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Atmospheric Chemistry and Physics
Discussions

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.

Anonymous Referee #2

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This paper takes advantage of the dense network of pyranometers deployed during the HOPE campaign to characterize with high detail the spatio-temporal variability of solar radiation at several scales. Thus, the paper addresses relevant scientific matters which are within the scope of ACP. The paper uses relatively known tools (mainly wavelet analyses) to a new and comprehensive dataset. Substantial conclusions are reached, which are based on methods and assumptions that are in general clearly outlined. In my opinion, the authors give proper credit to related work and clearly indicate their own new/original contribution.

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

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 5, point ii, where 3 minutes (and not 1 minute) and 1 km (and not several hundred meters) are mentioned.

In general, the paper is well structured and clear; the language fluent and precise; mathematical formulae and symbols correctly defined and used; and tables and, particularly, figures are of great quality. I do have however one general comment and several minor comments and technical corrections to be considered.

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 very different (impossible to compute, in fact) and the diffuse and global radiation 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 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.

Minor comments and technical corrections.

- P2, l 14-16. 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), Stubenrauch et al (2013).

** Norris JR, Evan AT (2015) Empirical removal of artifacts from the ISCCP and PATMOS-x satellite cloud records. J Atmos Ocean Technol 32:691–702. doi:10.1175/JTECH-D-14-00058.1

** Stubenrauch CJ, Rossow WB, Kinne S et al (2013) Assessment of global cloud datasets from satellites: project and database initi- ated by the GEWEX Radiation Panel. Bull Am Meteorol Soc 94:1031–1049. doi:10.1175/BAMS-D-12-00117.1

**Enriquez-Alonso, A., A. Sanchez-Lorenzo, J. Calbó, J. A. González, and J. Norris, 2016: Cloud cover climatologies in the Mediterranean obtained from satellites, surface observations, reanalyses, and CMIP5 simulations: validation and future scenar-

ios. Clim. Dyn., 47, 249–269, doi:10.1007/s00382-015-2834-4.

- 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"?

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

- P7, l 11 (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?

- P7, l 17-19. The sentence is not clear. Maybe you could use some other equation to explain what are you doing here?

- P8, l 12-13. "..., 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?

- P8, l 20-23. At the end of the sentence, you could mention that this phenomenon is usually referred to as "enhancement effect".

- 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.

- P10, l 28-30. 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 where included in each domain besides the central station? I understand that you used only one 10x10 km2 domain (since this is the size of the whole HOPE campaign domain) but did you use one of more domains of the other sizes (1x1, 3.2x3.2 km2)?

- 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 3min”, 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 in the first figure where time period is used, you highlight in the caption the use of the reverse axis and the relation with frequency.

- P13, l 31-32. “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 the other way around? That is, only a small fraction of the high-frequency variability recorded in a point measurement can be described by area-averaged (satellite, model, reanalysis) data?

- P14, conclusion iv. You should add to what time average correspond the value you give here (80 W m-2)

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

- 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).

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

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