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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.

Anonymous Referee #1

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General comments

This paper uses numerical modelling and a variety satellite and ground-based observations to study a dust veil originating from the Takliman Desert. The veil of dust travelled a remarkable distance around the globe and was still discernible in CALIOP scans over the North Atlantic some 10-12 days after the initial uplift. This kind of long-range intercontinental transport is of significant interest in the scientific field but few individual events have been studied in detail. The unique orography surrounding the Takliman desert plays an important role in lofting the dust to high altitudes thus enabling the

long-range transport of the dust within the free troposphere. In particular updraughts generated on the southern slope of the Tamin basin promote the lofting of dust to high altitudes within the troposphere. On this case the dust is shown to reach altitudes of 10km. This phenomenon makes the Takliman Desert a particularly effective source of dust for the mid to upper troposphere where it may play a role in ice nucleation. These interesting aspects are already known but there have been few modelling studies specifically demonstrating the process and studying an individual event in detail. This paper is therefore interesting and novel and makes a significant contribution to the understanding of global dust emission and transport.

The paper focuses mainly on the dynamics controlling the dust uplift and transport rather than the properties of dust or its interaction with radiation and clouds. This is fair enough. The meteorological conditions, and the way that these interact with the unique orography of the region are important factors in making the Takliman Desert such an effective global dust source. Therefore it is certainly relevant to focus the paper on the dynamical processes responsible for the emission, lofting and transport of dust and to evaluate the performance of the model on its ability to capture these processes. The models seem to capture the emission, lofting, and long-range transport of dust fairly well despite the inevitable limits on horizontal and vertical resolution. However, the concentration of dust in the models seems too low and therefore estimates of dust deposition to the ocean may not be accurate.

In summary I judge the significance of this paper to be good. It explores an interesting issue and adds some understanding of just how high and how far such dust events can go and what kind of meteorological conditions can cause such events. The scientific quality is also good as it makes use of very appropriate state-of-the-art observations and models to examine the dust event. The paper also makes good references to previous associated work. The presentation quality is fair. Although there are a number of technically advanced figures in the paper the amount of information and detail is a little over-whelming on some of them. This makes the results a little difficult to digest.

Significant revisions will also be necessary to the text to improve the use of language and English grammar (see specific comments below). My overall recommendation is that this paper should be accepted subject to minor revisions.

Comments on scientific content

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. 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. 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. 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. 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? 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? 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. 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. 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...". 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?

Specific comments on language and presentation

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. 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. 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. 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. 15. Line 14 of abstract. Use "lift" rather than "inject". 16. Line 23 of abstract. Use "a key" rather than "the key". 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." 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. 19. Line 2 of page 14456 (introduction). Plankton are not emitted, please delete that word i.e. "...can influence the dimethyl suplfide (DMS)...". 20. Line 4 of page 14457 (introduction). Replace "featured by" with "to emphasize". 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. 22. Line 7 of page 14461. Consider replacing "wildly changing" with " sharply changing" or "highly variable". 23. Line 16 of page 14461. Has the word "altitude" been missed out, e.g. "northern and higher altitude"? 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...". 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. 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." 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. 28. Line 21 of page 14465. Consider re-writing as "On 22 May the eastward transport of the dust increases in speed...". 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. 30. Is "d"

a recognised scientific unit. For clarity I suggest using "day" i.e. km / day. 31. Line 14 of page 14467. Consider re-placing "formed, bringing" with "brought" and omitting the word "further".

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 14453, 2009.