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Atmospheric Measurement Techniques
Discussions

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

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I cannot recommend publication of the manuscript in the current form. I have the impression that the manuscript was written quite hastily, but also that several oversimplifications have been made without appropriate justification. I absolutely acknowledge the validity of simplifications and statistical representations in the treatment of radiative transfer, especially in the context of standalone satellite retrieval methods. Nevertheless the claims presented in section 3 seem to be too drastic. At several occasions the authors claim proportionality of variables, which is not justifiable in the presented form. For example the surface emissivity is by no means proportional to the brightness temperature differences. Of course the BTD is a function also of the spectral surface emissivity, but in a highly non-linear way, even in the simplemost representation by Planck emission and radiative transfer described with the Schwarzschild equation.

Consequently the claim that the ratio of BTD and surface emissivity at 8.7μm can be represented by an arbitrary distribution function (which has been introduced without any motivation of its use) cannot be physically sound. Still there might be good reasons to follow that approach, but then I would expect the authors to concisely describe the simplifications made and why they nevertheless use this statistical method. There are good examples of such approaches being successful, but the manuscript in the present form could easily yield the impression that the authors lack understanding of radiative transfer (which I do not believe). The coefficients for the solution of the statistical approach seem to fall out of the sky, the authors present no description of how they have obtained them. By solving a regression scheme? By a least-squares fit? By correlation methods? Several other issues (see specific comments below) support the impression that this study lacks the quality to be published in AMT. I fully acknoledge that the results presented by the authors look very promising and that retrieving dust particle size from space is of high importance. So I would encourage the authors to carefully read the comments and to provide a revised manuscript in response. This then has to undergo review once again. I see the potential in the authors work, that is why I am not suggesting rejection. But the manuscript definitely needs some serious work to be done before publication in AMT.

In this review I feel it justified to disclose my name, as I think there will be no harm to the overall review if the authors know, who I am. This will also likely support my arguments in the specific comments, as I do not intend to promote publication of my own papers but rather would like to draw the authors' attention to a couple of recent studies relevant to this work.

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.

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. 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.

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.

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.

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.

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 proportionality 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 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.

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. 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.

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

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?

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 Acknowledging 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.

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.

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 comes with so many simplifications and assumptions, that no modeller would believe it is more accurate than the modelled values.

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

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).

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