General comments:

The manuscript titled "*Impact of Topographic Wind Conditions on Dust Particle Size Distribution: Insights from a Regional Dust Reanalysis Dataset*" by Huang et al. is a manuscript that attempts to investigate what factors change the emitted dust particle size distribution (PSD). They did this analysis by employing the MONARCH chemical transport model with the assimilated dust optical depth (DOD). The authors claimed that by assimilating MONARCH dust using observed DOD, MONARCH has the ability to produce adequate emitted dust PSD. The authors used the MONARCH surface dust concentrations to construct atmospheric dust concentration PSD, claiming this represent the emitted dust PSD. The authors tried to truncate aged transport dust and retain fresh dust by selecting only strongly and freshly emitting dust events using several criteria. Then, the authors performed a multiple linear regression (MLR) analysis to quantify the sensitivity of coarse dust fraction (coarse dust concentration divided by total dust concentration) to various meteorological (wind speed, wind direction) and land-surface variables (e.g., topography, soil texture, soil moisture). The authors found out a predictor model that shows that coarse dust fraction increases with wind speed and with hillslope. They also showed that some times of the day or some seasons favor the production of coarse dust.

This is an interesting and impactful study showing that the observed coarse dust might not be decreasing with or invariant with wind speed as some of the previous theories depicted (e.g., Shao, 2001; Kok, 2011). Therefore, the contribution of this paper is significant. The paper is also nicely written. However, since the paper is based on model/reanalysis and not observations, the authors need to show enough evidence to convince us that MONARCH produces correct dust PSDs. I have some major and minor comments below concerning some parts of the arguments and analysis, and I suggest a major revision.

Major Comments:

1a. What I concern the most is that the findings from this paper are highly contingent upon how successfully MONARCH assimilates the fresh dust concentration PSD. All major findings of this paper are based not on observations but on the MONARCH reanalysis data. As the authors said, the correlation between coarse dust 'concentration' fraction and wind speed can be due to other reasons (e.g., turbulence can hold coarse dust longer in the atmosphere). Even though the authors tried to focus on fresh dust events to minimize the transport effect, the correlation could still be just because there are just more fresh dust particles that contains coarse dust, as the authors referred to Cristina's argument several times (Gonzales-Florez et al., 2023). This means that the correlation the authors found will only be valid for atmospheric dust concentration PSD, but not for dust emission PSD.

1b. Following the previous point, it is great that MONARCH shows this correlation between coarse dust concentration fraction and wind speed, but this is not only the evidence in concentration (not in emission) but also model evidence (not from in-situ measurements). So, the current evidence to me is not strong enough to reject Shao's saltation bombardment theory (Shao, 2001) or Kok's brittle fragmentation theory (Kok, 2011), which focused on the dust 'emission' PSD. But, at least, if MONARCH' dust PSD is correct, then other chemical transport models using Shao or Kok's theories should still replicate this correlation between coarse dust concentration and wind speed.

2. Line 103: Why do you think that MONARCH, by only assimilating coarse-mode DOD (and not fine-mode DOD), is enough to fully constrain the whole dust PSD? (I surely know we can hardly isolate fine dust from other fine-mode aerosols in MODIS). The PSD curve has two ends, and MONARCH

only assimilates coarse-mode DOD, which constrains the right/high end. There is no information to constrain the left/low end, and MONARCH could be underestimating fine dust by not assimilating fine-mode dust. The correlation between coarse dust and wind speed might be a spurious artifect from MONARCH.

3. Lines 106-107: Following the first point, dust surface concentration PSD is not dust emission PSD, although they tend to correlate more in fresh dust events. It inevitably includes also transported dust, and so using dust concentrations PSD already underestimates coarse dust particles. Does this partially weaken the slope and R^2 you found?

4. Lines 176-180: I am curious about Claire Ryder's FENNEC data. Since they have several FENNEC flights, did the authors see a correlation between the coarse dust fraction from her fresh dust PSD and wind speed? It would be better to look at observed correlations too.

5a. Line 189: Since the whole paper's findings is contingent upon the accuracy of MONARCH's assimilated dust concentration PSD, I am not sure if one plot (Fig. 3) of MONARCH vs FENNEC is enough for model evaluation. Fig. 3 only shows that MONARCH's simulating adequate coarse dust, but it does not show that the posterior (assimilated PSD) is doing better than the prior (first guess PSD), and it is hard to judge from Fig. S1. Could you include the first guess in Fig. 3 in a different color and describe how the assimilated PSD is better than the first guess?

5b. Moreover, this evaluation in Fig. 3 does not show that the correlation between coarse dust fraction and wind will hold true in observations.

6. Lines 115-117: I am concerned that this paper would yield very different regression results if the authors used MONARCH's predictor variables rather than MERRA-2's fields. Please describe how MONARCH has its own driving meteorology. Did they do any meteorological assimilation or was it a complete free run? Please also briefly describe how we should expect MONARCH's met fields to be different from MERRA-2's met fields, and the implications on your regression analysis (e.g., will the sensitivities of coarse dust to wind directions largely enhance using MONARCH's meteorology?)

7a. Line 255-257: Since year, season, and time of day are correlated with winds and soil moisture, adding them in your regression analysis likely weakens the R^2 of winds and soil moisture on dust PSD. Please comment on how this collinearity between predictor variables impacts the regression results.

7b. Following the previous comment, wind speed and soil moisture are highly correlated. Including soil moisture likely highly changes the value of β_1.

8. Lines 313: I agree with another reviewer that the R^2 of the regression analysis is rather low. From Fig. 6, it looks like to me that instead of a spatial regression analysis, it might be better construct regression models for each grid/small region rather than putting them together into a single plot.

9. Lines 364-365: This statement does not sound correct to me. A multiple linear regression (MLR) analysis does not give the slope/sensitivity of a predictor variable while holding other predictor variables constant, i.e., it is not a partial differential $\frac{\partial(\text{coarse dust})}{\partial(\text{slope})}$. The magnitude of the slope from MLR is heavily impacted by the multicollinearity among different predictor variables. It is all right to say that coarse dust increases with slope regardless of wind direction, but other factors are not held constant in the MLR when this slope is estimated. Please clarify this sentence.

10. Lines 372-376: The model shows that regardless of wind direction (uphill, tangential, downhill), coarse dust fraction increases with slope. Normally we will expect if coarse dust increases with more uphill slope, then the opposite should be true (fine dust increases with more downhill slope). The three explanations to why increasingly uphill, tangential, and downhill slopes can all generate more coarse dust fraction sound a little ad hoc. Under what circumstance should the fine particle fraction increase?

Other comments:

Lines 72-74: Does this finding come from Malinovskaya 2021? Please cite if so. Does this conflict this paper's findings?

Lines 97-99: A reference is needed on how coarse-mode DOD was obtained from Aqua/MODIS, so people do not need to go to MONARCH's paper to look for it. Is it Paul Ginoux's method?

Lines 100-102: What prior (first guess) dust PSD does MONARCH assume? This requires a more detailed description here. How much is the posterior dust PSD (after assimilation) depend on the assumed prior dust PSD? This question also needs some elaboration.

Line 103: Did MONARCH "nudge" the size-resolved concentrations or used a Kalman filter? They are different methods.

Lines 129-131: A description of how MERRA-2's assumed topography can alter MERRA-2's winds is needed. MERRA-2 has a coarse grid resolution of 0.5°, so the winds can only see a regional topographic slope. Also, I think that SRTM GL3's topographic slope map and MERRA-2's topographic slope map could be quite different. Please comment in the manuscript how this discrepancy impacts the calculation of wind directions and slopes in Fig. 1.

Line 187: It looks like this is the first appearance of the term super coarse dust. Please include a diameter range for super coarse dust.

Line 276: I think you can mention here that you eventually adopted the multiple linear regression (MLR) model.

Lines 305-307: For Fig. 6, can you color code the three panels for three wind directions by the value of wind speed? I am curious how wind speed varies with those vertically aligned scattered points.

Line 314: For Fig. 6, I am also interested in looking at a plot for soil moisture (can be in the supp).

Lines 339-340: Instead of seeing the relationship between coarse dust fraction and the 16 soil type/class, readers might be more interested to see the relationship between coarse dust fraction and clay fraction.

Line 360: For table 2, I am curious about the statistics of the five other predictor variables. Please include them in Table 2 or in the supp.