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Atmospheric Measurement Techniques Discussions

Interactive comment on "Surface solar irradiance from SCIAMACHY measurements: algorithm and validation" *by* P. Wang et al.

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Reply to referee #2

We would like to thank referee #2 for the comments, suggestions, and corrections. We have answered all the questions and revised the paper.

Because we submitted the manuscript in word file format, the page and line numbers in the questions from referee #2 are corresponding to this word file but not the online AMTD paper. Therefore, for every question we added the page and line number in the AMTD paper. The answers start with 'A: The question refers to page XX, line XX in the AMTD paper.

Interactive comment on "Surface solar irradiance from SCIAMACHY measurements:

algorithm and validation" by P. Wang et al.

Anonymous Referee #2

Received and published: 17 February 2011

The paper describes a new algorithm to calculate broadband surface solar irradiance from Sciamachy satellite measurements. Results of the method are compared with data of the BSRN network of pyranometers and the International Satellite Cloud Climatology Project. The new dataset introduced by the authors is a welcome addition to the pool of surface irradiance data. The topic is appropriate for AMT, and I recommend publication after considering the following minor comments:

Introduction, page 3: It should be better described how the EUMETSAT CM-SAF and Heliosat algorithms relate to (1) the algorithm described in the paper and (2) the "Pinker and Laszlo" algorithm introduced earlier in the introduction. For example, the authors could state early in the introduction that their method is based on algorithms a, b, and c, etc., before they start describing these algorithms and their method in more detail.

A: The question refers to page 875 in the AMTD paper.

Thank you for the suggestions. We have described the relation between the CM-SAF algorithm and the Pinker and Laszlo algorithm in the revised paper. In the CM-SAF algorithm the radiative transfer calculation for the clear-sky global irradiance is similar to the MAGIC algorithm which is used in the FRESCO SSI algorithm. The first generation of the CM-SAF surface solar irradiance algorithm was based on the Pinker and Laszlo algorithm.

Page 3, line 22: At this point it is not clear how the "cloud index" is defined. This is confusing. Mention that the definition is provided in Section 2.1.2.

A: The question refers to page 876, line 2 in the AMTD paper.

Agree. We have mentioned that the definition of cloud index is given in Sect. 2.1.2.

Page 6: At the top of page 6, the authors mention that cloud parameters can either be retrieved from oxygen absorption spectra (I presume the oxygen A Band) or from polarization measurements. They then introduce the FRESCO algorithm. It should be made clear how this algorithm relates to the earlier works by Koelemeijer et al., 2001; Kokhanovsky et al., 2006; Acarreta et al., 2004; Loyola, 2004; and Grzegorski et al., 2006 cited earlier. For example, does the FRESCO algorithm take polarization measurements into account or is it only based on Koelemeijer et al., 2001 while the other references just indicate that the problem can also be tackled with a different approach?

A: The question refers to page 877, line 18-30 in the AMTD paper.

The FRESCO algorithm was first developed by Koelemeijer et al. (2001). The FRESCO version used in this paper is improved by the addition of single Rayleigh scattering. The other references are given to indicate that the problem can be tackled with a different approach, like using PMD data. We have clarified this in the revised paper.

Page 6, lines 10-19: This section is difficult to understand and should be rephrased. For example, how is the cloud albedo value estimated "properly"? What happens if the assumption of the cloud albedo value does not match the albedo of the real cloud? Are there calculations to determine the sensitivity of the algorithm to the initial choice of the albedo value? What does "effective" mean in "effective cloud index"?

A: The question refers to page 877, line 26- page 878, line 5 in the AMTD paper.

We have moved the discussion about the cloud albedo to Sect. 2.1.1 and 2.1.2. After the FRESCO cloud retrieval algorithm is described, the cloud albedo assumption should be easier to understand. This section is removed.

Because the large pixel size of SCIAMACHY (30x60 km2), most of the pixels are partly cloudy and it is not possible to retrieve cloud albedo and cloud fraction from O2 A band alone for these cases. This means that either the cloud fraction or the cloud albedo (or

optical thickness) has to be assumed. In the FRESCO algorithm we assume that the cloud albedo has a fixed value, mostly 0.8, therefore the cloud fraction retrieved with the fixed cloud albedo is an effective cloud fraction.

We chose to assume the cloud albedo to be 0.8 (i.e. a high value), since this leads to the smallest error in trace gas retrievals for cloudy pixels, as shown e.g. by Stammes et al. (2008). The cloud albedo of 0.8 corresponds to optically thick clouds, which is usually brighter than the real cloud. Therefore, the effective cloud fraction is usually lower than the geometric cloud fraction. If we would assume a higher (lower) cloud albedo, the effective cloud fraction would be smaller (larger); see Fig. 2.

Probably the referee means 'effective cloud fraction', because the 'effective cloud index' is not appeared in the paper.

Effective cloud fraction of a pixel means the cloud fraction for a Lambertian cloud with a fixed cloud albedo (of 0.8) which yields the same reflectance at the top of the atmosphere (TOA) as the real clouds in the pixel.

Page 10, Equation (4): Change "0.2" to "-0.2". Also, albedo can only be between 0 and 1. If the "cloud index" is interpreted as albedo, it should also range between 0 and 1. Eq. (2) confirms this. (When it is completely cloudy, R equals R_max, and n becomes one. In turn, if the scene is cloud free R equals R_min and n comes 0.) How can n become smaller than -0.2 (Eq. (4) after correction) or larger than 1.1 (Eq. (7))? I understand that surface irradiance under scattered clouds can become larger than the clear-sky irradiance, but I don't understand how this observation is considered in the calculation of n (Eq. (2)) or c (Eq. (3)). Also, how can cloud enhancement (i.e. k>1) be interpreted in the context of the independent pixel approximation where the contributions of the clear and cloudy fractions of the pixel are treated independently and combined in a linear fashion?

A: The question refers to page 881 in the AMTD paper.

We already changed ‘0.2’ to ‘-0.2’ in the AMTD online paper.

We agree with the referee that albedo can only be larger than 0. However, the Heliosat formula has some adjustments to be able to process all the measurements. Rmin is usually determined from all the measurements in a month for every location (pixel). If the actual R is smaller than Rmin, then the cloud index is negative.

The reflectance (R) at TOA can be larger than 1.1 for scattered cloudy case, especially at large solar zenith angle and line of sight because the light path is very long. In the calculation of cloud index and effective cloud fraction, the real measurements of R include these cases (R > 1.1), therefore, the cloud index and effective cloud fraction can be larger than 1.1.

In principle the cloud enhancement cannot be interpreted in the independent pixel approximation. In the Heliosat method, the cloud enhancement could be taken into account with a small negative cloud index, which leads to k>1. In the FRESCO effective cloud fractions, we do not have negative values; therefore the cloud enhancement cannot be interpreted.

We have removed the negative cloud index in the equations, because the FRESCO effective cloud fraction cannot be negative.

Page 12, lines 3-5: Please make clear that the choice of input data for water vapor column density, aerosol optical thickness, single scattering albedo, and broadband surface albedo is discussed in Section 2.4.

A: The question refers to page 882, line 22-24 in the AMTD paper.

Thanks for the suggestion. We have referred to Section 2.4 for the choice of input data.

Page 14, line 15: It is stated that all climatological databases are based on monthly mean data. Does that mean that the same dataset is for applied every year? For example, is the ancillary dataset for the month of July applied to Sciamachy July data regardless of the year, or does the ancillary dataset change from year to year? Also,

how sensitive are the retrieved surface irradiance values to variations of the actual values from the monthly mean? It might be interesting to calculate one month of data using daily averages and compare to results obtained with the monthly average.

A: The question refers to page 884, line 19 in the AMTD paper.

The same databases are used for every year. The ancillary datasets for the month of July applied to SCIAMACHY July data regardless of the year.

The sensitivities of the retrieved surface irradiance values to the variations of water vapor column amount, aerosol optical thickness and surface albedo have been analyzed by Mueller et al. (2009) and Wang et al. (2011). The surface solar irradiance shows a weak dependency on surface albedo for clear-sky cases (Mueller et al., 2009), e.g. a variation of the surface albedo of 50 % relative to a 0.2 reference value leads only to a variation of 1% in “clear sky” solar surface irradiance. The sensitivity of SSI to water vapor can be estimated from Fig. 3 in Mueller et al. (2009). For example, if the water vapor column in the climatological database is 15 mm, but the actual water vapor column is 10 or 20 mm, the variation of SSI values is within +20 and -15 W/m2. The effect of aerosol optical thickness (AOT) on the SSI is briefly discussed in Wang et al. (2011). For clear-sky cases, changes in AOT of +/-0.02 lead to changes in global irradiance of about -/+5.2 W/m2 at cosine of solar zenith angle of 0.42. For fully cloudy cases (cloud optical thickness of 20.1), AOT changes of +/-0.1 cause changes in SSI of -/+ 2.2 W/m2. The sensitivities are added in the paper (see Sect. 2.3).

We agree with the referee that it is interesting to derive SSI using daily water vapor, aerosol data and compared with the SSI values that used the monthly mean databases. However, we do not have daily global aerosol data yet. We plan to use the simultaneous measured SCIAMACHY water vapor column data in the next version of the SSI product.

Wang, P., W. H. Knap, and P. Stammes (2011), Cloudy sky shortwave radiative closure for a Baseline Surface Radiation Network site, J. Geophys. Res., 116, D08202, doi:10.1029/2010JD015141.

Page 16, line 21 (". . .because the FRESCO effective cloud fraction is not accurate over bright surfaces in deserts due to the surface albedo problem . . ."): That basically means that the FRESCO algorithm cannot be trusted over desert areas, such as the Sahara. If so, this should be mentioned in the Conclusions.

A: The question refers to page 886, line 20 in the AMTD paper.

We have mentioned this in the conclusion of the revised paper. The SCIAMACHY SSI product over desert regions may not reliable, because of the coarse spatial resolution of the GOME surface albedo database. The FRESCO version 6 with the MERIS surface albedo database has been reprocessed and available on the TEMIS website since March 2011. The SSI product in the FRESCO v6 data may be improved over desert areas.

Figure 7 and Figure 9: The color scales are linear, not logarithmic.

A: We have corrected it. The number density of data is in a logarithmic scale.

Table 1: What are the values in the second column (BSRN W/m2)? Is it the average of all measurements that were used for Figures 5 and 6?

A: Yes, the BSRN data in Table 2 is the average of all the BSRN measurements used in Figs. 5 and 6. We have explained this in the revised paper.

Page 27, line 14: Why would low temporal resolution cause a systematic negative bias?

A: The question refers to page 894, line 24 in the AMTD paper.

This sentence is removed. We do not know the reasons for the systematic negative bias yet.

Technical comments:

Page 5, line 15: Define LT (i.e., local time)

A: The question refers to page 877, line 11 in the AMTD paper.

We have corrected it.

Page 9, line 10: Change "Define.." to "By defining. . ."

A: The question refers to page 880, line 14 in the AMTD paper.

We have corrected it.

Page 11, lines 17-19: Delete "the" in "the correction formulas and parameterizations" as these formulas and parameterizations have not been introduced yet.

A: The question refers to page 882, line 16 in the AMTD paper.

We have corrected it.

Page 25, line 5: Change "required detailed cloud parameters as input, which is completely different as the Heliosat method." to "that requires detailed cloud parameters as input, which is completely different from the Heliosat method."

A: The question refers to page 893, line 1 in the AMTD paper.

We have corrected it.

Interactive comment on Atmos. Meas. Tech. Discuss., 4, 873, 2011.