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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Source backtracking for dust storm emission inversion using adjoint method: case study of northeast China" *by* Jianbing Jin et al.

Anonymous Referee #4

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The paper presents the application of an adjoint model in the context of dust data assimilation. The paper is motivated by some of the limitations shown in a previous paper by the authors (Jin et al. 2019b), where the assimilation system was not able to correct a large underestimation of dust in north east China. The adjoint model is used here to detect what the authors conclude to be the most likely source region that would explain the unresolved dust loads (Horqin desert). After adding the source in the model by increasing soil erodibility in that region, the emission inversion is performed again and it is concluded that the new results are much closer to observations. The authors also recommend to include the Horqin desert as a more active source region in models.

The study is interesting, in particular the use of the adjoint as a tool that could be used detect potentially omitted dust sources in models. However, I have some major comments (both on content and structure) that would need to be addressed before the paper can be considered for publication in ACP.

1) I find that the identification of the Horqin desert as the source that would explain the unresolved dust loads is not robust enough and would need further clarification or at least nuancing. There are few issues that make this identification uncertain:

a. The study uses Himawari-8 AOD. How can it be concluded that the AOD signals in regions MR-A and MR-B are due mostly to dust? The authors should provide evidence of that. Forecasts from the US Navy on May 3, 2017 (https://www.nrlmry.navy.mil/aerosol/globaer/ops_01/mongolia/201705/2017050312_globaer show a strong influence of sulfate optical depth in addition to dust. This effect could be strongly biasing the results as it would be overestimating the dust optical depth.

b. Figure 8 shows the backward time series of emission sensitivity. The conclusion that the Horqin desert is the most likely source is based on an a priori assumption: (Page 17, line 5; "If the dust was emitted earlier, it seems to originate from regions further south. However, these are densely populated regions covered with vegetation, and therefore not a likely to be a source of dust.") There are two problems with this statement: 1) The areas highlighted in panels a.5 and a.6 overlap with potential dust emission areas highlighted in the in-depth study and dust source inventory of Ginoux et al. (2012) (see their Figure 10, region # 6, North China Plains). 2) If the air masses are coming from the southern populated areas they may come with anthropogenic aerosols (this is consistent with the trajectory of the high sulfate optical depth from the Navy forecasts highlighted above) which goes back to my previous comment.

c. On a side note the Ginoux et al. (2012) inventory already highlights the Horqin desert as a potential dust source, and it is not the only study. Please note that the absence of emissions in your model there may be model-specific. Many models do not assume zero emissions from sparsely vegetated areas and use LAI or fractional cover to scale emissions in those areas so they are not zero.

2) The comparison of the final results with those from the previous study (Jin et al. 2019b) after including the new source in the model is not consistent. At least that is what I understood from reading the paper. In Jin et al. (2019b) AOD is assimilated from theÂăHimawari-8 satellite AOD and PM10 is used as an independent evaluation dataset. In this paper, PM10 is also used in the assimilation (page 19, line 15: "Himawari-8 AOD values are assimilated in the first cycle, of which the measurement error configurations are similar as in Jin et al. (2019b). However, almost no AOD values are retrieved in the second window over the MR-A region, hence the ground PM10 observation are assimilated instead, of which the representation errors are set similar to those in Jin et al. (2019a)."). Therefore, the comparison between both experiments is inconsistent and the improvements (or at least a large fraction of them) stated in the text and the abstract haven't been shown to be due (at least solely) to the inclusion of the new dust source, and they could just be due to the assimilation of the PM10 (which is presumably used as the evaluation dataset as well). Experiments should be compared under the same assimilation conditions both without and with PM10.

3) Structure of the paper: I strongly recommend restructuring the paper to follow the classic introduction, data and methods, results, discussion, conclusion format. The paper combines methods, data with results and discussion thought-out the paper and this makes the reading difficult.

Additional comments or minor issues:

- Equation 3 in the original publication of Ginoux et al. (2001) is the same expression but to the power of 5.

- Page 7, line 32: there is a typo ("eThe red box")

- Again, I recommend restructuring the paper and include all the methods after the

introduction. Also in my opinion, section 5.2 interrupts the flow of the manuscript. It would be enough to say thatÂăboth the finite difference and adjoint method seem able to derive emission sensitivities and refer to an include this section in an appendix

- It is strange to read this in the conclusions: "Note that also the presence of non-dust particles in the PM10 observations limit the assimilation accuracy; removal of the non-dust part as in (Jin et al., 2019a) should become part of the standard procedure." The first author of the paper is the first author of the referred paper. Why not removing the non-dust part in this paper?

References use here and not cited in the manuscript: Ginoux, P., J. M. Prospero, T. E. Gill, N. C. Hsu, and M. Zhao (2012), Global-scale attribution of anthropogenic and natural dust sources and their emission rates based on MODIS Deep Blue aerosol products, Rev. Geophys., 50, RG3005, doi:10.1029/2012RG000388.

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