The manuscript addresses an interesting and timely question: whether recent changes in Mongolian cyclone activity can explain spatially divergent dust variability across northwest China. However, I do not think the central conclusion is currently supported by the analysis. My main concern is the **Taklimakan–Alxa comparison**. The manuscript uses these two regions to diagnose contrasting responses to Mongolian cyclone forcing, yet the Taklimakan is described as topographically sheltered and only weakly influenced by Mongolian cyclones. However, synoptic-scale cyclone forcing may interact with the basin topography and generate mesoscale and local wind systems in the Tarim Basin. While the Mongolia–Gobi source region, which is much more directly linked to Mongolian cyclone activity, is not included. A second concern is the **attribution of cyclone change**. The manuscript largely infers cyclone intensification and southward displacement from relative vorticity, and then interprets concurrent changes in wind, dust and land-surface variables as a causal pathway. However, there is no objective cyclone tracking or cyclone-conditioned dust analysis. A third concern is the attribution framework. The manuscript interprets concurrent changes in Mongolian cyclones, surface wind, dust, soil moisture and vegetation as evidence of a causal pathway, but the analysis mainly demonstrates statistical associations and synchronized trends. It does not directly quantify how cyclone activity modifies these environmental variables, nor does it test cyclone-conditioned dust responses. Addressing these issues would require substantial re-framing and additional analysis beyond a normal major revision. I therefore recommend rejection.

Major comments

1. The Taklimakan–Alxa comparison does not isolate a Mongolian-cyclone effect.
Relevant text: pp. 2–3, lines 50–80

The manuscript itself argues that the Taklimakan is topographically sheltered and less directly affected by Mongolian cyclones, whereas Alxa is more exposed to northwesterly synoptic flow. Observing opposite dust trends in two regions governed by fundamentally different circulation regimes does not by itself demonstrate a spatially divergent response to Mongolian cyclone forcing. The Taklimakan also contains strong mesoscale (Aoki et al., 2005), thermally driven winds (e.g., low-level jets and haboobs, Chen et al., 2026) and topographically mediated winds, and the northern/eastern basin margins should not be represented by a single prevailing northeasterly regime. Also, the monitoring stations are concentrated along the northern rim of the basin rather than the northeastern edge. Importantly, the Mongolia–Gobi source region, which is more directly linked to Mongolian cyclone activity, is not included in the comparison. The study therefore needs a stronger justification for the regional framework and should distinguish direct cyclone influence from topographically mediated and local circulation effects.

2. Mongolian cyclone activity is not objectively defined or tracked.
Relevant text: p. 1, lines 15–25; pp. 6–7, lines 165–200; p. 12, lines 385–410.

The manuscript infers cyclone intensification and a southward shift mainly from sea-level-pressure and relative-vorticity anomalies. A positive relative-vorticity anomaly is not itself equivalent to an objectively identified Mongolian cyclone, and the paper does not clearly explain how cyclone centres, tracks, frequency and intensity are detected. This is especially important because published cyclone-tracking studies may report different spatial tendencies.

3. Coincident post-2019 changes are treated as causal attribution.
Relevant text: pp. 6–8, lines 165–225; p. 12, lines 390–415.

A central argument is that cyclone intensity/latitude, near-surface wind and dust all changed around 2019–2021. Synchronised changes are not sufficient to establish that cyclone changes caused the dust reversal, particularly over such a short post-break period. Also some statistics are needed to clarify the reverse.

4. ERA5-based long-term surface-wind trends require independent validation.
Relevant text: pp. 3–5, lines 100–155

The main mechanism depends on relatively small long-term changes in near-surface wind derived from ERA5. Reanalysis is useful for circulation diagnostics, but surface-wind trends can be sensitive to models. The claimed wind recovery should therefore be independently evaluated using meteorological-station observations.

5. Pooled station-year correlations may mix spatial and temporal covariance.
Relevant text: p. 5, lines 125–155; pp. 9–10, lines 280–330.

The manuscript pools 26 stations over 10 years (n = 260 station-year pairs) for correlation and segmented-regression analyses. This can confound spatial differences among stations with interannual variability within each station. A positive relationship may therefore partly reflect that dustier locations are also windier, rather than that year-to-year wind anomalies drive year-to-year dust anomalies.

6. Dust metrics are not equivalent and their disagreement needs more careful treatment.
Relevant text: pp. 3–4, lines 90–120; pp. 7–8, lines 205–245.

The manuscript combines PM10, PM10-defined dust-storm days, MERRA-2 DUCMASS and visibility-based dust-storm records. These metrics represent different aspects of aerosol loading, emission and transport and can respond differently to observing practice and background pollution. Over the Taklimakan, the metrics even show contrasting behaviour. Interpreting this disagreement specifically as 'more frequent but weaker dust events' is plausible but not uniquely demonstrated.

7. The future PM10 projection is insufficiently documented and over-interpreted.
Relevant text: p. 4, lines 120–125; p. 12, lines 390–420.

The manuscript projects very large PM10 increases through 2100 using three CMIP6 models, but the workflow linking CMIP6 climate variables to PM10 is not described in sufficient detail. It is unclear how model bias, spatial-scale mismatch, future NDVI/HAI, PGI, soil moisture and cyclone activity are treated or validated. In particular, the claim that future PM10 is primarily controlled by climate-driven strengthening of Mongolian cyclone activity requires explicit diagnosis of future cyclone behaviour. Without this, the future attribution and the very large projected increases are not sufficiently supported.

8. Spatial heterogeneity within the Taklimakan is under-resolved.
Relevant text: pp. 7–8, lines 210–225.

Again, treating the entire Tarim Basin as one homogeneous source region may obscure substantial differences among the northern, eastern and southern basin margins, where dust activation and wind regimes differ. And I did not see any maps reporting this spatial distribution.

9. Causal language throughout the manuscript is stronger than the evidence supports.
Relevant text: p. 1, lines 15–25; p. 13, lines 420–425.

The manuscript frequently moves from association or predictive importance to causal statements, including claims that Mongolian cyclone intensification 'reshapes' dust activity, that wind is a direct reflection of cyclone forcing, and that large-scale circulation outweighs land-surface controls. The analyses presented are primarily observational and statistical. Unless stronger causal tests are added, the Abstract, Results, Discussion and Conclusions should consistently distinguish association, prediction and physical causation.

Other comments:

Page 1, Lines 12–14: Please specify what is meant by "recent years." Dust activity has generally been reported to decrease over a longer period, so the recent increase may partly reflect interannual or decadal variability.

Page 1, Lines 15–16: The previous sentence mentions both land-surface changes and circulation changes, but here the knowledge gap refers only to circulation changes. Please clarify the role of land-surface changes in the identified gap.

Page 1, Lines 16–18: The Taklimakan Desert is relatively weakly affected by Mongolian cyclones, as the authors also acknowledge later in the manuscript. Therefore, the rationale for investigating Mongolian cyclone impacts on the Taklimakan should be clarified.

Page 1, Lines 18–20: Please double-check the reported southward shift after 2019. Mu and Fiedler (2025) appear to report a different, or northward, tendency for Mongolian cyclone activity.

Page 2, Lines 38–43: I do not fully understand why the increasing contribution of Mongolia as a dust source is introduced here, as this study mainly investigates dust activity within China. Please clarify its relevance to the study objective.

Page 2, Lines 45–48: The preceding sentences describe different mechanisms, but it is unclear how they lead directly to the stated central question about Mongolian cyclone control. The logical connection could be strengthened.

Page 2, Lines 49–50: I am not sure that land-surface properties and Mongolian cyclone activity should be framed together in this way.

Page 2, Lines 50–52: The manuscript later examines the effects of wind, precipitation, soil moisture and vegetation on dust activity, rather than explicitly quantifying how Mongolian cyclones modulate each of these variables. Please revise this statement accordingly.

Page 2, Lines 55–58: I do not think an increase in dust storms and a greening trend necessarily constitute a direct contradiction, as they describe different processes. In addition, much of the post-2021 dust resurgence in northern China might originate from the broad Gobi region rather than the Taklimakan. Please clarify how this motivates the focus on Mongolian cyclones.

Page 3, Lines 65–69: The first part of this paragraph discusses environmental controls on dust, whereas the final sentence shifts to the lack of long-term studies versus individual dust-storm events. I do not see a clear logical connection between these points; they may need to be separated or better linked.

Page 3, Line 70: "Building on this foundation" is unclear. Does "this foundation" refer to the divergent dust behaviour between the Taklimakan and Alxa regions? Please specify.

Page 3, Lines 70–72: Why is Mu Us Sandy Land not included here, given that it was introduced earlier as one of the study regions? Also, please clarify why the Mongolian Plateau itself is not considered.

Page 3, Lines 71–73: The second hypothesis is presented as a hypothesis of this study but is immediately supported by a citation to another study, which is confusing. I think the intended comparison may instead be between the Tarim Basin interior and regions outside the basin; please clarify.

Page 3, Lines 73–74: It is not clear how the authors establish that there is "significant controversy" regarding the primary driving mechanisms. There is certainly debate regarding dust trends, but the basic physical controls on dust emission might be relatively well established. Please clarify what specific controversy is meant.

Page 3, Line 84: Please check the letter/article formatting here.

Page 3, Lines 85–89: Four dust-storm centres are mentioned, but the study subsequently focuses on only two major source regions. Please clarify the relationship between these four centres and the regions actually analysed.

Page 6, Lines 165–167: The "recent rebound of dust storm activity" is introduced before the corresponding dust records are presented. It may be clearer to present the observational evidence for the dust rebound first and then investigate the associated circulation changes.

Page 6, Lines 168–171: I am not convinced that the change in relative vorticity alone is sufficient to identify an intensified and southward-shifted Mongolian cyclone. Positive relative vorticity indicates cyclonic circulation but does not necessarily identify a cyclone centre. In addition, Mu and Fiedler (2025), using an explicit cyclone-tracking algorithm, report a different trend. A more robust cyclone identification/tracking method or additional circulation diagnostics would be helpful.

Page 7, Line 175: Could the authors provide the corresponding figure or analysis supporting this value?

Page 7, Lines 176–178: The text states that the regional wind-speed differences are shown in Fig. S1A, but I cannot clearly identify these details in that figure. Please check the figure reference or make the differences more explicit.

Page 7, Lines 190–194: Please double-check whether Mu and Fiedler (2025) actually demonstrate that topographic shielding weakens Mongolian cyclone influence on the Taklimakan in the way stated here. My understanding is that they identify this as an issue requiring further investigation rather than establishing it directly.

Page 7, Lines 195–202: This discussion introduces Arctic forcing and dust-radiative feedbacks, but no analysis or figure in this study directly evaluates these mechanisms. At present, these statements rely mainly on previous studies and appear somewhat detached from the analyses presented here.

Page 8, Lines 213–216: The statement that the entire Tarim Basin is dominated by northeasterly winds may be too general. The northern rim of the basin is also an active dust-source region associated with different wind regimes (e.g., Chen et al., 2026, ACP). Regional differences within the basin should be considered.

Page 8, Lines 216–219: The text gives percentage changes between the two periods, but were these changes tested for statistical significance? Earlier sections state that the study aims to "quantify" these changes, so significance or uncertainty should also be reported.

Page 8, Lines 220–223: It would be useful to show the spatial pattern of the DUCMASS trends rather than only basin-mean values. I suspect that the eastern and northern parts of the Tarim Basin may exhibit different trends, which could be obscured by averaging over the whole basin. .

Page 9, Lines 247–250: Why is March used for the long-term wind analysis? Dust activity and circulation can differ substantially between spring months (Wang et al., 2026), so the choice of March requires justification, or the authors could examine the full spring season and individual spring months separately.