

Interactive
Comment

***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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Received and published: 19 May 2014

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,

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Interactive Discussion

Discussion Paper



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 measurable 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

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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 **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

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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 $> 10 \mu\text{m}$) 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

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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 known. 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

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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.

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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.

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Please also note the supplement to this comment:

<http://www.atmos-chem-phys-discuss.net/14/C2496/2014/acpd-14-C2496-2014-supplement.pdf>

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