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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "Retrieving the global distribution of threshold of wind erosion from satellite data and implementing it into the GFDL AM4.0/LM4.0 model" *by* Bing Pu et al.

Anonymous Referee #2

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This is an interesting paper that produces the first estimation of the global distribution of threshold wind speeds for wind erosion (dust aerosol emission). They do so by combining a calculation of the frequency of dust events per grid box with a probability distribution of wind speeds per grid box from a reanalysis product (NCEP/NCAR). They then implement their estimation of threshold wind speeds into a global model and study the results relative to a control run with a globally-constant threshold wind speed. The paper is overall well-written and easy to follow, and the results could be important because they could help advance dust models beyond the use of a globally constant threshold friction velocity. However, I think there are some important issues with the methodology, the interpretation of the retrieved threshold wind speeds, and with inter-

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Discussion paper

preting the results from the global model. The paper would need substantial revisions. Comments follow below.

Main comments:

- A major weakness of the methodology is that it equates high dust AOD in a gridbox with the occurrence of dust emission. This causes problems in their methodology because it causes advected dust to be interpreted as emitted dust, and thus results in an underestimation of the dust emission threshold. Since there are large differences in advected dust between regions – for instance areas in major dust regions are bound to be more affected by advected dust – this problem could cause potential biases in the retrieved threshold wind speed. Although the authors commendably acknowledge the problem (e.g., on line 340-2), the magnitude of this bias is not investigated. And unfortunately, without a reasonable analysis of the magnitude of this bias, I do not think the authors can conclude that the threshold wind speed in the Sahel is actually lower than in Northern Africa. And similarly, it is not clear that the lower threshold in the major source regions (e.g., the Sahara) than in the more marginal regions (e.g., the US) is real, or is a result of this bias. In fact, both these results are consistent with the anticipated effect of this bias, as the authors acknowledge for the Sahel. Therefore, the authors need to add an analysis that reasonably bounds the effect of this bias. Perhaps the authors could analyze the wind speed threshold in different regions, conditional on the DOD in the surrounding regions, in order to try to quantify and bound this bias?

- I also think the interpretation of the differences between threshold wind speed must be improved. Of relevance here is that wind speed itself is not the main explanatory variable for dust fluxes. Rather, this is the wind stress on the surface as quantified by the friction velocity, which is linked to the 10m wind speed through the aerodynamic surface roughness. There are strong experimental constraints on the threshold friction velocity above which surface particles become mobile and dust emission starts (e.g., Shao, 2008). It is therefore very relevant what the NCEP/NCAR surface roughness in the different source regions is: do differences in the roughness between source regions explain the differences in the threshold wind speed? Are threshold wind speed variables substantially correlated with the roughness values used in NCEP/NCAR for each grid box? The authors can also use the surface roughness to determine the distribution of threshold friction velocities for the different regions, which is more fundamental and thus more useful to the community. Another important consideration that follows from this above concern is that, since it's the friction velocity (and wind stress) that drives dust fluxes, the roughness used in GFDL should match the roughness used in the NCEP/NCAR reanalysis. Is this the case?

- Similarly, the authors should investigate differences in other parameters that determine the threshold friction velocity (and 10m wind speed), namely soil moisture, vegetation, and soil texture. If the authors can provide plausible physical reasons for the variations between the threshold wind speed between the regions, that would also help alleviate the concern that their results might be primarily driven by biases arising from using high DOD as a proxy for dust emission (previous comment).

- The rationale for implementing the retrieved threshold wind speed into the GFDL model is not made very clear in the paper, but I assume it is to try and show that using the retrieved threshold wind speed improves GCM simulations of the dust cycle. If so, although the analysis presented is interesting and draws on a commendably wide variety of data, it has some important problems that need to be addressed. First, the proportionality constant in the dust emission equation (Eq. 3) is not constrained by physics (i.e., there's no reason it should be 0.75e-9 ug/s2/m5 instead of 1e-9 or 0.1e-9 ug/s2/m5), and presumably C was set at an earlier stage by maximizing agreement against observational data. Therefore, the fact that using the retrieved threshold wind speeds reduces the underestimation of DOD and dust concentration is not an indication that the retrieved threshold wind speeds actually improve the realism of the model simulation. You would get the same effect simply by increasing the (unconstrained) value of C. The authors should therefore compare apples to apples by tuning the simulations to the same global loading or DOD, and then compare against the AERONET and other data. This is especially important because using the retrieved threshold wind speeds results in a very large (and again, arbitrary, because C is unconstrained) increase in emissions by a factor of $\sim$4 (Table S2).

- Another problem with the model comparisons against data is that its interpretation requires more rigorous statistics. Keeping in mind the previous comment that the absolute values of DOD and concentration are arbitrary because the emission proportionality constant is unconstrained, the authors would need to show statistically significantly increased correlations between the model and data in order to conclude that the retrieved threshold wind speeds improve the model realism. Otherwise, I do not think the conclusion in the abstract and the paper that the retrieved threshold wind speed improve the simulation can be supported. Correlations are reported in Figs. 4 and 5, and I'm guessing that the improvement is large enough that it's statistically significant, but this ought to be shown. Correlations are not currently reported for the varied results in Figs. 8 – 14, so should be added.

Other comments:

- Line 2: I'd suggest saying "many" instead of "most", as I believe most models at least account for the effect of soil moisture on the threshold wind speed.

- Do you have a sense of how sensitive your results are to the particular reanalysis product used?

- Line 304: it seems hard to imagine that snow cover of 0.2% would prevent or substantially reduce the occurrence of wind erosion. Please provide support for this assumption.

- Line 311-2: "soil moisture ranging from 1.01 to 11.2 kg kg-3"; the units here are incorrect, and I think the number is much too high if the intended unit was kg of water per kg of soil.

- Line 317: I don't think it makes sense to only pick out the daily maximum surface wind speed when you have wind speeds at 6-hours resolution. You could either argue that the DOD is a product of emission that occurs over a longer time period and thus use winds at all time steps, or you could argue that you are using DOD as a proxy for emissions in the moment and thus use the wind speed closest to your DOD observation (presumably noon since overpasses are at 10:30 am and 1:30 pm). But using the daily maximum does not make sense to me.

- The authors use a threshold DOD of 0.2 over the major source regions of North Africa, the Middle East, etc, which is consistent with previous work in Ginoux et al. (2012). But they use a threshold DOD of only 0.02 in lesser source regions such the US, South America, etc. This is a very large difference of a factor of 10, and seems rather arbitrary. Could the authors either provide an analysis of the sensitivity of their results to this choice or use the actual frequency distribution of DOD in the different source regions to inform these thresholds?

- Section 3.2.3: How are you obtaining AERONET data as a gridded product since data density is so sparse in most dust source regions?

- It's not clear to me whether the control run accounts for the effects of soil moisture on the threshold wind speed or whether it truly uses a constant threshold wind speed, regardless even of soil moisture content. Could you clarify?

Editorial comments:

- Line 57: Since wind speeds are a function of height, please note what these wind speeds refer to.

- Since the methodology is quite involved and lengthy, I recommend you provide an overview of your methodology in a paragraph at the beginning of section 2 to make the paper easier to read.

- 182-184: Please provide more info or a citation to a peer-reviewed paper here for the reader to understand how LAI is calculated.

- Line 254-5: This is a common assumption in using the dust concentration data, so you could support this by citing precedent in previous studies.

- Section 3.3: I think this section would be placed more logically before the case study.

- Figure 8: since the data here span 3 orders of magnitude, providing statistics in linear space is not very meaningful as it weighed heavily toward the large concentration data. Please provide statistics in logarithmic space.

- Fig. 14: What is the bin spacing on the horizontal axis? The reader needs that to interpret the percentage given on the vertical axis.

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