

Atmos. Chem. Phys. Discuss., referee comment RC3  
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## Comment on acp-2021-12

Anonymous Referee #3

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Referee comment on "Dust Induced Atmospheric Absorption Improves Tropical Precipitations In Climate Models" by Yves Balkanski et al., Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2021-12-RC3>, 2021

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This study adds new large dust particle sizes to the IPSL-CM6 climate model simulations. Model computations that include these larger particle sizes produce more atmospheric heating and affect the transport of moisture over the Sahel; this results in improved distribution of modeled precipitation rates (in comparisons to observations). This is an interesting paper with significant potential, but there are some inconsistencies in the paper that weaken the overall message.

For instance, although Section 5.2 states that the dust simulations are either done with one mode to represent both the accumulation and coarse modes or with four modes that encompass a much larger size range, I do not see any analysis that uses the single-mode runs in the paper. Everything refers to "with dust" and "without dust." None of the figures or tables present differences associated with single- vs multi-mode dust.

Other issues that should be addressed (majors):

One thing that bothered me throughout the paper is that the authors assume that the wind-blown particles have hematite concentrations of about 3% by volume in both the clay and silt fractions (per lines 81 and 225), citing Nickovic (2012). (At line 231 they vary the volume fraction of hematite from 0.9 to 10%, but this sensitivity test is not mentioned anywhere else in the paper). Thus, they assume constant iron-oxide mineralogies wrt size (first presented around lines 70-80). We don't necessarily expect the silts to have the same composition as clays, though. Some discussion about the robustness of assuming that all particle sizes have the same composition would improve the value of this paper. A literature search for measurements that provide the relative abundance of hematite/goethite in clays and silts would be worthwhile. If you can find measurements that demonstrate that the proportion of iron is constant wrt size, that will make this work stronger. If it turns out that this is not the case, then an adjustment to your analysis that reflects this variability will make this work stronger. If the literature is ambiguous, then it would be useful to also include non-absorbing silt as part of the analysis (to show the minimal effect of the large particles).

As I read this paper, it was not clear to me whether the addition of the larger particles increased the model emissions, or if the modeled emissions were scaled to larger sizes.

This should be stated very explicitly, because adding more particles to the system will of course increase absorption, regardless of particle size. More particles --> more AOD --> more AAOD --> more radiative effect, even if SSA is held constant.

Also, Figures 4 & 5 discuss the effect of "dust" vs "no dust," but elsewhere in the paper the importance of large particle absorption is emphasized. The thing that is missing from this paper is a comparison of "dust with large particles" vs "dust without large particles." Alternatively, I would like to see "dust with large particles" vs "no dust" AND "dust without large particles" vs "no dust." The way that the paper is written right now, though, the effect of the newly added large particles is still unquantified.

Lines 111-113: I don't understand this sentence wrt Figure 6; Fig 6 show a large \*positive increase\* in water flux (+0.365) at the southern border of the Sahel. I understand that  $-0.41 - (-0.05) = -0.36$ , but this is still not consistent with the +0.365 of the figure.

Line 114-115: I don't see 0.40 mm/day anywhere in Fig 6. Overall, I am having difficulty aligning the energy budget of Fig 6 with the text.

+ On line 224 the authors state that they used 3% iron oxides by volume. Then on line 230 they state that they vary the volume of hematite from 0.9 to 10%. The remainder of the text, however, does not discuss the sensitivity of varying the hematite fraction.

+ On line 240, the authors state "We refer the reader to Table 1 that explains the abundancies of the different assemblages and minerals.", but I do not see this information in Table 1 or in any other table.

+ Figures 2 and 6 refer to a "Methods" section that does not exist.

+ Figure 3 caption mentions "The effects indicated to the left of the Figures...", but I do not see anything to the left of the figures.

Other issues (minor):

In some ways it was nice to jump right to the results and discussion (without first presenting the methodology), but it is unusual for an ACP article. We usually see this in very compact articles that target a larger range of scientific disciplines. Such articles use this format because scientists outside of a certain specialty may have little knowledge or interest in the exact methods, but that is generally not the case with ACP audiences. Additionally, the format was problematic because the authors kept referring to a section called "Methods," but no such section exists in this article. Eventually I figured out that the "Methods" section is the Appendix.

The readability of the article would be much better if the contents of the Appendix immediately followed the introduction, in my opinion. Readers can skip this part and return to it later, if they choose (and many will). A 2nd choice would be to put the methodology after the Conclusions but not in the Appendix. An appendix is a supplement, so to some extent it is superfluous. Methodology, on the other hand, is not superfluous. Finally, if the authors are adamant about keeping this material in the Appendix, I recommend that they make the name of the Appendix descriptive (e.g., Appendix: Methodology).

Figure S1 is a nice visual that can help readers understand material in the main text. It should be moved to the main body of the article, in my opinion.

Lines 88-91: Comparing episodic absorption of dust to global forcing of greenhouse gases

is a bit of an apples-to-oranges hoodwink, eh?

Lines 232-241 seems like a complicated way of using the Maxwell Garnett (MG) effective medium approximation (EMA). Bohren and Huffman (1983) present a nice equation and discussion about the MG EMA for multi-component mixtures. It is on page 216 in my paperback version (between Equations 8.49 and 8.50).

The discussion begins with "We now compare the distribution of the surface precipitation between the two model simulations... " This comes as a complete surprise, since the authors have already discussed all six figures. I thought that we had already seen comparisons between "with dust" and "without dust" simulations?

Line 130: ..." (larger particles being more absorbing than smaller ones)." Since the authors have not demonstrated that this is the case (at least they have not provided large/small comparisons thus far), they should provide the reader with a citation to another study.

There article could use further proof reading, in places.

+ Lines 69-81 a little bumpy.

+ Line 100: bumpy

+ Line 104-106: bumpy

+ On line 232 they discuss a Maxwell-Bruggeman approximation; I suspect that they really mean the Maxwell Garnett approximation, as in Balkanski (2007).

+ Lines 242-252 in the Appendix are redundant with the main text.

+ Line 284 has a variable missing.

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Bohren, C., and D. Huffman (1983), Absorption and scattering of light by small particles, Wiley.

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