Atmos. Meas. Tech. Discuss.,
doi:10.5194/amt-2017-440-RC2, 2018

Atmospheric Measurement Techniques Discussions
Open Access

Interactive comment on "Evaluation of Himawari-8 surface downwelling solar radiation by SKYNET observations" *by* Alessandro Damiani et al.

Anonymous Referee #2

Received and published: 26 January 2018

Review of the manuscript "Evaluation of Himawari-8 surface downwelling solar radiation by SKYNET observations" by Damiani et al., 2018.

Summary: The manuscript describes the validation of the solar irradiance dataset from the Himawari-8 satellite by surface based observations using the SKYNET and JMA networks in Japan for the whole year 2016. The solar irradiance dataproduct from Himawari-8 is obtained from the EXAM algorithm. The comparison is performed between surface point observations and satellite derived solar irradiance data ed for different spatial and temporal resolutions. Several influencing parameters such as aerosol optical depth and surface albedo is investigated. The largest part of the paper discusses the quality of the solar irradiance product with respect to varying cloudiness, which is responsible for the largest variation in solar irradiance. The overall objective

Printer-friendly version

Discussion paper

is to validate solar irradiance retrievals at high temporal and spatial resolution in order to use these datasets in near realtime for informing photovoltaic installations of the expected solar power because rapid transients can be harmful to PV plants, as stated in the manuscript at page 3, line 3.

Comments: The manuscript is well written and has a good structure. The results and conclusions are discussed and presented quite objectively so that the reader can make his own opinion as to the quality of the comparison. The validation of the dataset over only one year of measurements is rather short and might not allow to draw conclusions as to the long-term performance of the satellite-derived solar irradiance product. Nevertheless the results are encouraging and worth to be published as they provide a very good view of the current state-of-the-art in retrieving surface solar radiation from an assimilation of surface based and space-based data on an unprecedented time and spatial resolution.

In contrast to the authors I conclude from this study that the solar radiation product as presented here is far from reaching the objectives stated in the introduction and which seem to be the rationale for the satellite itself. In general I have the impression that the authors have drawn very optimistic conclusions from their dataset. I suggest modifying the abstract and conclusions to provie a more objective assessment of the satellite solar irradiance product, especially in the frame of the overall objective of using the high temporal solar data for PV applications.

- The all-sky solar radiation data shows large deviations with respect to the surface based datasets, especially at the high temporal resolution of 2.5 minutes. This is very well seen in Figure 6, where deviations between the SKYNET pyranometer and Himawari-8 are strongly dependent on cloudiness (Clear Sky index is used here) with deviations which can exceed a factor of 3! With regard to the stated objective to use these high temporal resolution data for near realtime forecast for PV applications, especially under fast varying gradients induced by changing clouds, I would like to have this aspect more clearly addressed in the discussion and conclusion section of the

Printer-friendly version

Discussion paper

manuscript. Specific comments:

-Page 5, line 25: The CSI is also a function of the cloud optical depth, not only cloud fraction. A fully overcast sky with cirrus clouds might have a CSI close to 1. Does cloud type have a significant influence on the dataproduct?

-page 6, line 10. I assume this should be irradiance, instead of radiance?

- page 7, lines 13-16 and Figure 3, left inset: I disagree with the statement made by the authors that the data supports the assumption that a larger RMSE is correlated with cloud fraction. The left inset to Figure 3 is made up of two distinct dataclouds, 6 at high cloud fraction, of which 4 are outliers, and only the two leftmost which confirm the authors's assumption. If these 6 datapoints at high cloud fraction are neglected, then the remaining datapoints show no correlation at all between RMSE and cloud fraction. The linear fit is too suggestive and not representative for the dataset.

- page 7, discussion on surface albedo. I have understood that the surface albedo is retrieved from the satellite data itself, and that problems occur when clouds and albedo are misclassified. My question: Is the surface albedo retrieved independently for every time slot, e.g. every 2.5 minutes, or does the algorithm use the fact that while clouds can very extremely fast, surface albedo will be slowly varying, on the scale of days or more?

-page 8, line 17-18. It would the interesting to know more about the dependency between RE and SZA and add some quantitative values.

-page 11 and Figure 9. In my opinion the solar radiation forcing with respect to AOD should depend to some extent on airmass, e.g. SZA, since the attenuation of solar radiation is mainly from the direct beam radiation and thus follows the Beer-Lambert law.

- Conclusion, page 13, line 14. I do not see how this statement is an outcome of this paper, since the only comparison to an other satellite product was Himawari-7.

Figures: Figure 2: The resolution is not very good and the values shown in the subsets are not easy to read.

Figure 3: As mentioned in my previous comment, the suggestive linear fit between RMSE and cloud fraction is not convincing, since it depends on the two outlier points at high RMSE and high cloud fraction. Another view could be that there the top six points are outliers, and the remaining points show no clear correlation between RMSE and cloud fraction.

Figure 6: The text in the four sub figures is very difficult to read, possibly for lack f resolution, and the chice of colors. Especially the yellow text is unreadable. The variables need to be defined in the caption.

Figure 9: as mentioned previously, I expect the aerosol radiative forcing to also depend on airmass. Is it possible to add this information, or mention it in the accompanying text?

Figure 10: I suggest to plot the right (red) AOD axis in reverse, to show even more clearly the anti-correlation between AOD and residuals. In the caption, the error bars need to be better explained. Does it for example represent the standard deviation?

Table 2: I assume that the information given in table 2 should be consistent with the information given in Figure 6? In that case, it would be better to harmonise this information, and give the slope in the same units, not the inverse.

Interactive comment on Atmos. Meas. Tech. Discuss., doi:10.5194/amt-2017-440, 2018.