Dear Editor and Referees,

Thank you very much for providing us with the opportunity to submit a revised version of our manuscript titled “Spatiotemporal dynamics of shear stress partitioning around a single shrub in a sparsely vegetated dryland”. We appreciate the thoughtful comments of the reviewers and their erudite suggestions on how we might improve the manuscript. We have carefully considered each of their comments and have made the appropriate revisions. We are confident that we have addressed all the comments from the three anonymous referees and incorporated them into the revised version.

Sincerely,
Pei Zhang
On behalf of all authors

In this document, we report our responses to each reviewer and show the changes made. Suggestions are presented in black font, our responses are in *italics and blue font*, and our changes and edits are in red font and shown also with tracked changes in the manuscript.

Reviewer 1:

This manuscript addresses the spatiotemporal dynamics of surface shear stress partitioning around individual shrubs in drylands. They use high-quality field measurements with dense Irwin sensors across two years and three phenological phases, providing direct observations of shear velocity that are rare in the literature. The data are well calibrated, the analyses are statistically sound, and the main findings are robust. The primary finding is that the surface shear stress ratio R' is independent of wind speed but shows a significant decreasing trend with foliar growth—challenges conventional drag-partitioning theory and has important implications for process-based wind erosion modeling. While the study is limited to a single shrub, it provides a valuable empirical dataset and a clear conceptual advance.

To my knowledge, this is the first study to deploy a dense array of Irwin sensors to directly measure u_{*s} (not just wind speed) around a natural shrub over an extended period (~11 months).

The demonstration that is phenology-dependent but wind-speed-independent is a significant conceptual contribution. It means that in sparsely vegetated landscapes, the spatial pattern of erosion potential is governed more by vegetation structure than by instantaneous above-canopy wind forcing. This should prompt a re-evaluation of assumptions in many wind erosion models. I like this result.

The authors thank Reviewer 1 for the positive feedback and succinct summary of one of the paper's main findings. We appreciate Reviewer 1's interpretation so much that even though the last paragraph in the above comment is not a suggested edit, we have added it nearly verbatim to the end of the second paragraph in the Conclusions section.

The figures could be refined; they appear rather rough and unpolished overall.

Thank you for the comment. We agree that the figures could be further improved and polished. Reviewers 2 and 3 also made some minor comments about the figures, and we have refined them where appropriate in response to those comments as well. We have updated all the figures with high-resolution versions shown in the revised manuscript with track changes. High-resolution figure files will also be provided upon acceptance.

The analysis is restricted to wind speeds between 4 and 10 m/s at 1 m height. The conclusion that is independent of wind speed is robust within this range. Is it sound?

We thank the reviewer for this comment. We interpret the reviewer's concern as relating to the applicability of the observed wind speed independence outside the analyzed wind speed range. The observed independence of R' from wind speed is constrained to the quality-controlled wind speed range analyzed in this study (4-10 m s^{-1}). We agree that the present dataset does not allow us to evaluate whether the conclusion still holds under substantially stronger wind conditions. To clarify the analyzed wind speed range, we have added the following sentence to the Methods section.

Therefore, the analysis was restricted to quality-controlled observations with wind speeds between 4 and 10 m s^{-1}.

The experiment is based on only one individual shrub of a single species and only the dominant wind direction (225°±30°). The authors must clearly and explicitly state that their conclusions are site-specific and shrub-specific, and cannot be broadly generalized to other vegetation types, morphologies, densities, or wind directions. Overstatements about "shrubs in drylands". The study only focuses on one single shrub, not "vegetation" in general, so the title might be something like "Spatiotemporal dynamics of shear stress partitioning around a single shrub in a sparsely vegetated dryland" or "Shear stress partitioning around a single shrub: spatiotemporal responses to wind magnitude and phenology in a dryland"…

Thank you for the comments and insightful suggestions. The authors agree with the reviewer's concern regarding the limitations and potential overgeneralization of the conclusions. We have thoroughly revised the corresponding Discussion and Conclusion sections to explicitly clarify that the findings are specific to the investigated shrub type, morphology, site conditions, and dominant wind directions; the results therefore should not be broadly generalized without additional field observation evaluations. Please see the revised text in the track changes version of the manuscript.

We also appreciate the reviewer's suggestions regarding the manuscript title. Following the reviewer's recommendation, along with Riewer 3's suggestion, we have revised the title to:

"Spatiotemporal dynamics of shear stress partitioning around a single shrub in a sparsely vegetated dryland".

I did not see the quantitative measurements of shrub morphology (canopy width, height, porosity, branch density).

Thank you for the comment and for pointing out the missing shrub morphology information. We used a terrestrial laser scanner (model: RIEGL VZ-400i) to survey the shrub morphology on March 2nd, 2023. The investigated shrub was approximately 1 m tall, with canopy length (along-wind direction) and width (cross-wind direction) both approximately 2.5 m, derived from the collected point clouds. Regarding the reviewer's concern for porosity, the average porosity was estimated to be about 0.66 following the voxel-based framework of Nowak et al. (2022). Quantitative measurements of shrub morphology, including a 3D view of the shrub dimensions and estimated 2D porosity along-wind and cross-wind directions, have been added to the supplementary materials. For the reviewer's convenience, we have included the supplementary figure below for reference. We also would like to clarify that branch density is implicitly incorporated within the voxel-based porosity estimation framework.

We added the text below to the "Study site and data collection".

"A terrestrial laser scanner (model: RIEGL VZ-400i) was used to survey the shrub morphology on March 2nd, 2023. The investigated shrub was approximately 1 m tall, with canopy length (along-wind direction) and width (cross-wind direction) both approximately 2.5 m, derived from the collected point clouds

(Supplementary Fig. S1a). The average porosity was estimated to be about 0.66 following the voxel-based framework of Nowak et al. (2022) (Supplementary Fig. S1b,c)."

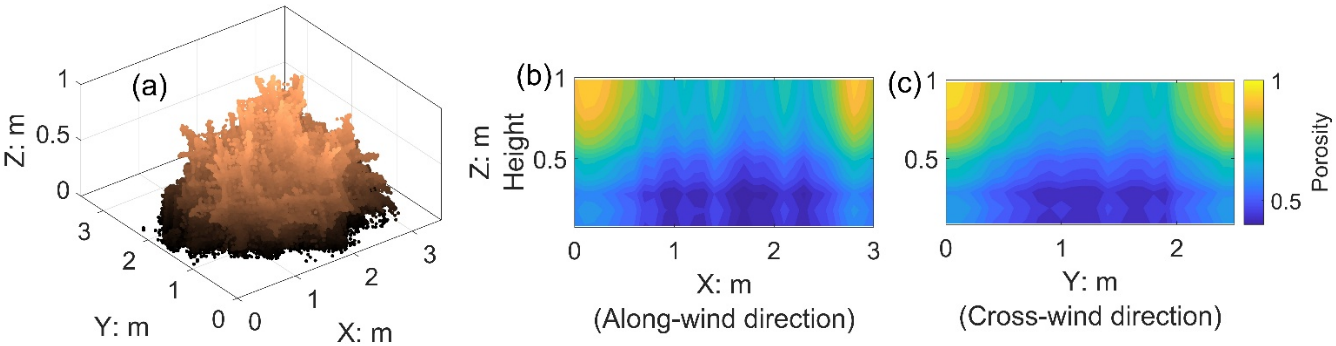


Figure S1. Morphological characteristics of the investigated shrub derived from terrestrial laser scanner surveys. (a) Shrub dimensions; (b) estimated 2D porosity along-wind direction; and (c) estimated 2D porosity cross-wind direction.

Figures 5 and 6 visually appear to show rather weak trends, and even if so-called "objective results" are derived from non-parametric statistical tests like the Mann-Kendall test, they may not truly explain anything substantial.

Thank you for the thoughtful comment. We agree that statistical significance alone doesn't necessarily explain strong physical relevance or processes importance, especially in a sparely vegetated dryland system characterized by substantial seasonal and natural variability. Our intention was not to suggest that the trends represent strong deterministic relationships, but rather to identify modest phenological tendencies in shear stress redistribution around the investigated shrub.

We agree that the visual trends in Figures 5 and 6 are relatively weak and have revised the corresponding text in the Abstract, Discussion, and Conclusions sections to moderate our interpretation. Specifically, we emphasize that the observed relationships represent weak but statistically detectable tendencies rather than dominant controls on shear stress variability. We also clarified that substantial spatial heterogeneity and turbulent variability around vegetation are expected to weaken simple monotonic relationships, even when systematic tendencies are present.

The conclusions section largely repeats findings already in the abstract.

Thank you for the suggestion. We agree the original conclusions section overlapped substantially with the abstract. We have extensively revised the Conclusions section to reduce unnecessary repetition and focus on broader implications of the findings, limitations, and relevance for improving drag partition and aeolian transport modeling efforts. Please see the track changes in the revised manuscript. For the reviewer's convenience, we have also highlighted the relevant edits below in red font.

"This study presents a detailed characterization of shear stress ratio (R') along the side of an isolated shrub in a sparsely vegetated rangeland, based on field data collected over 11 months including three phenological phases across two years. The greater variability of R' observed in this study suggests stronger spatial heterogeneity in shear stress redistribution around vegetation than previously recognized and, consequently, greater spatial variability in wind erosion and dust emission potential across sparsely vegetated drylands. The temporal variability of R' was also substantial, reaching 75 % of the magnitude of spatial variability, suggesting that localized wind erosion may occur intermittently before spatially averaged stress reaches a threshold value. Considering the intermittent and event-based nature of aeolian transport and wind erosion, temporal variability should be incorporated into erosion models to improve predictions of threshold exceedance and sediment transport occurrence.

Results also show that patterns in R' were independent of wind speed for the 4-10 m s^{-1} range observed in this study. On average, R' exhibited a modest decrease with foliar growth, yet the shear stress pattern remained highly heterogeneous, including localized areas of enhanced shear near shrub margins. Compared to previous studies examining erosivity patterns around individual plants, our results further suggest that vegetation type and dimensional traits may contribute to differences in the extent and morphology of erosivity extent. These findings indicate that in sparsely vegetated landscapes, the spatial pattern of erosion potential is governed more by vegetation structure and phenology than by instantaneous above-canopy wind forcing, which should prompt a re-evaluation of assumptions in many wind erosion models.

The findings presented here are specific to the investigated shrub type, morphology, site conditions, and dominant wind direction range. Additional field observation across vegetation types and flow regimes are needed to evaluate the broader generality of the observed shear stress redistribution patterns. The authors acknowledge that it is unrealistic to measure shear stress redistribution around all sparsely distributed vegetation types in the field. Nevertheless, further work could expand observations across dominant vegetation forms and morphological characteristics to improve the modeling of drag partition parameterization. For example, future studies could examine how lateral shear velocity responds to variations in vegetation shape and scaling across different plant types."

Th "first" are slightly overstated, appearing many times in the text.

Thank you for the comment. We agree with the overstatement. We have made the corresponding revisions throughout the manuscript.

The 4 m s^{-1} wind threshold and 5-minute averaging interval are acceptable, but the authors should briefly justify why this choice does not bias the estimation of shear velocity.

Thank you for the comments and suggestions. We have edited the Methods section accordingly in the track changes version of the manuscript. Specifically, the threshold value was selected to reduce potential underestimation of shear velocity associated with low-frequency spectral loss at a 1-meter measurement height, following van Boxel et al. (Fig. 3 2004). The filtering was intended to improve the reliability of shear velocity estimation rather than preferentially bias u_* to higher values. We also clarified that the analysis focuses on moderate and stronger wind conditions relevant to aeolian transport and wind erosion processes.

Reviewer 2:

The authors quantified the surface shear stress ratio, its spatial and temporal variability, and lateral flow acceleration along the side of a shrub across a range of wind magnitudes and phenological phases. Incorporating phenology-dependent, yet wind speed independent, shear stress redistribution and spatiotemporal variability into drag partition schemes provides a pathway toward improved process-based representation of vegetation-wind interactions in wind erosion models. The paper is well written. The results are well presented and discussed.

The authors thank the reviewer for the positive feedback.

There are minor comments as follows:

Comments:

Graphical abstract.

Shrub center has been indicated by in the legend but has been missed in the figure.

Good catch. Thank you for pointing out the missing marker indicating the shrub center. The figures have been updated to include the shrub center.

Line 197-201. This paragraph seems to be a part of methos section, and it is better to be moved to method section where figure 2 is placed.

Thank you for the comment and suggestion. We agreed that this paragraph is better suited to the Methods section. As the reviewer suggested, we moved the first sentence of the paragraph to the paragraph above Fig. 2, while the second sentence was moved to the paragraph describing the location bias filtering process. Please see the track changes in the revised manuscript. For the reviewer's convenience, we have also highlighted the two sentences in red font in their corresponding new locations below.

"A Gill WindMaster 3-D sonic anemometer with an acoustic path length of 0.15 m was installed ~3 m upwind of the shrub to continuously measure the wind vector at 1 m height above the soil surface (Fig. 1). ... The Green Chromatic Coordinate (G_{CC}) is a derived metric from PhenoCam imagery ($0 \le G_{CC} \le 1$) that represents the relative greenness of vegetation within the defined region of interest. Three phenological phases were identified based on the three-day average G_{CC} and field site photographs (Figs. 2 & 3): dormant (February 14–April 26, 2023; February 12–April 19, 2024), green-up (April 27–May 23, 2023; April 20–May 19, 2024), and leaf-on (May 24–July 7, 2023; May 20–June 20, 2024)."

"Estimates of shear stress ratio features for Eqs. 2–3 were filtered to reduce potential bias from samples being collected from different sets of locations. … Therefore, we filtered the data to require that the same sensor locations were functioning simultaneously: 8 sensors during 2023; 10 sensors in the dormant phase of 2024; and all 18 sensor locations for the remainder of 2024. The coordinates of these sensor locations were summarized in Table 1. After filtering, 706, 87, and 477 5-min averaged samples remained for the dormant, green-up, and leaf-on phases, respectively, in 2023, and 804, 182, and 443 5-min averaged samples remained for the corresponding phases in 2024 (Zhang et al., 2025). Each sample consists of simultaneous measurements from all functioning sensors for the corresponding analysis period. … ”

Line 203:

In “The Mann-Kendall trend test ($p < 0.05$) indicated a statistically significant decrease trend” the location of ($p < 0.05$) is not fine, rephrase as “The Mann-Kendall trend test indicated a statistically significant decrease trend ($p < 0.05$)”

Thank you for the comment and suggestion. We have revised the sentence as suggested. Please see the track changes in the revised manuscript.

In “showed a statistically significant decrease trend across phenological phases in both years (Fig. 6).” include P value after (significant decrease)

Thank you for the comment and suggestion. We have revised the sentence as suggested. Please see the track changes in the revised manuscript.

Line 210:

Include P value for “significant increase trends”

Thank you for the comment and suggestion. We have revised the sentence as suggested. Please see the track changes in the revised manuscript.

Line 266 and Line 275:

No statistically significant relationships were found between U and R' ($p < 0.05$, R2 >0.1).

No statistically significant relationship was found between R' and U for $p < 0.05$, R2>0.1

“$p < 0.05$” should be “$p > 0.05$”

I suggest including P values everywhere statistically significant trend, decrease or increase has been indicated.

Thank you for the correction and suggestion. Our intention was to emphasize that no relationships between U and R’ satisfied both criteria of $p < 0.05$ and $R^2 > 0.1$. We agree with the reviewer that statistical significance is determined by the p-value, and our original wording was misleading. We have revised the two sentences as follows:

No relationships between *U* and *R’* satisfied the criteria of $p<0.05$ and $R^2 >0.1$.

We also reviewed the entire manuscript to ensure that p-values are reported wherever statistically significant trends/relationships are found/discussed. Please see the track changes in the revised manuscript.

Figure 5:

This figure shows spatiotemporal trends of the shear stress ratio across phenological. The phrases are from Feb 14 to July 3 for 2023 and from Feb 19 to June 2 for 2024 which seems slightly inconsistent with Feb 14 to July 7 for 2023 and Feb 12 to June 20 for 2024 in Paragraph 200.

Thank you for the comment. The time periods from Feb 14 to July 7, 2023 and from Feb 12 to June 20, 2024 represent the overall monitoring periods, including all measurement collected before quality-control filtering. As described in the fourth paragraph of Section 2.3, samples were filtered to reduce potential sample location bias arising from external issues or user error during data collection. For example, several Irwin sensors stopped working at specific monitoring locations around the shrub. Depending on the affected sensor locations, this could result in overestimates of R′ (within a zone of enhanced wind forcing) or underestimates of R′ (within a zone of reduced wind forcing). We have revised the caption of Fig. 5 to avoid potential misunderstanding regarding the sampling period. Please see the tracked changes for details.

Line 283: " (Fig. 9a–b)" is probably "(Fig. 10a–b)"

Thank you for catching this oversight. We have corrected this oversight. Please see the track changes in the revised manuscript.

Reviewer 3:

The manuscript "Spatiotemporal dynamics of shear stress partitioning around vegetation in a sparsely vegetated dryland investigates" an interesting and relevant topic with the potential to provide valuable insights into the spatial and temporal variability of surface shear stress around a typical shrub vegetation. The dataset is unique and the field measurements are ambitious; however, several aspects of the manuscript require clarification and strengthening before the conclusions can be fully supported.

We thank Reviewer 3 for the positive summation of the manuscript. Many of Reviewer 3's concerns (both major and minor) have already been addressed during revisions in response to Reviewers 1 and 2. We have noted in our response where this is the case.

(1) In particular, the study would benefit from clearer articulation of the research objectives and hypotheses

The objectives of the paper were detailed in the second half of last paragraph of the introduction, i.e: "This study quantifies the spatiotemporal dynamics of u_{*s} and compares the response of drag partitioning (shear stress redistribution) to vegetation phenology and wind forcing along the side of a shrub-interspace mosaic in a sparsely vegetated dryland. We used up to 18 wireless Irwin sensors to measure u_{*s} around a Prosopis glandulosa Torr. (honey mesquite) shrub. Specifically, we analyzed whether drag partitioning is dependent on wind forcing or on vegetation phenology."

To add clarity, we changed "This study quantifies" to "The objective of this study is to quantify...".

(2) more detailed descriptions and justification of the methodology and calibration procedures, and additional discussion of key assumptions underlying the shear stress measurements and their interpretation.

Please see responses to comments: 1, 10, 11.

(3) There are also opportunities to improve the statistical treatment of the data, and reduce potential issues associated with pseudo-replication to overfit regressions.

Please see responses to comments: 22, 35.

(4) Finally, revisions to the figures, terminology, and overall organization would substantially improve readability and help readers better interpret and generalize the findings. The comments below are intended to help strengthen both the scientific rigor and the clarity of the manuscript.

Many of the figures have already been revised in response to Reviewers 1 and 2. See also responses to the specific comments below.

(5) Page 2 : Line 40 missing verb in sentence – suggest rephrasing : "speeds at"

Thank you. We have corrected it to "speeds are at or below"

(6) Page 2 : Line 42 ??well recognized?? by wind tunnel studies then or simulations or both? Maybe worth specifying

No revision made. The statement is directly followed by seven references, and the sentence ending with "around individual plants in natural environments" already provides the requested context.

(7) Page 3 : Line 71-72 Are any of these are omnidirectional horizontal wind speeds (cup anemometer or rotated 2D or 3D) versus wind vector measurements from LDA or PIV that could be ignoring spanwise flow?

Perhaps the line references are off. This comment does not appear to correspond to the referenced sentence, which is: "The amount of turbulent energy reaching the surface, i.e., u_{*s}, is the key driver of wind erosion and dust emission, but estimates of acceleration and sheltering based solely on height-dependent wind speed measurements can yield substantial differences in u_{*s}."

(8) Page 4 : Line 88 This is a ratio with the denominator fixed and so this should be specified as surface shear stress redistribution

Perhaps the references are off. We are unable to interpret this comment in the context of the referenced sentence, which is: "The objective of this study is to quantify the spatiotemporal dynamics of u_{*s} and compare the response of drag partitioning (shear stress redistribution) to vegetation phenology and wind forcing along the side of a shrub-interspace mosaic in a sparsely vegetated dryland." Is the reviewer referring to u_{*s} as a ratio? In any case, drag partitioning distributes shear stress over roughness elements, plants, rocks, other obstacles, and the surface so "drag partitioning (shear stress redistribution) seems *like a more appropriate appositive than "drag partitioning (surface shear stress redistribution). As such, no revision was made.*

(9) Page 4 : Line 90-95 Expecting a series of hypotheses here or maybe objectives/research questions but these are the results. Perhaps reword to better suit a research article model.

Please see our response to comment (1) above. The sentence containing the objective was rephrased for clarity.

(10) Page 4 : Line 103 What pressure transducer was used? Are there inter-sensors calibrations made to estimate their inter-sensor error?

Thank you for the comment. The details about the model transducer and basic circuitry are provided in Zeigler et al., 2026. Text edited to make this clear. "Wireless Irwin sensors were used to quantify u_{*s} *(Ziegler et al., 2026)." changed to "Wireless Irwin sensors were used to quantify* u_{*s} *(instrument design detailed in Ziegler et al., 2026)."*

(11) Page 4 : Line 106 Is a frequency of 1 Hz sufficient for resolving temporal variability when compared to lab/model experiments?

This is a good question, and we did not test it, but it is outside the scope of the current study. We used 5-minute averages of the 1 Hz samples to correspond to the u_* *averaging period.*

(12) Page 4 : Line 108 Horizontal error of 19 cm: do you mean +/- 19 cm? These seems a lot for a ~1 m tall shrub

The GPS unit has a mean error of 19 cm. The text was edited for clarity from "Their positions were recorded using a Trimble Geo XH 6000 unit, with a mean horizontal error of 19 cm. to "Their positions were recorded using a Trimble Geo XH 6000 unit, which has a mean horizontal error of 19 cm."

(13) Page 4 : Line 110 What is the variation in density of these? Scattered grasses and forbs along the fetch? over the measurement period? Is this something that could influence the total shear stress

There were only very few, isolated, low-height forbs or grasses. We considered them scattered and small enough to not directly influence the measurements. The text has been edited to make this clearer and a reference to Figure 3 (picture of the setup) was added. The sentence now reads: "The fetch length to the nearest upwind shrub in the dominant wind direction was ~15 m with negligible isolated grasses and forbs along the fetch (Figure 3)."

(14) Page 5 : Line 116 Pictures show four sonic anemometers installed quite close to each other that could cause interference. Assuming that the most upwind is the one used in this study?

Yes, the upwind anemometer was used in this study as the reference, as indicated on Figure 1. For clarity, we added the following note to the end of the Figure 3 caption:

"Note: The three sonic anemometers in the lateral transect in the foreground of panels (a) and (b) were not used in this study."

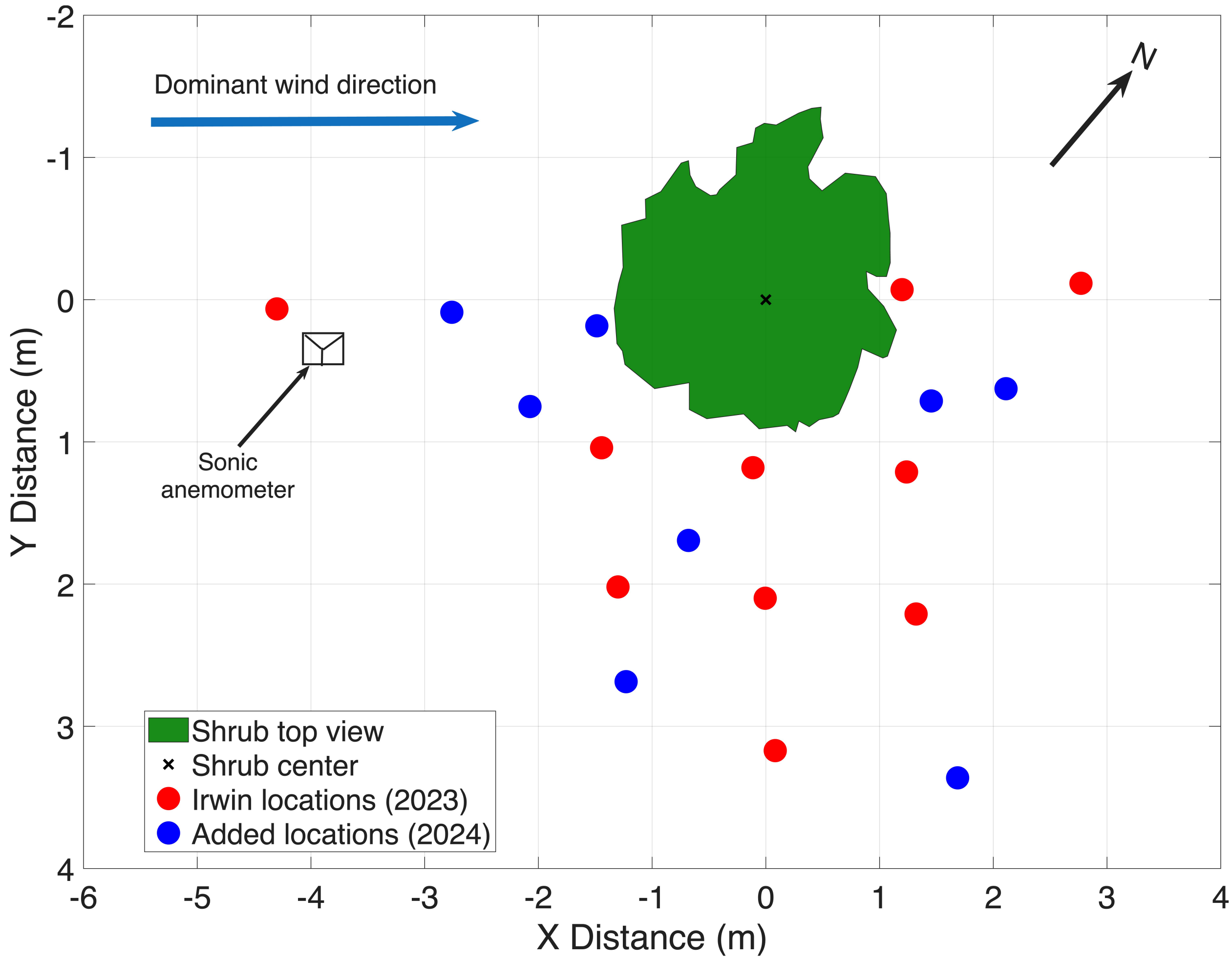
(15) Page 5 : Line 117 What is the path length and therefore maximize eddy size that these sensors can capture installed only 1 m above the surface?

Thank you for the comment. We are not entirely sure that we understand the reviewer's question. Measurement time is usually more associated with the largest eddies that will be recorded, and frequency and path length with the smallest eddies that can be recorded. We interpret this question as asking if the path length and sampling frequency of the sonic anemometer were sufficient to resolve the turbulent motions contributing to the quantification of shear stress. The Gill WindMaster sonic anemometer has a path length of 0.15 m and was sampled at 20 Hz. Based on the frequency-response analysis of van Boxel et al (2004, Fig. 4), a 20 Hz sampling frequency with a 0.15 m path length at a measurement height of 1 m is expected to limit high-frequency losses in quantifying shear stress to less than 5% for wind speed up to 10 m s^{-1}. As stated in the final paragraph of Section 2.2, wind speeds exceeding 10 m s^{-1} accounted for only about 2 % of the total samples in 2024. Therefore, high-frequency attenuation is expected to have a negligible influence on the results.

We have added the acoustic path length of the sonic anemometer to the Methods section 2.1. Please see the revision in the track changed version.

(16) Page 5 : Line Fig. 1 If a linear interpolation was targeted why are the sensors not installed on a standard grid?

We were limited by the number of sensors. The first set of sensors in 2023 were installed in transects perpendicular to the dominant wind direction. Additional sensor locations were added in 2024 to fill in the sampling area. We added different colors for the installation years in the Figure 1 and text to the figure caption that states the number of the original and added locations. However, we do not see why this matters for using linear interpolation to visualize the results.



(17) Page 5 : Line Fig. 1 What about the different years - are all these present for both years? Helpful to have some different colors or symbols to identify the different years

We agree this was a bit confusing in the original manuscript. See revision to previous comment.

(18) Page 5 : Line Fig. 1 Why wasn't the full shadow area targeted as much as the upwind area?

We located the sensors to address the objective of the study, which was to quantify the spatiotemporal dynamics of u_{*s} and compare the response of drag partitioning (shear stress redistribution) to vegetation phenology and wind forcing ***along the side*** *of a shrub-interspace mosaic in a sparsely vegetated dryland."*

(19) Page 7 : Line Fig. 4 Indicate which points are from what method

Thank you for the comment. Figure 4 has been updated.

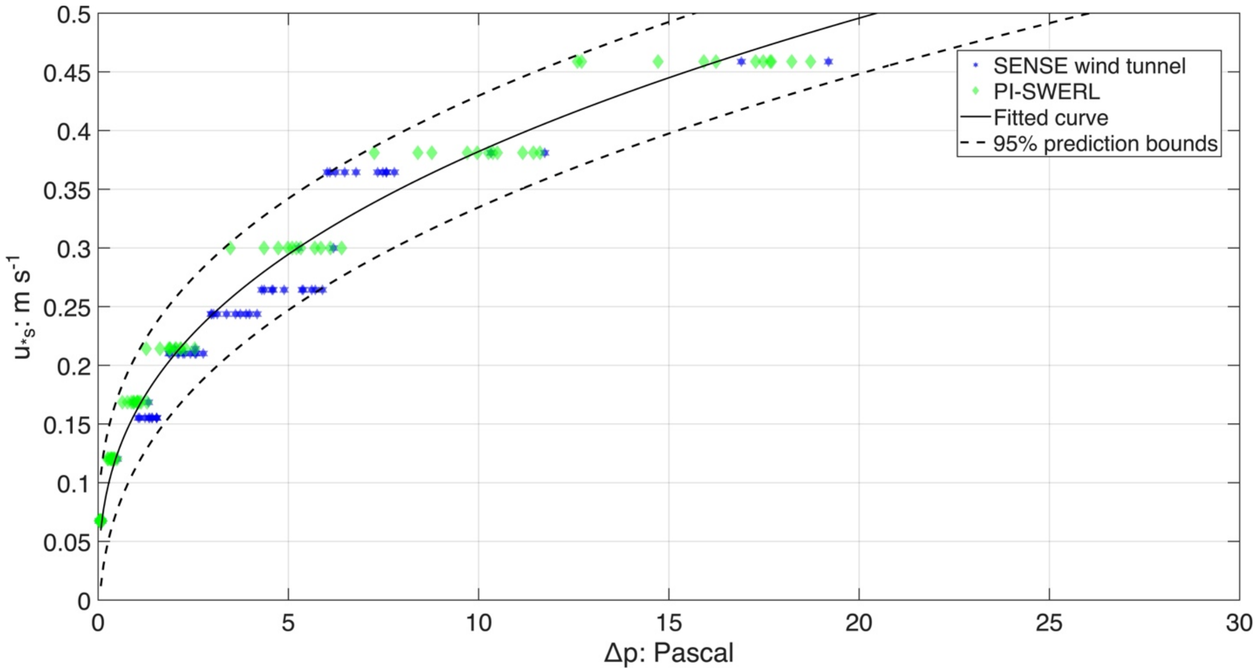


Figure 4. Calibration curve and 95 % prediction bounds for surface shear velocity u_{*s} (m s^{-1}), measured in the SENSE wind tunnel (blue hexagrams) and with the PI-SWERL (green diamonds) as a function of the differential pressure measured by individual Irwin sensors. A single set of regression coefficients was fitted to data for all sensors.

(20) Page 7 : Line 138 Under what conditions were they calibrated in the SENSE tunnel - what kind of surface fetch etc?

The details associated with Irwin sensor calibration are described in the following published paper.

Ziegler, N.P., Webb, N.P., Edwards, B.L., Nikolich, G., Gillies, J.A., Patel, S.G., Wheeler, B., Zhang, P., Van Zee, J.W., LeGrand, S.L. and Trautz, A.C., 2026. An updated Irwin sensor for measurement of surface shear velocity. Aeolian Research, 75, p.101024.

To clarify the reviewer's concern, we added the sentence below in Section 2.2.

Detailed procedures for the wind tunnel and PI-SWERL calibration are described in Ziegler et al. (2026).

(21) Page 7 : Line 138-140 Aren't the PI-SWERL shear velocity estimations based on wind tunnel calibrations to the RPM? Are there overlaps between the wind tunnel and PI-SWERL calibrations for continuity? More explanation/discussion is needed here to justify this approach and it is crucial to the estimation of main variable used in this study

If the reviewer is referring to the mechanism of how PI-SWERL is used to estimate shear velocity, then yes, shear velocity from the PI-SWERL is calibrated to RPM.

However, we are not sure we understand the question about overlap. Is Reviewer 3 asking if the PI-SWERL was calibrated in the same wind tunnel as the Irwins, or is the reviewer asking if there is overlap in the u_{*s} values tested by the two different methods. In any case, the rationale for the approach of using two combined methods is quite straightforward and is already described in the text by: "Wind tunnel calibration was limited to a relatively narrow range of u_{*s} (< 0.4 m s^{-1}). Therefore, PI-SWERL (Etyemezian et al., 2014) experiments were conducted to extend the calibration to higher shear velocities representative of field conditions." The relevant references provided in the text describe how the PI-SWERL is calibrated to estimate u_{*s}. Also, as above, we added: "Detailed procedures for the wind tunnel and PI-SWERL calibration were described in Ziegler et al. (2026)" and added detail to the figure to show the points recorded in the wind tunnel and with the PI-SWERL.

(22) Page 7 : Line 144 The R^2 and RMSE are over and underestimated respectively due to the pseudo-replications of 10+ measurements made at the same U*s. These values should be averaged before the power law is applied to reduce over fitting and inflated confidence in the calibration.

We respectfully disagree. 1) The point here was to provide a single set of calibration coefficients for all of the sensors; hence we used all of the data points. We wanted the reported error to represent that. The 10+ measurements at each u_{*s} are from different sensors. 2) With fewer points in the fit, e.g., using the mean pressure values for the different u_* values, wouldn't the R^2 increase and the RMSE decrease? That is opposite to Reviewer 3's contention. Also, the revision we made to Reviewer 3's previous comment adds additional context and clarity vis a vis the instrument calibration. Accordingly, no further revision was made

(23) Page 8 : Line 150-151 This wind speed corresponds to what U* value? How does sediment transport impact the results knowing that this windspeed filter likely results in below and above transport threshold conditions to be included?

Thank you for the comment, but u_* is not constant for a given wind speed. It changes with several variables at the site, e.g., wind direction, weather pattern, stability, etc. Thus, we did not include a u_* value that corresponds to the wind speed cut off used to parse the data.

Although this study didn't focus on sediment transport, Reviewer 3 is correct that the data includes wind speeds both below and near or above threshold (depending on conditions such as degree of soil crusting, moisture, etc.). However, we removed the sensors when extreme events were anticipated to prevent damage. Also, when transport does occur at the site, it is often highly variable across space and intermittent, so it does not approach equilibrium conditions and form a 'saltation layer,' so to say. There were time periods during the study when intermittent saltation was present, but for this study we considered the potential impacts to the boundary layer structure and u_* measurements to be minor given the long-term deployment, quality control methodology, and averaging interval.

(24) Page 8 : Line 157-158 A better explanation on how the authors can justify that a 1 m high shear stress measurement is representative of the total shear stress of the surface boundary layer in 1 m high sparsely vegetated surface. Although the surface values are all normalized by this same value the likely

underestimation from placing the instrument in the inertial sublayer along with larger variability will provide erroneous estimates of the SSR.

Thank you for the comment. We agree that the estimated shear velocity measured at 1 m above the surface should not be interpreted as representing the plot-scale shear velocity within the constant-stress layer. However, this is not the objective of this study. Our goal is to quantify the relative redistribution of surface shear stress around an individual shrub by normalizing simultaneous measurements from all Irwin sensors using a reference shear velocity. Therefore, any systematic bias in the reference shear velocity would affect all normalized values equally and would have little influence on the spatial patterns of R'. Although the estimated u_* from the 1 m sonic anemometer is not intended to represent the plot-scale shear velocity within the constant-stress layer, our previous evaluation (Table 3 in Zhang et al., 2024) showed that, for 10 m wind speeds between 5 and 11 m/s, the difference between u_* estimated from the 1 m sonic anemometer using the double rotation method (the approach adopted in this study) and u_* estimated from the logarithmic wind profile using wind speeds measured at 0.5, 1.0, 1.5, 2.5, 5.0, and 10 m height was generally less than 9%. Since the logarithmic wind profile is commonly used to estimate plot-scale shear velocity, this suggests that any systematic bias associated with using the 1 m sonic as the reference is relatively small over the wind speed range considered in this study.

Besides, the measured momentum flux is representative of the approaching flow governing the shrub mosaic system. Using a sonic anemometer installed substantially higher above the canopy would increase the measurement footprint and characterize momentum exchange over a larger area than the local flow relevant to the observed shear stress redistribution that the shrub was directly responsible for. We have added the following text in Section 2.1 to clarify these concerns.

"The shear velocity estimated from the sonic anemometer was used as the reference for normalizing u_{*s} to quantify the relative distribution of surface shear stress around an individual shrub. Therefore, any systematic bias in the reference shear velocity would affect all normalized values equally and would have little influence on the spatial patterns of shear stress redistribution. The shear velocity estimated from the sonic anemometer should not be interpreted as representing the plot- or landscape- scale shear velocity within the constant-stress layer at the site."

(25) Page 8 : Line 167 Again this is not likely representing the total shear stress - justification needed.

Thank you for the comment. This comment is addressed above.

(26) Page 8 : Line 172 ?5-min samples? are these not averages?

Thank you for the comment. Yes, they are five-minute-long samples from which the average is the reportable value. To address this confusing concern, we have revised the text as below for clarity.

"We compared temporal variability of the shear stress ratio using simple linear regression between R' and R'' for each location separately, using all 5-min average values within each phenological phase, following King et al. (2006)."

(27) Page 8 : Line 176 This is indeed a location minimally influenced by the shrub of interest, but is it representative of the average surface U*? If when takes the ratio of 1 as the threshold for sheltering/acceleration it assumes this is the average or median value (depending on the distribution) of the surface values, not just a value without influence by the shrub. More explanation is needed on why this value was chosen.

Thank you for the comment. The threshold value of $e = 1$ does not assume that the reference location represents the average or median surface shear stress across the entire landscape. The erosivity was defined relative to the reference location, which was selected because it was expected to be minimally influenced by the shrub and the fetch is sufficient that the influence of other shrubs upwind is minimal, therefore it represents the local background surface condition. Consequently, $e > 1$ indicates locally enhanced surface shear stress relative to the reference location, and $e < 1$ indicates reduced surface shear stress. Once again, the objective of the metric is to quantify the relative redistribution of surface shear stress around an individual shrub, rather than to compare each location with the average surface shear stress over the entire landscape. Please see the objective statement in the last paragraph of the introduction section.

(28) Page 9 : Line 184 ??we filtered the data to require that 8,10 and 18 sensors??: But to minimize spatial bias it is not only the number of sensors, but where they are placed - which row and what distance from the shrub. Where can this information be found?

Thank you for the comment. We agree that the original text was confusing. We have revised the text to clarify the filtering procedure and included the corresponding sensor locations. The revised text reads as follows:

"Estimates of shear stress ratio features for Eqs. 2–3 were filtered to reduce potential bias from samples being collected from different sets of locations. Because of external issues or user error during data collection (e.g., sediment burial, insect intrusion in pressure tubing, battery depletion, or loose wiring), not all time periods contained useable data from the same set of sensors. Therefore, we filtered the data to require that the same sensor locations were functioning simultaneously: 8 sensors during 2023; 10 sensors in the dormant phase of 2024; and all 18 sensor locations for the remainder of 2024. The coordinates of these sensor locations were summarized in Table 1. After filtering, 706, 87, and 477 5-min averaged samples remained for the dormant, green-up, and leaf-on phases, respectively, in 2023, and 804, 182, and 443 5-min averaged samples remained for the corresponding phases in 2024 (Zhang et al., 2025). Each sample consists of simultaneous measurements from all functioning sensors for the corresponding analysis period. Estimates of erosivity (e) were additionally filtered to a wind direction range of 245 ±10°, as any bias from wind direction at the reference location (the farthest in the upwind direction, with coordinates of (-4.3, 0.1), Fig. 1) could be amplified in the subsequent analysis."

(29) Page 9 : Line 185 Filtered to a narrower wind direction narrower than what values? If you do not need to compare then you can omit the comparative word.

We have removed the comparative word.

(30) Page 9 : Line 192 Is it not more informative to compare these values to the freestream windspeed like in other studies?

Thank you for the comment. We used wind speed measured by the sonic anemometer. This measurement represents the approaching near-surface flow directly relevant to the shrub and the surrounding surface. A freestream wind speed measured at 10 m or higher would characterize a different portion of the atmospheric surface layer and would not necessarily be more representative of the forcing associated with the observed surface shear stress redistribution. The same sonic anemometer also provided the reference shear velocity used throughout the study.

(31) Page 9 : Line 199-200 This is quite a wide variation and relatively small number of 5 minute average samples for the 10 and 18 sensors for 2023 and 2024 respectively.

Thank you for the comment. First, this text has been moved to the last paragraph of Section 2.3 in response to a comment from Reviewer 2. The three phenological phases also varied in length, hence the variation in number of samples per phase. We respectfully disagree that a total of 2699 5-minute averaged samples is a small dataset. This corresponds to ~13,495 minutes, or ~225 hours of observation after all quality-control procedures.

(32) Page : Line 706 87 and 477 correspond to roughly 70, 8, and 47, 5 minute periods in 2023 and 804, 182, and 443 corresponds to only 44, 10, and 24 5-minute periods in 2024 with the information provided.

Thank you for the comment. First, this text has been moved to the last paragraph of Section 2.3 in response to a comment from Reviewer 2.

It looks like Reviewer 3 has interpreted the text as 706/10 sensors ≈ 70. The numbers reported (e.g., 706, 87, and 477) represent the number of 5-minute samples including all the sensors, after quality control filtering, rather than the number of 5-minute periods implied in the comment. Each sample consists of simultaneous measurements from all functioning sensors for the corresponding analysis period. For example, 706 indicates that each sensor contributed 706 corresponding 5-minute averaged measurement. We have addressed this concern in response to the previous comment 26.

(33) Page 9 : Line 203 Mean values of R' or R in space or in time? They appear to be the 5 minute average samples of U*t divided by U* for each sensor and for each 5 minutes (not explained in methods 2.4). If the goal is to identify temporal trends, would it make sense to take instead the daily average of all sensors to again minimize what seems like a pseudo-replication to boost trend significance

Thank you for the comment. We have added the description to Section 2.4. Please see the tracked changes for details. Each data point used in the temporal trend analysis represents the spatially averaged R'*, or* R''*, across all functioning sensors for a single 5-minute average sample. Therefore, the reported values refer to a single spatially averaged value per 5-minute interval. The Mann-Kendall trend tests were performed using one observation (the spatial average) per 5-minute interval, not separate observations from individual sensors, which addresses the concern regarding pseudo-replication.*

The objective of this study is to quantify the influence of a shrub on the spatiotemporal dynamics of u_{*s} and drag partitioning. Using daily averages would substantially mask the shear velocity dynamics at relevant scales that are central to this study, especially considering the intermittency of wind erosion issues in drylands. Daily averaging would answer a different scientific question rather than the 5-minute timescale considered here.

(34) Page 10 : Line Fig. 5 Figures could be improved by placing the legend in a single box on the first figure providing letters for the subplots, improving the caption to point out which figures are for which years

Thank you for the comment and suggestion. We have updated the figure and caption as suggested.

(35) Page 11 : Line Fig. 6 If the goal is to look at differences as a result of greening, daily averages or indeed an anova on the values regrouped by the three greening phases would be more appropriate. Here the significance of the trend is masked/emphasized by the clumps of values that just identify a change over the greening phases versus a rate of change.

Thank you for the comment. Comparing differences among the three phenological phases was not the goal of Fig. 6. The goal was intended to examine any trends in U or u_* with time, across all three phenological phases for each year, rather than among the different phases. We edited the Fig. 6 caption to avoid potential misunderstanding.

(36) Page 11 : Line Fig. 6 Subplot letters are needed here

Thank you for the comment. We have updated the figure and caption as suggested.

(37) Page 11 : Line 217 Remove or at least replace the X and Y variables with the actual variables

Thank you for the comment and suggestion. We have replaced the X and Y variables with the actual variables as suggested in the main text.

(38) Page 11 : Line 225 Why is this not also calculated for the temporal variability?

Table 2 includes the temporal variability in the fifth column.

(39) Page 12 : Line 234-240 Suggest moving to Annex or supp material and explaining in methods that there was similar results between years when not accounting for new sensors. Although it is curious that if indeed there are no differences that the two years of data are not amalgamated in the regression analyses. Also, only S2 is referenced in manuscript. Suggest perhaps combining figures.

The purpose of additional analysis was to evaluate whether the eight additional sensor locations installed in 2024 influenced the estimate of spatial variability of R'. The negligible difference (< 1.5%) indicates that the observed interannual differences are not an artifact of the different sensor deployments. It was not intended to demonstrate that the 2023 and 2024 datasets are equivalent. The regression analyses were performed separately because the two years represent independent observations periods with different wind and surface conditions. They were analyzed to assess interannual consistency rather than *to derive a single combined relationship. We believe it is appropriate to keep the paragraph in the main text because the validation supports the interpretation of the results.*

We referenced all supplementary figures in the original manuscript (see line 305). In response to Reviewer 1, we also added another supplementary figure. Please see the details in the tracked changes version.

(40) Page 12 : Line 241 Are these regrouped by downwind positions or treated as individual? Where are these results shown (table/figure)?

Thank you for the comment. The referenced sentence is about the temporal variability of R' at each sensor location. The sensor locations are not grouped by any criterion. The results are presented in the last two columns of Table 2. We have added the following text to make this clearer.

"A significant linear relationship (R^2 *>0.87, p <0.05*) was found between R' and R'' at each sensor location. The average temporal variability across all sensor locations for each phenological phase and year was summarized in the last two columns in Table 2."

(41) Page 13 : Line Fig. 8 Sensor location names need to be added here to help guide the reader while following the text

Thank you for the comment and suggestion. Figures 8, 10, and 11 are intended to highlight the spatial distribution of R' and its associated characteristics. To avoid visual distractions from sensor location names, we have updated the sensor location nomenclature to normalized distances throughout the manuscript.

(42) Page 13 : Line Fig. 8 Are the phases a1/a2 b1/b2 and c1/c2? They are not referred to this elsewhere if that is the case and overlap with the sensor position naming convention adding confusion

Thank you for the comment and suggestion. We have updated the sensor location nomenclature to normalized distances throughout the manuscript. Also, we have updated the panel labels from(a1-c2) to (a-f).

(43) Page 13 : Line 251 Based on the discussion below perhaps expressing the values in Figure 8 as the percent difference from the upwind value is more helpful for the discussion? Or a combined graphic with colors as absolute R and contours as percent difference.

Thank you for the comment and suggestion. Figure 11 already presents the erosivity (e), defined as the ratio of R' at each sensor location to the upwind reference location. So, it already provides a relative measurement of the spatial variation in R'. Combining Figures 8 and 11 into one figure would make the visualization more complex and more difficult to interpret. We decided to preserve the current design of the two figures, with Figure 8 highlighting the spatial distribution of R' and Figure 11 showing its relative magnitude with respect to the upwind reference.

(44) Page 13 : Line 252-253 First mention that this is where (some) of the additional eight sensors are located ? this info needs to be in the methods.

Thank you for the comment and suggestion. The locations of the additional eight sensors were already described in the Methods (Section 2.1), but we agree that making these locations much more apparent in the instrument deployment layout would improve clarity. Figure 1 has been updated to clearly identify the locations of the eight additional sensors.

(45) Page 14 : Line 269 Used relationship and regression before and now trend to explain these results - perhaps try to utilize the same terminology.

Thank you for the comment and suggestion. We have revised the text to use consistent terminology throughout this section and the whole manuscript.

(46) Page 15 : Line Fig. 10 Suggest providing a qualitative colorbar instead of a continuous one that is divergent from the value of zero if the intention is to show the negative or positive changes

Thank you for the comment and suggestion. The purpose of Fig. 10 is to illustrate both the direction of change (positive or negative) and the magnitude of the changes in $\Delta R'$, as described in the main text (original line number 278-290). A continuous color scale allows readers to interpret both aspects. Therefore, we decided to preserve the current color scheme.

(47) Page 15 : Line 283 These locations should be referenced relative to the axes provided (normalized distance) to allow for transferability versus project specific nomenclature.

Thank you for the comment and suggestion. We have updated the sensor location nomenclature to normalized distances throughout the manuscript.

(48) Page 15 : Line 288 So the figure includes outliers or not? Not clear from this phrasing and without letter labeling it is impossible to tell. Suggest removing outliers on result figures, while providing raw results in supp mat.

Thank you for the comment and suggestion. The outliers have been removed from the current figure. To clarify this, we have updated the figure caption.

(49) Page 15 : Line 289-290 Not sure there is enough evidence here alone to suggest that the wind forcing drives growth.

Thank you for the comment. We agree that our data do not directly demonstrate that enhanced wind forcing constraints vegetation growth. We have revised the text to clarify that the potential influence of enhanced wind forcing on vegetation growth is speculative and needs further investigation.

The observed spatial pattern suggests a potential feedback between vegetation morphology and near-surface wind forcing, although this requires further investigation.

(50) Page 15 : Line 298-299 Again here the relative description of changes to the row letters is not helpful for the reader nor for transferability of the results

Thank you for the comment. We have updated the sensor location nomenclature to normalized distances throughout the manuscript.

(51) Page 15 : Line 304 Is this just a function of windspeed then? Perhaps the data should be partitioned by below and above threshold vs an arbitrary windspeed to better reflect logistical challenges (scouring).

Thank you for the comment. We are not sure we fully understand the reviewer's concern. This sentence describes the spatial correspondence between higher erosivity and field observations of scouring rather than the dependence of erosivity on wind speed. As described in the Methods, all analyses were performed with wind speeds exceeding 4 m s^{-1}. The influence of wind speed was evaluated in Section 3.3.

(52) Page 16 : Line Fig. 11 Not clear what the subfigure alphanumeric references here as well. Suggest providing (a) through (f) captions

Thank you for the comment and suggestion. We have updated the panel labels to (a-f).

(53) Page 16 : Line Fig. 11 The annotated row names on the x-axis are somewhat helpful but from figure 1 do not necessarily seem to be properly placed as they do not follow an actual grid spacing. Therefore my suggestion is to use normalized distance rather than row names anywhere in the manuscript

Thank you for the comment and suggestion. We have updated the sensor location nomenclature to normalized distances throughout the manuscript.

(54) Page 18 : Line 354-355 Could this lack of relationship also be due to the U reference being in the sublayer and not above canopy flow making R' and U correlated

Thank you for the comment. Our analysis showed no systematic relationship between R' and U over the measured wind speed range. If the reference location introduced a correlation between R' and U, an apparent relationship would be expected rather than the lack of one. The sonic anemometer was located approximately 3 m upwind of the shrub and was used as the common reference for both wind speed and shear velocity. The upwind fetch was about 15 m with only scattered grasses and forbs. Our previous study (Zhang et al, 2024) showed that 1 m reference u_* differed by less than about 9% from the log-law estimate. Besides, this study was conducted on a sparsely vegetation dryland rather than a horizontally homogeneous canopy, a distinct above-canopy or roughness-sublayer depth cannot be defined unambiguously. Therefore, we preserve the current interpretation.

(55) Page 20 : Line 402-404 I am not sure how this was elucidated by the present results. Scouring effects were not described beyond a logistical problem for sensor measurement.

Thank you for the comment. We agree that the original wording could be misleading. We intended to place the observed acceleration zones and localized scouring into the broader context of wind erosion processes in drylands. We have revised the sentence as follows:

Our results highlight the geomorphic importance of acceleration zones around vegetation, where enhanced wind forcing may promote localized scouring. Over longer timescales, repeated scouring associated with intensified saltation has been shown to abrade plant tissues, expose roots, promote plant mortality, contribute to canopy-gap expansion, and facilitate the formation of streets and nebkhas (Ravi, et al., 2010).

(56) Page 20 : Line 411 Likely due to the reduced spatial coverage of the measurements in this study than it being due to the study being conducted in a natural environment.

*Thank you for the comment. We are unable to interpret this comment in the context of the cited text. The referenced sentence reads, "*Results show that the spatial variability of R' was greater than that reported in earlier studies.*" This is simply a statement of fact and does not attribute the greater variability to any particular cause. Nowhere in this paragraph do we refer to a "natural environment" or suggest that the observed differences are due to the study being conducted in a natural environment. As such, it is unclear what revision Reviewer 3 is requesting, and no revision was made.*