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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Simulation of the mineral dust content over Western Africa with the CHIMERE-DUST model from the event to the annual scale" *by* C. Schmechtig et al.

C. Schmechtig et al.

marticorena@lisa.univ-paris12.fr

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Reply to Reviewer 1

1/ The authors present a comprehensive overview of their dust model's application over Africa. In particular, it was refreshing to see a discussion of the verification procedures using surface PM10 as well as satellite based AOD and AI observations from seasonal means to the performance for individual events.

Reply : As noticed by the reviewer, the key point in the comparison presented here is the complementary use of AOD and surface concentrations to test the model and the different scales of comparison.

2/ However there is one aspect that deserves further discussion. The key to the model's performance was the empirical adjustment of the surface wind prediction to match the observations at Faya-Largeau. The authors note that the equation is only applied for winds higher than the erosion threshold. This suggests that the original estimate of the friction velocity is used to determine if a grid cell has dust emissions and then the wind speed adjustment is applied to compute the emission amounts. This procedure should be clarified.

Reply : This is now clearly stated in the manuscript: our assumption is that the main bias in this region comes from the ECMWF surface wind and not from the estimated erosion threshold. Indeed the same approach has been used to estimate the erosion threshold over the whole Sahara and the estimation only depends on surface features and not on the surface wind fields used for the simulations. To be clear, there is no "tuning" of the erosion threshold to compensate any errors on the wind fields. What is done is an adjustment of the surface winds in a region where it has been demonstrated that they are significantly underestimated by global models.

3/ Because the Marticorena-Bergametti emission equation uses the friction velocity as the key meteorological variable, the authors need to connect how the adjusted wind speed is turned into a friction velocity used in the emission equation. The relationship between friction velocity and wind velocity also involves a vertical stability term. The authors should also note whether they used the friction velocities computed by the ECMWF model or computed their own values because the problem may not be the surface wind prediction but how the friction velocity is computed.

Reply : As described in M97, the wind friction velocity is estimated from the 10m wind from ECMWF and using the mapped Z0 using a logarithmic wind profile, i.e. an assumption of neutral conditions. This is also described and tested in Darmenova et al.2009. As mentionned above, there are numerous evidence that the surface wind velocities are underestimated by global models in the region of the Bodélé Depression, as pointed out, for example, by Koren and Kaufman (2004), but also in the comparison

with the measurements in Faya-Largeau (Chad) shown in figure 2. Obviously, the computation of the wind friction velocity can also be a problem for dust simulations, however if the surface winds are not realistic, simulated dust emissions cannot be correct whatever the way the wind friction velocity is computed. Except with an" a posteriori" tuning of the simulated dust emissions. But whatever the limitations in our computation of the wind friction velocity (neutral conditions, empirical correction of surface winds against measurements), there is no tuning of the simulated dust emissions.

4/ It may be useful to evaluate the ECMWF model gustiness prediction as a surrogate for correcting the 10 m wind velocity.

Reply : Theoretical equations sustaining dust emission models are derived from saltation models and are parameterized as a function of the wind friction velocity. This is due to the fact that the main driver of Aeolian erosion is the wind shear stress (proportional to the square of the wind friction velocity). To use the gustiness factor, one needs to know how it relates to the wind shear stress. Otherwise, it is an additional tuning factor of the simulations.

5/ There is no disagreement that gridded these global numerical weather prediction models will underestimate the magnitude of peak events, but the discussion of the surface wind correction deserves more attention if the intent of the paper is to raise confidence that the model can be applied at other locations and times without requiring a empirical correction factor.

Reply : This remark is interesting, since we do not really think that no empirical correction is required at other locations and times. We simply note that without any correction elsewhere that in the region of Bodélé, the model gives reasonable simulations over the Sahel. This let us think that if any other correction is required, it is not as large as in Bodélé or it concerns source areas that does not impact the dust load over the Sahel.

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 8027, 2011.