Review of Karasewicz et al. (EGUsphere-2026-5029): Vertical aerosol flux in urban ecosystem: how far can we push Doppler lidar aerosol observations?

Overall assessment

The paper presents a valuable three-year, height-resolved Doppler lidar flux record from an urban ACTRIS site (Warsaw, Poland), with sensible screening using co-located instruments and an openly released dataset. The descriptive results (strong daytime upward transport in MAM and JJA, weak and mixed fluxes at night and in winter) are plausible and consistent with Figs. 8-12. The main issue is not the analysis but the way it is presented: several statements go further than the figures show, and some wording does not match the method. I recommend minor-to-moderate revision, mostly clarification and more careful wording; a few optional additions are suggested below.

Main findings

1. **The title is not supported by the text.** "How far can we push Doppler lidar aerosol observations?" promises a study of method limits, which the paper does not provide. Suggested: retitle to describe what is shown, for example, "Seasonal and diurnal characteristics of vertical aerosol flux velocities from three years of Doppler lidar observations in Warsaw". The Introduction would then only need two or three sentences naming the gap: lidar-based flux studies are few (Engelmann et al., 2008, a case study at Leipzig; Abril-Gago et al., 2025, dryland case studies), and a multi-year, height-resolved record is missing. The "first" claim is safer worded as "first multi-year" and should acknowledge the Leipzig case study. The urban setting is clear from the site and needs no further justification.
2. **"Resuspension" and "sedimentation" are acceptable as a sign convention, but should be defined as such.** The authors define the terms as a sign convention (l. 120, 124-127) and explain why they avoid emission/deposition, which is reasonable; the risk is that readers may interpret them as physical processes. For unit-density spheres, Stokes settling with the Cunningham slip correction gives about 0.0035 cm/s for a 1 µm particle and about 0.3 cm/s for 10 µm (my calculation with unit density, $\mu = 1.81\times10^{-5}$ Pa s and a mean free path of about 0.066 µm; Seinfeld and Pandis, 2006, Chapter 9, give the same relation and note that particles larger than 10 µm settle faster than 10 m/h, i.e. 0.3 cm/s; see also Mailler et al., 2023); a settling speed of 1 cm/s needs a diameter of about 18 µm. Fluxes above ±1 cm/s therefore cannot be gravitational settling of the fine particles that dominate the backscatter. One sentence stating that the terms denote net upward or downward turbulent transport (downward flux aloft being likely entrainment) would remove the ambiguity.
3. **"Flux occurrence" (Fig. 6) is the frequency of valid lidar signal.** It reflects aerosol load, cloud and fog attenuation and sensitivity, especially in DJF. This should be stated, and the inference that ABL growth "changes more rapidly" between DJF and MAM (l. 247-248) should be softened accordingly.
4. **Humidity filter.** Please define ΔRH (time and height difference) and acknowledge the radiometer humidity uncertainty: figures quoted by the UK AMOF facility for a HATPRO G5 (the

paper uses a G2) are a vertical resolution of 200 m below 2 km and about 5% RMS in RH (AMOF), so ΔRH thresholds of 1-2% are probably below the retrieval accuracy and should be presented as indicative only. The R^2 test (l. 169-175) compares class proportions, which are dominated by the central classes, so it is high almost by construction. Comparing the filtered and retained distributions directly (quantiles, or a Kolmogorov-Smirnov statistic) would be more informative, and the quantities compared should be defined. Also, "fully justified" (l. 165) should be softened, given that the authors themselves report poorer agreement in winter. The passage on normalisation (l. 121-123) is hard to follow; it could say plainly that dividing by the mean backscatter removes calibration and overlap effects, so that vf equals the mass transport velocity if backscatter is proportional to mass, and that deviations from this (e.g. hygroscopic growth) are what the RH filtering addresses.

5. **Stationarity.** The decision not to filter is reasonable, but l. 178 needs rewording. It says non-stationary cases are "mostly infrequent", while the authors' own test finds only 30.98% of intervals stationary (l. 199). Fig. 4 in fact supports their choice: the two distributions have similar centres (medians 0.08 and 0.00 cm/s), and the non-stationary one is narrower (75th percentile 0.35 against 1.06 cm/s). This is consistent with the test being a relative one, so that intervals with weak fluxes fail it more often; filtering them out would then remove weak fluxes preferentially and could bias the statistics towards strong ones. Suggested changes: (1) correct l. 178 to say that most intervals fail the test; (2) add the explanation above as a supporting argument for not filtering, which the authors could confirm by showing the share of stationary intervals against flux magnitude; (3) present the 60% tolerance (l. 184) as a deliberate choice and give its reason in one line: the original test (Foken and Wichura, 1996) accepts deviations up to 30% (as quoted by Vitale et al., 2020), but because that statistic is disproportionately sensitive to small fluxes (Vitale et al., 2020), a more tolerant limit is understandable.
6. **Uncertainty.** The ≈3% uncertainty (l. 207-211) refers to the full-dataset mean. Subsets, such as DJF at upper levels, have far fewer samples, and nighttime direction shares of 51-59% (Fig. 11) are close to 50%. Two inexpensive additions would help: (i) a null test in which w is shifted in time by more than the turbulence time scale and the covariances are recomputed, which gives the noise-only distribution of vf and shows how much of the ±1 cm/s class limit, and of the nighttime and winter flux, is signal; (ii) day-block bootstrap (or binomial) confidence intervals for the direction shares, since 30-min samples within a day are correlated. A noise correction of the lidar statistics (e.g. the autocovariance method of Lenschow et al., 2000, developed for noisy lidar data) could also be mentioned as not applied.
7. **"Within the ABL" versus 105-1995 m.** The algorithm is applied to all heights regardless of ABL height (l. 129-130), while the results are described as ABL results (l. 235). One year of lidar data at Leipzig, a continental central European site, gave seasonal mean daytime maximum ABL tops of about 800 m in winter, 1400 m in spring, 1800 m in summer and 1200 m in autumn (Baars et al., 2008), so upper winter levels in Warsaw are likely above the ABL. Please state this and word the DJF results accordingly.
8. **Statements to qualify or remove.**
    - "Strongly asymmetric" seasonal cycle (Abstract): based on four seasonal bins, and l. 246 also notes a notable JJA-SON change.

- "Winter intensity is low": the index uses the occurrence score, so it partly reflects signal availability; Figs. 8 and 10 show large and frequent DJF fluxes. The authors themselves note that DJF values may be chaotic (l. 371).
- Intensity peaks in MAM while ABL height peaks in JJA (l. 430-431): no ABL-height data are shown. Either add the radiometer ABL height or delete the sentence.
- Improved accuracy of aerosol optical property assessments (Abstract, l. 436): not demonstrated; better placed in the outlook.
- "Most intense in spring": at the lowest level MAM and JJA are equal in Fig. 12; the difference is aloft.
- L 125 reference needed (Statement begins with “In a literature…”)

Suggested additions

- **ABL height overlaid on the results.** The radiometer products already include boundary-layer height (l. 95-96). Overlaying it on Fig. 8 or Fig. 9, or adding a seasonal panel, would support or replace the unsupported statement at l. 430-431, show which upper bins lie above the ABL in winter, and link the daytime upward transport to ABL growth.
- **One robustness figure or table, preferably in a supplement.** Repeat the headline result (the median profile, or the Fig. 11/12 classes) for all data, RH-filtered data and the stationary subset already computed, and add the number of samples per bin to Figs. 11 and 12. If the versions agree, the claims that RH filtering and non-stationarity do not distort the statistics become demonstrated, and readers learn which DJF bins to trust. If the authors prefer to keep the paper short, the sample sizes alone are the most useful part.

Text and figure inconsistencies to correct

- Table 1 versus l. 159-160: with ΔRH defined as the maximum allowed change (l. 147), the rise from 1% to 5% with height makes the filter more permissive, not "more restrictive", and the ">" signs in Table 1 should be explained (are data above these values removed, and are the two conditions combined with "and" or "or"?); and in a well-mixed ABL (nearly constant potential temperature and water-vapour mixing ratio) RH increases with height, because temperature falls with height while the mixing ratio stays constant (for a Raman-lidar example in neutral layers, see Laly et al., 2025, who find RH to vary mainly with the temperature gradient); the statement about a typical decrease of RH with altitude therefore needs qualifying.
- The top altitude interval is given as 1555-1995 m (text, Table 1, and the legends and axis labels of Figs. 6 and 9-12); all other interval limits fall on the same 30 m grid (105, 555, 585, ..., 1515, 1995). Why not use 1545 m that would fit this resolution?
- Table 2: please recheck the sublayer intervals; they do not join up (for example in JJA the WML, 12:00-17:00, overlaps the TLss, 16:00-20:00, while in MAM, SON and DJF some hours belong to no layer).
- Fig. 12 (TLsr) shows JJA turning to sedimentation at levels 3-4, but l. 391-392 says only DJF and SON do. The WML bars at levels 1-2 exceed the 30 axis limit.
- "Dominance" is used for direction shares above 67% (l. 319) but also for SON daytime sedimentation at levels 2-4, where Fig. 11 bars are about 51-54%.

- Fig. 6 shows aerosol occurrence decreasing with height in all seasons, so l. 244-246 should say "decreases less" in MAM and JJA.
- Section numbering skips from 4.3 to 6; Chinchella et al. is cited as 2020 and listed as 2025; IPCC is cited as 2023 and 2021 but only 2021 is listed; "30 mn" should be "30 min" throughout.

Minor editorial corrections

The remaining minor items can be handled at copy-editing.

Line	As written	Suggested
9	"presents analysis of"	"presents an analysis of"
12	"applied it based on the aerosol flux parameters"	"defined it from the aerosol flux parameters"
23	"a priori knowledge for aerosol vertical transport"	"prior knowledge of aerosol vertical transport"
30-31	"clouds microphysics"; "climate changes"	"cloud microphysics"; "climate change"
41-44	"Nevertheless, ... by a vertical air movement ... sedimentation ... lift aerosol particles"	"To travel far, aerosols must first be lifted by vertical air motion"; sedimentation does not lift particles
125	"In a literature"; "an air particle exchange"	"In the literature"; "particle exchange between air and surface"
131-133	"If any 30mn time interval within was flagged ... classified as so"	"If any part of a 30 min interval was flagged as precipitation, the whole interval, up to the cloud base, was classified as precipitation"
239-241	"are still included, but in further data analysis, since we are interested only to quantify"	"are included here but excluded from the later analysis, since we only quantify the presence of fluxes"
244-245	"The occurrence ... of aerosol fluxes occur"	"Aerosols, and consequently aerosol fluxes, are frequent"
354	"low and higher latitudes"	"low and high altitudes"
368-370	"..., where the slow resuspension only in MAM and DJF"	"where slow resuspension occurs, in MAM and DJF only"
408-411	"...have been used" (subject does not agree)	"...were used"
426	"associated during rapid ABL decay"	"associated with the rapid decay of the ABL"

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