Geosci. Model Dev. Discuss., 7, C1891–C1897, 2014
www.geosci-model-dev-discuss.net/7/C1891/2014/

Geoscientific Model Development Discussions
Open Access

Interactive comment on "Testing the performance of state-of-the-art dust emission schemes using DO4Models field data" *by* K. Haustein et al.

Anonymous Referee #2

Received and published: 3 October 2014

General:

The authors developed a box model framework for three different dust emission modules. This work exposes the shortcomings of all these three modules in reproducing measured dust emission fluxes from a field campaign in Botswana in 2011. The authors show that the simulated horizontal (vertical) fluxes are several orders (one order) of magnitude too high. They ascribe the differences to crusted surfaces that are not represented in the emission schemes and to dust entrainment and conclude that both processes should be included in future emission schemes.

The emission is the first step of the entire dust cycle and a good reproduction of the atmospheric dust load and its deposition in weather and climate models crucially depends on the dust emission. The here presented box models of different emission

Full Screen / Esc
Printer-friendly Version
Interactive Discussion
Discussion Paper

schemes and their systematic investigation will help to understand the critical processes that need to be captured by models in order to simulate the dust cycle in a realistic way. The description of the box model development is in general comprehensible but needs some clarification (see below).

My major criticism refers to the comparison with the measurements. Although the field campaign might provide one of the best data sets in terms of horizontal and vertical mass fluxes, emission fluxes were only measured on very few (five?) days. How representative are these measurements? How about measurement errors? One needs to be careful in drawing general inferences from these measurements about the overall performance of the dust emission schemes. Furthermore, there is the question on the representativity of the field sites. The authors mention that the crusted surface at Sua Pan "can be found in many dust source regions" (P5743) (where?) but on the other hand, that the soil combustion is different to "many other desert soil samples" (P5764). Please include a discussion of this in sections 4 and/or 5.

My recommendation is to publish this paper in GMD after addressing the following comments. To make the paper fit better within the scope of this journal, the focus should be little more on the development of the box models and on potential future applications/extensions while section 4 needs to be condensed (see major comment below).

Major comment:

Figs. 5, 7 and 8: The conclusions drawn from these figures mainly refer to the dependency of u*thr on the correction schemes in general, on the moisture and on the roughness. In total 18 panels are too many for this analysis. The question is: What is the "full range of observed u*thr values" (P5764, L12) and what do the emission schemes simulate? In my opinion, the observed u*thr values can be read from Fig. 7 for different conditions with regard to soil moisture and roughness. The median for the different clusters (Obs z0>1cm, . . ., Obs vsmc=0-3%) would represent a good estimate

GMDD
7, C1891–C1897, 2014

for the upper limit of u*thr under these different conditions. For the emission schemes u*thr can be calculated for all the different conditions explicitly, i.e., the u*-value at the leftmost end of the lines in these figures.

The same could be calculated for all sites separately (Figs. 5 and 8) and compared with the respective u*thr of the various emission schemes and all experiments. The comparison of these values of u*thr can then be done in a much more concise way without showing 12 panels in Figs. 5 and 8. Fig. 7 can stay as it is, as it gives a nice overview on how the observed and simulated fluxes depend on u*, z0 and w.

The distinction of different soil classes as given in Figs. 5 and 8 can be skipped as it not really discussed in the text.

Some of the minor comments below might become superfluous when the figures are changed and section 4 is partly rewritten by addressing this comment.

Minor comments:

- P5741, L17: What does “undisturbed” mean here?

- Equations in general: Please be more precise in the description of all the variables in the equations. Some examples: How is u*dry defined in Eq. 1? It is not the one from Eq. 7, I guess, as this one is “adjusted” (P5748, L23); What is the definition of w_s in Eq. 5?; What is rho and g in Eqs. 7 and 6?

- P5748, L11f: Please clarify the units of the fluxes and of alpha. HFLUX is given in g/m/s and VFLUX in g/m2/s (Figs. 2-4). Is alpha defined as HFLUX/VFLUX, as I assume from “horizontal-to-vertical-mass-flux-ratio? Then the unit of alpha is m or cm but not cm-1.

- section 3.4: The first paragraph confuses me (reference to Sect. 4.2 should be 4.3, I guess, and 4.2 is the “second step”). It should be skipped here as it is repeated in the beginning of section 4 where it is placed better.

GMDD

7, C1891–C1897, 2014

- P5753, L10: Just remind the reader what is done in these experiments: "...and 5a, i.e., all correction schemes are switched on, using the schemes MB95, SH04 and AF01.". Such a reminder could be included again later, e.g. on P5757, L5f.

- P5753, L24f: According to Table 2, (b) and (c) are mixed up here.

- P5754, L7: Which "box model components"?

- P5755, L6ff: The "peak shear velocities" are the same in Figs. 2-4 as this are the measured ones. What is meant here is the temporal agreement between observed and simulated fluxes, right? Otherwise, "particularly for MB95" would make no sense.

- P5755, L23: I would say even 4 orders of magnitude (10E3 against 10E7).

- P5755, L24ff: I disagree. There are black dots (=observed vertical fluxes) in Fig. 2f and 2j. My conclusion would be that the soil was not too wet for dust emission (observed HFLUX and VFLUX > 0) but that the moisture threshold in the model was exceeded, inhibiting dust emission. Please clarify.

- P5756, L4. "Fig. 5" needs to be Fig. 3!

- P5756, L6f: I do not see any drop in soil moisture in Fig. 3b. From the caption I assume that the soil moisture is above 0.05 kg kg-1 when no data are shown. Is this true? Please be more precise in the caption.

- P5756, L7-14: This is over-interpreted in my opinion. Both schemes produce extremely unrealistic horizontal fluxes (at most sites very much too often and too strong). I think the only conclusion from these figures can be that sometimes the one scheme performs better (MB95 at site I04) and sometimes the other (SH04 at site J11). I do not see the "advantage" of SH04 for site D10: The VFLUX is almost the same as with MB95 but the HFLUX is even worse than with MB95. The sentence in line 12 could be skipped as none of the schemes produces anything at L05. The question arises if Fig. 3 could be reduced to show only the panels for sites I04 and J11.

GMDD

7, C1891–C1897, 2014

- P5756, L15f: I do not understand why there are differences in the left panels of Fig. 3 and Fig 4. Reading Table 2, I would expect the same HFLUX for experiments 4 and 5. The left panels of Fig 4. could be skipped then. Please clarify.

- P5756, L17f: Point (1) kind of disagrees with the sentence in line 10f “Modelled emission frequency...”.

- P5756, L21f: I disagree that the “opposite is true...” which would mean that simulated fluxes are lower than the observed ones. At the few days with observed VFLUX at sites B03, I04 and J11, the simulated VFLUX fits quite well. But still there are many many days with no VFLUX observed but simulated, meaning an overestimation. Please correct this.

- Section 4.2 can be drastically shortened in my opinion. The conclusion from these two pages of text is that the threshold shear velocity strongly depends on the moisture and roughness.

- P5757, L16: Please mention that “emission fluxes” always refers to the vertical fluxes. This was sometimes confusing me when reading the paper for the first time.

- P5757, L18ff: How can one conclude on “soil and surface features” from this figures?

- P5757, L28: “sand transport models”

- P5758, L4: Observed values are not limited, better: “u* never exceeds 085...”.

- P5759, L28: How can I see this from Fig. 6?

- P5760. L6: One can hardly read values of alpha from Fig. 6. See my recommendation for Fig. 6 below.

- P5760, L14: What is meant with “direct entrainment”? From the surroundings? Would this be included when running a full 3D model allowing for dust transport from surrounding grid boxes?

GMDD

7, C1891–C1897, 2014

- P5761, L10: Skip “experiment” after “(JADE)”.

- P5762, L2f: The values for w in Fig. 7 are 1, 5 and 10%. Why not 1, 8, 16 to represent the observed range better?

- P5762, L18f: I would say about 50% of the red dots have a gray circle (w>6%) and values of u<0.4 m/s. This is not “occasionally”.

- P5764, L3: How can the observed u*thr be read from Fig. 8? This question is more general and can be asked for all figures 5, 7 and 8. Please give an explanation.

- P5764, L13: Should it be “fluxes < 0.001 mg m-2 s-1”? But why are these observations “questionable”?

- P5766, L12f: The conclusion could be that it might be a worthwhile effort to incorporate a sub-grid scale emission scheme in climate or NWP models.

Figures:

- Fig. 1: What is the difference between the green and the pink region? Both are labeled the same in the caption, so they should have the same color.

- Figs. 2-4: Please mention in the caption that u* and w refer to the right ordinates in the respective panels.

- Fig. 6: It would be better to plot alpha directly (against HFLUX). Then one could actually read the values of alpha from the figure. In the present form one needs to read VFLUX and HFLUX of a specific dot and divide them to get alpha. Why is the time color-coded here; is it important? I recommend to plot all sites in one panel with different colors for single sites. Values for Exp 1a might be skipped as alpha is constant.

- Fig. 7: “vsmc” never appears in the text and should be changed to “w” in the legend. There is no legend entry for the dark red dots. The caption says “black and dark grey open circles”. What is the difference between them? I cannot distinguish between them in the figure.

GMDD

7, C1891–C1897, 2014

- Fig. 8: The differentiation for the soil types is not necessary here as they are not discussed in the text. Furthermore, I cannot see any thin grey lines in the figure.

Interactive comment on Geosci. Model Dev. Discuss., 7, 5739, 2014.

GMDD

7, C1891–C1897, 2014