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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Elevated large-scale dust veil originated in the Taklimakan Desert: intercontinental transport and 3-dimensional structure captured by CALIPSO and regional and global models" *by* K. Yumimoto et al.

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Dear Referee #1,

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

Comments on scientific content

[Comment] 1. Line 28 of page 14455 (introduction). The indirect effect of dust on clouds is far from proven or understood. I suggest the authors give a more recent reference concerning the role of dust as cloud condensation nuclei, the Twomey (1977) reference is not really relevant for coarse insoluble aerosol.

[Answer] Thank you for your helpful suggestion. I will add a recent reference focusing the ice cloud formation and indirect climate effect with Asian dust (Sassen, K: Indirect climate forcing over the western US form Asian dust storms, Geophys. Res. Lett., 29, 1465, 2002) in the introduction.

[Comment] 2. The comparison of CALIOP against the model is a good thing to do. However it is difficult to judge the performance of the model against the observations knowing that all of aerosols and dust emissions were excluded in the simulation. How can the authors be sure that the dust veil they modelled was the dominant feature in the CALIOP cross-sections. Perhaps they have reached this conclusion by careful examination of the CALIOP data. I think the discussion at the beginning of section 3.1 should state this caveat briefly.

[Answer] At first, we apologize that our complicated plots and insufficient explanation about figure 2. In revised manuscript, we will re-organize figure 2 and ask the publish section to enlarge fig. 2 to full page. We also perform an off-Taklimakan dust simulation in which we eliminate dust emission from the Taklimakan Desert and include all other aerosol sources (i.e., sulfate, sea salt and black carbon). The off-Taklimakan dust simulation cannot reproduce the long-range transport of the dust cloud shown in CALIOP measurements and the Taklimakan dust only simulation. This fact means that dust from other sources and non-dust aerosols cannot be brought up to such a high altitude and experience the intercontinental transport. Therefore, coupling with results of the HYSPLIT back trajectory analysis, we judge that the CALIOP cross-sections capture the intercontinental transport of the dust veil originated from the Taklimakan Desert, and the models successfully track the dust veil. Also see [Comment 5] by Referee #2. We will add brief discussion as follows:

[line 10, page 14460] '. . . quite well. In addition, we also perform a preliminary SPRINTARS simulation in which dust emission in the Taklimakn Desert is eliminated and the other aerosol sources (sulfate, sea salt, carbonaceous, and dust from non-Taklimakan Desert) are included. The off-Taklimakan dust simulation cannot capture the dust veil measure by CALIOP (not shown). This fact indicates that dust from other sources (i.e. the Gobi Desert) and non-dust aerosols cannot be brought up to such a high altitude, and contribute formation of the dust veil. Figure 2b shows. . .'

[Comment] 3. The modelled concentration of dust seems to be too low in some of the comparisons against the CALIOP and NIES lidars. In Figure 3 the model extinction coefficient is about an order of magnitude lower than the data from the lidars although in Figure 4 it looks to be in agreement with the lidar. Is this due to an error in the position of the dust veil in figure 3 or is it a reflection that dust emissions were generally underestimated in the model? The authors state that the model dust concentration was generally too low but roughly what degree of underestimation are we generally talking about and what level of uncertainty does this therefore place on the estimates of dust deposition? Furthermore, because the numerical range in the color-scales of Figure 3 are not consistent between the model and observations the reader could easily miss this difference completely. This discrepancy should be pointed out and explained briefly in the discussion of figure 3.

[Answer] At first, we apologize that our complicated figures made you confused. Figure 4 (b) shows normalized vertical profiles by the maxima. This means that models capture shapes of the vertical profiles, but underestimate dust concentrations same as figure 3. We will add brief explanations about the normalization and discrepancy in color-scales in revised manuscript. From comparison of vertical profiles, the model underestimates dust extinction coefficient by 1-2 order in the North America and North Atlantic Ocean. This fact results in a considerable underestimate of dust deposition amounts. However, because of no measurement about dust deposition, we cannot estimate the uncertainty accurately. We will add brief discussion about model under-

estimates in dust concentration and deposition in the revised manuscript as follows:

[line 18, page 14462] 'though generally underestimate dust extinction coefficient: especially, in the North America and the North Atlantic Ocean, the model underestimates dust extinction coefficient by 1-2 order. This underestimate also leads a large uncertainty in the estimate of dust deposition amounts.' We will add more explanation about the numerical range of figure 3 in the revised paper.

[Comment] 4. I also notice that the numerical range of the color scales in figure 2a and 2c are not consistent. If readers failed to notice such subtle changes they would automatically interpret the model results in a much more favourable light.

[Answer] We will add brief explanation about the discrepancy in the discussion of figure 2.

[Comment] 5. It is not entirely clear to me what was done in the modelling regarding the release of particles and what the green markers correspond to in figure 5. Section 2.1 initially suggests that both RC4 and SPRINTARS use the dust emission scheme detailed in Uno et al. (2004) to predict dust emissions. This is presumably what was used to calculate dust extinction coefficients and AODs shown in the various figures. However, I am confused by lines 9 - 13 of that section (page 14458). What is this particles simulation? Does this refer to the included dust scheme or to an additional tracer released into the model? If so what is the logic behind having some simplified wind-driven tracer? Surely the dust mass mixing ratio in the model is the most relevant tracer to examine. At the moment Figure 5 gives a good overall impression but why not show dust mass or extinction?

[Answer] Yes. The particles simulation is an additional simplified wind-driven trace simulation to understand the dust emission and uplift processes directly and more visually. As referee mentioned, the distributions of the tracers almost agree with those of the dust mass mixing ratio. However, it is difficult to make visible plots with the dust mass mixing because of a great gradient between the surface and free atmosphere (mass

mixing near the surface is much heavier than that the injected dust), and additional contour plots also makes figure 5 more busy. Therefore we would like to discuss the emission and injection processes with the particle simulation. To avoid readers' confusion, we will revise some statements as follows:

[lines 9 – 13, page 14458] 'In addition, to understand how dust particles are injected into high altitudes in the Tarim Basin more visually, we also perform a simplified wind-driven tracer simulation with wind fields obtained from the RAMS/CFORS simulation.'

[line 10, page 14463]'. . . (i.e. the dust over the source regions). To understand the injection process visually, results from the simplified wind-driven tracer simulation are also shown (right panels). Note that distributions of the particles almost agree with those of the modeled dust extinction coefficient. '

[Comment] 6. In figure 4b, plot for 31 May at 70W. The model dust concentration begins to increase above 10km. This seems unusual and it is not there in the other profiles. What is the explanation for that feature?

[Answer] In figure 4a, an upper part of the dust veil branched and was brought up to 14 km. The vertical profile captured this uplifted dust. However, we cloud find no clear reason how this branching was caused.

[Comment] 7. The free troposphere? What altitude range do you consider this to be in your study? It would be helpful to define this in the conclusions (line 24, page 14467) and elsewhere in the paper.

[Answer] We will replace the free troposphere to the upper troposphere along [Comment 8] by referee #2. We estimate the tropopause is approximately 12-13 km from vertical gradient of modeled temperature (lapse rate tropopause: refer figure 4a). Therefore, in this study, we consider that the upper troposphere ranges about 9 to 12 km altitude where strong westerlies are dominant. Also see [Comment 7] by Referee #1. We will add the definition in the conclusion as follows:

[line 24, page 14467] 'The strong updraft can inject the suspended dust to the free troposphere (9-12 km), ' We also improve figure 8 along the revision.

[Comment] 8. From lines 4 - 7 of page 14467 in the conclusions section. I think this needs to be reworded slightly. It is well known that dust interacts with solar and terrestrial radiation. Therefore this point should be written as a general motivation for the study and not a conclusion or implication of the study. This point also applies to lines 12 - 14 of page 14462 (section 3.1). Furthermore, the authors only mention solar radiation but the interaction with terrestrial radiation is of similar importance and interest for dust aerosol.

[Answer] We will revise the conclusions along your suggestion as follows:

[lines 12 - 14 of page 14462] 'This fact imply that the Taklimakan dust can play an important roll in fertilizing the open ocean, providing background dust aerosol and affecting the radiative budget through scattering and absorbing over the region away from the source area.'

[lines 4 - 7 of page 14467: conclusion] 'This fact implied that the dust particles originated from the Taklimakan Desert can fertilize not only the Pacific Ocean but also the North Atlantic, contribute to the background dust, and affect the radiative budget through scattering and absorbing at high altitude (4-10 km) and in the region away from the Taklimakan Desert.'

[Comment] 9. On line 26 on page 14467 in the conclusions section the sentence needs to written in a slightly more careful way so as not to imply that the affect on ice clouds was explicitly studied in this paper. For example, "...the dust veil reached altitudes where it could have affected the formation of ice clouds..."

[Answer] We will revise the conclusions along your suggestion.

[Comment] 10. In figure 6 the visibility and dust mass mixing ratios have the same color scheme so as to allow the reader to evaluate the model with the obs. However,

do the mass concentrations of the color bar actually correspond to the visibility levels in the color bar? Since you have information of dust extinction coefficient from the model would it not be possible to convert this to visibility and actually compare the same quantities?

[Answer] Shao et al. (2003) found the empirical relationships between visibility and Asian dust concentrations by fitting the near surface observations to visibility. We will convert visibility measurements to mass concentrations and re-plot figure 6 to avoid misleading and provide quantitative comparison.

Specific comments on language and presentation

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.

[Comment] 11. It would have been much easier for me if the text had been 1.5 or double spaced. I don't know if this is a requirement of ACP so please take this as a suggestion for future.

[Answer] We apologize for the inconvenience in your review. We will apply your suggestion to the future.

[Comment] 12. Various sentences are written in the present tense and then others in the past tense (e.g. lines 10 -14 in the abstract). Formally the past tense is the appropriate tense to use. For example, "These results imply that the dust veil could have fertilized the open ocean and provided background dust...". This is just one example; there were several others that need correcting through the paper.

[Answer] We will check and revise to the appropriate tense.

[Comment] 13. Grammar. There are many grammatical errors throughout the paper and it would be exhaustive for me to point out each one. I suggest that someone reads through and checks the grammar. Just an example of one thing to watch for is the use of "the". In general we only need to say "the dust" if we are referring to a specific

instance of dust described in a preceding sentence, otherwise is it just "dust", "dust loading" etc.

[Answer] Thank you for your helpful and careful checking of our manuscript. However, before submission, our manuscript was examined by a native speaker. At this time, we will ask a native English speaking American person who is an experienced proof-reader of scientific papers to examine the revised manuscript.

[Comment] 14. I am a little confused by the use of the term entrainment. Technically entrainment refers to elements becoming engulfed from a quiescent environment into a turbulent one. In dust modelling the term is therefore used to describe the process whereby dust particles becoming entrained from the quasi-laminar surface layer (of order centimetres above ground level) into the turbulent boundary layer. In this paper however the term seems to be misused to describe the transport of dust from basin into the free troposphere via upslope winds. Technically, this is transport and dispersion process not an entrainment process. I suggest that the authors reconsider this terminology in the abstract and throughout the paper.

[Answer] We will replace entrainment into injection or uplift in revised manuscript.

[Comment] 15. Line 14 of abstract. Use "lift" rather than "inject".

[Answer] We will improve.

[Comment] 16. Line 23 of abstract. Use "a key" rather than "the key".

[Answer] We will improve.

[Comment] 17. Line 18 of page 14455 (introduction). Are you saying that there are supporting studies or that are no supporting studies? I presume the later but please make this explicit, e.g. "...evidences...are lacking, so there is little support for these previous studies."

[Answer] We will improve the suggested points in the revised manuscript as “However,

direct observational evidences of the Asian dust intercontinental transport to Europe are lacking, so there is little support for these previous studies."

[Comment] 18. Lines 22-24 of page 14455 (introduction). This sentence doesn't make sense. Please re-write, I suggest turning the order of the sentence around.

[Answer] We will improve in the revised manuscript.

[Comment] 19. Line 2 of page 14456 (introduction). Plankton are not emitted, please delete that word i.e. "...can influence the dimethyl suplfide (DMS)...".

[Answer] We will delete the words.

[Comment] 20. Line 4 of page 14457 (introduction). Replace "featured by" with "to emphasize".

[Answer] We will improve in the revised manuscript.

[Comment] 21. It would seem more logical or sequential to present the study of emissions first and long-range transport second, i.e. reverse the order of sections 3.1 and 3.2. If the authors have a logical reason for their choice fair enough but it doesn't come across at present.

[Answer] We will reorganize. In revised manuscript, we will start from brief explanation of the dust veil with figure 2 at the beginning of section 3, then discuss about emission and injection processes with figures 5, 6 and 7 in subsection 3.1, finally show the intercontinental transport with figures 2, 3, and 4 in subsection 3.2. Figure 8 will be shown in the summary as the present manuscript. Also see [Comment 6] by Referee #2.

[Comment] 22. Line 7 of page 14461. Consider replacing "wildly changing" with " sharply changing" or "highly variable".

[Answer] We will improve the suggested points in the revised manuscript.

[Comment] 23. Line 16 of page 14461. Has the word "altitude" been missed out, e.g.

"northern and higher altitude"?

[Answer] Yes. We will correct.

[Comment] 24. Line 15 of page 14463. We can not tell that the low pressure is generating we only see its location in the figure. Please re-write e.g. "A low pressure generated...and its location is shown in Figure...".

[Answer] We will improve the suggested points.

[Comment] 25. In general some of the figures are a little too detailed and compact leading to very small fonts and small area of each individual graph or cross-section. This makes some of the plots difficult to read and digest. It also leads to rather lengthy figure captions, which then add to the difficult of quickly understanding what has been shown. This is especially true for Figure 2, 4 and 5 and 7. I would suggest doing something to simply these plots or break them up into a greater number of figures so that each plot is bigger and easier to read. One specific example would be to avoid plotting the CR4 domain boundaries in Figure 2b. This is not necessary and it is confusing having the same color as the trajectories.

[Answer] I apologize that our complicated figures made you confused. We will check and re-organize all figures, and re-write figure captions along your suggestion. Figure 2: We will delete the RC4 domain and make font bigger. Add new figure focusing 21 May-190843 path. Figure 3: We will make font bigger. Figure 4: We will make font bigger and remove models' wind speed and potential temperature from the vertical profile plots. Figure 5: We separate OMI AI (horizontal distribution) and results of the particle simulation (vertical cross-sections) into two new figures. Figure 7: We delete middle panels where modeled wind and theta at 800 hPa, and make font bigger.

[Comment] 26. Line 19 of page 14464. The SYNOP reports rain at all three stations, they do not report rain over the whole basin; that is simply an inference. Try "The SYNOP reported rain at all three stations indicating a widespread rain event within the

basin."

[Answer] We will improve in the revised manuscript.

[Comment] 27. Line 28 of page 14464. Do you mean that the wind changes direction at higher altitudes? Currently the sentence doesn't quite make sense.

[Answer] No. The state describes how the strong 'surface' wind, which causes the extensive dust storm and subsequent uplifting of dust particles, blows into the basin. We will re-write the statement to avoid misleading as follows:

"The Pattern 1 is characterized by an easterly mesoscale surface wind, which is separated from the synoptic-scale cold westerly. The surface cold wind changes its direction to westward after going around the eastern side of the Tian Shan Mountains."

[Comment] 28. Line 21 of page 14465. Consider re-writing as "On 22 May the eastward transport of the dust increases in speed...".

[Answer] We will re-write in the revised manuscript.

[Comment] 29. Line 21 of conclusions (page 14466). These are "processes", not "procedures". The word "procedure" is inappropriately elsewhere in the paper. A procedure refers to a sequence of tasks that a person or computer carries out. The term is not relevant to discussion of what occurs in the atmosphere.

[Answer] We will improve the suggested points in the revised manuscript.

[Comment] 30. Is "d" a recognised scientific unit. For clarity I suggest using "day" i.e. km / day.

[Answer] We will correct.

[Comment] 31. Line 14 of page 14467. Consider re-placing "formed, bringing" with "brought" and omitting the word "further".

[Answer] We will replace in the revised manuscript.

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