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Interactive comment

Interactive comment on "Multiresolution analysis of the spatiotemporal variability in global radiation observed by a dense network of 99 pyranometers during the HOPE campaign" *by* Bomidi Lakshmi Madhavan et al.

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We thank the reviewer for providing his/her valuable comments and suggestions on our article "Multiresolution analysis of the spatiotemporal variability of global radiation observed by a dense network of 99 pyranometers during the HOPE campaign" (acp-2016-694). In the process of revision, we have made the following changes in the original manuscript:

- **Title of the manuscript -** *The title reflects the content of the paper, but I would suggest two minor changes. First, I would suggest adding "solar" when "global*

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radiation" is mentioned, or changing the adjective "global" and use "solar" instead. For me, it's more relevant let the reader know that the paper is about solar radiation, which is pretty obvious but not explicitly specified right now. Second, I think that "during the HOPE campaign" can be removed from title, as it is not totally relevant (note that the authors do not mention the campaign in the abstract) and the reader does not need to know what the HOPE campaign was.

- We decided to keep the term global radiation in the title, as the AMS Glossary defines it in a consistent meaning to our usage as: Solar radiation, direct and diffuse, received from a solid angle of 2π steradians on a horizontal surface, see http://glossary.ametsoc.org/wiki/Global_radiation.

- The title of the manuscript is revised as - "Multiresolution analysis of the spatiotemporal variability in global radiation observed by a dense network of 99 pyranometers".

- **Abstract -** *The abstract provide a complete summary of methods, results, and conclusions. However, I suggest mentioning in some way in the paragraph the geographical location of the measurements. In addition, the sentence "For frequencies below 1.0 min^{-1}, variations in transmittance become completely uncorrelated already after several hundred meters" needs clarification. First, because I think it would be "above" and not "below" not to enter in contradiction with the previous sentence. Second, because I can't find this result in the text of the paper. The most similar thing that I can find is in section %, point ii, where 3 minutes (and not 1 minute) and 1 km (and not several hundred meters) are mentioned.*

- The information related to the geographical location of the measurements is included in the abstract.

- The revised sentence is as follows: "For frequencies above 1/3 min^{-1} and points separated by more than 1 km, variations in transmittance become completely uncorrelated."

- **General comment -** *I suggest removing from the paper the part related with the measurements at the MORDOR site, including Figure 4 and the corresponding comments. I would say that the paper will have exactly the same value if the authors remove these parts. From my point of view, including the information about MORDOR campaign is confusing: I didn't understand, when I read section 2, why a campaign that was performed two years later had something to do with the HOPE campaign. Then in section 4, I realized that data from MORDOR was needed to generate Figure 4, which is commented in as short as 8 lines of page 8. Figure 4 is intended to justify the use of global radiation, but I think that it is unnecessary. In addition, I think it is somewhat misleading. Under truly "overcast" conditions, direct radiation is null, so the power spectrum of the direct radiation under these conditions should be identical. I understand that the Figure is generated by using only one "overcast" day, which probably was overcast with very thin clouds. In fact, for clouds with optical depth as low as 5, direct beam is totally extinguished, so the day used by the authors is not, from my point of view, a good example of a typical overcast day. If, as I suggest, the authors get rid of this part, they should also eliminate part of the conclusion 5.i and a sentence from the abstract. I insist that without this part, the paper keeps being worth of publication, and in my opinion, more "round", clear, and consistent.*

- While we understand the concerns related to the lengthy description of the MORDOR measurements, the results of Fig. 4 (old manuscript) are quite relevant and important for understanding the variability of global radiation, and how the direct/diffuse contributions affect overall variability. Instead of dropping Fig. 4 (old manuscript), we would like to keep it by moving it to the very end of the results section and make clear that these results are more of an outlook to future research than final results. Hence, the text from page 8, lines 3-10 are moved to a short subsection 4.4, which also makes it clear that this is only an initial assessment and clarifies that the large direct contribution in overcast situations is likely due to our not very strict classification of situations. In particular, even on the days classified as overcast, some periods with significant direct irradiance due to cloud gaps were observed and evidently dominate the power spectrum of the global transmittance.

Minor comments and technical corrections:

- **P2, L14-16:** *I wouldn't say that the references given to support the validation of satellite retrievals are the best ones. You could mention instead (or in addition), among many others: Enriquez-Alonso et al (2016), Norris and Evan (2015) and Stubenrauch et al (2013).*

- These references are included in the revised manuscript.

- **P4, L28-29:** *The phrase "As the spectral composition of the measured global radiation in the field deviates due to non-uniform spectral sensitivity" is unclear to me, as it mixes what happens in the field with the instrument sensitivity. Or maybe the problem is that I don't understand from what the measured radiation "deviates"?*

- The revised statement is as follows: "Changes in the spectral distribution of downward irradiance compared to the conditions during calibration can lead to errors of up to 5%, particularly at higher solar zenith angles."

- **Eq. (1), Eq (2):** *If the right-hand side depends on index J, the left-hand side var(T) should also come with this subindex, shouldn't it?*

- The equations (1) and (2) are corrected.

- **P7, L11 (and elsewhere):** *Why it is relevant that the second considered domain is 3.163 km? Shouldn't 3.2 km be accurate enough? Why do you use square domains instead of circular areas of a given radius from the central point?*

- To maintain consistency with satellite pixel representation, we have defined the square domains.

- The second considered domain is corrected as 3.2 $\times$ 3.2 km^2 instead of 3.163 $\times$ 3.163 km^2.

- **P7, L12-13:** *The sentence is not clear. Maybe you could use some other equation to explain what are you doing?*

- Reference to the equations from the Appendix A are included in the statements.

- The revisions are done as follows: "... and α_A (from Eq. A11) is a linear reduction factor relating the variance of the point measurement (from Eq. A2) to the variance of an area-averaged time series (from Eq. A8). The explained variance (i.e., $\gamma^2_{S,J}$ and $\gamma^2_{D,J}$ from Eq. A10) between the point and area-averaged values are obtained separately for transmittance smooths (S_J) and details (D_J) for the different spatial and temporal scales. Then, the expected deviation δ_J for each wavelet detail is calculated based on the explained variance and summed to yield an estimate of the total variance, accounting for a reduced temporal variability of the spatially-averaged transmittance by the reduction factor."

- **P8, L12-13:** *"..., obtained as average power spectrum obtained from all pyranometer stations". Do you mean that you first obtained a power spectrum for each station measurements, and then you averaged all these spectra?*

- Yes. For better clarity, the statement is revised as follows - "The average power spectrum is obtained by considering the power spectra of all the pyranometer stations."

- **P8, L20-23:** *At the end of the sentence, you could mention that this phenomenon is usually referred to as "enhancement effect".*

- We have included this in the revised manuscript.

- **P9, Eq. (3) and related comments:** *I understand that all this development is for "details" D3-D9, and not for "smooth" S3. But you might consider making this explicit in the text.*

- Eq. 3 is applicable to the "details" (D_3 to D_9) as well as to the "smooths" (S_3 or any other smooth with a different index). This is also shown in Figure 6. So, there is no requirement for making an explicit statement in the text.

- **P10, L28-30:** *Ok with this sentence, but it is unclear to me how do you actually compute the "area-averaged" transmittances. I assume that you use all measurements from all stations in a given domain. Could you mention how many sites were included in each domain besides the central station? I understand that you used only one 10x10 km^2 domain (since this is the size of the whole HOPE campaign domain) but did you use one of more domains of the other sizes (1 $times$ 1, 3.2 $\times$ 3.2 km^2)?*

- In this paper, we utilise the spatial auto-correlation functions determined in the previous section to calculate the power spectral density of spatial averages and the deviation of spatial averages from point measurements.Thereby, we avoid averaging of multiple stations to obtain an approximation of a spatial average but rely on the assumption that the global transmittance field within the observation domain is statistically homogeneous and isotropic, and that its auto-correlation function follows Eq. 4.

- **P13, L25:** *Besides what I already told above (regarding what it is written in the abstract) here a potential confusion is evident. You say "for higher frequencies above 3 min", but minutes is a unit of time (period) and not of frequency. Here and elsewhere in the text, you should be cautious when mixing the use of frequency and time periods. In fact, this is also relevant regarding many figures, which* *are correctly labeled in "time periods", but using a reverse axis, and in general, commented in the text in the frequency domain, I suggest that at least the first figure where time period is used, you should highlight in caption the use of the reverse axis and the relation with frequency.*

- The units of frequency and time are checked in the manuscript and corrected.

- The following statement is included in the caption of Figure 4 - "As the time period is inversely proportional to the frequency, the time periods (on the x-axis) are represented in ascending order of frequency scales. "

• **P13, L31-32:** *"As a consequence, only a small fraction of the high-frequency variability within an extended domain can be explained by a point measurement". Isn't this recorded in a point measurement can be described by area-averaged (satellite, model, reanalysis) data?*

- The statement is corrected in the revised manuscript.

• **P14, confusion iv:** *You should add to what time average correspond the value you give here (80 $W\,m^{-2}$).*

- The statement is revised as following - "This effect can reach as much as 80 $\mathrm{W\,m^{-2}}$ for a grid-box of $10\times10\,\mathrm{km}^2$ corresponding to an averaging time period of 5.25–10.5 s (D_{13}) during broken cloud conditions."

• *Could you please double-check or update the links to WMO documents? I checked both links and they didn't work for me.*

- The links are updated and working.

• **Table 5:** *I think that 4-5 significant figures for the delta-G values are not required (and in fact, do not make sense). Indeed, in the text you correctly work with 2-3 significant figures (e.g. 79 instead of 78.288).*

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- The values of δG in Table 6 (in revised manuscript) are rounded to 2 significant digits in the decimal place.

- **Caption fig 3:** *Shaded gray color bands are found in panels (a) and (d) (not b).*

- Corrected.