened and some parts have been moved to the SI.

Here below major and minor comments.

Major comments:

RC1: Line 158: MAC of 7.5 m2 g-1 (at 550 nm). Please compare your data with ones of Savadkoohi et al. (2024; https://doi.org/10.1016/j.envint.2024.108553) and use the proper eBC nomenclature accordingly to the aforementioned paper.

AR1: The suggested study presents interesting insights into the use of filter-based absorption photometers for the determination of eBC concentrations and how important a correct determination of the MAC value is. Based on the presented results, it appears that the MAC values derived from our approach are relatively low (6.3 to 6.6 $m^2 g^{-1}$ at 624 nm), translating to MAC values between 6.1 and 6.4 at 637 nm using equation Eq. (2) and the range of AAE values reported in Sect. 5. These values remain however within the range reported by Savadkoohi et al. (2024) of 10.6 ± 4.7 $m^2 g^{-1}$. As mentioned in the manuscript, brown carbon from biomass burning is also a major contributor of air pollution in the Fairbanks area and is known to have very low MAC values, which would agree with the values we use. We remain aware that the use of a value that is too low can lead to overestimation of the eBC concentration but without direct comparison to EC measurements, we follow the theoretical approach described in the manuscript. We have added a note on that to make the reader aware of this and reported the range of MAC values used.

Lines 170-176: *Note that the determination of the eBC concentration highly depends on the appropriate quantification of the MAC value. Here we followed a theoretical procedure (cf. Eq. 2 and 3) based on values obtained from laboratory studies in the absence of direct elemental carbon measurements, yielding MAC values between 6.3 and 6.6 $m^2 g^{-1}$ at 624 nm, which is close to the nominal value of 6.6 $m^2 g^{-1}$ at 637 nm of the MAAP. These relatively low values can however lead to an overestimation of the eBC mass concentration as suggested by the study from Savadkoohi et al. (2024), which reported that local MAC values for the MAAP were typically higher than the nominal value of the instrument (10.6 ± 4.7 $m^2 g^{-1}$). In the absence of comparison with direct elemental carbon measurements, the reader should keep in mind the range of MAC values used to derive eBC in this study.*

For the nomenclature, we have adapted the text and used the term eBC, which is commonly used in the literature. Given that our method to determine the "local" MAC value differs from Savadkoohi et al. (2024) and since we do not compare different MAC values, we believe that eBC is more appropriate than another variation such as LeBC.

RC2: Lines 266-268: "The ε threshold was set to 0.8 °C per layer Δz based on visual examination of the resulting simplified profiles that confirmed that the major temperature inflection points were correctly captured by the adapted algorithm". Which is the physical meaning of this threshold?

AR2: The physical meaning was not specifically investigated here but would constitute an interesting analysis to identify what temperature gradient difference is meaningful in terms of atmospheric stability and turbulence. It is however beyond the scope of this paper since we do not have co-located turbulence measurements. Overall, the threshold was based on a subjective visual inspection of all profiles suggesting that the piecewise temperature profiles satisfactorily replicated the measured temperature profiles. As in Fochesatto et al. (2015), the relationship between the thermal gradient dT /dz, the preset threshold ε and the overall final error between the resampled temperature profile and the original temperature profile is not straightforward. A higher ε threshold will yield fewer layers in the piecewise temperature profile but larger differences with the measurements. Here the threshold was not selected based on a specific physical reason but rather on the visual examination of different thresholds applied to all profiles to capture the main temperature gradient layers. We have adapted the text to clarify this.

Lines 296-301: The physical meaning of this threshold was not further investigated as turbulence observations were lacking and the aim was primarily the identification of temperature inversions, their depth and mean temperature gradient. As in Fochesatto et al. (2015), the relationship between the threshold ε, the captured temperature gradient dT dz^{-1} and the overall final error is not straightforward and depends on the thickness of the layer. From the analyzed profiles, the temperature gradient difference between all pairs of adjacent layers had a median of 4.0 °C 100 m^{-1} with an interquartile range from 1.6 to 7.4 °C 100 m^{-1}. The lowest difference between two layers was 0.12 °C 100 m^{-1}.

RC3: Section 3 is quite long. Could you squeeze it or move some parts in supplementary material?

AR3: Following your suggestion, section 3 has been shortened. We removed Fig. 4 and the descriptive text, since this general description was not essential for the discussion of the observed conditions at the site. We also removed the text at lines 348 to 353 discussing alternative observations made by Maillard et al. (2022) since they were not further investigated in this study.

RC4: Section 4: MsL and ML: the cited Seibert et al. (2000) suggest to use gradient method to infer the mixing layer height. From your application you seem to use the point at which the derivative is zero and not at the minimum value. Please discuss this choice

AR4: A method based on the strongest gradient or on the second derivative has been tried but given the complex layered structure or the cases where the concentration gradient was directly negative at the surface, these methods did not yield satisfactory results as they were not fully consistent with species concentration profiles. We therefore argue that the method used in our study is more appropriate to capture the height where the influence from surface emission is still perceptible in the concentration profiles and represent the layer where the mixing process is ongoing. We added the following discussion in section 4:

Lines 412-414: This method differs from methods that use the maximum absolute gradient or the second derivative to identify the MLH. Given that multiple layers were frequently observed or strong gradients occurred close to the surface, an automatic detection method based on the strongest concentration gradient is not applicable in the observed profiles.

RC5: Section 4 Line 410: weakly polluted background layer (WPBL): why not refers to it as residual layer? The explanation at the following 411-412 lines is just general: could you address a specific figure with measured WPBL and RL profiles?

AR5: As indicated in the text, we did not call this layer a residual layer because it is not necessarily formed by the same mechanism (i.e., from diurnal cycle). It is however, a possibility and without a knowledge of the history of the boundary layer structure, it is not always possible to say if the WPBL is indeed a classic

RL or if it stems from direct emissions above the ML. The WBPL is therefore a generic term that includes the RL as well. We have adapted the text to clarify this.

Lines 423-427: Here, we distinguish the WPBL from a residual layer (typically observed above the nocturnal boundary layer) because of the possible long-lived nature of the observed SBL. The observed pollution signature is therefore not necessarily a residual of a well-mixed boundary layer. Since, our observations do not provide a historic context of the boundary layer development, it is not necessarily possible to define if the WBPL corresponds to the residual of a previous higher mixing layer (i.e., classic RL) or if it is the result of direct emissions above the ML. Hence, the WPBL is used here as a generic term to describe the layer located between the ML and the clean background.

RC6: Section 4.1 Lines 455-459: despite the huge description of the second case in Figure 7 I do not agree with the interpretation. Figure 7e clearly show multiple temperature inversion layers and accordingly to Figure 7f,h there are two separate layer. The MLH should be placed around 50 m in this case even because the geometric standard deviation (Fig. 7g) increases up to 2 in the second layer. It is a mistake to place MLH at 100 m

AR6: Thank you for this input. We agree that this situation is complex and illustrates well the difficulty, even with in situ measurements, to identify the mixing layer height when the structure presents multiple layers. After careful consideration, we agree that this situation could exhibit a higher mixing layer height resulting from a previous state of the boundary layer with a more recent and shallower MLH at ~50 m when measurements were taken. Indeed, surface emissions appear to be trapped below the first inversion, hence h_{mix} should be identified there (39 m). Figure 7 (now Fig. 6) and the description paragraph have been adapted.

Lines 471-475: In the second example (Fig. 7e, f, g, h), the vertical profile exhibits a more complex structure with two strong temperature inversion layers, resulting in a layered structure of pollutants. ***Here, the mixing layer height is identified at 39 m. We observe a first increase from ~1.75 to 2 in σ_{geom} in the second layer (between 39 and 100 m) and to more than 2.25 above 100 m, indicative of increasing dilution with background air in each layer.*** *This specific example is only observed on one flight but illustrates the added benefit of σ_{geom} to identify h_{mix} in more complex situations.*

This modification of the MLH impacted results of the subsequent analysis in the manuscript. All figures have been reproduced with the new h_{mix} and the text adapted to report new values when needed. Changes are however minor and did not impact the general message of the study.

RC7: Section 4.3: I suggest to move all this section, Tables 3-4 and Figure 10 in supplementary material. It does not add significant information. The Pearson coefficients are so low that practically show the absence of any correlation

AR7: Following the reviewer's suggestion and the suggestions from reviewer #1, we moved this section to the supplementary material and added a short paragraph to refer to it and report the main observations.

Lines 567-574:

While the results presented in this section provide a direct assessment of the mixing layer height in the stable boundary layer around Fairbanks, tethered-balloon measurements do not represent a practical method for routine operations. To understand if h_{mix} can be predicted from ground-based measurements alone, a comparison between the observed h_{mix} and formulations of the SBL height based on surface flux measurements was performed. Details on the formulations and all results are presented in the SI. Generally,

all models have a negative bias and large root mean squared error in comparison to our derived h_{mix}, indicating that the extremely stable conditions observed in Fairbanks might represent a limit to these models, which further motivates the need to investigate pollution dispersion in the very stable boundary layer.

RC8: Figure 11: normalized vertical profiles are very clear but I suggest to add another panel, the one of the count median diameter of particles on the same size range used for the vertical profiles of geometric standard deviation. This enables to add the information of the average size together with this dispersion.

AR8: The figure has been adapted as suggested. We added the count median diameter on the same panel as the geometric standard deviation.

A short description to complete this addition has been added at lines 590 to 594:

For σ_{geom} and CMD, we observe an increase in both quantities above h_{mix}. The observed shift is explained by a PNSD dominated by freshly emitted Aitken mode particles in the ML. Above the ML, the air is more diluted with background air that contains larger aerosol particles. This results in a shift towards a larger σ_{geom} and CMD. A detailed analysis of the PNSD in each layer is discussed in Sect. 5.3.

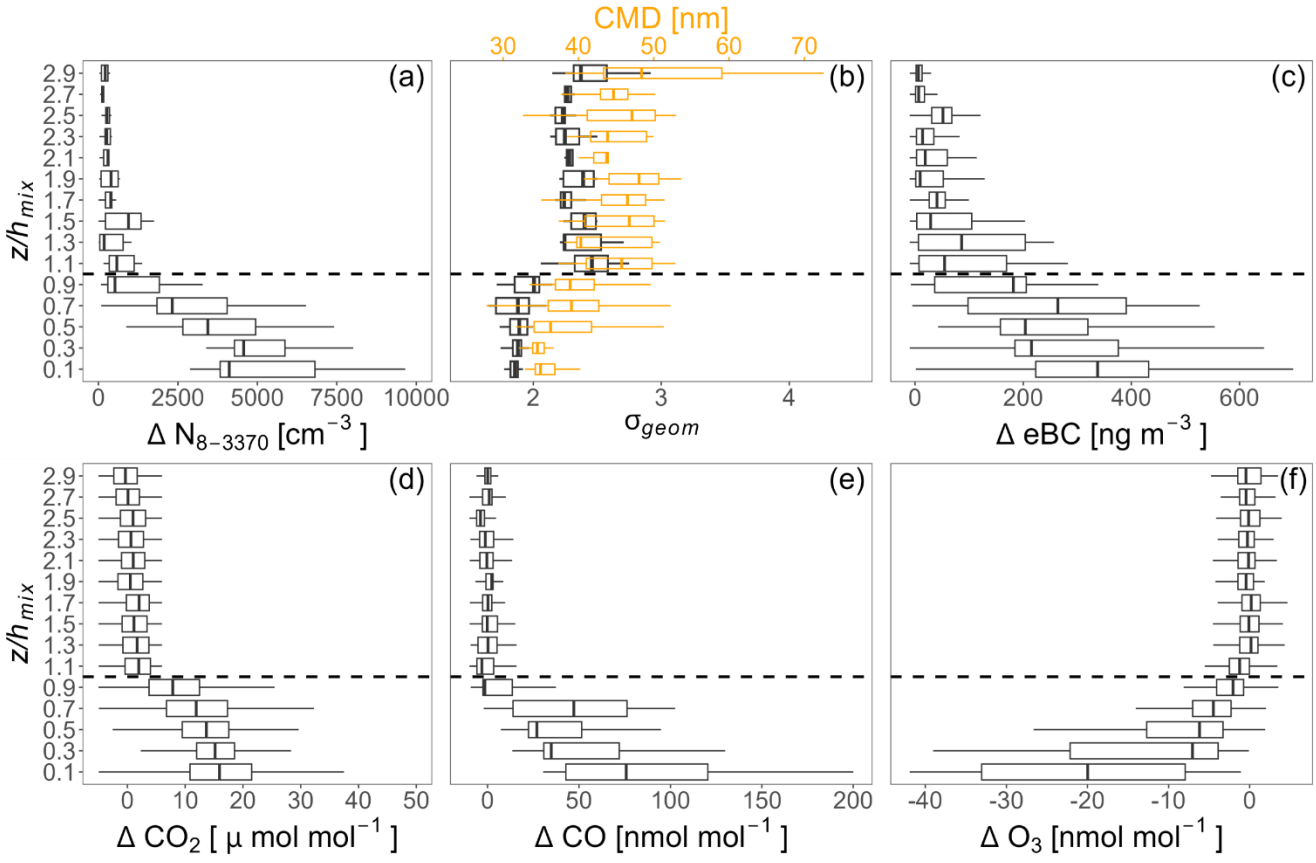

Figure 9 Vertically normalized analysis of various tracers' profiles. (a) $N_{8\text{-}3370}$, (b) σ_{geom} and CMD (orange) of the PNSD from 8 to 270 nm, (c) equivalent black carbon concentration, (d) CO_2 mixing ratio, (e) CO mixing ratio and (f) O_3 mixing ratio. All values except for (b) are expressed as the difference to the profile's WPBL average concentration. The altitude z is normalized by h_{mix}. The boxplots represent the median and interquartile range, the wiskers' length equals to 1.5 times the interquartile range.

RC9: Lines 770-772: The eBC over Ny-Ålesund and Svalbard is taken from Mazzola et al. (2016). However the situation there is more complex. I suggest to complete the comparison of the obtained profiles with the ones reported by Ferrero et al. (2016; doi:10.5194/acp-16-12601-2016), by Marcowicz et al. (2017; https://doi.org/10.1016/j.atmosenv.2017.06.014) and by Cappelletti et al. (2022; https://doi.org/10.1016/j.atmosenv.2022.119373).

AR9: We have completed the comparison and included the suggested references. Indeed the situation over Svalbard presents its own complexities and is also different from the situation in Fairbanks. This is mainly due to the distance to main sources of emissions. In our study, we are located very close to the sources, hence we observe the highest concentrations at the ground, despite any vertical mixing or direct emissions above the ML. But our observations of increased concentrations in the outflow of Fairbanks in the WPBL illustrate well the importance of elevated transport of anthropogenic emissions and the potential impact on Arctic regions, which is also observed in the studies in Svalbard. In our case, we observe the phenomenon shortly after the emission, while over Svalbard, the observed plumes are already more aged. We have included the suggested studies to reinforce the comparison and discuss these points, while trying to keep it as succinct as possible.

Lines 714-725:

Upper level transport of eBC was also observed in profiling studies performed over Ny-Ålesund (e.g., Cappelletti et al., 2022; Ferrero et al., 2016; Markowicz et al., 2017; Mazzola et al., 2016). Increasing concentrations with altitude up to 1000 m AGL with values between 100 and 300 ng m^{-3} were reported by Mazzola et al. (2016). Cappelletti et al. (2022) reported mean concentrations of 110 ± 10 and 150 ± 30 ng m^{-3} below and above 500 m, respectively. Ferrero et al. (2016) identified different atmospheric profile types during spring. Their "decoupled negative gradient" (DNG) type presents a similar thermodynamic structure to the profiles observed in Fairbanks with an SBI and an EI. For these types, eBC was more elevated between the SBI and the EI with mean concentrations of 121 ± 5 ng m^{-3}. This situation is similar to what we observe in the outflow of Fairbanks above the ML which illustrates similarities of efficient and direct transport of anthropogenic emissions (Stohl et al., 2007). Differences between our study and measurements in Ny-Ålesund come from the distance to the emission sources. In our study, the highest concentration of eBC is still observed at the surface due to the presence of emission sources.

RC10: Section 6.1 and 6.2: these sections are quite long. Please consider the possibility to move them in supplementary material or to short them.

AR10: Following the reviewer's comment and in an attempt to generally shorten the manuscript we have shortened Sect. 6.1 and 6.2. While we believe that these sections should not be completely moved to the SI as they provide valuable insights into elevated emissions of the power plants, we have simplified sect. 6.1 and moved the explanation of the plume identification (including Table 6) of Sect. 6.2 to the SI.

Minor comments:

RC11: Lines 119-120: Which is the expected impact of power plants in Fairbanks on your measurements?

AR11: The expected impact is the presence of elevated concentrations of particles and gases at higher elevations. We have modified the text to clarify this :

Lines 120-124: *The power plants emit particles and gases from tall stacks, which release emissions at higher altitudes. This elevated release height may lead to increased concentrations of pollutants in the upper portions of the boundary layer. As a result, the measured vertical profiles can reflect these elevated concentrations. The UAF power plant (a) had the most frequent influence on the vertical measurements due to its higher proximity but plumes from the other power plants from Fig. 1 were also sampled on several occasions.*

RC12: Lines 401-402: "Note that in certain situations, no MsL is observed and the concentration gradient is strongly negative directly from the surface (dashed lines in Fig. 6)": no red negative dashed line from the surface is present in Figure 6. Please add it as in the description at these lines.

AR12: Thanks for noticing this mistake. We have corrected Fig. 6 (now Fig. 5).

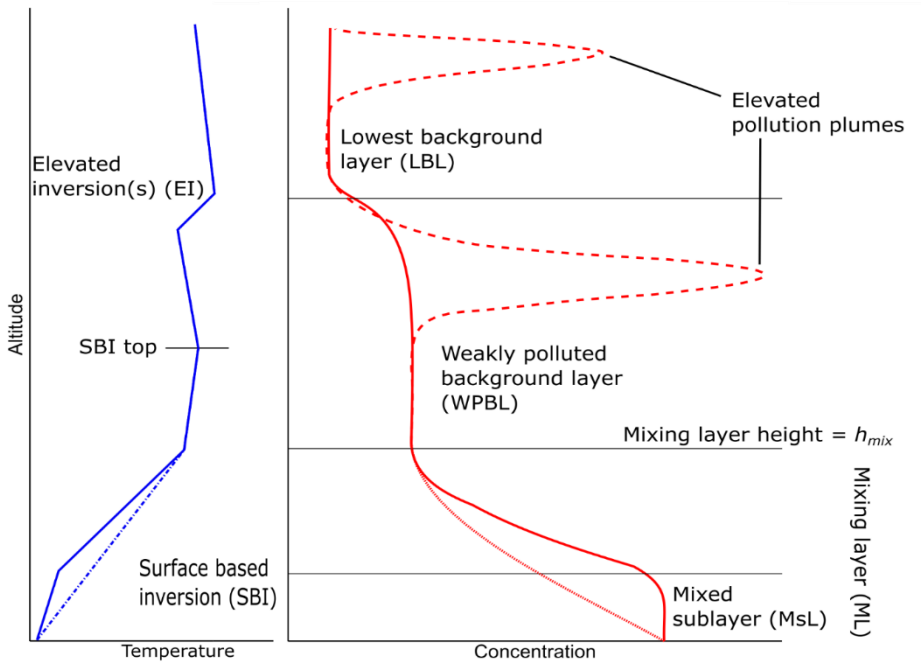

Figure 5 Schematic illustration of profiles observed in suburban Fairbanks during stable boundary layer conditions. The full red line illustrates the concentration profile of a generic pollution tracer. The various dashed lines show observed alternative profiles.