We would like to thank the anonymous referee for his comments mentioning different points listed below. The reviewer's comments are in black, and the answers are in red. New information and explanations in the new version of the article are italicized.

Anonymous Referee 1

I thank the authors for the revised manuscript, which can now be accepted. I just have a minor technical corrections: in Figure 5 the caption still says "No significant changes in confidence intervals indicated in light color" I think the correct line should be "confidence intervals indicated in light color". Please double check the caption.

Done.

We would like to thank the anonymous referee for his comments mentioning different points listed below. The reviewer's comments are in black, and the answers are in red. New information and explanations in the new version of the article are italicized.

Anonymous Referee 3

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.

We agree this is an important limitation to take into account in this study. Our aerosol scheme has indeed dust aerosols up to 20 um, as in most climate models. We have therefore added a sentence in the conclusion to mention it: *"However, it is important to note that these results may be underestimated because the coarsest dust particles (with a diameter greater than 20 um) are not yet taken into account in the model."*

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).

Our results indeed show a slight overestimation of coarse AOD in Algeria (Tamanrasset) and in Saudi Arabia, as well as an underestimation of coarse AOD at souhtern latitudes (notably in Niger and Senegal), thus leading to this latitudinal dipole in the coarse AOD bias. This bias may come either from dust emissions, which themselves depend on the wind simulated by the model, or from a bias in precipitation. The simulation used to produce these aerosol fields is already known to suffer from a misrepresentation of the West African monsoon (Roehrig et al. 2020), which could contribute to biases in dust emissions near the Sahel. AeroCom phase 3 models do not show this dipole, but they underestimate coarse AOD by 46 % when compared to 222 AERONET stations around the globe, as already mentioned in the manuscript.

The results presented in this study depend in fact on these different biases in the coarse AOD and we can legitimately assume that in regions where the coarse aerosols AOD is underestimated (as is the case over the Sahel), the radiative and climatic impacts shown are also underestimated. Conversely, the results shown over the Sahara, where the coarse AOD is overestimated, are perhaps less robust. The following two sentences have been added to the conclusion: *"It is important to note, however,* *that these results may be affected by the various coarse AOD biases discussed above."* and *"Moreover, the underestimation of coarse AOD near the Sahel, probably linked to a poor representation of the African monsoon in the simulation in which the AOD dataset was produced (Roehrig et al. 2020), could contribute to underestimating the effects in this region."*

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.

The increase in high clouds observed over the Sahel in September is associated with a deep convection regime (negative vertical velocity absolute value, see Figure A7-A) which is significantly reinforced in the LWAS simulation. A new parameter, wind divergence, has been added to the study (Figure A7-B and Figure A9). Figure A7-B clearly shows that the increase in convection observed above 700 hPa is caused by a significant increase in wind convergence at 700 hPa. The following sentence has been added/modified in the text for greater clarity: *"Conversely, above 700 hPa, Figure A7-A highlights a significant increase in convection. Associated with a deep convection regime (negative vertical velocity absolute value, Figure A7-A) and coupled with a humidity augmentation (Figure A7-C), this stronger convection, caused by a significant increase in wind convergence at 700 hPa (Figure A7-B and Figure A9), favors high clouds over the Sahel in September."*

A similar study has now been added over the Sahara in August, where a significant increase in high clouds was also observed. Figure A8, showing vertical profiles of vertical velocity, wind divergence, specific humidity and temperature over the Sahara in August has also been added to the study. As previously over the Sahel, we can see here that the high clouds increase is due to an increase in humidity (Figure A8-C) coupled with a significant increase in convection (Figure A6 and Figure A8-A) due to a significant augmentation in wind convergence at 700 hPa (Figure A8-B). The following sentence has been added to the text of the article: *"Similarly, the increase in high clouds observed over the Sahara in August is also due to an increase in humidity (Figure A8-C) coupled with a significant convection increase (Figure A6 and Figure A8-A) above 700 hPa, which is due to a significant increase in wind convergence at 700 hPa (Figure A8-B)."*

This study was not carried out over the Arabian Peninsula because no significant increase in high clouds was observed over this region (Figure 5).

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

The increase in water vapour observed in September over the Sahel is due to an increase in wind from the Atlantic Ocean. Figure A10, which shows a significant increase in ua (eastward wind) over the Sahel in September from 850 hPa, has been added to the article. The following sentence has also been added to part 5 of the article: *"The humidity augmentation observed over the Sahel in September is due to an increase in wind from the Atlantic Ocean, particularly at 850 hPa and above as shown in Figure A10."* The increase in humidity observed over the Sahara in August seems to be due more to an increase in wind from the Sahel, which is wetter. The following sentence has been added to the article: *"The humidity increase observed here over the Sahara in August seems to be due to an increase in wetter winds from the Sahel (not shown here)."*

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.

We acknowledge this sentence was not very clear. The increase in precipitation observed here over the Sahel in September is rather associated with an increase in humidity and in low and high clouds. This has been clarified in the text of the article: *"These increases in humidity and in low and high clouds over the Sahel during September may explain the increase in convec-*

tive rain previously observed during this month."

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?

The output of liquid water content has not been saved, so we cannot analyze the liquid water content in all clouds throughout the column. Similarly, looking at the change in precipitation as a function of intensity would indeed be very interesting to associate the precipitation increase with an augmentation in low clouds or, conversely, with an augmentation in high clouds, but the necessary hourly output of precipitation have not been saved so that we cannot tackle this issue.

Similar results, showing the impact of aerosols on convection, have already been shown in previous studies. The sixth IPCC report, which is based here on the study by Wang et al. 2013, shows that an absorbing aerosol layer can lead to a decrease in convection in the lower layers of the atmosphere below this aerosol layer and, conversely, an increase in convection facilitating more intensive vertical development of clouds in the upper atmospheric layers.

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?

This is indeed an interesting question. However, to answer it, it would have been necessary to carry out a simulation similar to the LWAS simulation but with a simplified account (corrective factor) of aerosol scattering in the LW spectrum and to compare the results. This question could be addressed in another study.

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.

The concluding paragraph on the limitations of the study has been amended.

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 TOA, LW¯SFC clear-sky, TSFC,max.

The text has been simplified to include fewer abbreviations for variable names. The following acronyms have been removed from the text and only retained in the figures: clh, cll, rls, rlut, tasmin, wap, tntrl.

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.

The yellow lines have been changed to brown and the lines have been thickened.

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.

Filled circles have been placed above the coast/country lines. Numbers 8 and 10 have also been clearly separated and the cut colorbar labels have been corrected.

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.

For the sake of clarity, and in order to have as much information as possible on the same figure, we thought it wise to keep these different figures. However, subplots, axis labels, etc have been harmonized to produce clearer figures. Figure A6 has also been split into two figures (vertical profiles have been given a second figure).

Consider using thicker lines in all line plots.

Done.

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.

Done, the cross hatching has been reduced to view data more clearly.