

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

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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

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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 negligible?. 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-

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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.

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 analyze the role of aerosol layering in your retrieval?

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 r_{eff} and nevertheless, in table 4 the radii appear in the 2 modes (fine and coarse). Please be clear and consistent.

3. The results should be analyzed 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.

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 ra-

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diative fluxes and 0.05 for AOD), why do the authors obtain such large differences in the ARE_{sw} (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.

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)

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

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

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and a negligible variation from the top of the aerosol layer to the TOA. These results should be better discussed and justified.

Minor comments:

P2-L3: Please change "contrasted" by "compared" P2-L21: Please rephrase the sentence, its meaning is no clear. P4-L5: Please change "..model estimates sensitivity..." by "..sensitivity of the model estimates.." P5-L2: Please change "..real and imaginary refractive indices..." by "..real and imaginary parts of the refractive index.." P5-L24: Plase remove "particle" P5-L29: Please change "..spatial integral..." by "..vertical integration.." P6-L6: Please change "in" by "by" P9-L27: How the surface temperature was estimated from the measurements at 2m above the ground? please provide a reference. 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 extinction). These differences may be also due to the AOD content from surface to the minimum altitude of the aircraft, or for the observation of different air masses (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. 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. P13-L11: Are you sure about this sentence? Is there any typo error? wavelengths below 3 μm are not considered LW range. What about for wavelengths over 16 μm ? P13-L14: What radius did you use in Mie calculations? the effective radius, or the r_F and r_C ? Table 4, Table 4 caption and the text result confusing. Please be clear. P14-L11. Please change "visible" by "shortwave" P14-L12. Please change "thermal" by "longwave" P14-L27. When the authors talk about "discrepancies", are they referring to "relative differences" (Fgame

- Faircraft)/Faircraft? The authors should define how they obtain these discrepancies, and always use the same term. Sometimes they use "discrepancies" and sometimes "differences". 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. 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. P15-L17. The authors should take into account that relative differences about 7% for a F_{downward} around 430 and 530 Wm^{-2} yield absolute differences of 30 and 37 Wm^{-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 insignificant as the authors assert. Have you evaluated them? P15-L27. Again, a 60 Wm^{-2} differences between datasets may contribute to large uncertainties in the determination of ARE using this data. Please evaluate this. 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. 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 previously 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. P17-L21. Please change "..from Mie calculations from..." by "..from Mie calculations for.." P17-L23:P18-L4. I think that a more detailed analysis is needed to establish why you observe these differences between GAME and aircraft

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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. P18-L7. Please change "the diffuse radiation..." by " the longwave radiation.." P18-L9. The differences observed are within the uncertainties of the pyrgeometers, that for a well maintained and calibrated instrument are below 5 Wm⁻² (Meloni et al., 2012). Therefore, the authors does not need qualitative explanations based on variables as LST to justify the differences. 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. P18-L28. Please be consistent, rc or reffc?

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

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