Reply to referee 1

We thank the referee for the comprehensive and constructive review which helped a lot to revise the manuscript. Below please find our point-by-point reply to the comments which are repeated here in shaded boxes.

> This work described how the mixing of dust and pollution aerosol changes dust optical and radiative properties by increasing its extinction and reducing its absorption. Furthermore it quantifies the change in radiative effect of natural dust at the top of the atmosphere, at the surface and in the atmospheric column. A short discussion of regions where the mixed dust cools or warms the atmosphere over 4 regions is also included. To be worthy of being published in ACP, this paper needs to give a reasonable amount of details on how the dust cycle is treated, what are the assumptions under which acids can be uptaken by dust and give the limitation of the optical properties derived by mixing dust with other aerosols. Here are the elements to add to this work to place it in the context of what has already been published on dust.

We have included more details on the implementation of the dust cycle, the condensation of acids and the aerosol optical properties in section 2 of the revised manuscript.

> Ridley et al., 2016 and Kok et al. (2017) that you cite have established constraints on the dust cycle using both satellite observations and 3 independent models. Please state tha amount of total yearly emissions, the yearly mean total optical depth and total deposition flux of dust in your model and compare them to these constraints.

In the revised manuscript we discuss these numbers in the last paragraph of the methodology section.

> How do you represent the particle size distribution (PSD) of dust. The PSD has a large influence on the value of your LW radiative effect. You should state how large a particle you represent and what fraction of dust particles suspended are PM1, PM10, or above 10 microns for example.

We have added information on the modal size distribution to the methodology section. One indicator for a realistic size distribution are the comparisons with AOD observations at different wavelengths shown in Fig. S1 in the supplement.

> Then, when you account for the uptake of acids by dust, please state which acids, which chemical species you consider, and what accommodation coefficients were chosen. No need to refer the reader to another paper, a simple Table can go a long way to help the reader navigate through your assumptions of uptake coefficients.

We have added Table 2.

> Other major points include:
> Page 2, between the 2 last paragraphs of this page it would be welcome to state the main questions that you are trying to answer in this paper. After reading the introduction, the reader should be fully aware in what directions the paper will take him or her.

We inserted a new paragraph.

> Page 3, line 26: The reader needs to know what refractive indices you took for dust both in the SW and in the LW, please state simply your reference for them and whether these refractive indices are coherent with AERONET observations or indicate a dust that is too absorbing compared to these measurements.

We have added the sources of all refractive indices used by the AEROPT submodel and added the refractive index figure and tables from the supplement of Klingmueller et al. (2014) to the supplement.

> Page 3, line 26. You state that you assume spherical particles and make an hypothesis of volume averaged refractive index when mixing particles. There is abundant literature that this approximation in invalid and that a dielectric or a Maxwell-Garnett approximation describes better the state of mixture of an aerosol. State why you made this choice and what is the error associated with it.

Using the volume average refractive index is the EMAC default mixing rule and has been applied in previous studies on which the present study is based. More importantly, comparing various mixing assumptions in EMAC (Klingmueller et al., 2014), where the optical properties integrate a large range of Mie size parameters and particle compositions, showed that using the Maxwell-Garnett mixing rule does not much change the results. In addition, in the same study the average refractive index mixing rule tends to yield optical properties closer to that of core-shell particles than the Maxwell-Garnett mixing rule. Considering that under certain conditions core shell particles represent real particles (in particular dust after the uptake of water) more closely than homogeneous mixed particles, there is no clear advantage in using the Maxwell-Garnett mixing rule for our application.

> Minor points:
> In the abstract please state that you treat both dust radiative effect both in the SW and LW range of the spectrum.

This is mentioned in the revised abstract.

> A Figure showing as a function of wavelength the refractive indices used separately for the SW and LW would be very useful to the reader.

We have added the refractive index figure and tables from the supplement of Klingmueller et al. (2014) to the supplement.

> Page 5 line 20: could you find any field measurements that could guide you as to whether the hygroscopic variations of dust that the model represents are well captured?

The model representation of the hygroscopic variations has been evaluated against field measurements by Metzger et al. 2016. Abdelkader 2017 evaluated the chemical ageing during the transatlantic dust transport using ground based AERONET observations and satellite retrievals from MODIS and CALIPSO. Consistent results where also reported by other studies (e.g., Abdelkader 2015, Klingmueller et al. 2018, Bruehl et al. 2018).

> Page 7 line 8: typo Change: "The distribution of the bottom of the atmosphere (BOA) forcing (Fig. 4, bottom) is similar to the that of the TOA forcing," with "The distribution of the bottom of the atmosphere (BOA) forcing (Fig. 4, bottom) is similar to that of the TOA forcing,"

This has been fixed in the revision.

> Page 7, line 25; I am expecting that for region 1 Figure 5 would show a cooling effect for the atmospheric column. On this Figure, the heating rates are positive at almost all heights, how one can reconcile this fact with an overall cooling effect?

The profiles depicted in pale colours are the heating rates caused by dust which is now explicitly mentioned in the caption. The heating rates due to the interactions are negative (blue).

> Page 8, lines 11 and 12: please give separately the contributions of the SW and the LW to the pollution free dust radiative effect of -0.08 W .m-2 and to the polluted dust of -0.13 W .m-2.

We have added the SW and LW contributions.

> I believe that this paper can be vastly improved if you account for these recommendations.
> References: Kok, J. F., Ridley, D. A., Zhou, Q., Miller, R. L., Zhao, C., Heald, C. L., Ward, D. S., Albani, S., and Haustein, K.: Smaller desert dust cooling effect estimated from analysis of dust size and abundance, Nat. Geosci., 10, 274–278, https://doi.org/10.1038/ngeo2912, 2017. Printer-friendly version
> Ridley, D. A., Heald, C. L., Kok, J. F., and Zhao, C.: An observationally constrained estimate of global dust aerosol optical depth, Atmos. Chem. Phys., 16, 15097-15117, https://doi.org/10.5194/acp-16-15097-2016, 2016.

We use both references in the revised manuscript.