Atmospheric Measurement Techniques Discussions

Atmos. Meas. Tech. Discuss., referee comment RC2
https://doi.org/10.5194/amt-2020-480-RC2, 2021

Comment on amt-2020-480

Anonymous Referee #2

Referee comment on "An alternative cloud index for estimating downwelling surface solar irradiance from various satellite imagers in the framework of a Heliosat-V method" by Benoît Tournadre et al., Atmos. Meas. Tech. Discuss., https://doi.org/10.5194/amt-2020-480-RC2, 2021

General comments:

The explicit strength of the "original" Heliosat approach (referred to as Heliosat-o in this review) is that the retrieved cloud index ("cloud transmission") is completely based on observations. No simulations or external data are needed to retrieve the cloud index (cloud transmission) but the observed radiances are used. This includes the retrieval of Xmin ("clear sky reflection") & Xmax ("calibration"). Heliosat-o and the resulting radiation data are well validated and established (e.g CM SAF, ISE, University of Oldenburg and Bergen, Satellight….) and already close to the accuracy of well maintained ground based stations. Of course, there are some limitations linked with the Xmin retrieval, as listed by the authors (L85). However, some of the mentioned handicaps are already partly resolved (e.g. shadow correction method by University of Oldenburg) or on average of relative small effect (e.g. long lasting clouds occur usually in the North-West during wintertime. This means high COD and low SZA. Hence, low solar irradiance and thus low absolute errors induced by uncertainties in Xmin). In my opinion there is a high likelihood that the simulation of Xmin adds more handicaps and uncertainties than it resolves. Thus, the central question is: Is there an overall benefit, concerning accuracy and precision, if the observational-based Xmin retrieval is replaced by simulations. Why should the simulations lead on average to more accurate results than using observations ? The authors mention "Simulations consider the anisotropy of the reflectances caused by both surface and atmosphere, and are adapted to the spectral sensitivity of the sensor. The anisotropy of ground reflectances is described by a bidirectional reflectance distribution function model and external satellite-derived data". Simulations might consider it, but to my experience they induce also additional uncertainties, e.g. the uncertainty induced by using 3rd party surface albedo data can easily lead to a bias of several per cent. Further, as for RMIN, clear sky situations are needed to retrieve the surface albedo, thus concerning long lasting clouds the same handicap is shared. The needed BRDF (ADM) functions induce further uncertainties and add complexity. A more complex method providing overall a lower accuracy would be of no significant value. The effect of SAL (surface albedo) and BRDF is already considered by observational-based Xmin for the same sensor and viewing geometry, no need for simulation.

Major concerns:

In my opinion the authors fail to show the advantage of combining the Heliosat relation (equation) with simulations of the radiances in order to get Xmin ("clear sky reflection"). If radiances (reflectances) are simulated than why not simply using one of the several RTM based LUT approaches or ECMWF. By the way, using BRDFs simulations to estimate radiances observed by satellite is already applied since decades in RTM based LUT approaches, thus this Is not a new idea. Where is the benefit to use the Heliosat relation (equation 1) when the special strength of Heliosat is disminished by using simulations ? These questions are not appropriately addressed in the manuscript. The authors mention that a motivation for the approach is the use of polar orbiting satellites, but again what is the advantage compared to RTM based LUT approaches (using COD&reff or TOA Albedo).

In summary, a more thorough discussion and description of the pros and cons of the presented method compared to established methods should be added (Heliosat-o and RTM LUT approaches). Uncertainties of BRDF and SAL should be discussed, more information on SAL source should be added. Also the solar zenith angle dependency of SAL in relation to BRDF should be discussed in more detail. Further, the potential improvements should be proven and discussed thoroughly by comparison with established high quality data sets, which are using the original observational-based Heliosat-o approach and with other data sets from external sources, e.g. ECMWF. Please note, comparison with Helioclim might be not a real benchmark for improvements, see e.g. Posselt et al, Remote Sensing of Environment Vol 118, 2012, pp, 186–198. Respective open data sets are available for inter-comparison. Concerning polar orbiting satellites, results should be compared to the ECWMF radiation data set.

I think that simulations of Rmin has been already used for the so called "Heliosat-2" version. Thus, the novel aspects of the approach should be reflected in more detail relative to "Heliosat-2" as well. By the way, calling a method with Rmin simulation still Heliosat is quite confusing. Rmin simulation breaks with the basic idea of Heliosat, thus using the name Helioat should be avoided in order to avoid misleading interpretations. Overall the discussion should be modified to be more balanced and reflected , lessons learnt in other projects and communities should be considered.

Specific comments.

- Please change the title, improved is not prooven, see general comments.

- 70 „raw satellite numerical counts (Pfeifroth et al., 2017; Perez et al., 2002)";

Here and throughout oft he manuscript. Misleading citations. Raw satellite counts has been used already decades before within the Heliosat community. Please modify accordingly. In general ATBD, PUMs are grey literature. Please check the citations and replace them with peer reviewed articles where possible.

- 80 "In this paper, we aim at finding an alternative to the need for archives of satellite imagery."

This is misleading, as long as radiances are needed using actual and/or 30 day is not a serious problem and not worth mentioning.

- 140 "Kc = 1−n introduced by Darnell et al. (1988)"

I think it is a well known and established that a modification for higher n is needed and respective modifications are published, please refer them.

- 190 "Cloud-index methods in the literature use various ways to estimate the TOA reflectances in overcast conditions (Perez et al., 2002; Lefèvre et al., 2007; Pfeifroth et al., 2017)."

Pfeifroth et al. 2017, again misleading citation. Please refer to the original peer-reviewed publications . In general ATBD, PUMs are grey literature. Please check the citations and replace them with peer reviewed articles where possible.

- 65 Xmin is ued later on rho_clear please unify.