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Atmospheric Chemistry and Physics Discussions

Interactive comment on "On realistic size equivalence and shape of spheroidal Saharan mineral dust particles applied in solar and thermal radiative transfer calculations" *by* S. Otto et al.

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Dear Referee 3!

Thank you very much for your 'General Comments':

This article presents informative new results on the effects of non-spherical dust particles and coarse particles on the shortwave and longwave radiative balance. I recommend the article is published subject to minor revisions, which are mostly clarifications and additions to the abstract and conclusions.

In the following we would like to discuss your 'Specific Comments':

1. **The abstract is a nice summary of the article but has a weighting towards the non-spherical results – it would be beneficial to include some more information on the nice results presented in the paper regarding the effects of the large particles.**
Our paper focuses rather on the effect of size equivalence and particle shape. The role of the large dust particles was already noticed by Otto et al. (2007) for a dust layer observed during ACE-2. Thus, we decided not to concentrate on the coarse mode effects in the Abstract. But, if desired, we would add to the last sentence 'They strongly enhance the absorbing properties and forward scattering in the solar spectral range as well as they increase predominantly, e.g., the total forcing at TOA of the dust over land.'

2. **The advances and different methodologies presented in this paper compared to Otto et al. (2009) should be emphasized more, especially towards the end of the introduction – i.e. if it is the case that the present work uses an AR which varies with particle size, and that the work includes the effect in the IR, this should be explicitly stated towards the end of the introduction to avoid confusion. For example, it would be useful to move or replicate the paragraph on page 29205 lines 8-12 to the introduction to emphasize this.**
Yes, the present work uses an AR (axis ratio) distribution, which varies with particle size (and altitude), and includes the effects in the thermal spectral range. To avoid confusion and not to provide too detailed information in the Introduction we suggest to remove 'over the entire spectral range' on page 29196 (line 13). W.r.t. the paragraph on page 29205 we suggest to replicate it as given next: 'Main differences to the previous investigations (Otto et al., 2009) are the following: we now consider the total (solar plus thermal) spectral range and in-situ measured AR distributions (Fig. 1) that vary with particle size and altitude instead of constant axis ratios for each model

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particle when dealing with spheroidal scatterers. With both particle models we wish to demonstrate the ambiguities to which the treatment of non-spherical particles may lead (Sect. 4.2)'.

3. **The notation the authors use for radiative forcing is presented in equation 1, and is opposite to the conventional definition of forcing, where a net warming of the earth-atmosphere system (TOA forcing) is represented by a positive number (e.g. IPCC reports). The way that forcing is calculated in equation 1 means that a positive forcing results in a net cooling of the earth-atmosphere system. I would urge the authors to follow the notation of IPCC in order to avoid confusion and misinterpretation of their results.**
The same notation was applied in our previous paper (Otto et al., 2009) to which we would like to be consistent. Moreover, from an energetic point of view a positive forcing means that more radiation is transferred to space when adding the dust to the clear-sky atmosphere. We think this is more conceptional.

4. **As a point of interest, it would be interesting to comment how the spherical vs non-spherical forcings might be affected by different solar zenith angles, or when considered as a diurnal average, rather than just with an overhead sun as calculated here. The conclusion is a nice summary of some of the implications of the work and further areas of study, but would benefit from including a short summary of the key results presented in the main part of the article.**
Indeed, this would be a nice task for a future paper. However, here we aim mainly on the determination of realistic size equivalence and particle shape which we tried to constrain, that is, to determine some of the free parameters. A second step would be to perform sensitivity studies w.r.t. additional independent parameters or settings as: zenith angle, additional background aerosol (e.g. sea salt) and presence of clouds or different radiative transfer solvers (two- and four-stream approximations). Therefore, we would like to defer this task to future.

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'**Technical Corrections**':

Notation is not consistent throughout the article for 1) the asymmetry parameter is defined with ASP earlier in the paper (e.g. page 29194 line 3) and later referred to as g. 2) the notation for aspect ration is also variable – beginning with AR and later switching to ASR (e.g. page 29200 line 1). Notations should be changed to be consistent throughout the article.
We think that it is well-known that g means the asymmetry parameter and sometimes it is better to mix both notations to avoid the too frequent usage of ASP or g. AR and ASR are not the same. AR means 'Axis Ratio' and ASR 'ASpect Ratio'. Two different ASRs may have the same AR, e.g., ASR=0.5 and ASR=2.0 refer to the same AR of 1:2. The discrimination of both terms is important and is consistent throughout the paper. But for final version of the paper we will check this point.

Page 29195 line 8 – 'But these authors assumed constant ARs for each model particle.' It is not clear to me what is meant by this – I assume it means that AR is constant for all particle sizes? Please clarify this sentence in the article. Likewise, in the following sentence it would be clearer to say that AR varies with particle size, if this is what is meant. This wording is also used at various other points in the article (e.g. page 29197, line 24). It would be clearer to reword this description in all cases from 'each model particle' to become 'each particle size' or something similar.
With 'AR is constant' it is mainly meant that no AR distribution is applied but a single AR. By the way it implies that AR does not vary with particle size but this may be possible. To write 'constant ARs for each particle size' might also be confusing because a 'size' cannot have an AR, only a model particle can have an AR. Obviously, this is not clear. We suggest to use 'non-distributed AR for each model particle' or 'monodispers

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AR for each model particle' or 'monodispersely distributed AR for each model particle'?

Page 29195, paragraph starting line 1 – this paragraph is interesting, but is it strictly of relevance to the article?
Yes, it is. It shows that the chemical composition measurements by Kandler et al. are confirmed by an independent study. The same holds for our assumption of an internal mixing of the dust particles.

Page 29204, lines 3 to 7 – so in the ocean case does the dust reach down to 1.15km? Please clarify this.
Yes, the dust layer always extends from 1150 to 5600 m asl. In the land case the surface is at an altitude of 1150 m asl, in the ocean case at 0 m asl, that is, the layer is lifted. We will clarify this. 'While in the case of land the observed environmental conditions around OZT site on 19 May 2006 (see Sect. 2) are used, namly a model atmosphere down to 1.15 km a.s.l. (OZT), we apply a tropical standard atmosphere (Anderson et al., 1986) down to 0 km over ocean. In both cases the dust layer ranges from 1.15 km up to 5.6 km a.s.l. such that in the latter scenario it represents the case of the same dust plume but lifted as during long-range transport over the Atlantic ocean.' Ok?

Page 29211, lines 27-29 – This sentence is not clear – do you mean uncertainty in solar irradiance measurements due to the surface albedo data?
No, uncertainty of 3 % in the irradiance data and 5 % in the albedo data. We suggest 'For example, Otto et al. (2009) reported uncertainties of about 3 % in the solar irradiance measurements and of 5 % in the solar-surface-albedo data (Bierwirth et al., 2009).'

Page 29217, line 16 – I believe 'in the infrared' is missing at the end of the line?

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Yes, 'Since the dust absorbs solar radiation and blocks thermal emission to space, always positive values of the heating effect are obtained in the solar as well as negative and positive ones in the thermal range within the upper and lower altitude range of the dust plume, respectively, which is qualitatively independent of the underlying reflecting surface (land or ocean).'

Page 29218, line 5 – change 'validate' to 'extend' or similar – other days of data will not validate this data but would put it into a wider context.
We will write 'extend'.

Figure 12 and 13 – These are busy figures showing a lot of useful detail. I am struggling to see all the lines clearly as the figures are – I suggest separating the left and right figures within both fig 12 and fig 13 into two separate figures each to create four overall larger, clearer plots.
This would be possible, of course, but it may be unfavourable for the structure of the paper from an editorial point of view.

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 29191, 2010.