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Interactive comment on "Climatology in Asian dust activation and transport based on MISR satellite observations and trajectory analysis" *by* Yan Yu et al.

Anonymous Referee #2

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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?

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 ob-

servations 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).

3) The title does not reflect the contents of the paper and is misleading.

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.

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.

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.

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