We sincerely appreciate the reviewer's expert guidance on our manuscript. In response to all comments, we have meticulously implemented comprehensive revisions throughout the paper. The specific modifications are detailed as follows:

Blue text indicates the authors' point-by-point responses to each suggestion.

Blue text highlighted in yellow denotes sections of the original manuscript revised according to the reviewer's recommendations.

All modifications in the revised manuscript are highlighted in red text for clear identification.

This study presents a potentially useful global wind-erosion dataset with long temporal coverage and relatively high spatiotemporal resolution. The dataset could be valuable for future research on wind erosion, dust emission, and land degradation. However, several issues still require clarification or further improvement:

Lines 100–105: ERA5 meteorological data are typically provided at 0.25°, whereas ERA5-Land is provided at 0.1°. Please clarify the exact source and spatial resolution of the meteorological inputs used in this study. Since the Weather Factor is a major driver in RWEQ, and the meteorological data appear to have been resampled to 0.05°, it is unclear whether the resulting wind-erosion product can truly be regarded as a 0.05° assessment?

Responds: We sincerely thank the reviewer for pointing out this critical issue regarding data sources and spatial scaling. We fully agree that clarifying the native resolution of driving data and the rationale for the 0.05° output grid is essential. Please find our detailed responses below:

1. Clarification of the exact meteorological data source:

We apologize for the ambiguous description in the original text. In fact, the meteorological variables used in this study (including 10m wind speed, 2m temperature, precipitation, snow depth, and solar radiation) were derived from the ECMWF ERA5-Land reanalysis dataset, rather than the native ERA5 atmospheric grid. ERA5-Land provides a native spatial resolution of 0.1°, which has been specifically optimized for land-surface applications. We have revised the text in Section 2.1 to clearly state.

2. Why do we still consider the final product a 0.05° assessment?

We completely understand the reviewer's concern that resampling the Weather Factor (WF) from 0.1° to 0.05° does not genuinely create new meteorological information. However, we argue that the final 0.05° wind erosion assessment is not merely a resampled weather map, but an integrated product where the high-resolution surface parameters play a dominant role in shaping the spatial heterogeneity.

In the RWEQ framework, the Wind Erosion Force is modulated by the Combined crop factor (COG). Our COG is derived from the GLASS Fractional Vegetation Cover (FVC) product, which features a native 0.05° (8-day) resolution. Meanwhile, our land use data (GLC_FCS30D, 30m), soil properties (SoilGrids, 250m), and topographic roughness (DEM, 30m) all possess much finer spatial details.

Therefore, the 0.05° grid serves as a unified computational framework that allows the high-resolution spatial heterogeneity of vegetation, soil erodibility, and surface roughness to precisely modulate the relatively coarse meteorological forcing. If we had produced the output at a coarser 0.1° resolution, we would have lost the critical spatial gradients of vegetation cover and soil texture that determine where erosion actually occurs within a meteorological grid cell. To avoid misleading readers, we have added the following statement in Section 2.1 (Data Processing):

"Although the meteorological forcing was originally at 0.1°, the final 0.05° output grid was adopted to integrate and preserve the high-resolution spatial heterogeneity of vegetation cover, soil properties, and land use, which are also the primary modulators of the spatial pattern of wind erosion. The 0.05° grid does not imply that the wind speed data were downscaled to that resolution, but rather that all driving factors were unified into this grid for consistent spatial computation and product standardization."

3. Acknowledgment of limitations:

We entirely agree that this scale mismatch remains an inherent uncertainty in global wind erosion modeling. Following the reviewer's suggestion, we have strengthened the Limitations and Future Improvements (Section 4.3) to explicitly point out this issue:

"Finally, while we used 0.05° as the final output resolution, the meteorological inputs (ERA5-Land) were originally at 0.1°. Although the high-resolution vegetation and soil data justify a finer computational grid for spatial modulation, we acknowledge that this does not represent a true downscaling of wind fields. Future work could incorporate dynamic downscaling or higher-resolution meteorological datasets to further improve the physical accuracy of the Weather Factor at finer scales."

Line 106: For the GLASS FVC product at 0.05°, please specify which sensor-based product was used, i.e., AVHRR_0.05° or MODIS_0.05°. This is important because the MODIS-based product is only available after 2000.

Responds: We sincerely thank the reviewer for this critical clarification. We fully agree that specifying the exact sensor source is essential, especially for a long-term study spanning 1982–2020.

Upon rechecking our data processing workflow, we confirm that the GLASS FVC product used in this study is the AVHRR-based version at 0.05° resolution, rather than the MODIS-based version.

As clearly documented in the GLASS product suite overview (Liang et al., 2021), the AVHRR-based GLASS FVC product provides global coverage from 1981 to 2020 with an 8-day temporal frequency and 0.05° spatial resolution. This temporal coverage perfectly matches our study period (1982–2020). We have now revised the text in Section 2.1 to explicitly state this to avoid any ambiguity.

Line 141: Please specify the value assigned to the distance from the upwind edge (x).

Responds: We thank the reviewer for pointing out this missing parameter specification. In our global RWEQ simulations, the distance from the upwind edge of the field (parameter x in Eq. 3) was set to a constant value of 50 m. This value is consistent with the commonly adopted fetch length settings in other large-scale gridded RWEQ assessments (Sun et al., 2024). We have now explicitly added this specification in Section 2.2 (just after Eq. 3) to improve the reproducibility of our method.

Line 149: Please provide the reference for converting 10 m wind speed to 2 m wind speed (Equation 6).

Responds: We thank the reviewer for this suggestion. The wind profile conversion formula (Equation 6) used in this study follows the standard logarithmic profile applied in the RWEQ modeling framework. We have now added two key references to support this conversion in new manuscript.

Line 154: The numbering of Equation (6) appears to be incorrect. In addition, this equation seems to be used to convert daily wind speed into hourly wind speed. However, ERA5 and ERA5-Land both provide hourly wind data. Please clarify why hourly data were not used directly and justify the need for this conversion.

Responds: We sincerely thank the reviewer for identifying the numbering error and for raising this important question regarding the temporal resolution of wind input.

We apologize for this oversight. The equation for the cosine function (originally numbered as Equation 6) should indeed be renumbered. We have corrected the numbering throughout Section 2.2.1.

We fully agree that ERA5-Land provides hourly wind speed data. However, the key issue lies not in the temporal interval (1 hour), but in the observational averaging time (integration period). ERA5-Land hourly wind speeds represent 1-hour averaged values, which inherently smooth out high-frequency gustiness and short-duration wind extremes.

In RWEQ modeling, the wind factor (wf) is calculated as the Equation 6 , where wind speed is raised to the third power. This cubic relationship makes the wind factor highly sensitive to the upper tails of wind speed distributions. Using 1-hour averaged wind speeds directly would systematically underestimate wind erosivity, because the averaging process removes the high-speed gusts that are most effective for particle entertainment.

To overcome this limitation, we adopted the approach of Guo et al. (2012), which was specifically developed to convert daily average wind speeds into sub-daily wind speed series. Their study demonstrated that a cosine-based diurnal variation function provides a robust statistical approximation of the minute-scale wind speed distribution from daily mean data, effectively recovering the wind speed variability required by the RWEQ. This method is not designed for converting daily to hourly data, but rather for reconstructing a more realistic wind-speed frequency distribution that preserves the erosive wind peaks, which are critical for accurate wf calculation. Since global, long-term (1982–2020), minute-scale observed wind data are unavailable, the Guo et al. (2012) approach offers a validated and widely accepted compromise.

Lines 202–203: The formatting of β is inconsistent, appearing as upright and italic in different places. Please standardize the formatting of this symbol and check the consistency of other variables throughout the manuscript.

Responds: We thank the reviewer for carefully checking the symbol formatting. We have thoroughly reviewed the entire manuscript and standardized the formatting of all mathematical symbols and variables according to the common academic convention.

Line 328: RWEQ simulates surface wind erosion, whereas dust-related datasets reflect atmospheric signals after emission, uplift, transport, dispersion, and deposition. Therefore, such datasets can only provide indirect validation rather than direct validation of modeled wind erosion itself. Please discuss this limitation and the associated uncertainties more explicitly.

Responds: We sincerely thank the reviewer for this critical and insightful comment. We fully agree that the MERRA-2 dust emission data and AERONET coarse-mode AOD represent atmospheric signals that integrate not only surface emissions but also subsequent uplift, vertical mixing, long-range transport, dispersion, and deposition processes. Therefore, they cannot be considered as “direct validation”of the simulated surface wind erosion.

In response, we have made the following revisions to the manuscript:

First, we have added a new, dedicated paragraph in Section 4.3 (Limitations and Future Improvements) to thoroughly discuss the decoupling processes between surface wind erosion and atmospheric dust signals, including: (a) particle-size selective uplift (only the finest fractions are suspended); (b) vertical turbulent mixing efficiency; (c) long-range advection and removal mechanisms; and (d) the uncertainties introduced by our fixed 1000 km aggregation radius when comparing with AERONET stations.

Second, we have emphasized that despite these limitations, the significant spatial (R^2 = 0.79) and temporal (14 out of 16 AERONET stations with significant correlations) consistencies are still scientifically meaningful—they demonstrate that our modeled wind erosion captures the primary spatiotemporal variability of the wind-blown dust system, even after accounting for the complex post-emission processes. This provides indirect but valuable support for the dataset’s credibility.

Line 353: The discussion here appears to compare the present RWEQ-based results mainly with previous RWEQ-based studies. This type of comparison is useful for showing broad consistency, but it is closer to inter-model consistency than to independent validation. Please clarify the significance and limitations of this comparison. If possible, it would be helpful to include more direct observational evidence for validation.

Responds: We thank the reviewer for this thoughtful methodological comment. We fully agree that comparisons with previous RWEQ-based studies primarily reflect inter-model consistency rather than independent validation, and we have revised Section 4.1 to explicitly clarify both the significance and limitations of this approach.

Significance: Despite sharing the same RWEQ framework, each study differs substantially in its input data sources (e.g., meteorological reanalysis products, vegetation indices, soil databases), parameterize schemes (e.g., residue factor treatments, threshold wind speeds, fetch length assumptions), and spatial-temporal resolutions. The fact that our results remain within the same order of magnitude and show strong spatial correlation (Pearson correlation coefficient = 0.86) with these independent implementations provides compelling evidence that our results are robust. This consistency across independent modeling efforts reinforces the credibility of the RWEQ approach for global-scale wind erosion assessment.

Limitations: We acknowledge that this comparison does not constitute independent validation in the strict sense, as it does not directly evaluate modeled erosion against measured erosion magnitudes. Ideally, direct observational evidence, such as ^{137}Cs-derived erosion rates, sediment trap measurements, or long-term erosion monitoring plots, would provide stronger constraints. However, such measurements are inherently site-specific and not available at a globally consistent and spatially continuous scale. The absence of a global observational benchmark is a well-recognized challenge in the wind erosion community (Chappell et al., 2019; Webb et al., 2020), and our comparisons with MERRA-2 dust emission data and AERONET AOD (discussed in Section 3.4) represent our effort to provide multi-faceted consistency assessments using independent datasets. We have now added this clarification in Section 4.1 to frame the comparison appropriately.

Revised text added to Section 4.1:

"We acknowledge that this comparison primarily reflects inter-model consistency rather than independent validation. Nevertheless, it is scientifically meaningful because each RWEQ implementation relies on different input data sources and parameterize choices. The observed consistency across these independent implementations suggests that our results are robust and not overly sensitive to specific data or parameter choices."

Line 359: Is there direct evidence supporting this explanation? If not, this should be presented as a plausible interpretation rather than a near-confirmed mechanism. Please consider revising the wording accordingly.

Responds: We thank the reviewer for this careful comment. We have revised the sentence to more accurately reflect the intended meaning as follow:

"Short-duration strong wind events often contribute to a substantial proportion of the total wind erosion, even though they occur less frequently."

We believe this revision appropriately addresses the reviewer's concern and presents the statement as a plausible and evidence-supported interpretation rather than an over-confirmed mechanism.

Line 360: A finer spatial resolution may indeed improve the identification of local hotspots, but it may also better distinguish low-value areas or areas where wind erosion does not actually occur. Therefore, its effect on the global total is not necessarily unidirectional. I suggest presenting this point more cautiously.

Responds: We thank the reviewer for this important and insightful clarification. To avoid presenting an oversimplified or potentially misleading argument, we have removed this sentence entirely from the revised manuscript. The surrounding text has been checked and remains logically coherent without this statement.

Units of total wind erosion: When reporting total wind erosion in Pg, I suggest explicitly indicating the time scale, for example, Pg/year, to avoid ambiguity.

Responds: We thank the reviewer for this helpful suggestion. To avoid ambiguity in reporting total wind erosion amounts, we have carefully reviewed the manuscript and explicitly added the time scale wherever total wind erosion values are reported.

Figure 4a: Relatively humid or highly vegetated regions also appear to show widespread wind-erosion signals. Could the authors please clarify whether this might indicate a possible overestimation of the spatial extent of wind erosion?

Responds: We thank the reviewer for this question of clarification. We would like to emphasize that Figure 4a illustrates the trend in wind erosion (Sen's slope, i.e., change rate), rather than the mean wind erosion intensity. The spatial pattern of wind erosion magnitude is shown in Figure 2a, where humid and densely vegetated regions indeed exhibit very low erosion rates (mostly close to 0), which is physically reasonable.

Regarding Figure 4a, we applied the Mann–Kendall test and Sen's slope estimator to every grid cell across the globe. In the current figure, we plotted the Sen's slope values for all grid cells, regardless of whether the trend was statistically significant. Consequently, even in humid regions where wind erosion is extremely weak, any minor non-zero trend (whether positive or negative) will still yield a non-zero β value and thus appear as a colored pixel on the map. This does not imply that these regions experience substantial wind erosion or that the spatial extent of wind erosion is overestimated; rather, it simply reflects that a trend was calculated wherever wind erosion exists, no matter how small the magnitude.

Figure 5a: It would be helpful to explain the meaning of the x-axis numbers in the figure caption, since the current labeling is not immediately clear.

Responds: We thank the reviewer for this suggestion. The x-axis numbers in Figure 5a represent the 8-day temporal periods within a year (i.e., Period 1 corresponds to Days 1–8, Period 2 to Days 9–16, ..., and Period 46 to Days 361–365/366). We have revised the figure caption to explicitly state this, as follows:

"Figure 5 Seasonal wind erosion characteristics in different arid regions (a, seasonal distribution of wind erosion across 46 8-day periods within a year; b, concentration; c, fluctuation in each region; period 1: Days 1–8, period 46: Days 361–365/366)"

We believe this addition will make the figure more self-explanatory for readers.

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