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Comment on acp-2021-933

Anonymous Referee #2

Referee comment on "Record-breaking dust loading during two mega dust storm events over northern China in March 2021: aerosol optical and radiative properties and meteorological drivers" by Ke Gui et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2021-933-RC2, 2022

The paper "Two mega sand and dust storm events over northern China in March 2021: transport processes, historical ranking and meteorological drivers" by Ke Gui et al. investigates two remarkable sand and dust storm (SDSs) occurred on March 15–20, 2021 and March 27–29, 2021. The study characterizes the origins, transport processes, magnitudes of impact, and meteorological causes of these two SDS events, through satellite and ground-based observations combined with atmospheric reanalysis data. The study falls within the scope of ACP. The manuscript is well-written/structured, the presentation clear, and the language fluent. However, the submitted study is subject to major deficiencies in principal ACP evaluation criteria. Here are some of my main comments which I think will help the authors to improve their manuscript.

(1) The submitted manuscript in general presents limited novel concepts, ideas, tools, or data. An exception is the historical ranking of the dust events, although this is a secondary objective of the manuscript. To be more specific, as stated by the authors, the manuscript's principal objective is to "characterize the origins, transport processes, magnitudes of impact, and meteorological causes of these two SDS events". With respect to the most significant of the two SDSs events discussed here, the event on March 15–20, the meteorology and impact of the 3.15 SDS is discussed in Filonchyk et al., 2022 "Characteristics of the severe March 2021 Gobi Desert dust storm and its impact on air pollution in China", while the transport processes are provided by Liang et al. (2021) "Revealing the dust transport processes of the 2021 mega dust storm event in northern China". Thus, "the origins, transport processes, magnitudes of impact, and meteorological causes" have already been discussed. A recommendation would be to focus on the second event of March 27–29, 2021 which has not been discussed so far and on the historical ranking of the dust event, which however, according to manuscript and extend of material, is interpreted as a secondary objective.

(2) Providing the objectives of the study, it is stated by the authors that "although these two studies have strengthened our understanding of the 3.15 mega SDS event in 2021, the sources, three-dimensional evolutionary features during transport processes, historical ranking, and local meteorological anomalies of the 3.15 and 3.27 SDS events have not yet been elucidated." However, with respect to ACP evaluation criteria of "giving proper credit to related work and clearly indicate their own new/original contribution?", the significant published study of Filonchyk et al. (2022), presenting the characteristics of the severe March 2021 Gobi Desert dust storm and its impact on air pollution in China is not mentioned, and the outcomes not compared, nor discussed in terms of discrepancies, similarities – although substantial – and conclusions. A strong recommendation is to extensively discusses the similarities/differences/conclusions of the related published studies, and build on top of the previous studies.

(3) A third point is related to the similarities in satellite and ground-based observations. With respect to ground-based in-situ observation, the PM10 data from CNEMC (http://www.cnemc.cn), have already been provided, presented and discussed in previous studies. Similarly the reanalysis dataset. Similarly, MODIS and CALIOP EO of the SDS 3.15 event. The authors could use different datasets, not implemented in previous studies, instead of datasets already used. Some suggestions of satellite-based EO datasets not used in previous studies could include the Metop IASI DOD, Sentinel 3A/B DODs, the MIDAS DOD dataset, and more.

(4) The study could be improved in terms of providing and outlining more extensively the information of the scientific methods and assumptions used, which are only briefly discussed, in terms of limitations of the implemented datasets. For instance, very few information on the quality assurance criteria is provided. With respect to CALIPSO, as QA only the CAD score is mentioned that it is implemented, while in the literature, significantly more QA filters are recommended (e.g. Tackett et al., 2018). Another point could be the particulate depolarization ratio, when observed lower to 0.3, indicates mixtures of dust and non-dust components, thus the provided CALIPSO information, considered in the study as pure-dust, overestimates the actual dust extinction coefficient. With respect to MODIS-retrieved DOD, which are the limitations in terms of Deep-Blue algorithm, Ångström exponent, SSA and the quality of the DOD product? Which are the attenuation effects – thus the limitations – of the different datasets (active and passive) and how do they compare in terms of observations? The SDSs are transported detached in extended domains over China, and how does this compare with only the surface-based in-situ? There is a wealth of datasets incorporated in the study, however, the dataset observations are not extensively spatio-temporarily inter-compared to account for and extract the closest representation and actual state of the SDSs.

(5) The ground-based lidar observations are a novelty however, in terms of dataset. At this point, the authors could provide more information to characterize the SDSs, including the intensive properties of dust (e.g. LR used), the pre-processing of the dataset and retrieval (e.g. Raman/Klett), the temporal averaging techniques, observed wavelength dependencies, and more.

Although similar studies have been performed for the specific SDSs and the study domain, improvements of the present work may lead to very interesting contribution in the literature, due to the very special and diverse conditions encountered over the Eastern part of Asia. At its present state, and taking the above comments under consideration, I suggest to the journal to reject the paper. However, I would suggest the authors to go through the entire manuscript once more and build on top of the published studies and on this work, follow the suggested improvements/recommendations, and maybe focus on more novel ideas such the effects of the studied SDSs in terms of atmospheric chemistry/physics (i.e. RT, IN/CCN, dust deposition rate), and then I would encourage them to resubmit it.