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Interactive comment on "Complex refractive indices and single scattering albedo of global dust aerosols in the shortwave spectrum and relationship to iron content and size" *by* Claudia Di Biagio et al.

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Overall comment

This paper provides new data on the spectral shortwave refractive index of mineral dust. Different global dust samples are suspended inside a chamber, and subsequently scattering, absorption, size distribution and composition are measured, and the former are used to calculate the refractive index. A relationship between iron oxide content and refractive index is also found. The authors find that the imaginary (absorption) component is relatively low compared to the range of older datasets.

There is a serious lack of high quality dust refractive index data available in the literature, so this research is valuable and welcomed. The results will be important in providing refractive index data for any sort of atmospheric model representing dust (NWP, GCMs), as well as satellite retrieval algorithms, and I would expect the data from this paper to be widely used. The paper is of a high standard, clearly setting out methodology, propagation of uncertainties, and results. Several of the figures are very small and need to be made larger, but other than this I suggest only minor clarifications and suggestions.

General Comments

Do similar chamber studies exist, such that estimates/measurements of refractive index have been attempted in a chamber? (perhaps not, other than the preceding studies already mentioned done by the same group). In the case that they do, the authors should mention them in the introduction, if relevant. If not, I suggest the authors point this out, as it increases the novelty and originality of the work done for this paper.

In the abstract and conclusion, please add information on optical properties/refractive index (RI) at either 520 or 590nm. $\sim$550nm is the wavelength most frequently required for these properties, and also represents the peak solar intensity, so this wavelength (or one close to it) would be most useful to state properties at, in addition to 370 and 950nm already provided.

Specific Comments

Abstract - I suggest adding a sentence reflecting how the new RI results compare to older datasets – i.e. the real part is similar but the imaginary part falls at the low end of the published range. This is an important finding.

P2 l46 – ‘an intrinsic property of matter’ – although this is true, I suggest rewording, such as, ‘k was found to be independent of size’ since several other studies have found k to be size-dependent.

L85 – insert 'geographic' before 'differences persist. . .'

L106-108 – It would be worth saying why models still assume the same dust composition globally – e.g. due to computational cost of additional tracers? And/or lack of a globally consistent information dataset?

L207 – 'Ogreen' typo

L257 – RI range for n and k – which values were used? Different values for different experiments? Where/how is this range applied?

L268 – 'cut at 10 microns' - this contradicts p5 l178 which says 5 microns/8 microns (50/100% efficiency).

L278-279- It would be useful to mention the cut-off diameters again here as deff,coarse does not represent the full size range.

L280 – it would be useful to say why modes were fitted to the size distribution, for non-experts.

L283 – how were modes fitted? (I think this is provided later in the paper but it should be mentioned here).

L301 – what about other uncertainties to the size distribution, such as shape assumptions and/or Mie-regime singularities?

L342-3 – and also when comparing the RI data?

L402 – 'contrasting' → 'contradictory'?

L447 – Muller et al (2011) report observations at Capo Verde, not transported across the Atlantic.

L460-464 – And also the fact that the size distribution above the 50% transmission efficiency (5 microns?) is not well represented, should be mentioned.

Figure 4 and discussion in lines 451-464 – the authors should consider that some of the observational data they show from other campaigns was also restricted by maximum size measured or by inlet transmission efficiencies (e.g. NAMMA, PRIDE). Information on some of these restrictions are provided in Ryder et al. (2018), table 1. As such, some of these datasets likely underestimate the coarse mode size distribution. Transported dust size distributions are also available for the AER-D campaign in the same paper which would add to the data already shown in Figure 5 and did not suffer from inlet restrictions.

L466 – it would be useful to add a line on the importance of iron oxides vs elemental iron for the benefit of non-specialists.

L469 – 'Australia' – should this be Namib-2? (values are not consistent with those in the table).

L469 – 'Iron oxides account for 11 and 62% of the iron mass' – where do these values come from? They are not shown in table 3?

Section 4.1.2 – did iron content vary with particle size? Was that measured?

L487 – angstrom exponents across which wavelengths?

L549 – Steigmann – typo either here or in references

L527 – this is a very long paragraph and would benefit from being broken up a bit.

L557-560 – if this is the case regarding the Wagner dataset, wouldn't that make their dataset more reliable?

L544-560 – Comparing the new results to the older data is an interesting and important discussion point of the paper. It would be useful to expand this discussion to include a little more information on the methods of Wagner et al (2012) and Steigmann & Yang (2017) to allow the reader to better understand the different approaches. Are the authors able to justify that their method is more reliable than the other studies? This is done for the comparison to the Volz & Patterson work, but not the other mentioned publications. It would help readers if the authors are able to justify their results more strongly, as this will enable readers of the paper to use the new data over the older datasets with a high level of confidence, rather than just adding to the spread of existing data.

L569-580 – can the authors comment on the fit for hematite vs goethite? Based on these results, is it therefore necessary to measure *both* hematite and goethite (rather than only hematite, as is sometimes done), in order to retrieve appropriate absorption estimates?

L589-593 – So is this several (or only one) data point per dust sample?

L596-598 – is the same true if you plot deff_coarse vs iron oxide content? Wouldn't this be a more direct comparison?

L599-601 'RI . . . is independent of size' – this is not always the case – e.g. Kandler et al. (2009) show that composition, and RI, change with size, and this is also reflected in the figures of Otto et al. (2009).

L604-605 – this was also found by Ryder et al. (2018) – i.e. SSA was dominantly dependent on composition.

Section 4.5 – do the authors have any thoughts on how much, if at all, their RI data may apply (or differ) for dust close to the source, when the coarse mode is more prevalent? Would it be worth mentioning this again in the conclusions as a potential area for future research?

Summary & concluding remarks – Do the authors have any suggestions for how to extrapolate RI at wavelengths lower than 370nm or larger than 950nm? These wavelengths are often required for spectral data within GCMs and/or NWP. Or if not, perhaps this should also be mentioned as an area requiring work in the future.

L625-627 – Can the authors comment on the reason for their k data falling at the lower edge of the literature data, while SSA data seems very much in the middle of the literature range? This might be considered somewhat contradictory.

L646-648 – this sentence contradicts the discussion around figure 9 where the authors say that the coarse mode (deff) impacts the SSA – clearly the size distribution impacts the SSA too.

L657 – ‘we propose... a set of regionally-averaged n, k and SSA values...’ I wonder how useful this would really be – e.g. RI values are severely different for several samples in the same region – e.g. the Sahel, South Africa, East Asia (fig 6) so that even a regional representation may underestimate the sub-regional variability. Additionally there is the computational cost of additional tracers that may be required. The authors should mention some of these issues.

678 – ‘in link’ $\rightarrow$ linking?

Tables & Figures

Table 3 caption +/-10%, 15%, 10% - what exactly are these uncertainties? They seem very large for % values in the table of $\sim$1-10%?

Figure 2 caption – it’s not quite clear which data is which – dots are Journet data, so are the box+whiskers Ginoux/DB17? Also, if the current work uses the same samples, shouldn’t the iron oxide data be the same?

Figure 3 – x-axis label – ‘time (hours)’ – given this starts at @1330, is this time of day? Time from start of experiment would be more appropriate.

Figure 6 – these panels are all far too small to interpret. These should be made significantly larger. I also suggest changing the axis ranges so that the data takes up more than a minimal fraction of the plot.

Figure 7 caption – L1256 ‘measured’ – I suggest using ‘represented’ instead as for several of these studies the properties shown (RI, SSA) are not directly measured, but calculated from other more directly measured properties.

Figure 7 – These panels & data displayed are far too small to interpret. I suggest removing the left hand column (data from this study) since that is replicated on the right hand column and can be seen here. The literature data is virtually impossible to interpret. I suggest making all these plots much larger and also expanding the y-axis range for the k and n plots.

Figure 9 – This figure could also do with the plots being larger, and having more zoomed-in displays of the data.

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