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Comment on acp-2021-12

Johannes Quaas (Referee)

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-RC2>, 2021

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?

- 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?

Minor remarks:

I17 "and is unrelated" (?)

I68 At which wavelength is this SSA defined?

I71 "less than 10 μm " in radius or diameter?

I72 What size distribution is assumed for this result? Is it an average over the simulated one? Or a certain prescribed distribution?

I142 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.

I176 "too little"

I193 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 parameters/satellites.

I200 "several" modes – is this an arbitrary number?

I201 "either" not followed by an "or"?

I240 This seems to be a mistake, as Table 1 is the comparison of precipitation statistics.

I250 June typically is defined part of boreal summer, but September usually isn't.

I258 Reference is missing

I284 " ρ "; q and u should be explained separately, and u needs to be written as a vector.

I292 What is the Δ for in this equation? Also in the vertical gradient, it should be a " ∂p " in the denominator.

I294 Why not again q rather than "ovap"?

I382-394 Double references

I491 It is not understandable what is meant by "The effects indicated to the left of the Figures..."

I507 The units need to be provided

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?

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.