Reply to Comment RC2 from Johannes Quaas

We would like to thank Johannes Quaas for his thoughtful review that helped us think about ways to better compare dust properties with observations and strengthen the evaluation part of the manuscript. We start by answering his general comments and then answer point by point his specific comments point by point. 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.

Balkanski et al. present a modelling sensitivity study focusing on the North African region. They implement a revised representation of dust radiative effects into an Earth system model with interactive aerosols. Specifically, revisions to the dust iron oxide content, based on observations, as well as to the dust particle size distribution, are implemented. As the authors note at some part of the manuscript, they found some interesting improvement in the precipitation climatology in the region almost accidentally, and report about these in the present manuscript.
The study is of interest to the readership of Atmos. Chem. Phys. and largely well-written (although some copy-editing will help the reading).

I have two major comments and a few specific remarks:
- Energetic analysis: I think that even if some rather elaborate analysis is presented, it would be useful to present the energetics in a more straightforward way to allow for a more stringent understanding of the mechanisms. Some of the analysis is presented in units of W m^{-2}, other figures in mm day^{-1}. I recommend to convert all figures to one of the two units. It is then interesting to note that the extra dust absorption warms the atmosphere above the Sahel by 20 W m-2, and the extra precipitation by another 1.5 mm day-1 = 42 W m^{-2}. Judging from the results that are only presented as global, annual mean numbers in Table S1, only some 20% of the solar absorption are offset by additional terrestrial cooling. Is it thus a strong reduction of the sensible heat flux, due to the surface cooling, that balances the atmospheric energy budget? Or is it indeed lateral advection of MSE out of the domain?

Although we understand the desire to simplify units when they are equivalent, we chose to keep units of units of W m^{-2} when presenting dust direct radiative effect. Presenting a change in precipitation in W m^{-2} would surprise the reader. Hence we chose to keep the figures related energy fluxes in W m^{-2} but we added a double scale for the plots of precipitation change to show the correspondence in terms of latent heat. The conversion factor from W m^{-2} to mm day^{-1} is 86400/2500e3.

The sensible heat over Sahel is reduced from -61 W m^{-2} to -52 W m^{-2}.

- Corroboration of the results: So far, the study is a bit weak in evaluation. The sensitivity study is nicely motivated by improved observations of dust mineralogical composition and size distributions, but then only one outcome is evaluated with observations, which is the precipitation. The process analysis only relies on the model itself. In my opinion, the study would be much more robust if it was possible to corroborate improvement of the model outcome also by other observations. The strong reduction in surface radiation, would it not be possible to detect and attribute it in surface radiation measurements? And/or a possibly strong impact on free-tropospheric temperatures, is there hope to find a positive impact e.g. in comparison to reanalysis? In case a strong extra LW cooling due to dust is modelled, is it maybe possible to evaluate this with, e.g., CERES retrievals?

We extended the comparison of the model output to observation by comparing both terms of LW and SW top-of-atmosphere (toa) outgoing fluxes to CERES observations for all regions. We present the comparison below. Both the LW and SW toa outgoing fluxes are improved significantly over the Sahel, North Atlantic and North Africa. More surprisingly, these fluxes are also improved over the West Indian Ocean. We thank Johannes Quaas for pushing us to look at this comparison.

Regions	rlut vs CERES No Dust vs. GPCP			rlut vs CERES Dust vs. GPCP		
	Bias	RMSE	Correlation	Bias	RMSE	Correlation
Globe	-3.71	11.0	0.956	-3.65	10.6	0.959
N. Atlantic (50°W-20°W; 0-30°N)	-8.17	10.2	0.915	-7.48	9.12	0.939
N. Africa (18W-40E; 0-35N)	6.55	18.1	0.877	2.64	13.4	0.922
Sahel (16W-36E; 10N-20N)	26.1	28.0	0.92	15.7	17.7	0.947
West Indian Ocean (50E-70E; 10S-15N)	-6.24	10.8	0.889	-5.34	8.94	0.918
Equatorial Pacific (120E-90W; 10S-10N)	0.263	18.4	0.767	0.202	18.0	0.779
Western Europe (0-50E; 35N-60N)	1.01	6.95	0.94	2.12	6.79	0.95

Regions	rsut vs CERES No Dust vs. GPCP			rsut vs CERES Dust vs. GPCP		
	Bias	RMSE	Correlation	Bias	RMSE	Correlation
Globe	4.0	16.3	0.917	3.78	16.0	0.918
N. Atlantic (50°W-20°W; 0-30°N)	10.4	14.1	0.912	13.7	16.3	0.932
N. Africa (18W-40E; 0-35N)	-3.42	18.7	0.78	-2.11	17.1	0.797
Sahel (16W-36E; 10N-20N)	-15.1	19.8	0.801	-13.5	16.0	0.816
West Indian Ocean (50E-70E; 10S-15N)	5.56	11.6	0.778	5.89	10.8	0.801
Equatorial Pacific (120E-90W; 10S-10N)	0.712	20.9	0.774	0.674	20.8	0.777
Western Europe (0-50E; 35N-60N)	-11.7	15.1	0.932	-11.6	14.9	0.938

For 2-D maps see: https://vesg.ipsl.upmc.fr/thredds/fileServer/IPSLFS/ybalkanski/C-ESM-EP/Yves_first_comparison_ybalkanski/DustPrecipitationMaps_vs_obs/atlas_DustPrecipitationMaps_vs_obs_Yves_first_comparison.html

Minor remarks:

l17 "and is unrelated" (?)

We have modified the sentence from:
" We show that the improvement results from a thermodynamical and dynamical response to dust absorption is unrelated to natural variability."
To
"We show that the improvements documented here for the IPSL-CM6 climate model results from a thermodynamical and dynamical response to dust absorption, which is unrelated to natural variability."

l68 At which wavelength is this SSA defined?

We now indicate that the SSA is defined for 550nm in lines 127-128:
"Figure 2 illustrates how co-albedo (1.-SSA), SSA (single scattering albedo at 550 nm) varies with increasing iron oxide content and the effect of considering large particles (diameter > 10 µm)."

L71 "less than 10 µm" in radius or diameter:

Lines 171-176:
"In this Figure, the aerosol absorption increases along the x-axis with the aerosol co-single-scattering-albedo (coSSA) calculated at 550 nm. The coSSA is defined as:

$$coSSA = 1. - SSA \quad (1)$$

the solid blue line illustrates the absorption calculated when only dust particles with diameter less than 10 µm are considered."

l72 What size distribution is assumed for this result? Is it an average over the simulated one? Or a certain prescribed distribution?

We added the following information in the caption of Fig. 2:
"The assumed size distribution comes from observations made during the Fennec campaign by Ryder et al. (2013) and is shown in Fig.1 of Di Biagio et al. (2020)."

l142 I recommend to denote the surface solar radiation flux similarly to the terrestrial one. Also using the partial-derivative symbol is uncommon. It should be clarified that this form of the equation only holds in some sort of equilibrium since it does not include a surface heat storage term. As noted as major comment, I think it would be useful to show also the sensible heat flux and the impact of the dust on it as a figure.

Following the remark from the Reviewer, we changed from the partial-derivative symbol to a change 'Δ'
Equation 2 is now written as:

$$\Delta F_{surf}^{SW} + \Delta F_{surf}^{LW} + \Delta LE + \Delta S_E \approx 0$$

To illustrate how accurate this equation was, we now added Table 3 to the manuscript.

Flux Differences (W m^{-2}) between the cases 'Dust 3% Haematite' and 'No Dust'					
	ΔF_{surf}^{SW}	ΔF_{surf}^{LW}	ΔLE	ΔS_E	$\Delta F_{surf}^{SW} + \Delta F_{surf}^{LW} + \Delta LE + \Delta S_E$
Annual Global	*-1.57*	*0.74*	*0.25*	*0.61*	*-0.03*
Annual Sahel	*-26.85*	*18.48*	*-2.87*	*11.10*	*-0.14*
JJAS Global	*-1.34*	*0.86*	*0.09*	*0.48*	*0.08*
JJAS Sahel	*-23.43*	*18.44*	*-2.71*	*7.66*	*-0.05*

Table 3. Annual and JJAS flux differences estimated globally and for the Sahel region (15°W-35°E; 10°N-20°N) for the net SW and LW fluxes at the surface, the latent heat and sensible heat fluxes averaged over the 30-year period from 1985 to 2014.

L176 "too little"

Corrected.

l193 In the introduction, the authors explain also the importance of dust for aerosol-cloud interactions. It would be useful if this model description section clarified what aspects of such interactions are represented in the IPSL model with which parametrisations.

We added to following sentences in lines 127-130: "In the IPSL-CM6 model, dust is considered hydrophobic although laboratory measurements have shown that dust can act as condensation nuclei in certain environmental conditions (Nenes et al., 2014). The parametrization of dust acting as an ice-seeding particle has yet to be included."

L200 "several' modes – is this an arbitrary number?

We now indicate lines 81 to 83 that they are 4 modes that represent the size distribution. This new size-distribution representation was published in Di Biagio et al. (2020).
"The size distribution is represented by one or by four modes, each one represented with a log-normal distribution consisting of a mass median diameter which varies in response to the sink processes that affect the dust cycle."

L201 "either" not followed by an "or"?

We modified this sentence accordingly.

L 240 This seems to be a mistake, as Table 1 is the comparison of precipitation statistics.

We had omitted a reference to Balkanski et al. (2007), which we have now added:
" *We refer the reader to Table 1 of Balkanski et al. (2007) that explains the abundances of the different assemblages and minerals.* "

L250 June typically is defined as part of boreal summer, but September usually isn't.

When we reviewed the literature on the West African Monsoon, many studies covered the period JJAS instead of the boreal summer (JJA). We took out the term 'summer'.
Lines 134 to 136: "We analyzed the last 30 years of the coupled simulations (1985-2014) for the period when precipitation is most abundant over the Sahel from June to September referred to as JJAS in the rest of the text."

L258 Reference is missing
The following reference has been added:
Fouquart, Y. and Bonnel, B.: Computations of Solar Heating of the Earth's Atmosphere—A New Parameterization, Beiträge zur Physik Atmosphäre, 53, 35–62, 1980.

L284 "\rho"; q and u should be explained separately, and u needs to be written as a vector.

We added the following text, lines 161-162: "*where* $\boldsymbol{\rho_w}$ *is the density of water,* $\boldsymbol{g}$ *is the acceleration due to gravity,* p_s *is the surface pressure,* $\langle \boldsymbol{q\vec{u}} \rangle$ *represents the monthly mean of the product of the water content times the wind speed.*"

l292 What is the \delta for in this equation? Also in the vertical gradient, it should be a "\partial p" in the denominator.

Following the recommendation of the Reviewer, this moist static energy change is no longer shown in this manuscript.

L294 Why not q rather than 'ovap'?

This variable does not appear anymore

l382-394 Double references

We have suppressed the double references.

L491 It is not understandable what is meant by "The effects indicated to the left of the Figures..."

This sentence has been deleted

l507 The units need to be provided

We now indicate that the precipitation changes are in mm.day^{-1}

Fig. S1 I do not understand the colour coding. The legend says this is the transport at 800 mb over oceanic regions, yet the height is also expressed in mb. How can this be isobaric over ocean but then change pressure height as soon as it crosses the shoreline? Also it seems not overly useful to analyse (a) in terms of RH, and (b) at one level only. Why not rather corroborate the budget analysis shown in Fig. 6 by a supplementary figure that shows the vectors of the moisture flux as integral from the surface to 200 mb?

The pressure follows terrain coordinates hence the change over land. We have merged this Figure with the one showing the changes in precipitation due to dust absorption (Figure 4) following the recommendation of Reviewer Jim Haywood.

Fig. S2: I do not find this figure very instructive, and the authors seemingly not either: it is almost not explained and discussed in the manuscript, and accordingly I have difficulties understanding what the authors want to convey with it.

We agree and we have removed this Figure from the paper.

References :

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Di Biagio, C., Balkanski, Y., Albani, S., Boucher, O., & Formenti, P. (2020). Direct radiative effect by mineral dust aerosols constrained by new microphysical and spectral optical data. *Geophysical Research Letters, 47,* e2019GL086186. https://doi.org/ 10.1029/2019GL086186.

Ryder, C. L., Highwood, E. J., Rosenberg, P. D., Trembath, J., Brooke, J. K., & Bart, J. K. (2013). Optical properties of Saharan dust aerosol and contribution from the coarse mode as measured during the Fennec 2011 aircraft campaign. *Atmospheric Chemistry and Physics*, 13(1), 303–325. https://doi.org/10.5194/acp-13-303-2013.