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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.

T. Cheng et al.

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

-We rewrote these sentences and added some references.

(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.

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-We added a short description of the original scheme in the 2nd paragraph of section 2, in which, the setup of size distributions and calculation of horizontal and vertical fluxes are also stated.

We modified the new "conclusion" section to clearly indicate the significance of improvements.

(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.

-We added some explanation on the tuning parameter in the 1st paragraph of "results" section. Some discussions on the use of tuning parameter are included in the new "discussion" section.

(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?

-The dust emission scheme in ECHAM5-HAM applies 192 internal size classes in the flux calculation, which ranges from 0.2 to 1300 micrometer in diameter. The setup of size distribution could well cover most of the moveable particles in source areas (i.e., Marticorena et al (1995a) mentioned that the soil aggregates larger than 1-2 mm are erodible only by extremely high winds). Fluxes in the 192 classes are integrated

ACPD

7, S7880–S7882, 2007

into the two dust modes with radii of 0.37 and 1.75 um for the subsequent advection and deposition processes. The super-coarse mode dusts are ignored in the emission calculation because they settle down very quickly due to large masses. We added the above contents in the 2nd paragraph of section 2.

Both dry and wet deposition rates are proportional to the tracer mass mixing ratio. The neglect of super-coarse mode particles may reduce the emitted masses (Stier et al., 2005) and so do the deposition rates in or near the source regions, but will not have significant influence away from the sources. Moreover, no current evidence shows that the ignorance of super-coarse mode particles is responsible for the underestimation in Figure 4.

(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".

-We changed the title of subsection as the reviewer suggested and added some detailed analysis on the 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.

-We corrected them.

Interactive comment on Atmos. Chem. Phys. Discuss., 7, 13959, 2007.

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7, S7880–S7882, 2007