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Atmospheric Chemistry and Physics Discussions

Interactive comment on "An improvement on the dust emission scheme in the global aerosol-climate model ECHAM5-HAM" *by* T. Cheng et al.

Anonymous Referee #1

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This paper is to improve the soil dust emissions in the ECHAM5-HAM model by using the up-to-date soil properties. It is a good start to improve the dust aerosol prediction in the ECHAM5-HAM and the results are encouraging. Though there is no significant new scientific knowledge in this paper as the improved parameterization and soil data have been used in other global soil dust models, it is interesting to see the application of these data in the ECHAM5-HAM model. It is a difficult job to put all these improvements together and make it work. This review is to point out some loose ends that need to be addressed before its publication.

(1) Some of the statements need reference to support. For example, in page 13961,

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lines3-5, this is a very general statement about the model performance of all models. You need to use references to support it. I don't think this statement is true.

(2) Since this is a paper on the improvements of the dust emission schemes, it is useful to briefly describe the original scheme used in the system. The paper in the current form is difficult to understand with respect to the significance of these improvements to the dust emissions. Critical things missing in the paper are how the horizontal and vertical fluxes were computed, how the size distributions of the fluxes were assumed.

(3) In page 13967, a parameter of wind stress correction factor was introduced. Yet, there was no description of this parameter and its connection to the improved scheme. According to Table 2, this is a very sensitive parameter to the global dust emission fluxes. Is the wind stress correction factor a constant all over the globe? If this parameter can change the flux so dramatically, you may not need to improve the soil properties such as the roughness length and soil texture. Please explain.

(4) Soil dust aerosol has a large size distribution spanning from sub-micron to hundred microns in the atmosphere, depending on the locations. In this paper, only two modes of dust aerosol were used with rather small mass-median radii: 0.37 um and 1.75 um. This classification may apply to down wind regions far away from the source areas but definitely fail to capture the source area dust size distributions. What is the consequence of this assumption on the dust dry deposition and wet deposition in the source areas? Would the under-estimate in Figure 4 have anything to do with these size classifications?

(5) In page 13969, the section title "validation of East-Asian dust emission" seems not appropriate as (1) there are no direct measurements of dust emissions in East Asia; (2) the comparison between model predicted and observed surface concentration does not validate the emissions. Suggest using "evaluation".

(6) In a number of places, you have mixed the "threshold wind velocity" with the "threshold wind friction velocity", e.g. in page 13964, line 17.

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