

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

Anonymous Referee #2

Received and published: 25 May 2020

General comments

In this paper, the authors use irradiance from satellite data to calculate solar PV yields and variabilities in West Africa and their dependence on latitude, using an empirical linear model and validating their results by comparison to ground-based irradiation data.

Overall, this is an interesting paper that can certainly provide good bases for discussion of the future of solar PV in West Africa, which is likely to be bright (apologies for the lame wordplay) as various recent literature has shown.

The manuscript is generally well-written, although at times it is unclear whether the

C1

authors are talking about observed or satellite data, and the order of the section is not always logical, at least in my eyes. However, if the authors can address this and the specific comments below, I would consider to recommend this article for publication.

Specific comments

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.

Line 1: “Long-term changes” -> do the authors mean historical, or future, or both?

Line 2: “Here we use satellite irradiance” -> and temperature from reanalysis, right?

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.

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?

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.

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.

C2

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?

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.

Line 67: The authors mention the mountainous areas in Nigeria, but what about the Guinée highlands where peaks >1000m are also found?

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.

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>

Line 119: Here, I believe a flow chart would be highly useful, showing the different data

C3

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.

Line 124: “temperature levels” -> this is explained later, but at this point in the text it's not clear what is meant with this.

Line 206: “assumed climatological AOD” -> and that assumption is what, and comes from where?

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.

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.

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.

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?)

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.

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

C4

a binary variable.

Figure 7 and 8: In the caption, the authors should explain what type of data is analysed here: satellite or ground-based.

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 unambiguous?

Line 385: Somewhat strange that the authors here talk only about winds without even mentioning the word “clouds”.

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.

Line 394: This reference does not seem to exist (yet). Can the authors check this?

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?

Technical corrections

Line 6: “The dry and the wet season (. . .)” -> suggest to delete this sentence

Line 15: Sustainable Development Goals should be capitalised

C5

Line 17: “energies” -> “energy resources”

Line 21: “the power system needs to be built up (. . .)” -> suggest to write: “power systems will need to be strongly expanded in West Africa”

Line 33: “in at half hourly” -> “at half-hourly”

Line 36: I would delete the sentence comparing irradiation to a fuel.

Line 79: “as 15 min values” -> “at a 15-minute resolution”?

Line 111: “total West Africa” -> suggest to reword, perhaps “the entire region”, “all continental ECOWAS countries”, etc.?

Line 164: suggest to keep the names of the countries within brackets

Line 231 (and elsewhere): “temporally mean” -> “temporal mean”

Line 249: “periodes” -> “periods”

Line 251: “Mai” -> “May”

Figure 4 (and later): “interquartil” -> “interquartile”

Line 298 (and elsewhere): “tendency's” -> “tendencies”

Line 376: “dedicated” -> strange choice of words, what does this mean?

Line 381: “complementary” -> “opposite”

Line 386: “expansive” -> “expensive”

Interactive comment on Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2020-306>, 2020.

C6