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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.

**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

Changed figures are all included at the end of this document.

*Title: I wonder if the long-term variability is the most important output from this paper. Isn't it rather the validated use of satellite data over the region, and the yield-latitude plots in Fig. 10? The authors may, if they agree, reconsider the suitability of the title for the paper.*
We changed the title to:
"Photovoltaic power potential in West Africa using long-term satellite data"

*Line 1: "Long-term changes" -> do the authors mean historical, or future, or both?*
We are referring to historical changes and adjusted the beginning of the sentence such that it now reads:
"This paper addresses long-term historical changes in solar irradiance [. . .]"

*Line 2: "Here we use satellite irradiance" -> and temperature from reanalysis, right?*
Yes, the sentence has been adjust in the revised version of the manuscript so that it now reads:
"Here we use satellite irradiance (Surface Solar Radiation Data Set-Heliosat, Edition 2.1, SARAH-2.1) and temperature data from a reanalysis (ERA-5) to derive photovoltaic yields."

*Line 22: "located close to the equator, (. . .)" -> yes, but in reality, it's the locations furthest from the equator that have the highest PV potential in West Africa, as your research shows.*
We started the sentence with "West Africa" and left out the addition of where it is located.

*Line 24: "PV power system" -> this wording occurs at several instances in the paper. What exactly do the authors mean with it? Is it a power system where a certain share of power generation is from solar PV? Or solely based on PV without any other power generation sources? Is there a quantitative definition for it?*
A PV power system is a power system solely based on photovoltaic power. PV system might be a better wording for this kind of power system and is used in the new version of the manuscript.

*Line 35: "no assessment over total West Africa (. . .)" -> what is meant with "assessment"? Do the authors mean a validation of satellite data? Since this is one of the core pieces of this study, I would recommend the authors to be a lot clearer about the added value of their research here compared to the "no assessment" state-of-play.*
We changed the sentence so that it now reads:
"However, a detailed validation of the full 35 year SARAH-2.1 data set has not been performed so far for total West Africa."

*Line 42-44: "However, they need (. . .) certain assumptions." This sentence confuses me – how does it relate to the problem the authors are trying to solve? I thought the focus was long-term changes, but here it sounds as if hourly resolution is the most important problem to be solved by such research.*
The problem we tried to describe here, was that this high resolved data is often not available and that we need other solutions. Therefore, we changed this part so that it now reads:
"However, they need explicit input data in a high temporal resolution which is often not available. Therefore, a simplified model for PV yield estimations based on daily data is developed and applied here."

*Line 45: The authors do not really explain here why analysing the long-term changes*

*in West Africa is so important. Is there literature explaining why this is crucial, in particular for solar PV, either for West Africa or for other regions worldwide? Especially as compared to the variability on diurnal and seasonal timescales?*
It is important to analyze long-term data before planning and constructing a solar power plant to project the potential outcome, select the location and optimize the dimension of the power plant. We tried to make the motivation clearer in the second paragraph of the introduction (line 24-30 in the revised manuscript) and included the following text there:
"Therewith the development of a PV system is worthwhile. Before investing in a PV system three points need to be considered, using differently resolved global horizontal irradiance (GHI, the sum of direct (DIR) and diffuse horizontal irradiance (DHI)). First, to select a profitable location high spatially resolved GHI is needed. Second, to estimate the profitability and risks of the plant long-term variability and trends of historical GHI can be analyzed as a basis to project future system performance. And third, to optimize the plant high temporally resolved GHI can be used for the dimension of the plant size and storage system as well as for the maintenance. However, ground-based measurements of irradiance are not available continuously over long-term time scales and cover only a few discrete locations in the region."

*Line 59-60: I have some trouble with the definition of dry and wet season that the authors employ here – the definition seems rather generic for a region spanning a large latitude range. For example, the rainy season does not start in the same month in every country; moreover, the very south of the region (say, the coastal regions of Côte d'Ivoire, Ghana, etc.) have two distinct seasonal rain peaks, typically in June and September, with a drier lull inbetween as the ITCZ moves south -> north -> south again. Thus, speaking of "the rainy season" as if it were the same thing across the region, and basing a large part of the analysis thereon, belies the climatological differences between the West African countries/regions. This also affects the results of eg Fig 10, which changes depending on the precise definition (generic vs country-*

*specific) of a "rainy season". I'm not saying the authors should necessarily change their analysis, but at the very least a justification for their choices is in order.*
We tried to describe the difference of seasons over the entire region and why we used one single definition, when introducing the seasons:
"West Africa is a region with a pronounced dry and wet season. In large parts of West Africa one wet season occurs during the summer month. However, the length of the wet season decreases with rising latitude and along the coastal region, two wet seasons occur (typically in June/July and September). Nevertheless, here we use one single definition of seasons according to (Mohr 2004) assuming one dry season: October - April and one wet season: Mai - September. To reinforce our results we performed the analysis with a sharper definition of seasons (dry: November - March and wet: June - August) and found similar results."

*Line 67: The authors mention the mountainous areas in Nigeria, but what about the Guinée highlands where peaks >1000m are also found?*
We included all higher elevations over the entire region into the sentence so that it now reads:
"Only a few exceptions are the Mount Cameroon on the south-east of the study area along the border of Nigeria and Cameroon, Fouta Djallon and the Guinea Highlands in Guinea, Jos Plateau in the center of Nigeria and the Air Mountains in northern Niger."

*Section 2.2: I am wondering why the authors don't start with this section. After all, the satellite data are the main source for this study, with the ground-based data serving as validation material. It feels the other way around when reading this chapter, as if the ground-based data are accorded primary importance.*
We changed the order of sections (first Satellite-based data, second Ground-based data).

*Line 118: "monthly mean temperature" -> why not hourly? ERA5 has much higher resolution than monthly. Is the day-night temperature effect not important for solar PV yield? Also, the authors may want to cite the paper on ERA5: https://rmets.onlinelibrary.wiley.com/doi/10.1002/qj.3803*
To provide the PV yield map shown in Fig. 10 we used daily satellite data to calculate daily PV yields. Therefore, we included daily temperatures into our model. However, for use cases, where a higher temporal resolution is required, hourly irradiance and temperature data would be appropriate. Furthermore, we included the reference for ERA5.

*Line 119: Here, I believe a flow chart would be highly useful, showing the different data and modelling efforts, their characteristics, and how they feed in to the different calculations. This would include at least (i) the GHI-PV model, (ii) the validation approach for satellite data, (iii) the ERA5 data, (iv) the results (parameters), and (v) arrows indicating what feeds into what and how. This will make the paper much clearer to read and allow the reader to follow the author's train of thoughts.*
We included a flow chart, connecting all calculation steps and needed input data and adjusted the paragraph accordingly:
"Our ultimate goal is to describe the PV potential over the entire region for a standardized PV power plant. For this purpose, a simplified linear regression is fitted on the basis of the three reference sites where the necessary information is available. Furthermore, the uncertainties concerning cell temperature are estimated (see Section 3.2) and the used GHI (from SARAH-2.1 data set) is validated (see Section 4). The single calculation steps, including all necessary input data is shown in Figure 1."

*Line 124: "temperature levels" -> this is explained later, but at this point in the text it's not clear what is meant with this.*
We deleted the sentence here and the information about the source of the temperature (from ERA5) is included later.

*Line 206: "assumed climatological AOD" -> and that assumption is what, and comes from where?*
Here, we mean the climatological AOD used for the SARAH data retrieval (see Figure 1d). We included the reference to the Figure when we first mention the climatological AOD in this paragraph:
"To study whether deviations from the climatological AOD used in SARAH-2.1 (see Figure 1 d) might explain the deviation we investigate the impact of the difference between the measured AOD and the climatological AOD for the [. . .]"

*Line 248: "the wet season is actually longer in southern West Africa" -> and it is also bimodal in many places; see above comment. This is not mentioned at all in the paper.*
See answer to your comment above.

*Figure 4, 5, 6, 9: Here, I believe that the authors have placed the "Lagos" location in the wrong spot. Lagos is in south-western Nigeria, not in southern Togo.*
You are right, thanks for this comment. We corrected the location in all Figures.

*Figure 4: I think the figure may look better if the authors used a land-sea mask. The bright colours and patterns appearing on the ocean surface are not relevant for solar PV assessments.*
We included a land-sea mask to all image plots, as only the land areas are important for solar power generation.

*Line 269: Here, the authors suddenly talk about "summer months" instead of dry/wet season (but see previous comments). How are summer months defined? (I guess they refer to European summer. Is this a suitable comparison?)*

We changed the term ”summer month” to ”wet season” so that it now reads:
”[. . .] occurring during the wet season.”

*Section 5.2 and 5.3: I think this order of sections is strange. I would start first with time series analysis at four locations (because this validates the use of long-term satellite data) and then explain the trend analysis afterwards. This doesn't need to be two different sections, they can be merged into one. Then, section 5.1 could be ”spatial analysis” and section 5.2 ”temporal analysis”, or so.*
We changed the order and named the sections according to your suggestions in the revised manuscript.

*Figure 6: I find the blue/red colour scheme of the ”significance” figures confusing, given the similarity to the GHI graphs where the colours represent physical values instead of a binary variable.*
Figure 9 in the revised manuscript (here Figure 2): We included the information about the significance in the trend plots, so that only the significant trends are pictured in three maps (see Figure 2). This has the additional effect of reducing the size of the document.
The full caption of this figure now reads:
”Linear trend for global irradiance of the annual mean (a), as well as the dry (b) and the wet season (c), each for all significant cases. Ouagadougou, Burkina Faso and Dakar, Senegal are additionally visualized here, as values at these locations are compared within this section.”

*Figure 7 and 8: In the caption, the authors should explain what type of data is analysed here: satellite or ground-based.*
We included the information on the data source in the caption.

*Figure 10: If the authors keep their current definition of dry and wet season, perhaps it would be good to include here a vertical line showing the latitude at which, typically, the used definition (dry: October-April, wet: May-September) is the most accurate? Or else, the authors could simply replace "dry season" and "wet season" by "October-April" and "May-September" in the legend, which makes the graph fully unambigious?*
Figure 11 in the revised manuscript (here Figure 3): We included the latitude range as a gray box in the background of the Figure, where the definition of seasons is the most accurate. The full caption of this figure now reads:
"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 stars in (a) mark the temporal mean PV yield calculated with the explicit model and measured ambient temperature 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."

*Line 385: Somewhat strange that the authors here talk only about winds without even mentioning the word "clouds".*
We restructured the sentence so that it now reads:
"This seasonality is dominated by the moist monsoon winds, going along with high cloudiness and coming from the south-west during the wet season and the dry Harmattan winds from the north-east during the dry season."

*Line 389-392: Given this discussion, which is highly relevant, why don't the authors append Figure 10 with a graph of typical population density by latitude? If such data is not available, a simple solution could be to plot cities with e.g. >500,000 inhabitants as circles (radius proportional to population size) as function of latitude. This would make the point the authors try to make much more tangible.*

We included a plot of population density for the corresponding longitude box in the Figure, here Figure 3 (Figure 11 in the revised manuscript), using data from the NASA (Gridded population density (NASA 2020)).
Furthermore, we included a sentence under the Figure to describe the plot:
"Population density shows the opposite latitudinal gradient compared to PV potential, with a higher density at low and a lower density at high latitudes (see Figure 11b)."

*Line 394: This reference does not seem to exist (yet). Can the authors check this?*
The reference was still in the review process, when we submitted this manuscript. In the meantime, the title changed and the manuscript was recently published in Nature Sustainability. We included the right reference in the list (Sterl 2020).

*Line 400: Why are storage capacities necessarily unavoidable to deal with dust storms? A dust storm lowers power plant availability during a few days. Power systems nowadays sometimes have to deal with power plants being unavailable during months, eg for maintenance, and yet we don't have massive storage capacities yet... Is it because dust storms are so unpredictable and massive that no reserve capacity could make up the difference? Can the authors substantiate this?*
Of course we do not need such high storage capacities if different power sources are used and reserve capacities can be used from other power sources if there is only few solar irradiance available. However, in the conclusion of this study, we describe a solely based solar system, where these storage capacities would be necessary, because no other power sources exist. By combining solar power with other power sources, storage capacities can be reduced drastically due to compensating possibilities. To make clear, that this statement is for a solely based power system, we included the word 'solely' in the sentence so that it now reads:
"For such events storage capacities for several days might be needed e.g. in solely solar based micro grids."

Technical corrections were included into the manuscript.

## References

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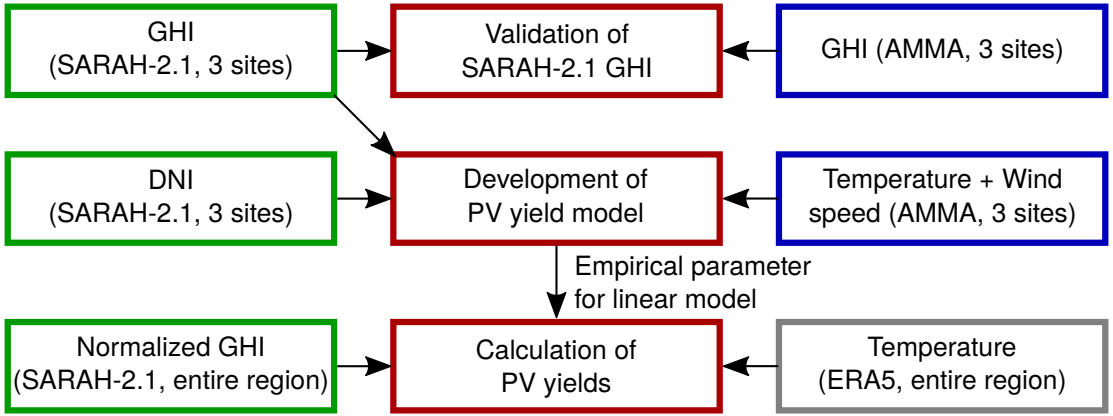


**Fig. 1.** Connection of calculation steps (red) within this study, including all needed input data (green: satellite data, gray: reanalysis data, blue: observational data).



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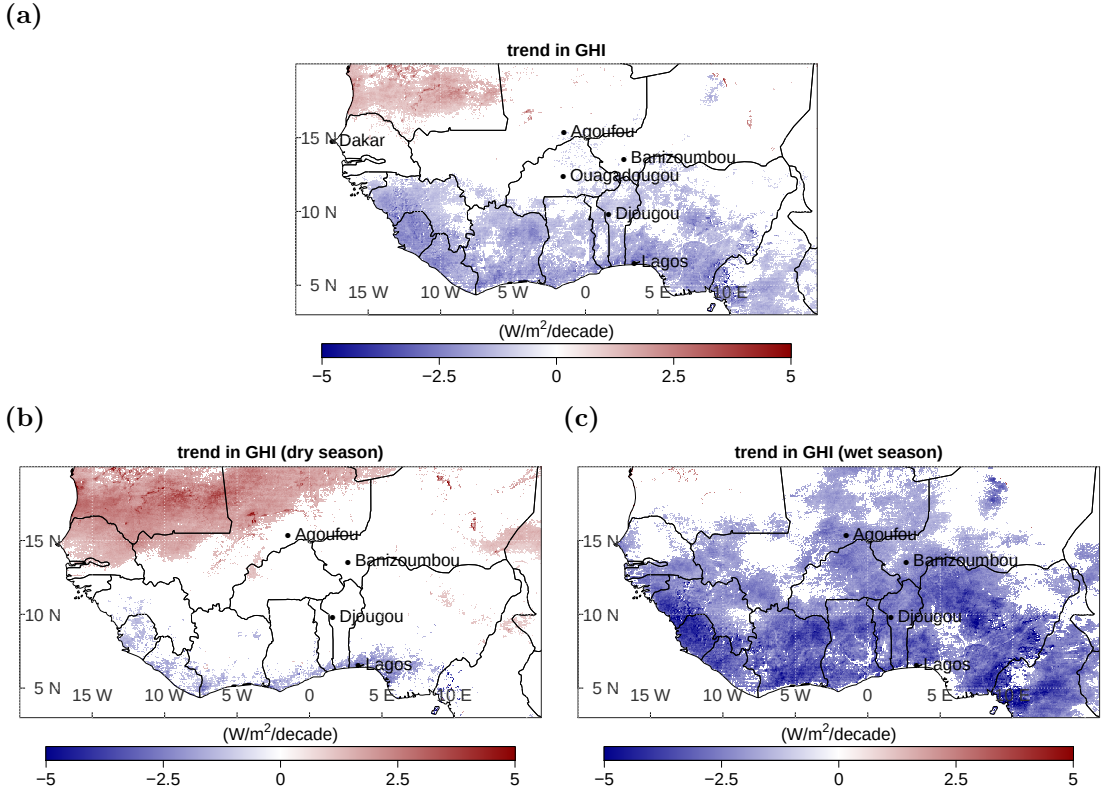


**Fig. 2.** Linear trend for global irradiance of the annual mean (a), as well as the dry (b) and the wet season (c), each for all significant cases.



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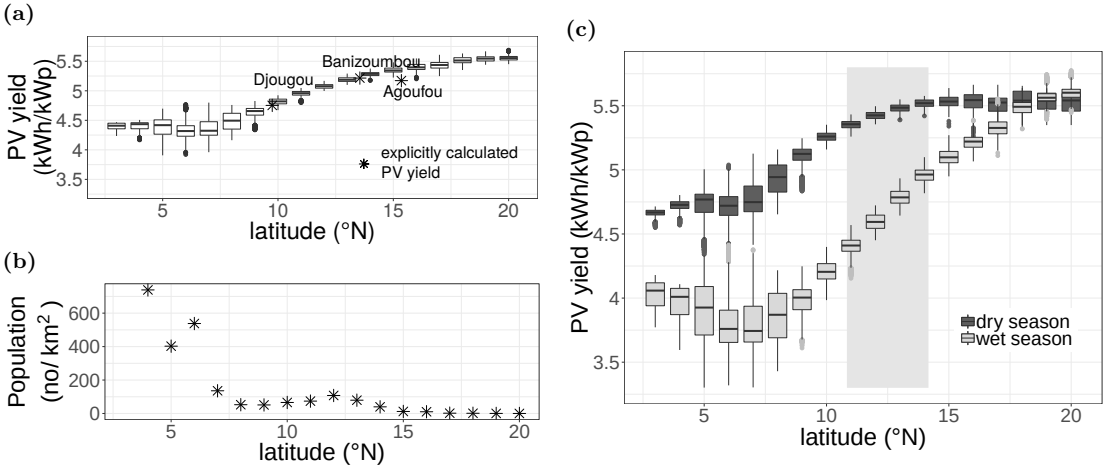


**Fig. 3.** Mean PV yield at each latitude, for the total year (a), population density for each latitude (b, (NASA 2020)), as well as mean PV yield for the dry (light grey) and wet season (dark gray) (c).



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