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 Huang 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, Huang, 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…..

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.

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?

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.

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.

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.

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.

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."

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 μm 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.

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.

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.

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.

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.

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.

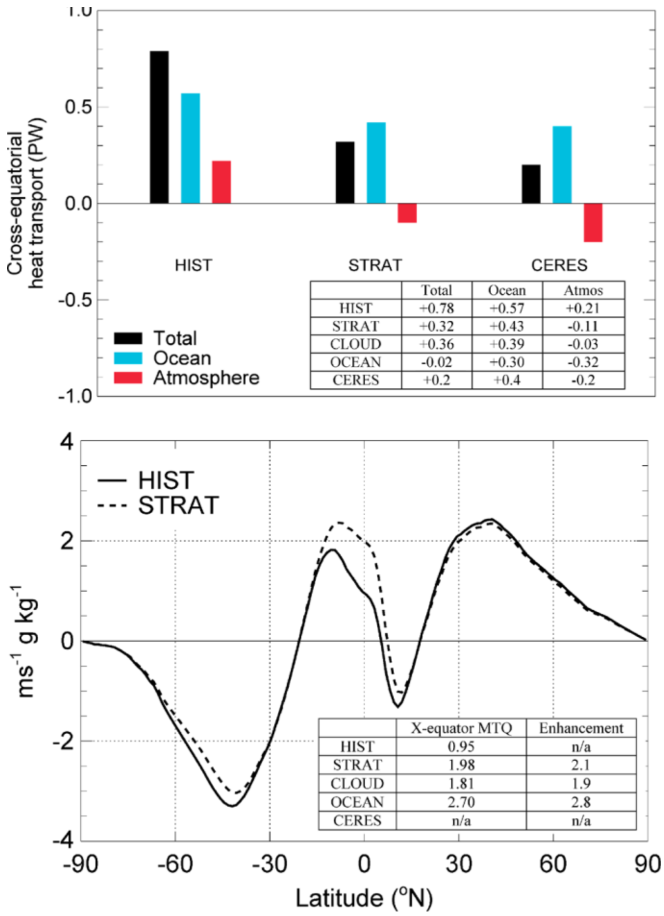
Fig 3 caption suggests: The effects indicated to the left of the Figures are the sum of SW+LW. Where are these? They don't seem to be presented on the figures.

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).

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.

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.

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.



	Total	Ocean	Atmos
HIST	+0.78	+0.57	+0.21
STRAT	+0.32	+0.43	-0.11
CLOUD	+0.36	+0.39	-0.03
OCEAN	-0.02	+0.30	-0.32
CERES	+0.2	+0.4	-0.2

	X-equator MTQ	Enhancement
HIST	0.95	n/a
STRAT	1.98	2.1
CLOUD	1.81	1.9
OCEAN	2.70	2.8
CERES	n/a	n/a

References:

Haywood, J.M, Allan, R.P., Culverwell I., Slingo, A., Milton, S., Edwards. J.M., and Clerbaux, N., Can desert dust explain the outgoing longwave radiation anomaly over the Sahara during July 2003? J. Geophys. Res., 110, D05105, doi:10.1029/2004JD005232, 2005.

Haywood, J.M., A. Jones, N. Bellouin, and D.B. Stephenson, Asymmetric forcing from stratospheric aerosols impacts Sahelian drought, Nature Climate Change, 3, 7, 660-665, doi: 10.1038/NCLIMATE1857, 2013.

Hwang, Y. T., and D. M. Frierson (2013), Link between the double-intertropical convergence zone problem and cloud biases over the Southern Ocean, Proc. Natl. Acad. Sci. U.S.A., 110(13), 4935–4940.

Oman, L., Robock, A., Stenchikov, G. L. & Thordarson, T. High-latitude eruptions cast shadow over the African monsoon and the flow of the Nile. Geophys. Res. Lett. 33, L18711 (2006).