Review of the ACPD manuscript „Retrieving the global distribution of threshold of wind erosion from satellite data and implementing it into the GFDL AM4.0/LM4.0 model“ by Pu et al.

The article by Pu et al. describes a new data set for the threshold wind velocity for dust emission and shows the impact on dust aerosol simulated with the GFDL model. The authors used a comprehensive collection of observational data to approach the problem. In principle, the contribution is relevant to the field, since modeling dust aerosol is fraught by uncertainty. I have, however, concerns that should be address prior to publication of the article. These are the unclear description of the method, the lack of an uncertainty assessment for the retrieval, as well as the need for a comparison to independent data and citing of relevant literature. In the following, I provide more details.

Main comments:

1) The description and uncertainties of the method are unclear.
The article suffers from an unclear description and partly missing information on the retrieval technique. Moreover, the value of the article would be substantially improved when the uncertainty in the retrieval would be quantitatively assessed. The many threshold criteria in the retrieval currently cast some doubt on the robustness of the retrieval when these values would be slightly changed.

2) The article needs more comparisons to existing works.
The current article does not acknowledge other existing treatments of the threshold of wind erosion for global models. For instance, Cheng et al. (2008), Jones et al. (2011) and Rieger et al. (2017) do not prescribe globally constant threshold wind speeds for dust emission, but parameterize it with dependencies on other variables. These are the global models ECHAM-HAM, HadGEM2-ES, and ICON-ART. Such studies should be cited and used for comparison of the new development in the GFDL model.

Specific comments:

P1. L.37: „enhancing net radiant energy loading“ Use a physically better phrase.

P1. L46: „the life cycle of dust“ -> „the life cycle of dust aerosols“

P.6 :.124-126: „We require that the single scattering albedo at 470 nm to be less than 1 for dust due to its absorption of solar radiation. This separates dust from scattering aerosols, such as sea salt.“ The single scattering albedo is by definition smaller than 1. So it will not separate dust and sea-salt aerosol. This statement leaves me puzzled about the adopted method for obtaining dust aerosol optical depth from MODIS. The method needs to be revised and the description clarified. The remaining sentences of the paragraph give more details, but it is not obvious how the method works without reading all the other publications. My recommendation is giving a more concrete and easier to follow description of the method here. For instance, how is dust separated from other aerosols and how are dust sources identified. Also provide important numbers, e.g., for the separation of fine-mode vs. dust aerosols and the definition of high-resolution.

P.6 L.134-137: What does a flag of QA=1 and QA=3 imply for the quality of the data?

P.6 L.139-143: I understand combining the morning and afternoon measurements is the best we can do, but the text should acknowledge that the location and amount of dust emission typically changes between the morning and afternoon. Peak contributions from

convective storms would be missed due to the temporal resolution. A relatively large number of literature assesses the diurnal cycle of dust emission and some of those studies could be cited here. My point is that the strengths and weaknesses of the method need to be named as far as it is currently known. This also applies to the other satellite products (soil moisture, snow cover, LAI) introduced in the next paragraphs.

P.8 L.177: „Vegetation can protect soil (…)“ -> Vegetation protects soils (…)

P.8 L. 182-184: The description of the data set is not published. At least a short description of the retrieval is needed and also a statement on where one can access or request that data.

P. 9 L. 186-187: A six hourly resolution of the winds does not sufficiently resolve their diurnal cycle and hence their effect on dust emission. Again, the diurnal cycle of dust emission is an issue here, but for the model data we could fix it.

Section 2.1.2: Why did you choose two different re-analyses? Did you also consider using MERRA?

P.9 L. 192: „closet“ -> closest

P.9 L. 205: „coarse mode AOD“ What is the radius for separating coarse and fine-mode AOD in your work?

P.10 L. 209-210: Three years is a very short time period for a climatology, especially in light of the strong year-to-year variability in dust aerosol burden. I agree that as little data as possible should be removed. However, I recommend giving an estimate of the uncertainty, e.g., try a stricter criterion and compare the climatologies.

P.10 L.227: Refer to the section of the article.

Section 2.1.3: Consider showing a map with the location of the different stations used for this research. You could use color to indicate the record length of the stations.

P.12 L. 254-255: „ (…) assume that the climatology of the surface dust concentrations do not change greatly from the 1980s to the 2000s“ Why is this a reasonable assumption?

P.14 L.303-307: Why did you choose these thresholds? For instance, why not a snow cover of 0% and an LAI of 0? I can imagine this is due to fractional difference within a grid box, but it is unclear whether a slight change in the thresholds would have a big effect on the results. Maybe you could test it for obtaining more confidence in the results.

P.15 L.321-333: I understand that you choose different background dust AODs per region, but where does 0.2 and 0.02 come from? Could you use the minimum in dust AOD from daily values in your MODIS climatology to accurately compute the background values?

P.15 L. 339-343: I appreciate the general acknowledgement of potential uncertainty in the thresholds. I think a quantitative assessment of the uncertainty would substantially strengthen your work. You could easily do so by varying the threshold criteria within bounds you perceive reasonable (justified by physical arguments) and show the associated changes in your results.

P.16 L.365: How was the scaling factor determined?

P.18 L.399: „differences in simulated dynamic vegetation by LM4.0 among the three simulations are actually very small and can be ignored“ add that this is the case because of the short simulation when the land use does not change as much as over longer time periods.

P.18 L.412: What primarily controls the threshold differences between North Africa and Eurasia? A threshold of 3ms-1 is very low and needs an explanation.

P.19 L.435: „weed“ -> wind

P.19 L.423- 439: A discussion is useful, but the results keep me thinking of the potential impact of the threshold choices in the retrieval. This is not picked up in the discussion of your lower threshold velocities than in previous studies.

P.25 L.572: Harmattan winds are important in winter and spring. Fiedler et al. (2015) provide a complete climatology of dust aerosol associated with the Harmattan.

P.27 L. 608: „storm centers a bit“ -> storm center is located

Section 3.3: It would be useful to compare against independent data sets already published since both the model and the observational estimates have been newly developed in the current article. Relevant works are for instance Schepanski et al. (2007) and Evan et al. (2015).

Figure 8: Refine the color scale for the surface concentration in the dust belt. The same red shading does not allow a comparison of the results in the dust regions.

Figure 10: Except for India, US and South America, the difference in the annual cycles in Vthres12mn and VthresAnn is very small. It suggests that the month-to-month variation in threshold wind velocities does not have a large impact on the climatological mean dust aerosol optical depth in main dust sources. Is this primarily so because the variations in soil moisture of deserts are small or what explains the similarity?

Figure 14: Add VthresAnn.

References:

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