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An improved cloud index for estimating downwelling surface solar irradiance from various satellite imagers in the framework of a Heliosat-V method

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Background

The study is in the framework of the development of the HeliosatV method for estimating downwelling solar irradiance at the surface from satellite imagery. It is claimed that a new way to retrieve a cloud index from a large variety of satellite instruments on geostationary and non-geostationary platforms was developed. The method uses simulations from a fast-radiative transfer model to estimate overcast (cloudy) and clear-sky (cloud-free) satellite scenes of the Earth's reflectance. An implementation of the method is applied to the visible imagery from a Meteosat Second Generation satellite. Results from preliminary implementation of Heliosat-V and ground-based measurements show a correlation coefficient reaching 0.948, for 15-minute means of downwelling surface radiation, similar to operational and corrected satellite-based data products (0.950 for HelioClim3 version 5 and 0.937 for CAMS Radiation Service).

General Comments

1. It was difficult to read the paper due to lack of transparency caused by following:
   a) Superfluous information dominates the text.
   b) This is not a review paper so there needs to be a strong focus on the objective of the paper.
   c) Many statements were repeated several times in the text.
   d) There was a frequent jump from one topic to another.
2. The discussion in many instances went into detail on a special topic (like aerosols) that were not utilized in implementing the methodology. One wonders why dwell on it.
3. If a new methodology is proposed there is a need to demonstrate that it is better than anything else that is available. The Authors state in the Abstract:

   Results from our preliminary implementation of Heliosat-V and ground-based measurements show a correlation coefficient reaching 0.948, for 15-minute means of DSSI, similar to operational and corrected satellite-based data products (0.950 for HelioClim3 version 5 and 0.937 for CAMS Radiation Service).

Since improvement was not demonstrated (against an earlier version of their methodology or any other methodology) why would one be interested in the described approach? Moreover, why do they provide information on the correlation only?

4. Something is amiss in the logic of the approach: the Heliosat idea is to use a cloud index to get Downwelling surface solar irradiance (DSSI). This, for simplicity of the process and contrary to the LUP table approach that is based on simulations. In order to use the LUP tables one needs to know the parameters used in the simulations to do the matching with the observed TOA radiance/albedo. Not clear what is the benefit in doing the simulations that are not appropriately utilized?
5. The argument that the simulated SAL is better than the library of min SALs or that it can be used with every satellite, is weak. To estimate the DSSI for each case using the Heliosat approach one needs the SAL at the time of the observation. How is such matching achieved?

Some misleading and unsubstantiated statements:

It is stated:

"the lower boundary is "archive-based", in most literature we reviewed: it is a minimum based on a time series of past satellite imagery. Such an approach is hardly applicable to non-geostationary satellites due to variable viewing geometries and a low revisit time

In this paper, we aim at finding an alternative to the need for archives of satellite imagery. It would then be easier to consider imagery from non-geostationary spaceborne platforms and produce a worldwide coverage.

It was not shown how the simulated albedo is used in the context of geostationary satellites and/or polar orbiters.

Stated:

Heliosat-V is a method approximating the attenuation of DSSI radiation by clouds with a cloud index, n.

We aim at developing an alternative "stateless" method to extend the application field of the cloud-index approach to a wider variety of orbits and optical shortwave sensors

What is "stateless"? How was it extended to polar orbiters? The paper deals only with SEVIRI.

Briefly, in addition to the lack of clarity of the text it seems that it was not demonstrated that the stated objectives of improvement and generalization have been achieved.

In the section between lines 245-250 the following statements are made:

1. *The use of optimal calibration is out of the scope of our work. Still, we compared gains coefficients proposed by EUMETSAT gEUM with those provided by Doelling et al. (2018) gD2018 for the measurements produced by the Meteosat-9 250 0.6 and 0.8 µm channels in 2011.*

2. *They show a mean disagreement, calculated as (gEUM − gD2018)/gD2018, of about -9 % for 0.6 µm and -8 % for 0.8 µm during this period (also illustrated on Fig. A1). Such errors will affect with the same magnitude the agreement between numerical simulations and measurements of clear-sky TOA reflectances, underlining the importance of absolute calibration for the Heliosat-V method.*

   Not obvious what is the message of the Authors here: on one hand, the calibration is out of the scope of their work. Then they report on the evaluation of different gains which show large differences (-9 %). They continue to state:

   Such errors will affect with the same magnitude the agreement between numerical simulations and measurements of clear-sky TOA reflectance, underlining the importance of absolute calibration for the Heliosat-V method.

   Which is it? Is it important or not?

In Figure 6 provided are:

Simulation of clear-sky reflectances at the TOA (ρclear) for MSG 0.6 µm (left panel) and 0.8 µm (right panel) spectral channels compared with actual satellite measurements. The comparison is done for all 11 locations, for the year 2011.

How was this comparison done? At each of the 11 locations, the atmospheric conditions are different. The atmospheric correction would be different. Not clear how the comparison was performed.

In summary, this manuscript is not ready for publication.