nature portfolio

Peer Review File

The distribution of Earth's wind-blown sand dunes

Corresponding Author: Dr Andrew Gunn

This file contains all reviewer reports in order by version, followed by all author rebuttals in order by version.

Version 0:

Reviewer comments:

Reviewer #1

(Remarks to the Author)
This is a fine paper and I look forward to seeing it published. It provides a new global dataset on sand dunes and valuable analysis helping explain why they are found where they are. The dataset will find many uses beyond this study. The work fully supports the conclusions and I see no significant flaws in the methodology. My comments are minor:

The use of superlatives is not common in scientific writing and I encourage you to consider removing or re-wording them. Examples: Line 31: exhaustive; Line 63: well-chosen; Line 75: highly relevant

Line 74: a short sentence explaining saturated flux divergence would be useful here so that readers don't have to go to the methods

Figure 3: legends for AI and flux divergence are hard to read. Is it possible to make them larger so that the colors can be read more easily?

Line 104: by convention, u is wind speed and u* is friction speed (with * as subscript). Is there a reason to not follow convention?

Figure 4: the orange and red lines are too similar. Can one be changed to a more obvious color?

Line 113: Are you equating barchan and parabolic dunes. Are there parabolic dunes in the image? It seems that you are only working with barchans. (I'm likely missing the parabolics, in which case you can ignore this comment.)

Line 130: …as these ARE a small…

Line 157: visually identified? If identification was automated for this first step, it should be explained. OK, I see the refence to Supp 1. It might help to put that earlier in this section.

Line 188: there should be a reference for RDP/DP (or greater explanation). Are you using all wind speeds or only those above threshold?

Line 198: Please explain more, preferable in the main text. Divergence from what? (Sorry, this term is new to me.)

Supplementary Table 1: Is there a reason that Tsoar's mobility index (https://doi.org/10.1016/j.physa.2005.05.067) is not included? (I don't insist that it be added)

IF others were involved in the research (visually checking images, coding, etc.) you might consider listing them in the Acknowledgements.

(Remarks on code availability)

Reviewer #2

(Remarks to the Author)
3. Comments for transmission to the author
The manuscript presents a global compilation of dune presence on a 0.1° grid and uses it to re examine the climatic and sedimentological conditions under which dunes occur. The overall scope is impressive, and the dataset could be a useful community resource. However, several central claims and aspects of the framing need to be moderated and better anchored in the existing literature.
1 Claims about novelty and global mapping
Throughout the manuscript, you imply that this is the first complete and accurate global map of Earth's sand dunes with distinguishable morphologies, and that you are the first to reassess, quantitatively, the conditions necessary for dune formation. This is overstated.
• Wilson (1973) produced a global map of ergs, and several subsequent studies have also presented global or near global maps of aeolian sand seas. While these did not systematically encode dune morphology at your grid scale, they did map the global distribution and extent of major dune fields.
• In addition, the book of Martínez and Psuty (2004) compiled a global map of coastal dunes; many of these coastal dune systems are not visible in your Figure 1, despite your description of the map as "complete and accurate" for dunes with distinguishable morphologies.
If Figure 1 is intended to be the "first exhaustive global map," then it needs to (a) be much more carefully validated against existing global and regional maps, and (b) explicitly acknowledge that it generalizes and sometimes coarsens previous work, rather than replacing it. At present, the map appears too general: for instance, most of the Sahara is shaded as dune covered, in contrast to Wilson's (1973) mapping of 27 discrete ergs that collectively occupy only about 25% of the Sahara. Moreover, dune morphologies per se are not visible in Figure 1; only presence/absence is shown.
I suggest you rephrase the novelty claim along the lines of: a global, manually curated 0.1° grid of dune presence, with ancillary information on morphology derived from imagery, rather than "first complete and accurate global map."
2 Use of AI and saturated flux versus previous indices
You emphasize AI and saturated sand flux magnitude and divergence as the key predictors of dune presence. These are certainly important variables, but they are not new to the aeolian literature:
• Lancaster's (1988) Mobility Index explicitly combines AI and wind speed above the threshold for sand transport. This index has been widely used to distinguish between active and stabilized dunes and to explore climate change impacts on dune fields.
• Wilson (1971, 1973) already highlighted the role of transport convergence and divergence in controlling where ergs form in the Sahara.
In your framework, saturated sand flux magnitude is used as a proxy for wind power. This may be physically appealing, but most aeolian geomorphological studies of dune mobility have historically used DP as wind power potential (DP in Fryberger, 1979).
Papers such as Wasson and Hyde (1983) and Yizhaq et al. (2007) (which you cite) emphasize wind drift potential (and vegetation response to DP) as the variable that most directly explains the coexistence of vegetated and unvegetated dunes and the hysteretic response of dune systems to climate. It would strengthen the manuscript if you:
• Explicitly discuss the relationship between your saturated flux metric and classical DP, including advantages and limitations.
• Clarify why saturated flux is a better or more appropriate predictor in your global analysis than DP (or, alternatively, show that they yield qualitatively similar results).
Your own result that "neither AI nor saturated flux alone can explain the distribution of dunes" is important, but it needs to be more clearly framed in relation to existing indices. At present, it reads as if AI and flux are being introduced de novo.
3 Limitations of AI as a proxy and role of vegetation and biogenic crusts
You argue that AI correlates well with vegetation presence and that it is therefore a reasonable proxy for the inverse of sediment availability. This is broadly true, but there are important exceptions that your framework does not address and should at least acknowledge:
• Active or partly active coastal dunes exist in humid climates with AI > 0.65 (e.g. Oregon coast, north eastern Brazil, Baltic coasts). These dunes draw directly on large beach sand supplies, and their activity is controlled by a combination of wind climate, shoreline dynamics, and local vegetation, not primarily by aridity.
• Many desert dunes in regions with mean annual rainfall around 100–150 mm are stabilized by biogenic crusts, even though AI would classify them as relatively arid and ostensibly suitable for dune mobility (e.g. Kidron et al., 2000; Maman et al., 2011; Kinast et al., 2013). In these cases, biological soil crusts can strongly reduce sand availability without a corresponding signal in AI.
How do such systems fit into your AI–flux framework? At a minimum, the manuscript should:
• Explicitly acknowledge that AI is an imperfect proxy for sediment availability, especially in coastal dune systems and crust stabilized deserts.
• Discuss whether these types of dunes are under represented or misclassified in your global dataset, and what that implies for the generality of your AI based inequality.
4 Interpretation of Fig. 1 and global coverage
The title of Figure 1 ("Distribution of Earth's wind blown sand dunes with discernible morphologies") suggests both accurate spatial coverage and explicit morphology mapping. In practice:
• The Sahara is shown as almost entirely dune covered, which conflicts with Wilson's (1973) more detailed mapping of discrete ergs occupying a minority of the Sahara's area.
• Dune morphologies are not actually visible in the figure; the map is essentially presence/absence at 0.1° resolution.
• Many coastal dune systems shown in Martínez & Psuty (2004) do not appear clearly on your map.
You should either refine the figure and the underlying mapping or moderate the wording to reflect that this is a coarse resolution global presence map, not a detailed morphological atlas.

5 Conditions for dune formation and missing controls
You convincingly show that AI and saturated flux alone cannot explain global dune distribution (line ~48). This is, in fact, a key conclusion, but it needs more thorough exploration. Several additional factors may be critical:
• Topographic and sedimentary traps (structural or subsidence related depressions) that accumulate thick fluvial and lacustrine deposits during humid phases (e.g. in the Sahara and Sahel; see Armstrong et al., 2023) can create large sediment reservoirs that persist into arid phases.
• Human activities (e.g. vegetation removal, grazing, agriculture) can destabilize dune fields in relatively wet climates or, conversely, stabilize dunes in dry climates via land use change and restoration.
It would be clearer if you explicitly acknowledged:
• The importance of antecedent sediment supply and geomorphic setting (e.g. basins filling with fluvial/lacustrine sediment during humid periods, later reworked as dunes).
• That your present day climate and flux fields will not fully capture the formative climates of relict dunes or those strongly modified by human activity.
6 Treatment of Antarctica and Greenland
You note the “near absence of sand dunes on glacial, windy Antarctica and Greenland” and use this to argue that dunes are most prevalent at moderate, not high, saturated flux. However, in these regions the absence of dunes is primarily due to ice cover and the lack of sand sources, not the wind regime itself. Recent work has even documented snow dunes and megadunes (e.g. Traversa et al., 2023; Poizat et al., 2024), highlighting that given a granular medium, dune like forms can indeed develop. This should be mentioned to avoid conflating high flux with lack of dunes where there is simply no sand.
7 Resolution and the 0.1° grid
Your analysis is conducted on a 0.1° (~9–11 km) grid. At this resolution:
• Small dune fields and narrow coastal dune belts will be missed or diluted within mixed land cover cells.
• Detailed dune morphology is not resolvable, which limits the strength of some of the morphological claims.
You should more clearly justify that this resolution is adequate for the conclusions drawn about global distribution and controls, and explicitly state the scale limitations (e.g. that small coastal systems and narrow linear dunes are under represented).
8 Clarifications on figures, notation, and methods
• Figures 3 and 4: please define “CDF” (presumably cumulative distribution function) clearly in the caption or text.
• Figure 4: the world map is very small and visually dense; it is difficult to distinguish spatial patterns. Consider enlarging the map, adding regional insets, or simplifying the colour scale.
• Line ~84: “Sahul”, this term usually refers to the combined Australia–New Guinea continental shelf. If you mean the Sahel belt, please correct the terminology.
• Lines 129–138: you discuss relict dunes and acknowledge a gradient of activity states. It would be useful to explicitly note that dunes in populated semi arid and coastal zones may be strongly affected by human land use, so that present day AI and flux may not reflect formative conditions.
• Equation for volumetric saturated sand flux Q (around line 179): please provide a clear derivation and/or exact reference (e.g. Martin & Kok, 2017, building on Bagnold, 1941) and briefly explain the assumptions (threshold, grain size, etc.).
9 Conclusions of key messages
After reading the manuscript, two questions remain insufficiently answered:
1. What are the genuinely new insights that this study provides about global dune distribution and controls, beyond formalizing known relationships and assembling a global presence dataset?
2. What are the main take home conclusions you want readers to retain?
Given the ambition and length of the paper, the Conclusions section should be strengthened to:
• Clearly list the principal new findings (e.g. specific quantitative thresholds, the relative importance of $\nabla \cdot q$ versus AI in certain regimes, new constraints on where dune migration direction is sensitive/insensitive to sediment properties).
• Explicitly link these findings to future applications (e.g. paleoclimate inference, climate change projections, planetary analogues).

References cited in this review
Armstrong, E. et al. (2023). North African humid periods over the past 800,000 years. Nature Communications, 14, 5549.
Fryberger, S. G. (1979). Dune forms and wind regime. A study of global sand seas. E. D. McKee. Washington, U.S. Geol. Surv. Prof. Pap. 1052. 1052: 137-169.
Kidron, G. J., Barzilay, E., et al. (2000). Microclimate control upon sand microbiotic crusts, western Negev Desert, Israel. Geomorphology, 36, 1–18.
Kinast, S., Meron, E., et al. (2013). Biogenic crust dynamics on sand dunes. Physical Review E, 87, 020701.
Maman, S., et al. (2011). The Central Asian ergs: A study by remote sensing and geographic information systems. Aeolian Research, 3, 353–366.
Martínez, M. L., & Psuty, N. P. (eds.) (2004). Coastal Dunes: Ecology and Conservation. Springer, Berlin.
Poizat, G. et al. (2024). Widespread longitudinal snow dunes in Antarctica shaped by sintering. Nature Geoscience, 17, 889–895.
Traversa, G. et al. (2023). Megadunes in Antarctica: migration and characterization from remote and in situ observations. The Cryosphere, 17, 427–444.
Wilson, I. G. (1971). Desert sandflow basins and a model for the development of ergs. Geographical Journal, 137, 180–199.
Wilson, I. G. (1973). Ergs. Sedimentary Geology, 10, 77–106.

(Remarks on code availability)
1. Manuscript background information
The author claims to present the first exhaustive global map of Earth’s sand dunes with recognizable morphology. The study further argues that dune presence is primarily governed by two factors: (i) sand availability, parameterized by the Aridity

Index (AI), and (ii) the magnitude and divergence of saturated sand flux. The manuscript is ambitious in scope, but the conceptual framework and many of the key variables build strongly on previous work and are, in my view, not especially original.

Reviewer #3

(Remarks to the Author)
This is an interesting and significant article that makes good use of global data sets to map the location of inland and coastal sand dune areas, providing the first truly global map of dune area locations for Earth. It does not (and does not pretend to be) a high resolution map of dunefields – such as the work in progress (Hesse,and Lancaster,2026)

The spatial distribution of the identified dune areas is then compared to data sets on aridity, wind field, and calculated saturated sand flux to identify key boundary conditions for dune occurrence.

The data analysis provides new insights into key boundary conditions, especially saturated sand flux and aridity index. Spatial variation in flux divergence is also identified as a key boundary condition for accumulation of sand. It would be interesting to examine the role of flux directional variability on dune occurrence.

Extended data are valuable. Extended Fig 3 could be updated with current data set (7 k ages)

Weaknesses

This is a very densely written manuscript that introduces many important concepts. Sometimes it is hard to follow the discussion. Careful attention to phrasing is needed.

The figures could be improved especially the use of different color palette to clarify inferences being made. Fig 4 c is hard to understand. Because of the use of very similar colors for different variables.

The section “ Boundary condition inversion from morphology “ in which the sensitivity barchan dune orientation and migration direction to transport threshold and therefore sand particle size is evaluated is less convincing compared to other sections. I do not think it adds anything to this article.

Line comments

16 – “environments of friable sandstones” - is misleading as most dune areas are not directly associated with sandstone

23 - promote the formation of aeolian dunes

Fig 3 a – areas mapped as “aeolian sediments” are in fact sand sheets (or paleodunes in the Kalahari region) and may or may not be sources for adjacent dunes

Reference -

Hesse,, P. and Lancaster, N. 2026. The Distribution of Sand Dunes on Earth. 11th IAG International Conference on Geomorphology, Christchurch, NZ< February 2026. (Poster).

(Remarks on code availability)
I just looked at the data sets available. Cautions on running the code are appreciated.

Version 1:

Reviewer comments:

Reviewer #1

(Remarks to the Author)
All of my concerns have been addressed satisfactorily. Thank you. I have no additional suggestions.

(Remarks on code availability)

Reviewer #2

(Remarks to the Author)
I recommend that the paper conclude with a dedicated concluding section addressing two key questions:
1. What genuinely new insights does this study provide regarding the global distribution and controls of dunes, beyond formalizing previously recognized relationships and compiling a global presence dataset?

2. What are the principal take-home messages that readers should retain from the study?

(Remarks on code availability)

Reviewer #3

(Remarks to the Author)
Thank you for your careful and detailed responses to the reviewers' comments. I am satisfied that you have addressed them all and that they improve an already high-quality article.

This is a seminal article and it represents a major advance on previous studies, such as Wilson (1973). It will be widely cited.

(Remarks on code availability)
I did not review the code in detail, as it requires more computing power than I have access to. I did download the instructions and they seem reasonable.

Author response to reviews of NCOMMS-26-017897-T

The distribution of Earth's wind-blown sand dunes

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Document Formatting

In this review response I have pasted the Reviewers' comments in blue and, where relevant, copied text from the original unrevised manuscript in orange and the revised manuscript in purple. My responses are organised directly below each pasted comment, and line numbers refer to the revised manuscript. I also provide line numbers for this review where reference to other responses is useful: these are denotes with an "R" prefix. I include page breaks per Reviewer for clarity. The final page includes self-chosen revisions. I have also provided a 'tracked-changes' version.

Reviewer 1

This is a fine paper and I look forward to seeing it published. It provides a new global dataset on sand dunes and valuable analysis helping explain why they are found where they are. The dataset will find many uses beyond this study. The work fully supports the conclusions and I see no significant flaws in the methodology. My comments are minor:

Thank you for your constructive feedback – I'm glad you like the results!

The use of superlatives is not common in scientific writing and I encourage you to consider removing or re-wording them. Examples: Line 31: exhaustive; Line 63: well-chosen; Line 75: highly relevant

I have removed each of these superlatives.

Line 74: a short sentence explaining saturated flux divergence would be useful here so that readers don't have to go to the methods

I have added the statement "This is the spatial gradient in $\overrightarrow{q_s}$".

Figure 3: legends for AI and flux divergence are hard to read. Is it possible to make them larger so that the colors can be read more easily?

No problem: I have made them 50% thicker and panel a's longer.

Line 104: by convention, u is wind speed and u* is friction speed (with * as subscript). Is there a reason to not follow convention?

I've altered all references to u_t and replaced them with $u_{*,t}$.

Figure 4: the orange and red lines are too similar. Can one be changed to a more obvious color?

I have changed the orange colour to green.

Line 113: Are you equating barchan and parabolic dunes. Are there parabolic dunes in the image? It seems that you are only working with barchans. (I'm likely missing the parabolics, in which case you can ignore this comment.)

No problem: there's both, and both indicate the same flux direction as indicated by the red line.

Line 130: …as these ARE a small…

Thanks, added "are".

Line 157: visually identified? If identification was automated for this first step, it should be explained. OK, I see the refence to Supp 1. It might help to put that earlier in this section.

I believe it's ok to leave the order of explanation as it is here. The first paragraph explains the primary sources and scale, the second explains the basic method, and the third explains the relation to other methods. I reference Supplementary Text 1 in the last two paragraphs. I have added the word "manually" in Line 176 right at the beginning of the first sentence so this is clear.

Line 188: there should be a reference for RDP/DP (or greater explanation). Are you using all wind speeds or only those above threshold?

I have added a classic reference to RDP/DP here and a sentence to the end of the paragraph: "Note that the value expressed here only considers winds above threshold (as per Methods M3)." The fact that equation is defined by $\overrightarrow{Q_s}$ ensures this is only for winds above threshold.

Line 198: Please explain more, preferable in the main text. Divergence from what? (Sorry, this term is new to me.)

I have added the statement in the main text as per Line R28 and a more detailed statement to the end of Methods M6: "Note that saturated flux divergence describes the gradient in saturated flux in space. Through mass conservation, the change in bed elevation through time $\partial h/\partial t$ is expressed as $-\partial Q_s/\partial x$ in 1 dimension, $-\nabla \cdot \overrightarrow{Q_s} = -(\partial Q_{s,x}/\partial x + \partial Q_{s,y}/\partial y)$ in 2 dimensions, and the above equation on the surface of a sphere. Here we use the latter since our domain is defined in latitude ϕ and longitude λ. Negative values indicate deposition."

Supplementary Table 1: Is there a reason that Tsoar's mobility index (https://doi.org/10.1016/j.physa.2005.05.067) is not included? (I don't insist that it be added)

Ideally I would but the reason I didn't include it is because that mobility index doesn't define what is required for dunes to develop when the average rainfall is less than 50 mm/yr. I didn't want to make a choice about whether to apply the same rule for <50 mm/yr cases or not, so I left it out since it can't be applied everywhere like the others.

IF others were involved in the research (visually checking images, coding, etc.) you might consider listing them in the Acknowledgements.

I worked on it alone, but I did get some feedback on the manuscript (along with all yours!) which I have added to the Acknowledgements, thank you for the reminder. It now has an additional statement "I thank three anonymous referees, the editor, and Victoria Milanez Fernandes for their constructive feedback on drafts of this manuscript."

Reviewer 2

The manuscript presents a global compilation of dune presence on a 0.1° grid and uses it to re examine the climatic and sedimentological conditions under which dunes occur. The overall scope is impressive, and the dataset could be a useful community resource. However, several central claims and aspects of the framing need to be moderated and better anchored in the existing literature.

1. Claims about novelty and global mapping

Throughout the manuscript, you imply that this is the first complete and accurate global map of Earth's sand dunes with distinguishable morphologies, and that you are the first to reassess, quantitatively, the conditions necessary for dune formation. This is overstated.

- Wilson (1973) produced a global map of ergs, and several subsequent studies have also presented global or near global maps of aeolian sand seas. While these did not systematically encode dune morphology at your grid scale, they did map the global distribution and extent of major dune fields.
- In addition, the book of Martínez and Psuty (2004) compiled a global map of coastal dunes; many of these coastal dune systems are not visible in your Figure 1, despite your description of the map as "complete and accurate" for dunes with distinguishable morphologies.

If Figure 1 is intended to be the "first exhaustive global map," then it needs to (a) be much more carefully validated against existing global and regional maps, and (b) explicitly acknowledge that it generalizes and sometimes coarsens previous work, rather than replacing it. At present, the map appears too general: for instance, most of the Sahara is shaded as dune covered, in contrast to Wilson's (1973) mapping of 27 discrete ergs that collectively occupy only about 25% of the Sahara. Moreover, dune morphologies per se are not visible in Figure 1; only presence/absence is shown. I suggest you rephrase the novelty claim along the lines of: a global, manually curated 0.1° grid of dune presence, with ancillary information on morphology derived from imagery, rather than "first complete and accurate global map."

I understand your points, thanks for taking the time to write this detailed review. I believe it is the first global map of dunes that is both complete, accurate *and* digitally available to analyse. I want to be clear that does not provide differentiated morphologies (Methods M1), rather, I used 'can I distinguish a recognisable dune in this grid cell?' as a criterion for their presence. I do not make the claim that it is the first quantitative assessment of the conditions for dune formation. Technically anyone could make the claim they are the first to reassess something but I get your point! These older maps (e.g., Wilson, 1973) which, as per the references and Supplementary Text 1, I relied on when developing this data are great, but they are printed – one can't use them in data analysis without, for example, manually tracing them digitally. Put it this way: I don't believe there is a simple way to validate this map against another global map (other than the one noted in Methods M9) because there isn't another one you can measure its overlap with quantitively. As per responses elsewhere this is now clearer in the new paragraph in the final section of the main manuscript and Methods M1.

As stated in Supplementary Text 1, I only consider where the ERA5-Land reanalysis data is available, "i.e., any 0.1°x0.1° grid cell where the area is majority water (except some lakes) and therefore omits many coastal dunes." I am aware there are some coastal dunes not included in the dataset and this is noted. I have added an additional statement directly to the end of the Methods M1 section: "Only squares where ERA5-Land data is present (that is, where the majority of the square is covered by land) were mapped, hence some coastal dunes are not included in this analysis."

This map does not 'coarsen previous work' or over generalise. All the cells in the Sahara that are shaded have dunes in them, however sparse. I recognise that 27 discrete ergs were identified by Wilson (1973) but there are dunes between these ergs. Without any specific reference to a location that is falsely identified (this can be checked using the available dataset) there's not much I can do with this comment. If you are referring to the hatched map in Figure 1 then yes I agree that it is coarsened as stated in the caption: "for clarity, dune locations in **a** are padded by a 100-km radial buffer."

But overall, I am happy to change the claim to suit your comments, I believe I only make the claim in the abstract and it now reads "…there has been no complete and accurate digital map of the occurrence of Earth's sand dunes with distinguishable morphologies. Here we present that map…"

2. Use of AI and saturated flux versus previous indices

You emphasize AI and saturated sand flux magnitude and divergence as the key predictors of dune presence. These are certainly important variables, but they are not new to the aeolian literature:

- Lancaster's (1988) Mobility Index explicitly combines AI and wind speed above the threshold for sand transport. This index has been widely used to distinguish between active and stabilized dunes and to explore climate change impacts on dune fields.
- Wilson (1971, 1973) already highlighted the role of transport convergence and divergence in controlling where ergs form in the Sahara.

In your framework, saturated sand flux magnitude is used as a proxy for wind power. This may be physically appealing, but most aeolian geomorphological studies of dune mobility have historically used DP as wind power potential (DP in Fryberger, 1979).

I agree with all this yes. I would like to note that generally over the last decade authors have moved away from using 'wind power' as a metric for sand transport (e.g., Kok et al., 2012). As you're aware, wind only moves sand (in dry environments) a few percent of the time so including all the non-transporting winds (e.g., those below the threshold required to move sand) in a measure that is supposed to describe sand transport is not ideal. That is why some of these mobility indices (e.g., references 18 & 23, Supplementary Text 2) use the fraction of time wind exceeds threshold as a proxy for transport instead of DP. Granted, 'wind power' or 'drift potential' is often defined as the square or cube of wind speed, but it is still not preferred when it is quite simple and now well established to use an equation like in Methods M3 to find the average saturated sand transport rate more directly. Indeed, Wilson recognised this in his 1971 paper (as did Bagnold in 1941) by including grain diameter in the sand transport equation, then using this metric for calculating drift potential. The words 'potential' and 'saturated' are synonymous. I'm not saying there are no studies including aridity, sand flux, divergence, or supply before on dune presence, but I believe none have combined all four in a global study. I have added a statement at the end of Methods M6 noting Wilson's work on convergence and divergence: "This measure has been defined in 1 dimension at the bedform scale[16], along 1-dimensional paths in 2 dimensions at the dune field scale[46], or in 2 dimensions at the dune field scale[12,25]." I have added the Wilson (1971) reference.

Papers such as Wasson and Hyde (1983) and Yizhaq et al. (2007) (which you cite) emphasize wind drift potential (and vegetation response to DP) as the variable that most directly explains the coexistence of vegetated and unvegetated dunes and the hysteretic response of dune systems to climate. It would strengthen the manuscript if you

- Explicitly discuss the relationship between your saturated flux metric and classical DP, including advantages and limitations.

I don't see any limitations. As you can see from Supplementary Table 1 it performs better. I don't think I need to add much further explicit discussion for three reasons 1) there is plenty of work comparing DP and sand flux, 2) this is now routine analysis (e.g., references 10, 11, 12, 17, 20, 21, 22, 25, 26, 27, 31, and 35), 3) and I already explain this in Supplementary Text 2. I have added a preamble to the Methods M3 text to explain the difference between DP and saturated flux: "While some past studies focus on the so-called 'drift potential' DP as a measure of the wind's potential to transport sand[11], we elect here to use an equation which explicitly predicts dimensional saturated (i.e., potential) sand transport rate (i.e., flux).

This measure is linearly proportional to a common definition[11] of the drift potential (i.e., $DP = |\vec{u}|^2(|\vec{u}| - u_t)$ for $|\vec{u}| > u_t$) near threshold but has been validated against measurements[17] and is also widely used[26]."

- Clarify why saturated flux is a better or more appropriate predictor in your global analysis than DP (or, alternatively, show that they yield qualitatively similar results).

I show in Supplementary Table 1 that they yield qualitatively similar results. If you inspect the equation in Methods M3, you see that when wind speeds are above threshold, potential sand transport (e.g., saturated sand flux) is proportional to the square of wind speed above threshold. The constant of proportionality just changes the magnitude of *some* definitions of DP (e.g., reference 11). I have added a sentence to the end of Methods M3 to explain its appropriateness: "One additional reason we use $\overrightarrow{Q_s}$ instead of DP is to produce a sand transport measure with the correct dimensions (m^2/s rather than m^3/s^3) so that its divergence (Methods M2) provides the correct dimensions for erosion and deposition (m/s)."

Your own result that "neither AI nor saturated flux alone can explain the distribution of dunes" is important, but it needs to be more clearly framed in relation to existing indices. At present, it reads as if AI and flux are being introduced de novo.

The two sentences after the one you've quoted are "Over the last century it has been established[16,18] that dunes prevail when both sediment is available and saturated flux is sufficient. AI correlates well with how vegetation presence[18,19] and other major factors[20] inhibit open, loose sand surface cover, thus making it a reasonable proxy for the inverse of sediment availability." I do not think it is framed 'de novo', especially since the Methods sections M2 and M3 are referenced beforehand.

3. Limitations of AI as a proxy and role of vegetation and biogenic crusts

You argue that AI correlates well with vegetation presence and that it is therefore a reasonable proxy for the inverse of sediment availability. This is broadly true, but there are important exceptions that your framework does not address and should at least acknowledge:

- Active or partly active coastal dunes exist in humid climates with AI > 0.65 (e.g. Oregon coast, north eastern Brazil, Baltic coasts). These dunes draw directly on large beach sand supplies, and their activity is controlled by a combination of wind climate, shoreline dynamics, and local vegetation, not primarily by aridity.
- Many desert dunes in regions with mean annual rainfall around 100–150 mm are stabilized by biogenic crusts, even though AI would classify them as relatively arid and ostensibly suitable for dune mobility (e.g. Kidron et al., 2000; Maman et al., 2011; Kinast et al., 2013). In these cases, biological soil crusts can strongly reduce sand availability without a corresponding signal in AI.

How do such systems fit into your AI–flux framework? At a minimum, the manuscript should:

- Explicitly acknowledge that AI is an imperfect proxy for sediment availability, especially in coastal dune systems and crust stabilized deserts.
- Discuss whether these types of dunes are under represented or misclassified in your global dataset, and what that implies for the generality of your AI based inequality.

These are reasonable points. The two cases you bring up are exactly why I included saturated (potential) sand flux and sediment supply (in the case of Africa) in the analysis: aridity alone can't explain dune presence. The inequality shown in Fig. 2k, and other mobility indices (Supplementary Table 2), say that dunes can be present in temperate locations if there is a lot of sand transport. I do not include the effect of shoreline dynamics or vegetation directly. I have changed the sentence beginning on Line 55 to read "AI correlates well with how vegetation presence[18,19] and other major factors[20] inhibit open, loose sand surface cover, thus making it a reasonable but imperfect proxy for the inverse of sediment availability." In Extended Figure 1a you can see where the inequality misclassifies dunes and I have added a sentence to the end of Methods M5 noting your comments: "This inequality does not include the important roles of sediment supply, flux divergence, or sediment availability (through numerous factors such as biocrusts, shoreline dynamics, etc.; Methods M2)." I have also added a more expansive section in the Methods M2 section "Our use of this index as a proxy for sediment availability follows past studies[9,18,23] (Supplementary Text 2), however it should be noted that it is not a perfect measure. The sensitivity of sand availability to vegetation, biogenic crusts, and other factors which vary with aridity is dependent on those specific factors (e.g., species). In lieu of detailed local analyses of those factors and their aridity-dependence (including hysteretic properties[43]) in each dune field, the Aridity Index is the most useful, albeit imperfect, globally available sediment availability proxy." I have added the Kinast et al (2013) reference.

4. Interpretation of Fig. 1 and global coverage

The title of Figure 1 ("Distribution of Earth's wind blown sand dunes with discernible morphologies") suggests both accurate spatial coverage and explicit morphology mapping. In practice:

- The Sahara is shown as almost entirely dune covered, which conflicts with Wilson's (1973) more detailed mapping of discrete ergs occupying a minority of the Sahara's area.
- Dune morphologies are not actually visible in the figure; the map is essentially presence/absence at 0.1° resolution.
- Many coastal dune systems shown in Martínez & Psuty (2004) do not appear clearly on your map.

You should either refine the figure and the underlying mapping or moderate the wording to reflect that this is a coarse resolution global presence map, not a detailed morphological atlas.

As stated above on Line R123 and in the Figure 1 caption this map is presented with a buffer around the cells to help with visual clarity. You can see in Figure 3a what the actual unbuffered data looks like for the Sahara but many small regions are illegible in Figure 1 on a global scale without the buffer. I have addressed most of these comments repeated from your section 1. Perhaps this is a linguistic issue: I'm not discerning their morphologies I'm using their morphologies to discern if they are dunes. As stated in multiple other places elsewhere in the manuscript: this is just a map indicating presence, not morphology. To make this very explicit, I have now added a statement to the end of the Methods M1: "Note this map does not specify the morphology of dunes, just their presence."

5. Conditions for dune formation and missing controls

You convincingly show that AI and saturated flux alone cannot explain global dune distribution (line ~48). This is, in fact, a key conclusion, but it needs more thorough exploration. Several additional factors may be critical:

- Topographic and sedimentary traps (structural or subsidence related depressions) that accumulate thick fluvial and lacustrine deposits during humid phases (e.g. in the Sahara and Sahel; see Armstrong et al., 2023) can create large sediment reservoirs that persist into arid phases.
- Human activities (e.g. vegetation removal, grazing, agriculture) can destabilize dune fields in relatively wet climates or, conversely, stabilize dunes in dry climates via land use change and restoration.

It would be clearer if you explicitly acknowledged:

- The importance of antecedent sediment supply and geomorphic setting (e.g. basins filling with fluvial/lacustrine sediment during humid periods, later reworked as dunes).

- That your present day climate and flux fields will not fully capture the formative climates of relict dunes or those strongly modified by human activity.

I like these comments a lot and agree that they should've been developed in the first draft, thank you. To the first paragraph in the discussion, I have added the sentence "Indeed, we anticipate that this approach could help deconvolve the direct role of aridity on aeolian activity via sediment availability from its indirect, lagged role in increased fluvially-supplied sediment during antecedent humid hiatuses (e.g., within Milankovitch cycles)[34]." I have added the Armstrong et al (2023) reference. In the second paragraph I have added the clause "—and potentially attributing deviations to land-use change" on Line 161. I believe your last point is already covered by the first paragraph's statement: "This introduces bias—limited as these are a small fraction of the dataset—to some results because, e.g., present-day aridity and flux divergence do not correspond to their formative conditions."

6. Treatment of Antarctica and Greenland

You note the "near absence of sand dunes on glacial, windy Antarctica and Greenland" and use this to argue that dunes are most prevalent at moderate, not high, saturated flux. However, in these regions the absence of dunes is primarily due to ice cover and the lack of sand sources, not the wind regime itself. Recent work has even documented snow dunes and megadunes (e.g. Traversa et al., 2023; Poizat et al., 2024), highlighting that given a granular medium, dune like forms can indeed develop. This should be mentioned to avoid conflating high flux with lack of dunes where there is simply no sand.

I agree with this. The sentence is explicitly about **sand** dunes and sand availability, drawing a contrast between environments where sand is (shorelines) and isn't (glaciers) available. I believe like you that it is very relevant to explain that Greenland and Antarctica have the highest saturated fluxes/drift potential on Earth but don't have sand dunes because of ice cover. Perhaps I wasn't clear enough. I have rephrased this sentence on Line 74 to be "This can be explained by the near absence of exposed sand on glacial, windy Antarctica and Greenland for sand dune development, whereas the prevalence at extreme fluxes occurs mostly where consistent, fast onshore winds over beach sands produce transgressive dunes[24]."

7. Resolution and the 0.1° grid

Your analysis is conducted on a 0.1° (~9–11 km) grid. At this resolution:

- Small dune fields and narrow coastal dune belts will be missed or diluted within mixed land cover cells.
- Detailed dune morphology is not resolvable, which limits the strength of some of the morphological claims.

You should more clearly justify that this resolution is adequate for the conclusions drawn about global distribution and controls, and explicitly state the scale limitations (e.g. that small coastal systems and narrow linear dunes are under represented).

As explained in Methods M1 I identify dune presence as "A grid square is classified as true for dune presence if part, all, or many dunes are contained within it. Dunes were identified at 1:30,000 scale (nominal), however a smaller scale was used to verify potential dune candidates found at this scale." I believe it's quite explicit that therefore small dune fields and narrow coastal belts (if that grid cell is majority land) will be marked 'True', and not only if it is completely covered (as you may have interpreted it). I do list omissions/limitations in Supplementary Text 1. To make it very clear, I have added a statement about coastal grid cells to Methods M1: "Only squares where ERA5-Land data is present (that is, where the majority of the square is covered by land) were mapped, hence some coastal dunes are not included in this analysis."

8. Clarifications on figures, notation, and methods

Figures 3 and 4: please define "CDF" (presumably cumulative distribution function) clearly in the caption or text.

I did write "cumulative distribution function" in both those captions but have added "(CDF)" in each caption for clarity.

Figure 4: the world map is very small and visually dense; it is difficult to distinguish spatial patterns. Consider enlarging the map, adding regional insets, or simplifying the colour scale.

I have changed the colour scale to grey-scale to ease the eye. I agree it is small but as stated in the text (Line 128-133) this map is not attempting to illustrate specific regions, rather that broadly there are some locations where the direction range is large and that it is mostly small, but that it is heterogenous. As an aside (for a more detailed study in the future), the spatial patterns here are very complex but I believe they are mostly where persistent wind climate features compete (either due to surface properties or seasons) which makes them quite interesting to interpret changing climate regimes over time (see reference 12).

Line ~84: "Sahul", this term usually refers to the combined Australia–New Guinea continental shelf. If you mean the Sahel belt, please correct the terminology.

Thank you for picking this up! It is a much more common word for me to write as I live here. I have corrected this typo.

Lines 129–138: you discuss relict dunes and acknowledge a gradient of activity states. It would be useful to explicitly note that dunes in populated semi arid and coastal zones may be strongly affected by human land use, so that present day AI and flux may not reflect formative conditions.

No problem. On Line 149 I have included "or anthropogenic restoration[8]."

Equation for volumetric saturated sand flux Q (around line 179): please provide a clear derivation and/or exact reference (e.g. Martin & Kok, 2017, building on Bagnold, 1941) and briefly explain the assumptions (threshold, grain size, etc.).

I have provided the exact reference (reference 13, Martin & Kok, 2017), defined the parameters and the constant values I used in Methods M3. For clarity I have adjusted the sentence "These assumed values are common global approximations in lieu of more accurate local values[26]." I have also provided a clearer introduction to this section with reference to its relation to Drift Potential (see point 3 in Section 2 of your review), and additionally I have rewritten the equation slightly to define the friction velocity outside the main equation, so the resemblance to the Drift Potential equation and other flux laws is clearer: "where $\vec{u_*} = \vec{u_z}\kappa/ln(z/z_0)$ is the friction velocity…" Note though that this equation has not changed, I've just moved some like terms away from the equation into its explanation.

9. Conclusions of key messages

After reading the manuscript, two questions remain insufficiently answered:

1. What are the genuinely new insights that this study provides about global dune distribution and controls, beyond formalizing known relationships and assembling a global presence dataset?
2. What are the main take home conclusions you want readers to retain?

Given the ambition and length of the paper, the Conclusions section should be strengthened to:

- Clearly list the principal new findings (e.g. specific quantitative thresholds, the relative importance of ∇·q versus AI in certain regimes, new constraints on where dune migration direction is sensitive/insensitive to sediment properties).
- Explicitly link these findings to future applications (e.g. paleoclimate inference, climate change projections, planetary analogues).

I accept that the ‘Conclusion’ is not typical for a long-format paper but this is a short format paper where the style for the journal is to weave discussion and results together in the main text. I tried to weave these points into the original paragraphs but it clearly has not worked. I believe I (and the other reviewers) are satisfied by how the section satisfies each of the points, especially the last one. As for ‘genuinely new insights’, I believe each of the main text figures encapsulate a ‘genuinely new’ result (including the two you mention; Figs. 2k & 1a, respectively). I have added a new paragraph at the start of the final section of the manuscript to address your points which reads:
“This study provides the first complete digital map of aeolian sand dune presence (terrestrial at 0.1-° resolution; Fig. 1A, Methods M1). With this map we provide a thorough quantitative analysis of how the four (sediment supply, sediment availability, saturated flux, and sand convergence) major factors relevant to their formation combine to give rise to aeolian sand dunes (e.g., Fig. 3). In doing so we identify the most accurate ‘Mobility Index’ to date (Fig. 2k, Extended Data Figs. 1 & 4, Supplementary Table 1): $\log_{10} AI < 10^{-11/5}|\vec{q_s}| - 4/5$. Finally, we show that it is possible to infer geologic boundary conditions (e.g., grain size) from dune orientation if wind climate is known (Fig. 4b) and use this map to demonstrate where on Earth that approach is best suited for unidirectional dunes (Fig. 4c).”

References cited in this review

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Remarks on code availability

1. Manuscript background information
The author claims to present the first exhaustive global map of Earth’s sand dunes with recognizable morphology. The study further argues that dune presence is primarily governed by two factors: (i) sand availability, parameterized by the Aridity Index (AI), and (ii) the magnitude and divergence of saturated sand flux. The manuscript is ambitious in scope, but the conceptual framework and many of the key variables build strongly on previous work and are, in my view, not especially original.

This is repeated from points above – see my responses there. Thank you for your thorough review it has definitely improved the manuscript.

Reviewer 3

This is an interesting and significant article that makes good use of global data sets to map the location of inland and coastal sand dune areas, providing the first truly global map of dune area locations for Earth. It does not (and does not pretend to be) a high resolution map of dunefields – such as the work in progress (Hesse,and Lancaster,2026)

Thank you! I look forward to that vector-based map. (Editor: please recall this reviewer's claim when considering reviewer 2's point 1).

The spatial distribution of the identified dune areas is then compared to data sets on aridity, wind field, and calculated saturated sand flux to identify key boundary conditions for dune occurrence.

The data analysis provides new insights into key boundary conditions, especially saturated sand flux and aridity index. Spatial variation in flux divergence is also identified as a key boundary condition for accumulation of sand. It would be interesting to examine the role of flux directional variability on dune occurrence.

I definitely agree on this last sentence but feel a thorough examination is beyond the scope of the present manuscript.

Extended data are valuable. Extended Fig 3 could be updated with current data set (7 k ages)

I do not know where to access the 'current' INQUA dataset – the website no longer contains a link for it. The version I used has 4,449 entries. I don't think it would change the meaning of this manuscript but if you review this again and you can direct me to a publicly accessible version, I would be happy to include it in a revision.

Weaknesses

This is a very densely written manuscript that introduces many important concepts. Sometimes it is hard to follow the discussion. Careful attention to phrasing is needed.

I hope the extensive edits below and those from the other reviewers satisfy this concern.

The figures could be improved especially the use of different color palette to clarify inferences being made. Fig 4 c is hard to understand. Because of the use of very similar colors for different variables.

I have changed the colour scale to grey-scale and changed the orange to green – I think hope it's a lot easier to read now.

The section " Boundary condition inversion from morphology " in which the sensitivity barchan dune orientation and migration direction to transport threshold and therefore sand particle size is evaluated is less convincing compared to other sections. I do not think it adds anything to this article.

I believe it's a nice result which demonstrates how dune orientation can be used to infer grain size, and (with the map in hand) how possible that is on a global scale. Perhaps you find it less convincing but personally it's my favourite part. What it adds to the article is a demonstration that dune morphology can "be a proxy for geologic boundary conditions," thereby constraining some of the free parameters that are required to fully describe dune presence. I have added a new paragraph at the beginning of the final section which states this clearly: "Finally, we show that it is possible to infer geologic boundary conditions (e.g., grain size) from dune orientation if wind climate is known (Fig. 4b) and use this map to demonstrate where on Earth that approach is best suited for unidirectional dunes (Fig. 4c)."

Line comments

16 – "environments of friable sandstones" - is misleading as most dune areas are not directly associated with sandstone

Fair enough. I have replaced "sandstone" with "sedimentary rocks."

23 - promote the formation of aeolian dunes

I have replaced "necessitate" with "promote the formation of."

Fig 3 a – areas mapped as "aeolian sediments" are in fact sand sheets (or paleodunes in the Kalahari region) and may or may not be sources for adjacent dunes

This may be the case but that is the terminology used in the source, and I think it's reasonable to use that as an estimate for where active or relict sand dunes could have been sourced from during their formation. I have added a statement in Methods M7 to address this "Note that we rely on the expert opinion used developing this map and do not edit it where there could be debate on whether they are truly potential aeolian dune sources."

Reference

- Hesse,, P. and Lancaster, N. 2026. The Distribution of Sand Dunes on Earth. 11th IAG International Conference on Geomorphology, Christchurch, NZ< February 2026. (Poster).

Remarks on code availability

I just looked at the data sets available. Cautions on running the code are appreciated.

Thanks for that and your review.

Self-revisions

There was a missing word ("in") in the caption of Figure 1, this has been added.

The caption title of Extended Data Figure 2 incorrectly stated the map was of sand flux "percentiles" not "magnitude." The panel caption was correct, but I have rectified the caption title.

I have added 9 more pixels to the dune map (e.g., 9 of 106,540) which I missed originally. This is 'Sand Mountain' in Nevada, USA. All the figures have been updated and the reference to this total number in and it has no meaningful impact on the results.

All data and code are now available in online repositories with DOIs, with their respective availability statements edited.

The editor and reviewers have been added to an updated Acknowledgements section.

Author response to reviews of NCOMMS-26-017897A

The distribution of Earth's wind-blown sand dunes

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Document Formatting

In this review response I have pasted the Reviewers' comments in blue and, where relevant, copied text from the revised manuscript in purple. My responses are organised directly below each pasted comment, and line numbers refer to the revised manuscript. I include page breaks per Reviewer for clarity.

Reviewer 1

All of my concerns have been addressed satisfactorily. Thank you. I have no additional suggestions.

Thank you for your constructive feedback in the first revision.

Reviewer 2

I recommend that the paper conclude with a dedicated concluding section addressing two key questions:
1. What genuinely new insights does this study provide regarding the global distribution and controls of dunes, beyond formalizing previously recognized relationships and compiling a global presence dataset?
2. What are the principal take-home messages that readers should retain from the study?

Thank you for your thorough which substantially improved the manuscript. This comment is almost identical to one provided in the first review (see *"Author response to reviews of NCOMMS-26-017897-T"* Lines 321-326), in response to which I added an entirely new paragraph at the beginning of the final section. The difference between that first review and this comment is "I recommend that the paper conclude with a dedicated concluding section." Since there are no comments or criticisms provided for that introduced paragraph and the other two reviewers are completely satisfied, I believe moving the introduced paragraph from the beginning of the section **'Implications for geomorphology and sedimentology'** to a new final section **'Conclusion'** will satisfy this concern. I have created that final concluding section and paragraph on Lines 170-178.

Reviewer 3

Thank you for your careful and detailed responses to the reviewers' comments. I am satisfied that you have addressed them all and that they improve an already high-quality article. This is a seminal article and it represents a major advance on previous studies, such as Wilson (1973). It will be widely cited.

I did not review the code in detail, as it requires more computing power than I have access to. I did download the instructions and they seem reasonable.

Thank you for your review and kind words.