Atmospheric Chemistry and Physics Discussions

Interactive comment on "Numerical modeling of Asian dust emission and transport with adjoint inversion using LIDAR network observations" *by* K. Yumimoto et al.

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Reply to Reviewer #1:

Thank you very much for appropriate and adequate comments. We have deliberately confirmed and considered your comments. We believe that we have made sufficient changes to the revised manuscript after considering all comments. Below we will provide a point-by-point response to the Reviewer comments.

General Comments:

1) As the Reviewer mentioned, we should normally use the estimate of detailed background error of modeled dust emission flux, which takes spatial (if necessary temporal)

correlations into account, for the adjoint inversion. However, different from other anthropogenic chemical species (of which emissions are estimated using energy consumption and population), the difficulty of direct observation of dust emission flux over inland desert regions renders realistic and detailed estimation of the background error quite difficult. For this study, instead of assigning covariance elements of the background error, we used the smoothing term. New measurements of dust uplift flux (e.g. an intensive observation campaign) will help to elucidate the background error in greater detail.

2) The uncertainty of numerical dust modeling includes uncertainties of emission, transport, and removal processes (i.e., gravitational settling and dry/wet deposition). First, the uncertainty of transport depends largely on meteorological fields. In the current version of RC4, the meteorological fields were provided by the meso-scale model RAMS, which used NCEP reanalysis data (already analyzed using the assimilation procedure) for initial and boundary conditions, and for nudging data. Moreover, a recent dust model intercomparison (DMIP; Uno et al. 2006) revealed that the model results correctly captured the major dust onset and cessation timing at surface observation sites; horizontal distributions of surface level dust concentrations appeared quite well in each model. However, the concentration levels were quite different (the difference over the Beijing region became more than 10 times greater). Secondly, Carmichael et al. (2007) estimated that the uncertainty of emissions is greater than that of wet removal by a factor of 2.5 (the respective factor uncertainties of emission and wet removal are 5 and 2). From these discussions, the emission is apparently the most uncertain. We assumed that the other model uncertainties were negligible. We will add a brief discussion of each model uncertainty to the revised Section 2.

3) I thank you for your careful checking of our manuscript. Before submission, our manuscript was examined by a native English speaking American. This time, a native English speaking American person who is an experienced proof-reader of scientific papers will examine the revised manuscript.

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Specific comments : 1) We will revise the title to 'Adjoint inversion modeling of Asian dust emissions using LIDAR observations', as you have suggested.

2) We will remove the relevant statements from the Abstract.

3) We will improve that point in the revised manuscript.

4) The temporal scale is one dust event (about 10 days). The special scale is eastern Asia (including the Gobi Desert, Mongolia, Inner Mongolia region, and Taklimakan Desert). We will add more details (related to spatial and temporal scales) to the revised manuscript. Please also see the response to comment (Specific comment #22).

5) This describes which meteorological data the dust models used. For example, the CFORS model in DMIP used meteorological fields provided using a meso-scale meteorological model RAMS in the on-line manner. We will improve the misleading statement in 'meteorological fields'.

6) Thank you for your suggestion. We will add that reference to the revised manuscript.

7) You are correct. I will correct that point.

8) We can include the emission rate (i.e. epsilon in Eq. (4)), initial and boundary conditions (i.e. dust concentration), and parameters in removal process (e.g. dry deposition velocity). In this study, the emission rate is set as the control vector. We will re-organize and rewrite Section 2 (please also see the technical comment for P15963 Eq. (6), Section 2, and P15959 Eq. (1) and the specific comments #6 and #7 for Reviewer #2), and add more detailed description related to control parameters to the revised manuscript. Please also see the response for comment (General comment #02).

9) Please see the response for comment (Specific comment #08).

10) (Please also see technical comment for P15963, Eq. (6), Section 2 and P15959, Eq. (1) and Specific comments #6 and #7). We want to re-construct and rewrite Section 2, especially the descriptions of the model and assimilation procedure, as you have

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suggested. We believe that we will have made sufficient changes considering all the comments and satisfied the suggestions in the revised manuscript.

11) No. In this version of RC4, we have no preconditioning.

12) In this, we had seven iterations for Experiment A and nine iterations for Experiment B. We stopped the iteration procedures when the norm of the gradient of the cost function had been reduced to an extremely small value, meaning that another iteration would have produced little difference.

13) Your suggestion is correct. Equation (4) is defined when the friction velocity is larger than the threshold ($u^* > u^*$,th). We will improve Eq. (4).

14) C is the function of snow cover, soil wetness, and soil texture. The dimension of C is (kg/m2/s) / (m/s)4. For details, please refer to Uno et al. (2003).

15) Gamma is the strength of the penalty. ∆ means the Laplacian. We will add more detailed explanation of these parameters in the revised Section 2.

16) In the dust event targeted by this study, the forward dust model (w/o the assimilation) generally underestimates the dust concentrations. Slightly negative values occurred through the adjoint inversion of both experiments. We assumed that the negatives are negligible; they were replaced with zero emissions.

17) Of course you are right. We will correct that expression.

18) In this study, we used the LIDAR dust extinction coefficient. The LIDAR dust extinction coefficient is derived from the extinction coefficient using the contribution of mineral dust, which is estimated using the particle depolarization ratio (Shimizu et al., 2004).

19) S1 is the 'lidar ratio'. We will add a brief description to the revised manuscript.

20) Sometimes, dust loadings emitted over the Taklimakan Desert took more than 4 days to reach the Japan archipelago. For this study, we decided to perform simulations

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over a period of two weeks to ensure that all relevant data were collected.

21) 'Surface boundary data' include soil texture data, soil moisture, and snow cover data. We will add more detailed descriptions to the revised manuscript.

22) Please refer the response to Specific comment #03. We will improve the manuscript.

23) Please refer to the response to General comment #01.

24)Yes.

25) As the Reviewer has described, air particles from different regions might have the same potential temperature. Figures 2 and 6 show that the dust loading targeted in this study was caused and trapped by the low-pressure and its associated cold front hitting eastern Asia in early April 2007. The dense dust loadings, which were observed from 31 March to 1 April at Seoul and on 1-2 April at Matsue and Tsukuba, follow decreases in the potential temperature by the cold front, and are captured at theta = 285-295 K. Therefore, we presumed that those dust layers were transported from the same source region. We will improve the related statements as follows: 'A heavy dust event occurred from 31 March to 1 April at Seoul (dust extinction coefficients were greater than 2 km-1) and during 1-2 April at Matsue and Tsukuba. Figure 2 shows that the dense dust loading was caused and trapped by the low-pressure system and its associated cold front, which hit Mongolia and north-central China on 30 March 2007 and traveled eastward. The dust events observed at each LIDAR site are presumed to be transported by this low-pressure system and its associated cold front.'

26) The blacked-out areas denote missing observations because of clouds, rain, and an overly dense dust layer, which prevented the LIDAR signal from penetrating the upper layers. We will add a more detailed description to the caption of Fig. 3. In addition, see the response to Specific comment #47.

27)As described in the manuscript (P15965, L16-20), the upper dense dust layer might

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not be an independent (separated) layer; it might be continuous with (or extended from) the lower dense dust layer. We will improve the explanation given in the manuscript in that regard.

28) You are right. We will improve the statement according to your suggestion.

29) I cannot say for certain. Observations measured after 28 April had sensitivities to dust emissions on 23 and 25 April (Fig. 7), and increased the emissions, which caused increases of the posterior at Seoul and Matsue on 26 April.

30) You are correct. The modeled AOT (solid line) is calculated by the integration only taken over the height range within which valid LIDAR observations were made. In the manuscript of the former version, the description is poor. We will add a more detailed description to the caption of Fig. 4. Please also see the response to Specific comment #48.

31) We will remove that superfluous text from the revised manuscript.

32) We will improve that by revision into RMS difference (RMSD).

33) In this revised version of manuscript, we use data from 25 March to 4 April for the calculation of RMSD. We will revise RMSD of Table 1 according to your suggestion.

34) In Fig. 5, the observations show a very rapid decrease of PM10 concentrations at offsets of the dust event (at Banryu station, PM10 concentration was reduced by more than 500 μg/m2 during one hour). The model results (both a priori and a posteriori) were unable to reproduce the sharp decrease. Although the peak PM10 concentrations are improved, the assimilation also engenders over-estimation of the concentrations after the offset. Degrading of the RMS because of this overestimation overcomes improvement of RMS through better agreement of the peak PM10, resulting in the total degradation of the RMS.

35) We will remove that duplication from the revised manuscript.

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36)We will revise u*,th into u*.

37)We will remove this sentence and rewrite the previous or subsequent statements.

38) We will rewrite the summary of the results in the revised manuscript.

39) We will improve that point in the revised manuscript. Please also see the response to Specific comment #38.

40) As the Reviewer described, quality control and proper correction are important processes for assimilation using multi platform observations. We will add statements describing the biases and the correction to the revised manuscript.

41) We will make that replacement in the revised manuscript.

42) Please also see the response for Specific comment #33. We will add the time window in the caption.

43) 'dust ext., 1/km' means the observation unit assimilated (e.g. vertical profiles of LIDAR dust extinction coefficients). We will clean up the table to avoid misleading readers.

44) One reason might be that we use the period of 25-29 March to calculate the statistics (see the response for (Specific comment #33)). At the two added stations, the revised-mean-modeled AOTs in Experiments A and B are almost equal.

45)Of course you are correct. I will improve the relevant text in the revised manuscript.

46) We will add all SYNOP sites to Fig. 1 (to avoid making Fig. 2 so busy).

47) We will add a description of the blacked out area of the upper row in the caption (remove duplicative explanations from the text). In addition, we will improve the color scale to show that white in the lower three rows means low extinction values.

48) You are correct. We will add more detailed explanations of dashed and solid lines to the revised manuscript.

49) Modeled wind fields at lowest model grids. We will improve that expression from 'surface wind' to 'modeled wind field at lowest model grids'.

Technical comments: Thank you again for your careful review of our manuscript. We will improve the suggested points in the revised manuscript. Some suggestions require individual replies.

P15972, L17-18: I don't understand this sentence.

Response: I will rewrite that text.

P15973, L26-27: What are 'observations that can enable measurement'?

Response: I wanted to mention that few observations exist, which provides dust size distributions. I will rewrite the related text.

P15981, Fig. 1: What is the blue square? What are the black circles?

Response: I will add descriptions to the caption of Fig. 1.

Section 2: This section jumps back and forth between description of model and 4D-Var. Separate these, first describing the model (including parameterization of emissions) and then the 4D-Var. P15963, Eq. (6): B ! R (or better use indices, R_{ii} and y_i, since otherwise left-hand side is a matrix, while right-hand side is a vector). Also, use of letter E for errors is confusing, since E without subscript was used for emissions in Eq. (1). P15961, L2: What is F_k? What is f_k?

Response: We would like to rewrite Section 2 and correct equations and symbols in the revised manuscript.

Interactive comment on Atmos. Chem. Phys. Discuss., 7, 15955, 2007.

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