Interactive comment on "Impact of mineral dust on shortwave and longwave radiation: evaluation of different vertically-resolved parameterizations in 1-D radiative transfer computations" by Maria José Granados-Muñoz et al
Anonymous Referee #3

We would like to thank the reviewers for their efforts and thorough review of our manuscript. We realize that the notes and suggestions made will improve the quality of the paper. Hereafter, the reviewers' comments are presented in bold font and the text included in the manuscript is marked in italics. Line numbering is referred to the new version of the manuscript.

The paper examines a case of a moderate dust intrusion in Granada with the aim of calculating the dust radiative effect either in the SW and in the LW regions at the surface, at the top of the atmosphere, and within the dust layer by means of the GAME radiative transfer model. The focus of the study is the sensitivity of the SW and LW radiative fluxes and effects on the dust microphysical and optical properties derived in three different ways, using a combination of remote sensing (AERONET and lidar) measurements and the GRASP inversion code, and in situ airborne measurements from the SAFIRE ATR42 aircraft. This study is carried out in the framework of the ChArMEx/ADRIMED campaign and takes advantage of the large observation efforts placed during the project, using either ground-based, airborne and satellite observations. The results of the model calculations are those expected (SW cooling and LW heating by dust, with a not negligible LW/SW ratio), and the case study is not that of an extraordinary dust transport (AOD moderately low). However, the most important conclusion provided by the authors is that optical properties derived from different measurements and with different techniques may provide non-negligible differences in the radiative effects, and should be of concern when estimating dust forcing. I recommend publication, but after some major issues are resolved by the authors.

Major issues

The main issue on the presented results concerns the simulation of the SW irradiances using the three different aerosol optical properties DS1, DS2, and DS3, and the evaluation of the ARE. The authors found that very close SW irradiances at surface are obtained with the three datasets (the values seem coincident in Figure 7, but no quantitative information is provided in the text), while differences in the vertical profiles of the downward SW irradiances simulated with GAME are visible in Figure 6 close to the surface when using DS1 or DS2 (which seems to provide identical irradiances than DS3). So there seems to be an inconsistency between the simulations of the vertical profiles and of the surface SW irradiance.

The same values are represented in both Figures 6 and 7 for the different datasets. They seem inconsistent because of the scales and symbols used. Figures have been modified for clarity. Additionally, quantitative information about Figure 7 is now included in the text.

As a second point, the LW ARE is very low, due to the values of the AOD, and they might be comparable to the model uncertainties. So a careful evaluation of the uncertainties on the model output should be performed. The fact that the net LW irradiance on 17 June is overestimated by the model is likely not a problem of the

CO2 and O3 profiles (the water vapor one should have been taken into account in the simulation, as it measured during the flights) used in the model (to my knowledge the impact of this minor gas is negligible), but comes from the fact that NET=Fâ ¿E ¸S-Fâ ¿ES, and Fâ ¿E ¸S is overestimated, while Fâ ¿ES is comparable to measurements. The model overestimation of the LW irradiance at the surface on 17 June (which, however, is as large as the measurement uncertainty) cannot be due to clouds affecting the measurements and not accounted in the model, because clouds increase the LW irradiance, and this should cause a model underestimation. I suggest to perform a sensitivity study to assess the uncertainty on the modelled SW and LW irradiances due to the uncertainty of the input parameters.

We agree with the reviewer that the uncertainty of the modelled irradiances should be estimated. However, a thorough evaluation of the uncertainty in GAME is out of the scope of the present study and it is worthy a publication itself. Anyway, we performed some sensitivity test to study the influence of the input parameters in the outcome of the model. Variations of the SSA within the uncertainty values considered in AERONET (0.02-0.07 depending on the AOD) show a linear variation of the ARF_{SW} at the BOA reaching up to 10 W·m^{-2} for variations in the SSA of 0.07. In the case of the asymmetry parameter, a variation of 5% which is the uncertainty considered for AERONET data, changes of up to 2 W·m^{-2} are observed in the ARF at the BOA. In the case of the AOD, we observed a maximum variation in the F_{SW} of 6.5 W·m^{-2} (0.7%) at the surface, decreasing with height, for changes in the AOD of up to 0.05, which is the difference we observe between the AOD for DS2 and DS1 on June 16. Considering these results, the total uncertainty associated to the aerosol parameterization is approximately 12 W·m^{-2}.

Page 16, line 17 onwards: "*In order to quantify these differences, we performed a sensitivity test by varying the AOD while the other parameters were kept constant. We observed a maximum variation in the F_{SW} of 6.5 $W·m^{-2}$ (0.7%) at the surface, decreasing with height, for changes in the AOD of up to 0.05, which is the difference we observe between the AOD for DS2 and DS1 on June 16. This result partly explains the differences among the three datasets. In addition, a sensitivity test performed by varying exclusively the SSA indicates that more absorbing particles are related to less $^{\downarrow}F_{SW}$ at the surface, namely a variation of 1% is observed at the BOA for a decrease in the SSA of 0.03. The influence of the SSA decreases with height being negligible at the TOA. For the $^{\uparrow}F_{SW}$, a decrease of 0.8% is observed at the BOA if more absorbing particles are present, but in this case the influence at the TOA is larger (2.2%).*"

For the LW, we have introduced a variation in the radius of 10% for the fine mode, for the coarse mode and for both simultaneously. In any case, the differences observed in the radiative fluxes are below 1 W·m^{-2}. By assuming an uncertainty in N of 10%, the variations in the fluxes are in the same range, always below 1 W·m^{-2}. However, this differences translate in differences in the ARE_{LW} of 1.5 and 0.8 W·m^{-2} at the BOA and the TOA, which is relatively large considering that the obtained values range between 2.5-4.1 and 1.3-2.9 W·m^{-2}. As discussed before, variations in the refractive index introduce variations in the ARF at the BOA of up to 1 and 0.6 W·m^{-2} at the TOA and the BOA respectively. Additionally, a strong influence of the LST on the $^{\uparrow}\text{F}_{\text{LW}}$ is observed, as will be discussed later on.

Page 19, line 25 onwards: "*On this latter day, larger differences are observed on the Net F_{LW} compared to 16 June, which might be explained by the inaccurate value of LST used due to the lack of precise data. A sensitivity test performed by increasing the air surface* *temperature measured at the meteorological station 5K indicates that the* $^{\uparrow}F_{LW}$ *increases its value up to 30* $W{\cdot}m^{-2}$ *at the surface, and around 10* $W{\cdot}m^{-2}$ *from 1 km onwards which is non-negligible. This would lead to an overestimation of the aircraft measured values, but still within a 6% difference. This highlights the need for accurate LST measurements for radiation simulations in the LW spectral range. Additionally, a sensitivity test performed by assuming a 10% uncertainty in the PSD parameters* (r_{eff}, *N and* σ) *leads to an estimated uncertainty of the* F_{LW} *retrieved by GAME of around 1.2* $W{\cdot}m^{-2}$. *As stated before, the assumption of the refractive index can also introduce variations as large as 0.8* $W{\cdot}m^{-2}$. *Considering the uncertainty of the pyrgeometer and the fact that the aircraft and the model present different vertical resolutions and time samplings and the uncertainties due to the use of the standard atmosphere or the parameterization of the surface properties the obtained differences are not significant."*

As the authors state, the CERES observations are too far in space (600 km) and in time (2 hours) to the surface observations, and a quantitative comparison with the RT model simulations is not possible. I think that the approximate results on the TOA fluxes using CERES data should be removed.

CERES data and related discussion have been removed from the manuscript.

Minor issues

Introduction: a quantitative description of the SW and LW dust radiative effect in the Mediterranean from previous studies is missing.

Additional information has been included in the introduction:

Page 2, lines18-25: *"The ARE in the Mediterranean region can be responsible for a strong cooling effect both at the surface (or bottom of the atmosphere, BOA) and the top of the atmosphere (TOA). The so-called forcing efficiency (FE), which is defined as the ratio between the ARE and the AOD, for the SW ranges between -150 and -160 W·m-2 for solar zenith angles (SZA) in the range 50-60° (di Biagio et al., 2009), being able to reach values larger than 200 W·m-2 at the BOA during strong dust events in the Mediterranean region (Gomez-Amo et al., 2011). The LW component accounts for an effect of up to 53% of the SW component and with an opposite sign (di Sarra et al. 2011; Perrone et al., 2012; Meloni et al. 2015)."*

Page 6, line 9: remove "diffuse" before downward radiative fluxes for the LW.

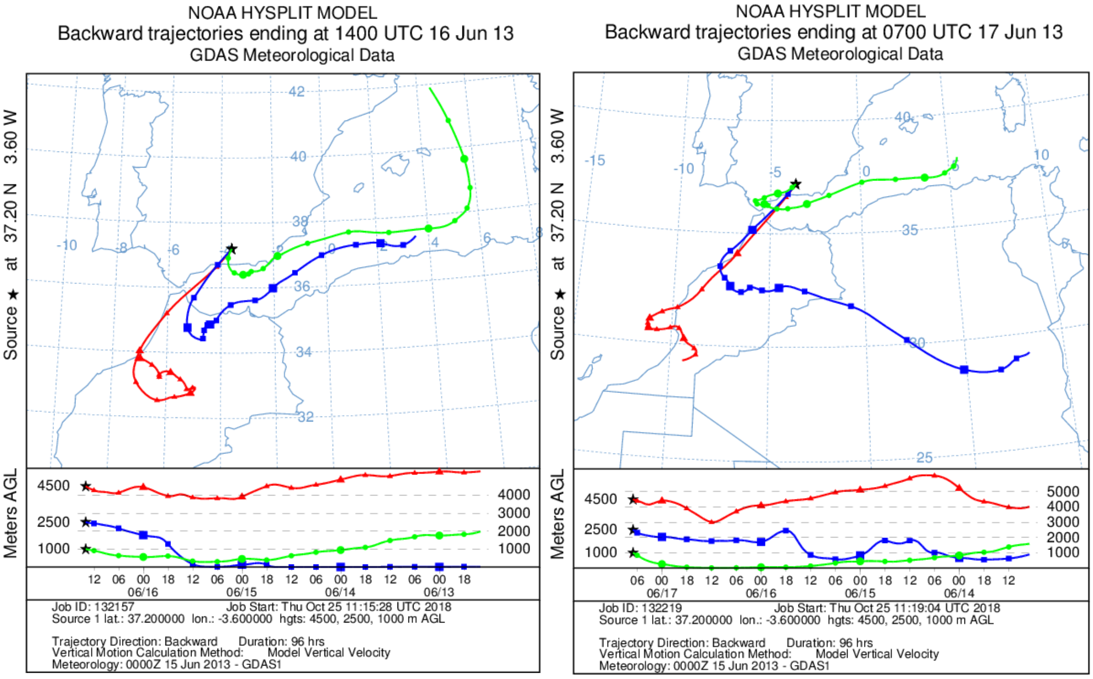
Done

Page 7, line 10: the authors should better explain how measurements corresponding to large pitch and roll aircraft angles are filtered. Moreover, this should be applied not only to downward pyrgeometer measurements, but to either pyranometers and pyrgeometers, both downward and upward-looking. The description of the correction of the ATR42 radiation measurements for the variation in the solar position and in the aircraft attitude during flight applies to pyranometers and not the pyrgeometer measurements, as stated in line 10. The final uncertainty in the airborne SW irradiance profiles is not reported.

The reviewer is right. This paragraph was referred to both pyranometers' and pyrgeometers' measurements. It has been modified.

Page 8, lines 23-24: a figure showing the airmass back trajectories may be useful.

Figure of the air mass trajectories will be added as supplementary material.


NOAA HYSPLIT MODEL
Backward trajectories ending at 1400 UTC 16 Jun 13
GDAS Meteorological Data

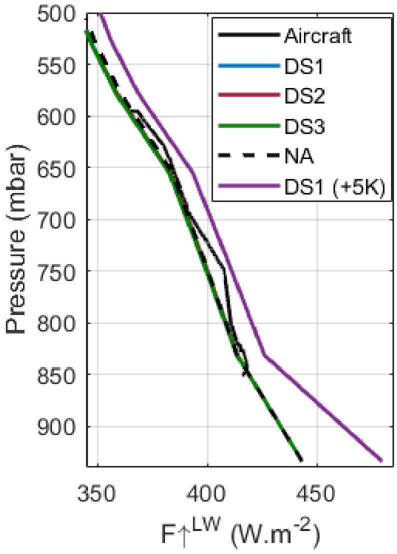
NOAA HYSPLIT MODEL
Backward trajectories ending at 0700 UTC 17 Jun 13
GDAS Meteorological Data

Page 8, line 26: change "profile" with "flight". Do the same at Page 9, line 5.

Done

Page 9, lines26-27: the authors mean that, due to the lack in MODIS data of LST the air temperature 2 m a.g.l. have been used as surrogate of the LST? This may lead to a relevant underestimation of the upward LW irradiance. Did the authors verify the differences in air and surface temperature in other cases and the impact of using one or the other in the simulation of the upward LW irradiances?

We performed a comparison between LST MODIS data and the air temperature at the meteorological station located at Granada experimental station using data corresponding to a 3-month period (May-July 2013). During daytime, the LST is on average 9.8 K larger than the surface air temperature around noon, when the satellites overpasses the station. At nighttime, the LST is 3.95 K lower than the surface air temperature. Considering the LST daily cycle and that the time of the flight was at 07:30UTC, we can expect a difference between the LST and the air temperature, but not as large as 9.8 K. A sensitivity test using the air temperature +5 K (which is the difference observed on June 16 between MODIS and the meteorological station) shows that the $^{\uparrow}F_{LW}$ increases its value up to 30 W·m^{-2} at the surface, and around 10 W·m^{-2} with increasing altitude (see attached figure). The effect on the ARF is however negligible at the BOA and it is 0.3 W·m^{-2} at the TOA.



Page 13 lines 11-12: maybe the authors mean "extrapolation" instead of "interpolation" and "longer" instead of "shorter".

Text has been modified according to reviewers' suggestions.

Page 14 line 11: use "solar" and not "visible".

Done.

Page 15 line 2: the sentence "Even though the discrepancies in the AOD are within the uncertainty..." is misleading. DS1 and DS2 use AERONET AOD measurements as input, so both should agree within the measurement uncertainty (±0.01) at 550 nm. But a difference of 0.05 is reported in Table 4. This difference should be addressed and explained.

This sentence has been modified. In our case, the AOD indicated in Table 4 is not exactly the one provided by GRASP (extinction integral from surface to TOA), but the integral of the extinction profiles between the surface and the assumed as tops of the aerosol layers, which are the ones used as input for GAME simulations. In the extinction profiles provided by GRASP, values above these tops are not exactly equal to zero, because GRASP assumed presence of stratospheric aerosols decreasing exponentially with the altitude (see Lopatin et al. 2013). For the comparison presented here we consider it is more accurate to consider only the region between the surface and the top of the aerosol layer so that we have homogenous criteria for the three datasets. Thus, a larger difference than the uncertainty is obtained between DS1 and DS2, since part of the GRASP extinction profiles (stratospheric aerosol) is not considered in the calculation of AOD.
Page 11, line 24 – Page 12, line 6: *"The AOD values presented here (included in Table 4) are obtained by integrating the* α_{aer} *profiles at 550 nm from the surface up to the considered top of the aerosol layer (4.3 km on June 16 and 4.7 km on June 17). In GRASP retrieved* α_{aer} *profiles, values above this top of the aerosol layer are slightly larger than zero since GRASP takes into account stratospheric aerosols by an exponential decay (Lopatin et al., 2013), thus the approach used here to calculate the AOD leads to lower values compared to the column-integrated AOD provided by the sun-photometer.*

Differences among the three datasets are more noticeable on June 16, when the AOD for DS1 is 0.05 lower than for DS2 and DS3, whereas on June 17 the maximum difference is 0.03, obtained between DS1 and DS2."

Page 15 lines 5-7: the spectral variation of the SSA is provided only for the wavelengths at which it has been derived (Figure 4). For example DS3 from aircraft measurements is provided only for one wavelength. This not helps in understanding how optical properties may influence the SW radiation in the whole interval.

The values presented in Figure 4 correspond to the data obtained from the measurements which are used as input in the radiative transfer model. Unfortunately, the spectral variation of the SSA is not provided by the aircraft measurements. Anyway, this part of the manuscript has been rewritten in order to clarify it.

Page 15 lines 8-9: this sentence is rather obvious, since the comparison with aircraft radiation fluxes can be done only for the altitudes covered by the ATR42.

Removed.

Page 15 line 13: maybe the authors mean "below" instead of "above".

The reviewer is right; it has been modified.

Page 15 lines 25-28: the authors should present and discuss absolute and relative differences between measurements, AERONET calculations and GAME simulations. I don't understand the sentence stating that on 16 June the radiation presents large values (than 17 June?), please explain. An overestimation by 6% by GAME is much larger than the irradiance uncertainty. The estimation of the simulated irradiances is necessary in order to understand if such a difference is within the model and measurement respective uncertainties, otherwise it should be commented and possible causes addressed.

This part of the manuscript has been modified.

Page 17 lines 8-15: the SZA at which the RTM simulations are performed should be cleared. Are they those corresponding to the middle of the F30 and F31 flights (31.49◦ for 16 June and 61.93◦ for 17 June?). The SW ARE depends on the SZA, so when the authors presents the ARE from previous studies are they sure that such values can be direct compared with theirs? I don't think so, since results from Papadimas et al. (2012) are regional summer means. The same goes for the comparison with the other references (Sicard et al, 2014a,b; Barragan et al., 2017). Moreover, ARE depends on AOD. Thus, when comparing cases with different AOD, the radiative forcing efficiency (ARE per unit AOD) may be the most appropriate quantity.

The SZA used for the simulations is included in Table 4 (31.49º for 16 June and 61.93º for 17 June) and it does correspond to the middle of the flights. More information about the SZA is now added in the text. When comparing with previous studies the SZA is now taken into account. Moreover, the forcing efficiency is also included to ease the comparison with previous studies and avoid the dependence on the AOD values.

Page 18, line 7: change “diffuse” with “longwave”.

Done.

Page 18, line 13: the upward and not the downward component of the LW irradiance depends on LST.

This part has been removed.

Page 30, Table 3: albedo in not defined in the LW region! Do authors refer to emissivity? Is it spectrally integrated or for a single wavelength?

It is the surface LW emissivity product provided by CERES, spectrally integrated in the range 4-100 µm. This information is now included in the manuscript.

Page 34, Figure 5: a shift is visible in the volume concentration profile of 17 June form GRASP code and aircraft measurements. Can the authors comment on it?

Details on the comparison between GRASP and the aircraft volume concentration profiles are already discussed in detail in Benavent-Oltra et al. (2017). The differences observed between both profiles are attributed to the different temporal sampling (the aircraft data provide instantaneous values whereas GRASP retrieval is performed using a 30-minutes averaged lidar profile) and the spatial separation (around 20 km distance between the aircraft and the lidar measurements), which can be associated to slight changes in the vertical distribution of the aerosol layers.

Page 35, Figure 6: The uncertainty range of the radiative fluxes from airborne instrumentation should be added in the plot to help understanding.

The estimated uncertainty for the airborne measured radiative fluxes is 5 $W \cdot m^{-2}$. Due to the horizontal scale used here to show the data, the uncertainty range is not visible in the plots.