Reviewer #3

We acknowledge anonymous reviewer#1 for his/her constructive comments that helped us to improve the manuscript. Replies to the reviewer's specific comments are provided in the following (text in blue) after the reviewer's comments (text in black). Line numbers correspond to the manuscript version with tracked changes.

Before replying to the reviewer's comments, we must make a clarification for the used RIs.

The refractive indices that have been used for the analysis in the SW are those by Balkanski et al. (2007), Colarco et al. (2014), and OPAC, and not Balkanski et al. (2007), WMO (1983), and OPAC, as stated in the original version of the manuscript. For the LW, we used the refractive index by WMO (1983) (instead of Colarco et al. (2014)) because Colarco et al. (2014) do not provide the refractive index in the LW. We corrected this mistake and clarified what has been done in Section 2.1 (line 164).

This paper presents a sensitivity study on radiative effects due to Saharan dust aerosols. The parameter tested in the sensitivity study are 1) optical properties of aerosols under three different spectral-resolved refractive indexes (WMO 2023, OPAC: Köpke et al. 1998, 2015 and Balkanski et al. 2007). 2) Size range of aerosol size are: 0.1 - 10 µm (as done in most of former and current models) and 0.1 - 50 µm. 3) Shape form (spherical or spheroidal shapes). Calculations are made over the desert and over the ocean. The sensitivity is first led on macroscopic aerosol radiation properties (extinction, scattering and absorption coefficients), then thanks to simulations with the MYSTIC radiative transfer model (Mayer, 2009) on radiative fluxes, and at the end, the sensitivity on radiative forcing is computed and analysed.

The paper has to be understood and evaluated as an advance for radiative transfer simulations in atmospheres containing dust aerosols but not about generalities concerning dust aerosol DRE, in contrary to the wrong understanding of anonymous author of RC2 comment. In this way, the paper answers and quantifies some open questions of radiative transfer in atmosphere containing dust aerosols, a domain that needs such kind of detailed and structured studies, since the difficulty of parametrizing the radiative transfer equation in radiative transfer models for the case of dust aerosol is contained in the lack of order of magnitude and on the unknowledge about the influence of the different parameters (especially RI, size distribution and shape). Therefore, this study is a significant advance in this topic thanks to the clarifications and the quantifications that it brings. Thus, this paper is worth to be published in Atmospheric Chemistry and Physics. The paper is welled structured, the radiative transfer simulations selected are relevant, the method and the results are well explained and well presented. The paper itself is well presented, and very clear, as well from a didactic as from a linguistic aspect. Figures and tables, are presented in a clear and ergonomic way, and the results of the simulations shown in the figures are analysed in the text of the manuscript in a meticulous way.

For all these reasons, I kindly recommend to accept this article in Atmospheric Chemistry and Physics, after the minor corrections I suggest, and after the authors briefly answer the few questions here below. If the paper will be (as I suggest it) accepted with minor corrections, please give some explanations in answers to my comments

Major comments/questions

1) Please add an acronym table to define and summarize in the same place the main used acronyms (RI, TOA, BOA, AOD, DRE, IRE, SRE, SW, LW, etc...) this can really help the readers of the paper.

Table A1 has been added at the Appendix.

2) Why did you restrict the simulation in UV to a spectral range >350 nm? Especially for scattering, the UVB (290 – 315 nm) and UVA (315 – 400 nm) are very interesting, and a nonnegligible part of the radiation reaching the earth and absorbed in the atmosphere is part of this spectral domain. It is a pity not to consider the 290 – 350 nm band.

We agree with the reviewer that it would be interesting to extend the simulations to the region 290 – 350 nm and see how different dust parameterizations affect the modeling of the UV radiation. However, we did not do that for the following reasons:

1) In order to include the large spheroidal particles (i.e., radius up to 50 μm) in our calculations, we had to limit the minimum wavelength to the value of 350nm (the scattering calculations provided for spheroidal particles by MOPSMAP are for a maximum size parameter of 1005)
2) The refractive indices are more uncertain at such short wavelengths.
3) Interactions with ozone (especially tropospheric) would further increase uncertainties.
4) As can be perceived by Figure 10, not including wavelengths below 350 nm has a minor impact on the calculation of the integrals of the differences.

3) It is a bit difficult to isolate the ocean impact and the desert impact on the radiation due to the only albedo of the ocean and of the desert with this study. The reason of this is that in this study, you consider another aerosol mixture (and extinction profile) over the ocean than the one over the desert: The aerosol mixture (and extinction profile) you consider over the ocean, is an older aerosol mixture, with less large particles. It should be valuable to make a second set of simulations over the ocean with the same aerosol mixture (and extinction profile) as the one you used for the simulation over the desert.

As recommended by the reviewer, a second set of simulations was performed for the ocean using the same aerosol mixture as over the desert. In the revised version, the relevant information has been added in section 3.2.2 of the manuscript. New graphs have been also added in the supplement (Figs A5 and A6).

The new text is the following:

Since different albedo and temperature of the two surfaces, desert and ocean, also impact the differences between the simulations for SD10 and SD50 we tried to quantify at what extent they are responsible for the results shown in Figs 11 and 12. To estimate the impact of including large particles in the simulations with respect to surface albedo and surface temperature we repeated the simulations using the Balkanski et al. (2007) RI and the same SD, aerosol vertical profiles, and TCWV over the ocean and the desert (for the surface albedo and temperatures that have been assumed for the two surface types). The results are shown in Figs A5 and A6 in the supplement for the BOA and the TOA respectively. The results change by less than 1% and 2% over the BOA and the TOA respectively for both the SW and the LW, which shows the minor role of surface temperature and surface albedo for the differences over the desert and the ocean in Figs 11 and 12.

And the figures:

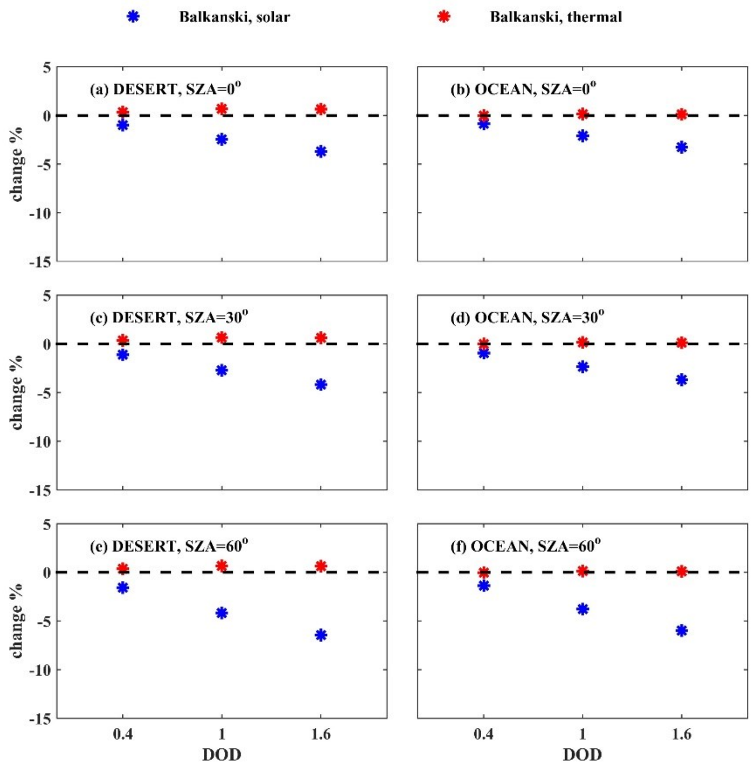

Figure A5: Relative % differences between the irradiances in the SW (blue) and LW (red) at BOA, considering SD10 and SD50 dust particles, with spheroidal shapes (FSD50,spheroids-FSD10,spheroids, Eq. 4), for the same aerosol SD and vertical distribution and the same atmospheric parameters. The differences are presented for calculations above desert and ocean for the Balkanski RI, and for different DODs and SZAs of the dust particles.

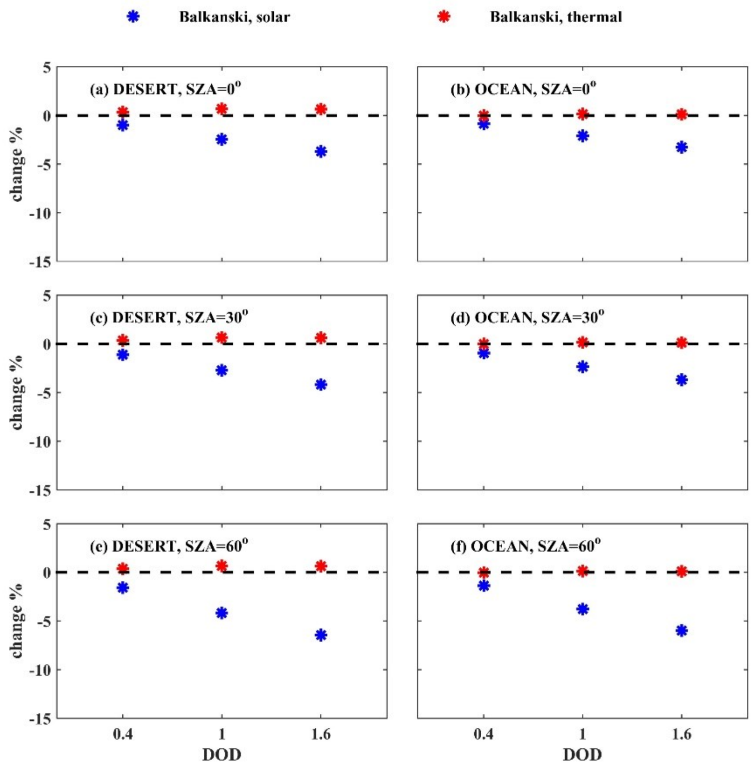

Figure A6: Similar to Fig. A5 for TOA.

4) At one point (during the presentation of the database or later during the analysis of the simulation results) you need to write something about the relative quality of the three RI spectral databases (Balkanski et al., OPAC and WMO): Which one is the more modern one? Which is the more realistic one? Which one suits better to which aerosol mixture (over desert and over ocean) and why? This commented analysis would be very welcome, since the results show that for some cases and situations, the choice of the RI database is a crucial source of differences on the radiative fluxes and on the radiative forcing.

We agree with the reviewer that the provision of such information is useful and improves the manuscript. Thus we have added relevant information (lines 167 – 169 in Section 2):

"The lower imaginary part of the RI by Balkansky et al., (2007) is considered to be more appropriate for accurately representing dust properties over the region of the campaign (Rocha-Lima et al., 2018; Ryder et al., 2019) relative to the OPAC and Colarco et al., (2014) RIs."

I definitively argue that this paper has to be accepted with only minor corrections, but if the editor decides to force you to resubmit or to make major changes, upgrading the simulations taking point 2) into account and adding a set of simulations taking point 3) into account would be a real quality gain.

Minor comments/questions:

1. Introduction

L101: "account" and "RI" written twice

corrected

2. Data and method

L111-112 Since we are at the beginning of a new part, please detail in the text the acronyms "DRE", "SD" and "RI"

Done

L145 and Figure 2: Please explain very clearly about the aerosol mixture over ocean and over desert. There are different aerosol mixtures, this is clear. But do you consider a "Lagrange approach" = these are aerosol of the same plume, that is consider at a later timestamp over the ocean, or is it an "Euler approach" = you look at the mixtures at the same moment and the ocean mixture shown at this moment was former over the desert with probably at this time the same properties as the desert mixtures?

We do not consider either, since the observations we used are derived as a mean of the observations conducted at different instances/days above ocean and desert during the Fennec campaign, (a detailed presentation about the in-situ measurements can be found in Ryder et al. (2013a), (2013b) and (2019)).

L155-159 and Figure 3: Here you can make the comments/analysis that I suggest in my comment number 4 concerning the differences of quality between the three RI datasets.

Some discussion has been added as recommended by the reviewer (see reply to major comment #4).

L170-171 and L175 (Figure 4): Explain better what is the aspect ratio. Is it something with the axis ratio of the ellipses of the ellipsoid? And explain the figure: If aspect ratio = 1 it is a sphere? And which aspect ratio did you use for the impact of shape further in the manuscript? The denomination "aspect ratio" will not be used anymore in the rest of the paper, therefore we do not understand why you show this graphic.

The aspect ratio of the spheroidal shape we consider as the dust particle shape is the ratio of the longest to the shortest diameter of the corresponding spheroid. If the aspect ratio is 1, then the particle is a sphere. In our work we consider that the dust particles have different spheroidal shapes described with the aspect ratio distribution shown in Fig. 4. The same explanation has been added to the manuscript.

L189: simulations are done on the spectral range 0.35 – 40 micrometres. At least explain why you do not consider the main part of UV spectral range (290 – 400 nm) -> See my major comment/question number 2)

See the reply to major comment #2

L233: You mention "the effect of the shape in the optical properties of dust" -> is the shape quantify with the "aspect ratio" mentioned in L170-171 and in Figure 4?

Yes, it is. See our reply in previous comment.

Table 1:

- Why did you split SW / LW at 2,5 micrometres? A rational border value is 3,5 micrometres because below 3,5 micrometres there are still solar radiation and only negligible atmospheric (thermal) emission of radiation.

Indeed, it is more common to set the border to longer wavelengths (e.g., 3.5 μm). However, at wavelengths 2.5 – 3.5 μm nearly all solar radiation is absorbed by water vapor in the atmosphere, and thus any changes in aerosol properties play a negligible role in this region. Thus, we preferred to take into account possible changes in the LW due to atmospheric emission.

- Do you have some values of the albedo (SW broadband, LW broadband, or some values at given wavelength: 500 nm and 10 micrometres for instance)?

Relevant information has been added in Section 2.3 (lines 306 - 311), as well as in Figure A2 in the appendix:

"Based on the extinction coefficient profiles and the assumed dust optical properties, the mass concentration profiles have been estimated and used as inputs for the simulations over the desert and the ocean (Fig. A1 in the appendix). It should be mentioned however that, at least for the SW, the aerosol profile has minor impact on the amount of radiation that finally reaches the top or the bottom of the atmosphere (Fountoulakis et al., 2022). The surface albedo used for the simulations in the LW is ~0.05 at 10 μm. For the SW it depends more significantly on the type of the surface (Fig. A2)"

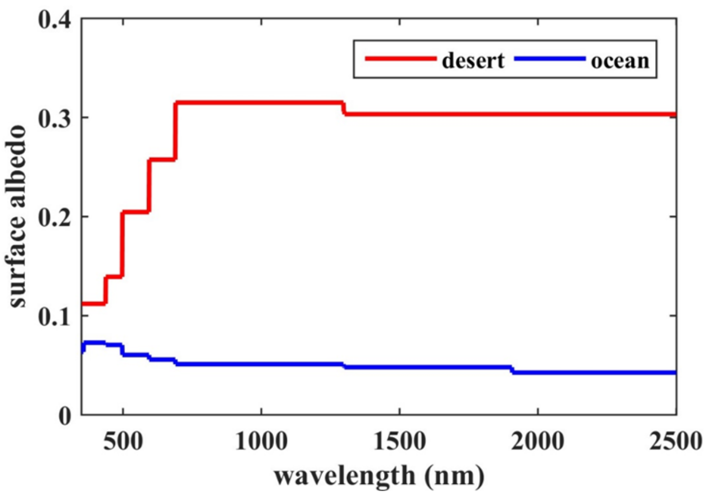

Figure A2: Surface albedo that has been used for the simulations over the desert and the ocean (International Geosphere Biosphere Programme (IGBP); Loveland and Belward, 1997).

L275: TCWV = 10 mm over the desert: Isn't it too much? I would never expect ore than 5 mm over the desert

Analysis of TCWV from MERRA-2, as well as from MODIS-Aqua shows that TCWV over western Sahara ranges from ~ 5 mm to ~25 mm depending on season. Thus, TCWV can be even higher than 10 mm, especially in the summer months.

Different studies report different values of the average relative humidity over different regions of the Sahara desert. For example, Zaiani et al. report an average value of ~ 8 mm at Tamanraset in June (month of the campaign). Even larger numbers are reported in other studies (e.g., Schrijver et al.)

Zaiani, M.; Irbah, A.; Djafer, D.; Listowski, C.; Delanoe, J.; Kaskaoutis, D.; Boualit, S.B.; Chouireb, F.; Mimouni, M. Study of Atmospheric Turbidity in a Northern Tropical Region Using Models and Measurements of Global Solar Radiation. Remote Sens. 2021, 13, 2271. https://doi.org/10.3390/rs13122271

Schrijver, H., Gloudemans, A. M. S., Frankenberg, C., and Aben, I.: Water vapour total columns from SCIAMACHY spectra in the 2.36 μm window, Atmos. Meas. Tech., 2, 561–571, https://doi.org/10.5194/amt-2-561-2009, 2009.

L289-290: A graphic with the vertical distribution of the extinction profile over ocean and atmosphere you used would be welcome

Some relevant discussion has been added in Section 2.3. A graph showing the vertical distribution of the mass concentration has been added in the appendix (Figure A2).

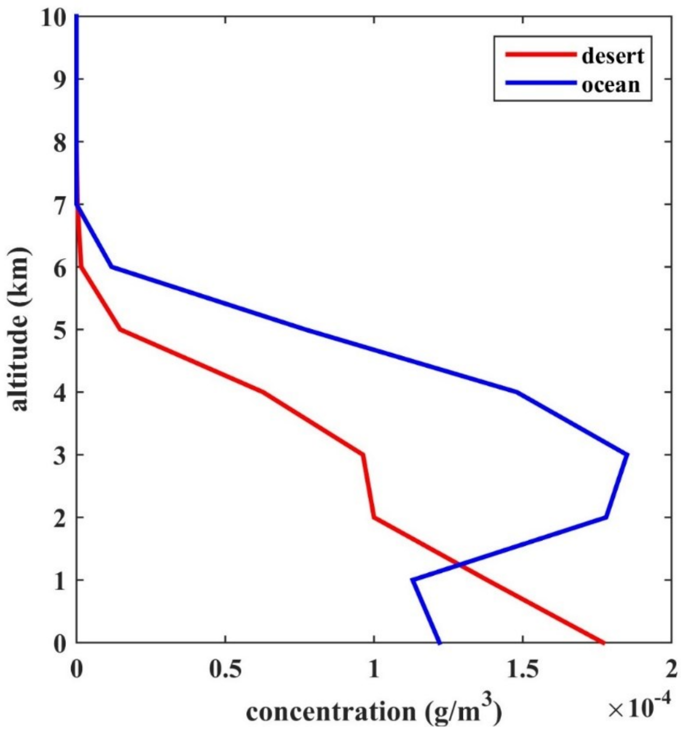

Figure A1: Dust concentration that has been assumed for the libRadtran simulations over the desert and the ocean.

3. Results

L324-331: In the analysis of the results shown in Figure 9, maybe you should in the discussion compare the differences between the results to the noise: A trend seen that is below the noise should be consider with caution.

Relevant discussion has been added in the manuscript (lines 354 - 356):

The differences that are shown in Fig 8 (for BOA) are comparable to the level of statistical noise and should be treated with caution. While the absolute magnitude of the differences for TOA shown in Fig 9 should be also treated with caution, the change in the sign of the differences is possibly real, and not the result of statistical noise.

L369 (and Figure 10): "the smallest absolute differences were found for the RI used in OPAC" -> Can you explain why OPAC leads to such different results than the results obtain with the other databases. Same question for Balkanski on graphic d (TOA desert) between 500 and 2000 nm?

The following text has been added in the document (lines 398 - 404):

The main reason why the smallest absolute differences in UV-VIS were found for OPAC is possibly that in this spectral region differences between the SSA for SD10 and SD50 are smaller relative to the corresponding differences for Balkanski et al. (2007) and WMO RIs (Figure 6g), which means that absorption of the solar radiation at this wavelengths changes less (relative to Balkanski et al. (2007) and WMO) with the inclusion of large particles in the dust mixture. At the NIR the differences in SSA for SD10 and SD50 are much smaller for Balkansky et al. (2007) relative to the other two RIs, which correspondingly results in smaller differences between the irradiances (Figure 10)

-> Here also it would be a good moment to discuss the what I asked in Point 4 of the major comments/questions above concerning the differences of quality between the three RI datasets.

We already did that earlier in the manuscript (see reply to comment 4)

L439: "For more realistic aerosol properties the DRE is less negative over the desert (by up to 25% for the RI of Balkanski et al. …" -> Should we understand that Balkanski is the most realistic RI description? If yes explain why.

Although The RI by Balkanski et al., (2007) is indeed the most realistic for the region of study, as it is now clarified in Section 2 of the manuscript, here when we write "more realistic dust properties" we mean that we are using SD50 and spheroids instead of SD10 and spheres. We changed the manuscript in order to make it clearer.

L469: Here maybe also the real place to make the comments concerning the quality of RI database (major comments/questions point 4) above)

We already did that earlier in the manuscript (see reply to comment 4)