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Atmospheric Measurement Techniques Discussions

Interactive comment on "Estimation of cloud optical thickness, single scattering albedo and effective droplet radius using a shortwave radiative closure study in Payerne" *by* C. Aebi et al.

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This Discussion paper addresses a matter that is of current interest: the determination of optical characteristics of clouds based on measurements and observations from the surface. Specifically, the authors suggest a new method to determine cloud optical thickness (COT) from the measurements of shortwave (solar) irradiances at the ground. The method is suitable for stratiform cloudiness, either low thick clouds (St, As), when diffuse irradiance is used; or thin high clouds (Ci, Cs) when direct irradiance is of use. In both cases, besides the COT another cloud characteristic can be derived: droplet

effective radius in one case; single scattering albedo in the other case.

Research is correctly designed, and the data used to develop and apply de method is also adequate. Several comparisons with previous methods that estimated COT are presented, which is also a value of this study. I would say that the method is somewhat brute-force (as many simulations are performed with the radiative transfer model for each particular measurement), but this is not a problem as the computational cost is relatively.

The only general comment, which is not really a criticism but a question that the authors may address either in the introduction or, more precisely, in the discussion of the results, regards the practical utility of this method. If I understood correctly, estimates of COT by the new method essentially match estimates with Barnard-Long (2004) expression. Being the latter much simpler that the former, in what conditions the new method is useful?

Some other minor comments and suggestions follow:

- Introduction. The work Matamoros, S., González, J.-A., & Calbó, J. (2011). A Simple Method to Retrieve Cloud Properties from Atmospheric Transmittance and Liquid Water Column Measurements. Journal of Applied Meteorology and Climatology, 50(2), 283–295. https://doi.org/10.1175/2010JAMC2394.1 could be of interest for the authors, as it presents also a simple method based on the measurements of transmittance at 415 nm.

- Line 75-77. "[. . .] can provide the limits of the minimum possible agreement[. . .]" Does this mean simply "the best possible agreement"?

- Section 2.1. Although pretty obvious, should you mention that you use a sun-tracker for diffuse and direct irradiance measurements? And, what does "homogenized" mean in this context (this is a term usually employed in a climatic, that is, long-term series, context)?

- Fig. 1. You should explain if the data shown is only for cloudless days (as I think is the case for AOD), or for all days.

- Section 2.2., lines 136-137, it is not needed to repeat the period included in the analysis.

- Line 189, Kokhanovsky is good reference, but not the only source where optical properties of clouds are explained. You could add “for example”.

- Why do you need a section 3.1.1 if there is no section 3.1.2?

- Starting in section 3.1 but then across all the rest of the paper. Be cautious with the use of significant figures in the numbers you give. Many times you give four significant figures, but only one (or two) are really significant. For example, 53.54 % $\pm$ 20.92 should be 53 % $\pm$ 21; 5.71 Wm-2 $\pm$ 11.91 should be 6 $\pm$ 12, and so on.

- Lines 233-234. The sentence “As ice habit the default solid column is included” sounds awkward to me.

- Section 3.2. I suggest reordering the description of these methods. First, all methods regarding St-As, that is COT and reff; then, methods regarding Ci-Cs, that is COT and SSA.

- Line 522. In the first moment, it is not clear if DSR refers here to global (or total) irradiance. Later, the reader infers it, as results for direct and diffuse are commented. But I suggest indicating always which irradiance the authors refer to. In addition, I encourage consistency regarding the use of “total” and “global”.

- Final comment in section 5.1. It should be discussed the fact that AOD cannot be measured under cloudy conditions. So, strictly speaking, AOD data are not introduced the same way under cloud-free and under cloudy conditions.

- Section 6.2. I don’t understand one result commented here. If MODISs underestimates COT, these lower COT should result in an overestimation of DSR.

- Conclusions. Your results seem to indicate that COT_modis are not correct, at least not correct under conditions analyzed in the present study. I suggest adding this conclusion.

Technical matters:

- Please check acronyms and their definitions. For example, DSR is defined in the abstract, but not before its first use in the main text. Or, SSA is defined in line 187 but it has been used many times before.

- Table 3. COT and SSA are dimensionless variables; they don’t have Wm-2 units.

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