--Referee 1---

The paper describes a careful experimental study of the suspension of tyre-wear particles from a glass substrate. The experiments were carried out in a small wind tunnel and the results used to investigate critical flow conditions for suspension as a function of particle size and shape. The paper is clearly written, though reading it is spoilt by the excessive use of abbreviations and acronyms, even the humble car gets reduced to PC. Most of this can be avoided by using plain language, pronouns, etc. I must admit that I found this annoying and distracting.

The work was carried out in a small wind tunnel, working section 170x54x27 (it might be 27x54) cm, at free stream speeds from 1 to 9.5 ms^{-1}. The floor of the wind tunnel was covered with roughness elements, except for the glass slide on which the particles to be studied were deposited. Tyre-wear particles come in a variety of shapes and sizes and are, generally, non-spherical, and a key question is whether suspension from a smooth, glass surface provides any guidance for suspension from, say, road surfaces, where surface irregularities will play an important role – as will the action of vehicles moving over the road. Might not the latter be the dominant factor? If that is true, then what is the practical value of this work? These matters deserve some discussion, so that the results can be placed in context.

The boundary layer in the wind tunnel was probably quite shallow, perhaps about 20 cm – it would be helpful to provide the values in the paper along with shear stress profiles. There is no discussion of the mismatch in flow scales (compared with full scale) and its likely consequences, nor of particle inertia and Reynolds number effects. The friction velocity and the probability distribution of the wall shear stress are probably similar to those at full scale, but the associated time and length scales are not and that could be important. Saltation is not discussed – it probably plays no role in the experiments, but I don't know. What might occur if the glass substrate were longer? Some discussion of these issues is essential.

A question: The term 'detachment' is used. I would use suspension – is there a reason for using detachment rather than suspension? How do you separate sliding from detachment?

> *We sincerely thank the reviewer for their thorough evaluation and constructive feedback, which greatly helped improve the quality of our manuscript. The present study was intentionally designed as a simplified framework to isolate the fundamental mechanisms governing particle detachment under controlled aerodynamic forcing as a first step.*
>
> *We acknowledged that real road surfaces are influenced by environmental drivers, including surface roughness variability and wetness, different particle compositions, vehicle-induced disturbances, and other environmental conditions, which may mask and outweigh the underlying physics investigated*

here. Since we start by isolating the fundamental aerodynamic transport mechanisms from these other environmental constraints, the latter were beyond the scope of this work, but some have been investigated by us and are the focus of forthcoming manuscripts. We now briefly discuss these aspects in the introduction to place our findings in the context of our current understanding.

This simplified configuration provides a necessary baseline for quantifying TWP detachment thresholds and examining the role of particle size and morphology under controlled conditions. Such an approach is consistent with established wind-tunnel studies used to develop and validate threshold models, including Iversen and White (1985) and Shao-type formulations, and enables direct comparison with previous experimental work, including Esders et al. (2023), which uses a simplified experimental framework. In this context, the present study extends these frameworks to irregular tyre-wear particles, which are of increasing environmental relevance.

The boundary-layer characteristics were clarified in the revised manuscript. Based on the measured velocity profiles, the internal boundary-layer depth is indeed shallow, around 7-8 cm, and depends on the height of the roughness element, the upstream flow structure, and the fetch length. Our choice of a small depth of the IBL was intentional to reduce the effects of the wind tunnel top. In addition, wind and shear-stress profiles were incorporated to characterise the vertical structure of the flow.

We also clarified that the present experiments were not intended as a strict, dynamically and geometrically scaled replica of full-scale road atmospheric flow. Rather, they followed a common laboratory approach used in resuspension studies, in which idealised and small-scale wind tunnel configurations were employed to isolate the effects of near-wall aerodynamic forcing, surface properties, moisture and particle properties (Esder et.al., 2023; Dong et al., 2007; Kim et al., 2016; Del Bello et. al., 2018). In this study, particle detachment was quantified over a small subset of the wall-mounted glass substrate (75 mm by 25 mm). Accordingly, the most relevant aerodynamic quantities were the local wall shear stress and near-wall shear fluctuations acting directly on the particles (Rasmussen et al., 2018), which can be locally self-sustaining and are not solely determined by the outer flow (Jimenez & Pinelli, 1999).

To further characterise the representative turbulence scale in the wind tunnel, we estimated the longitudinal integral length using the ESDU 74031 relation, as commonly adapted in boundary wind tunnel studies (Hui et.al., 2008; Kozmar, 2011; Trush, 2020). Based on the spectral peak of the longitudinal velocity fluctuations at the highest velocity stage, the longitudinal integral length scale was approximately 3.6 cm, with a corresponding characteristic time scale of 0.056 s. This estimated integral length scale was smaller than the internal boundary layer *thickness, indicating that the dominant energetic motions relevant to detachment were contained within this layer.*

Based on the estimated near-surface wind speed in the experiment (approximately 0.45 to 4 m/s), the particle Reynolds number was approximately 2.4 to 78.4 for the analysed particle sizes. This placed the particles outside the creeping flow Stokes drag regime, so purely viscous Stokes drag no longer applied, and the Reynolds number-dependent drag and lift corrections became relevant (Shao, 2000; Loth, 2012). In addition, the Stokes number was estimated as 7.1–100, indicating that particle inertia was not negligible. Therefore, especially for larger particles, the response to rapid flow was not expected to be instantaneous, and stronger and more sustained near-wall forcing may have been required to overcome adhesion and gravity. This was also consistent with the higher threshold velocities observed for the larger particle class in our findings.

Regarding particle motion, we recognised that detachment may occur through multiple mechanisms and modes, including saltation and its subsequent transport processes. However, the present work was designed specifically only to identify the onset of particle mobilisation for particles in the size range 80–300 µm, rather than to resolve the subsequent entrainment processes. For this reason, we use the term "detachment" rather than "suspension", since the latter may imply sustained transport, which is more commonly associated with smaller particles. We here clarify that 'bombardment', i.e., the momentum transfer of saltating particles onto other resting and moving particles, which is common in saltation layers, was not explicitly investigated. However, due to the low particle loading density and small substrate area, we expected the particle–particle interactions to be negligible.

Other comments on a page-by-page basis follow.

Line 56. What is meant by 'collision'?

Thank you for your comments. In this context, "collision" refers specifically to microsphere–microsphere interactions, where a particle rolling collides with another particle resting on the surface. As reported by Esders et al. (2023), such collisions can either facilitate or hinder detachment. To avoid ambiguity, we revised the sentence to state this explicitly.

Lines 73-75. Perhaps: "However, for particles with non-spherical geometries, such as those from tyre wear, it is important to evaluate theoretical models of the critical friction by using experimental results for different particle sizes and shapes."

Thank you for the suggested wordings. It was added.

Line 81. "… the deposited particles often settle in clusters …"

Indeed, we revised as suggested.

Line 101. "… water, and were able to …"

Indeed, we revised as suggested.

Line 112. Please make clear what is meant by: 'instance segmentation'?

The term "instance segmentation" was clearly defined, and the corresponding line was adjusted accordingly.

Line 116. Why is bold used here?

Indeed, the bold formatting was removed.

Line 132. Suggested wording: "Conventional image processing techniques have been widely adapted to detect particle images and analyse detachment under turbulent flow conditions in the wind tunnel." Do you mean in general, or in your laboratory?

Here, we intended this as a general statement and revised it accordingly.

Section 2.1. The wind tunnel features a 54 cm × 27 cm cross-section and is 730 cm long. This is confusing as the cross-section dimensions are for the working section, which we learn later to be 170 cm long. It would be useful to add here the dimensions of the substrate and its location in the working section. At present, I have to wait for line 395 to learn: "The detachment experiments were conducted on standard laboratory glass slides (Thermo Fisher Scientific), measuring 75 mm × 25 mm."

The wind tunnel flow exhausts to the atmosphere. How is the make-up flow provided that ultimately provides the inflow to the tunnel?

Use of a three-wire hot-wire probe requires a yaw and pitch calibration, either assumed or obtained from local calibrations. Assuming a wire responds to the flow component normal to it is generally considered inadequate. This is not discussed.

Thank you for the comment. The description of the wind tunnel geometry was revised to clearly distinguish between the working-section cross-section (54 cm × 27 cm), the total tunnel length (730 cm), and the test section (300 cm). The upstream fetch length for the roughness elements (170 cm) is now also specified.

The dimensions (75 mm × 25 mm) and the location of the glass substrate within the test section were added to Section 2.1 for clarity.

A description of the airflow path was included to clarify how ambient air is drawn into the tunnel and exhausted to the atmosphere.

Regarding the CTA measurements, the tri-axial probe (55P095) was calibrated using the Dantec probe calibrator, with temperature correction applied during calibration. Directional sensitivity (yaw and pitch) was accounted for using manufacturer-provided calibration coefficients, while a pitot tube served as the *velocity reference during calibration, and the probe was manually aligned with the mean streamwise flow direction. We clarified this procedure in the manuscript.*

Line 156 "… second interval, lit by …"

We rephrased the sentence for grammatical correctness.

Figure 3. "Scanning electron microscopy image of passenger car tire wear particles."

Indeed, we revised as suggested.

Line 213. "In particular, it adopts a CSP-style that replaces the C3 block with the C2f module and uses a spatial pyramid pooling fast (SPPF) block for efficient multi-scale feature aggregation." Am I supposed to understand this sentence? There are other, similar examples.

Thank you for this important comment. We agreed that the original wording was too technical for the main text and may not have been accessible to a broader aerosol audience. In response, we revised the description of the YOLOv8 architecture in simpler, more intuitive language and reviewed the manuscript throughout for similar instances of overly specialised computer-science terminology. Where appropriate, methodological specific details were shortened in the main text, and technical details were moved to the Appendix.

Line 230. "Manual creation of polygons around microparticles often exceeds 50 particles mm^{-2} and is both time-consuming and sometimes inaccurate." Do you mean that the process of manual creation of polygons around microparticles is both time-consuming and sometimes inaccurate because the density of particles often exceeds 50 mm^{-2}?

Indeed, we agreed that the original sentence was unclear. Our intention was to state that, when many particles are present on the substrate (e.g. at high particle number density), manual polygon annotation becomes difficult, time-consuming, and more prone to error, which motivates the use of a semi-automated workflow. We have revised the sentence accordingly.

Line 243 "Finally, the fine-tuned model was for a new set of data collected individually from the three seeding approaches." I don't understand this sentence.

The sentence has been revised accordingly for clarity.

Table 2. Some entries should have units. Have all the entries been defined? Are they all necessary?

We revised the table to specify units where applicable (e.g. image size in px and rotation in degrees), clarified the meaning of the augmentation-related parameters, and moved the table to the Appendix to improve the flow and readability of the main text.

Line 274. One example of the unnecessary use of an abbreviation: “The DSC is defined as:” – “It is defined as:” There are many others that could be resolved in a similar manner.

Thank you for this important comment. We agreed that the manuscript relied too heavily on abbreviations and acronyms, and we reduced and revised it accordingly to improve readability.

Line 277. “By computing the DSC for each individual TWP, we were able to obtain a detailed evaluation of the model’s ability to accurately delineate particle boundaries, quantifying the degree at which individual particles of different size and shape detach, and very efficient and sensitive to resolve TWPs capture on different surfaces.” Please rewrite and clarify the final clause, beginning 'and very’.

We rephrased the sentence for grammatical clarity.

Equs. 5 and 6. These are properties of 2-dimensional shapes. ‘A’ is the plan surface area of the particle. Similarly, (6) applies to the surface area in plan. It is not a measure that demonstrates a spherical shape (lines 302, 305).

Thank you for this valuable comment. We revised the text to clarify that A represents the projected (plan-view) particle area and that circularity refers to the 2D particle outline.

Line 306. Suggested wording: “This made the metric instrumental in determining how elongated or irregular the particles are, and how they exhibit different detachment behaviour due to variations in contact area and adhesion forces.”

Thank you for the suggestion. We revised the sentence accordingly.

Fig. 5. The lines joining the points have no meaning.

We acknowledge this comment and decided to replace the figure with a grouped bar chart to present the comparison more clearly.

Fig 6. Dice or dice?

Thank you for pointing this out. We have revised the figure.

Fig. 7. Which is which, density or relative frequency?

The caption was revised to explicitly state that the bars represent relative frequency (left y-axis) and the smooth curves represent density (right y-axis).

Line 354. I don’t see the need of the references: Hancock, line 354; Foken et al., line 361; Zhou et al., 375; Fernholtz etc., lines 375-376. The first is about simulation of stable boundary layers, the second the well-established the log-law, the third and fourth effectively the same.

We streamlined the references in this section and removed citations that were either not directly relevant or redundant in this context.

Equ. 7. How was 'd ' determined?

We clarified that d denotes the zero-plane displacement height. d was approximated as 2/3h, where h is the roughness element height (10mm), giving d = 0.007m. A supporting reference was added too (Folken, 2008).

Line 378. Measured at what height?

The height at which the eddy-covariance (EC) flux was evaluated was specified in the manuscript.

Line 385. How was u* adjusted? 2% uncertainty is remarkably low for hot-wire measurements.

We revised and clarified the calibration procedure for u. The friction velocity was obtained from the least-squares linear regression using u* values independently estimated from the eddy covariance and log law methods and referenced to the free stream velocities, rather than the near-surface velocities used previously. The term "adjusted" was misleading, as the 2% uncertainty did not represent the uncertainty of the hot wire measurement. This statement was removed. The fitting uncertainty was reported through the linear regression statistics, with ±4% relative uncertainty in the fitted regression coefficients based on the 95% confidence interval and a regression root mean square error of 0.01 m/s^2*

Fig. 8. Suggested caption: "Figure 8. Calibration of u∗ from the profile measurement and the eddy-covariance as a function of the surface wind speed." Add the value of z. As it stands, the axis has a label of the velocity profile, u(z).

Thank you, the caption was revised accordingly.

Line 431. Would linear regression be preferable to ordinary least squares?

We revised the word accordingly.

Equ. 9. Define the symbol, N*.

We defined N(u*) in the equation.*

Equ. 10. 'a' is an offset, implying suspension at zero u*. That's odd.

We agreed that the intercept 'a ' could be misinterpreted, and now we clarified that the linear regression was used only as an empirical approximation over the measured u range, and "a" was not assigned as a physical interpretation of u*=0.*

Line 436. Please define more fully: A (the maximum detachment fraction) and b (the rate of detachment change).

We now fully defined the parameters in the revised manuscript. "A " was inadvertently included in the previous version, and now omitted as the detachment fraction was normalised, so the maximum response was 1. Hence, this separate A parameter was not required for the 2-parameter logistic function.

Line 442. C has no units.

Indeed, the typographical error was corrected.

Equ. 11. Is 'd' the diameter of a sphere?

D denotes the equivalent particle diameter, the wording was corrected.

Fig.9. No side forces are shown, likewise no saltation effects. Can there be direct lift-off? L acts near the leading edge and has along lever arm to the trailing edge, greater than that of the weight. Not obvious that a sphere is more likely to lift off, all else equal.

Thank you for this comment. We acknowledged that direct lift-off is possible in principle. However, for irregularly shaped tyre wear particles, the detachment mode depends largely on particle shape, orientation, contact geometry, and the balance between the aerodynamic and stabilising forces. Therefore, detachment may occur by lift-off, rolling, or sliding, depending on the specific particle–surface configuration, geometry and shear intensity. Given their irregular morphology, tire wear particles may be more prone to sliding than spherical particles. Supporting evidence comes from the simulation of irregular micro-particles (Olivares et. al., 2024), which found sliding to dominate for irregular particles, whereas spheres predominantly rolled. Figure 9 is intended as a simplified two-dimensional conceptual schematic of the principal forces considered in the present force-balance framework and is not intended to represent all possible entrainment mechanisms. Saltation and other three-dimensional effects were not considered in the present experimental framework and are therefore beyond the scope of this study. We clarified this point in the revised caption. We hence chose the terminology of 'detachment' as it can be universally applied to all transportation modes.

Section 3.1. Do you need to discuss sliding as well as lifting?

We indeed acknowledged that particle detachment can occur through different motion modes, including rolling, sliding, and lift-off, and we clarified this in the revised manuscript, citing relevant literature. However, the present optical tracking approach was designed to identify detachment events rather than distinguish among these individual motion modes. Since resolving the specific entrainment motion was beyond the scope of this study, we did not expand on this discussion to maintain clarity and focus on detachment.

Fig 10b. What do the numbers in the boxes mean?

Thank you for this comment. The numbers shown in Fig. 10b represent the model detection confidence scores for the identified particles. For example, "TWP 0.95" indicates the class label "tire wear particle" and the associated confidence score of 0.95. A confidence threshold of 0.1 was applied, so detections below this value were not displayed or classified as TWPs. This was clarified in the revised caption of Fig. 10b.

Lines 483-500. There's a great deal of the obvious and well known here that could well be moved to introduction.

Thanks for pointing out this nice comment. The purpose of this paragraph was to relate the observed detachment behaviour to findings that have been reported in the literature and thereby provide context for the variability seen across replicates. In the revised manuscript, we reduced this paragraph to focus more directly on the interpretation of the present results, while reducing the more general background statement.

Line 486. What is meant by 'acceleration'?

We agreed that the term "acceleration" was unclear here. We revised the sentence to "velocity increment".

Line 489. How were the speed changes managed?

The wind speed changes were implemented through discrete manual adjustments of the transformer–fan system controlling the tunnel airflow. We clarified and added it to the manuscript. However, a detailed description of the resulting time–speed profile, including the transient adjustment between stages, was provided in response to the comment on Fig. D1.

Fig 11. What is meant by 'normalised' in the caption?

Here, "normalised" means that the detached fraction in each replicate was scaled by the maximum detached fraction observed in that replicate before ensemble averaging and fitting. This was clarified in the revised caption.

Line 523. "For particles larger than 75 μm, gravity term dominates over cohesive forces and the uth∗ increases with size." Provide a reference, or is this your inference?

Indeed, the statement was intended as an inference from the force-balance semi-empirical model of Shao and Lu (2000). In the revised manuscript, we clarified this interpretation and explicitly added the reference.

Fig. 12. There are no error bars in (a) and (b), just two points in (c). Should the results in (c) be plotted against the mean diameter for each group?

Thank you for this comment. Error bars were included in panels (a) and (b) as ±1 SD of the ensemble-binned data, but they were not clearly visible. We revised *these figures, including Figs. 13a–c, to improve the visibility of the ±1 SD error bars. In addition, panel (c) was intended to compare the mean threshold friction velocity between the two predefined size classes rather than to represent a continuous size dependence. We therefore retained the size-bin categories on the x-axis and clarified this accordingly in the revised caption.*

Line 534. "Consequently, the drag force (Fd) and lift force (Fl) can more easily pivot a more spherical particle off the surface," – not obvious really, see comments above on Fig. 9.

Thank you for this comment. We revised the manuscript to describe the effect of particle shape more cautiously in terms of possible differences in contact footprint, adhesion, and aerodynamic moment balance, rather than implying that more spherical particles necessarily pivot off the surface more easily.

Fig. 13. Note u* scales change. Plot (d) against mean C for each group?

Fig. 12 and 13 were revised to use a consistent u axis range, which improves direct visual comparison across the size and shape classes. In addition, Fig. 13d was adapted in the same way as Fig. 12c to allow comparison between the predefined circularity classes.*

Fig. 14. Tyre-wear results for particles of equivalent diameter around 20 micro-m would be very useful.

*This valuable suggestion is much appreciated. We agreed that TWP data in the smaller size range, approximately 10–30 µm in equivalent diameter, would be insightful, particularly because the Shao and Lu framework indicates an increasing importance of cohesive effects as particle size decreases in this regime. Such data would help assess whether TWPs exhibit a comparable increase in u*th at the lower end of the size spectrum. However, we simply do not have TWP in this size range for the present study as their size is determined by the conditions and material in the test stand producing the abrasion. We now acknowledged this limitation explicitly in the revised discussion.*

Conclusions, Line 586-589. This is true. Note though that De and C are actually measures of the top or plan view of the particles, not of the whole, three-dimensional shape.

We indeed appreciated this reiteration and now amended this point accordingly.

App. A1 is about Yolo. Is it needed? Please add brief explanations within A2, A3 and B1.

Yes, it is needed as it gives the reader an overview of what the model looks like, which aligns with what we already discussed in the body of the manuscript. Since we trimmed some of the technical details into the appendix, it should now flow *better. All other sections in the Appendix were worked on based on your suggestion.*

Fig. C1. Move to main text. Plot using dimensionless speed, U/Uref and (ln((z-d)/H), where H is the boundary layer depth, for the whole boundary layer and add the associated dimensionless shear stress profiles.

We moved Fig. C1 to the main text as suggested. However, dimensionless scaling of the velocity profiles using U/Uref and z/H was not applied, as particle detachment from an idealised surface is governed by local near-wall shear stress rather than outer-layer similarity scaling. In our configuration, the flow developed as an internal boundary layer over the measurement surface rather than a fully developed global boundary layer. Although its depth can be estimated approximately from the profiles, a single outer layer Height (H) was not used here as the primary scaling parameter. Instead, we used shear stress profiles and spectral analysis (presented in the Appendix) to characterise the flow conditions and their associated time and length scales relevant to the study.

Fig. D1. How was acceleration from one u* to the next managed? What was the time-speed profile?

As described earlier in the manuscript, the wind forcing was applied in a stepwise fashion through discrete adjustments of the transformer–fan system controlling the wind tunnel airflow. To make this clearer and as requested, we have now included the corresponding 50-minute time series of friction velocity in the Appendix, covering all 10 stages (5 minutes per stage). The time series shows that the flow does not change instantaneously between stages, but instead adjusts over a short transient period following each step change before approaching a quasi-steady level.

---Referee 2---

This manuscript presents an experimental modelling study investigating the detachment mechanisms of tire wear particles (TWPs). The authors utilized a laboratory test stand to generate pristine passenger car and truck tire particles, to avoid the confounding factors of road wear, environmental aging, and road dust. These particles were dispersed onto a glass substrate using a new method developed in this study. Using an established model, the study successfully identifies the detachment of these particles from a glass substrate within a wind tunnel. By investigating physical parameters such as size and morphology, the authors have established a framework for determining the critical velocities required for particle removal. This work represents a significant step toward characterizing the fundamental mechanisms controlling TWP transport.

While the topic is highly relevant to Aerosol Research and provides valuable insights into detailed detachment parameters, the manuscript is written in dense technical language that may be inaccessible to a broader audience. I recommend the manuscript for publication after addressing the following comments.

We greatly appreciate the reviewer's time, the constructive feedback and their encouraging overall assessment of our manuscript. We carefully revised the manuscript where necessary to address all the comments in our best capacity, including reducing dense technical language, simplifying specialised computer science terminology, and relocating less relevant technical implementation details to the Appendix. We hope that these changes have substantially improved the revised manuscript. Our responses to the individual comments are also provided below:

Specific Comments:

1. As this study focuses exclusively on pristine lab-generated TWPs, what specific insights should future studies apply when moving into natural environments (real-world, aged tires)? Additionally, could the authors further emphasize the practical benefit of achieving this higher level of detachment accuracy for environmental modeling?

Thank you for raising these important points, which we agreed that the application to the real-world environmental particles should be stated more elaborately. We now revised and expanded the conclusion section to clarify the broader significance of this study for environmentally relevant particles and the value of improved detachment quantification for informing future work on particle motion tracking and transport.

2. Regarding the resolution selection in Section 2.6, did the authors consider more robust methods for determining the optimum resolution beyond the sensitivity analysis in Figure 5? For instance, identifying the "elbow point" where increases in resolution no longer yield significant gains in mAP relative to the increase in inference time.

    Thank you for this comment. We did not perform a formal elbow-point analysis. The resolution was selected based on comparative performance across the tested image sizes, together with practical considerations of computational cost and inference speed. To better reflect the scope of the comparison, we revised the caption accordingly.

3. Section 2.7 (Line 348) mentions benchmarking against commercial equipment and refers to Figure B1. However, Figure B1 shows the size distribution of car TWPs without clearly identifying the reference data. Were these distributions measured by commercial equipment? Please elaborate on the specific benchmarking procedure and the equipment used.

    Thank you for this comment. We clarified the statement in the main text and expanded the Appendix to include necessary information relevant to this study, including the size and shape information extracted from the laboratory analysis.

4. Several sections use highly specialized computer science terminology that could be simplified for the Aerosol Research audience. For example, the description on Lines 213–214 regarding the model architecture could be made more intuitive, such as:

"The model has been upgraded with a CSP-style with a C2f module that retains more details without getting slower. It also uses spatial pyramid pooling fast (SPPF), a high-speed vision tool that lets it accurately identify objects of different sizes simultaneously."

I suggest that the authors review the manuscript and rewrite or elaborate on technical terms to ensure the work is accessible to aerosol scientists who may not be experts in Deep Learning.

Thank you for these valuable comments and suggestions. We agreed that several parts of the manuscript, particularly those describing the deep-learning model architecture, were written in language that was too specialised for the target audience. We rephrased these sections to improve clarity and simplified the technical details where necessary, focusing more on the role the model plays, applicability in the study, and its rationale for selection. In addition, all other relevant technical architectural details were moved to the appendix.

Technical Comments:

1. Acronyms: Please define all acronyms upon their first use in the text. Key examples to address include:

Line 15: PE

Line 96: DL

Line 158: CTA

Line 224: mAP.

Thank you for pointing this out. We acknowledged that some acronyms were not defined explicitly at first use. In response, we revised the manuscript throughout to ensure that all acronyms are defined at their first use. We also reduced the overall use of abbreviations by retaining only those that are necessary for technical clarity and are commonly used in the field, while replacing others with their full terms or with simpler wording where appropriate.

1. Line 365: The text says "see appendix." It is unclear if this refers to the appendix of this manuscript or the referenced paper (Thomas et al., 2009). If it is the latter, please include a brief description of the automated software tool in this manuscript's own appendix for clarity.

We clarified the reference to indicate that it refers to Thomas et al. (2009), in which the software description is provided in the paper's Appendix. However, a brief description of the tool was added in the body of the manuscript for clarity.

2. Line 378-381: In Figure C1 (Lines 378–381), it is unclear which one is the derived friction velocity (u_*) and the eddy covariance measurement ($u_{*,EC}$). Please use distinct colors or markers to clarify.

We clarified the Figure C1 caption to indicate that it presents only the logarithmic profile fit used to derive the friction velocity (u) and the roughness length (zo). As only one dataset is shown, additional markers were not required.*

3. Line 454: Specify the exact type and material of the "microspheres" used by Shao and Lu (2000) to provide a better baseline for the TWP comparison.

Thank you for this comment. We revised the text to specify the particle type and approximate size range used by Shao and Lu (2000).

4. Line 473: Lower case for “unique particle identifiers”.

Corrected to lower case

5. Line 554-555: For the adhesion analysis, provide the specific gamma values and citations for the natural dust and sand particles used as a comparison.

    Thank you for this comment. We revised the text to the specific γ range used for comparison and added the corresponding citation explicitly.