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Comment on acp-2022-758

Jasper Kok (Referee)

Referee comment on "Insights into the size-resolved dust emission from field measurements in the Moroccan Sahara" by Cristina González-Flórez et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2022-758-RC3, 2023

Review of “Insights into the size-resolved dust emission from field measurements in the Moroccan Sahara” by González-Flórez et al., for ACPD by Jasper Kok

This paper reports on field measurements of dust emissions and studies how the diffusive dust flux depends on particle size, wind speed, and fetch. The reported measurements are probably the most extensive measurements of dust emission ever made and, in my view, represent a critical contribution to the field. This study makes an additional important contribution to the literature by showing the effect of dust deposition on the emitted dust PSD. I do think that parts of the paper can be improved substantially in both style and content, which could help the results of this paper be more fully appreciated by the community.

Major comments:

- Similar to the review by Yaping Shao, I think the paper tries to do too much, resulting in a very long paper that risks diluting some of the important findings. I would suggest that the authors split the paper into two separate papers: one focused on bulk dust emissions and one focused on size-resolved dust emissions. I think that will enhance the value of this exhaustive work to the community and will make it easier on the finite attention span of most readers.

- One of the key contributions of this paper is the finding that the shift towards a finer dust PSD with u* is likely due to an increase in the deposition flux of coarse dust with u*. This is overall convincing but I noticed a few issues with the presentation that should be addressed to make this part of the paper stronger:
  - First, the paper does not actually show explicitly that accounting for deposition explains the observed shift to a finer dust PSD. Please show the emitted flux by subtracting the calculated dry deposition flux and discuss whether this indeed shows that accounting for the deposition flux eliminates the shift towards finer dust with u* (within the uncertainties). For this, it would also be needed to propagate uncertainty in the calculation of the deposition flux, which might be tricky. You could for instance use several different deposition flux models and use the range of their predictions as an estimate of uncertainty.
  - Second, the changes in the PSD with u* seem to be quite small, which is a point that's easily missed because the changes are discussed mostly in qualitative terms. But while it is of substantial interest whether or not there is indeed a shift in PSD with u*, the size of this effect might be of even greater interest because it for instance determines whether this is worth parameterizing in climate models. Therefore, could you include a plot that shows the shift in PSD quantitatively and with uncertainty? For instance by plotting the contributions of fine (D<2.5 um), coarse (2.5<D<10 um) and super coarse (D > 10 um) dust (per the size terminology in Adebiyi '23) as a function of u* for the two different wind directions and for the haboob events? That would be great to include both for the diffusive flux and for the estimated emitted flux (i.e., after subtracting the calculated dry deposition flux). And please also add some discussion of the changes in the PSD in quantitative terms and with uncertainty, especially for the estimated emitted flux, which will help the reader appreciate whether or not these changes are important from a broader Earth system perspective.
- The authors fit power laws to the bulk saltation and diffusive (≈ dust emission) fluxes in Fig. 4. However, these fluxes are well known to depend on both u* and the threshold u* so these fits are not particularly useful or insightful. Can the authors obtain the threshold u* (for instance, from fitting the flux versus wind shear stress; e.g., Martin and Kok, 2017) and compare their measurements of both saltation and dust emission fluxes against current parameterizations? This would need to be done for several periods if events (e.g., rainfall) changed the threshold during the campaign.
- The authors report measurements for dust with diameter up to 20 um. This is quite valuable, as very few measurements of diffusive dust fluxes for D > 10 um have been made. However, one of the reasons that there are so few measurements is that the transmission efficiency of inlets normally decreases sharply with particle size and furthermore that the diameter at which 50% is transmitted decreases with wind speed (e.g., von der Weiden et al., AMT, 2009). The authors are well aware of this problem and on lines 161-4 they cite several previous studies that have used their sigma-2 sampling head and concluded that this is accurate for coarse and super coarse dust. However, this issue seems critical for the papers conclusions - for instance, measured changes in dust PSD could be due to a decrease in sampling efficiency with u* for super coarse dust. Therefore, please elaborate on the evidence that this inlet is in fact suitable for super coarse dust. For instance, has the sampling efficiency actually been measured as a function of particle size and wind speed?
- The paper is made very long by the inclusion of no fewer than 10 appendices. Most of these appendices are in my view of interest only to a few readers and are not needed to appreciate the papers main conclusions. I therefore recommend moving most of the appendices to the supplement (this will also cut down on publication costs!). One exception is Figure J1, which shows the deposition velocity as a function of D and u*. This figure is in fact critical to understanding the paper's results and conclusions and I think it should be moved be moved to the main text.
- The authors compared their results to the predicted PSD from both the original and the

updated parameterizations based on brittle fragmentation theory (BFT), which is my own past work. This parameterization has some dependence on the fully dispersed size distribution of the parent soil (e.g., Eq. 3 in Kok, 2011). But because the fully-dispersed PSD of the soil of a GCM grid box is unknown, the standard version of this parameterization uses an "average" size distribution of desert soils. However, the authors actually measured the fully-dispersed soil PSD (Fig. A1)! Therefore, please perform a more test against BFT by inserting the cumulative soil PSD into Eq. 3 and redoing the comparisons.

Other comments:

- Do the authors know whether their measurements were of transport-limited or supply-limited saltation / dust emission? I assume the former, and it would be good to state that somewhere because it affects the physics of dust emission and thus the interpretation of the results.
- The authors used the law-of-the-wall to calculate u*. However, the law-of-the-wall is technically applicable only for idealized conditions that, as Yaping Shao also pointed out, might not apply. It'd therefore be more accurate to quantify u* from the Reynolds stress obtained from the two 3-D anemometers at 1 and 3 m (lines 140-1). Depending on how close the experimental conditions were to homogeneous to isotropic turbulence, there might be substantial differences between the Reynolds-stress based u* and the law-of-the-wall-based u*.
- Line 218: confront --> convert
- Line 242: The von Karman constant actually depends a bit on flow properties like the Reynolds number and for atmospheric boundary layer flow it was measured to be 0.387 (Andreas et al., J Fluid Mech., 2006). I recommend you use that.
- Line 282: please define the Schmidt number
- The relation used to link z0 and u* was a good first estimate at the time it was proposed (50s and 60s) but is outdated for several reasons, but primarily because it does not account for the presence of a threshold. The authors should thus use a more physical relation, such as that proposed in Sherman (1992), which does account for the threshold.
- Where appropriate, please specify whether R^2 values are calculated in linear space or log space (e.g., Fig. 4, where R^2 should be calculated in log space because the measurements span almost 3 orders of magnitude).
- The authors have several fits that extend down to u* = 0.1 m/s. However, presumably they found many negative (for dust) or zero (for saltation) fluxes in the range between 0.1 and 0.15 (or 0.20) m/s. So how did you treat those zeroes in the fit? Just omitting seems incorrect. I recommend only reporting fits for u* above the threshold.
- Line 463: there's no such thing as "instantaneous u*" because u* is by definition a time-averaged quantity over at least several minutes. Please correct.
- Lines 478-80: The authors note that they do not find "any clear effect of atmospheric stability independent of u* upon the PSD" but they do not actually show this. Since this is an ongoing debate in the literature, as the authors note (Khalfallah et al., 2020; Shao et al., 2020; Dupont, 2022), could you include a graph supporting your conclusion here?
- Line 531: statistically significant at what level? Please include a p-value.

- Lines 581-3: by how much is the submicron proportion higher for western winds? Is this difference statistically significant? And what's the p-value?
- Line 602: here and elsewhere in the paper, I think the use of the term "aggregate fragmentation" is confusing in the context of the Alfaro '97 and Shao '01 models because those papers did not hypothesize that saltation bombardment causes aggregates fragmentation, which is a term that has a specific meaning in material science (i.e., the size of the largest fragment is small compared to the size of the original object). So this could cause the reader to think this is referring to brittle material fragmentation, which does hypothesize that soil aggregates are fragmented by saltation bombardment. Please use a term that is more consistent with these theories, such as "aggregate disintegration".
- Line 609-613: I personally think that the explanation of the smaller fetch length during haboobs is not clear here. That's a shame because it's a very elegant natural experiment. So I recommend rewriting this more clearly, perhaps with a schematic illustration.
- Line 685: I think "proves" is too strong a word here. Maybe "provides evidence for" or "indicates"?
- At many places in the manuscript, the authors use parentheses to indicate the opposite of a statement or to include multiple values in a sentence, presumably to save space. This practice obscures the writing and makes the paper more difficult to read. I thus recommend the authors eliminate this from the paper. In most cases, the opposite statement is obvious anyways so it's really not needed (e.g., "the proportion of submicron (supermicron) particles decreases (increases) in the concentration PSD between calm (purplish and blueish lines) and well-developed conditions (yellow, orange, and red lines)"). See also https://agupubs.onlinelibrary.wiley.com/doi/abs/10.1029/2010EO450004