

Main overall comment: The paper by Wang et al. provides an interesting analysis using the dust observations and modeling to explain what driving variables of dust emissions have changed in the past decades that caused the springtime dust trend. The study topic is appropriate for the ACP journal and the investigate of the dust trends' drivers is timely. The validity of the study's conclusion is based on how well the dust emission model captures the observed dust variability. An issue is that the model evaluation seems to be a little loose, based on only Fig. 1a-b and 2a-b. Instead of showing grid-level model-observation comparison, these panels show regionally aggregated time series, and there are no statistics to quantify the model performance. It is not too convincing that the dust emission model captures the observed dust variability. The ground-based dust observations and the satellite retrievals are also not largely consistent with each other, and this requires an explanation too. The authors should address the issue of model accuracy, and I suggest a major revision. I also ask a few questions regarding the interpretation of the cyclone-induced winds in the specific comments.

Other specific comments:

Lines 112: Some readers may not be familiar with the duISD product. The logic of the formula requires a brief explanation. What is the definition of dust frequency, and where/how were the frequency and visibility data obtained? Please describe it in the main text in 2-3 sentences.

Line 114: From Fig. 1, it seems that the duISD measurements do not agree well with the MODIS dust AOD product in Sect. 2.2. The discrepancy requires an explanation. Which one should we trust?

Line 153: Please provide the top layer soil thickness from ERA5-land.

Figs. 1 and 2: The dust emission model seems to fail to reproduce the interannual variability of the MODIS and ground-based dust, although it captures the general secular trend displayed by the observations. E.g., in Fig. 2a, the blue line has a 2010 peak, which is absent in both the satellite and the ground-based dust observations. Please discuss the reasons for such mismatch in the main text.

Lines 261-262: Why does the box not include the area of the Taklamakan desert, which is the most important desert in East Asia?

Fig. 3: Why do we see strong control by soil moisture and snow in East Asia in April but not May?

Fig. 4: Why is soil moisture less important in North America overall?

Fig. 5: A similar figure for ERA5-LAND soil moisture is needed.

Line 341: A definition for “longer-lasting strong winds” is needed here.

Fig. 7: I might have overlooked, but a dynamical explanation for why the winds are decreasing in April and increasing in May in East Asia is needed. Why does North America not experience a similar seasonality?

Line 402: It is unclear to reader what are the “non-cyclone-affected” strong winds. Please define and explain. For instance, the semi-permanent Siberian High from the north or some sea breezes from the coastal east?

Fig. 9: it is unclear to reader why this article focuses on cyclones if both cyclone and non-cyclone winds have changed and have caused the dust emissions to change across decades. Aren't there other important synoptic weather patterns that dominate surface winds over the deserts, like the Siberian high-induced cold advections and fronts?

Sect. 4: It's not very clear how your modeled dust emissions are sensitive to the prescribed extratropical cyclones. Do you have any idea if you removed all the extratropical cyclone-driven strong winds from your model, how much would your simulated dust emissions drop? A sensitivity run may help quantify the sensitivity of the simulated dust emissions to the cyclones.

Lines 449-450: How did the extratropical storm track change in the past decades and how did they change the wind direction and intensity over Mongolia and the USA, respectively?