Answers to the Editor Ulrich Poeschl and to the Reviewer Jim Haywood

We would like to thank both the Editor and the Reviewer for helping us carry through more improvements to this manuscript. We have now considered all their suggestions and answer point by point to them. The suggestions and in italics, our answers are in blue in plain text.

Suggestions from the Editor:

Dear Yves and Colleagues,

On behalf of the ACP executive committee, I will be happy to accept your manuscript for final publication as an ACP Letter if you can properly address the following comments and revise the manuscript accordingly:

1) It might be worth working with the authors to find a better title. The current title is "Dust Induced Atmospheric Absorption Influences Tropical Precipitations In IPSL-CM6 Climate Model". Aside from the two grammatical errors (should be Dust-induced and Precipitation), the term "atmospheric absorption" is meaningless. To people working on radiation it might be obvious that it refers to solar radiation. I also don't like the restriction to one model (requested by the referee). The authors make some general points about ESMs, so it would be good if they could define a title that makes a broader point while not over-extending to imply all models.

Following your suggestion, we iterated between the authors and came up with a title without grammatical errors that is not restricted to one climate model and takes into consideration the point from Reviewer Jim Haywood:'' Better representation of dust can improve climate models with a too weak African monsoon''

2) I am surprised that neither referee picked up on the terminology. The first sentence "Mineral dust influences precipitation through direct radiative forcing (Miller et al., 2014), changing the vertical temperature profile" should really read "Mineral dust influences precipitation through aerosol-radiation interaction (Miller et al., 2014), changing the vertical temperature profile".

We corrected this sentence accordingly and looked throughout the text for possible other occurrences.

3) Later it is stated that "absorption causes a change in atmospheric radiation of several tens of watts per square meter". Where? I presume they mean at the surface, but the surface and TOA effects strongly depend on the SSA. And why not say positive or negative? Then it says "an effect stronger than the one exerted by aerosol-cloud interactions." I presume they mean anthropogenic ACI rather than dust ACI? So are they comparing ALL dust with anthropogenic ACI? I would ask the authors to go through the paper to check for other ambiguities like this. It's unfortunate that they were not picked up in review.

We realized from your comment that aerosol absorption needed to be defined. Once defined, it becomes clear that this quantity is always positive and hence we do not need to indicate its sign. The following sentence was added in lines 30 to 34: "The energy absorbed by mineral dust in the atmospheric column is defined as the difference between top-of-atmosphere and surface radiative effects. In source regions, it represents several tens of watts per square meter ($W.m^{-2}$) and hence is stronger than the anthropogenic aerosol-cloud interactions (Miller et al., 2014; Nenes et al., 2014). The Sahel precipitation is influenced by the energy absorbed by aerosols (Miller et al., 2004; Solmon et al., 2008; Yoshioka et al., 2007), and dust absorption of energy depends on iron oxides (hematite and goethite) that are part of dust mineralogical composition (Sokolik and Toon, 1996; Claquin et al., 1999)."

4) In various places they talk about radiative forcing when really they mean radiative perturbation or effect. For example on line 487 they say "To evaluate the impact of this change of radiative forcing". Radiative forcing is almost always defined as pre-industrial to present day (or some decadal period), whereas I think they mean the radiative effect of dust (with/without). They need to be much more careful with their definitions/terminology.

I apologize for leaving out these terms which are inappropriate here. They have been now changed to 'dust radiative effect'.

5) In line 297 they state that gz is the geopotential height, but I think they mean the geopotential.

You must have been working on an earlier version of the manuscript since the lines that erroneously stated that gz was the geopotential height did not appear anymore in the revised version of the manuscript (acp-2021-12-manuscript-version4.pdf).

Please consider and follow up on the above comments and suggestions, revise the manuscript accordingly, and let us jointly consider better options for the title.

Please let us know if this new title convey correctly the work described in this article.

Many thanks, best wishes, and so long,
Uli

Thank you Uli for reading this manuscript thoroughly!

Points raised by the Reviewer Jim Haywood

This manuscript is vastly improved. I have read the response to reviewers' comments and the new manuscript file in some detail. The authors should be congratulated for making such a good job of the improvements. There are a few minor issues and a few typos that the authors should probably correct - I have included this as technical corrections.

Guided by the comments of the three reviewers we focused on describing more accurately the simulations and on introducing early the implementation of the updated dust properties. We are happy to have convinced all three reviewers.

L13 (and throughout the manuscript): tropical precipitations -> tropical precipitation (precipitation is a plural form of precipitation)

Corrected

L13 (and throughout): Sahel -> the Sahel

This correction was made for all instances where 'Sahel' appeared in the text.

L89: "two particle size class the clays" -> "two particle size classes: the clays"

Corrected

L182: The reference to Pérez et al., 2006. Agreed that there are simulated impacts on the surface SW flux of several hundred Wm-2. However, it would be useful to reference an example of a dust event that actually measured (rather than simulated) such a reduction using broadband radiometers. Note Perez (2006) explicitly acknowledge that they do not include the larger particles in their simulations "However, the study has several limitations that should be improved in the future. Currently, the size bin distribution includes dust particles smaller than 10 mm. Although the lifetime of larger particles is short, they still could significantly modify the radiative balance over emission areas. Thus it is planned to extend the range of particle toward larger particles."

I would suggest including more directly inferred measurements as these do not rely on accurate simulations of the dust size distribution : "Since dust is highly variable in time, this average value is consistent with values of several hundred watts per square meter simulated or inferred from surface-based measurements during particularly strong dust episodes (Pérez et al., 2006; Milton et al., 2008)."
Milton et al (2008) documented a strong dust event on 3rd March 2006 which reduced the downwelling surface solar broadband irradiance by >350Wm-2.

We followed the reviewer comment and have included these sentences (lines 186-189).

Milton, S. F., G. Greed, M. E. Brooks, J. Haywood, B. Johnson, R. P. Allan, A. Slingo, and W. M. F. Grey. "Modeled and observed atmospheric radiation balance during the West African dry season: Role of mineral dust, biomass burning aerosol, and surface albedo." Journal of Geophysical Research: Atmospheres 113, no. D23 (2008).

Figure 4, is a great addition compared to the previous analysis as it shows where the moisture comes from. I am a little surprised that you chose to present RH flux (% which is not intuitive at it is temperature dependent) rather than absolute humidity (g/m3) or specific humidity (g/kg) – the units would be more intuitive.

We agree that this Figure eases understanding how the moisture is transported to the Sahel.

L269. I would say that it is a cause rather than a symptom…..

We changed the sentence (line 276) to indicate that it is a cause and not a symptom