Review of "Dust Constraints from joint Observational-Modelling-experiMental analysis (DustCOMM): Comparison with measurements and model simulations" by Adebiyi et al.

General Comments

A new dataset is presented and analysed to provide a global 3-D seasonally resolved dataset of dust properties with constrained size distribution, shape and refractive index properties. The dataset (DustCOMM) provides annual and seasonal mean 3-D dust size distributions, 3-D mass extinction efficiency at 550 nm wavelength and 2-D dust column mass loading. Since models are mostly unable to reproduce these properties in faith with measurements, this is a welcome and valuable step forwards in the dust field. The authors show that the DustCOMM results perform significantly better than global model simulations when evaluated against independent measurements. This is a notable achievement considering the challenges involved in representing global dust fields. The authors outline the benefits and potential future uses of the DustCOMM dataset to the community, which are likely to be substantial.

This paper covers a considerable amount of work and therefore methodology in order to deliver the final results in the DustCOMM dataset. Although the authors do a credible job of explaining the long and complicated methodology, in places it requires further explanation and clarity, potentially with re-ordering of some sections. The results impeccably described and presented. The description of measurements used requires some corrections and clarifications. Although the authors provide the datasets online, one of the links is broken. The article fits the scope of ACP and I recommend publication subject to the corrections detailed below.

General Scientific Comments

1. Overall, it is not clear why there is a need for the AOD reanalyses in this study – i.e. column 3 in Figure 1. Given the volume size distribution, and an assumed dust density, it should be possible to calculate column mass loading directly from size distribution without the requirement for AODs, and without any uncertainty involved in the mass extinction efficiency (MEE) values. Currently this is not explained, and therefore the AOD reanalysis section (fig 1 column 3 and sections 2.3) seems superfluous. More details are given below.
2. The authors do not include any dust properties in the longwave (LW) spectrum. The impact of the coarse particles on the radiative budget is one of the motivators of this study, and indeed highlighted in the discussions and conclusion as one of the benefits of the new dataset. The LW radiative effect makes up a large part of the total change in radiative effect due to a better representation of coarse particles (Kok et al., 2017). Thus the omission of LW dust properties here detracts slightly from the novelty of this work. There may be valid reasons for excluding the LW here, such as scope of material and lack of MEE observations in the LW spectrum. However, this should be discussed, since the total radiative impact of the constrained size distribution cannot be calculated without dust LW properties. Additionally, there may be some limitations in radiative studies which could be done with the DustCOMM data due to MEE being required spectrally (at least on some spectral resolution), rather than just at 550 nm as provided.
3. Section 2.1 (Constraining the 3-D atmospheric size distribution) is a crucial part of the paper, but difficult to follow. This section needs further explanation and clarity – see specific comments below.

Specific Comments

1. Links to datasets – the first asset link to the DustCOMM dataset v1 (https://doi.org/10.5281/zenodo.2620475) is broken.
2. Abstract – l17-21 – the use of model simulations should be mentioned here as this is a crucial part of the work.
3. It is not entirely clear what the benefits of model-constrained data presented here are over the data used in Kok et al. (2017). It would be useful to the reader to make this crystal clear, probably at the end of the introduction.
4. All the way through the article, but particularly in the method, the authors should be absolutely clear which size distribution they are referring to - the Kok 2017 globally constrained size distribution, or the model-simulated size distribution(s) – when they state 'globally averaged size distribution,' or similar generalisations.
5. Section 5.4 – would you expect DustCOMM to show improved inter-annual variability in dust loading compared to conventional models, if more than one year of data were constructed? Or would DustCOMM's ability in this context be limited by the underlying global models' limitations? This is another significant challenge for models, e.g. Evan et al. (2014).
6. Section 2.1 – p4 l21-35 – These lines cover methodology and results from Kok et al. (2017). While necessary and useful to repeat here, I suggest making it very clear that up to l35 this is a repeat of method and size distribution from Kok et al. (2017).
7. Section 2, p5, l1-17 – this section forms a crucial part of the method – the main correction/constraining of the model size distributions – yet it is difficult to follow. The authors should explain this section much more clearly and in detail, perhaps with additional figures in the supplement and/or outlining a specific example, to clearly illustrate how the PSDs are forced away from the model simulations. Some specific comments are given in the points below, but I suggest they review and rephrase these lines.
8. It is not very clear at which point annual averages vs spatially varying size distributions are used from the simulations. E.g. p5 l5 – "annually-averaged, globally-averaged" size distributions are forced here – how/at which point are the spatially varying size distributions corrected? Better signposting of global averages vs spatial variations throughout the paper would be extremely useful in understanding the methodology.
9. P5 l9-10 "to the global dust loading" – which global dust loading – simulated or from Kok 2017?
10. P5 l1-17 – the description of equation 2 is not clear enough. E.g. what is the numerator in the equation for alpha?
11. P5 l16-17 – "where the discrete sum over each location and height equals unity, that is:…" – Why is the sum unity? Is this because the size distributions are normalized?
12. Fig S1 – Please include the Kok 2017 globally averaged constrained size distribution on each panel. This would enable the reader to see what the size distribution is being constrained by.
13. Figure S1 – please also include (either in this plot, or as a separate one), how the size distributions changed due to the re-binning/extension/curtailing, as described in Section 2.1.1.
14. Section 2.1.1 – Is this diameter range correction performed after the size distribution correction (section 2.1)? Figure S1 suggests that first the diameter range correction is performed, and then the size distribution correction. But the ordering of the text (2.1 – size distribution correction, 2.1.1 – diameter range correction) suggests the opposite. Please clarify and order the text appropriately to follow the steps in the method.

15. P6 l1-3 – Ryder et al. (2019) show that over the Sahara D>20µm contribute to at least 18% of SW extinction and 26% of LW extinction – these values are not negligible and represent aged Saharan dust.
16. P6 l4, "dust particles with D>Dmax generally stay only for a short period in the atmosphere before they are deposited" – this is not the case in van der Does et al. (2018), as stated elsewhere in this manuscript.
17. P5 l1-6 – the authors should revisit the impact of particles d>dmax in the discussion (e.g. Section 5.4). For example, if better global constraints/observations on this size range became available, could such observations be incorporated into DustCOMM?
18. P6 l30-32 – the authors essentially extend 4 models' size distributions towards a larger size range based on the other 2, which cover a wider diameter range. Does this implicitly assume that all the models behave the same way in terms of the coarse end of the size distribution? This seems unlikely. This should be discussed more, particularly since many of the results are most sensitive to the size changes above d=10 microns
19. Equation 5 – please state how/if this equation is different to that from Kok et al. (2011), and if so why.
20. Equation 5 & p8 l6 – why choose D_s for the geometric median diameter by volume? S subscript typically implies with respect to surface area. D_v would be more appropriate.
21. P8 l20-27 – This seems a great generalization. It's not clear how b is applied to specific locations as implied.
22. Section 2.1.2 – What is the reason for choosing this method of fit (eqn 6) as opposed to fitting a series of lognormal modes, as is typically done for size distribution measurements? Presumably given the simulated size distributions have been corrected based on the same function, the fitting of the corrected size distribution is more naturally aligned with eqn 6?
23. P10, section 2.3, l26-30 – units for all quantities would be helpful. What do the authors mean by "mass-weighted" in "mass-weighted vertically-integrated 2-D mass extinction efficiency" and what are the units of epsilon_tau and epsilon_m?
24. P10, section 2.3, l26-30 – I believe this calculation is the same as first used by Kaufman et al. (2005), which should be cited.
25. P10 l31-32 – please list the reanalysis products (MERRA-2 etc) here to avoid confusion. Also see later comment about section ordering of 3.2. "Dataset" should be 'datasets.'
26. P10 l32-p11 l2 – "This individual reanalysis dataset…." - I suggest removing this (and adding to section 3.2 if necessary). It is confusing here given that the AOD reanalyses have not yet been described.
27. P11 l10 "the four data sets…" – this is also confusing given that the AOD datasets have not been properly introduced at this stage in the paper. See later comment about relocating section 3.2.
28. Ordering and section 3 – I suggest the authors move sections 3.1 and 3.2 to before section 2. This would be easier to follow and understand. Section 3.3 should remain after section 2 since it follows on logically.
29. P16 l35 – 2011-2015 is presumably limited by available years? Is there any impact of this difference in years used?
30. P17 l32 – and is also a 2-D diameter project of a 3-D shape, which may introduce bias (e.g. Chou et al., 2008).
31. P17 l 36 "separate channels for different particle sizes" - this is not really relevant and could introduce confusion.

32. P17 l25-40 – there is a 4th category, which covers imaging probes, as used in the AER-D field campaign (section 4.18 of supplement) – which are beneficial since they do not suffer from uncertainties in converting scattered light to size as OPCs do.
33. P18 l22-25 – OPCs have other sources of uncertainty – such as refractive index applied in the inversion of scattered light to size and the non-monotonic relationship between scattered light and particle size. These should also be mentioned.
34. P18 l11-25 and section 3.3.2 – in-cabin measurements are also subject to uncertainties and size-bias in sampling due to aircraft inlets. As such, the MEE values from studies in table 2 are likely biased high in some cases.
35. P18 l14 – although the size distribution measured does not allow aerosol type to be distinguished, various chemical composition measurements made in parallel are now mostly a matter of routine during airborne campaigns. Individual studies often use these to infer size distributions or ranges dominated by different aerosol types.
36. Section 3 – there is a huge variety of measurement data available, and I do not suggest the authors attempt to significantly widen their coverage. The authors should describe how and why the studies in Table 2 were selected. There also appears to be a geographical gap of sampling Arabian dust (Fig S1). Additionally, I suggest the studies of dust sampled during the AMMA airborne missions (Formenti et al., 2011) as being a very useful addition, since they provide summertime sampling in the Niger region, which is currently not covered by the studies in Table 2.
37. Table 2 – please indicate which studies relate to which numbers on the map on Figure S1.
38. Section 3.3.2 and Supplement section 4 – The descriptions of data taken from each measurement campaign are too vague, and occasionally in error. Often it is not enough just to reference a paper as within the measurement papers observations are collected/averaged in different ways (time periods, meteorological regimes, altitudes, etc.) and it is not clear which are being used here. The authors should state specifically which data are taken from each paper, and what the values of MEE or MSE are, preferably listing them in a table in the supplement. Specific comments about data described in the supplement are given in the Supplement section.
39. P18 l34-35 – Ryder et al. (2013b) SSA values fall well outside this SSA range. This is a fairly narrow SSA range selected. The authors should note that measured SSA is sensitive to the size range sampled in the observations, which is likely to exclude the coarse mode and often d>~2-3 microns in many cases due to the effects of inlets. Only 3 studies are cited, while there are a huge variety of studies in existence which have measured dust SSA.
40. P19 l9-11 – "These errors include errors due to the instrument measuring the extinction coefficient" – change to 'instrumental uncertainties.' "meteorological influence" – such as? "the assumption of internal or external mixing" – how is this important?
41. P20 l13-17 – Field campaigns additionally often sample a variety of cases which are representative of the within-season variability, and also often include uncertainties/ranges to cover the variability encountered.
42. P21 l2-3 "(1) the ACE-2 campaign (June/July, 1997) off the west coast of Western Sahara and Morocco (Otto et al., 2007)" – would be better referred to as in the vicinity of the Canary islands. Same for caption of Figure 3.
43. P21 l6 "(2) the Fennec project (June 2011) between the Canary Islands and Mauritania/Mali (Ryder et al., 2013)" – If the authors refer to measurements between the Canary Islands and Morocco/Western Sahara (not Mauritania/Mali which are inland) the citation should be Ryder et al. (2013a – GRL) and the geographic references corrected. The same applies to the caption of Figure 3.

44. Figure 3 – what is the reason for the selection of altitude choice? It seems biased very high – presence of dust at z>6km is unlikely and concentrations will be very low at 5.5km – therefore the value of such high altitude comparisons is questionable. What is the reason for the selection of these 3 studies for Figure 3? The geographic spacing is very close, with all sampling JJA SAL dust. "ACE-2" in line 7 of the caption should read "AER-D."
45. P22 l1 – 14% and 15% - in terms of which variable?
46. P23 l25 – and also Qinghai Province China?
47. P24 l17 – "weighted by the dust vertical distribution" – why is this necessary?
48. P24 l31-32 – as stated earlier, it is not clear why the MEE and AODs need to be used to calculate the column mass loading, given that this is typical model output, and the size distributions are already available. It should be a direct step to calculate column mass loading from size distributions, given a dust density.
49. Section 5.1 – are there any impacts of uncertainties in wet deposition on the size distribution biases?
50. P27 l30 – Fig 7a does not show MEE.
51. P27 l31 – there is no figure S7 in the supplement.
52. Section 5.4 – the implications of dust LW properties should be reflected on here, considering the points about the LW radiative impacts of dust being crucial to the total impact on the radiation balance described above.
53. P30 l20 – should 'indirect effects' be 'semi-direct effects'?
54. P30 l28 – some reference to the SW spectrum and 550 nm should be included, since refractive index and MEE are only considered at this wavelength.
55. Section 5 – There is a general focus on in-situ observations for validation of DustCOMM. However, remote sensing observations are developing rapidly and it would be useful for the authors to consider whether lidar retrievals, for example, would be usable within the DustCOMM framework.
56. P31 – l25-26 – the bias across the full size range should also be stated.
57. Are there any important dust altitude or seasonal changes in DustCOMM vs the models?
58. Discussion/Conclusion - It would be interesting if the authors could comment on bias of models vs measurements in previous studies (e.g. Hunneus et al., 2011; Evan et al., 2014), and similarities/improvements seen in those studies vs DustCOMM and the model simulations in this study.
59. AOD reanalyses – do the authors combine these into one single reanalysis dataset themselves? This is not really clear.

Technical Comments

P3 l12 – "The resulting product constrains the climatology of 3-D global atmospheric dust properties on seasonal and annual timescales" – change to "The resulting product constrains the climatology of 3-D global atmospheric dust properties **and is provided** on seasonal and annual timescales" – to avoid confusion that the authors are constraining the temporal variability of dust properties.

P3 l34-35 – "After correcting…" – unclear – do you mean you combine all models into one multi-model representation?

P6 l28 – "globally-averaged size distribution" – Kok 2017 or the simulated one?

P7 l13 - "globally-averaged size distribution" – Kok 2017 or the simulated one?

P9 l18 – typo – should be -10 to -4?

Eqn 8 – please provide units for epsilon_tau

P10 l27 – “atmospheric “column” dust loading”?

P14 l25 – change to “…of the in-situ emission measurements..”

Supplement Comments

1. The supplement contains two Figure S1s. The second should be S3 (?).
2. Section 3.1 – l7-8 – mention that it is the AOD which is assimilated.
3. Section 3.3 - “1.1ox1.1o” typo
4. Section 4 – To make this easier to navigate, relate each observational subheading to the numbers on fig S3 (map). Also include the campaign name in the heading for each section. Take care to state for each subheading whether the campaign was ground-based or airborne. Also explain the choice of altitude selection defined in table to where relevant. Please also be aware, and state where necessary, that although a large size range may have been measured, inlet-size effects may have prevented coarser particles from being measured for some campaigns.
5. Section 4 – a subsection on Kandler et al. (2009), as listed in table 2, is missing.
6. Section 4.1 – please make the locations listed consistent with those listed in table 2.
7. Section 4.7 – note that these size distributions were not corrected for refractive index. The FSSP was *not* used as it did not operate correctly. Instead the size distribution larger than d=3 microns was taken from a sunphotometer retrieval.
8. Section 4.10 – Please note that these studies operated instruments behind significant pipework and suffered loss of the majority of coarse particles (e.g. Ryder et al., 2018, Table 1).
9. Section 4.11 – Why is MEE only taken from DODO1 (winter time?). It appears that the MEE for DODO1 is taken from table 4 of Osborne et al. (2008), for the ‘AM+CM’ case (a value of 0.41). No coarse mode was measured during DODO1 (see McConnell et al., 2008). The AM+CM DODO1 case in Osborne et al. (2008) was calculated using the coarse mode size distribution from DABEX since none was available from DODO1. This should be stated, or preferably the value from DODO2 used, where coarse mode was measured. Why is only z<1km used for the DODO2 size distributions?
10. Section 4.12 – is the campaign average size distribution used?
11. Section 4.13 – and also same aircraft as SAMUM1? SAMUM1 also used a high spectral resolution lidar.
12. Section 4.14 – ‘used the same instrumentation…’ – as which paper/campaign? Presumably the same as Kandler et al. (2009) which is missing? It is not clear which instruments the size distribution comes from – but probably because the 2009 section is missing.
13. Section 4.15 – Data from Ryder et al. (2013a – GRL, Canary Islands) is also used in the paper (Figure 3) and should be described here. Please take care to specify whether Ryder et al. (2013a or 2013b) is being cited – both are given in the references as 2013. Comparisons of both can be found in Ryder et al. (2019). “For this study, mean distribution from PCASP and CDP were selected because they were the most credible based on the authors’ analysis.” – change to …”based on the authors’ analysis over the size range we use here.” MEE is not given in Ryder et al. (2013b - ACP), presumably this is taken from Ryder et al. (2013a) (please state). Mean values in Ryder et al. (2013a) are 0.15 for fresh dust or 0.23 for aged dust – these appear much lower than the value plotted in Figure 8 (around 0.3).

14. Section 4.16 “above the SAL” – I would expect a dust measurement to be taken ‘in’ the SAL – is this a typo?
15. Section 4.18 – “The AER-D campaign uses similar instrument as the Fennec 2011 campaign. They use wing-mounted optical particle counters and shadow probes to measure dust sizes between 0.1 and 100 µm diameter.” – but additionally this AER-D used cloud imaging probes (CIP15 and 2DS) for size distributions at d>10 microns (which were used in Fennec but were not mentioned in Section 4.15 as the authors did not use the shadow probe data (d>18.5 microns) in this study).

References

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