Review of Drugé et al. “Radiative and climate effects of aerosol scattering in long-wave radiation based on global climate modelling”.

I have reviewed this manuscript considering the interactive discussion that preceded this revised manuscript. The study provides an interesting insight into the role of longwave scattering from aerosols and its representation in modelling studies. The introduction very nicely sets up the knowledge gap that the study then focuses on. The methodology is appropriate, and the results provide some clear conclusions. However, I believe there are several elements that need to be discussed further and some limitations to the study that should be included in the conclusions. Therefore, I recommend publication in ACP following some minor revisions that are detailed below.

1. Missing larger dust particles

In the introduction, the authors note that the community has demonstrated an important (yet uncertain) source of sensitivity from coarse dust particles over 20 um in diameter. Di Biagio et al. (2020) make an important point that a lot of the impact from representing these larger dust size modes is due to a compensating reduction in the concentration of the smaller particles that have an opposing radiative effect (cooling vs warming). However, line 211 in the revised manuscript states that the aerosol scheme used to prescribe fields of aerosol extinction only represents dust up to 20 um. If this is the case, then I am interested to know how the authors think their results and conclusions are affected by the omission of larger dust particles. I suggest the authors include a short paragraph in the conclusions section to discuss this in reference to the cited studies from the introduction.

2. Dust evaluation

The authors demonstrate that there is a latitudinal dipole in the coarse AOD bias (around lines 227- 230). Does this point to structural deficiencies in the model? If the model is overestimating coarse dust over the Sahel, then does this weaken their conclusions over the region? I suggest that the authors expand the lines stated to provide a potential explanation for the opposing biases in the region. Do the AeroCom models also demonstrate a dipole in the bias over Northern Africa? This discussion should also be included in the conclusions around line 336, especially with regards to how this influences the other conclusions (i.e., the strong cloud/precip response over the Sahel).

3. Impacts to cloud / precipitation.

I agree with reviewer #2 that there was a lack of in-depth discussion around the drivers of the cloud fraction changes. These changes are key elements of the story. Although the authors have made some progress in this, I believe there are remaining questions. The current explanations are not convincing.

Line 264. What is the mechanism that is driving the enhanced high cloud fraction in all regions of interest? For the Sahel, the authors demonstrate that it is associated with enhanced updraught speeds aloft but do not provide a robust explanation. Is this deep convection? Isn't the atmosphere stabilized? What is happening in the other regions – I suggest the authors include thermodynamic profiles (as A6 for the Sahel) for the other two regions.

Line 279. Where does the significantly enhanced water vapour come from?

On line 279 the authors say there is a reduction in low-level convection due to stabilization but then associate the stabilization with more convective rain below 700 hPa. Please expand this to explain this juxtaposition.

I don't think 'wetter atmospheric layers' adequately explains the precipitation response. This suggests that there is enhanced liquid water content in all clouds throughout the column (do you see this?), but this is not consistent with enhanced convection above 700 hPa (which I assume is deep convection rather than elevated convection?). Looking at the change in precipitation as a function of intensity (mm hr-1) may provide a clearer picture – the lower intensities would be associated with lower altitude clouds / shallow convection and the higher intensities with deep convection.

Finally – have other studies seen cloud responses like this?

4. Conclusions

Line 324. How do the typical treatments compare to this full representation of aerosol LW scattering? Do these results help establish whether they are insufficient?

The authors should consider extending the final paragraph to detail other limitations of the study. This may include (but not limited to…) the lack of dust larger than 20 um, sensitivity to unresolved / parameterized convection, the representation of cloud microphysics, uncertainties in the aerosol model, and remaining uncertainty in the dust refractive indices.

5. Variable names

The variable names are often unintuitive – e.g., tntrl, Wap, rsscs. Please consider replacing all of them with alterative variable names – such as $LW_{\uparrow TOA}$, $LW_{\downarrow SFC}$ clear-sky, $T_{SFC,max}$.

6. Figures

The figures need some work to get to a necessary standard for publication.

Figure 1 is not colorblind friendly and could benefit from thicker lines.

Figure 2 I suggest plotting the filled circles above the coast/country lines and clearly separating the notation for 8 and 10 (is it??). The colorbar labels has been cut off.

Please try to avoid combining plots as in Figure 3 and Figure 4 (and others in the appendix) or have different sized subplots and axis labels etc. I hope you agree it doesn't look great. A6 is another plot that does not look good due to different sized subplots and text.

Consider using thicker lines in all line plots.

For the global/regional plots, consider reducing either the cross hatching or replace with dots – it's almost impossible to see the magnitude of the response below the hatching.