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Interactive comment

Interactive comment on "Retrieval of effective aerosol diameter from satellite observations" *by* Humaid Al Badi et al.

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We thank Dr. Lars Klüser for his constructive and valuable comments. This reply is structurted by introducing sections of the comments (in Italics) followed by our response. The page and line numbers of the updated version of the paper are used in the responses unless otherwise stated. The amended manuscript is attached in the supplement PDF file.

Specific comments: title: The authors should make clear that the method is designed for dust aerosol and not for all kind of aerosols. All: I would ask the authors to provide equation numbers in the revised manuscript.

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The authors agree with the suggested title as it reflect the content more clearly. Equation numbers have become necessary with the new amendment, which has a greater number of mathematical equations.

Action: The title has been changed to “Retrieval of effective dust diameter from satellite observations”. Equation numbers have been provided.

p. 2 l.12ff: Normally I do not ask authors to cite my papers during review. But this case is specific, so I will deviate from the normal approach. The authors cite our first paper as well as the first paper in a full series describing the French LMD dust retrieval algorithm and infer the claim that satellite methods tend to underestimate effective radius. I do fully believe in the fact. But - there has been a lot of work done since the publication of these papers. For example by Klüser et al. (2015) in Remote Sensing of Environment and by Capelle et al. (2014) in ACP to name only the latest ones for the two methods referred to by the authors. In the Klüser et al. (2015) paper the authors also would see the impact of a variety of dust property assumptions on the particle size retrieval. The most important impact is the one of assumed particle sphericity. And here is also lies one of my biggest problem with the study: the authors do not at all acknowledge that particle shape has an extremely important impact on the retrieved particle size (for non-spherical particles also the definition of what effective particle size is is important!). We have done a small experiment with using different refractive indices and different assumed particle shapes for dust spectra and compared it to laboratory measurements and to Mie calculations. The results have been published as Klüser et al. in Journal of Quantitative Spectroscopy and Radiative Transfer this year. The authors might wish to look into that study (or a similar experiment by Legrand et al., published in 2014 in the same journal) for good descriptions of the impact of particle shape and dust composition on infrared extinction.

p. 2 l.20ff.: Here again I would strongly recommend to comment on the non-sphericity of dust particles and its impact on infrared radiation. Nevertheless the authors may go on with the Mie calculations. Indeed for broadband instruments such as SEVIRI the impacts might be small compared to other aspects, so the study doesn't loose its value from describing the problem of non-sphericity. The authors could even prove this by a small comparison of Mie extinction efficiencies and, for example, T-matrix extinction efficiencies integrated over the relevant SEVIRI bands.

The recent improvements in particle size retrieval have been revised and referenced in the revised manuscript. The authors acknowledge the complexity of the retrieving effective dust particle size using analytical approach. Apart from the recent improvements, many studies tackled this problem through theoretical analysis, but had limited success in filling the gap between the observed and the modelled particle size. The reason, as the reviewer points out, is the high number of dependent variables that link the remotely sensed radiance and particle size in the radiative transfer theory. The high uncertainty in the approximation of variables such as the chemical composition and particle shape, might be one reason to limit the advance in improving the accuracy of retrievals. The results of previous particle size models seem to inherit the noise introduced by the vague estimation of the many dependent variables. In a single thermal SEVIRI band, the effect of dust diameter is potentially "diluted" and difficult to see while the case turn out to be different in Brightness Temperature Difference of 8.7 and 12.0μm (ΔT_{8-12}). This study approach aim to avoid the inherited noise of many dependent variables by exploiting the strong and dominate exponential effect of the particle size on the value of ΔT_{8-12}. Here we try to present the empirical evidence of this relation and then use it to build a formula based mainly on empirical data and a simplified conceptual model.

Action: To make the thesis of the paper clearer, Section 3 has been rewritten to include more details to explain the empirical evidence of dominate exponential effect of the particle size on the value of ΔT_{8-12}. More detailed description of the effect of surface emissivity, water vapour, and dust layer properties has been also included in section 3. In section 4.3(Discussion of Results) more details discussion on the limitation of the model has been presented.

p. 2 l. 26: Satellite instruments measure radiance, not radiative flux. As flux is the radiation emitted into the hemisphere, there will be no way of measuring flux by satellite. Indeed for climate studies flux has to be estimated from satellite radiance, which is extremely difficulty due to the anisotropic nature of earth-leaving radiation.

Action: corrected

p. 3 section 3: It is common knowledge how to derive Mie extinction efficiency. So it is sufficient to present the Q_ext formula and provide any textbook on radiative transfer as reference. In the given way it might be worth to at least state the definition of x.

The aim of that introduction is to explain the relation between the extinction efficiency difference, effective particle diameter and BTD.

Action: Some common-knowledge details have been removed in Section2 Page3.

p. 3 l.23: Delete the sentence starting with "Berg et al. (2011)" as the large particle limiting case is of no interest for this study.

Action: done.

p. 4 l. 6: Be again aware of claiming proportionality which is not true (such as *the one between Qe and I_0 and between I_0 and T). For example: if the claimed proportional-ity of 1/I_0 and Q_e would be true, that would signify, that for sufficiently large particles, regardless of AOD and temperature, the intensity would always be half of I_0 as Qe tends to be 2 for large particles. The authors will easily acknowledge that this cannot be true.*

p. 4 all: As I have outlined above, the claims made here by the authors are overly simplified and in the context of radiative tranfer just wrong. The authors may have good reasons for sticking with these simplifications, but in that case they should spend a lt more effort in explaining.

There is a typing mistake in the referenced line number. Instead of $Q_{ext}\ \alpha\ \frac{1}{I_0}$, it should be $Q_{ext}\ \alpha\ (1 - \frac{I_r}{I_0})$ $(from\ \ Q_{ext} = \frac{P_{removed}}{I_0})$ where (I_r) is the radiance received by satellite radiometer. The pretext in the introduction and the context that follows the relationships fits this intention. However, the Authors agree that there is over simplification in the analogy of the radiative transfer on *page 4* in the referenced manuscript because of using Rayleigh-Jeans law, which is not appropriate in thermal infrared part of the spectrum.

Action: more detailed mathematical description using Plank's function to justify using the difference of extinction factor has been presented.

. . . Also I am missing a description of the impact of dust layer height and temperature to the brightness temperature difference. These impacts are quite well understood and described in many papers.

It is clear that the dust layer brightness temperature decreases with height mainly due to decreasing ambient temperature. But the change of BTD with height is less obvious. Brindley & Russell (2006) and Merchant et al. (2006) used radiative transfer models to show that ΔT_{8-10} changes with changing the dust layer height and AOD, extinction

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Discussion paper

coefficient and absorption (Emissivity). Taking into the account that AOD, extinction coefficient and emissivity are all a function of the particle size, the change in ΔT_{8-10} also conveys information of the particle size. Hence, the change in ΔT_{8-10} value can be partly attributed to change in the particle size and it is misleading to conclude, based on these studies, that there will be a big impact on the accuracy of the effective particle size retrieval using ΔT_{8-12}.

Action: The above description of the dust layer height and temperature change has been added to Page 7, Line 19.

p. 4 l. 11: As already stated in the general comments the Ryleigh distribution falls from blue sky. Is there any physical motivation of using it? Otherwise it would be worth to quantitively prove that it is appropriate, for example with a small correlation experiment.

The main motivation derive is from the empirical observations which have been more clearly explained in section 3 of the revised manuscript. The formula has been changed to obtain better results but it still shows the distinctive exponential pattern as described by Figures 5, 6 and 7.

p.5 l. 6: I am missing a description how these numerical values have been obtained.

Excel solver (Fylstra et al., 1998; Harris, 1998) was used with the values of d and the corresponding ΔT_{8-12} from the four dust cases described above to solve for the $\frac{\Delta T_{8\ -12}}{(E+\Delta\varepsilon)} = (\frac{A\ d^3}{(e^{\alpha d}\ -1\ )} - C)$ model coefficients. The technique was based on converging the solutions of the points towards a minimum sum of square deviation.

Action: This detail has been added in Page 14, line 1.

p. 5 . l. 9: I do not understand this sentence. Does it mean, a LUT of the scaled brightness temperature difference has been created? For which d values? has the surface emissivity been kept constant?

In the amended paper (Page14, Line5), the LUT was be created for d versus $\frac{\Delta T_{8\ -12}}{(E+\Delta\varepsilon)}$. Neither $\Delta T_{8\ -12}$ nor emissivity are constants. But, since $\frac{\Delta T_{8\ -12}}{(E+\Delta\varepsilon)}$ is a function of d, there is a unique value of $\frac{\Delta\ T_{8\ -12}}{(E+\Delta\varepsilon)}$ corresponding to every value of d.

p. 5 l. 14f.: This is true only for intense dust storm conditions. Typically the majority of the radiation reaching the satellite still is emitted from the surface and has undergone no scattering at all. For example for a moderately thick dust optcial depth of 0.5 the transmittance is about 60% and for AOD of 1 the transmittance stil is approximately 37%. Taking into account emission by the dust layer itself (which the authors fail to comment on at all) these numbers reduce, depending on the single scattering albedo, but not in a way that the claim of the authors would become generally true. It is Ac-knowledging that these numbers refer to infrared optical depth and that the AOD ratio between the 11μm band and 0.55μm is somewhere around 2.7, these figures would translate to visible optical depths of 1.35 and 2.7, respectively.

p.5 l. 19: This dust flagging approach is by far too simple, see for example Ashpole et al. (2011) in JGR.

We agree that using the words “dominating” and “most of” are not appropriate since we are talking about a single band behaviours of 10.8 and 12.0 μm bands, not the difference between the two. We agree with the reviewer’s explanation which does not contradict the idea intended to be transmitted to the reader in that paragraph.

Action: The paragraph starting at Page 19, Line 7 has been rephrased.

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Discussion paper

p.5 l21-29: I do not believe the claim that AOD should have hardly any impact of the brightness temperature, especially as almost every other study published in this field claims the opposite. If the authors wish to maintain this claim, they need to prove it by rigorous radiative transfer simulations.

We are not claiming that AOD does not affect brightness temperature. To the contrary, we are saying that AOD is strongly related to the apparent brightness temperature. Rather our claim is BTD ($\Delta T_{8\ -12}$) has limited correlation with AOD as shown by the presented case (Page 9, Line 15).

Action: The paragraph (Page 9, Line 15) has been rephrased to make the case clearer.

p. 7 l. 11: One can compare model simulated particle sizes and satellite retrievals, but I would be extremely careful with calling this "verification of model results". Especially the proposed methods come with so many simplifications and assumptions, that no modeller would believe it is more accurate than the modelled values.

Action: The phrase (Page 20, Line 3) has been changed to “To provide an independent reference data for atmospheric aerosols model comparison”.

p. 7 l.13-17: If this would be the aim of the authors, they would need a good AOD retrieval as well, see comments to Q_e-I_0 proportionality above.

We agree that the performance of the solar power systems depends on the turbidity of the atmosphere; this depends on AOD, as well as the number of dust particle precipitates over the solar panels which correlate with the effective particle diameter. The technique proposed in the paper can give an indication of the amount of dust that will precipitate on solar energy systems from an upcoming dust event.

Action: The statement has been changed accordingly (Page 20, line 11).

p. 7 l. 21: This is the first time the words volcanic ash appear. The authors would need to explain why they are confident their method also works for volcanic ash (which in many ways is much more complicated than desert dust).

Action: Volcanic ash has been removed

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