Comment on acp-2021-1062
Anonymous Referee #1

Referee comment on "Inverse modeling of the 2021 spring super dust storms in East Asia" by Jianbing Jin et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2021-1062-RC1, 2022

General comments:

This work explores the dust emission and deposition distributions in East Asia induced by an extremely severe dust storm in spring 2021 using the four-dimensional variational method. The authors used scientifically plausible experimental methods with reasonable observation datasets. Therefore, this manuscript will be acceptable as a paper with minimum quality standards after a major revision of English errata and a minor revision of scientific descriptions. However, the experimental designs and conclusion of this work are not scientifically significant very much. To increase the scientific significance of this work, the authors should carry out additional data-assimilation experiments and reclassify dust-source regions in China/Mongolia. The reclassification of dust-source regions could change the authors’ conclusion. The decision to accept or reject this manuscript as an article of Atmospheric Chemistry and Physics, one of the high-impact journals, is left to the editor.

Major comments:

Page 12, Lines 17-18: generally, the official uncertainty of remote-sensing and in-situ measurements includes only instrumental errors. It doesn't include data-screening errors and spatial representative errors, while 0.25x0.25-degree grid dust models needs large representative errors. The official AOD uncertainty of the MODIS Deep Blue product is probably underestimated especially under cloudy conditions. The assumption of the PM10 observation error (10% in Jin et al. 2018) is also underestimated due to the lack of spatial representative errors. Plus, bias-correction errors are not included in this study. That means that the weight of observations will be overestimated in the data assimilation
system. This might be one of the reasons that the priori and posteriori emission distributions are very different in this study.

Page 15, Lines 20-21: the authors say “a three-dimensional grid distortion should be developed to solve this issue,” but I don’t agree with it. Only dust emissions are control variables in this study. Advection cannot be controlled. Therefore, the inconsistency in the wind field should be tried to solve first. Otherwise, the dust data assimilation system has to keep using the wind field data and advection model that cannot reproduce vertical sheers to inverse dust emissions. This inconsistency is first and foremost a matter of the model dynamics.

Page 16, Lines 10, 29 and 31; Page 18, Lines 6 and 23: it’s misleading to refer to domestic sources/emission/deserts as “local” ones. Local means neighborhood or specific. In this study, for instance, the distance from Beijing City in NCP to the Alxa Desert in China is more than 1000 km while it’s 550 km to the Sino-Mongolian border. However, the authors refer to Alxa as a local source but Mongolian Gobi as a faraway source. The “local” should be replaced by “domestic”.

Page 16, Lines 21-25: the authors found interesting characteristics of dust transport and deposition for each storm period. This study would have been a much better paper if the authors gave a deeper insight into the meteorological causes of the dust transport/deposition characteristics rather than just stating the facts. Each transport/deposition can be probably explained by the dynamics of synoptic meteorological fields.

In addition, I have a question. Did the source apportionment simulations last for only the SD1, SD2, or SD3 period? If so, the simulation periods are only 2 or 3 days. When a dust plume flows directly from the source region to NCP or NWP, the 2 or 3 days is long enough. However, when a dust plume is caught by synoptic disturbances multiple times, it might take more than 3 days for the plume to travel 2000 km from western Mongolia to NCP/NWP. Is it possible that the short simulation time is one of the reasons why the Alxa Desert less influenced NCP/NWP during SD1?
Point 1: The authors repeatedly emphasized that the MODIS AODs were screened by Angstrom exponents and bias-corrected by non-dust aerosol simulations. The PM10 data were also bias-corrected. The authors cited the papers of these preprocessing methods, but didn’t present the improvements of the inversion for the 2021 dust storms made by these preprocessing methods at all. Even if the preprocessing methods worked well in the case of previous studies, there might be not much positive impact on the inversion of the 2021 dust storms. If the preprocessing methods are emphasized in Abstract and Summary, it should be shown in this manuscript how much the inversion is improved by the preprocesses for the cases of the 2021 dust storms. If not, the preprocessing methods shouldn’t be emphasized in Abstract and Summary.

Furthermore, the authors emphasized that both the MODIS AOD and PM10 data were simultaneously assimilated to estimate dust emissions. However, its benefits were not quantitatively presented in this study. I’m very interested in the difference of the inversion results between a MODIS AOD-only assimilation, a PM10-only assimilation, and the simultaneous assimilation. If the difference is shown in the manuscript, it will be an alternative to independent validation with a subset of leaving data. This study would have been a much better paper if the authors presented more than one inversion results illustrating the quantitative improvements made by the preprocessing methods and the simultaneous data assimilation.

Point 2: The authors divided the dust source regions into Chinese sources and Mongolian sources in this study. However, it’s not a scientific classification because the Sino-Mongolian border in the Gobi Desert was artificially or politically drawn, not geologically or biologically. Although the authors cited Han et al. (2021) for Mongolian desertification, Han et al. (2021) evaluated only Mongolia and didn’t compare the Mongolian Gobi and the Chinese Gobi quantitatively. If the classification with the Sino-Mongolian border is a scientific or environmentally crucial issue, it should be clarified first that the desertification in the Mongolian Gobi is much more serious than in the Chinese Gobi, before the source apportionment study.

Furthermore, the authors concluded that “local” Chinese deserts play a small role in the dust deposition over FWP and NCP compared to the contribution of “long” transports from Mongolia. This conclusion is very misleading. The northeastern part of the Gobi Desert, which is located in Mongolia, is much closer to FWP/NCP than the southwestern part of the Gobi Desert, which is located in China. In other words, Chinese deserts are not always “local” for FWP/NCP and Mongolian deserts are not always far away from FWP/NCP. I think it will be more scientifically plausible to classify dust source regions using the distance, latitude, altitude, and vegetation, not using national borders. If the reclassification was performed, this study would be an excellent paper.
Specific comments:

Figures 2-5 and S1-S3: The font of each map’s title is too small.

Page 6, Lines 15-16: I don’t think the bias-corrected PM10 data CLEARLY shows the shape of the dust storm. It’s too subjective to say “clear” for this distribution.

Page 6, Line 17: the authors say “the shape of the simulated dust plume matches well with the observed shape,” but I think “it’s slightly matched.” To use “well” is overvaluation.

Page 8, Line 25: the model has only eight layers from the surface to 10 km, which is very sparse, especially in the PBL, to investigate aerosol emission and deposition. Usually, state-of-the-art aerosol dispersion models have more than 50 layers from the surface to the tropopause, including more than 10 layers only in the PBL. Why is the vertical resolution set so low? Even if the meteorological fields are provided from ECMWF, the sparse layers in the dust model will result in a very large vertical numerical diffusion, which deteriorates regional aerosol simulations. May I have the authors’ opinion?

Page 12, Eq. 4: there’s no explanation for the background error covariance matrix B in the text.
Page 12, Line 9: This study built a background error covariance matrix using ensemble simulations. If the tangent linear method used in this study resembles ensemble-variational (EnVar) methods, how to prepare the ensemble perturbations is critical for the assimilation performance. Although Jin et al. (2018) briefly describes how to make the perturbations, this important issue should be described in detail here.

Page 13 Line 9: I don’t think the name of Alxa desert is famous outside of China. Usually, the area is considered as a part of the Gobi Desert. Please indicate its location in Fig. 1 with a large font, not only in Fig. 6.

Page 13, Line 10, Line 15, Line 21: maximum emissions per unit area are not very meaningful because they strongly depend on the horizontal model resolution.

Page 15, Lines 10-11: “resulting in a RMSE of 833 ug/m3, 1.36 and 1.53” this phrase is very hard to understand at first glance because it’s not easy to realize 1.36 and 1.53 are AODs.

Page 15, Lines 11-12: I think “the observation-minus-simulation mismatch” means mean errors (ME). If the authors mean RMSE, this phrase has to be rewritten.

Page 15, Line 33: Fig. 4(b.1) Is this a mistake for Fig. 4(a.1)?
Page 16, Lines 26-28: It’s ok with “only 160 tons against 49k tons” and “only 15k tons against 97k tons,” but “only 74k tons against 50k tons” is not acceptable.

Page 16, Line 31: 58% is attributed to the transnational transport over FWP, right? Please describe it in the text.

Page 18, Line 3: the authors stated in Conclusion that three super dust storms resulted in profoundly effects to Earth system, but the impact of the dust aerosols on weather, climate change, or the Earth System was not investigated in this study. If someone has already researched the impact of the dust storms in 2021 on the Earth System, those references have to be cited.

Page 18, Line 29: I accessed http://106.37.208.233:20035, but couldn’t obtain the PM10 data. I think Chinese government usually prohibits foreigners from accessing and obtaining Chinese environmental observation data. Could the authors provide the PM10 data used in this study based on the EGU data availability policy?

Technical corrections:

The authors often use “a (two) decade(s)” to mention the period between the 2000’s and 2021 probably meaning one and half decades. However, this expression seems confusing and peculiar.

Besides, this manuscript contains too many errata and grammatical inaccuracies to
publish as it is. I strongly recommend that the authors polish the manuscript more earnestly or use an English proofreading service.

For instance,

Page 2, Line 7: Fig.1(a) □ Fig. 1(a) [Not only here, almost all of “Fig. xx” in this manuscript don’t have a space after a period.]

Page 2, Line 31: “BSCDREAM8b” □ “BSC-DREAM8b”

Page 3, Line 8: “next the simulation models” What’s this phrase?

Page 3, Line 31: Fig. 1)(a)

Page 4, Line 13: non-dust biase

Page 4, Line 31: “studied” □ “was studied”???

Table 1: China Stand Time □ Chinese Standard Time

Page 5, Line 11: Fig.1(b) □ Figure 1(b) [Not only here, a word at the beginning of a sentence shouldn’t be shortened.]

Page 5, Line 15: Table.1 □ Table 1

Page 5, Line 16: “2 to 3 day” □ “2 to 3 days”???

Page 7, Line 29: Angstrom □ The Angstrom exponent

Page 8, Line 1: Fig.5 □ Figure 5
Page 8, Line 29: “but not far away to FWP or NCP” Is this really NOT far?

Page 11, Line 7: by (Zender, 2003) □ by Zender (2003)

Page 11, Line 11: What’s F_h? Is it f_h?

Page 11, Line 14: this sentence is wordy, colloquial, and extremely hard to read.

Page 11 Line 22: in (Jin et al. 2018) □ in Jin et al. (2018)

Page 12 Line 18: in (Jin et al. 2018) □ in Jin et al. (2018)

Page 13, Lines 3-4: “by assimilation the bias-corrected ...” What’s this?

Page 13, Line 8: “in SD1 to SD3” □ “in SD1, SD2, and SD3”

Page 13, Line 10: “Mongolia desert” □ “Mongolian desert”

Page 13, Line 11: “a new the emission field” what’s this phrase?

Page 13, Line 12: “with more grid cells from which emission took place located in...” This phrase is hard to understand.

Page 13, Line 26: “map show that” □ “map shows that”

Page 15, Line 22: Fig.3 □ Figure 3

Page 15, Line 24-25: the tenses of the two verbs disagree.
Page 16, Line 20: “in SD1 to SD3” ☐ “in SD1, SD2, and SD3”

Page 16, Line 21: “panel (a.1)” belongs to which figure?

Page 16, Line 22: “almost not effect” ☐ “almost do not effect”???

Page 16, Line 27: “Mongolia dust” ☐ “Mongolian dust”

Page 18, Line 12: “the they are ...” ☐ “they are ...”

Page 18, Line 25: “Mongolia dust” ☐ “Mongolian dust”

Besides these errata, verb tense/article/punctuation mistakes are often found.