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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Fine dust emissions from active sands at coastal Oceano Dunes, California" *by* Yue Huang et al.

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Review for Fine Dust Emissions from Active Sands at Coastal Oceano Dunes, California ACP-2018-692

This paper examines vertical dust fluxes from a portion of the Oceano Dunes in Coastal California under conditions of windblown transport. It reports on the results from a series of measurements conducted with optical particle counter-style instruments that were spaced vertically to allow for the calculation of dust flux. A second measurement component involved the mineralogical analysis of sand grains to infer the source material of emitted dust. These experiments were conducted simultaneously and in coordination with measurement of sand transport (reported separately). The authors

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Discussion paper

include a discussion of how their results may be applicable to global emissions from sand dunes.

Overall, this was a well written paper on an important topic. I recommend its publication following some revisions. There are some technical areas that ought to be addressed. Additionally, I found that the assertion that the results from this work are relevant for global emissions from sand dunes to be somewhat overstated. I believe this is a stylistics judgment, but have provided comments that the authors may wish to consider.

The authors rely on the concentration differences between Met One Particle Profiler instruments (model 212) to calculate the vertical flux of dust. If I read correctly, they basically use the two instruments that are mounted closest to the ground. As can be seen from the calibration coefficients in Table S2, these instruments are not exactly the gold standard when it comes to aerosol measurement. They are very useful for the type of field work described, but care should be taken in using the information quantitatively. The inter-instrument comparability changes over time as well as over the concentration range of the measurement. For example, two instruments might have one relationship when the (say) PM10 concentration is 50 micrograms per cubic meter and another relationship at 300 micrograms per meter cubed. In view of this, it would be important to ascertain that the concentration ranges that were experienced by the instruments during calibration and the ranges experienced during vertical flux measurement were similar. If not, this could be a significant source of bias in the results.

Another instrument-related observation is that the results and several of the key findings are related to the fact that the measured particle size distribution (PSD) during wind erosion events appears to become finer with increasing friction velocities. There is the potential that this is a consequence of measurement artifact associated with the changing aspiration efficiency of the inlet with wind speed. Typically, inlets to particulate matter instruments are designed to allow over a range of wind speeds for large particles (larger than the maximum size of interest) to make the treacherous aerody-

namic journey from being in the open air to passing by the actual sensing element, whatever that happens to be. The Met One instrument almost certainly loses particle collection efficiency at higher wind speeds. This loss of efficiency is most pronounced for larger (say > 5 microns) particles, giving the appearance that the size distribution is becoming finer at higher wind speeds. The authors should explicitly address/examine this issue and correct any of their results and subsequent conclusions as needed.

On a related point, I think the subtraction of the sea salt deposition profile is something of a distraction and perhaps also an artifact of the measurements. However, if the authors choose to retain this portion of the analysis, I would suggest first providing an estimate of the magnitude of sea salt deposition that is inferred and whether this number seems reasonable at all. Second, what mechanism of deposition could account for such deposition rates? Third, is that mechanism shear stress independent as is assumed in the final treatment of the numbers?

On a stylistic point, I can understand that there is value in expanding the results from the Oceano Dunes to try to understand dust emissions from sand dunes globally. While the authors are careful to caveat their comparisons between the dust emission schemes of (for example) the Sahara and Oceano Dunes, the language in which their work is described blurs out some of the caveats that are stated. For example, in the abstract, it states that “These measurements thus support the hypothesis that considerable emissions of fine dust can be generated by the reactivation of inactive dunes with accumulated clay minerals.” Do they, actually? Elsewhere in the Abstract, there is the statement that “As such, dust emitted from sand sheet, and potentially from other active sands affected by similar dust emission processes, could have potent impacts on climate change, the hydrological cycle, and human health.” Sure, this seems quite likely, but it follows a sentence that starts with “We further find. . .”, giving the impression that the results of the paper pertain to climate, hydrological, and health impacts of dust from sand dunes. I have provided examples from the Abstract, but there are several locations in the manuscript where fuzzy sentence transitions amidst caveated

statements unintentionally inflate the achievements of the paper. Another example is the series of sentences on page 16 starting on line 4 with “we find that the PSD of dust. . .” through “. . .clay coating removal is likely a major emission process for active sands” on line 12. I would urge the authors to check the text for statements that may be misinterpreted as overreaching.

Minor comments: - The legends, axis titles, and figure titles of Fig S5 are too small to be legible. Suggest making more readable, especially if you elect to retain sea salt deposition corrections - Can you show the size distribution of Bullard et al Australian laboratory measurements alongside those presented in Figure 3? - Can you provide a brief explanation (few sentences describing main features of the technique) of how XRPD information is different from SEM-EDS and to what extent either provides bulk versus surface information? - Can you provide more detail on how many particles/samples (if applicable) were examined with XRPD and SEM/EDS? I couldn’t find details of this work and it seems to be a key point of the paper that the feldspars and clay coatings are important. - There are likely gray literature reports where the dust emissions were measured at the Oceano Dunes. Naturally, methodology would not be exactly comparable, but it might be worthwhile to ask your ODSVRA contact to provide some of those reports for a gross comparison. Perhaps there is a journal policy on gray literature that precludes this.

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