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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Long-term variability of solar irradiance and its implications for photovoltaic power in West Africa" *by* Ina Neher et al.

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The authors would like to thank the reviewer for comments and suggestions to improve the submitted manuscript. Below, all revision points are addressed and resulting text edits are included in the following way:

- *Reviewer's points are repeated cursive.*
- Answers to the reviewer's points are given.
- "New text included to the manuscript is given in quotation marks."

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Discussion paper

General comments

The difference between the surface and SARAH estimates of GHI are rather large at two of the sites. Assuming that these errors are representative of the uncertainty in the SARAH product across the region, how does this impact on the subsequent analysis of spatial/temporal variability and trends (the errors are comparable to the scale of much of the spatial and temporal variabilities presented in section 5 and much larger than the total trend estimates). The section concludes "the evaluation shows that the SARAH-2.1 data record can be used to get a reasonable overview on the irradiance variability and trends to estimate the PV potential in West Africa". What magnitude errors would mean that the SARAH data record isn't suitable?

The lack in data availability over the entire region (less than 20% of the time period of satellite data and only three sites) makes it difficult to generally validate the satellite product. In general, we find a high correlation between SARAH-2.1 estimates and GHI observations at all sites. The RMSE and MAE are given as non bias-corrected values. The bias, which dominates the RMSE and MAE, lies in the range of the uncertainties of ground based measurements (2% for Banizoumbou and Djougou and 10% for Agoufou) in Banizoumbou and Agoufou. At Djougou we find an offset of around 12%, which was mentioned and discussed in the manuscript and has been reported in other studies (Hannak et al. 2017). The offset in this region is known and would even strengthen our results, as an overestimated GHI in southern West Africa increases the actual north-south gradient of surface irradiance. Given the high correlation and a total uncertainty being lower than the variability of solar irradiance in the region, we judge the satellite data being reasonable enough to show general differences in PV yields over the entire region. We expanded the discussion in Section 4 (line 240), as it now reads:

"Given the good correlation and the fact that the uncertainty is dominated by the bias the evaluation supports the suitability of the data set to investigate the variability of solar irradiance. Thus, the SARAH-2.1 data record can be used to get an overview

on the irradiance variability and trends to estimate the PV potential in West Africa. However, especially in southern West Africa the systematical overestimation of solar irradiance in the SARAH-2.1 data set (Kniffka et al. 2019, Hannak et al. 2017) need to be considered in the conclusions of the variability and trend analysis. As a consequence of the positive offset in southern West Africa, the actual north-south gradient in the satellite data set is underestimated. In particular, for the trend analysis the systematic offset would not have an impact. Overall, an expansion of measurements over longer time periods (the measured data is available for less than 20% of the time period at only three sites) could increase the significance of our validation.".

On a related note, I would be interested to see how the errors impact on the photovoltaic power yield estimates. How different would the estimates at the three surface sites be if you used the surface measured GHI rather than the satellite GHI as input?
As we used a linear PV yield model, the uncertainty in GHI would propagate linearly. We include a sentence into the revised manuscript at the beginning of Section 6:
"As we used a linear approach, the uncertainty of satellite data would propagate linearly for PV yield estimates."
As the explicit PV yield model we used for the development of the simple PV yield model needs DNI besides GHI as an input, we could only model PV yields by using the measured GHI with the simplified model. For illustration, we include the points calculated with the simple model and measured GHI at the three location in the last figure (Figure 11 in revised manuscript, attached here as Figure 1). Furthermore, we include a short discussion on the results after the figure, reading as:
"In general, the overestimation of satellite data in Agoufou and Djougou as well as the slight underestimation in Banizoumbou (see Section 4) can be seen in the PV yields calculated with the simple model and using the measured GHI as an input (stars in Figure 11 a). In Agoufou, the PV yields, calculated with the linear model, are similar to the explicitly calculated PV yield. In Banizoumbou the results are higher and in Djougou they are lower compared to the PV yields calculated with satellite

data. Especially in Djougou, the irradiance decreases over the 35 years of satellite data availability. This leads to lower values in the 2000's values compared to the mean."

Full caption for Figure 1: "Mean (temporal) PV yield at each latitude, for the total year (a), population density for each latitude (b, (NASA 2020)), as well as mean PV yield at each latitude for the dry: October-April (light grey) and wet season: May-September (dark gray) (c), in the longitude range between 4°W and 4°E. The single points mark the temporal mean PV yield calculated with the explicit model and measured ambient temperature (star) as well as the PV yield calculated with the simple model and measured GHI (cross) at the three sites, Agoufou (2005-2008), Banizoumbou (2005-2012) and Djougou (2002-2009). The gray background box in (c) marks the latitude range, where the definition of seasons is most accurate."

I would consider changing the structure of the paper so that the description of the methodologies to calculate photovoltaic power yield (i.e section 3) is placed after the results for GHI and immediately before the presentation of the results for the photovoltaic power yield.
For the consistence of the story line, we decided to better describe the methodology first and the results thereafter. Furthermore, some parts of the structure were changed according to other reviewers comments.

Specific comments

I'm not convinced the surface albedo shown in Fig 1(a) is particularly relevant for this study as it has no impact on the GHI. I'd suggest using a different image instead. Perhaps a snapshot visible image from SEVIRI?
When GHI is retrieved from satellite reflectance the albedo is an important input

parameter. In Figure 1 of the manuscript we show (besides the topography) the input data for the SARAH-2.1 data retrieval. In the revised manuscript we changed the order of the figures, to the topography being a). Furthermore, we added a phrase to the text, that the other data is used as input for the SARAH-2.1 data retrieval. For completeness we kept the albedo figure, as the albedo impacts the diffuse part of GHI.

The paragraph comparing MVIRI and SEVIRI (L100) seems incomplete. Which channels are used for SARAH? Which channels are on SEVIRI? When does SARAH use MVIRI and when SEVIRI?
We tried to be clearer in our formulation and changed the paragraph to as it now reads: "For the generation of the SARAH-2.1 data record the visible channel (0.5 -0.9 um) of the METEOSAT Visible and Infrared Imager (MVIRI) is used until 2005 and the two visible channels (0.6 and 0.8 um) of the Spinning Enhanced Visible and Infrared Imager (SEVIRI) afterward. A detailed description of the retrieval is given in (Mueller 2015) and references within."

Please can you specify what the MAE quoted for SARAH (L108) is measured against? Is this compared to surface-based observations? If so where and when?
The SARAH data was compared to ground-based measurements at 15 BSRN stations. The earliest measurements started in the mid 1990's. We specified the measurement to which the SARAH data was compared in the sentence, so that it now reads:
"A mean absolute error (MAE, in comparison to 15 BSRN stations between 1994 and 2017) of 5.5 W/m^2 and 11.7 W/m^2 for monthly and daily GHI is reached, respectively (Pfeifroth 2019)."

I would move the text on ERA5 data (L117-120) to the following paragraph.
Thank you for your suggestion, we moved the description of the ERA5 data to the section on PV yield estimations (Section 3).

If I understand correctly, "b" in equation (5) represents the power required by the inverter, which is a function of temperature. Yet in table 3, for T>35 it has a positive value, which implies the inverter is generating power? Can you comment on this?
The parameter "b" describes the additional impact of the inverter for PV power estimations. The inverter needs a certain amount of solar irradiance to convert the direct current to alternating current. But you are right, actually it should not have a positive value for physical reasons. This effect occurs because at high temperatures, irradiance is comparably high. We corrected the parameter b for T>35°C to zero and included a description in the revised manuscript. Furthermore, we repeated our calculations with the new parameters.
(Line 192 in revised manuscript:) "The slope decreases at increasing temperatures, while the intercept shows a complementary behavior. However, for T>35°C the parameter b was set to zero, as for physical reasons it can not be positive."

Why are some of the points in Figure 3 grey? Are these points where Delta AOD is negative?
If there was no AOD measurements from AERONET available, we did not mark the points in color. We included a remark on this in the caption of the figure, as the full caption now reads as:
"Comparison of simulated and observed GHI as daily (left) and monthly (right) averages at three sites over the given timely horizon, a) Agoufou (2005-2008), b) Banizoumbou (2005-2012) and c) Djougou (2002-2009). The difference between the measured AOD and the climatological AOD for the satellite data retrieval (ΔAOD) is indicated as color. If no measured AOD is available, the points are grey."

For the trend analysis, can you take the uncertainty in the SARAH measurements into account in your estimates of the significance of the trend?

We tried to include the uncertainties into the discussion of the results for the trend analysis and changed the manuscript accordingly.
"Compared to the uncertainties of the satellite data (MAE up to 27.6 W/m^2, see Section 4), the trends might seem negligible. However, the reported uncertainties are not bias corrected and represent, in particular in the case of Djougou, the systematic overestimation of the GHI by the satellite estimate. The estimation of the temporal trend is unaffected by any systematic over- or underestimation and, hence, still can be derived with certain confidence."

Technical corrections were included into the manuscript.

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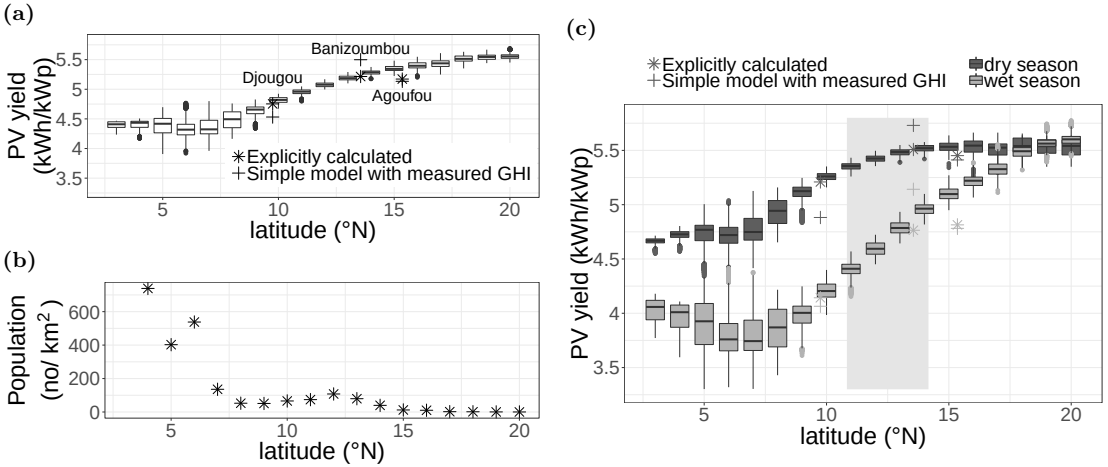

Fig. 1. Mean (temporal) PV yield at each latitude, for the total year (a), population density for each latitude (b, (NASA 2020)), as well as mean PV yield at each latitude for the dry and wet season (c).

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