Geosci. Model Dev. Discuss., 4, C1507–C1511, 2012
www.geosci-model-dev-discuss.net/4/C1507/2012/

Geoscientific
Model Development
Discussions

Interactive comment on "Importance of the surface size distribution of erodible material: an improvement of the Dust Entrainment And Deposition DEAD" *by* M. Mokhtari et al.

Anonymous Referee #3

Received and published: 6 February 2012

The paper introduces soil texture variability and more complexity into the Dust Entrainment and Deposition scheme (DEAD) within the atmospheric model ALADIN. 0-D sensitivity experiments for different types of soils and a 3-D simulation of a dust storm are presented. The model is evaluated with a few observations (AOD and dust surface concentration) during a dust storm in Northern Africa and compared to the previous model set-up.

General comments:

While there are some interesting aspects in the paper, such as the attempt to use soil texture information in the dust emission scheme, there seems to be several inconsis-

tencies in the development of the scheme, or at least the text is rather confusing and needs further clarification. On the other side, to my opinion, the results shown do not sufficiently support a main conclusion of the paper, which is the better behavior of the developed scheme compared to the previous set-up.

An English native speaker may revise the text.

Detailed comments:

- Throughout the paper, the terms "soil" and "surface" are used indistinctively. Please be precise in the use of both terms. Dust emission is affected by both soil and surface characteristics.

- Emission scheme:

It is not clear how the relative surface of the soil particles is used in the scheme. Why do you calculate the average relative surface? How is this used? Is the E parameter in equation 7? If yes, E is defined as the fraction of erodible surface... why the fraction of erodible surface is determined by the average relative surface of the particles in the soil? If not, where is the relative surface area used in the scheme?

Equation 7 is from White (1979) and used in Marticorena and Bergametti (1995), so please correct/add the reference accordingly. The originality of Marticorena's paper partly resides on determination of the Reynolds number (in the threshold velocity equations) based only on the particle diameter (since the relationship is implicit) and also on the specification of the horizontal to vertical flux ratio.

Since you assume a particle diameter of 75 μm in the soil for the calculation of the U*t, the terms (1+U*t/U*)(1-U*t^2/U*^2) are not dependent on Dp and can be out of the integral in equation 7. Finally the integral of the relative surface over particle size should be always 1. Which means that equation 7 becomes the one used in Zender et al. (2003), and (strangely) means that no explicit influence on soil particle size on saltation is taken into account as claimed in the text. My understanding (although it is not clear in the text) is that the soil texture information in equation 7 is introduced through the parameter E, which is defined as the erodible surface (represented in this case by the relative surface area). As mentioned before, why should the relative surface area represent the erodible surface? The main question here is why the authors don't use the soil information to explicitly model saltation in equation 7? Why do they use a U*t independent of the size of the saltator. Why is equation 7 is presented in such a way given the comments above?

Alpha in equation 8 is not introduced in Shao et al. (1993) but in Shao et al. (1996). However, from the text it appears to be constant (no spatial variability) since all the parameters are constant (Beta, gamma, g, U*, Dd, Ds). If I am wrong with my understanding of the text, the text should be clarified. If it is constant, what is the sense of introducing a constant alpha?

Which aerodynamic roughness lengths are used in the model for the drag partition correction? Are these satellite estimates, or derived from the atmospheric model used in the surface layer scheme?

In summary, the previous aspects related to the emission scheme need to be clarified and justified by the authors. Whether the emission scheme is more or less complex, the options used need to be clearly explained and consistent, and the assumptions justified.

What are the three modes following the AMMA parameterization? Is it table 3? It should be referred in the text.

O-D simulations:

In table 4, the different set-ups of the experiments are listed. When it comes to EXP4 the authors define the dust source intensity as the relative surface for each population. Again this is confusing. In section 2.2, line 13 you talk about average relative surface area as the potential dust source emission and in equation 7 you talk about

fraction of erodible surface. In table 4, it is relative surface for each population. Please homogenize and explain.

Concerning the 0-D experiments, how can EXP4 have different threshold friction velocity if, as you claim in section 2.3 (lines 20 to 24), you assume particles of size 75 μm?

3-D simulations

As stated in the abstract, the goal of the paper is to develop a global mineral dust emission parameterization. The main conclusion is that the developed scheme improves the previous one. However, I have two main concerns.

First, the results are based on a single event and a specific region. If the aim is the global scale, the evaluation may be global. Also it is difficult for me to accept that the comparison of different model set-ups for a specific event and a specific region, we can conclude than a scheme is better than the other. Only with longer term simulations covering at least a seasonal cycle and different regions (even within the North African domain) one may conclude that the developments represent a improvement.

On the other hand, I have some concerns with the evaluation performed and the discussion of the results:

- The evaluation is qualitative. There are no skill scores and is quite subjective. What are the biases, RMSE, and correlation of each experiment?

- The period of March is affected by biomass burning in the Sahel region. The total AOD measured by the sun-photometers may include contributions from these aerosols, so the comparison of the different experiments and the conclusions reached may be affected by it.

- The authors do not explain how and to which extend the different experiments are tuned (parameter a in equation 7). A different tuning can derive into different results. This is even more certain when the models are tuned with observations in one region and for a specific event. How were the experiments tuned? For example, in figure 10 it seems to me that EXP_THR is strongly underestimated in all the stations used. If the tuning constant is increased, the results would be much closer to the other experiments.

- It is argued that EXP3 and EXP4 give reasonable results for the AOD (Figure 10) but EXP3 overestimates the surface concentrations in Banizoumbou and Mbour (Figure 12). If we look in detail into the comparison of both experiments with the AOD in Banizoumbou and Mbour (Figure 10), we clearly see that also there EXP3 overestimates in the same way as in the case of surface concentrations (in the period 9-15 in Banizoumbou and 9 to 10 in Mbour). With this, I finally come back to the previous tuning issues and the influence of other aerosol. Depending on the tuning, the results may be different; also the effect of biomass burning is not clear. These effects can change the discussion of the results and the conclusions of the paper given the low amount and specific locations of the observations used in the evaluation.

Figures:

- I believe that Figure 1 is not needed since it is very general and well known. - Why is MODIS used in Figure 9? There is no data over sources and there are other satellite estimates providing information there (OMI and MODIS Deep Blue for example). Why not to show the differences between EXP3 and EXP4 in a map? It would help to understand the potential different patterns from both experiments.

Interactive comment on Geosci. Model Dev. Discuss., 4, 2893, 2011.