

Author's response

The comments and changes made by the authors are in blue text. The line numbers correspond to the file with changes, making them easy to identify.

RC1:

This is an interesting and relevant study, however, several areas require clarification to improve robustness and reproducibility. More detail is needed on the processing and analysis of aerial imagery (e.g. data sources, spatial resolution, and dune delineation). The manuscript would also benefit from the use of subsection headings to improve structure and readability. Finally, some key concepts and interpretations (e.g. model transferability and the distinction between wind intensity and Resultant Drift Potential) would benefit from clearer definition and supporting evidence.

L21: There is not enough evidence within the article to support the claim that the model is “transferable to other aeolian systems,” particularly as the manuscript concludes by suggesting that future research should test model performance in “diverse climatic and geomorphic settings.” Model transferability typically requires explicit testing, and this statement may therefore be overstated.

We changed “is” by “may be” because is correct that more tests are needed to support the statement as was. Additionally, we remove the term “worldwide” to better set the objectives (line 69). In this case, the model was tested in one environment and the model performed well. One example was taken and set the basis to use these models within other environments.

L36: A brief explanation of binomial logistic regression would be helpful, as this methodology underpins much of the analysis.

A brief explanation was added (lines 38-40).

L184 and throughout: The use of subsection headings is recommended to improve readability and overall structure.

We divided the sections methods and results into climate analysis, remote sensing and dunes migration, and statistical model. To maintain the order, “Remote sensing and dune migration” was moved after “Statistical model” in methods section.

L187: Please refer to Figure 1, where the meteorological stations are mapped.

Reference to Figure 1 was added (line 196).

Methods: Further detail is required regarding the processing and analysis of aerial imagery. For example:

- **L238:** Please clarify which datasets correspond to satellite imagery and what is included under “aerial” data.

We specified that the images obtained from Google Earth are satellite images. Then, the only aerial image is the one from 1986 (lines 328-332).

- **L239:** How was the spatial resolution of the Google Earth imagery defined or estimated?

It was measured in ArcGIS and double checked in QGIS. Information added in lines 332-333.

- **L238:** Was migration assessed only for unvegetated dunes? How were dune areas delineated? Further detail would improve clarity and reproducibility.

Information was added regarding this comment and the following one for clarifying the procedure (lines 336-342).

- **L245:** How was a “dune front” defined? An illustrative figure showing how dune fronts were identified and how migration rates were calculated would greatly assist the reader in understanding and reproducing the methodology.

Dune front was defined where the clearly bare sand area finish at the bottom of the slipface and the vegetated area begins (lines 336-342). Two additional references were added (Yao et al., 2007; Ding et al., 2020).

L309: Please provide a reference for these “established” procedures.

Reference and summary of the procedures were added (lines 351-352).

L356 to Table 2: A brief reintroduction of TsoLa would be helpful here, as it was last defined in L237.

An explanation to refresh the TsoLa index values was added (lines 395-397).

Figures 9 and 10: Please clarify what values of 0 and 1 represent in the classification, particularly as the term “classification” is not clearly introduced in the Methods.

A brief explanation was included in lines 442-443 and in figures caption. The possible outcomes (0 or 1) were added in the methods section (lines 249-251).

L458: No results relating to vegetation change or landscape “greening” are presented. Additional evidence or justification would be helpful to support this statement.

The percentage of vegetation covered in 1961 compared to 2021 and the reference to Toffani et al., 2024 were added (lines 523-525).

L462: A fuller description of the “worm-like” dunes is needed. An illustrative figure would also help the reader interpret and apply this concept to other sites.

More detailed description about worm-like dunes (line 532) was added.

L462–L464: Sand supply does not appear to have been included in the analysis. Additional evidence or justification would be helpful to support this interpretation.

For this case we did not include sand supply because one of the objectives is getting a model easy to apply with standard meteorological data, accessible without much extra calculations, thinking it as a tool to get quick estimations regarding dune migration from standard meteorological observation. There is a reference to this variable in lines 568-570.

L463–L464: The distinction between “wind intensity” and Resultant Drift Potential (RDP) would benefit from further clarification. As RDP inherently incorporates wind speed (i.e. intensity) alongside directional variability, treating “wind intensity” as a separate control introduces some ambiguity. It would be helpful to clarify whether this refers to aspects of the wind regime not captured by RDP or reflects overlap between these terms.

This was clarified in the sentence (line 535).

L466–L468: It may be helpful to frame this discussion in the context of Haim Tsoar (2005), who describes physical–biological interactions in dune systems using the concept of hysteresis, whereby dunes become stabilised when wind power is sufficiently low.

We add a few lines referring this concept in relation to what happen in the study area (lines 527-530).

Tsoar, H. (2005). Sand dunes mobility and stability in relation to climate. *Physica A: Statistical Mechanics and its Applications*, 357(1), 50-56.

Citation: <https://doi.org/10.5194/egusphere-2026-377-RC1>

Ding, C., Zhang, L., Liao, M., Feng, G., Dong, J., Ao, M., Yu, Y.: Quantifying the spatio-temporal patterns of dune migration near Minqin Oasis in northwestern China with time series of Landsat-8 and Sentinel-2 observations, *Remote Sens. Environ.*, 236, 111498, <https://doi.org/10.1016/j.rse.2019.111498>, 2020.

Yao, Z. Y., Wang, T., Han, Z. W., Zhang, W. M., Zhao, A. G.: Migration of sand dunes on the northern Alxa Plateau, Inner Mongolia, China, *J. Arid Environ.*, 70, 80-93, <https://doi.org/10.1016/j.jaridenv.2006.12.012>, 2007.

RC 2

The paper presents a study that combines two well established dune mobility indexes to create a new one (TsoLa), and relates this to dune mobility in Patagonia. The paper is well written and addresses the predictability of dune mobility which is an important research gap, particularly in face of climate change.

In my view it remains unclear what limitations of the existing Tsoar and Lancaster indices this new formulation is intended to address, and what additional insight it provides beyond these established approaches. Tsoar and Lancaster's indexes are based on similar parameters included in the model proposed here. A more explicit justification is needed, ideally supported by a direct comparison between the existing indices and the proposed one, to demonstrate whether and how the new index improves the assessment of dune mobility.

The aim is not proposing a different index from the previous, it is combine them into one because this combination reinforces the characteristic of dune mobility, counting them as active only when both indices indicate they are active. Besides, the statistical model performs quite well with this combination. A more detailed explanation in the manuscript was provided (lines 248-251).

A few other specific comments:

Figure 1 – the wind roses suggest that NW and S/SW winds are dominant, but the RDD indicates net dune migration towards the E. There seems to be a lack of W in the wind roses to reflect an easterly net direction of dune migration. A legend showing which speed classes are represented by the colours could be useful here.

A legend showing speed classes was added to the figure. The higher wind speed from SW winds than NW winds or the lack of NE winds may be the cause for this.

Table 1 – did the authors consider veg and non veg parts in the calculation of the dunefield dimensions?

Yes, it was addressed in the Table 1 caption.

Line 316 – Only 30% of the winds exceeded the threshold, but Figure 5 seems to suggest that winds above ~6m/s are quite frequent in the record. This needs clarification, or perhaps change the colours in the wind roses as changes in shades of blue are too subtle and hard to see. More information about the wind data would be helpful – e.g. was the met station data provided classified in any way?

The figure was changed for a new one with different colors. Extra information about meteorological stations data was provided (lines 209-210). A new figure with all winds from both meteorological stations was included to clarify this issue (-new- Fig. 4).

Figure 6 – The very high values in the early 1990s seem a bit inconsistent with the rest of the record. Sometimes older datasets can have quality or methodological issues, have the authors checked whether these values are real, or could they be artefacts? A bit more detail on data quality and consistency over time would help here.

We noted this, and we doble checked the raw data trying to observe inconsistencies or anomalous values. However, the only remarkable thing is the higher number of records of more intense winds for the years 1991, 1995, and 1996. Besides the time when each measure

was taken, there is no more information about them. The data previous to 1991 was only took three times a day, therefore, comparing those years with the selected time period do not provide very accurate information. No more data was provided.

Figure 8 – The meaning of this figure is a bit unclear, could the authors clarify what the axes represent? A more intuitive explanation in the caption or text would really help the reader.

More detailed information in the text (lines 428-433) and in Figure 8 (now Fig. 9) was included.

Similarly, Figures 9 and 10 need clearer legends, as it is currently difficult to interpret what is being shown

More detailed information for captions for Figures 9-10 (now Figs. 10-11) was provided.

Line 454 – DPs are only a function of wind and grain size, they do not reflect longer daylight hours etc.

This is correct. It is only directly attached to stronger winds. The sentence was changed accordingly (lines 518-521).

Line 429 – the authors claim that there is a progressive greening of the landscape associated with a decline in dune migration rates, but no data is offered to verify this.

This information was referred to a previous study for the same area (Toffani et al., 2024) and the percentages of vegetation covered surface were added (lines 523-525).

Line 489 – could the authors clarify how the index was able to particularly identify stable dune conditions if the dunefields have been active/migrating over the years? Figure 11 suggests that the dunefields have been migrating over time, although at decreasing rates. It's not really clear what extra insight the new index is adding here—would we see a similar pattern just using RDPs? Could the index be used to predict what will happen in the long term, e.g. with changes in the SAM? After reading the introduction, I thought that was the direction that the paper was going to take. Similarly, a more complete discussion about the applicability of this index in other areas of the world would be useful as this was one of the study's objectives.

The new index and model aim to predict whether dunes will or will not migrate under certain climatic parameters, which were addressed in Equations 6 and 7. We consider the selected index is better than just the RDP because it defines the boundary between fixed or active dunes according to two proved indexes. With certain chosen precipitation, wind speed, and SAM values, the model will provide a forecast regarding dune movement or not with accuracy of > 73 %. According to the results, there is a higher percentage of true negatives, that is why we mention that the model performs well to identify stable dune conditions. This was included in the referred sentence (line 560). In this paper, the efficacy of the combination of the two indices (TsoLa) was tested in a control case. Testing it in other regions exceeds the objective of the work.

Does the TsoLa index define a threshold above which dunes become mobile (such as in Fig 7)? And does it have similar limitations to existing indices (e.g. only working when rainfall is above ~50 mm)

To elaborate the model, we considered mobile dunes when both indices (Tsoar and Lancaster) indicate they were active. According to the model, a new threshold can be defined based on wind speed, precipitation, and the SAM values. The limitations are similar to the existing

indices. The complete equations (Equation 6 and Equation 7) to replace variables with actual data parameters were included in the results (lines 404-413).

Table 3 - legend must include that dunefield migration are rates in m/year. What are “Dune 1 to 5” and “Dune 1 to 6”?

The unit was added and "Dune 1 to 5" and "Dune 1 to 6" are the different dune fronts measured to estimate dune migration that are shown in Fig. 11 (new Fig. 12), which was also added to the table caption.