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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.

Anonymous Referee #2

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This paper is focused on the interesting topic of dust cloud properties. The authors present a new remote sensing approach for retrieving the effective particle radius in dust clouds observed by SEVIRI. While the objective of the paper has merit, the methodology seems to be flawed or, at the very least, poorly explained. Therefore I do not recommend this paper for publication in its current form. Please see the comments below for specifics.

Section 1: âĂć "The correlation between T and dust aerosols is rather complex and linked to many parameters. It is mainly caused by Aerosols Optical Depth(AOD), dust particle size and shape and the emissivity which in turn linked to dust chemical composition (e.g. Brindley et al. 2012; KluÌĹser et al. 2011)." The satellite measurements are also sensitive to the surface temperature, surface emissivity, atmospheric water vapor and temperature, and viewing angle. For optically thin dust clouds, the non-dust cloud

property components are especially relevant. Thus, I do not agree with the statement as written.

Section 2: âĂć The authors should acknowledge that dust particles are not spherical. While I believe that the assumption of spherical particles is a secondary issue, motivation for treating dust particles as spheres should be provided. âĂć It is also not clear as to what kind of size distribution was used in the Mie calculations. If the calculations were done for a single particle then the results are not at all representative of the particle size distributions present in nature. Also, the Mie calculations are a function of wavelength. Did the calculations take into account the SEVIRI spectral response functions?

Section 3: âĂć The proportionality arguments do not make physical sense. The extinction efficiency is solely a function of the microphysical properties of the dust cloud, and is intrinsically independent of the incident radiation. In addition, the measured brightness temperature and incident radiation have a complex, non-linear, relationship. Further, the 8.7-12 um BTD is a complicated function of many variables and is not simply proportional to the 8.7 um surface emissivity. As such, the algorithm theoretical basis seems to be badly flawed, which is a primary reason I cannot recommend this paper for publication at this time. The authors need to provide a much more convincing argument for the theoretical basis. The generation of the various empirical relationships is also poorly explained. âĂć The term "reemitted" is used. I recommend not using this term as matter emits radiation because it has a temperature. Once a photon is absorbed it should be considered dead and gone. âĂć Even though the algorithm is restricted to pixels that meet certain BTD requirements thought to be related to optical depth the background atmosphere and surface and viewing angle will still influence the retrieval to varying degrees. The authors should include a sensitivity analysis that justifies their assumptions, as most modern retrieval methods do not make such assumptions.

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