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Title: Role of Low-Level Jet Evolution in Vertical Aerosol Redistribution: A Doppler Wind Lidar Study Over East China

Authors: Tianle Bai, Yuanyi Lin, Mengya Wang, Tianwen Wei, Fangzhi Wei, Kuancheng Lv, Haiyun Xia

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Dear Editors and Reviewers,

On behalf of all co-authors, we sincerely thank the editors and reviewers for their careful evaluation of our manuscript and for providing constructive comments and suggestions. We have carefully revised the manuscript in response to all comments and have addressed each point in the following point-by-point response.

All changes in the revised manuscript have been highlighted using track changes. The revised manuscript and the detailed responses to the reviewers' comments are submitted for your consideration.

We appreciate the time and effort devoted to improving our manuscript and look forward to your further consideration.

Sincerely yours,

Mengya Wang
School of Atmospheric Physics,
Nanjing University of Information Science & Technology
219 Ningliu rd. Nanjing, Jiangsu, CHINA, 210044.

Response to Reviewer #1

We thank the reviewer for the careful reading of our manuscript and for the constructive comments. The suggestions have helped us improve the description of the methodology, moderate the interpretation of the observational evidence, and clarify the limitations of the case-based analysis. Our responses to each comment are provided below.

Major comments:

1. Lines 15–20, 194–195, 220, 275–280, and 386–395: the timing of the March 2022 PM_{10} peak appears inconsistent. The peak is reported as occurring at 16:00 LT on 15 March in several places, whereas the event description seems to suggest that the intended time may be 16:00 LT on 14 March. The authors should check the original observations and ensure that the date and

time are corrected consistently across the abstract, results, discussion, figures, and captions.

Response: We appreciate the reviewer's careful check of the event timing. We reexamined the original hourly PM_{10} observations together with the corresponding lidar and meteorological measurements. The maximum PM_{10} concentration during the March 2022 event occurred at 16:00 LT on 14 March 2022, rather than on 15 March 2022. The occurrence of "15 March" in the manuscript was a typo error. We have corrected this date throughout the manuscript, including the Abstract, Results, Discussion, Conclusions, and figure captions. The March 2022 dust episode remains defined as 09:00 LT on 14 March to 01:00 LT on 15 March 2022. We also rechecked the April 2021 event and confirmed that the PM_{10} maximum occurred at 08:00 LT on 17 April 2021.

2. Lines 120–126: Please provide more justification for the LLJ identification procedure. The criteria are adopted from previous work, but the manuscript should briefly explain their suitability for the present dust-transport cases, especially the choice of the lowest jet core when multiple wind-speed maxima occur.

Response: We appreciate this suggestion. We have expanded the description of the LLJ identification procedure in Section 2.3. Following Wei et al. (2025b), an LLJ event is identified when the maximum wind speed in the lower troposphere exceeds 8 m s^{-1} and the wind speed decreases by at least 4 m s^{-1} between the jet core and the first local minimum above the core. When multiple qualifying wind maxima occur, the lowest jet core is selected as the operational LLJ reference. This choice provides a physically relevant reference for examining the possible interaction between LLJ evolution and near-surface aerosol variations, which is particularly important for dust-transport cases. We note that this selection does not imply that elevated jet structures are unimportant; higher-level wind maxima are still considered when interpreting the complete vertical wind evolution.

3. Lines 237–241: The fixed ALT threshold of 10-6 m-1 sr-1, and the 0–5 km centroid range should be briefly explained, since both are used to interpret the relationship between aerosol layers and LLJ evolution. Authors should justify the definitions of backscatter centroid height and aerosol layer top.

Response: We thank the reviewer for this important comment. We have expanded Section 2.2 to provide additional details on the calculation and interpretation of these parameters. The backscatter centroid height is calculated as the backscatter-weighted mean altitude within the 0–5 km range, as described in the revised Section 2.2. It represents the vertically integrated position of aerosol loading within the lidar sampling range and is used as a diagnostic quantity for comparing aerosol-layer evolution with LLJ-core height. It does not represent the trajectory of an individual aerosol air mass.

The 0–5 km range was selected because the major aerosol layers and LLJ structures during both events occurred within this altitude range. Applying the same altitude range to both cases also ensures consistency in the comparison. The ALT is determined using a fixed attenuated backscatter threshold of 1.0×10^{-6} m^{-1} sr^{-1} ($\log_{10}\beta = -6.0$). We have clarified that this threshold is an operational criterion used to identify enhanced aerosol structures rather than an absolute physical boundary or instrumental detection limit. The ALT is therefore interpreted as a relative indicator of aerosol-layer vertical extent.

4. In Lines 315–320 and 328–335, the reported spectral-width values appear to refer to velocity spectral width and should therefore be given in m s^{-1}. The physical interpretation also needs refinement. Spectral width can indicate Doppler spectral broadening and radial-velocity variability, but it should not be described as a direct turbulence measurement without additional supporting analysis. A more cautious interpretation would improve the physical accuracy of this section.

Response: We appreciate the reviewer for identifying this issue. The variable presented in Figs. 9 and 10 is Doppler velocity spectral width, and its correct unit is m s^{-1} rather than s^{-1}. We have corrected the unit labels in the figures, captions, and related descriptions in Section 4.3. We also agree that spectral width is not a direct measurement of turbulence intensity. Spectral broadening can be influenced by turbulence, wind shear, and other sources of Doppler velocity variability. Therefore, we have revised the relevant descriptions to avoid directly equating spectral width with turbulence intensity. The revised manuscript interprets enhanced spectral width together with wind shear as evidence of increased dynamical variability and possible enhanced turbulent mixing, rather than as a direct turbulence measurement.

5. Lines 399–413 and 438–465: Please moderate the generality of the proposed “dynamic injection” and “stratified transport” framework. Since only two events are analyzed, these should be presented as case-based representative pathways rather than a general classification.

Response: We appreciate this comment and agree that two case studies cannot establish universal transport regimes. We have therefore revised the terminology throughout the manuscript. The descriptions of “dynamic injection” and “stratified transport” have been moderated throughout the manuscript and are now interpreted as case-based aerosol evolution pathways observed during the April 2021 and March 2022 events, rather than universal or exhaustive classifications of LLJ–aerosol interactions. We have added statements in the Discussion and Conclusions emphasizing that the occurrence and general applicability of these pathways require validation using additional events, multi-site observations, and further modeling studies.

6. In Lines 205–208, 220–228, and 386–397, ERA5 temperature and wind fields are used to describe inversion evolution. These data are useful for characterizing the background synoptic and thermal structure, but they should not be treated as precise observations of shallow

inversion height or inversion breakdown. The wording should be revised to acknowledge the resolution limitations of ERA5 and to avoid overinterpreting shallow boundary-layer features.

Response: We appreciate this important point. We agree that ERA5 does not have sufficient spatial and vertical resolution to precisely resolve the structure, thickness, or exact evolution time of shallow boundary-layer inversions.

We have revised Section 2.4 and the related discussions to clarify that ERA5 is used to characterize the background synoptic circulation and the general lower-tropospheric thermal structure. Temperature variations between ERA5 pressure levels are interpreted only as qualitative indications of atmospheric stability, rather than direct observations of inversion boundaries. Statements implying that ERA5 precisely resolves shallow inversion formation or breakdown have been revised accordingly.

7. The clockwise rotation In lines 365–373 and 419–424 shown in the hodographs is interpreted as evidence of inertial oscillation. This explanation is plausible, but the evidence presented remains largely qualitative. The authors should acknowledge that other processes, such as frontal forcing and synoptic-scale wind turning, may also contribute to the observed rotation. This point would be stronger if framed as a possible explanation rather than a definitive mechanism.

Response: We appreciate this comment. We agree that the hodograph rotation alone cannot demonstrate that inertial oscillation is the only mechanism responsible for the observed LLJ evolution. We have revised the relevant discussion to describe the clockwise wind-vector rotation as being "qualitatively consistent with a possible inertial-oscillation contribution." We also acknowledge that frontal forcing, synoptic-scale wind turning, and interactions between these processes and boundary-layer decoupling may contribute to the observed wind evolution. Statements attributing LLJ persistence exclusively to inertial oscillation have been removed or moderated.

Response to Reviewer #2

General comments

The manuscript presents an interesting Doppler wind lidar study of low-level jet (LLJ) evolution and aerosol vertical structure during two dust episodes over Hefei. By combining coherent Doppler wind lidar wind profiles, aerosol backscatter, surface PM observations, surface meteorology, and ERA5 reanalysis, the study provides useful observational evidence for boundary-layer transport processes during dust events. The topic fits the scope of Atmospheric Measurement Techniques, as it demonstrates the capability of Doppler wind lidar to resolve coupled wind and aerosol structures at high temporal resolution.

Overall, the manuscript is suitable for consideration after major revision. The following comments are intended to help improve the clarity, robustness, and presentation of the analysis.

Response: We appreciate the reviewer's constructive suggestions. In the revised manuscript, we have improved the description of the LLJ identification method, clarified the interpretation of lidar-derived parameters, moderated the generality of the proposed transport pathways, and expanded the discussion of limitations associated with the two-event case analysis.

Major comments

1. Please add a compact stage-based comparison table for the April 2021 and March 2022 cases. The table could summarize the key timing of each stage and indicate how each stage corresponds to LLJ evolution, aerosol-layer response, WSZ/SWZ changes, inversion evolution, and PM10 variation. This would help readers follow how the proposed mechanisms are inferred from the observations and compare the two events more directly.

Response: We appreciate this suggestion. We have added a new summary table (Table 1) at the end of the Results section. The table compares the main stages of the April 2021 and March 2022 events, including the pre-event, dust-impact/peak, and post-peak stages. The table summarizes the corresponding LLJ evolution, aerosol-layer response, wind shear and spectral-width variations, ERA5-derived thermal conditions, and surface PM_{10} evolution. The table is intended to provide a concise synthesis of observations already described in the text and figures, rather than introducing additional interpretation.

2. The manuscript uses both BLJ and SLLJ in the case analyses. Please briefly clarify the basis for this classification, or cite the previous study or definition from which the BLJ/SLLJ distinction is adopted. This would help readers understand how the different jet regimes are identified and interpreted.

Response: We appreciate this suggestion. We have clarified the LLJ classification procedure in Section 2.3. Following Du et al. (2014) and Wei et al. (2025b), LLJs are classified according to their jet-core height. Jets with cores below 1 km above ground level are classified as boundary-layer jets (BLJs), whereas jets with cores between 1 and 4 km are classified as synoptic-system-related LLJs (SLLJs). The jet-core height is used as the operational classification criterion, while the synoptic environment is considered when interpreting the physical evolution of each event. Because LLJ cores may migrate vertically during their lifecycle, the classification is applied according to the jet height during each analyzed stage rather than assigning a single fixed category to an entire event.

3. Please state the specific 30-min averaging period represented by each panel in Fig. 8, rather than only labeling the profiles as before, during, and after the PM_{10} peak. Please also clarify whether the numerical changes discussed in the text are calculated from β or from $\log_{10}(\beta)$. If

these values refer to changes over selected height ranges rather than a formally calculated vertical gradient, please replace "gradient" with more accurate wording, such as "change over the height range".

Response: We appreciate this comment. We have added the averaging periods used for Fig. 8 in the figure caption and Section 4.2. For the April 2021 event, the selected periods are 13:00–13:30 LT on 16 April, 07:45–08:15 LT on 17 April, and 00:30–01:00 LT on 18 April. For the March 2022 event, the corresponding periods are 02:00–02:30 LT, 15:45–16:15 LT on 14 March, and 07:30–08:00 LT on 15 March. We have also clarified that the quality-controlled attenuated backscatter coefficients were converted to $\log_{10}(\beta)$, interpolated onto a common vertical grid, and averaged at each height. Therefore, the profiles shown in Fig. 8 represent the mean logarithmic backscatter structure rather than averages of linear backscatter values. In addition, we replaced the term "gradient" with "change over the selected height range" where appropriate, because the comparisons describe differences between selected altitude intervals rather than formal vertical derivatives.

4. Please revise the relevant descriptions in Section 4.2 to better distinguish aerosol advection from local vertical redistribution, especially when discussing ALT/centroid changes and the inferred LLJ-related downward transport. Since the lidar observes a fixed column, changes in backscatter structures, ALT, and centroid height may reflect the horizontal advection of different dust-laden air masses as well as local vertical redistribution. Please avoid over-attributing these changes to local downward transport unless supported by additional evidence.

Response: We appreciate this important comment. We agree that observations from a single vertically pointing lidar represent temporal changes within a fixed atmospheric column and cannot uniquely separate horizontal advection from local vertical redistribution. We have revised the interpretation of aerosol-layer evolution throughout the manuscript. The revised text focuses on the observed temporal relationship among LLJ-core evolution, wind shear, spectral width, aerosol structure, and surface PM10 variation. When downward redistribution is discussed, it is described as a possible contribution supported by the combined observations rather than as a directly demonstrated transport pathway. We have also added this limitation to the Discussion and Conclusions and noted that multi-site lidar observations, trajectory analysis, and numerical simulations are required to further quantify the relative contributions of horizontal transport and vertical mixing.

5. Please make the limitations in the Discussion and Conclusions more explicit. The manuscript briefly mentions future multi-site lidar work, but the current interpretation relies on one lidar site and two dust cases. Please add a short limitation statement noting that the proposed LLJ–aerosol coupling mechanisms are inferred from single-column observations and should be further tested with multi-site lidar observations, trajectory analysis, or model simulations.

Response: We appreciate this suggestion. We have added a clearer statement in the Discussion and Conclusions regarding the limitations of the present study. The proposed LLJ–aerosol coupling pathways are derived from two dust events observed at one lidar site. Therefore, they should be interpreted as case-based process interpretations rather than statistically established mechanisms. The revised manuscript now emphasizes that additional observations from multiple sites and a larger event sample are needed to evaluate the frequency and broader applicability of these pathways under different synoptic conditions.

Minor comments

1. Please state clearly that LT = UTC + 8 h in the manuscript.

Response: We have clarified in Section 2 that all times are reported in local time (LT, UTC+8) unless otherwise stated. Synoptic weather charts explicitly labeled as UTC remain in UTC.

2. Please describe the PM_{10} peak marker consistently across figures, including its style, color, and label.

Response: We have standardized the PM_{10} peak marker in all relevant figures. A dark-red triangle without a black border is now used consistently, and its meaning is described in the corresponding captions.

3. Please use either "post-event stage" or "post-case stage" consistently. The April case currently uses both styles.

Response: We have revised the terminology throughout the manuscript. "Post-event stage" is used for the overall evolution following the dust event, whereas "post-peak period" is used only for the profile comparisons relative to the PM_{10} peak shown in Fig. 8.

4. In Section 4.2, the March 2022 profile description contains an incomplete phrase: "characterized by multi-layered vertical transitions and structural reconfigurations across." Please revise the sentence.

Response: We have corrected the sentence as follows: "The March 2022 event shows more pronounced layered variations in the profiles across the three selected periods, particularly below approximately 1.5 km."

5. Please check the consistency of figure-reference formatting, e.g., use "Fig. 3" rather than "Fig.3".

Response: We have checked all figure references and standardized the formatting throughout the manuscript. Spaces after "Fig." and "Figs." have been corrected, and figure references now follow a consistent format, such as "Fig. 3", "Fig. 3a", and "Figs. 3a–b".

6. In Appendix Fig. A2, "The evolution of temperature inversion layer" could be revised to

“Evolution of the temperature inversion layer”.

Response: We have revised the Fig. A2 caption to avoid implying that ERA5 directly resolves shallow inversion layers. The revised caption now reads: “Evolution of the ERA5-derived lower-tropospheric thermal structure during the dust events.”

7. In Section 4.4, “the presence of inertial oscillation signal” could be revised to “the presence of an inertial oscillation signal”.

Response: We have revised the related statement in response to this comment and Reviewer 1’s Major Comment 7. The original interpretation has been moderated, and the clockwise wind-vector rotation is now described as: “The observed clockwise wind-vector rotation is qualitatively consistent with a possible inertial-oscillation contribution.” We also note that frontal forcing, synoptic-scale wind turning, and boundary-layer decoupling may contribute to the observed wind evolution.