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Atmospheric Measurement Techniques Discussions

Interactive comment on "Effects of temporal averaging on short-term irradiance variability under mixed sky conditions" *by* Gerald M. Lohmann and Adam H. Monahan

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The work presented here has a major interest in the measurements of the downwelling solar irradiance at surface. The authors put emphasis on the production of electricity of PV panels but their work will reach a broader community as solar irradiance has much broader applications. The amount of work is impressive and I may highlight several points. However, I am not a reviewer and I will keep to a few points and questions.

1. The use of clear-sky index

The authors use a clear-sky index which is defined as the ratio of the irradiance G to

that resulting from a model predicting the irradiance in cloud-free conditions Gclear. Such a model is called a clear-sky model.

The authors have used the clear-sky model described in Fontoynont et al. (1988). Actually, this reference does not describe any clear-sky model and should be changed. I guess that the model used is that developed by Dumortier which is a revised version of an original model by Kasten (see Kasten and Young, 1989; Kasten 1996). This model has been criticized and modified to better account for changes in irradiance with ground elevation (see e.g. Geiger et al. 2002).

With respect to the subject of the proposed work, this model has the drawback of using Linke turbidiy factor as input which is unknown at any instant of the time series exploited by the authors. It is likely that the authors use some averaged values of the factor. Hence, Gclear is not the actual cloud-free irradiance but rather a sort of mean value. It follows that variations of k* includes variations in optical properties of the atmospheric composition in cloud-free conditions (aerosols, water vapour mostly). These variables have temporal variabilities that may partly account for the observed variability in k*. As mentioned by the authors, it is likely that variability of k* is mostly due to changing cloudy conditions.

I would not dare to suggest the use of a more advanced clear-sky model such as the McClear model (Lefèvre et al. 2013) that has been validated at several occasions (Lefevre et al. 2013; Eissa et al. 2015; Lefevre et al. 2016). Inputs to McClear are atmospheric properties that are derived from the CAMS chemistry-transport model and the uncertainties of these inputs create uncertainties in Gclear that would be included in the variability of k*.

Nevertheless, I believe that a discussion is missing on the possible role of the selected model and its inputs on the results.

Since the authors are excluding the cases with high solar zenithal angle, I wonder whether the clearness index KT would not better fit. This index is the ratio of G to the irradiance that is impinging on a horizontal surface at the top of atmosphere G0. G0 is clearly better known that Gclear as it is mostly a function of the sun-earth geometry. This would remove possible ambiguities.

One of these ambiguities is the precise reference to some values of k*. It is clear that changing the clear-sky model will change the values of k*, in terms of means and variance (dynamics). This could at least be mentioned.

2. The cloud-enhanced state

The authors found k* much greater than 1. Though not clearly stated, they call such cases "cloud-enhanced state". I believe that the work will be stronger if the authors show such cases while discussing data itself. The discussion above shows that k* and thus its range (variance, dynamics) depends on the selected clear-sky model and its inputs. Hence, the assumption that "cloud-enhanced state" = "k*»1" must be substantiated.

Note that cloud-enhanced state is also evidenced in KT unambiguously.

3. The method for evidencing the variability

The authors are using appropriate tools for characterizing the variability.

I belive they may use other tools that are better known and that have strong mathematical support that would help for the analysis.

Such a question of variability is not new at all in meteorology. Analysing the change in k* as a function of the time lag (tau) for various time scales (Eq. 4) is a good idea that has been particularly studied in air turbulence by the researchers in the USSR several decades ago (see e.g. Kolmogorov, 1941).

The tool used is called structure function and is an extension of Eq. 4. It has been discovered later in geology where it is called semi-variogram (Matheron 1963). The variogram is the same than the structure function and they differ from the semi-variogram

by a factor 2 (see e.g. Wald, 1989).

The variogram is defined by D(tau) = E[[k*(t+tau)-k*(t)]**2], where E is the mean value.

The difference with Eq. 4 is the square, and the difference with the standard deviation in Fig. 6 is that the variogram do contain the influence of the mean value of [k*(t+tau)-k*(t)] which is removed in the standard deviation.

Using this tool would permit to rely on a considerable literature and a strong mathematical support, with possible links to another considerable literature relating to the Fourier power density spectra, another tool for studying variability with a considerable mathematical background.

4. A few editorial comments.

Page 12, line 6. The deviation for 10 s is not as noticeable in Fig. 6 as claimed. The meaning of "full shadow coverage" is unclear. The "'s" is only for persons in English.

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