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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Determining the infrared radiative effects of Saharan dust: a radiative transfer modelling study based on vertically resolved measurements at Lampedusa" *by* Daniela Meloni et al.

Anonymous Referee #1

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General comments: This paper lies in the framework of the ChArMEx/ADRIMED experiment, that took place in the Mediterranean in summer 2013. Three vertical profiles of atmospheric and aerosol properties, made at Lampedusa in conjunction with surface, airborne and satellite IR broadband and narrowband radiation as well as radiosonde are analyzed in order to 1) identify the sensitivity of the different radiative measurements to mineral dust microphysical properties (size distribution and refractive index) and 2) analyze their impact in term of radiative forcing. The main result of this study is that if LW irradiance is poorly sensitive to aerosol microphysical properties compared

to brightness temperature, the IR dust radiative forcing is non-negligible, and strongly depends on size distribution (SD) and refractive index (RI). This study highlights the importance of a precise knowledge of the dust microphysics to infer correctly their radiative effect. The paper is an interesting sensitivity study of the radiative variables to the aerosol microphysics, leading, in particular, to the conclusion that spectrally resolved measurements of brightness temperature is more fitted to infer dust properties than broadband LW irradiances. However, the part that concludes on the most appropriate refractive indices is less convincing. Such a study would require a more detailed analysis of the differences between refractive indices (at minimum a figure displaying their values in the spectral domain concerned), as well as a more exhaustive variability in the choice of the indices. Here among the three indices used, two indices are quite similar and only one coming from recent measurement campaign of DiBiagio et al., 2017 is really different. Moreover, the study, based principally on RT simulations, lacks of discussions on the uncertainties due to the RT model itself, as well as to the different hypothesis used. In particular, which is the impact of an error in surface temperature or surface emissivity? An error on the water vapor profile? No reference error is calculated under clear sky condition for example, to distinguish error directly due to the model from errors due to the impact of aerosols properties. The resulting biases obtained with the different aerosol properties configurations cannot therefore be really discussed. For example, large biases between simulated and calculated irradiances are not explained (and apparently not due to wrong aerosol properties), implying that something is missing in the RT model, but not enough discussed. The section on IASI data is not enough developed. All the spectra within a box of about 100 kmx100 km are averaged before analysis, causing a standard deviation in the averaged spectrum larger than the effect of the aerosols properties analyzed! Here again, since no reference errors are given, biases obtained from the different aerosol configurations are finally equivalent and it's not possible to state on the best configuration. This part doesn't really bring new information compared to the previous sections or previous studies, or required a more precise development. Finally, some details on the inputs

used are missing. A few details are provided on the size parameters used (the reader has to refer to the paper of Denjean et al. 2016 to have the precisions) and the exact refractive index from DiBiagio et al., 2017 use in this study is not given: 3 different indices are coming from ÂńÂăthe source regions (Tunisia, Algeria, Morocco) in DiBiagio et al., 2017 whereas only one is used here without any precision!

Specific comments: - p2line 21Âă: ÂńÂăMost of these studies have been carried out close to the dust source regions and did not take into account the possible modifications in dust optical properties during long range transport. ": You can also find some studies dealing with the variation of the aerosol properties with transport (e.g. Maring 2003; Ryder et al., 2013; Weinzierl et al., 2017 and so on). Moreover, I don't see the link with the subject of this paper since there is no discussion on the possible change of dust properties with transport. Maring, H.: Vertical distributions of dust and sea-salt aerosols over Puerto Rico during PRIDE measured from a light aircraft, J. Geophys. Res., 108(D19), 1–11, doi:10.1029/2002JD002544, 2003. Ryder, C. L., Highwood, E. J., Lai, T. M., Sodemann, H. and Marsham, J. H.: Impact of atmospheric transport on the evolution of microphysical and optical properties of Saharan dust, Geophys. Res. Lett., 40(10), 2433–2438, doi:10.1002/grl.50482, 2013. Weinzierl, B., Pros-Pero, J., Chouza, F., FomBa, W., Freudenthaler, V., GasteiGer, J. and Toledano, C.: THE SAHARAN AEROSOL LONG-RANGE TRANSPORT AND AEROSOL–CLOUD-INTERACTION EXPERIMENT Overview and Selected Highlights, [online] Available from: http://journals.ametsoc.org/doi/pdf/10.1175/BAMS-D-15-00142.1 - P2line 29: the paper Sellitto et al., 2016 deals with aerosols in the ULTS and not with dust. This reference is not really appropriate here. - p4line21: why describing AERONET AOD and associated uncertainties if not used? Even not compared in the following to the MFRSR? - P5: For the surface observations, several instruments measuring irradiance are described, but it's not clear if they are all used in this study. Why describing every instrument available if they are not used? Which ones are really used? This section would gain in clarity if simplified. - P6line13: maybe a summary of the description of the meteorological and dust conditions given in Denjean et al., 2016 would help? It's

easier for a reader to get all the relevant information in one paper. - P7line15: unity switched from μm to cm-1. Maybe it would be clearer for the reader to stay in μm? - P8line 18-19: How the AOP (i.e. spectral extinction, single scattering albedo, and phase function at each layer) can be derived from AERONET observation and from Deanjean et al., 2016, in particular for longwave? Observations made by AERONET or Denjean et al. are made in visible wavelength, not in the infrared part of the spectrum. Optical properties cannot be derived in the longwave by these measurements. This sentence is in contradiction with the procedure described later where the size distribution from AERONET and the ATR-42 are used with independent refractive indices to derived these optical properties. - P9line 14-15: which is the RI used from OPAC? MITR? Must be cleared. Similarly, which RI from DB2017 is used? In Table 2 "Algeria-Tunisia-Morocco dust" is mentioned, but it corresponds to 3 different indices, which one is used? - P9line 19: the sentence "the AOPs are calculated using the AERONET and the in situ SD and the OPAC water soluble RI" have to be rewritten by something like "the AOPs are calculated using either the AERONET or the in situ SD and the OPAC water soluble RI. - P9 line 4: A table given the main parametrization of the SD (radius and width of the distribution size) used would avoid to refer systematically to the paper of Deanjean et al., 2016. - P10 Eq 2: What does the symbol Delta stand for? - P10-11 and Table 3: the uncertainty for the WINDOW irradiance have been changed from 2 to 6 W.m2 from the previous version of the paper. In any cases in the text I read an uncertainty of 3W.m2 (p5, line 19). Which is the good one? In addition, the observed values for the downward irradiance and BT correspond to the average over a 10 minutes interval, what is the standard deviation of the measurement compared to the uncertainty? If the standard deviation is of the order of uncertainty, it means that the signal present small variation within the 10 minutes and maybe it would be better to compare simulations with observation, for every observation within the 10 minutes and average after instead of comparing with the average observation? If the standard deviation is larger, it means that using constant aerosol distribution is not valid. - All the section 4.1 need to be slightly reorganized. In particular, the sentence line 23-24 page 10 is very general

for the three days and the three variables, and therefore need to be at the beginning of the paragraph, as well as the sentence line 18-21, which is the associated explanation or put at the end of the section as conclusion. Furthermore, the paragraph need an overall analysis of the results obtained at the end: For LW irradiance and WINDOW irradiance, the impact of the refractive index is below the uncertainty of observation, the impact of the RI for a given SD is close to the uncertainty. For WINDOW, simulations always overestimate the observation, implying that the RT model or the calibration is not correct for this simulation and therefore it is not clear to understand what bring this variable in the study... On the contrary, for IR BT, the impact of the SD as well as the RI is significant compared to the uncertainty, this variable seems to be more appropriate to analyze AOP. - P11 line 27: "The average AOD during the descent is assumed as model input.": Which AOD is used? The average column integrated value measured by the MFRSR? - P11line30-33: there is no reason for which the NOAER simulation agree well for all the profile except close to the surface given the aerosol distribution of Figure 3. Something may be missing in the simulation to reproduce the observed downward irradiance in the lower part of the atmosphere that is not due to aerosols. - P12line 1-3: "This confirms the results found for the surface irradiance, i.e. that the broadband irradiance alone cannot help discriminating which SD and RI provide the best representation of the dust optical properties". This conclusion is not clearly stated in the previous part (see my comment on the section 4.1). - P12line 12-13: "However, while the model-measurement agreement is very good at 600 and 3300 m, where the aerosol impact is small, a systematic overestimation is obtained at 5670 m.": from Figure 7, there is no evidence of a "systematic overestimation [...] at 5670 m". COL1 and INSU3 seems to fit well observations at 12μm, whereas an overestimation is obtained at 8.7μm. At 10.6μm COL1 induces an overestimation, but INSU3 an underestimation of the observation. I don't see therefore "a systematic overestimation". - P12 line 15-16: "These results show that exploring the BT in the thermal infrared is a useful tool to infer dust optical properties if the SD is provided.": this sentence should be slightly attenuated: These results show the better sensitivity of BT to dust optical properties

than broadband irradiance but for the two other days, where aerosols are lower or with a smaller AOD, the differences between simulations with different AOP are of the order of the observed uncertainty. - P13line 26: "while no BT increase is detected at 1700 m": I don't understand this sentence, on Figure 9, there is an increase of BT at 1700m. - As for the section 4.1, this section lacks of a conclusion that summarizes the different simulations. Basically, the LW irradiance is not really sensitive to AOP (impact under the uncertainties). In addition, something appears to be missing in the simulations, because simulated upward irradiances are systematically overestimated by simulation in some part of the profile (for the three cases, even it is less important in the first one). An explanation, or at least some hypothesis, of this overestimation is missing in the paper. - P14 line 14-15 "The resulting standard deviation on the TOA spectral radiance is around 1% for 22 and 28 June and 0.5% for 3 July": this value requires to be in K, in order to be compared with the radiometric noise and more over to be compared with the impact of the different AOP. Given that 1% corresponds to a variation of about 2.9K ($\sim$1% of the surface temperature), this standard deviation is larger than the impact of the aerosol properties themselves. It should be better to apply the simulations to each spectrum and then average the differences. - Table 7: In the caption is written "Differences (K) between modelled and measured IASI BT spectra" at the beginning and "Differences are expressed as percent RMSD and standard deviation" at the end. The differences are in K or in %? The sentence "In bold the significant differences with respect to the NOAER simulations." Is not clear. What means "significant differences with respect to the NOAER simulations"? What is the criterion to put in bold the difference? - P14line23 to p15line 6: this paragraph need to be reorganized by day instead of analyzing figure 12, and then Table 7 since the conclusions are the same and it would avoid redundant sentences. - P15 line 4-5: this sentence repeat statement already given in the previous paragraph or has to be rewritten. - P15 line 7-24: the paragraph describing the analysis of Liuzzi et al. (2017) is very long to finally conclude that the impact of INSU3 is of the same order. Either better details of what this study bring compared to the previous one, or why this study use a simplify RT models compared

to the previous one is given, or this paragraph has to be shortened. But for now, it's difficult to see where the author is going. - P15 line 10: correct “or” by “for” in “The real part is also generally lower or Shettle and Fen “ - Section 4.4: it would be interesting to see also the results for the two other days and the AOP INSU1 in order to have an idea of the variations of the radiative forcing from very different cases.

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