

Interactive comment on “Coarse and Giant Particles are Ubiquitous in Saharan Dust Export Regions and are Radiatively Significant over the Sahara” by Claire L. Ryder et al.

Anonymous Referee #2

Received and published: 12 August 2019

The authors address a topic of scientific significance. They present and analyze new data retrieved from experimental campaigns in the Sahara and the Saharan Air Layer, which provide information on the dust particle size distribution close to sources and in aged and transported dust masses. That information is relevant, among other aspects, to characterize the dust radiative effect, which remains nowadays uncertain.

The authors also apply a valid methodology, which is described in an appropriate way, and they put into context their results by considering previously published works.

Finally, the results are presented with a relevant number of figures and tables, as well as as well as an appropriate use of English language. Some sections could be simplified

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(e.g. the methods section) in order to make it more concise, but overall the article is well structured and clear.

For those reasons, I believe that the article fully meets the Atmospheric Chemistry and Physics quality criteria and merits being published. I would recommend some minor corrections, that could help improve further the manuscript quality. Please, find them below.

** General comments **

The authors identify the particle size distribution as one of the key factors in characterizing the dust radiative effect. However, there are other factors that influence the dust optical properties that could be further discussed in the introduction section.

In addition, they present a thorough review of complex refractive indexes applicable to dust from different sources. They discuss the variability of the dust optical properties in the short and long-wave considering the ranges of uncertainty of the PSD and RI together. In my view, they have the opportunity, with the data presented, to discuss further the contribution of each of those separately, providing a valuable insight for the modelling community.

Finally, I would recommend to comment further on the representativeness of the data presented when the authors introduce and describe the different campaigns.

** Specific Comments **

Introduction

Page 2; line 15: Jickells et al. (2005) focuses on oceanic ecosystems, rather than Amazon rainforest effects. The authors could provide additional references regarding the effect of dust deposition on the Amazon rainforest (as they do in line 24).

Page 3; line 28 to page 4; line 4: I would suggest to move this paragraph to page 3, line 2, and link it to the discussion on the uncertainties on optical properties due to the size

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distribution. This will also allow to avoid repeating the “sensitivity of satellite retrievals to assumed PSD.” It would be also advisable to acknowledge at some point in the introduction other sources of uncertainty in the dust optical properties (e.g. mineralogical composition, shape, mixing state).

Methods

The methods section includes all the relevant details to understand the measurements and analyses performed. However, I believe that it would be easier to follow if it could be simplified or slightly reorganized. I would suggest to:

Include a summary table with the most relevant details of the campaigns

Summarize all novel data and analyses in one paragraph if possible. For instance, page 5, line 32 explains a new metric from Fennec data, later on page 6, lines 6 to 9, new data and analyses are highlighted.

Rename or reorganize the sub-sections. Section “2.1 Size distribution” provides details about the spatial sampling (e.g. horizontal flight legs, vertical profiles, etc.), which, in my view, would be part of the fieldwork setup. The last paragraph of the same section mentions the optical properties calculations. I would move that information to section “2.2. Optical property calculations”.

Results

Page 10; line 14: Health effects could be pointed here too, as they are highlighted later in lines 21-23.

Page 10; line 30: Would it be possible to provide a measure of the underestimation of particles above $5 \mu\text{m}$ in models?

Page 11; lines 5-10: Due to dust seasonality, a direct comparison of the DMP values obtained from the summertime campaigns and the modelled annual mean cannot be used to draw conclusions. Also, the authors refer to satellite data that is not mentioned

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in the text. I would recommend to compare to seasonal (summertime) modelled values, if possible. In line with this comment, and as suggested in the General comments section, I would suggest to briefly comment on the representativeness of the data earlier, when the different campaigns are introduced.

Page 12; lines 8-12: I would suggest to specify that only information on panel a of Figure 7 relies exclusively on Colarco RI and the mean PSDs. Panels b and c, as the reader understands from line 10, include the uncertainty due to the variability of RIs and PSDs. Would it be possible to disentangle both sources of uncertainty? In my view, it would be very interesting to have a measure of the relative contribution to the uncertainty attributable to PSD and RI separately.

Page 13; line 10: Please, specify in the text, as done for Figure 7, the PSD and RI source used as a reference to calculate the size resolved contribution to optical properties at $10.8 \mu\text{m}$.

Page 13; line 14: Please, specify the source of the range of SSA (0.4-0.5).

Page 13; lines 25-27: I would suggest to include also the information related to absorption in Figure 8. It would make it fully consistent with Figure 7. Alternatively, I would move the justification for not including this information to the paragraph presenting Figure 8 (i.e. lines 10 and below). As commented for the short-wave, it would be very interesting to distinguish in the uncertainty the relative contribution of the variability of PSDs and RIs.

Page 17; lines 23-24: Only the effect of coarse particles as ICN is mentioned. I would suggest to list other possible processes affected by a misrepresentation of coarse particles.

** Technical corrections **

Page 3; lines 20-25: I would suggest to identify the reference for each specific campaign, instead of listing all at the end of the paragraph.

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Page 5; line 29: Ryder et al. (2018)

Page 6; lines 2-3: Ryder et al. (2018)

Page 6; line 24: Add the acronym for refractive index (RI) here, and remove it later in line 31.

Page 8; line 14: “The age [...] was” or “The ages [...] were”

Page 9; line 24: Specify what z refers to ($z < 100\text{m}$).

Page 12; lines 24-25: The definition of panels b and c of Figure 7 has already been provided in lines 10-11 of the same page.

Page 13; line 27: Please, specify what does the 50% underestimate refer to.

Page 14; line 3: The parenthesis in “(and therefore do not [...])” should be removed or closed somewhere later.

Page 14; line 6: Ryder et al. (2018)

Page 17; lines 28 and 30-31: For the values: “1-4%(0-4%)” and “2-10%(0-13%)”, please, specify in the text what do the ranges correspond to (mean values for the two SAL campaigns and range of variability due to RI and PSDs?).

Page 18; line 12: Please, include references in the same format. “Kok et al. (2014); (Evan et al., 2014)”.

Figure 5 caption. Please, include a space between the number and units of 250m and 350m.

Figure 9 caption. Please, include a space between number and units of 3km and $20\mu\text{m}$.

Please, find the comments to the article also in the attached pdf document.

Please also note the supplement to this comment:

<https://www.atmos-chem-phys-discuss.net/acp-2019-421/acp-2019-421-RC1->

supplement.pdf

Interactive comment on Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2019-421>, 2019.

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