Reply to Jim Haywood's review of "**Dust Induced Atmospheric Absorption Improves Tropical Precipitations In Climate Models"**

We would like to thank Jim Haywood for his thorough review that helped us organize the results and their discussion in a more convincing way. We start by answering the general comments and then answer point by point his specific comments. To distinguish the review from the answers, the review is in plain text and the answers are in italics.

Review of Dust Induced Atmospheric Absorption Improves Tropical Precipitations In Climate Models, by Balkanski et al, ACPD.

General:

This paper brings together the idea that dust absorption is larger than previously thought owing to the presence of iron oxides and the presence of larger particles, which leads to a greater degree of solar and terrestrial absorption, heating the Sahelian region. Heating the northern hemisphere relative to the southern hemisphere (through whatever mechanism) has long been known to alter the cross equatorial energy and moisture flows leading to an increase in moisture available to the monsoon system and a northward progression of the ITCZ and vice versa (e.g. Oman et al., 2006 and Haywood et al., 2013). Putting the two things together is therefore logical, but the authors have to be careful not to overstate their results given the results come from a single model. For example the study of CMIP5 models by Hwang and Frierson (2013; Figure 3) shows that not all models suffer from an ITCZ that is too far south and hence a lack of Sahelian precipitation – some have the opposite bias.

Although I rather like the idea of the paper, ultimately I was frustrated by it. It comes across as rather incomplete, not logically organised, not formatted for ACP and consequently quite difficult to decipher. It does not put the results into a wider context in terms of analysing the changes in the equatorwards transport of energy and the change in the cross-equatorial energy and moisture transport. Without this link to the more detailed physical mechanisms that have been studied by many dynamicists (e.g. papers by Kang, Frierson, Held, Hwang, Hawcroft, Voigt, Schneider etc) the paper will not have the impact that its results deserve.

I conclude that, despite there being a very interesting result in the paper, the presentation is not of a suitable standard yet for publication. However, I do believe that the results are interesting and the authors should be encouraged to spend some time revising the paper as there is a good paper in there just waiting to get out.....

Thanks for these encouraging remarks. We changed the title of the manuscript to reflect that dust absorption will not necessarily bring an improvement in all climate models and explicitly state that the improvements were discussed here apply to the IPSL-CM6 model. The title is now: "Dust Induced Atmospheric Absorption Influences Tropical Precipitations In IPSL-CM6 Climate Model".

We added the following paragraph to make the link with previous studies by dynamicists about the cross-equatorial transport of energy and tropical precipitation (lines 39 to 51 in the revised version):

"The intensity and the seasonal pattern of tropical precipitations are controlled by the northward cross-equatorial transport of energy (e.g. Hwang and Fierson, 2013). CERES observations of the Earth's energy budget indicate a net northward cross-equatorial transport of energy. The atmospheric component of this cross-equatorial transport is southward whereas the oceanic component is northward (see Fig. 4 in Stephens et al., 2016). Haywood et al. (2016) discussed how, in the UK Met Office HadGEM2 model, tropical precipitation biases can be reduced by setting the northern and southern hemispheric albedos to be equal in agreement with multi-decadal satellite observations. Hemispheric albedo changes thus strongly influence tropical precipitations. The link between hemispheric albedo, aerosol loadings and properties in general, and dust atmospheric absorption in particular remains however poorly understood. Here we discuss the role played by dust on tropical precipitation and describe an end-to-end physical mechanism that ties improvement in tropical precipitation to observational support for a higher level of dust absorption based on measurements of iron oxide in dust particles, measurements of the full dust particle size distribution and detailed climate simulations with interactive dust."

Major Comments:

Some parts of the paper (for example the refractive index and SW impacts of dust where the lead author is most familiar with the literature) are very well referenced, but other aspects are not for example the fundamentals of the ITCZ position, moisture flux diagnostics etc – more effort is required in these areas.

I would question the logic of including description of the model simulations as an Appendix. This really should be included in the body of the text. I found myself wondering how the simulations were performed, what differences there were in the simulations compared to previous simulations etc. It seems to me that the paper was possibly designed for a high impact journal, where methods are typically shunted to the end of the paper, to concentrate on the results. This is however inappropriate for ACP. The description of the modelling efforts are quite jumbled and not clear.

We regret to have left this section as an Appendix when it should have been part of the main text as the three Reviewers agree upon. We now moved the section describing the simulations and how the optical and physical aerosol properties are treated to Section 2 immediately after the introduction. We have also tried to sharpen the description of the model and the experiment design in this section.

The SW and LW impacts are, for me, very difficult to interpret as they are not presented in a logical way. What are the SW and LW impacts at the surface and the TOA? They really should be documented better – a Table perhaps?

Following the Reviewer's suggestion, we have added the following Table that details the SW and LW impacts at the surface, at TOA and in the atmosphere both for the Sahel region and globally. Since observational constraints exist on the dust aerosol optical depth, we have 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 Effect (Wm^{-2})		
			SW	LW	Net
Sahel (15°W-35°E; 10°N-20°N)	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 effect in the different model simulations. The atmospheric radiative effects (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).**

There are several omissions that compound the lack of completeness for example, what wavelength are you considering in Figure 2? This really does need to be stated as I can't find it in the text. This should be included both in the text and in the caption.

We now indicate both in the text and in the caption of Figure 2 that the wavelength considered is 550nm to be consistent with the measurements from Ryder et al. (2013, 2018).

I completely appreciate that it must be difficult to write in a non-native language, but some of the authors (e.g. Olivier Boucher) are bilingual and would be able to sort out some of the problems with the lay-out that are currently hampering the reader's understanding.

Thank you for your understanding, we have tried collectively to iron out these problems.

Specifics comments (Major and some more minor):

Introduction: The discussion needs to include some of the more fundamental aspects of the control of the ITCZ position (see general comments). I was quite surprised to see this absent. Huang and Frierson (2013) provide one of the most accessible analyses and physical explanation of the processes that drive the Hadley circulation and the relationship between energy and moisture transport in the upper and lower branches of the Hadley cell.

Done. See answer to the main comments above

L33-34: "We show here how a better representation of dust aerosols leads to an unequivocal improvement in the simulation of precipitation over key climatic tropical region" – you should state that this is for a single model.

Changed to (lines 16 to 17 in the revised version):
" We show that the improvement of the simulated precipitation, documented here for the IPSL-CM6 climate model, results from a thermodynamical and dynamical response to dust absorption, which is unrelated to natural variability."

L40: The sentence : "Conversely, (Haywood et al., 2016) discuss how some tropical precipitation biases can be reduced by changing the model's energy balance between the Northern and the Southern Hemispheres, but they did so through ad hoc hemispheric albedo changes." This makes it sounds as though there was little rationale behind the Haywood et al (2016) study. There was; the hemispheric albedo was changed so that the NH albedo = SH albedo, in agreement with hemispheric albedo data from e.g. CERES. I suggest changing the sentence to "Conversely, (Haywood et al., 2016) discuss how, in the UK Met Office HadGEM2 model, tropical precipitation biases can be reduced by setting the northern and southern hemispheric albedos to be equal in agreement with multi-decadal satellite observations."

Changed to (lines 47 to 50 in the revised version):
"Haywood et al. (2016) discussed how, in the UK Met Office HadGEM2 model, tropical precipitation biases can be reduced by setting the northern and southern hemispheric albedos to be equal in agreement with multi-decadal satellite observations."

Section 2 in general. I wasn't sure what the model simulations were being compared against. Some of this is because of the strange lay-out without a methods section (which has been demoted to an appendix). Is it 3% versus 1.5% haematite? I suspect so, but this needs to be made clearer. I wasn't sure whether the large particles had changed? This from the appendix - "Accounting for large particles of more than 10.0 ?m follows a treatment of the size distribution with four modes (Di Biagio et al., 2020). The four-mode distribution has mass median diameters of 205 1.0, 2.5, 7.0 and 22.0 µm, respectively. The mineral composition which is described below is chosen to have the same dust absorption on all simulations." Do you mean that the difference between the models is that you have changed from a single mode to the four mode parameterisation above? This needs to be spelled out much more clearly.

We compare the simulation with dust and 3% haematite with the simulation with no dust. This is now more explicitly stated in the revised manuscript.

L65. There is no indication of the wavelength at which the co-albedo is calculated. This is fundamentally important information and needs to be corrected.

Thank you for pointing this out. We have now added in both lines 217-220 of the revised text and the caption of Figure 2 that the co-albedo is calculated at 550nm, for the same wavelength at which measurement by Ryder et al. (2013, 2018) were made.
"Figure 2 illustrates the influence of the iron oxide content and the size of a particle on its radiative absorption. In this Figure, the aerosol absorption increases along the x-axis with the aerosol co-single-scattering-albedo (coSSA) calculated at 550 nm."

L85. The statements "The difference between TOA and surface determines the dust JJAS mean atmospheric absorption due to dust (+26 W m-2 over the Sahel). Since dust is highly variable in time, particularly strong dust episodes are characterized by atmospheric absorption that reaches several hundred watts 90 per square meter (Pérez et al., 2006). Note that, in comparison, greenhouse gases contribute to a globally averaged radiative forcing of only 3 W m-2 (Myhre et al., 2013) relatively constant on short timescales."
a) I really do not think that this is a useful comparison! The radiative forcing from GHGs reported by IPCC is the CHANGE in the atmospheric concentrations since pre-industrial times.

Agreed, this sentence has been deleted.

b) I am left scratching my head about the role of LW heating within the study. This is mainly because I don't know whether the size distribution representation has been changed from the base case. The LW direct effect is not documented satisfactorily.
I will add a figure that separates the LW effect from the SW effect in the supplement of the paper.
Discuss also the Table that separates the LW from the SW effects.

L95. "A general feature of most ESMs is to have a summer African monsoon that does not reach far enough North compared to observations". Could you reference this statement? Some models have the opposite bias from what I recall…. See Huang and Frierson (2013) plot.

This statement was made from looking at Fig. 12 from Roehrig et al (2012). We have reproduced their Figure below so that the Reviewer does not have to search for this reference. Although several models show this bias, we agree with the Reviewer that the opposite bias also exist in some models. Hence, we changed the title of the paper and indicated much more clearly that the improvements in tropical precipitation may only apply to models for which the summer African monsoon does not reach far enough North compared to observations. This conclusion is conditional on a good representation of dust absorption. We hope to carry out the message that it is necessary to well describe dust optical and physical properties to capture well the African monsoon for the good reasons.

L82-100. What are the relative roles of SW and LW? LW impacts of mineral dust are far from negligible on the monthly or seasonal means (e.g. Haywood et al., 2005) and need to be drawn out better.

The new Table 1 explicitly calculates these effects for comparison. The relative values of the SW and LW effects are also reported in the caption of Figure 3.

Fig 3 caption suggests: The effects indicated to the left of the Figures are the sum of SW+LW. Where are these? They don't seem to be presented on the figures.

These effects were described in the Figure caption. We now also give the global values of dust direct radiative effect in Table 1. The caption of Figure 3 now reads:
"Over the Sahel region (10°N to 20°N; 15°W to 35°E), the net effect at the top-of atmosphere amounts to +6.0 W.m^{-2} (SW=+4.1, LW=+1.9); the atmospheric absorption amounts to +16.7 W.m^{-2} (SW=+19.9, LW=-3.2); the surface effect is -10.8 W.m^{-2} (SW=-15.8, LW=+5.1). Table 1 also indicates the values for the dust global direct radiative effect."

Figure 5. Why not concentrate on the summer months where the signal is more significant? It would be better to see this in more detail (see e.g. Haywood et al., 2016).

Great suggestion. We now concentrate on the summer months on the x-axis of Figure 5 as in Haywood et al., (2016).

Figure 6. Is a box the best way to present the detailed response of the change in inflow of the moisture flux? It provides a simple diagnostic, but it doesn't show the details of where the additional moisture that drives the

increased precipitation is coming from. I have my doubts that the moisture flux is coming from central Africa as indicted schematically by the 0.365 arrow. Moisture flux diagnostics are normally plotted as vectors (e.g. Figure S4, Haywood et al., 2016) and provide more detail about where the source of moisture is.

We agree with the Reviewer that merging the two pieces of information in a single Figure is much better. We produced such Figure which is Figure 4 in the revised manuscript.

Table 1. Is the bias in mmday-1? This should be stated. How is the change in the bias calculated? The difference in the bias does not seem to relate to the change in the bias in a consistent way. I think that it should.

We have added the units in the Table as well as the following sentence to the Table caption: "The last column indicates the precipitation change (mm day^{-1}) over the period JJAS period."

Finally, what controls the performance of the tropical precipitation appears to be the change in the cross-equatorial energy transport, which is intimately linked to the cross equatorial moisture transport (Huang and Frierson, 2013). You can see how much this improved from the "HIST" to the "STRAT" simulations in Haywood et al (2016) below when compared to CERES observations. This is not that straightforward to calculate, but at least should be referred to.

We show below the graph with this computation and the case with Dust and without Dust. This graph has been added to the Supplement Information of the revised article.

References:

Roehrig, R., Bouniol, D., Guichard, F., Hourdin, F. and Redelsperger, J.-L.: The Present and Future of the West African Monsoon: A Process-Oriented Assessment of CMIP5 Simulations along the AMMA Transect, J. Clim., 26(17), 6471– 6505, https://doi.org/10.1175/JCLI-D-12-00505.1, 2013.

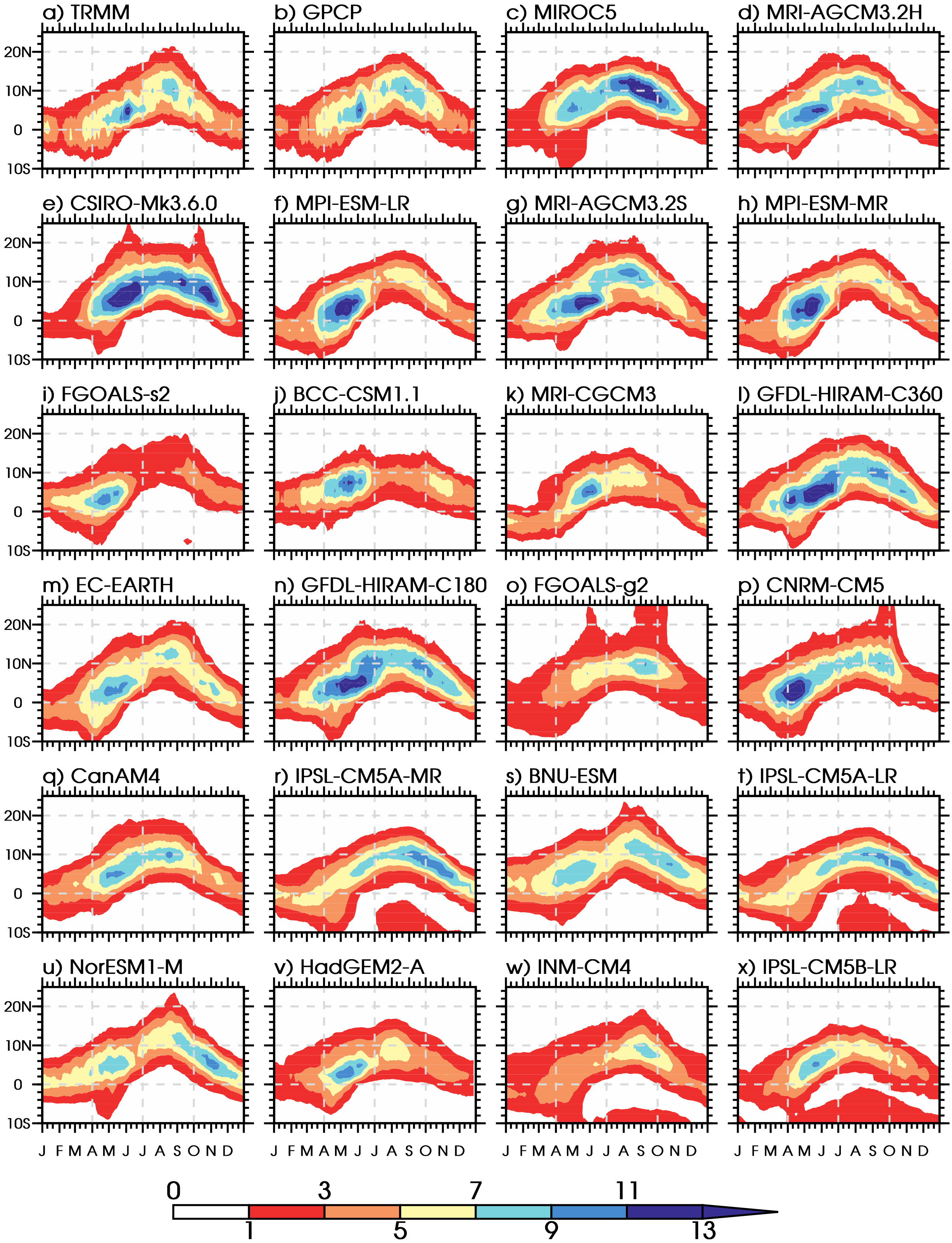

FIG. 12. Annual cycle of precipitation (mm day^{-1}) averaged over 10°W–10°E. A 10-day running mean was used on each dataset. Models are organized (top left)–(bottom right) from the warmest one over the Sahara (20°–30°N, 10°W–10°E) to the coldest one.

Meridional transport of moisture (ms^{-1} g kg^{-1}) to compare with Fig. 3 from Haywood et al 2016.
The values at the Equator are respectively: 1.128 ms^{-1} g kg^{-1} for the run NO DUST (solid black line) and 1. 326 ms^{-1} g kg^{-1} for the run with DUST, hence the enhancement factor as defined by Haywood is 1.18.