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 #2

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.

This work focuses on analyzing the differences in the aerosol radiative forcing obtained by the same radiative transfer model (GAME) using different datasets as input, during a dust intrusion in the Iberian Peninsula within the ADRIMED/CHARMEX campaign. The methodology that the authors use in this work is sound, and similar research has been already done in previous papers in order to analyze the sensitivities of radiative fluxes and aerosol radiative forcing (e.g. Gómez-Amo et al., 2010; 2011; Meloni et al. 2015; 2018), and aerosol heating rates (e.g. Meloni et al. 2015; Peris-Ferrús et al., 2017), to the aerosol properties used as input in the radiative transfer models. Despite this, the most novel and interesting part of this work is the comparison between the results obtained using a very advanced and complete characterization of the aerosol properties, as well as their vertical distribution (GRASP) against those obtained from most known and widely used measurements and algorithms (i.e. Klett inversion lidar + photometer; and airborne in situ measurements). This reason is sufficient for this paper to be of interest for aerosol research in order to understand the uncertainties associated to aerosol radiative effect. Therefore, the argument of this paper is solid and then suitable for publication in ACP. However, I think there are several important aspects that can be improved, and it should be addressed before the paper is published.

GENERAL ASPECTS

In general, I miss a deeper analysis of the results, especially from a quantitative point of view.

1. Therefore, I would suggest that the authors focus their work on estimating the sensitivity of the GAME model to the different aerosol inputs., answering the following question that underlies their own figures: why the authors observe notable differences in the ARE among the datasets, in shortwave and longwave ranges, despite the differences in the vertical profiles of radiative fluxes they obtain are negligibles?. This should be done in a quantitative way by taken into account the differences among the aerosol properties used in the three datasets.

2. For this, I think that the differences among the aerosol datasets used should be better explained, in terms of the aerosol properties (i.e. extinction, absorption and scattering): If I understand well the inputs that GAME model requires for aerosol characterization ext(wavelength,z), SSA(wavelength,z) and asymmetry parameter(wavelength,z):

a. In the shortwave range DS1 - GRASP provides the spectral profiles (7 wavelengths) of the aerosol extinction and SSA. DS2 - However, the Klett inversion only provides the spectral (3 wavelengths) extinction profile (taking into account vertically constant LR). The SSA is constant with height and column-integrated

AERONET values (4 wave-lengths) are assumed. DS3 - Airborne measurements also provide information about extinction and absorption profiles; with no spectral considerations. In the three cases the column-integrated AERONET asymmetry parameter (4 wavelengths) is assumed. This information is well summarized in Table 2, but I miss better explanation in the text.

The differences in ARE observed between the three datasets are comparable to those observed in the radiative fluxes in absolute terms and therefore the results are consistent. A thorough quantitative discussion on the differences between the aerosol properties was already presented in Benavent-Oltra et al. (2017) and thus it was out of the scope of the present study. However, additional information has been added to the manuscript for completeness considering the reviewer's suggestion. Additionally, a small paragraph summarizing Table 2 and Section 3.2.2 has been added.
Page 14, lines 1-8: *"Summing up, for the SW aerosol parametrization in GAME three datasets are tested. In DS1, GRASP provided spectral profiles at 7 wavelengths of the aerosol extinction and SSA are used. In DS2, the Klett retrieved extinction profiles at 3 wavelengths are used together with the AERONET SSA columnar values at 4 wavelengths, which are assumed to be constant with height. For DS3, one extinction profile at 550 nm and a column-averaged single-wavelength value of the SSA from the airborne measurements are considered. In the three cases, the column-integrated AERONET asymmetry parameter at 4 wavelengths is assumed to be constant with height and used as input."*

On the other hand, there is a different aerosol layering among the studied days that can play an important role on the retrieved ARE. Looking at the aerosol extinction profile (Figure 2) and the concentration of Fine and Coarse modes (Fig. 5): June 16, a single homogeneous aerosol layer is observed June 17, aerosol are uncoupled in two layers. The same is observed in the SSA profiles shown in Fig. 3. have you consider to analize the role of aerosol layering in your retrieval?

We thank the reviewer for this suggestion. Because of the different SZA during both days and the different atmospheric conditions, a direct comparison between June 16 and 17 in order to study the influence of the aerosol layering is not possible. However, additional comments on the aerosol layering influence on the ARE are now included in the text considering the information available in the literature (e.g. Meloni et al., 2004; Guan et al., 2010). These studies show that the vertical structure of the aerosol has lower impact in the ARF at the surface level for low-to-moderate absorbing particles, which should be our case for mineral dust.

b. In the longwave range. The authors obtain the aerosol properties by Mie calculations as appears in Tables 2 and 4. However, it is not clear what radius are used in Mie calculations. Sometimes the authors assert they use the reff and nevertheless, in table 4 the radii appear in the 2 modes (fine and coarse). Please be clear and consistent.

The radii values presented in Table 4 are those corresponding to the effective radius of both the fine and the coarse modes. We show these values here because they are directly taken from the different databases. These data are later on processed and converted to the corresponding modal median radius for Mie calculations. Text and corresponding symbols have been modified for clarity purposes.

3. The results should be analized taking into account the quantitative differences among the aerosol properties used in the aerosol datasets, considering - spectral variation- vertical variation. Considering that the main differences among the aerosol datasets are based on differences in the vertical profile of extinction and absorption, the authors should take into account the work already published in this regard, using other models and different datasets. For example, to help in the interpretation of the differences observed in the shortwave, I would recommend reading of: Meloni et al. 2005. Where the effect of the extinction profile on the calculation of the ARE is analyzed. Different works by Gómez-Amo et al., 2010 and 2011, as well as by Guan et al., 2010. Where the effect of vertically varying the aerosol absorption in the determination of ARE is analyzed.

We thank the reviewer for these references. Additional discussion considering the results presented by those authors is now included in the manuscript (e.g. Page 17, lines 3-5: *"The vertical distribution of the SSA also influences the radiative fluxes in the SW as demonstrated in previous studies (Gomez-Amo et al., 2010; Guan et al., 2010), contributing to explain the differences observed among the three datasets analyzed here."*)

Main concerns about results and conclusions sections: SW: Is difficult to understand that with such small differences among the different dataset input (below 1% for radiative fluxes and 0.05 for AOD), why do the authors obtain such large differences in the AREsw (up to 33%)? I think that this is the question you have to answer in this paper, using your data and simulations, which is missing in this paper. At fixed solar zenith angle, the shortwave fluxes are mainly dependent on the AOD. The direct flux is totally driven by the extinction (AOD) and with such small AOD variations between datasets I do not expect large differences in the fluxes (just what you obtain and is shown in Fig. 5 and 6). However, the diffuse radiation is extremely dependent on SSA and the phase function (i.e. asymmetry parameter). If AOD and asymmetry parameter remain fixed, Gómez-Amo et al., (2010) showed that the differences observed in the ARF (at the surface and TOA) are driven by the vertical distribution of SSA that results in different distribution of the diffuse radiation. I would suggest repeating the analysis by removing the small variation of AOD. For example by normalizing the three datasets to the AERONET AOD, or working with the forcing efficiency, and focus the analysis on the variations due to the SSA.

The AOD difference on June 16 is 0.05, which represents nearly a 22% variation with respect to the AERONET AOD, and therefore is small but still significant, leading to corresponding differences in the ARE. Besides, we have performed a sensitivity study by fixing all the parameters except one as suggested by the reviewer to analyze the influence of each parameter on the final results and we have included the forcing efficiency values. Additional discussion is now included in the manuscript according to these results where appropriate. As for the ARE differences, if we understand the reviewer's point, even though they are large in relative values, absolute values are below 15 $W \cdot m^{-2}$, which is in the same range of the differences obtained for the fluxes. According to the ARE definition, the differences in the ARE obtained values are coherent with the differences in the fluxes.

I think it would be useful for the interpretation of the results: - to show in Fig. 4 the spectral variation of SSA for the 2 layers observed on June 17, and for the homogenous layer on June 16. - the vertical profiles of SW and LW fluxes in aerosol-free conditions should be shown in Fig. 6 and 9., respectively. (see Meloni et al., 2015; 2018)

Unfortunately, spectral variation of the SSA is not provided by the aircraft measurements and it is not possible to include it in Fig. 4 for the different layers. The more complete information regarding the vertical distribution of the SSA is already provided by GRASP results in Fig. 3. The aerosol-free conditions profiles are now included in Figs. 6 and 9.

LW: P20-L20: I do not understand this sentence: "Considering the low influence of the aerosol in the LW radiative fluxes, the influence of the assumed CO2, O3 and the used water vapor profiles and LST are needed to fully explain this discrepancy". Why do you think that the differences in the LW fluxes are due to the assumed CO2, O3 and the used water vapor profiles and LST? Did you change them among the simulations DS1, DS2 and DS3? According to table 4, these values do not change with the dataset considered.

The reviewer is right, these values did not change for the simulations using DS1, DS2 and DS3. We mean here the differences between GAME retrievals and the aircraft data. The sentence has been modified to make it more clear.

Page 22, lines 21-23: ***"****Considering the low influence of the aerosol in the LW radiative fluxes, the influence of the assumed CO_2, O_3 and the used water vapor profiles and LST are needed to fully explain this discrepancy between the aircraft and the simulated profiles.****"***

Fig 12. The authors report an ARE offset LW/SW increase with altitude, up 90% at higher altitudes, when there was not aerosol layer anymore. This is totally opposite at what is reported in Meloni et al., (2015), that found the maximum offset at the surface and a negligible variation from the top of the aerosol layer to the TOA. These results should be better discussed and justified.

A few nuances with regard to the reviewer's comment are necessary to answer this point; there are some similarities between our work and Meloni et al. (2015) but also some differences that need to be considered.

1. We also find that the LW/SW ratio between the highest aerosol layer and the TOA is nearly constant (see Figure 12).
2. In Meloni et al. (2015), SZA = 55.1º. This is comparable to our results on June 17, when SZA = 61.9º. If we look at the evolution of the LW/SW ratio between the surface and the highest aerosol layer (~4 km) on that day a decrease is observed for all datasets (the highest decrease is ~40% for DS3). This decrease in Meloni et al. (2015) is ~50 %.
3. The behavior of the LW/SW ratio plots above the highest aerosol layer may have some intrinsic artefacts due to the different vertical resolutions of the SW and LW model versions. As can be seen in Table 1, the SW version has a 2-km resolution between 2 and 10 km and the LW version has a 1-km resolution below 25 km. This makes that the vertical distribution of the AOD per layer (in the parametrization of GAME) near the top of the aerosol layer is different between

both SW and LW spectral ranges. If we take DS3 on 16 and 17 June, the ARE_{SW} slightly decreases between 6 and 8 km (Fig. 8) while the LW ARE_{LW} is constant above 6 km (Fig. 11). This effect will produce an artificial increase of the LW/SW ratio near or just above the top of the dust layer, which may not be necessarily insignificant as shown on the profile of DS3 on Fig. 12a.

Minor comments:

P2-L3: Please change "contrasted" by "compared" Done.

P2-L21: Please rephrase the sentence, its meaning is no clear. Done.

P4-L5: Please change "..model estimates sensitivity..." by "..sensitivity of the model estimates.." Done.

P5-L2: Please change "..real and imaginary refractive indices..." by "..real and imaginary parts of the refractive index.." Done.

P5-L24:Plase remove "particle" Done.

P5-L29: Please change "..spatial integral..." by "..vertical integration.." Done.

P6-L6: Please change "in" by "by" Done.

P9-L27: How the surface temperature was estimated from the measurements at 2m above the ground? please provide a reference. Done.

The LST is assumed to the temperature measured at the meteorological station, located at 2 m agl. The sentence has been rephrased and discussion is now added regarding this assumption.

Page 19, from line 25: *"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."*

P11-L12: This sentence is really surprising. I do not understand well the differences in the AOD among the datasets reported in table 4. Since the AOD measured with the CIMEL photometer is imposed as a closure condition in the GRASP and Klett inversions, one would expect similar AOD for DS1 and DS2 datasets, contrarily to what is reported in Table 4. On the other hand, AOD differences are expected from DS1 and DS2, with respect to DS3 (aircraft exctinction). These differences may be also due to the AOD content from surface to the minimun altitude of the aircraft, or for the observation of different airmasess (20km far from ground-based station and aircraft). I think the authors should clearly discuss these AOD differences, since the discussion about the differences in the ARE results is mainly addressed in terms of AOD.

In our case, the AOD indicated in Table 4 is not exactly the one provided by GRASP, but the integral of the extinction profiles between the surface and the altitude assumed in this work as the top of the aerosol layer, which are the ones used as input for GAME simulations. The GRASP extinction values above this assumed top layer are not null since GRASP assumes stratospheric aerosol presence which exponentially decreases with altitude from the highest altitude used as input (as can be seen in Lopatin et al. (2013),

being the GARRLiC scheme the same used in GRASP lidar retrieval). This fact makes that the integrated GRASP extinction used in this work (integrated until the assumed top aerosol layer but not until TOA) underestimates the AOD from AERONET (stratospheric aerosol is not considered). For the comparison presented here we consider it is more accurate to consider only the region between the surface and the assumed 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.
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."*

P13-L2: This approach is similar to the used in Meloni et al., (2015,2018) and Peris-Ferrús et al., (2017). This papers should be cited in this paragraph.

References have been added.

P13-L11: Are you sure about this sentence? Is there any typo error? wavelengths below 3 um are not considered LW range. What about for wavelengths over 16 um?

The Mie code in GAME requires this info to perform the computations of the aerosol properties in the LW. Extrapolation for longer wavelengths is also used, text has been corrected.

P13-L14: What radius did you use in Mie calculations? the effective radius, or the rF and rC? Table 4, Table 4 caption and the text result confusing. Please be clear.

See previous response.

P14-L11. Please change "visible" by "shortwave" Done

P14-L12. Please change "thermal" by "longwave" Done

P14-L27. When the authors talk about "discrepancies", are they refeering to "relative differences" (Fgame- Faircraft)/Faircraft? The authors should define how they obtain these discrepances, and always use the same term. Sometimes they use "discrepancies" and sometimes"differences".

Discrepancies here is referred to the absolute difference between the simulated fluxes obtained by the three datasets. Text has been reviewed according to the reviewer's comment to account for possible inconsistencies. The difference with the aircraft data is not considered in this part or the manuscript. In case of relative differences, it is now explicitly indicated in the text and it is defined how they are calculated.

P15-L5. Please clarify the meaning of this last sentence of the paragraph. The main aerosol effect over the radiative fluxes is due to the AOD, the SSA is a second order effect.

We agree with the reviewer that this sentence was confusing. It has been rephrased.
Page 16, line 26- Page 17, line 2: *"In our case, the larger AOD assumed for DS2 on both days (see Table 4 and Figure 2), causes the ↓FSW to be slightly lower compared to DS1. For DS3 the AOD is similar to DS2, but the SSA values used, which are relatively smaller compared to those measured by AERONET (see Figure 4), lead to lower values of the radiative fluxes than for DS2."*

P15-L11. I do not see the influence of the boundary layer due to the distance between the ground-based station and the aircraft leg. The DS3 simulation is set with the aircraft data, so the 20 km distance between the ground-based station and the aircraft leg should be reflected in the results obtained from the DS3 simulation.

Removed

P15-L17. The authors should take into account that relative differences about 7% for a Fdownward around 430 and 530 Wmˆ-2 yield absolute differences of 30 and 37 Wmˆ-2. These differences may represent a large fraction of the aerosol effect, with a large contribution to the uncertainties in the determination of ARE using this data. I don't think that these differences are quite insignificat as the authors assert. Have you evaluated them?

The 7% value indicated in the manuscript correspond to the maximum difference observed, whereas on average differences are below 4%. Considering that the maximum difference observed between the three datasets reaches 19 $W\cdot m^{-2}$ and the uncertainty of the pyranometer is 5 $W\cdot m^{-2,}$ together with the differences between the aircraft and the model in vertical resolution, time sampling and data acquisition and processing techniques a difference of 30 $W\cdot m^{-2}$ between GAME and the aircraft measurements is quite reasonable. Additionally, the uncertainties introduced due to the use of the standard atmosphere or the parameterization of the surface properties may also be partly responsible of the differences observed here. When calculating the ARE, the uncertainties due to the vertical resolution of the model, temporal sampling and the assumption of the standard atmosphere, the gases concentration or surface parameters are minimized.

P15-L27. Again, a 60 Wmˆ-2 differences between datasets may contribute to large uncertainties in the determination of ARE using this data. Please evaluate this.

In order to understand the differences observed here an evaluation of the CM11 pyranometer data at the surface against AERONET $^{\downarrow}F_{SW}$ has been performed using the simultaneous data available on June 16 and 17 (6 pairs of data). AERONET surface radiative fluxes have been extensively validated at several different sites around the world by Garcia et al. (2008). In addition, all AERONET sun-photometers are mandatorily calibrated once a year. Large differences are obtained between AERONET and the CM11 pyranometer, reaching up to 130 $W\cdot m^{-2}$ in one of the cases. These results point out to a likely malfunction of the pyranometer during the campaign that would explain the differences observed. We have performed simulations with GAME for the time of the closest AERONET measurement on June 16 (at 16:22UTC), assuming that the aerosol parameterization is constant with time between the flight time and the photometer measurement (even though there is an AOD increase from 0.23 to 0.25 between 14:30 and 16:22UTC according to the sun-photometer data). This simulation provides $^{\downarrow}F_{SW}$ values at the surface of 564.8, 551.8 and 547.0 $W \cdot m^{-2}$, similar to the 531.4 $W \cdot m^{-2}$ provided by AERONET. On June 17, GAME simulations at 07:40UTC (instead of 07:30UTC, which is the time of the flight), provide values $^{\downarrow}F_{SW}$ at the surface of 466.3, 468.3 and 456.4 $W \cdot m^{-2}$, very close to the AERONET value of 463.7 $W \cdot m^{-2}$.

P16-L14. I think that the comparison GAME-CERES have no sense, even qualitatively, with CERES overpass 600km. The upward flux at TOA is mainly dependent on surface albedo and clouds, and both can be really different at 600km away. I would suggest remove this comparisons, since they do not present a relevant contribution to this work.

Removed

P17-L8:L15. Since this paper is mostly to analyze differences in ARE using different datasets. The authors should carry out a more in deep analysis of the results. Specially regarding to the surface and TOA values. Even if the values obtained for the three datasets fall within the range of the values previosuly observed in the Mediterranean, notice that the values shown in Table 6 differ from a 30-50%. These differences are really large and dependent on the used dataset, and consequently it should be analyzed in detail. Please take into account the following references to help with the interpretation.

Discussion has been extended taking into account the references suggested by the reviewer.

P17-L21. Please change "..from Mie calculations from..." by "..from Mie calculations for.."

Done.

P17-L23:P18-L4. I think that a more detailed analysis is needed to establish why you observe these differences between GAME and aircraft measurements. On 16 June, the differences may be explained "by the assumed profiles of gases such as CO2, O3 or water vapor, or the uncertainty in the LST", as the authors assert, or may be not. It is just a too simple qualitative explanation.

For the LW spectral range, the differences are mostly lower than 10 $W \cdot m^{-2}$, which can be considered within the uncertainty limits. Considering the uncertainty of the pyrgeometer, which is 5 $W \cdot m^{-2}$ 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.

P18-L7. Please change "the diffuse radiation..." by " the longwave radiation.."

Done.

P18-L9. The differences observed are within the uncertainties of the pirgeometers, that for a well mantained and calibrated instrument are below 5 Wmˆ-2 (Meloni et al., 2012). Therefore, the authors does not need qualitative explanations based on variables as LST to justify the differences.
Removed.

P18-L11. As in the SW case, I think it is not worth including the comparison with CERES due to the large distance between the ground-based and satellite measurements.

Removed.

P18-L28. Please be consitent, rc or reffc?
We have updated all the symbols, using $r_{eff,c}$ and $r_{eff,f}$ for clarity.

Tables 6, 7, 8: The standard deviation is an statistical parameter then it have not sense if obtained over three values only.

It is included just as an indicator of the variability of the ARF values even though the number of simulations is low, as pointed out by the reviewer.

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