In this document, reviewer comments are in black, authors' response is in red, and the revised text is in blue.

On behalf of all authors, I would like to thank Referee #2 for their valuable suggestions, especially those on results interpretation. We have made corresponding changes, which hopefully improve our manuscript. Responses to each point follow below.

Yan

The paper "Climatology in Asian dust activation and transport based on MISR satellite observations and trajectory analysis" presents and discusses the transport of dust aerosols, emitted from the arid and semiarid deserts of Taklamakan and Gobi, over the northern Pacific Ocean. The study falls within the scope of ACP. The study is based on MISR observations, MINX aerosol top height, and accordingly, forward HYSPLIT trajectory analysis. The manuscript is well-written/structured, the presentation clear, the language fluent. However, the submitted study is subject to major deficiencies and I would recommend publishing in ACP considering major revision.

Comments:

1) Regarding the "Asian dust activation climatology". Dust aerosol classification is crucial in the scope of the study, since it is the initial point of the trajectories analysis. Therefore, I would recommend to the authors to describe briefly the dust aerosol classification in MISR/MINX (including the necessary references). The scientific methods and assumptions are not clearly outlined. How is a "dust plume" defined in the paper and how is a "dust event"? In addition, in case of air parcels containing dust aerosols originating from both the Taklimakan and Gobi desert, how is the discrimination performed to the different sources? Which are the uncertainties in the classification?

We now describe briefly how dust plumes are identified in the current study (P3 L30-32): "Dust plumes are identified through MINX from MISR radiance imagery by a trained user, with the assistance from the Support Vector Machine (SVM) datasets in the MISR cloud classifiers product (Nelson et al. 2013)".

We now define "dust plume" and "dust plume data point" clearly in the revised manuscript (P3 L27-30): "Following Nelson et al. (2013), a "dust plume" is defined in this paper as a region of optically distinct dust that extends from an identified source to a downwind region, with visible connection to the source, so that the direction of aerosol transport can be determined visually by the user. A "dust plume" typically contains hundreds to thousands of "dust plume data points"".

"Dust event" was the same as "dust plume" in the previous version of the manuscript, and now are all changed to "dust plume".

In terms of the dust plume dataset, every dust plume retrieved by MINX has its identifiable source, either in the Taklamakan or Gobi Desert, as suggested by the definition of "dust plume". In terms of the trajectory analysis, we use forward trajectory so that the origin of any air parcel is either in the Taklamakan or Gobi Desert, depending on the trajectory starting point. There is possibility that two trajectories initiated from either desert merge. But the trajectory endpoint (Figs. 6 and 7) and trajectory passages (Figs. 8, 9, and 10) are analyzed for each desert separately, thereby such air parcels containing dust aerosols from both deserts will not affect the statistical analysis and conclusions presented in the current paper.

2) Regarding the "Asian dust transport climatology". Although the paper presents an interesting approach to study dust transport the results are not sufficient to support the conclusions, due to the lack of observations provided on parallel with the trajectories.

The study uses MISR observations-MINX provided top height to initiate HYSPLIT forward trajectories. Accordingly the climatology of trajectories is provided and not the Asian dust transport climatology. The difference is substantial. HYSPLIT computes the air parcel's transport and dispersion from a source region (Taklamakan and Gobi here) and describes where the air parcel will go. In the framework of the study, the climatology of the trajectories is provided (spatial distribution - % of trajectory endpoints / Trajectory passage frequency - % of trajectories after a specific number of days), without providing any observation/evidence on the presence of dust (per trajectory, distance or area). Dust aerosols may already have been removed along the transport/trajectory due to dry (gravitational settling) or wet deposition, although the air parcel will reach further distances. The paper does not even provide quantitative information on the probability of dust to have been transported. The trajectory may extend over the Pacific Ocean, and even further, to the western coast of the United States, however this does not provide any guarantee that dust is present and has reached that distance. I would suggest the authors to do any necessary modifications to the manuscript. Either provide dust observations per trajectory or to focus on the trajectories analysis without giving the impression on the presence (and transport) of dust to the trajectories endpoint. Which are the uncertainties? Alternatively, the authors could implement observations on the presence of dust to the western coast of USA (i.e. AERONET and AE, MODIS DT AOD and AE over ocean/ CALIOP volume/particle depolarization ratio) and use HYSPLIT back-trajectories. In addition, assuming a dust plume over an area, HYSPLIT initiated at different altitudes may provide different dust transport pathways. Therefore the study is representative only for the trajectories of the dust top-height and not for the dust plume (trajectories initiated at center of mass/scale height most probably would be more representative to discuss dust transport climatology).
Thanks for the valuable suggestion. We have been completely aware of the limitations in the trajectory analysis, and we have been very careful not to over-interpret results from the trajectory analysis. We also discussed the limitations of the current analysis in the discussion section of the original manuscript, including failure of considering wet or dry deposition and uncertainties caused by using the NCEP-NCAR reanalysis. We certainty agree that examining dust observations per trajectory will enable more robust conclusions regarding dust transport. We really appreciate reviewer's understanding of the amount of additional work regarding dust observations per trajectory. So we decide to go with the reviewer's suggestion about focusing on the trajectories analysis without giving too much impression on the presence (and transport) of dust to the trajectories endpoint. Therefore we revise the entire paper to focus on trajectory, such as replacing "dust transport" with either "dust trajectory" or "dust transport potential" throughout the paper. We also emphasize the uncertainty in the current study in the conclusion, such as (P10 L20-22) "Therefore, the current trajectory analysis provides an upper limit of the actual frequency of long-range dust transport, in particular the trans-Pacific dust transport from Asian sources to North America". We also expand the discussion on future work, adding sentences like (P10 L26-29) "These hypothesis can be tested by analyzing particle size distribution along trajectories from both deserts using ground and satellite observations, as well as performing advanced trajectory analysis that considers gravitational settling and wet deposition of dust particles", and (P11 L12-14) "In order to verify the identified seasonality in dust trajectory patterns, we suggest future studies to take advantage of both geostationary and polar-orbiting satellite observations, as well as ground-based lidar observations. Such dust observations per trajectory will eventually connect the trajectory analysis with actual dust transport.".

Regarding trajectory initial height, we now point out in the data section that the trajectory climatology presented in the current paper refers to that from dust plume top-height: "Given the capability of MISR at observing plume top features, we only analyse trajectories initiated at the observed dust plume top height in the current study". Given the capability of MISR, we are not able to infer the height of dust plume mass/scale center. In a future study, we will consider initiating trajectories using CALIOP or other lidar observations of dust profiles. This is also briefly discussed in the discussion section of the revised manuscript (P10 L30 – P11 L2): "In addition, the current study focuses on trajectories initiated at the top of observed dust plumes. Given the capability of MISR at observing plume top features, we cannot infer the vertical structure of dust plumes from MISR stereo observations. With observed dust plume vertical structure, future studies are encouraged to analyze trajectories initiated at all vertical levels with the presence of dust aerosols".

3) The title does not reflect the contents of the paper and is misleading.
Corresponding to the limitations in trajectory modeling to infer dust transport, we change the title to "Climatology of Asian dust activation and transport potential based on MISR satellite observations and trajectory analysis".

4) The figures are of high quality. I would suggest on Figure 2 to reverse the axes, time on horizontal axis and Injection height at the vertical height.
We revised Fig. 2 according to the reviewer's suggestion. The revised figure is attached.

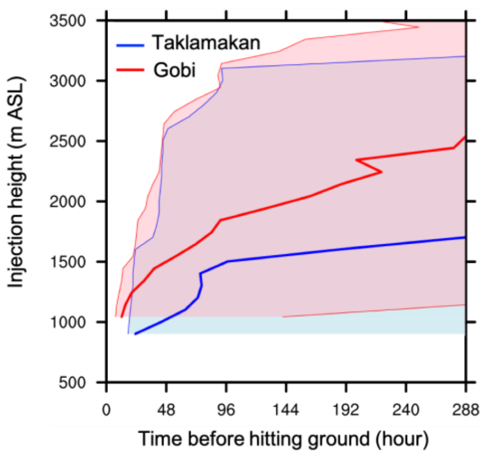

Figure 2: Atmospheric suspension time (hours) of dust particles emitted from the Taklamakan (40˚N, 89˚E, elevation = 805 m ASL) (blue) and Gobi Deserts (43.5˚N, 130˚E, elevation = 954 m ASL) (red) as a function of injection height (m ASL), based on trajectories in March-May of 2001-2003. The thick lines (shading) represent the median (10^{th} to 90^{th} percentiles) of suspension time among 276 trajectories for each injection height.

5) Regarding Figure 3 and the Gobi Desert the lack of continuity in wind speeds between 3 and 12 m/s is a strange feature. I would suggest the authors to describe this feature.
Given the definition of "dust plume" in this study, the dust plume dataset contains dust plume data points both over the source and downwind. While dust particles are activated by strong surface wind (> 10 m s^{-1} in Gobi, in this case), it is not necessary that strong wind persist to the downwind areas. This difference in wind speed over source and downwind regions is the most likely explanation of the discontinuity in wind speed distribution over the Gobi Desert. We now describe this feature in section 3.1 (P6 L17-20): "Indeed, since the dust plume dataset contains points both over and downwind of the source, dust particles from the Gobi Desert are usually activated by strong surface winds exceeding 10 m s^{-1}. The wind speed decreases quickly downwind of the actual source, causing the apparent discontinuity in the wind speed distribution over the Gobi Desert (Fig. 3b)".

6) Regarding references, a brief list of references is provided. I would suggest the authors to expand the list of references in order to strengthen the manuscript and at the same time to give credit to related work. Indicatively, here a brief list of related

studies is provided, describing features of dust aerosol transport emitted from the Taklamakan and Gobi deserts, based on synergies of passive and active ground-based and satellite-based instrumentation, models, campaigns and the meteorological and topographical mechanisms.

Thanks for the list of additional reference. We incorporate all the additional references in the revised manuscript.

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