Review of "How to observe the small-scale spatial distribution of surface solar irradiance, and how is it influenced by cumulus clouds?" by He et al.

15 May 2024

General comments

This study provides an analysis of observed and simulated surface solar irradiance (SSI) variability over land at small spatial scales for broken cloud conditions. After establishing the realism of the simulations, several sensitivity tests are applied to the simulations to explore the impact of cloud properties on the SSI variability. It is also proposed that a dense network of 10 surface-based pyranometers integrated over 10 mins can capture the main details of the SSI variability, providing useful guidance for sampling strategies targeted at future measurement campaigns.

The manuscript is very well written. Figures are mostly clear. The references are mostly appropriate. I feel that the features of SSI variability that are discussed are largely already known and documented in other the recent literature that has combined LES of shallow cumulus clouds with 3D radiative transfer. However, the observational focus of this study provides a somewhat different perspective that I think will still be of interest to the community. There are a few areas that would benefit from some further clarification and/or discussion, as outlined in my comments below. After addressing these minor comments, I recommend that the study be considered for publication in ACP.

Specific comments

L20-22: It is a common misconception that 3D cloud effects vanish with spatial and temporal averaging. Systematic 3D biases can remain after averaging. For example, see Fig. 6a in Gristey et al., 2020b (already cited in the submitted paper).

Section 2.1: It would be helpful to include some basic statistics such as the min/max/mean distances between the pyranometers. This will help with comparisons to the LES grid.

L133: The spatial resolution of the 3DRT seems to be much finer than the LES, is that correct? If so, relatively coarsely resolved clouds are contributing to relatively finely resolved surface irradiance. Can a justification be provided that this is physically reasonable?

Section 2.3: Are aerosols ignored in the simulations? It has been shown that aerosols, which are always present in reality to some extent, can have a substantial impact on the SSI PDF in such conditions (https://doi.org/10.1029/2022JD036822). This should at least be noted in the manuscript as a potential source of error assuming that aerosols are indeed excluded. It is interesting that the clear-sky PDFs (Fig. 3b) match quite well between the observations and simulations even without aerosols in the simulations. Perhaps this was a very pristine day. I recommend adding some discussion on this topic.

L177-178: The all sky images are a good check but are somewhat limited in spatial extent. The authors could also check satellite images from those days to confirm the presence of a widespread shallow cumulus cloud field at the location of interest.

L214: The choice of 500 W/m2 and 900 W/m2 seems rather arbitrary. Also, since the location of the two modes can vary from case to case or with different cloud properties (for example see Fig. 7), using fixed thresholds seems like it would incur some error because different portions of the PDFs will be represented. The authors could consider adjusting these thresholds depending on the PDF shape for a given case. Otherwise, I think a short discussion of this limitation in the manuscript is required.

L221-222: This approach assumes that the impact of different cloud properties on SSI can be linearly combined. Non-linearities are inherent in radiative transfer, especially 3D radiative transfer, so this might not be a great assumption. It should be noted that non-linearities are not accounted for, unlike the machine learning approaches used in other studies that are already mentioned.

L254-255: I expect the enhancement increase with cloud cover is because the remaining clear-sky region is receiving scattered radiation from more surrounding clouds.

Figure 3: The figure caption does not match what is plotted. I think b&c are swapped with d&e in the caption. I recommend revising the caption to improve clarity.

Section 5: The discussion and conclusions from this section are broadly consistent with that found in Fig. 7 here: https://doi.org/10.1175/BAMS-D-19-0227.1. They found that, when averaging for 1 hour, the bimodal PDF was difficult to identify from a single site, but became much clearer when considering 10 sites. I recommend including this relevant comparison.

L327: Cloud lifetime is only about 15mins for these clouds so the Taylor hypothesis probably does not hold very well in this environment.

L371-372: As well as the optically-thinner slant path, the wider footprint of scattering also means one cloud can be scattering more radiation into the shadow of a neighboring cloud, therefore adding to the “brightening”.

L422: The suggestion that SSI observations could provide a constraint for LES cloud droplet effective radius heterogeneity is not related to any of the sensitivity tests performed in this study. It’s an interesting suggestion but it comes a bit out-of-the-blue. If the authors would like to keep this statement, I think another sentence or two is required to provide some physical reasoning to support this claim.

L424-426: Cloud classifications that utilize SSI observations are already being developed. An appropriate reference to cite here is here: https://doi.org/10.1175/JAMC-D-20-0153.1.

L434-435: *“It does not include Large Eddy Simulation 3D output fields but they will be provided on demand.”* I am not sure that a statement like this fits well with the journal data policy. Since the authors are providing the data needed to produce the plots, and the underlying tools are available (correct?), it might be best to remove this statement.