This study looked at how aerosols exchange near the surface in a cold, snowy Arctic city, Fairbanks, Alaska. The authors used EC during a 23-day period in winter 2022 to measure whether particles were rising into the air (emitted) or falling back to the ground (deposited). They studied different particle sizes, including ultrafine particles, mid-sized particles, and larger ones that are more likely to settle.

They found that the smallest particles were mostly emitted, especially during the day, likely due to traffic and other local sources. They also found that the bigger particles behaved differently. They were often depositing to the ground, especially when the weather was calm and cold (AC). But these upward and downward movements didn't always match up with the particle concentration in the air, showing that just looking at concentration isn't enough to understand pollution. Importantly, they discovered that synoptic conditions played a major role. When the wind was weak and the air was still, pollution built up near the ground. But stronger winds and temperature differences between the air and the snowy ground helped push particles back down, even if there weren't many in the air.

A major finding was that downward movement (deposition) didn't just happen because of gravity. Instead, turbulence in the air was a key driver. In extremely cold conditions, sharp temperature differences near the snowy surface may have created an extra push for particles to settle, a process known as thermophoresis. This may be a missing piece in current models, potentially leading to an underestimation of dry deposition rates.

Overall, the manuscript is well structured and scientifically sound. I have some comments that I believe, once addressed, will further strengthen the clarity and impact of the study and bring it to a publishable standard.

Major comment:

The manuscript discusses the likely influence of local sources (e.g., traffic, UAF activity) and regional sources (e.g., downtown Fairbanks) based primarily on the diurnal patterns. While these observations are suggestive, they remain qualitative. To strengthen the attribution of aerosol sources under different synoptic regimes, I suggest incorporating back trajectory analyses (e.g., using HYSPLIT or similar tools). Stratifying trajectories by synoptic condition and/or flux event (e.g., high UFP emissions or deposition episodes) would provide more robust evidence for the origin of aerosols and help distinguish between local vs. advected pollution events. This would also complement the existing discussion of transport under anticyclonic vs cyclonic regimes. If high-resolution meteorological data to run the back trajectory analysis is not available to resolve local-scale features, extending the footprint-based approach separately under different synoptic conditions would still provide a valuable, site-relevant alternative. A brief discussion of typical source characteristics by particle size (e.g., UFP, ACC, and Q-CRS) would also be helpful to support this analysis.

Throughout the manuscript, inversion layers and stability regimes and vertical profile of atmosphere are discussed as key factors influencing aerosol fluxes. However, the meteorological context is not sufficiently demonstrated. I suggest including synoptic-scale information, such as MSLP composites, and vertical profiles (e.g., radiosonde soundings), to better illustrate the evolution and structure of inversion layers during the campaign periods for different dominant synoptic conditions.

Minor comments:

The manuscript does not discuss whether EC has previously been used in snow-covered or Arctic urban environments, nor does it acknowledge any potential limitations of using this technique under such conditions. If this study is among the first to apply EC to aerosol deposition in Arctic snow-covered urban areas, it would be helpful to state this clearly and reflect on associated uncertainties.

Figure 1 shows the spatial distribution of the flux footprint, but it lacks clarification of what the coloured contours represent.

In Section 2.1, you mention several nearby potential aerosol sources, including the Tanana River. To support later discussions on aerosol sources under different synoptic conditions, I suggest highlighting these key features (e.g., the river, airport, major roads) directly to Figure 1. This would provide helpful spatial context when interpreting the footprint and probably back trajectories influences on observed fluxes.

Please clarify whether a constant z = 11 m (instrument height) was used for the calculation of the atmospheric stability parameter $\zeta = z/L$. Also, briefly describe how the Obukhov length L was derived.

Regarding the data processing and all filtration processes described in lines 171 to 202, clarify on how representative the final dataset is. Especially whether there is sufficient coverage across stability regimes, times of day, or meteorological conditions. Also in line 171, you mention that data were discarded for being “outside the absolute limits.” Could you clarify what specific thresholds were used in this step?

Please clarify on the average error of 101% in line 193.

The final sentence on page 16 states that particle fluxes during the cyclonic period are "on average null for the whole day" (referring to Fig. 6). However, this statement does not appear to be consistent with panel (a) of Fig. 6 - for UFPs. Please clarify or revise this statement for accuracy.

In the conclusion of Section 4.2, it has been stated that the UAF Farm site is devoid of local sources and that aerosol concentrations are mainly influenced by transport. However, earlier in the manuscript (e.g., Section 2.1), the potential influence of local sources such as the nearby airport has been discussed. Please clarify what is meant by "local" in this context and reconcile these two statements to avoid confusion.

In lines 306–314, the elevated night time accumulation-mode particle fluxes are attributed to re-entrainment under stable, calm conditions. While this is a plausible mechanism, the role of varying emissions, such as increased night time residential heating or episodic sources is not discussed. How did you ignore considering whether higher night time emissions may also contribute to the observed fluxes, particularly since these could be source-driven rather than solely due to surface-layer turbulence.

Since the mean values are available in the boxplots in Figure 3 & 5 (I assume the dots are the mean values), Table 1 may no longer be necessary (maybe keeping it in the supplementary materials). Presenting this information visually would improve clarity and reduce redundancy.

In Figure 6, the diurnal variability in emission fluxes is interpreted primarily as a result of traffic emissions, based on the timing of observed peaks. While this is plausible, I recommend also considering the role of deposition (i.e., net negative fluxes) and turbulence strength (e.g., vertical wind speed or shear) in shaping the diurnal pattern. Do they show similar consistency? Including these factors would provide a more complete picture of the boundary-layer processes controlling flux variability, and help to better separate source-driven vs. transport/mixing influences.

In Section 3.4 and Table 2, the transfer velocity v=F/N is only presented as a mean value. For consistency with Figures 3 and 5, and to better capture variability and uncertainty, why not presenting them as a boxplot grouped by stability class and particle size. This would allow readers to more directly compare patterns in v with those in N and F, and assess whether the observed means are representative of the distribution.

Why haven't you had included time series plots of V_n, V_d, and friction velocity (u*)? This would help the reader assess how these variables evolve and interact dynamically, especially under changing meteorological conditions.

Where is equation 3 mentioned in line 429?

In Section 3.6, the discussion of meteorological and deposition differences across regimes (e.g., ACa, ACb, C, T1, T2) is presented primarily in qualitative terms (e.g., "median u* was higher...", "Vd values were greater…"). I suggest making this section more quantitatively grounded by explicitly referencing values from Tables 4 and 5 within the text to support these comparisons. Additionally, to assess whether the observed differences are statistically meaningful, I recommend performing basic statistical tests on key variables such as deposition velocity or ΔT. This would help substantiate the conclusions about the influence of meteorological regimes on aerosol deposition and strengthen the interpretation of the results.

The observation that median deposition velocities for ACC particles are lower than for UFPs is counterintuitive given their larger size. This might reflect differences in turbulence coupling, rebound effects, or diffusional deposition mechanisms, but it would be helpful if the authors could provide a brief discussion or hypothesis to interpret this behavior.

While the surface-based eddy covariance measurements provide valuable insight into particle emission and deposition processes, the study would benefit from acknowledging the limitation posed by the lack of aerosol flux measurements at the top of the boundary layer. Without data on vertical flux divergence, it is difficult to fully constrain the aerosol budget or to distinguish surface-driven processes from entrainment or dilution effects. A brief discussion of this limitation, and its implications for interpreting the observed surface fluxes, would improve the overall clarity and transparency of the analysis.