Answer to EC C1390

I suggest some specifications and additional comments in the manuscript:
Section 2.2 and Fig. 3: Isn't it assumed that mineral dust aerosol size distributions are made of lognormal modes? If this is the case you might specify it and also that sigma is then a "geometric" standard deviation.
REPLY: Yes, the mineral dust aerosol size distributions are assumed to be lognormal. It is explicitly said in the Appendix (p. 8553, l. 9). It has been added to the revised manuscript at the beginning of Section 2.2. We call σ_V the standard deviation associated to the volume median radius. In the Appendix (p. 8553, ec. 3) it is explicitly said that σ_V is equivalent to the geometric standard deviation, σ_g.

Section 4, p.8546 and Fig. 7d: you probably assume that the (geometric ?) standard deviation of the size distribution is kept contant and might specify it.
REPLY: Yes, the standard deviation is maintained constant. It has been specified in the revised manuscript.

Section 5, top of p.8548: the cases summarized in this table would deserve some comments when the Table 3 is cited.
REPLY: The table has been commented in the revised manuscript.

Section 5, middle of p. 8550: you might comment why those particular cases result in such a low forcing.
REPLY: Some comments have been added in the revised manuscript. The main reason is the reduced incoming solar radiation due to large solar zenith angles.

End of section Section 5: you might comment why those two cases are specific.
REPLY: Cases 7 and 11 are not specific by themselves. They have been selected as they produce the highest and lowest radiative forcing (in absolute value) at the BOA, respectively, in order to somehow represent an envelope in which the profiles of the heating/cooling rates of all cases vary.

Answer to RC C1434

The changes in the revised manuscript (posted soon) will be indicated in bolt font.

1) p 8535, l 6-11: I think the opposite is the case: The most crucial point for aerosol radiative transfer calculations is the complex refractive index. In particular, for dust such data are mainly given in the thermal spectral range as Otto et al. (2009, 2011) explicitly point out. These authors stressed the need for more detailed data "in the solar spectral range and for further minerals". Thus, in the thermal range there is a series of more recent works on dust microphysical properties and radiative effects (Otto et al., 2007, 2011; Hansell et al., 2010; Haywood et al., 2011; Köhler et al., 2011; Osborne et al., 2011; and papers cited therein).
REPLY: We, the authors, thank very much the referee for the extensive list of papers dealing with our topic. All of them are now cited properly in the revised manuscript. This part of the introduction has been revised. In our original writing, by "the difficulties to parameterize accurately the model", we meant that 1D longwave forcing estimations require a lot of parameters from different sources of instruments which are usually not acquired in a single place on a routine basis but in dedicated field campaigns. By "the lack of knowledge of the aerosol properties in the LW range", we meant that, because aerosol radiative properties are not directly measureable in the longwave spectral range, they need to be computed (=approximated) by dedicated codes (Mie code here) starting from the microphysics.

2) p 8535, l 20-22: Terms like "small" and "very" are quite relative. What size do you mean exactly? I suggest to discuss in more detail the results of the field experiments like SAMUM-2 and FENNEC (see, e.g., Weinzierl et al., 2011 and Ryder et al., 2013a,b) here, e.g., how large the particles can be. The size of the particles transported over long distances is very important. In this regard it should also be mentioned up to which particle size the applied size distributions are integrated to calculate the optical properties. This could also be discussed in Section 2.3 in retrospect to the Introduction and to the role of "large" dust particles. For example, in the papers of Otto et al. "large" means particle diameters larger than about 3 microns.
REPLY: We have changed the text accordingly to that comment in the introduction and discussed our results against the references suggested here in Section 2.3. We have removed from the text the part relative to the very small particles which is not relevant for mineral dust particles. However we would like to stress that the references suggested here deal with the observation of mineral dust in layers close to the source (over the Sahara desert during FENNEC and over Cape Verde Islands during SAMUM-2) while our measurements are in Barcelona, a few thousands kilometers from the source. In that sense we totally agree with the reviewer that the coarse mode and the "very large" particles are relative to the place of the measurements. Numbers have been given in the revised manuscript.
The size distribution is integrated between radii of 0.001 and 25 µm. This information has been added in Section 2.3 of the revised manuscript.

3) p 8536, l 25-26: Otto et al. (2011) state that the non-sphericity can have an radiative impact of about 10 % to the forcing in the thermal range. It may be significant. But to deal with non-spherical particles means big computational effort (see, e.g., Otto et al., 2009). So it is still reasonable to consider spherical particles, if no further information with respect to the shape are available.

REPLY: 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.

4) p 8537, l 6: What does "good" mean?
REPLY: The last sentence in p 8537, l 6 has been replaced in the revised manuscript by "This implies that the variations of the refractive index with wavelength have to be known.".

5) Section 2.2: The authors use AERONET remote sensing products. This is, of course, the only they can do, if no other microphysical particle information are available. However, I would like to stress that size distribution retrievals of mineral dust are problematic: In a row of papers it is reported that the AERONET size distributions might misinterpret the lower number of "large" dust particles as a higher number of accumulation mode particles (Otto et al., 2007, 2009; Müller et al., 2010a,b, 2012). In summary, the AERONET distributions might underestimate the presence of giant particles and, by the way, this also corresponds to cut-off effects (Otto et al., 2011), which both may lead to misinterpretations. One has to keep in mind this when using retrieval products of mineral dust, in particular with regard to the importance of coarse mode particles.
REPLY: We totally agree with the referee and are aware of the AERONET limitation. However let us say that our work deals with mineral dust after long-range transport whereas all the references suggested in the comment are from measurements close to the Saharan desert (in Morocco or in the Canary Islands). According to Maring et al. (2003), Ryder et al. (2013) and Osada et al. (2014) cited in the introduction, it is very unlikely that very large particles (with a diameter > 10 µm) remain in the atmosphere in Barcelona. We have said so in the revised manuscript and we have also mentioned the problems of measuring size distribution of large particles. The references suggested have also been added.

p 8539, l 17-20: Please specify in detail the used distribution parameters so that the reader is able to re-calculate your results. A table would be nice.
REPLY: All parameters are given in Table 1. It has been said in the revised manuscript. The conversion formulae of AERONET volumetric products to the median parameters necessary as inputs in our Mie code are given in the Appendix.

6) p 8540, l 23: 40 layers are not much but ok. Is a constant vertical resolution chosen or does it change with altitude?
REPLY: The vertical resolution for the atmospheric profile is not constant. The atmosphere is vertically discretized into 40 layers with a resolution of 1 km from the surface to 25 km, 2.5 km between 25 to 50 km, 5 at 55 and 60 km and 20 km at 80 and 100 km. This information has been added in the revised manuscript.

7) p 8542, l 3: Would it be possible to present the coefficients a_{i} and k_{i} as a function of p and T as supplement? Then the reader would be able to re-consider your transmission parameterisation.
REPLY: Unfortunately, it is not possible because there are about 100000 coefficients for the k-distribution (a_{i}, k_{i} and coefficients for pressure and temperature dependences). Consequently, it is necessary to contact the authors to get these coefficients.

8) Section 3.1: The gas absorption is parameterised by the k-distribution method which refers to "bigger" spectral bands. How did you calculate (numerically integrate) the spectrally averaged optical properties of the dust aerosol?
REPLY: Optical properties of aerosols are considered as constant in each spectral interval. They are accurately precalculated using Mie theory at the mid-interval value of each considered spectral interval.

9) p 8542, l 22: 20 cm-1 is not "high" in my opinion but ok. For instance, Otto et al. (2011) use a 1 cm-1 resolution for their forcing calculations.
REPLY: A spectral resolution of 20 cm-1 is high in comparison to large broadband radiative transfer codes (CGM), but it is indeed not so high if compared to high spectral resolution codes. We now use "moderate" in the revised version instead of "high". This resolution is a reasonable compromise between speed of calculation and accuracy. Especially, this spectral resolution allows to accurately account for spectral variations of aerosol optical properties.

10) Section 3.2.2: For which area are the applied data representative? This point should be discussed more critically and can also be seen in connection with the question to which scenario the cases refer, a rather ocean or land case? The value of 0.017 of the surface albedo is very low which is quite typical for an ocean surface (see, e.g., Fig. 3 in Otto et al., 2011).
REPLY: By integrating the surface albedo of Fig. 3 of Otto et al. (2011) for land surfaces between 8 and 12 µm, one gets roughly 0.045, which is indeed higher than 0.017 and is representative of a mixture of ocean and land. We have revised the retrieval of the surface albedo and extended the discussion in Section 3.2.2 (as well as in Section 5) in the revised manuscript.

11) Section 3.2.2: Does CERES really "measure" the surface albedo or temperature? I think it would be better to write that these quantities are "derived"?
REPLY: It has been corrected in the revised manuscript.

12) Section 3.2.3: It would be interesting for the reader to get an impression of the vertical structure of the observed dust plumes. Would it be possible to add a figure of all vertical profiles of the number concentration of all cases applied?
REPLY: The profiles of the extinction coefficient showing the vertical distribution of the mineral dust for the 11 cases are added as supplementary material in the revised material and discussed at the beginning of Section 5.

13) Section 4: Most of the results presented here are not knew. That's why they should be discussed in the context of former works (see, e.g., the reference list of this review). The various investigated cases should also be motivated more clearly why they could be of interest. In particular, the role of coarse mode dust particles was recently stressed by the authors of Otto et al. as well as McConnell/Ryder et al. However, former works of d'Almeida, Tegen and Sokolik et al. (cited therein) also showed their impact on the optical properties and forcings.
REPLY: Initially the goal of our paper is not to focus on the effect of large particles on the radiative forcing. This point is included in our study (Fig. 7c, 7d and 7e) but it belongs to a more general sensitivity study. However because of the strong dependency of the longwave radiative forcing to the size of large particles, the context and the references exposed by the referee have been added at the beginning of Section 4 in the revised manuscript.
The results of Fig. 7c have been compared to the results from Otto et al. (2011).

14) Section 5: Against the background that satellite products refer to relatively large surface areas, how representative are they and for which scenario (see also point 10 of this review) do they stand? The title of this paper is "... over Barcelona" which refers to a land surface. This could be misleading, since a rather mixed area of land and ocean was the case. To avoid confusions, the title could be changed a little accordingly?
REPLY: Please see answer to point 10. The title has been changed to "… in the region of Barcelona".

15) Section 5: In this section also SW calculations appear in the discussion. But in the previous sections only the thermal spectral region was of interest and in the title it is said of "longwave radiative forcing". Either the title is chosen in a more general way, but then the refractive indices, optical properties and so on must be discussed also and in more detail in this spectral range in the Introduction and Sections 2 as well as 3 which means an extension of the paper, or this spectral part is not discussed. The SW consideration seems to be only additional at the moment. If it is considered, it is definitely of interest what values of, e.g., the single scattering albedo was applied, since the coarse mode dust particles affect this quantity and thus the radiation budget extremely (in this regard keep in mind point 5 of this review).
REPLY: Our paper is about longwave radiative forcing. During the writing of Section 5 we thought that including the SW component would allow us to estimate the ratio LW/SW and therefore quantify the importance of the LW forcing contribution in cases of dust outbreaks in Barcelona. The idea we have in mind is to draw the attention of the regional and global climate model community that the LW component is not always negligible. Also, two referees commented that removing the SW part would be a shame for the paper. So we have decided to keep the sensitivity analysis (Section 4) only in the longwave and to state clearly in the introduction that SW calculations are made to quantify the importance of the LW contribution (only in Section 5). 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.

16) p 8549, l 7-24: Based on the sensitivity studies in Section 4 it would be of interest what basic properties might lead to this or that forcing. In other words, the results here should be interpreted also in retrospect to the findings of Section 4.
REPLY: 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.

17) p 8550, l 4-11: This statement assumes that the retrieval procedures result in physically correct and realistic optical properties. With regard to point 5 of this review it might also be the case that retrievals underestimate the coarse mode and hence the SW absorption by a too high value of the single scattering albedo. This could then lead to misinterpretations as mixtures come into play. One has to be careful here.
REPLY: In the revised manuscript we have included in the Supplement some parameters of interest for the SW forcing calculation, including the single scattering albedo. We also now compare our values to the literature, including Otto et al. (2007), and find equivalent results. The most critical point with case 7 is probably the mixture as pointed out by the referee. We believe that the underestimation of the coarse mode in AERONET product which has been demonstrated

over the Saharan desert and close to it does not come into play for long-range transport dust in Barcelona. Please see also answer to point 5 and Section 2.2 in the revised manuscript.

18) Conclusions: As in point 5 the SW properties are also discussed here although the paper is actually about the thermal part only. Why this?
REPLY: Please see answer to point 15.

19) Last paragraph of the Conclusions: The spatial variability of the dust plumes is stressed here. That's why point 12 of this review seems to be highly relevant to me to point out how variable the observed plumes really are.
REPLY: Please see answer to point 12.

20) The terms "shortwave" and "longwave" are relative. It is better to refer to the sources to indicate the spectral regions. "shortwave" –> "solar" and "longwave" –> "thermal" or "terrestrial"? In general, this paper is based on a variety of measurements at various observed dust events in order to calculate radiative effects. Its title contains the word 'longwave' but, with respect to the results, it is also about effects in the solar spectral range, while the microphysical and optical dust properties are not discussed in this spectral range. Thus, I suggest to restrict the paper only to the thermal region of the spectrum or to extend it in all parts of it by discussions of solar properties. In both cases, however, I recommend it to be published in ACP and hope that my comments might help the authors to improve it here and there.
REPLY: About the vocabulary, we agree with the referee that solar/thermal might be more appropriate than shortwave/longwave. But from a practical point of view, in the present state of the paper there are 121 "longwave" words (in the form LW) and 58 "shortwave" (in the form SW). So, in order to keep the length of the paper reasonable (the revised manuscript has currently 58 pages + 2 supplements against 52 pages for the initial ACPD manuscript), we think that maintaining the SW/LW spelling is useful. If the referee strongly disagrees with that decision, please tell us and we will make the changes (shortwave->solar and longwave->thermal) in the manuscript for ACP. For the rest of the comment, please see answer to point 15.
Your comments surely helped in improving our paper. Thank you.

References:

Hansell et al., J. Atmos. Sci., 67, 1048-1065, 2010

Haywood et al., Q. J. R. Meteorol. Soc., 137, 1211-1226, 2011

Köhler et al., Tellus, 63B, 751-769, 2011

McConnell et al., J. Geophys. Research, 113, D14S05, 2008

McConnell et al., Atmos. Chem. Phys., 10, 3081-3098, 2010

Müller et al., J. Geophys. Research, 115, D07202, 2010a

Müller et al., J. Geophys. Research, 115, D11207, 2010b

Müller et al., J. Geophys. Research, 117, D072112012, 2012

Osborne et al., Q. J. R. Meteorol. Soc., 137, 1149-1167, 2011

Otto et al., Atmos. Chem. Phys., 7, 4887-4903, 2007

Otto et al., Tellus, 61B, 270-296, 2009

Otto et al., Atmos. Chem. Phys., 11, 4469-4490, 2011

Ryder et al., Atmos. Chem. Phys., 13, 303-325, 2013a

Ryder et al., Geophys. Res. Lett., 40, 2433-2438, 2013b

Weinzierl et al., Tellus, 63B, 589-618, 2011

Here you will find some elements of response to the last comments of S. Otto (Referee #1).

RESPONSE: I meant that it might be relative what the words "small" or "large" or "very" could mean without to clarify them in more detail, e.g., by numbers. Moreover, I am aware that Barcelona is located far away from the source. I only would like to stress the importance of the coarse mode particles. I am not a field scientist but know how difficult measurements of coarse particles might be, performed close to the source or far away from it. For this reason it would actually be of importance to measure dust particles directly rather than to trust on retrievals only. This is, of course, not your turn. But on the other hand I could ask you about the literature about in-situ dust particle measurements over the Barcelona site which really covered the coarse mode, against the background of the measurement problems. So, can you exactly state what particle sizes were really present? ... Of course, I also believe that far away the "largest" particles are removed from the dust layer. But only more or less direct measurements can illuminate questions like this. ... OK

REPLY: Sorry to put the numbers in the revised manuscript and not state them clearly in my first answers. We define very large particles as particles with a diameter larger than 10 microns and base our discussion about the deposition of very large particles on the results from Maring et al., 2003; Ryder et al., 2013a; Osada et al., 2014.

RESPONSE: You should not remove the citation because it is part of the literature. Yang et al. used, e.g., different size distributions. I am sure that their results are correct within the scope of their basic assumptions. You could state that the contribution of the particle shape may have different effects depending on the particle properties assumed.

REPLY: Yang et al. has been put back in the references in the revised manuscript.

RESPONSE: I understand that. It would be nice if you could encourage the interested reader in the main text whether it is possible to get in contact with the authors with respect to the k-distribution data.

REPLY: We have added the following sentence in the revised manuscript: "The k-distribution coefficients are available upon request to the authors.".

21) Wouldn't it be possible to move the supplemental figure and table to the main paper?

REPLY: The supplementary material has been included in the main paper.

Best regards and thanks a lot!

Michael Sicard

Answer to RC C1624

The changes in the revised manuscript (posted soon) will be indicated in bolt font.

1) I completely agree with the 1st reviewer (Dr. Otto) as he points out in page 8535, lines 6-11: "I think the opposite is the case: The most crucial point for aerosol radiative transfer calculations is the complex refractive index. In particular, for dust such data are mainly given in the thermal spectral range as Otto et al. (2009, 2011) explicitly point out.". Please re-arrange your text accordingly.
REPLY: This part of the manuscript has been changed in the revised manuscript according to this comment and that from S. Otto (referee). Please see also answers to S. Otto (referee).

2) p. 8534 (line 20,22). The term "opposite sign" is obscure in this context. Please re-phrase to provide your idea more clearly.
REPLY: Would the referee prefer “in absolute value”? If yes, the authors will change the text accordingly even though they prefer the original writing “with opposite sign” since it conveys the additional information on the sign of the SW forcing.

3) p. 8535, l.28, replace "those’ by "these".
REPLY: Corrected.

4) p. 8536, l. 2, replace "development" by "data set"; l. 6, replace "properties’ by "physical"; l. 7, replace "aerosol layer" by " geometrical".
REPLY: Corrected.

5) p. 8540, l. 24, replace "the ground and 100 km" by "ground and 100 km height".
REPLY: Corrected.

6) p. 8545, l. 29, replace "Those" by "These".
REPLY: Corrected.

7) p. 8546, l. 20, the "omega zero" is not defined?
REPLY: omega zero is defined p. 8540, l. 11.

8) p. 8551, l. 15, replace "AOT" by "in terms of AOT"
REPLY: Corrected.

9) P. 8552, L. 21, REPLACE " AND" BY "WHILE"
REPLY: Corrected.

GENERAL COMMENTS:
Please correct your references according to ACP format (e.g the Journal names should be given in abbreviation, p. 8556, l. 8-9, "J. Clim. Meteor."
REPLY: Corrected.

Provide citations from SAMUM I, II campaigns regarding radiative forcing of dust particles.
REPLY: Citations from SAMUM I and II campaigns have been added. Please see also answers to S. Otto (referee) and to Referee #3.

Answer to RC C1751

The changes in the revised manuscript (posted soon) will be indicated in bolt font.

This paper deals with SW and LW radiative calculations applied over Barcelona. The paper is very interesting and well written. The sensivity study (Fig 7 and discussion) and comparison with CERES (Fig 8 and discussion) are especially impressive.
My main suggestion for improvement is a more comprehensive review and comparison with existing literature. Some references are given but many of them are not discussed.
In addition to the references given by another reviewer, I would recommend that references (and discussion where possible) are included of the following papers (AND references therein):
REPLY: We, the authors, thank very much the referee for the long list of papers. Most of them, but not all of them, have been referenced in the revised manuscript. In particular Highwood et al. (2003), di Sarra et al. (2011) and Yu et al. (2006) have been very helpful to extend the discussion in Section 5.

Balkanski, Y.; Schulz, M.; Claquin, T. & Guibert, S. Reevaluation of Mineral aerosol radiative forcings suggests a better agreement with satellite and AERONET data Atmos. Chem. Phys., 2007, 7, 81-95

Highwood, E.; Haywood, J.; Silverstone, M.; Newman, S. M. & Taylor, J. Radiative properties and direct effect of Saharan dust measured by the C-130 aircraft during Saharan Dust Experiment (SHADE): 2. Terrestrial spectrum J. Geophys. Res., 2003, 108, 8578

di Sarra, A.; Biagio, C. D.; Meloni, D.; Monteleone, F.; Pace, G.; Pugnaghi, S. & Sferlazzo, D. Shortwave and longwave radiative effects of the intense Saharan dust event of 25-26 March 2010 at Lampedusa (Mediterranean Sea) J. Geophys. Res., 2011, 116, D23209

Zhang, L.; Li, Q. B.; Gu, Y.; Liou, K. N. & Meland, B. Dust vertical profile impact on global radiative forcing estimation using a coupled chemical-transport-radiative-transfer model Atmos. Chem. Phys., 2013, 13, 7097-7114

Zhao, C.; Liu, X.; Ruby Leung, L. & Hagos, S. Radiative impact of mineral dust on monsoon precipitation variability over West Africa Atmos. Chem. Phys., 2011, 11, 1879-1893

Yu, H.; Kaufman, Y. J.; Chin, M.; Feingold, G.; Remer, L. A.; Anderson, T. L.; Balkanski, Y.; Bellouin, N.; Boucher, O.; Christopher, S.; DeCola, P.; Kahn, R.; Koch, D.; Loeb, N.; Reddy, M. S.; Schulz, M.; Takemura, T. & Zhou, M. A review of measurement-based assessments of the aerosol direct radiative effect and forcing Atmos. Chem. Phys., 2006, 6, 613-666 (especially the references given in section 4.1 of this paper)

With respect to 'rather complete review of MD microphysical and optical properties', I recommend inclusion of (if the authors deem these appropriate - and see also references in these papers):

Ahmed, A.; Ali, A. & Alhaider, M. Measurement of atmospheric particle size distribution during sand/duststorm in Riyadh, Saudi Arabia Atmos. Environ., 1987, 21, 2723 -2725

Gu, Y.; Rose, W. & Bluth, G. Retrieval of mass and sizes of particles in sandstorms using two MODIS IR bands: A case study of April 7, 2001 sandstorm in China Geophys. Res. Lett., 2003, 30, 1805

Reid, J.; Jonsson, H.; Maring, H.; Smirnov, A.; Savoie, D. L.; Cliff, S.; Reid, E.; Livingston, J. M.; Meier, M. M.; Dubovik, O. & Tsay, S.-C. Comparison of size and morphological measurements of coarse mode dust particles from Africa J. Geophys. Res., 2003, 108, 8593

Laskina, O.; Young, M. A.; Kleiber, P. D. & Grassian, V. H. Infrared extinction spectra of mineral dust aerosol: Single components and complex mixtures J. Geophys. Res., 2012, 117, D18210

Chou, C.; Formenti, P.; Maille, M.; Ausset, P.; Helas, G.; Harrison, M. & Osborne, S. Size distribution, shape, and composition of mineral dust aerosols collected during the African Monsoon Multidisciplinary Analysis Special Observation Period 0: Dust and Biomass-Burning Experiment field campaign in Niger, January 2006 J. Geophys. Res., 2008, 113, D00C10

Sokolik, I.; Andronova, A. & Johnson, T. C. Complex refractive index of atmospheric dust aerosols Atmos. Environ., 1993, 27, 2495-2502

Sokolik, I. & Toon, O. Incorporation of mineralogical composition into models of the radiative properties of mineral aerosol from UV to IR wavelenghts J. Geophys. Res., 1999, 104, 9423-9444

Balkanski, Y.; Schulz, M.; Claquin, T. & Guibert, S. Reevaluation of Mineral aerosol radiative forcings suggests a better agreement with satellite and AERONET data Atmos. Chem. Phys., 2007, 7, 81-95

Claquin, T.; Schulz, M.; Balkanski, Y. & Boucher, O. Uncertainties in assessing radiative forcing by mineral dust Tellus B, 1998, 50, 491-505

Minor comments:
Page 8535, line 17: revise (English)
REPLY: We have changed “even though” by “although”.

Page 8535-8536: I suggest to remove the entire discussion of sea salt. I think it is not needed and out of the scope of this paper. This space would be better used to review literature on dust.
REPLY: In our initial manuscript (before the first submission) sea salt was not mentioned. We finally included it in the introduction for completeness of the topic and thinking of the referees. We still think it is worth mentioning sea salt in the introduction and will leave it in the revised manuscript. If the referee strongly disagrees with that decision, please tell us and we will remove it in the manuscript for ACP.

Page 8541, line 7: aerosol cooling effect: cooling of what? surface/atmosphere/Earth?
REPLY: ΔF can be either at the surface (BOA, (Eq. 1)) or at the top of the atmosphere (TOA, (Eq. 2)). With the convention chosen, a negative sign of ΔF (either at BOA or a TOA) will correspond to the same effect of the aerosols: a cooling effect. A positive sign will produce a heating effect. Some precisions have been added in the revised manuscript.

Page 8542, line 17: ’refined compared’ this is unclear, please revise
REPLY: “compared to” has been replaced by “with respect to” in the revised manuscript.

Page 8545, line 7: 'aerosol emission'. This is correct but has not been mentioned before. Please explain.
REPLY: It is true that this sentence was not completely clear. It has been partly re-written in the revised manuscript.

Page 8545, line 13: 'the more radiation will be reflected'. I think this is not true. In my opinion, what is seen here is a temperature effect. The lower the aerosol layer, the higher its temperature, and therefore the higher its emission.
REPLY: 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 8550, line 14: the total atmospheric forcing. What is the physical meaning of this? Can this be measured? Please explain the importance of this quantity in some detail.
REPLY: We suppose the referee refers to ΔF_{ATM}, the atmospheris forcing, i.e. in the atmospheric layers. This quantity is an indicator of the forcing due to the absorption of aerosols. More details can be found in Roger et al. (2006): "In the case of pure scattering aerosol (SSA = 1), ΔF_{ATM} is equal to zero. The presence of aerosols contributes to a loss of energy at the surface level, this lost energy being scattered upward to space. In a case of an absorbing aerosol (SSA < 1), a part of this loss at the surface level is absorbed into the atmospheric layer. The increase in energy in the atmospheric layers leads to a heating of these layers."
Theoretically ΔF_{ATM} can be measured. But as far as we know, this is not a common practice. A way to measure it could be to measure radiation with pyranometers (shortwave) and pyrgeometers (longwave) oriented nadir and zenith in the aerosol layers, e.g. with airborne measurements.

Page 8551: It would be nice if the discussion on heating/cooling rates would be expanded. Can you explain why the peak of the SW heating rate is at such high altitude? (given that most dust occurred below 6 km?) Perhaps it is worth adding averaged dust profiles (if available).
REPLY: The profiles of the extinction coefficient showing the vertical distribution of the mineral dust for the 11 cases are added as supplementary material in the revised material and discussed at the beginning of Section 5.
The discussion on heating/cooling rates has been expanded in the revised manuscript, mostly by comparing to existing literature. The vertical levels in the RTM in the shortwave are discretized at [… 2, 4, 6, 8, … km] as can be seen in Figure 10. The aerosol content in the layer [h_i, h_{i+1}] is attributed to the layer at h_{i+1} (e.g. in terms of AOT, the AOT at 6 km is the one cumulated between 4 and 6 km). And as can be seen in the figure of the vertical profiles of the supplement there are aerosols between 4 and 6 km in both cases 7 and 11 shown in Figure 10.

Figures: Another reviewer made a comment that the paper is also on the SW effect. I think it would be a great shame to remove all SW info from this paper. On the contrary, where possible I would expand the discussion to include SW (e.g. to show in figure 5 and figure 6 also to the SW part of the spectrum.). Perhaps the sensitivity study (Fig 7 and discussion) can be expanded to include an extra figure for the SW?
REPLY: The SW part is not removed from the paper. As we answered to S. Otto (referee), our paper is about longwave radiative forcing. During the writing of Section 5 we thought that including the SW component would allow us to estimate the ratio LW/SW and therefore quantify the importance of the LW forcing contribution in cases of dust outbreaks in Barcelona. The idea

we have in mind is to draw the attention of the regional and global climate model community that the LW component is not always negligible. We have decided to keep the sensitivity analysis (Section 4) only in the longwave and to state clearly in the introduction that SW calculations are made to quantify the importance of the LW contribution (only in Section 5). 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.

Answer to RC C2061

The changes in the revised manuscript (posted soon) will be indicated in bolt font.

The paper addresses the study of radiative forcing due to atmospheric aerosol, with special emphasis on the longwave spectral range. Being this spectral range less studied than the solar spectral range the paper is worthy to be published in ACP. The use of a radiative transfer code that includes absorption and scattering effects of the aerosols in the longwave spectral range is an added value of the manuscript. The paper is well written and presents an appropriate structure. Nevertheless there are some points that must be improved before the paper would be ready for publication in ACP.

Particular Comments

- There is relevant question concerning the methodology and the way the authors define the radiative forcing concept. According to the literature the aerosol radiative forcing represents the change in the net solar irradiance associated to the inclusion/exclusion of atmospheric aerosols. Using this approach the use of equations 1 and 2 for the longwave spectral range is correct. For the shortwave spectral band equation 2 is correct at TOA but equation 1 is wrong at BOA, In fact the radiative forcing at BOA will be equal to equation (1) multiplied by the factor (1-alpha) with alpha the surface albedo. This fact needs to be clarified and carefully took into account in any comparison with results derived in other studies. In fact, the use of equation (1) implies an overestimation in the absolute values of radiative forcing strongly dependent on the surface albedo.

REPLY: We thank the referee for pointing out this issue. 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. This has been clearly modifies in Eq. 1 and 2 in the revised manuscript. This modification also answers to the second question about the surface albedo which is included in the term F(BOA, up).

A second question concerns the way the authors do the radiative forcing computations. Thus they comment on line 7 page 8541that they compute the daily values, I understand that this means the integration over 24 hours for both the longwave and the shortwave forcing. But in section 5 they analyze particular cases that according to Table 3 correspond to short periods, when the lidar profiles are available, so it seems that these are instantaneous values. These points must be clarified in the revised manuscript.

REPLY: The word “daily” on line 7 page 8541 was a typo. It has been deleted from the text. All forcings in Section 5 are instantaneous forcings. It is now clearly indicated in the captions of Figure 9 and Table 3.

- Recent studies published in ACP journal analyzed the aerosol direct radiative forcing in the shortwave spectral regions for Mineral Dust events detected over the Iberian Peninsula, Valenzuela et al. (2012). The authors must include these results in their comparison of radiative forcing estimates presented in section 5.

REPLY: The discussion in Section 5 has been extended in the revised manuscript and includes now comparisons with Valenzuela et al. (2012).

- Along the text the authors use AOT, aerosol optical thickness, to describe the aerosol load in the vertical column. The right term is AOD, aerosol optical depth that is the AOT in the vertical path. AOT depends on the solar elevation while AOD does not.
REPLY: In the revised manuscript the term AOT has been replaced by AOD. Thank you for this comment.

- Concerning the average volume size distribution in Figure 3, the authors must clearly state since the beginning that in addition to the mean size distribution there is some information informing about the deviation around the mean, included in Table 2. At least in terms of the standard deviation of the different parameters that the define de bilognormal distribution they use. In this table is a little bit surprising the rather low values of standard deviation for the different parameters, how the authors did these computations. Anyway in some cases the number of significant figures for the standard deviation is excessive, more than one significant figure is not justified is the more significant is larger than 2, otherwise two significant figures are enough to identify the uncertainty of the parameters
REPLY: In the revised manuscript the caption of Figure 3 makes now reference to Table 1, which contains information about the deviation around the mean. All the standard deviations given in the paper have been calculated the same way as the square root of the variance.
About the number of significant figures for the standard deviation, I am afraid we do not understand the referee's comment. In the ACPD manuscript all standard deviations in Table 1 are given with a number of significant figures equal or less than the number of significant figure of the mean value. Can the referee precise where the number of significant figures is not justified?

- In section 3.2.2 provide an average temperature from CERES. The value is offered with up to two decimal figures and with an standard deviation of 6.56 K, that clearly has no sense as a measure of uncertainty, 7 K will be the right figure. More information on the use of CERES data, like level and version of the data, acquisition time and temporal and spatial resolution are required. Furthermore, I have an additional question concerning the use of a fixed temperature for the "whole day", because the surface temperature is not constant along the day. How this hypothesis affects the study?, at least the part where the authors use the "model" they describe in Table 2.
REPLY: The standard deviation has been rounded to 7 K. Information about CERES products has been added in Section 3.2.2 in the revised manuscript. See also answers to S. Otto's (referee) comments 10 and 14.
The effect of the surface temperature on the longwave radiative forcing is discussed based on Figure 7f in Section 4. Also in Section 5 the surface temperature is identified as the reason for the differences observed between the measured and the modeled outgoing longwave radiation.

- More details on the atmospheric heating rates computation are required. Furthermore, it would be worthy discussing the results with the heating rates computed by Guerrero-Rascado et al. (2009) during an extreme episode of Saharan dust outbreak that affected the Southern Iberian Peninsula. The authors must revise Figure caption 10 that seems to be incomplete.
REPLY: Some details about the computation of the atmospheric heating rates have been given at the end of Section 5 in the revised manuscript. It also includes references to the work from Guerrero-Rascado et al. (2009).

- The conclusions must be revised according to the previous comments.
References Guerrero-Rascado, J. L., Olmo, F. J., Avilés-Rodríguez, I., Navas-Guzmán, F., Pérez-Ramírez, D., Lyamani, H., and Alados Arboledas, L.: Extreme Saharan dust event over the southern

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Valenzuela, A., Olmo, F. J., Lyamani, H., Antón, M., Quirantes, A., and Alados-Arboledas, L.: Aerosol radiative forcing during African desert dust events (2005–2010) over Southeastern Spain, Atmos. Chem. Phys., 12, 10331-10351, doi:10.5194/acp12-10331-2012, 2012.

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 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 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 µm and LW: 4 – 50 µm. 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.

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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