Answers to Comment RC3

We would like to thank Reviewer RC3 for his/her thoughtful review that helped us improve the manuscript and clarify the simulations presented. We start by answering the general comments and then make a point by point answer to his/her specific comments. To distinguish the review from the answers, we use format in plain text for the review and italics for the answers or any text from the authors.

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.

We now clearly indicate that dust simulations were performed both with a size distribution without coarse particles of more than 10 µm (1-mode run), and with a size distribution that includes the coarser particles from <1 to 100 µm range (4-mode run). These simulations were made comparable by normalizing the globally-averaged dust optical depth at 550nm to the value of 0.030 as discussed in Kok et al. (2017).

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).

To our knowledge, they are no existing measurements of how the amount of iron oxide (in particular hematite) varies with size. Some authors have speculated (and we are amongst them) that the iron oxide content decrease from the clay to the silt fraction but no hard measurements have confirmed this yet. Rather than speculating on this, we have preferred to start from established results documented in the literature. We found that Nickovic et al. (2012) had formulated a method to compute the iron oxides separately for the estimated clay and silt fractions. We used his results and we added a sentence to clearly indicate it to the reader.

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.

We relied on a previous publication that two of us coauthored (Di Biagio et al., 2020), where we constrained the emissions of dust according to the constrained of Kok et al. (2017) discussed above. We thank the Reviewer for pointing out that we need to be more explicit about the amount of dust emitted in both cases. We now indicate that for particles with diameters of less than 10 µm represented with a mono-modal size distribution (MMD=2.5 µm, sigma=2.0) the total yearly emissions of dust are 1,764 Gtons yr^{-1} whereas for the 4-modes size distribution representing the particles up to

100 μm these emissions amount to 18,122 Gtons yr^-1^. Both simulations have the same gobally-averaged dust aerosol optical depth at 550nm of 0.030 following Kok et al. (2017).

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.

Indeed, we had not clearly stated in the original manuscript that we had two equivalent cases in terms of radiative effect of dust at top-of-atmosphere, at the surface and in the atmosphere. We have now added a Table (Table 1, see below) and clearly state in the text the equivalence between two cases: 1-mode and 5% iron oxide content, and, 4 modes and 3% iron oxide content. We thank the Reviewer for helping clarify this key point in the study.

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 - (-.05) = -0.36, but this is still not consistent with the +0.365 of the figure.

The sign convention as indicated in the caption of this Figure is a positive water flux for the water entering the Sahel region and a negative water flux when water is exiting the Sahel. Hence this value of +0.365 mm day^{-1} is a net flux entering the South boundary of the Sahel region. The last sentence of the caption of Figure 6 in the revised manuscript has been changed to: "The sign of the water budget difference is positive (resp. negative) if water enters (resp. exits) the Sahel box." to make this clearer:

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.

The 0.40 mm day^{-1} is not on Fig. 6 since it is the difference in precipitation between the simulation with dust and the simulation without dust. Figure 6 only presents the terms of advection of water vapor into the Sahel region, not the terms of precipitation and evaporation differences resulting from the presence of dust. We now indicate the following in lines 232 to 236: "The change in water flux into the Sahel region amounts to an increase of 0.365 mm day^{-1} for the JJAS period. Precipitation and evaporation over the Sahel region increase by 0.40 and 0.09 mm day^{-1}, respectively, for the same months over the 30-year period (1985-2014). Hence there is a small residual in the water budget which we attribute to imperfections in the way the advected water fluxes are diagnosed in the model."

+ 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.

The case studied in the paper is 3% iron oxides by volume. We now state it mode clearly. We think that the variations we studied are useful to understand how we proceeded and clearly indicate that we are interested in dust from the Sahel region with a content of 3% iron oxides by volume.

+ 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.

This was also pointed out by Johannes Quaas. We had omitted a reference to Balkanski et al. (2007), which we have now added in the revised manuscript: " We refer the reader to Table 1 of Balkanski et al. (2007) that explains the abundancies of the different assemblages and minerals. "

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

We regret to have left this section that we referred to as "Methods" as an Appendix when it should have been part of the main text as the three reviewers agree upon. We have now moved the section describing the simulations and how the optical and physical properties are treated to Section 2 immediately after the introduction. We also tried to improve this section.

+ 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.

We added the following Table that details the SW and LW impacts at the surface, at TOA and the atmospheric absorption both for the Sahel region and globally. Since observational constraints exist on the dust aerosol optical depth, we added these constraints to encourage all authors of future publications to submit a similar Table for dust radiative effects for both the Sahel region and the globe.

Region	DOD at 550nm (Dust Optical Depth)	Height	Dust Radiative Perturbation (Wm^{-2})		
			SW	LW	Net
Sahel (15W:35E; 10N:20N)	0.37*, 5% iron oxide Dust diameter < 10μm	Top-of-atmosphere (TOA)	+3.00	+0.37	+3.37
		Atmospheric Absorption (TOA-Surface)	+19.5	-0.65	+18.8
		Surface	-16.5	+1.02	-15.5
	0.37*, 3% iron oxide Dust diameter < 100μm	TOA	+4.11	+1.87	+5.98
		Atm. Absorption	+19.9	-3.21	+16.7
		Surface	-15.8	+5.09	-10.8
Global	0.030**, 5% iron oxide Dust diameter < 10μm	TOA	-0.15	+0.02	-0.14
		Atm. Absorption	+1.07	-0.04	+1.02
		Surface	-1.22	+0.06	-1.16
	0.030**, 3% iron oxide Dust diameter < 100μm	TOA	-0.14	+0.12	-0.02
		Atm. Absorption	+1.28	-0.29	+0.98
		Surface	-1.42	+0.41	-1.01

Table 1. Dust radiative perturbation in the different model simulations. The atmospheric radiative perturbations (corresponding to atmospheric absorption) are highlighted in blue.
***Sahel Dust Optical depth at 550 nm was scaled to 0.37 to match the according to MODIS JJAS mean from 2000 to 2014 (both on-land and Deep-Blue products)**
****Global Dust Optical depth at 550 nm was scaled to 0.030 according to observational constraints described in Kok et al. (2017).**

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.

We regret to have left this section referred to as "Methods" in the text that is in fact the Appendix when it should have been part of the main text. We have now moved the section describing the simulations and how the optical and physical properties are treated to Section 2 immediately after the introduction.

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).

We are aware that this was not optimal as the three reviewers pointed out. We have now moved the section describing the simulations and how the optical and physical properties are treated to Section 2, immediately after the introduction. We have also included and improved the description of the simulations.

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.

We merged this information into the new Figure 6 by now showing the wind vectors on top of the budget of water advected to the Sahel.

Lines 88-91: Comparing episodic absorption of dust to global forcing of greenhouse gases is a bit of an apples-to-oranges hoodwink, eh?

We agree and have deleted the sentence where this not-so-welcomed comparison was done.

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).

We simplified this formulation.

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?

In this part of the manuscript, we compare the precipitation fields to observations using standard statistics: bias, the root mean square error (RMSE) and the spatial correlation. We changed the sentence to: "We now seek to analyze if the changes in precipitations brought about by the presence of dust improve or degrade the satistics of surface precipitations when compared to observations. We selected the observations from the Global Precipitation Climatology Project (GPCP) from June to September over the period 1985 to 2014."

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.

We now cite the measurements made by Claire Ryder during the FENNEC campaign (Ryder et al., 2013) that illustrate how much more absorption is caused by large dust particles compared to smaller ones.

There article could use further proof reading, in places.

The authors worked collectively on the text to improve its wording and the logic of the different parts.

+ Lines 69-81 a little bumpy.

Lines 71 through 81 were deleted as they did not bring new information.

+ Line 100: bumpy

We replaced this line with the following sentences (ll 239-244): "We compare the SW and LW radiative effects of two simulations that have the same absorption: 4 modes (including large particles, 10um < D < 100um) with 3% iron oxides, to the simulation with 1 (without large particle, 10um < D < 100um), the results are shown in Table 1. We ran for the full 100 -year period only the simulation with 1 mode and 5% iron oxide content equivalent to the full size distribution (4 modes) and 3% iron oxide. "

The addition of Table 1 reinforces this point.

+ Line 104-106: bumpy

We changed this sentence to: "Hence, the substantial increase in aerosol absorption caused by large particles will be particularly marked over dust source regions."

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

Thank you, this has been corrected.

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

We eliminated this redundancy.

+ Line 284 has a variable missing.

It is now corrected.

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

Citation: https://doi.org/10.5194/acp-2021-12-RC3

References

Di Biagio, C., Balkanski, Y., Albani, S., Boucher, O. and Formenti, P.: Direct Radiative 335 Effect by Mineral Dust Aerosols Constrained by New Microphysical and Spectral Optical Data, Geophys. Res. Lett., 47(2), https://doi.org/10.1029/2019GL086186, 2020.

Kok, J. F., Ridley, D. A., Zhou, Q., Miller, R. L., Zhao, C., Heald, C. L., Ward, D. S., Albani, S. and Haustein, K.: Smaller desert dust cooling effect estimated from analysis of dust size and abundance, Nat. Geosci., 10(4), 274– 278, https://doi.org/10.1038/ngeo2912, 2017.