Review of "From fine to giant: Multi-instrument assessment of the dust particle size distribution at an emission source during the J-WADI field campaign"

The study sheds light on coarse dust emission PSD characteristics, which is extremely difficult to measure yet important for dust transport understanding and modelling, and impacts of dust on weather and climate. This is a thorough, interesting and novel study and I commend the authors for their attention to detail and impressive results. Overall I applaud the authors for clarity of method, data collection and processing.

The article reads well, despite being quite long. In a few limited places information seems to be missing, which may be a question of better signposting or inserting some clarifications. I suggest that discussion around the main findings could be much stronger. Other than the main points below, all comments are minor and relate to improved readability and signposting through the article.

Main points:

- The method applied to harmonize the PSDs appeared missing – i.e. how PSDs from multiple instruments (e.g. fig 9) were combined to give one single PSD (e.g. fig 11). This should be clearly explained and included in the main article as it is a very important part of the data analysis and processing.
- The article is weak on scientific discussion of the very interesting findings relating PSD to u* and stability. This is touched on in the discussion around figure 16, but could be much more extensive and thorough. How do the results agree/disagree with previous work relating PSD to wind speed? This has been an ongoing discussion in dust emission for decades and the authors are in a position to shed new light on this. See further comments below.
- Some sections require further clarification or explanation – see specific points below. In particular, information regarding the methodology of the FPS approach.
- It would be very insightful to provide a composite of diurnal averages of figure 8, since there is a clear diurnal cycle in weather variables, including wind speed and stability and therefore dust emission.
- The article would be enhanced by providing some statistics/data on number and surface area distributions, in addition to mass PSDs, since number/surface area affect various dust-climate interactions such as CCN, radiation etc. See further points below.

Specific points:

Abstract - I recommend adding something about the findings relating to atmospheric stability.

Can friction velocity mentioned in the abstract be provided along with windspeed(s)? This would provide a more relatable variable of wider application.

Line 11 – typo in symbol for friction velocity? (threshold friction velocity is given).

Introduction

Paragraph starting line 38 is not quite correct in terms of warming/radiative effects. Be clear on whether you mean TOA warming/cooling, or specifically atmospheric heating/cooling, which refers to the atmospheric column. All dust will cause an atmospheric SW heating, while it is the TOA effect whose sign can vary as a result of the strength of the atmospheric heating, which is sensitive to particle size. In the LW, dust cools the atmosphere but causes a positive radiative

effect at the TOA. Please correct and sharpen the scientific terminology here. Be specific on whether you mean shortwave, longwave or total radiation. If SSAs are mentioned, wavelengths should be specified.

L44 – unclear what you are saying causes the decrease in SSA from 1 to 0.95?

L107 – if the site is downwind of a solar plant, does this have any associated local particulate emissions which could influence measurements?

Sect 2.1 – how were the instruments powered?

Fig 1 caption – (c) – 'instruments used' – add 'in this article' – it is not entirely clear what the pink/black labelled instruments are – please make this clear.

L122 and next few lines – add a definition of the meanin of Obukhov length, and how u* and L were obtained from the scintillometer.

L137 – it's not clear which markers on the figure relate to the different masts mentioned.

Two SANTRI instruments are used – SANTRI and SANTRI2. I was somewhat confused as I read the threshold friction velocity section (~L145). Why is SANTRI not included in the instrument table? It would probably be useful to have a table of meteorological instruments/variables at this stage in the paper to help navigation. Further confusion – the map on fig 1 shows a 'SANTRI4.'

Table 1 caption – add that diameters are optical/projected diameter.

L204 – 'accurately' model is a bit misleading – it accurately applies Mie theory, though this in itself has various limitations around dust particle measurement.

L208 – Table 1 gives a different max d for the Fidas

L239 – was particle density measured during the campaign?

L247 – what does 'finer' than the emission flux PSD mean specifically? More resolution in diameter space?

L260 – 'primarily' – perhaps 'only' – as only asphericity and RI were considered?

L277 (&C2, UCASS) – this is an interesting approach – has this been previously applied in other studies, and would it be relevant to other scattering instruments for PSD?

L338 – NDR as an acronym is not expanded

L338-360 it is unclear what the motivation for these paragraphs is. A bit of background on the FPS system and how it can be used is required, or a statement to this effect. I'm guessing the measurements from the SEM are being combined with deposition assumptions, to recreate an atmospheric airborne PSD? Why can the sampled data not simply be used to represent atmospheric concentrations? Please make this clearer for the benefit of those not familiar with FPS sampling.

L361-366 – section numbers don't quite match up with what is presented in each section.

Fig 8 – it would be more natural to give mass concentration units in milligrams or micrograms per cubic meter, and avoid many decimal places.

L368 and Fig 8 – are temperatures dry bulb temperature or potential temperature? The symbol theta should only be used for potential temperature, T for standard dry bulb.

Fig 8 – this figure contains a wealth of information which is skipped over quite quickly. It would be interesting to see a daily composite version of the figure, given the strong diurnal cycle. This would also emphasize the times of day which certain features discussed occur at. This could also shed some novel light on dust emission processes and how they are affected by the diurnal cycle, which impacts many areas, including satellite detection by overpasses at certain times, and model emission processes.

Fig 8 caption – include what the dashed line in f and g represents.

Whole article – It is confusing that results are presented and described in UTC, rather than local time. Given the importance of the diurnal cycle, local time would be much more informative and avoid confusion. I suggest switching to local time, if possible.

Section 3.1 – there appears to be a change in regime between around the start of the campaign to around 22 Sept, and then 22 Sept to 30 Sept or possibly the end of the campaign. Meteorology switches from cool/humid/low pressure to warm/dry/higher pressure, and it looks like the behaviour of the diurnal cycle in wind changes too. Has this been investigated and what caused it? Was there any impact on dust measurement results?

Fig 9 caption – would be useful to add 'fine/coarse/super-coarse/giant' after classifications.

Fig 9 – top row – UCASS A appears to have no error bars?

L408 – seems like the roughly constant concentration starts at more like 5 microns rather than 10

L411 – 'both instruments maintain similar distribution shapes' – I would disagree – they look fairly different at d>10um in fig 9a.

L419-420 – this statement is vague and not very helpful – can you be more specific about the UCASS instruments? Also make it clear that both are excluded from further analysis.

L425 – why would the CDA have a lower sensitivity in this size range? Inlet efficiency seems logical, but can this be explained based on the instrument design/engineering?

P21 'use of inlets' section – first paragraph – please can you re-state whether the inlet sampling efficiency has been applied to the data in figure 9.

L518 – it would be helpful to add a sentence summarizing the main finding of this part of the appendix.

Section 3.5/Fig 11 onwards – The article seems to suddenly skip to using harmonized PSDs from the SANTRI2/Welas/Fidas, without explaining how this is achieved, since in some size ranges the instruments duplicate data, while in others there are measurement gaps, and in some instances the SANTRI2 does not follow well from the other instruments. What processing has been done here? This deserves its own subsection to explain. I was surprised by this given the attention to detail thus fur, and went back to search for something I missed but could not find anything.

Fig 11 – it is somewhat difficult to pick out the individual colors of the legend – can the legend color lines be made thicker? It would also be useful to add diameter tick marks to the top axis.

L553-555 – it is interesting that the PSD starts to respond at around d=60 microns in this u* range, but not at smaller particle sizes, and might be worth mentioning.

L550 – although the large variability here detracts from this finding.

L561 – should be 'as u* increases'?

Fig 12 – it would be interesting to consider adding bars to show the same % contributions, but for surface area and number, as these parameters contribute to different processes (e.g. radiative effects, CCN/IN, Mahowald et al. (2014)). Three bars for each u* range could be displayed, instead of just 1 corresponding to mass.

L581-2 – or more likely due to the large errors (variability) for this size range which we see in fig 11, which cannot be shown on fig 12.

L583-587 and fig 13 discussion – relating to the discussion here it would be useful to have a version of fig 8 composited into a diurnal cycle, since the stability and therefore the PSD are so dependent on time of day.

L592 – 'No clear trend…' – only in the normalized figure (b). The trend in fig 13a seems clear for the largest sizes.

Fig 14 – the definitions and ranges on L130 exceed the values shown on the color bar in the figure (e.g. unstable and stable are not included). Given this some of the discussion around the different types of stability in the figure are a bit difficult to follow. Ideally it would be good to make the color bar blocked, with different colours corresponding to specific stability classes. Specifically, 'For instance, for $u* = 0.25$m s−1 and for near unstable conditions, mass concentrations could reach approximately one order of magnitude higher than the average for unstable conditions' – the data doesn't seem to suggest this.

L613 – what were the findings of these studies?

L623/fig 15 – would be useful to state why we now see PSDs in deposition rates rather than concentrations.

Fig 15 – why no error bars for the FPS?

Fig 15 – the data is quite small and possibly makes discrepancies look better. I suggest at least adding additional tick marks for dDM every order of 10 magnitude to make interpretation easier.

L638-9 – in a different field experiment? Same instrumentation?

L644 paragraph – it would be useful to recap on what size the deposition velocity used represents.

Fig 16 – can the normalization process be made clearer? It is also unclear what 'MEAN' refers to – this needs to be made clearer in the caption, legend and main text. 'SOURCE' data should be added to the caption.

L601 – avoid 'elevated' dust conditions which might be interpreted as altitude.

Discussion around the main findings: L695 – how does the J-WADI compare to the other listed data sources in fig 16? Were they ground-based and at emission sources? Would we expect similar PSDs? Were they limited in maximum diameter measured? A discussion on similarities and differences, and causes for these between campaigns, should be included, and related

back to paragraph L78-83 in the introduction. Since you find that u* influences PSD, does this contradict the findings of Kok (2011) and the move of some models to implement this emission scheme?

L712 – friction velocity or threshold friction velocity?

L712 – 'during *active* dust emission events' – I was under the impression that the PSDs presented had been campaign averages, with no filtering based on 'active' periods or other – please clarify.

L718-723 – it would be useful to revisit the smaller super-coarse/giant concentrations and PSDs under unstable conditions here, with an explanation for this.

It would be entirely acceptable to move the extensive appendices to a supplement, should the authors wish to reduce the length of the main manuscript.