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# *Interactive comment on* "Estimation of mineral dust longwave radiative forcing: sensitivity study to particle properties and application to real cases over Barcelona" *by* M. Sicard et al.

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Answer to RC C2116

**The changes in the revised manuscript (posted soon) are indicated in bolt font.**

The paper by Sicard et al. addresses an interesting and relatively poorly investigated aspect of mineral dust radiative effects in the Mediterranean atmosphere. The paper is interesting, the data and the methodology are appropriate for the objectives

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of the study. However, some aspects need to be clarified, and the conclusions need more discussion before publication of the study. The main aspects to be improved are: 1. The study is based on the assuption that (something similar to) pure dust is present at Barcelona. Although the cases are selected according to the observed Angstrom exponent and its spectral dependence, it is possible, and in some cases there are evidences, that dust is mixed with different aerosol types (or, at least we may have different types of particles at different atitudes). As suggested by the authors, case 7 is clearly attributable to mixing of dust and smoke. In general, the authors assume that the dust is confined approximately between 1.5 and 3.5 km altitude. Thus, other aerosol types (marine? urban?) must be present in the boundary layer, although dust may still be dominant over the column. The use of refractive indices for dust in a urban environment should be better discussed, since the occurrence of particle layers with different optical properties is expected to affect the radiative budget (see e.g., Gomez Amo et al., 2010).

**REPLY**: By assuming that dust is confined between 1.5 and 3.5 km, we exclude the PBL which in Barcelona does not extend higher than 1.5 km (see Sicard et a., "Mixed-layer depth determination in the Barcelona coastal area from regular lidar measurements: methods, results and limitations", Boundary-Layer Meteorol., 119, 135 – 157, 2006). This implies that local, urban and marine aerosos are discarded (to be mixed with MD higher than 1.5 km). However nothing prevents the dust to be mixed with other aerosol of long-range transport, and in some cases this happens. This point has been discussed at the end of Section 2.1 where we also emphasize the assumption that the study is based on the assumption of pure dust. The work of Gomez-Amo et al. (2010) is cited as reference.

2. The forcing at the surface is generally calculated using the net irradiance instead of the downward component only. By using equation (1) (page 8541), what is derived at the surface is different with respect to what is generally used, and the comparisons with literature data should take into account this aspect. In the LW, the

surface forcing for the downward irradiance and the net irradiance are equal only if the surface temperature is the same with and without aerosols. This occurs only if we assume that the aerosol is not affecting the surface temperature. If this assumption is made, it should be discussed and verified. Most importantly, if the upward component at the surface is neglected, expression (4) for the atmospheric forcing is not valid, since some terms are missing. Figure 9 and the related discussions should be corrected.

**REPLY**: This comment is similar to the first point of Referee 4. The equations 1 and 2 in the manuscript are wrong, or not completely self-explicative. The forcing at BOA and TOA are the difference between the net fluxes with and without aerosol, and each net flux is the difference between the downward and the upward fluxes (same as in Gomez-Amo et al., 2010). This has been clearly modifies in Eq. 1 and 2 in the revised manuscript.

3. Figure 8 shows the scatterplot of CERES outgoing longwave irradiances and modeled data. Both datasets cover a limited interval of values of about 12-15 W/m2, and the evaluation of the results needs reporting estimated uncertainties.

**REPLY**: There are some uncertainties on CERES SSF products. Errorbars have been added to the CERES OLR in Figure 8 and the following text has been added in Section 5: "The uncertainty on CERES OLR has been calculated as 2.9 % of the OLR value. This uncertainty corresponds to the CERES instantaneous LW TOA flux uncertainty for Terra Angular Distribution Models (ADMs), in the mid-latitude region and for clear-sky available at https://eosweb.larc.nasa.gov/sites/default/files/project/ceres." The discussion of Figure 8 has been extended including the uncertainties on CERES products.

4. The discussion on the sensitivity based on the model is interesting; however, it is of limited use in the interpretation of the results. If possible, it would be useful to use the derived sensitivities to assess the uncertainties associated with the model calculations.

**REPLY**: This comment is identical to point 16 of Referee 1 (S. Otto). This exercise is

difficult since the sensitivity analysis and the real cases are not directly comparable. However we have tried to link "basic" relationships of the sensitivity analysis (e.g. high vertical distribution produce high LW RF at TOA, etc.) to the real cases. This relationship real cases – sensitivity analysis has been added in the revised manuscript at the end of each paragraph about the BOA and TOA forcings.

5. The setup used in the calculations in the SW spectral range is poorly discussed. The refractive index from Volz (1983) is used throughout the SW and LW ranges. The refractive index used in the SW should be at least compared with the values coming out for the different cases from the AERONET retrievals. This camparison may also help verifying if dust is really the dominant component in the atmospheric column (see point 1). If the refractive index is kept fixed, changes in some optical parameters, and in the single scattering albedo in particular, are due only to changes in size distribution. What are the SW single scattering albedo for the different cases used in the calculations (i.e., with the AERONET size distribution and the Volz refractive index)? And how do they compare with the AERONET retrievals of single scattering albedo? If the SW RF calculations are maintained in the paper, more information should be added to table 3. This additional information should include the single scattering albedo, the asymmetry factor, possibly some data on the size distribution, and the solar zenith angle.

**REPLY**: The setup of the model in the SW spectral range is given as references to Roger et al. (2006), Mallet et al. (2008) and Sicard et al. (2012) at the beginning of Section 3. Since the RF in the SW spectral range is not the main goal of the paper and since the model in that range has already been used in published works, we think that those references may be enough. In the SW spectral range, no refractive index is assumed and no Mie computation is made. The AOD, the single scattering albedo and the asymmetry factor are all taken from multi-wavelength sun-photometer measurements (AERONET). The scattering albedo and the asymmetry factor at one wavelength have been added in the table in the Supplement and discussed at the

beginning of Section 5.

6. The SW RF and the ratio LW versus SW radiative forcing are of very limited utility without an information on the solar zenith angle, since the SW RF varies between zero and the noon value during the day. The comparison with literature data should be made at the same solar zenith angle, or for daily averages (whose values depends on latitude and day number, which should be stated), and considering differences arinsing from different albedoes.

**REPLY**: This comment is identical to point 14 of Referee 1 (S. Otto). A Table containing properties of interest in the shortwave (solar zenith angle, single scattering albedo, asymmetry factor) for the 11 cases is added in the Supplement and discussed at the beginning of Section 5. A discussion is provided at the beginning of Section 5.

7. The conclusions should be strengthened. I would suggest discussing the behavior of the forcing efficiency (RF divided by AOT) in order to compare results obtained on different days, and to investigate if and how the LW forcing efficiency depends on size distribution and other key parameters (Angstrom exponent, single scattering albedo in the IR window, etc.). Few studies on the dust LW radiative forcing have been carried out so far in the Mediterranean (e.g., di Sarra et al., 2011; Perrone and Bergamo, 2011; Spirou et al., 2013). A comparison with those studies would help in the discussion of the results.

**REPLY**: Thanks a lot for those 2 references that are indeed precious sources of information regarding estimation of longwave radiative forcing in the Mediterranean. They have been included in the discussion in Section 5. Regarding the forcing efficiency, the main objective of the second part of the paper (Section 5) is to quantify the LW/SW ratio in terms of radiative forcing for different mineral dust scenarios. To assess that goal the forcing efficiency is not necessary. In the comparisons with previous works in Section 5, we have indicated when the conditions were quite different (measurement position, time of the day, aerosol load, . . .). For those reasons we have decided to

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Discussion Paper

leave the forcing efficiency apart of the paper. If the referee strongly disagrees with that decision, please tell us and we will include the forcing efficiency in the paper for ACP.

Other minor points follow: I would suggest defining somewhere in the text the limits of the SW and LW spectral ranges used in the study.

**REPLY**: SW: $0.2 - 4 \ \mu$m and LW: $4 - 50 \ \mu$m. Those limits have been defined in the revised manuscript at the beginning of Section 3.

Page 8535, lines 19-21: wet scavenging may or may not occur, and the relatively small particles may still be present in the dust size distribution.

**REPLY**: This is absolutely true. This comment is partly answer in the answer to point 2 of Refere 1 (S. Otto). Please see that answer! In summary and directly connected to this comment, we have removed from the text (in the revised manuscript) the part relative to the very small particles which is not relevant for mineral dust particles.

Page 8535, lines 25-26: the sea salt altitude contributes to reduce the top of the atmosphere forcing, but does not prevent to have an effect (see e.g., Markowicz et al., 2003).

**REPLY**: Thanks a lot for that comment. The sentence saying the opposite has been deleted in the revised manuscript, and the paper of Markowicz et al. (2003) has been referenced.

Page 8536, line 25: as suggested by other reviwers, non-sphericity may still play some role, although small.

**REPLY**: This point is equivalent to comment 3 of Refere 1 (S. Otto). This part has been completely re-written in the revised manuscript according to those comments. In order to avoid any inconsistency with older works referenced in the initial paper, the reference to the work by Yang. et al. (2007) has been deleted.

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Discussion Paper

Page 8537, lines 4-5: the sentence is not clear. In my opinion the significant changes in the optical coefficients are not due to the "small spectral variations" in the refractive index.

**REPLY**: This point is similar to comment 4 of Refere 1 (S. Otto). It is true that the significant changes in the optical properties in the LW are due to rather large (compared to the SW) spectral variations in the refractive index (see Figure 1 of the manuscript). The beginning of Section 2.1 has been rewritten in the revised manuscript to leave that idea clear.

Page 8540, line 1: I would not expect a close correspondence between PM10 and dust cases detected from the AOT spectral dependency. As the authors state, dust is generally present above the boundary layer, and its occurrence probably is not directly linked to transport in the boundary layer. This is often the case over the Mediterranean (see e.g., Marconi et al., 2013).

**REPLY**: Thanks a lot also for that comment. This part of Section 2.3 has been re-written in the revised manuscript and the reference of Marconi et al. (2014) has been added to justify the differences between ground and columnar dust presence.

Page 8542, lines 19-21: if I understand well, all optical properties, except the extinction optical thickness, are the same in the different vertical layers.

**REPLY**: Yes this is correct. The values of SSA and g calculated for the MD model are attributed to each atmospheric layer in which dust is present (i.e. between 1.5 and 3.5 km in the sensitivity analysis). The columnar AOT is distributed homogenously into the layers between 1.5 and 3.5 km. This has been clarified in the revised manuscript.

Page 8543, lines 4-21: the analysis for different atmospheric profiles does not seem particularly useful, since radiosonde data at the same place are available. I would remove this section.

**REPLY**: This section, mostly based on former studies, shows that the use of a mid-

latitude summer model in the computation of LW RF can give results quite different than using measured atmospheric profiles. This result seems important to us and we think that maintaining this section is useful. If the referee strongly disagrees with that decision, please tell us and we will remove this section in the manuscript for ACP.

Page 8544, lines 1 and 6: surface emissivity and temperature are not among the CERES products. They are very likely derived from MODIS on the same platform. **REPLY**: We greatly thank the referee for this important comment that made us revise thoroughly the origin of the surface emissivity and temperature available in CERES SSF Level2 product files. The surface temperature comes from auxiliary data, and more precisely from the Global Modeling and Assimilation Office (GMAO)'s Goddard Earth Observing System (GEOS). The origin of the surface emissivity is hardly explained in the CERES NASA webpages. It comes from the CERES/SARB (Surface and Atmospheric Radiation Budget) surface properties. More information is available at http://www-surf.larc.nasa.gov/surf/pages/explan.html. It says "... Imager data from the same satellite (TRMM - VIRS, TERRA AQUA - MODIS) are collocated inside the CERES footprint and on the CERES scene type map. This determines the percentage of each scene type within the CERES footprint. The imager data is convolved with the CERES point spread function providing an energy weighting function for each scene type. A table lookup determines spectral albedo (emissivity) for each scene type which are then weighted by the scene type percentages from the imager and integrated giving a spectral albedo (emissivity) curve for the entire footprint. If the footprint is clear, a TOA to surface parameterization is used to determine broadband albedo and this is used to adjust the spectral curve up or down such that the spectral integral of the albedo is equal to the observed broadband albedo. ..." Section 3.2.2 has been deeply re-written to clear up the origin of the surface emissivity and temperature available in the CERES SSF Level2 product files that were used in this work.

Page 8544, line 24-25: the sentence "This is due to the fact ... close to the surface" is awkward.

**REPLY**: This sentence has been partly re-written in the revised manuscript. We now refer to the lowermost aerosol layer and not the surface.

Page 8545, lines 5-15: how is changed the AOT? Is the particles number varied? The temperature of the emitting particles is largely relevant for the determination of the forcing. The radiance outgoing from the dust layer depends on its temperature, and a much larger emission occurs when the dust is at low altitudes because its temperature is larger. This affects both the forcing at BOA and TOA, and is in my opinion much more important than the effect of radiation "reflection".

**REPLY**: The AOT at 500 nm (distributed by levels) is an input parameter in the model. Another input parameter is the spectrally-resolved extinction coefficient in the LW normalized to that at 500 nm (see end of Section 2.3) previously calculated with a Mie code. The combination of both allows the calculation of the spectrally-resolved extinction coefficient in the LW for any value of the AOT at 500 nm. In Figure 7a, the AOT at 500 nm was changed from 0 to 1 by steps of 0.2.

About the second question, this comment coincides exactly with one comment of Referee 3 to who we answered "This is totally true. The temperature effect is visible on the forcing at the surface. The revised manuscript has been revised accordingly. However the scattering effect is still mentioned as the explanation of the behavior of the forcing at the TOA (opposite to that at the surface).".

Page 8545, lines 28-29: the Mie theory incorporates the Rayleigh theory, and for very small particles the same results should come out.
**REPLY**: This is totally true. This sentence has been deleted in the revised manuscript.

Page 8546, lines 1-2: at which wavelength applies this statement?
**REPLY**: This applies to the LW spectral range. It has been specified in the revised manuscript.

Page 8546, lines 10-11: for this case the AOT is not constant. It may help the reader adding this information.

**REPLY**: It has been specified at the beginning of the paragraph about Figure 7d in the revised manuscript.

Page 8548, lines 24-25: the selection of the CERES pixel may have a large impact on the SW albedo. Which is the used albedo in the SW? Do the auhors expect any change in the aerosol vertical distribution when they move out of the city?

**REPLY**: Similarly to the answer of point 5, the surface albedo in the SW spectral range is taken from multi-wavelength sun-photometer measurements (AERONET), not from CERES.

Page 8549, line 7-8: please, add solar zenith angles and other size ditribution information (see point 6 in the first section of the review) in table 3.

**REPLY**: A new table has been added in the supplementary material that will come attached to the paper. The information on the sized distribution has not been added because the size distribution is not used in the parametrization of the model in the SW range. Please see answer to point 5.

Page 8549, line 14: the RF by Meloni et al. (2003) refers to the visible range, and is obtained over the sea. There are many other studies dealing with the dust SW RF over coastal areas or throughout the basin (e.g., Derimian et al., 2006; Gomez Amo et al., 2011; Horvath et al., 2002; Papadimas et al., 2012; Perrone and Bergamo, 2011; Roger et al., 2006; Saha et al., 2008; Spirou et al., 2013, and others) which may be used in the comparison. In any case, the values reported in table 3 are instantaneous, and depend strongly on the solar zenith angle; it may be useful to compare forcing efficiencies instead of RFs.

**REPLY**: The solar zenith angles for each case have been added in a new Table in the Supplements. The discussion in Section 8 has been extended including most of the

references suggested by the referee.

Page 8549, lines 20-21: the comparison between SW and LW RF is not significant without information on at least the solar zenith angle.
**REPLY**: The solar zenith angle has been added in a new table in the Supplement.

Page 8550, line 14: as discussed in the introduction of this review, this equation is not valid if the RF at the surface is calculated using only the downward component.
**REPLY**: Please see the answer to point 2! The definition of the forcing at the surface has been corrected in the revised manuscript.

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Interactive comment on Atmos. Chem. Phys. Discuss., 14, 8533, 2014.

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