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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.

Anonymous Referee #5

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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 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

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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).

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.

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.

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.

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.

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.

7. The conclusions should be strengthened. I would suggest discussing the behaviour 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.

Other minor points follow:

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I would suggest defining somewhere in the text the limits of the SW and LW spectral ranges used in the study.

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.

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).

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

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.

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).

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.

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.

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.

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

awkward.

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".

Page 8545, lines 28-29: the Mie theory incorporates the Rayleigh theory, and for very small particles the same results should come out.

Page 8546, lines 1-2: at which wavelength applies this statement?

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

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?

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.

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.

Page 8549, lines 20-21: the comparison between SW and LW RF is not significant

without information on at least the solar zenith angle.

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.

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